Intergenerational Mobility Across Origins in France

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Abstract

To what extent is our position in society inherited or self-acquired? Understanding the forces shaping socio-economic trajectories from one generation to the next is crucial to addressing the perpetuation of inequalities. This thesis explores how the interplay of key parental characteristics determines individuals' socio-economic prospects. I focus namely on parents' income, country of birth, and place of residence. I rely on a combination of administrative datasets which allows me to describe comprehensively the intergenerational transmission of socio-economic (dis)advantage in France, and to isolate some of its underlying mechanisms.

In the first chapter, co-authored with Gustave Kenedi, we provide new estimates of intergenerational income mobility in France for children born in the 1970s using rich administrative data. Since parents' incomes are not observed, we employ a two-sample two-stage least squares estimation. We show, using the American Panel Study of Income Dynamics, that this method slightly underestimates rank-based measures of intergenerational persistence. Our results suggest that France is characterized by a strong persistence relative to other developed countries. 9.7% of children born to parents in the bottom 20% reach the top 20% in adulthood, four times less than children from the top 20%. We uncover substantial spatial variations in intergenerational mobility across departments, and a positive relationship between geographic mobility and intergenerational upward mobility. The expected income rank of individuals from the bottom of the parent income distribution who moved towards high-income departments is around the same as the expected income rank of individuals from the 75$^{\text{th}}$ percentile who stayed in their childhood department.

In the second chapter, I investigate the differences in intergenerational mobility between children born in France to native versus immigrant parents. For most origin groups, and systematically among daughters, income gaps with children of natives disappear when comparing individuals whose parents had the same income. Still, a gap persists for sons of immigrants from North Africa, despite higher rates of college graduation at the lower end of the parents' income distribution. The gap is lower among positive-income earners, and vanishes in terms of hourly wage, hinting at a labor market access mechanism. I investigate the role of residential segregation in this remaining gap using an instrumental variable approach. I estimate a spatial division index based on how geographical barriers partition the urban units individuals grew up in to isolate exogenous variations in segregation. Results suggest that residential segregation has a significantly negative effect on intergenerational mobility for sons of na-

tives, and even more so for sons of North African immigrants. A marginally significant effect is found for daughters of natives as well, but no effect is observed among other groups.

In the last chapter, co-authored with Yajna Govind, we examine how changing the costs of acquiring citizenship translates into naturalization decisions for second-generation immigrants, and the effect of naturalization on their labor market outcomes. We exploit the abolition of mandatory military service in France as an exogenous reduction in the cost of citizenship for men. In line with the predictions of our theoretical framework, we find that the reform induced a jump in male naturalization rates, entirely driven by European Union citizens. Using a Synthetic Difference-in-Differences, we show that the probability of employment for EU males consequently increased by 1.7 percentage points, through a reduction in inactivity rather than unemployment. We provide suggestive evidence that this effect is mainly driven by an increase in public sector employment and a reduction in self-employment.

Résumé

Dans quelle mesure doit-on notre position sociale à ce dont on hérite ou à ce qu'on acquiert soi-même ? Comprendre les forces qui gouvernent les opportunités socio-économiques d'une génération à l'autre est essentiel pour lutter contre la reproduction des inégalités. Cette thèse explore comment l'interaction entre trois caractéristiques parentales fondamentales détermine les perspectives socio-économiques des individus. L'analyse porte spécifiquement sur le revenu des parents, leur pays de naissance, et leur lieu de résidence. Cette étude est conduite sur une combinaison de sources de données administratives me permettant d'établir un portrait global de la transmission intergénérationnelle des (dés)avantages économiques en France, et d'en isoler certains mécanismes sous-jacents.

Dans le premier chapitre, co-écrit avec Gustave Kenedi, nous mobilisons de riches données administratives afin d'établir de nouvelles estimations de la mobilité intergénérationnelle des revenus en France pour les enfants nés dans les années 1970. Les revenus des parents n'y étant pas observés, nous utilisons la méthode des moindres carrés en deux étapes sur échantillons distincts. À partir du *Panel Study of Income Dynamics* états-unien, nous montrons que cette méthode sous-estime légèrement les mesures de persistance intergénérationnelle basées sur des quantiles. Nos résultats indiquent que la France est caractérisée par une forte persistance par rapport aux autres pays développés. 9,7 % des enfants nés de parents parmi les 20 % les plus pauvres atteignent les 20 % les plus riches à l'âge adulte, soit quatre fois moins que les enfants des 20 % les plus riches. Nous mettons en évidence d'importantes variations spatiales de mobilité intergénérationnelle entre départements, ainsi qu'une relation positive entre la mobilité géographique et la mobilité intergénérationnelle ascendante. L'espérance du centile de revenus pour les individus issus du bas de la distribution des revenus parentaux et qui ont déménagé vers des départements à hauts revenus est similaire à celle des individus issus du 75^{e} centile et qui sont restés dans leur département de résidence durant l'enfance.

Dans le deuxième chapitre, j'étudie les différences de mobilité intergénérationnelle entre les enfants nés de parents immigrés et ceux nés de parents nés en France. Pour la plupart des groupes d'origine, et systématiquement parmi les filles, les écarts de revenus avec les enfants de parents nés en France se réduisent en comparant les individus dont les parents avaient des revenus identiques. Néanmoins, un écart persiste pour les fils d'immigrés originaires d'Afrique du Nord, malgré un pourcentage plus élevé de diplômés du supérieur en bas de la distribution des revenus parentaux. L'écart est moindre parmi les individus ayant des

revenus strictement positifs, et disparaît en termes de salaire horaire, évoquant un mécanisme lié à l'accès au marché du travail. Je me concentre alors sur le rôle joué par la ségrégation résidentielle dans la persistance de cet écart en utilisant une approche par variable instrumentale. J'estime un indice de division spatiale basé sur la manière dont les barrières géographiques divisent les unités urbaines où les individus ont grandi, afin d'isoler les variations exogènes de la ségrégation résidentielle. Les résultats suggèrent que la ségrégation a un effet significativement négatif sur la mobilité intergénérationnelle pour les fils de natifs, et un effet plus fort encore pour les fils d'immigrés nord-africains. J'observe un effet marginalement significatif chez les filles de natifs, mais aucun parmi les autres groupes.

Dans le dernier chapitre, co-écrit avec Yajna Govind, nous examinons comment une variation des coûts associés à l'acquisition de la nationalité française se répercute sur les décisions de naturalisation des descendants d'immigrés, et nous quantifions l'effet de la naturalisation sur leur situation sur le marché du travail. Nous exploitons pour cela l'abolition du service militaire obligatoire en France en tant que baisse exogène du coût de la nationalité pour les hommes. Conformément aux hypothèses apportées par notre modèle théorique, nous constatons que cette réforme a provoqué une augmentation des taux de naturalisation chez les hommes, entièrement portée par les citoyens de l'Union européenne. À l'aide d'une méthode de double différence synthétique, nous estimons que la probabilité d'emploi des hommes européens a en conséquence augmenté de 1,7 point de pourcentage, et que cette hausse est majoritairement liée à une réduction de l'inactivité plutôt que du chômage. Une analyse complémentaire suggère que cet effet est principalement dû à une augmentation de l'emploi dans le secteur public et à une diminution de la part de travailleurs indépendants.

Contents

General introduction

If someone was born to parents from the bottom 20% of the income distribution, what would be their chances to reach the top 20% as an adult? According to the average French person this probability would be about 9%, showed Alesina et al. (2018). This is the most pessimistic guess among the five Western countries considered in their study, and also the largest underestimation. Are the French simply bad at guessing, or is there something more to it? Is there maybe a greater heterogeneity in social mobility across specific demographic characteristics, or more stickiness at the top of the distribution than in other countries? Either way, these pessimistic beliefs certainly echo growing concerns about equality of opportunities and socio-economic persistence across generations, upheld by a context of raising economic inequalities.

However, very little is known about the specifics of the French intergenerational mobility, especially in terms of income. One of the goals of this thesis is to bridge this gap by posing a comprehensive quantitative description of intergenerational income mobility patterns in France. The main takeaways from this analysis is that France is doing relatively poorly according to many intergenerational mobility indices, particularly for sons of North African immigrants. This raises moral concerns of social equity and fairness, as well as very pragmatic concerns related to trust in public institutions, or to the oversight of latent potentials referred to as "Lost Einsteins" (Bell et al., 2019).

Hence, the second core objective of this thesis is to better understand the determinants of intergenerational persistence, especially factors that polic makers may act on. Given that immigration background stands out as a key predictor of intergenerational persistence, I specifically focus on residential segregation and on citizenship acquisition.

I address these issues quantitatively using a combination of administrative datasets. This allows me to produce a rich description of intergenerational income persistence in France, and to provide evidence on some of its key determinants. This thesis is structured into three chapters, each focusing on a specific aspect of intergenerational mobility.

The first chapter, co-authored with Gustave Kenedi, explores intergenerational income mobility in France in a comparative and geographic perspective. The main objective of this paper is to estimate intergenerational mobility in France in a way that is as comparable as possible with the most recent literature, in order to formulate meaningful cross-country comparisons. We find that France is slightly more mobile than Italy and the United States, but much less than, for instance, Northern European countries.

We then deepen the analysis by exploring the spatial variations of intergenerational mobility within France. We find substantial variations across departments, with more persistence in the North and in the South, where the unemployment rate is also the highest. Finally, we characterize the relationship between intergenerational mobility and geographic mobility. Individuals from higher economic backgrounds tend to be more geographically mobile, but intergenerational mobility and geographic mobility are positively associated all along the parents' income distribution.

The second chapter focuses on intergenerational mobility differences across immigration backgrounds. I find very heterogeneous intergenerational mobility patterns according to the country of birth of parents, and gender. Immigrant parents are over-represented at the bottom of the income distribution, but in most cases, comparing individuals with identical parental incomes closes the income gap between children of immigrants and children of natives. Still, sons of immigrants from North Africa end up lower in the income distribution than sons of natives on expectation, even when comparing individuals whose parents had the same income. This remaining gap is only attributable to differences in unemployment and hours worked, as no gap is observed in terms of hourly wage.

These results echo the phenomenon of hiring discrimination but it may also partly result from residential segregation, through, for instance, a less diversified network. I investigate the role of residential segregation in this residual income gap using an instrumental variable strategy in order to isolate the causal component of this relationship. Specifically, I concentrate on variations in segregation induced by the placement of geographical barriers such as rivers, roads, or rails, to shut down the implication of potential confounding factors. I find that for a given level of parental income, segregation has a significantly negative effect on income for sons of natives, and even more so for sons of immigrants from North Africa, hence contributing to the residual income gap.

The third chapter, co-authored with Yajna Govind, focuses on a potential lever for the socio-economic integration of second-generation immigrants, that of citizenship acquisition. Indeed, while citizenship is often seen as a *reward* for integration, it may also be an effective *tool* to promote it. To study this question, we exploit the abolition of compulsory conscription as a sudden drop in the costs of acquiring citizenship for sons of immigrants. Indeed, this reform implied that for men born in France to foreign parents, doing military service was no longer necessary in order to acquire citizenship at age 18, while nothing changed for women.

We observe that the rate of citizenship acquisition remained very stable among women, but increased sharply among men at the time of the policy change. However, only children of immigrants from the European Union (EU) reacted to the reform, as the cost of doing military service does not appear to be binding for other groups of origin countries. In a synthetic difference-in-differences framework, we find that EU males are also the only ones to concomitantly experience a jump in employment, particularly in public sector employment, and a drop in self-employment, hinting at a positive effect of citizenship on labor market outcomes.

The remaining of this section consists in the more detailed introductions specific to each chapter.

Chapter I

To what extent is the income of individuals related to that of their parents? This question has seen renewed interest both in the general public and in academia as rising income inequality raised concerns about equality of opportunity. Examining this link is essential to understand whether children from different socio-economic backgrounds are afforded the same opportunities. It also matters for economic efficiency, as high persistence across generations may reflect an inefficient allocation of talents (so-called "Lost Einsteins"). Intergenerational persistence has now been estimated for a large number of countries, paving the way for insightful cross-country comparisons. Yet, much remains to be known for France, a country with relatively modest post-tax/transfers income inequality in international comparison and largely inexpensive higher education tuition fees.

The few existing studies for France only estimate the traditional intergenerational income elasticity (IGE), which captures the elasticity of child income with respect to parent income, and are based on small-sample surveys with self-reported incomes (Lefranc and Trannoy, 2005; Lefranc, 2018). Using a large sample combining census and tax returns data, we estimate two additional measures of intergenerational mobility: (i) the rank-rank correlation (RRC), increasingly prominent in the literature, which corresponds to the correlation between child and parent income percentile ranks, and (ii) transition matrices, which capture finer mobility patterns along the parent income distribution. While previous studies on France used self-reported labor earnings, we focus on household-level income measures. They provide a better depiction of one's economic resources and allow the inclusion of children raised by single mothers. Integrating these improvements from the "new" intergenerational mobility literature enables us to conduct a detailed international comparison to rank France relative to other advanced economies for which comparable estimates are available.

In addition, we investigate the spatial variations in intergenerational mobility across the 96 metropolitan French departments. Such subnational analyses, pioneered by Chetty et al. (2014a), help shed light on the mechanisms that may underlie income persistence across generations. Importantly, they highlight that national level estimates provide an incomplete assessment of a country's intergenerational mobility. We make use of the panel dimension of our data to describe the geographic mobility patterns of individuals and study the relationship between geographic mobility and intergenerational mobility. We investigate the separate roles of moving to a higher-income department from that of climbing the income ladder within departments, conditional on parent income rank.

Our analysis is conducted on almost 65,000 children born between 1972 and 1981, and observed in the Permanent Demographic Sample (EDP). This rich administrative dataset allows us to implement the contributions discussed above and to convincingly address concerns related to lifecycle and attenuation bias (Haider and Solon, 2006; Black and Devereux, 2011; Nybom and Stuhler, 2017). Since parents' incomes are not observed, we use a two-sample two-stage least squares (TSTSLS) estimation which consists in predicting parents' incomes using other parents drawn from the same population but for whom income is observed (Björklund and Jäntti, 1997). This method has been employed previously to estimate the IGE in the French context (Lefranc and Trannoy, 2005; Lefranc, 2018) as well as in many other countries (Jerrim et al., 2016, Table A1).

While studies typically use education and/or occupation to predict parent income, we make use of the richness of our data to also include detailed demographic characteristics of parents (French nationality dummy, country of birth, household structure, and birth cohort), and characteristics of the municipality of residence (unemployment rate, share of single mothers, share of foreigners, population, and population density). Our results are largely insensitive to the set of predictors. Parent income is then defined as the average[1] of father and mother predicted mean pretax wage over ages 35-45, and child income as pretax household income averaged over the same age range between 2010 and 2016. These two income definitions represent the most comprehensive household-level income definitions available for either generation.

TSTSLS Validation Exercise. Using the United States' Panel Study of Income Dynamics (PSID), we find that TSTSLS slightly underestimates rank-based measures of intergenerational persistence relative to what would be obtained if parent income were observed (OLS). The downward bias relative to the OLS estimate for the RRC ranges from 11% when education is the only predictor, to around 3-5% once occupation is also included. Subnational TSTSLS estimates are also fairly close to their OLS counterparts, though they tend to deviate more when the number of observations is small. Our results highlight that in settings like ours, where parent income cannot be directly observed, rank-based measures of intergenerational mobility obtained with TSTSLS likely provide lower bounds that are reasonably close to the true estimates. These findings confirm those obtained in different settings and samples by Cortes-Orihuela et al. (2022) and Jacome et al. (2023). We find that this reasoning also applies to the transition matrix.

National Results. Our main finding is that France exhibits relatively strong intergenerational income persistence compared to other developed countries. Our baseline estimate of the intergenerational elasticity in household income is 0.527, suggesting that on average, a 10% increase in parent income is associated with a 5.27% increase in child income. Put differently, if one's parents earn 10% more than the average of parents' incomes, then one is expected to preserve about 50% of that relative advantage. This estimate should be interpreted with caution considering our validation exercise suggests the TSTSLS IGE is significantly greater

[1]See Section 1.3.3 for an explanation for why we take the *average* rather than the *sum*.

than the true estimate. Applying the correction factor we find, the IGE decreases to 0.396.

Moving to the rank-rank relationship, we find that the conditional expectation of child income percentile rank with respect to parent income percentile rank is linear throughout most of the parent income distribution, with steeper relationships at the tails. Our baseline estimate of the rank-rank correlation is 0.303, implying that a 10 percentile increase in parent income rank is associated, on average, with a 3.03 percentile increase in child income rank. This estimate is of similar magnitude to that found for Italy (0.3; Acciari et al. (2022)), somewhat smaller than for the United States (0.341; Chetty et al. (2014a)), and markedly greater than existing estimates for other advanced economies such as Sweden (0.197; Heidrich (2017)), Australia (0.215; Deutscher and Mazumder (2020)) or Canada (0.242; Corak (2020)). Applying the correction factor we find in the validation exercise gives an RRC of 0.314 which does not affect France's relative position.

Intergenerational persistence, as captured by the transition matrix, is strongest at the tails of the parent income distribution: 9.7% of children from the bottom 20% of the parent income distribution reach the top 20% as adults. This probability is almost 4 times greater for children born to parents in the top 20% (38.4%). In comparison, the probability for a child born to a family in the bottom 20% to reach the top 20% in adulthood is 7.5% in the United States (Chetty et al., 2014a) and 12.3% in Australia (Deutscher and Mazumder, 2020). Moreover, persistence at the top becomes stronger and stronger as we zoom in on the right tail of the parent income distribution. As with the RRC, the validation exercise suggests these estimates represent upper (lower) bounds on mobility (persistence).

We show that our baseline results are robust to potential biases. Foremost, we evaluate how sensitive they are to the parent income prediction specification. In particular, we check whether varying the set of predictors or using non-parametric estimation methods influences our estimates. IGE estimates are overinflated when using only education as a predictor, while the RRC and transition matrices remain surprisingly stable regardless of the set of predictors used. Slightly improved prediction from using flexible models does not quantitatively alter our estimates. Moreover, we assess our estimates' sensitivity to the lifecycle and attenuation biases by varying the ages at which child and parent incomes are measured as well as the number of parent income observations used. Our baseline results do not appear to under- nor over-estimate intergenerational mobility due to measuring child and/or parent incomes too early or too late in the lifecycle nor because of averaging incomes over too few years.

Subnational Results. We uncover substantial spatial variations in intergenerational mobility across departments, comparable to those observed across countries. We define individuals' location as their department of residence in 1990, when they are between 9 and 18 years old. Higher levels of mobility are typically found in the West of France, and lower levels in the North and South. While the IGEs range from 0.30 to 0.45 in departments in Brittany (West), they range from 0.42 to 0.70 in departments in Hauts-de-France (North). The distribution of department-level RRCs is tighter than that of IGEs, but displays very similar spatial patterns.

We also characterize departments' absolute upward mobility (AUM), defined as the expected income rank of children born to parents at the 25$^{\text{th}}$ percentile, which is obtained from the fitted values of the department-level rank-rank regression (Chetty et al., 2014a). Absolute upward mobility ranges from the 36.8 in Pas-de-Calais (North) to 54.4 in Haute-Savoie (East). The Paris department stands out in terms of AUM (49.8) but exhibits around average intergenerational persistence levels in terms of IGE (0.51) and RRC (0.28). The cross-department correlation between the IGE and RRC is only 0.65, and -0.55 with AUM. This highlights the importance of using a variety of intergenerational mobility measures to characterize a country's income persistence across generations (Deutscher and Mazumder, forthcoming).

As a first step to understand the sources underlying these cross-department variations in intergenerational mobility, we undertake a simple correlational analysis. We find that absolute upward mobility exhibits much stronger relationships with department characteristics in general, than either the IGE or the RRC. This suggests that factors that affect absolute mobility might differ from those that affect relative mobility. The only characteristic consistently negatively correlated with intergenerational mobility is the unemployment rate. Intriguingly, we find no evidence of a within France "Great Gatsby Curve"[2] with respect to the IGE nor the RRC. This contrasts with findings from other countries (Acciari et al., 2022; Chetty et al., 2014a; Corak, 2020).

Lastly, we conduct a descriptive analysis of the relationship between intergenerational income mobility and geographic mobility. We document important gains in expected income rank for movers, which are slightly decreasing in parent income rank. For children from families in the bottom decile, movers have an expected rank approximately 5.6 percentiles greater than stayers, while this difference is of roughly 4.4 percentiles for children from families in the top decile. These gains are partly attributable to movers locating in higher-income departments in adulthood relative to stayers, but also to movers reaching local ranks in their adulthood department that are further away from the rank of their parents in the childhood department. Destination departments are on average characterized by higher income levels than origin departments only at the tails of the parent income distribution. However, regardless of parent income rank, the absolute upward mobility gains associated with moving to a higher-income department appear to be large and increasing with average income in the destination department. All these findings combine self-selection and causal effects, and we leave the disentangling of these two channels for future research.

Chapter II

Being born to natives or immigrants is an innate characteristic that has consistently been shown as one of the strongest determinants of socio-economic outcomes. In theory, in soci-

[2]The "Great Gatsby Curve" refers to the positive correlation between intergenerational income persistence (defined by the IGE) and income inequality (defined by the Gini index) found across countries (Corak, 2013).

eties with low intergenerational mobility, the mere fact that immigrant parents tend to earn less than natives mechanically lowers their children's expected outcomes. In practice, in most western European countries, children of immigrants are shown to be still worse off when compared to children of natives with similar parental backgrounds. Discrimination has largely been documented as one driver of this conditional gap by the correspondence testing literature, but other factors may be at play. In this study, I investigate the role of residential segregation, a potential driver of this remaining gap which is easier to quantify in a more exhaustive and systematic manner, and thus easier for policy-makers to act on.

Results show that in most cases, accounting for parents' position in the income distribution closes down the income rank gap between children of immigrants and children of natives. Sons of immigrants from North Africa, however, remain persistently lower than natives in the income distribution, even within parents' income decile. In an instrumental variable approach, I show that sons of immigrants from North Africa are also the only group besides natives to experience a negative effect of residential segregation on conditional income rank. These results extend the literature by providing evidence on the effect of residential segregation on intergenerational mobility differences between children of immigrants and natives in a different cultural and historical context of immigration than in the United States, more representative of most Western European countries.

I conduct the analysis on a French administrative dataset which combines lifetime census data, tax data, and employer-employee data. This allows me to observe the share of immigrants where individuals grew up in, down to the building level, their own immigration background, and their detailed earnings when adults. This merger of administrative data sources is available for a pseudo-random 1% sample of the population, which contains about 85,000 individuals for the 1972-1984 birth cohorts considered. Following the methodology developed in the first chapter, I estimate parents' income rank based on their detailed socio-demographics observed in individuals' childhood census data. Robustness checks suggest that inaccuracies in income rank predictions, as well as lifecycle and attenuation bias, are mild and homogeneous across origin groups.

To begin with, I describe the overall income rank convergence with natives between first-generation immigrants and their children. The over-representation of immigrants at the bottom of the income distribution is much more pronounced among parents born in North Africa, with 50% of them pertaining to the first decile. Children of immigrants, however, are distributed much more evenly in the income distribution, especially those with a South European background as they completely caught up with natives. Still, a significant gap of about 10 percentile ranks is observed for children of immigrants from North Africa.

Comparing income ranks within parents' income decile systematically raises the average income rank of children of immigrants relative to children of natives. Children of South European immigrants perform even better than natives conditional on parents' income, hence the absence of unconditional gap despite large differences in parents' income backgrounds.

The gap between daughters of natives and daughters of North African parents closes down completely within income deciles, but for sons, a gap of 4 percentile income ranks persists. These results are very reminiscent to what Chetty et al. (2020) documented across race for the United States: a black-white gap among men but not women, and intergenerational mobility rates much more similar to natives among hispanic individuals.

This remaining gap between sons of immigrants from North Africa and sons of natives does not seem to mainly be explained by differences in educational attainment. Indeed, higher education graduation rates tend to be larger for children of immigrants conditional on parents' income ranks, irrespective of gender and origin group. To better understand the nature of the conditional income-rank gap, I distinguish what comes from higher-paying jobs and what comes from more hours worked using two alternative specifications. First, when I exclude non-positive incomes, the conditional income rank gap between children of natives and children of immigrants from North Africa reduces. Second, in terms of hourly wage, it vanishes completely. This suggests that the remaining gap stems from differences in labor market access both at the extensive and at the intensive margin, despite conditional higher education attainment that are higher than natives' on average.

I focus on residential segregation as a potential driver of this reduced labor-market access. I measure shares of immigrants in individuals' areas of residence based on the exhaustive population census, which I then match at the urban unit, neighborhood, and building level. The average share of immigrants in one's building of residence is 8% for children of natives, and 26% for children of immigrants. The strength of the relationship between the local share of immigrants and individuals' income rank conditional on parents' rank varies depending on the geographical level considered. It is at the neighborhood level that this relationship is the most strongly and consistently negative.

I measure residential segregation in urban units using the Duncan dissimilarity index, which captures how unevenly immigrants are distributed across neighborhoods. Residential segregation tends to be lower in the South and in the East, and higher in the North and in the West. To get a naive sense of the role played by residential segregation in the negative relationship between the local share of immigrants and conditional income rank, I consider their relationships jointly. The interaction between the share of immigrants and segregation absorbs the entirety of the negative effect, hinting at the fact that segregation may actually mediate the negative relationship initially attributed to the share of immigrants.

To isolate the causal component of this relationship, I implement an instrumental variable strategy adapted from Chyn et al. (2022), based on how geographical barriers divide urban units. Specifically, I consider the combination of waterways, roadways, and railways to compute an index capturing how unevenly the area in urban units is distributed across the sub-units generated by these features, controlling for their length in the urban unit. The underlying idea is that conditional on the density of such features in a given urban unit, the way they divide space can be more or less prone to residential segregation without otherwise affecting the intergenerational mobility prospects of resident children.

Baseline results document a negative effect of residential segregation on income rank conditional on parents' income rank both for sons of natives and for sons of immigrants from North Africa. A one standard deviation increase in residential segregation reduces by 0.68 percentile rank the effect of a one percentage point increase in the local share of immigrants on individuals' income rank for sons of natives, conditional on parents' income rank, on average. For sons of immigrants from North Africa, this effect reaches -1.9 percentile income ranks. No significant effect is observed for children of South European origin, and a smaller and inconsistently significant effect is found for daughters of natives.

The assumption that these interpretations rely on is that the way in which these features divide space more or less equally into more or less sub-units has no effect on intergenerational mobility other than through residential segregation. The validity of this assumption is jeopardized by the fact that some of the barriers considered constitute transportation networks, which can themselves foster intergenerational mobility as shown for Argentina (Pérez, 2018) and England and Wales (Costas-Fernández et al., 2020). In addition to controlling for the length of each feature in urban units, I construct an index capturing the extent to which the railway network offers labor-market opportunities using aggregate wages in urban units directly connected via the railway network, weighted by their bilateral distances. Results are robust to the inclusion of this index.

This chapter contributes to two main strands of the literature. First, it relates to the literature on the intergenerational mobility and socio-economic integration of immigrants and their children. Intergenerational mobility prospects of children of immigrants are often shown to be less favorable than those of children of natives. This was notably documented for Estonia (Kivi et al., 2021), the Netherlands (Van Elk et al., 2024), and Sweden (Bratu and Bolotnyy, 2023), but not for Denmark (Jensen and Manning, 2024) or the United States (Abramitzky et al., 2021). Still, Mazumder (2014) and Chetty et al. (2020) find significant heterogeneity in intergenerational mobility across race in the United States. Because it is much less common to have racial information in European datasets, and because of different historical contexts of immigration, it must be kept in mind that variation in the overlap between race and recent immigration background may hamper international comparisons.

In France specifically, the intergenerational mobility of second-generation immigrants has notably been studied based on the *Trajectoires & Origines* survey conducted by the Insee and Ined (Beauchemin et al., 2016). In particular, Beauchemin (2018) identifies migration background as a key factor of heterogeneity in intergenerational socio-economic mobility, and shows that sons of immigrants from North Africa are particularly disadvantaged. Achard (2024) also shows that for children of immigrants, parents' characteristics are less predictive than grandparents' characteristics due to the non-lasting socio-economic downgrading experienced by first-generation immigrants. In this study, I complement these findings using administrative data to document the intergenerational mobility prospects of children of immigrants and natives in more details along the parental income distribution.

Second, this chapter contributes to the literature focusing on residential segregation and neighborhood effects. In the United States, Andrews et al. (2017) show that past racial segregation explain a significant part of the variation in intergenerational mobility documented by Chetty et al. (2014b). In France, Weber et al. (2024) document a persistent neighborhood disadvantage among the offspring of non-European immigrants, and McAvay and Safi (2018) elicit a higher risk of cumulative spatial disadvantage for North African and Sub-Saharan African immigrants. Regarding neighborhood effects, Hémet and Malgouyres (2018) notably show that in France, diversity at the neighborhood level in terms of parents' origins matters less than diversity in terms of nationality for employment prospects. It is in that context that this chapter endeavors to quantify the causal effect of residential segregation on intergenerational mobility differences across origins.

Chapter III

The continuous growth of migration flows raises crucial questions on the most adapted integration and regulation policies to implement. Naturalization, which represents the final legal step in the integration process for migrants, has consistently been a focal point of this debate. On the one hand, naturalization may be seen as a policy tool to boost migrants' integration. On the other hand, it is considered a reward for a successful integration. The latter standpoint currently guides the dominant policy-making approach. Host countries impose substantial costs on citizenship acquisition, intending to screen immigrants.

However, there is limited causal evidence on how migrants respond to changes in the cost of naturalization, and on the impact of naturalization on the labor market integration of second-generation migrants. This is due to three main challenges. First, naturalization take-up is an endogenous decision that raises concerns of selection bias. Second, exogenous shocks in existing studies often impact cohorts that are still too young for their labor market outcomes to be studied. Third, studies on citizenship acquisition for second-generation immigrants, which in most cases rely on reforms impacting individuals at young ages, may not fully be able to disentangle its effects on education from its direct labor market effects.

In this chapter, we overcome these challenges by relying on two key aspects of the French context. First, individuals born in France to foreign parents face almost no costs in acquiring French citizenship at the age of 18. Second, during the 1990s, compulsory conscription for male citizens made naturalization a costly choice for foreign men. Altogether, these two features induced a salient trade-off for second-generation men at the age of 18 between renouncing French citizenship and doing military service. In this context, we leverage the abolition of compulsory conscription in 1997 for men born after 1978 as an exogenous shock in the costs of acquiring French citizenship for children of immigrants. We exploit administrative and survey data to explore how this reform affected the take-up of citizenship, and the potential repercussions on labor market outcomes.

Our results show that the abolition of compulsory conscription induced a sharp increase in naturalization rates for males relative to females. This effect is entirely driven by European Union (EU) citizens, the group of second-generation immigrants for whom acquiring French citizenship should matter the least. Still, we find that the surge in naturalization induced a significant 1.7 percentage point increase in employment for this group.

Our setting has the advantage of mechanically shutting down three candidate channels to this effect. Indeed, the fact that compliers are EU citizens ensures that the employment effect does not stem from either the right to reside and work in the host country, the access to welfare benefits, or the stability granted by citizenship. Two of the classical potential mechanisms put forward in the literature remain relevant for this group: labor-market access restrictions, and discrimination.

We show that the increase in employment was accompanied by higher shares of public sector jobs and a departure from self-employment, supporting the labor-market access hypothesis. We postulate that hiring discrimination also contributes to this effect, as we observe significantly positive shares of self-reported discrimination and racism even among second-generation immigrants with European Union origins. We also rule out the possibility that these are driven by a direct impact of military service.

To rationalize the decision to naturalize, we introduce a theoretical framework in which individuals take up citizenship as long as the benefits exceed the costs. We consider a cost function that is decreasing with skills, which is typically the case of policies such as language tests or financial requirements. The model predicts that if benefits are homogeneous, such costs would screen the top of the skill distribution. However, if benefits are heterogeneous, such costs may screen the bottom of the skill distribution by excluding low-benefit high-skilled individuals. This scenario applies to settings where groups that are the most discriminated against on the labor market are also the lower educated.

In the French context, we expect European Union (EU) citizens to benefit the least from citizenship. Indeed, unlike individuals from other birth nationalities, they can freely work and reside in France. Thus, they are the least likely to take up citizenship under compulsory conscription, and the most likely to react to its abolition. On the contrary, we expect individuals from nationalities that are typically discriminated against in the labor market, such as African nationalities, to benefit the most from citizenship. Costs are likely to be heterogeneous as well because low-educated conscripts were typically assigned more strenuous positions (Maurin and Xenogiani, 2007). As a result, we expect the abolition of compulsory conscription to have a larger impact on take-up at the bottom of the education distribution within groups of birth nationalities.

To test these hypotheses empirically, we exploit the fact that women were exempt from compulsory conscription and therefore unaffected by its abolition. Using a Difference-in-

Differences approach, we compare the naturalization rate of foreign men and women born in France before and after December 31st, 1978. We find that at the abolition of compulsory conscription, the naturalization rate of males increased from 68.5% to 78.9%, while the rate for females remained stable at around 84%. This suggests that almost a quarter of the missing citizenship take-up among young males was due to compulsory conscription and that its abolition halved the gap with women.

Consistent with our theoretical framework, results show that this effect is entirely driven by European Union citizens at birth, for whom the benefits of acquiring French citizenship are lower. Within this group, the abolition of compulsory conscription increased male naturalization rates by 11.9 percentage points. No significant effect is observed for other birth nationalities, for which the cost of military service is therefore not binding. Among EU citizens, we find that the increase in naturalization rates is more than 50% larger for low-educated individuals compared to high-educated individuals, supporting the hypothesis that the cost of military service is lower for the latter.

We then take advantage of the fact that only EU males experienced a jump in naturalization to study its effect on their labor market outcomes. Specifically, we exploit every unaffected group in a Synthetic Difference-in-Differences approach to best capture how the outcomes of EU males would have evolved absent the abolition of compulsory conscription. The synthetic control group closely mirrors the trend in the employment rate of EU males until the reform, after which the employment rate in the treated group diverged from its path with a 1.7 percentage point increase. Given that this effect is driven by 11.9% of EU males who reacted to the reform, it corresponds to a 14.5 percentage point increase among compliers. We show that this positive effect on employment is primarily attributable to a decrease in inactivity rather than in unemployment.

We explore two potential mechanisms explaining these results. First, we observe a significant increase in the probability of being employed in the public sector. Second, we find a decrease in self-employment for EU males relative to the control groups, in line with the idea that citizenship acquisition expands the set of labor market opportunities for naturalized individuals.

We conduct sensitivity checks showing that our results are robust to the set of control groups considered in our Synthetic Difference-in-Differences setting, to anticipation effects, to general equilibrium effects, to differential attrition, and to the relative length of military service in the origin country nationality. In addition, we address the concern that compulsory conscription might directly impact educational and labor market outcomes. Conscription has been shown to have either no impact or a positive impact on educational outcomes, in line with draft avoidance behavior, and on labor market outcomes in the French context (Maurin and Xenogiani, 2007; Mouganie, 2020). We provide evidence that second-generation immigrants' education levels were not impacted by compulsory conscription. We also rule out a potential direct effect on labor market outcomes given the absence of labor market effects for the birth nationality groups which only experienced the abolition of military service but no changes in naturalization take-up. In addition, Mouganie (2020) documents that in France, military service had either no effect or positive effects on labor market outcomes. Given these factors, our study may, if anything, underestimate the actual labor market impact of naturalization.

This chapter relates and contributes to three different strands of the literature. First, it sheds light on the effects of acquiring citizenship on the labor market integration of second-generation migrants. The related literature has largely focused on first-generation, establishing a positive correlation between naturalization and labor market outcomes, starting with the work of Chiswick (1978). An emerging strand of this literature has explored the causal link between the two, for first-generation immigrants and refugees (Gathmann and Keller, 2018; Hainmueller et al., 2019; Govind, 2021; Fasani et al., 2023; Hainmueller et al., 2023). We contribute to this literature by studying the labor market integration of second-generation immigrants, touching upon the literature on birthright citizenship which has so far focused on educational outcomes (Felfe et al., 2020, 2021; Dahl et al., 2022). Our findings demonstrate that even populations who might have less to gain from naturalization, here second-generation EU citizens, experience improved economic integration from naturalization.

Second, this chapter contributes to the literature on citizenship take-up. Various studies have explored the association between the propensity to naturalize and individuals' or origin countries' characteristics such as age at migration, gender, educational attainment, and political conditions in the home country (Yang 1994; Chiswick and Miller 2009; Fougère and Safi 2009. See Gathmann and Garbers (2023) for a detailed review of the literature). In addition, citizenship acquisition costs such as civic knowledge requirements, naturalization fees, and multiple citizenship restrictions, have been shown to directly affect take-up, especially that of low-educated individuals and EU citizens (Yasenov et al., 2019; Peters and Vink, 2023; Vink et al., 2021). This chapter contributes to the existing literature by formalizing citizenship take-up with a cost-benefit theoretical framework. We discuss the unintended implications of increasing naturalization costs in the face of heterogeneous benefits, echoing recent evidence of potential backlash of integration policies (Fouka, 2020; Dahl et al., 2022; Arendt et al., forthcoming).

Third, we contribute to the literature on the effects of military service. Existing research has mainly focused on the impact of conscription on citizens' outcomes such as education, employment, earnings, political behavior, and crime (e.g., Angrist 1990; Bauer et al. 2012; Card and Cardoso 2012; Hubers and Webbink 2015; Hjalmarsson and Lindquist 2019; Savcic et al. 2023, and more specifically on France: Maurin and Xenogiani 2007; Fize and Louis-Sidois 2020; Mouganie 2020). To the best of our knowledge, we are the first to investigate the effects of military service on non-citizens, and more specifically its implications for their naturalization decisions.

Introduction générale

Pour une personne issue d'une famille parmi les 20 % les plus pauvres, quelles seraient les chances de percevoir un revenu parmi les 20 % les plus élevés à l'âge adulte ? Selon le français moyen cette probabilité serait d'environ 9 %, d'après les résultats d'Alesina et al. (2018). Il s'agit de l'estimation la plus pessimiste parmi les cinq pays occidentaux considérés dans leur étude, et également de la sous-estimation la plus importante. Les français font-ils simplement preuve d'une mauvaise intuition, ou cette sous-évaluation est-elle bien fondée ? Y-aurait-il, par exemple, de plus grandes différences de mobilité sociale selon certaines caractéristiques démographiques, ou bien une plus grande rigidité en haut de la distribution des revenus que dans d'autre pays ? Quelle qu'en soit la raison, ces perceptions négatives font écho aux préoccupations croissantes concernant l'égalité des chances et la persistance socio-économique d'une génération à la suivante, renforcées par un contexte d'accroissement des inégalités économiques.

Cependant, on ne sait encore que très peu des caractéristiques de la mobilité intergénérationnelle française, notamment selon le revenu. L'un des objectifs de cette thèse est de combler cet espace en proposant une description quantitative la plus exhaustive possible de la mobilité intergénérationnelle de revenus en France. Les principaux résultats de cette analyse montrent que la France est plutôt mal positionnée d'après la plupart des indices de mobilité intergénérationnelle, en particulier pour les fils d'immigrés d'Afrique du nord. Cela soulève des questions morales d'équité et de justice, ainsi que des préoccupations très pragmatiques concernant la fragilisation de la confiance accordée aux institutions publiques et la mauvaise gestion des potentiels, ceux qu'on appelle alors les *"Lost Einsteins"* (Bell et al., 2019).

Ainsi, le deuxième objectif de cette thèse est de mieux comprendre les déterminants de la persistance intergénérationnelle, en particulier les facteurs sur lesquels les décideurs politiques peuvent avoir la main. Étant donné que l'ascendance migratoire se révèle être un prédicteur majeur de la persistance intergénérationnelle, je me concentre spécifiquement sur la ségrégation résidentielle et sur l'acquisition de la nationalité française.

Tout au long de cette thèse, je traite ces questions de manière quantitative à partir d'une combinaison de bases de données administratives. Cela me permet de produire une riche description de la persistance intergénérationnelle de revenus en France et de tirer des conclusions sur certains de ses principaux déterminants. Cette thèse est structurée en trois chapitres, chacun se concentrant sur un aspect spécifique de la mobilité intergénérationnelle.

Le premier chapitre, coécrit avec Gustave Kenedi, investi le sujet de la mobilité intergénérationnelle des revenus en France selon une perspective comparative et géographique. L'objectif principal de cet article est d'estimer la mobilité intergénérationnelle en France de manière aussi comparable que possible avec la littérature la plus récente, afin de formuler des comparaisons informatives entre pays. Nous constatons que la France est légèrement plus mobile que l'Italie et que les États-Unis, mais beaucoup moins que les pays d'Europe du Nord.

Nous approfondissons ensuite l'analyse en explorant les variations spatiales de la mobilité intergénérationnelle sur le territoire métropolitain. Nous observons des variations substantielles entre départements, avec une plus grande persistance dans le Nord et dans le Sud, où les taux de chômage sont également les plus élevés. Enfin, nous caractérisons la relation entre la mobilité intergénérationnelle et la mobilité géographique. Les individus issus de milieux économiques plus favorisés ont tendance à être plus mobiles géographiquement, mais la mobilité intergénérationnelle et la mobilité géographique sont positivement associées tout au long de la distribution des revenus parentaux.

Le deuxième chapitre se concentre sur les différences de mobilité intergénérationnelle selon l'ascendance migration. J'observe des schémas de mobilité intergénérationnelle très hétérogènes selon le pays de naissance des parents et selon le genre. Les parents immigrés sont sur-représentés en bas de la distribution des revenus, mais dans la plupart des cas, le fait de comparer des individus ayant des revenus parentaux identiques comble l'écart de revenus entre les enfants d'immigrés et les enfants de parents nés en France. Toutefois, les fils d'immigrés originaires d'Afrique du Nord se retrouvent plus bas dans la distribution des revenus que les fils de natifs, même à revenus parentaux donnés. Cet écart résiduel est uniquement attribuable à des différences de taux chômage et de nombres d'heures travaillées, car aucun écart n'est observé en termes de salaire horaire.

Ces résultats font écho au phénomène de discrimination à l'embauche, mais ils peuvent également en partie résulter de la ségrégation résidentielle, via un réseau moins diversifié par exemple. Ainsi, j'étudie le rôle de la ségrégation résidentielle dans cet écart de revenus résiduel. J'utilise une stratégie de variable instrumentale afin d'isoler la composante causale de cette relation. Plus précisément, je me concentre sur les variations de ségrégation résidentielle induites par la présence de barrières géographiques telles que les rivières, les routes, ou les voies ferrées, afin d'éliminer l'implication de potentielles variable confondantes dans la relation. Les résultats indiquent qu'à revenus parentaux donnés, la ségrégation résidentielle a un effet significativement négatif sur les revenus des fils de parents nés en France, et un effet plus négatif encore pour les fils d'immigrés originaires d'Afrique du Nord, contribuant ainsi à l'écart de revenus résiduel.

Le troisième chapitre, coécrit avec Yajna Govind, se concentre sur un potentiel levier d'intégration socio-économique pour les descendants d'immigrés : la naturalisation. En effet, si la nationalité française est souvent perçue comme une *récompense* à l'intégration, elle peut également constituer un outil pour la promouvoir. Pour étudier cette question, nous exploitons l'abolition du service militaire obligatoire en tant que baisse soudaine du coût d'acquisition de la nationalité pour les fils d'immigrés. En effet, cette réforme a impliqué que pour les hommes nés en France de parents étrangers, il n'était plus nécessaire d'effectuer le service militaire pour acquérir la nationalité française à l'âge de 18 ans, tandis que la situation des femmes est restée inchangée.

Nous observons que le taux de naturalisation est resté très stable parmi les femmes, mais a fortement augmenté parmi les hommes au moment du changement de politique. Cependant, seuls les descendants d'immigrés provenant de l'Union européenne (UE) ont réagi à la réforme. Par une approche de double différence synthétique, nous constatons que les fils d'immigrés de l'UE sont également les seuls pour qui l'emploi a concomitamment augmenté, en particulier dans le secteur public, et pour qui l'auto-emploi a baissé, suggérant un effet positif de la citoyenneté sur les résultats sur le marché du travail.

La suite de cette section est constituée des introductions plus détaillées spécifiques à chaque chapitre.

Chapitre I

Dans quelle mesure le revenu des individus est-il corrélé à celui de leurs parents ? L'augmentation des inégalités de revenus a remis la question de l'égalité des chances sur le devant de la scène, et provoqué un regain d'intérêt tant dans le débat public qu'au sein du monde académique. Examiner cette relation est essentiel pour comprendre si les enfants de milieux socio-économiques différents bénéficient ou non des mêmes opportunités. Cela importe également du point de vue de l'efficacité économique, car une forte persistance intergénérationnelle peut refléter une allocation inefficace des talents (les fameux « *Lost Einsteins* »). La persistance intergénérationnelle a désormais été estimée pour un grand nombre de pays, ouvrant la voie à des comparaisons internationales particulièrement instructives. Pourtant, la France reste relativement sous-étudiée sur ces thématiques, un pays pourtant particulièrement intéressant au regard des inégalités des revenus relativement modestes après impôts/transferts en comparaison internationale, et des frais de scolarité de l'enseignement supérieur plutôt abordables.

Les études existantes sur la France estiment uniquement la traditionnelle élasticité intergénérationnelle des revenus (IGE), qui mesure l'élasticité du revenu des enfants par rapport à celui de ses parents, et se basent sur des enquêtes à faible échantillon contenant des revenus auto-déclarés (Lefranc and Trannoy, 2005; Lefranc, 2018). En utilisant un large échantillon combinant des données de recensement et des déclarations fiscales, nous estimons deux mesures supplémentaires de la mobilité intergénérationnelle : (i) la corrélation rang-rang (RRC), de plus en plus mise en avant dans la littérature, qui correspond à la corrélation entre les rangs en centile de revenus des enfants et des parents, et (ii) les matrices de transition, qui capturent plus finement les comportements de mobilité tout au long de la distribution des revenus parentaux. Tandis que les précédentes études sur la France utilisaient des revenus du travail auto-déclarés, nous mobilisons une mesure plus globale des revenus au niveau ménage. Celle-ci offre une meilleure représentation des ressources économiques d'un individu et permettent d'inclure les enfants ayant grandis auprès d'une mère célibataire. La prise en compte de ces avancées de la « nouvelle » littérature sur la mobilité intergénérationnelle nous permet de mener une comparaison internationale détaillée pour situer la France par rapport aux autres économies avancées pour lesquelles des estimations comparables sont disponibles.

De plus, nous examinons les variations spatiales de la mobilité intergénérationnelle à travers les 96 départements de France métropolitaine. Ce type d'analyses infranationales, initiées par Chetty et al. (2014a), permet de mieux comprendre les mécanismes qui pourraient sous-tendre la persistance des revenus à travers les générations. Cela montre révèle généralement que les estimations nationales offrent une évaluation incomplète de la mobilité intergénérationnelle d'un pays. Nous utilisons la dimension longitudinale de nos données pour décrire les schémas de mobilité géographique des individus et étudier la relation entre mobilité géographique et mobilité intergénérationnelle. Nous analysons les rôles distincts du fait de déménager dans un département à revenus plus élevés et du fait de progresser dans l'échelle des revenus au sein de son département, selon le rang de revenus des parents.

Notre analyse porte sur près de 65 000 enfants nés entre 1972 et 1981 et observés dans l'Échantillon Démographique Permanent (EDP). Ce riche jeu de données administratives nous permet de réaliser les contributions mentionnées ci-dessus et de répondre de façon convaincante aux préoccupations liées aux biais de cycle de vie et au biais d'atténuation (Haider and Solon, 2006; Black and Devereux, 2011; Nybom and Stuhler, 2017). Les revenus des parents n'étant pas observés, nous utilisons une estimation par moindres carrés en deux étapes sur échantillons distincts (TSTSLS), consistant à prédire les revenus des parents en mobilisant d'autres parents tirés de la même population mais dont le revenu est observé (Björklund and Jäntti, 1997). Cette méthode a déjà été utilisée pour estimer l'IGE dans le contexte français (Lefranc and Trannoy, 2005; Lefranc, 2018) ainsi que dans de nombreux autres pays (Jerrim et al., 2016, Table A1).

Alors que les études utilisent généralement l'éducation et/ou la profession pour prédire le revenu des parents, nous exploitons la richesse de nos données pour inclure également des caractéristiques démographiques détaillées des parents (variable binaire pour la nationalité française, pays de naissance, structure familiale et cohorte de naissance), ainsi que des caractéristiques de la municipalité de résidence (taux de chômage, part de mères célibataires, part d'étrangers, population et densité de population). Nos résultats sont largement insensibles aux variations de combinaisons de ces prédicteurs. Le revenu des parents est ensuite défini comme la moyenne[3] du salaire brut moyen prédit des pères et des mères entre 35 et 45 ans, et le revenu des enfants comme le revenu brut du ménage moyenné aux mêmes âges, entre 2010 et 2016. Ces deux définitions de revenu au niveau ménage sont les plus complètes disponibles

[3]Voir la section 1.3.3 pour une explication sur les raisons pour lesquelles nous prenons la *moyenne* plutôt que la *somme*.

pour chaque génération.

Exercice de Validation du TSTSLS. À l'aide du Panel Study of Income Dynamics (PSID) états-unien, nous constatons que le TSTSLS sous-estime légèrement les mesures de persistance intergénérationnelle basées sur des quantiles par rapport à ce qui serait obtenu si le revenu des parents était observé (moindres carrés ordinaires - OLS). Le biais à la baisse par rapport à l'estimation OLS pour la RRC varie de 11 % lorsque l'éducation est le seul prédicteur, à environ 3-5 % une fois la profession également incluse. Les estimations infranationales par TSTSLS sont aussi assez proches des résultats OLS correspondants, bien qu'elles aient tendance à diverger davantage lorsque le nombre d'observations est faible. Nos résultats soulignent que dans des contextes comme le nôtre, où le revenu des parents ne peut pas être directement observé, les mesures de mobilité intergénérationnelle basées sur les quantiles obtenues par TSTSLS fournissent plausiblement des bornes inférieures raisonnablement proches des vraies valeurs sous-jacentes. Ces conclusions corroborent celles formulées à partir d'autres contextes et échantillons par Cortes-Orihuela et al. (2022) et Jacome et al. (2023). Nous constatons que cela s'applique également aux matrices de transition.

Résultats Nationaux. Notre principale conclusion est que la France présente une persistance intergénérationnelle des revenus relativement élevée par rapport aux autres pays développés. Notre mesure de référence de l'élasticité intergénérationnelle des revenus des ménages est de 0,527, ce qui suggère qu'en moyenne, une augmentation de 10 % du revenu des parents est associée à une augmentation de 5,27 % du revenu des enfants. En d'autres termes, si les parents d'un individu gagnent 10 % de plus que la moyenne des revenus parentaux, cet individu devrait conserver environ 50 % de cet avantage relatif. Cette estimation doit être interprétée avec prudence, car notre exercice de validation suggère que l'IGE estimé par TSTSLS est significativement supérieur à celui qui aurait été estimé par OLS. En appliquant le facteur de correction issu de notre exercice de validation, l'IGE diminue à 0,396.

En ce qui concerne la relation rang-rang, nous constatons que l'espérance conditionnelle du rang de revenu des enfants par rapport au rang de revenu des parents est linéaire sur la plupart de la distribution des revenus des parents, avec des relations plus prononcées aux extrémités. Notre estimation de référence de la corrélation rang-rang est de 0,303, impliquant qu'une augmentation de 10 centiles du rang de revenu des parents est associée, en moyenne, à une augmentation de 3,03 centiles du rang de revenu des enfants. La magnitude de cette estimation est similaire à celle obtenue pour l'Italie (0,3 ; Acciari et al. (2022)), légèrement inférieure à celle des États-Unis (0,341 ; Chetty et al. (2014a)), et nettement supérieure aux estimations existantes pour d'autres économies avancées comme la Suède (0,197 ; Heidrich (2017)), l'Australie (0,215 ; Deutscher and Mazumder (2020)) ou bien le Canada (0,242 ; Corak (2020)). L'application du facteur de correction que nous trouvons dans l'exercice de validation donne une RRC de 0,314, ce qui ne modifie pas la position relative de la France.

La persistance intergénérationnelle, telle que capturée par la matrice de transition, est plus forte aux extrémités de la distribution des revenus parentaux : 9,7 % des enfants issus des 20 % de familles les plus pauvres atteignent les 20 % les plus riches à l'âge adulte. Cette probabilité est presque 4 fois plus élevée pour les enfants nés de parents parmi les 20 % les plus riches (38,4 %). En comparaison, la probabilité qu'un enfant né dans une famille parmi les 20 % les plus pauvres atteigne les 20 % les plus riches à l'âge adulte est de 7,5 % aux États-Unis (Chetty et al., 2014a) et de 12,3 % en Australie (Deutscher and Mazumder, 2020). De plus, la persistance au sommet devient de plus en plus forte à mesure que l'on se concentre sur l'extrémité droite de la distribution des revenus parentaux. Comme pour la RRC, l'exercice de validation suggère que ces estimations sont des bornes supérieures (inférieures) de la mobilité (persistance).

Nous montrons que nos résultats de référence sont robustes aux biais potentiels. Nous évaluons premièrement leur sensibilité à la spécification de l'équation de prédiction du revenu parental. En particulier, nous vérifions si la variation de la combinaison des prédicteurs, et l'utilisation de méthodes d'estimation non paramétriques, influencent nos résultats. Les estimations de l'IGE sont surestimées lorsqu'on utilise uniquement l'éducation comme prédicteur, tandis que la RRC et les matrices de transition restent étonnamment stables, quel que soit l'ensemble des prédicteurs utilisés. Nous observons une légère amélioration de la prédiction grâce à l'utilisation de modèles flexibles, qui ne modifie pas pour autant nos conclusions. De plus, nous évaluons la sensibilité de nos estimations aux biais de cycle de vie et d'atténuation en faisant varier les âges auxquels les revenus des enfants et des parents sont mesurés, ainsi que le nombre d'observations de revenus des parents utilisées. Nos résultats de référence ne semblent ni sous-estimer ni surestimer la mobilité intergénérationnelle en raison d'une mesure des revenus des enfants et/ou des parents prise trop tôt ou trop tard dans le cycle de vie, ni en raison d'une moyenne des revenus calculée sur trop peu d'années.

Résultats Infranationaux. Nous mettons en évidence des variations spatiales substantielles de la mobilité intergénérationnelle entre les départements, comparables à celles observées entre pays. Nous définissons le lieu de résidence des individus comme leur département de résidence en 1990, lorsqu'ils ont entre 9 et 18 ans. Les niveaux de mobilité les plus élevés sont généralement observés à l'Ouest de la France, tandis que les niveaux les plus faibles sont au Nord et au Sud. Alors que les IGE varient de 0,30 à 0,45 dans les départements bretons, ils varient de 0,42 à 0,70 dans les départements franciliens. La distribution des RRC au niveau départemental est plus resserrée que celle des IGE, mais présente des schémas spatiaux très similaires.

Nous caractérisons également la mobilité ascendante absolue (AUM) des départements, définie comme le rang de revenu attendu des enfants nés de parents au 25^{e} centile, et obtenue à partir des valeurs projetées sur la droite de régression rang-rang au niveau départemental (Chetty et al., 2014a). La mobilité ascendante absolue varie de 36,8 dans le Pas-de-Calais à 54,4 en Haute-Savoie. Le département de Paris se distingue en termes d'AUM (49,8) mais présente des niveaux de persistance intergénérationnelle autour de la moyenne en termes d'IGE (0,51) et de RRC (0,28). La corrélation inter-départementale entre l'IGE et la RRC n'est que de 0,65, et de −0,55 avec l'AUM. Cela souligne l'importance d'utiliser une variété de mesures de mobilité intergénérationnelle pour caractériser la persistance des revenus entre les générations au niveau national (Deutscher and Mazumder, forthcoming).

Comme première tentative pour comprendre les sources de ces variations inter-départementales de mobilité intergénérationnelle, nous proposons une simple analyse corrélationnelle. Nous constatons que la mobilité ascendante absolue présente généralement des relations beaucoup plus fortes avec les caractéristiques des départements que l'IGE ou la RRC. Cela suggère que les facteurs affectant la mobilité absolue pourraient différer de ceux affectant la mobilité relative. La seule caractéristique systématiquement corrélée négativement avec la mobilité intergénérationnelle est le taux de chômage. Étonnamment, nous ne trouvons aucune indication d'une « *Great Gatsby curve* » en France[4] en ce qui concerne l'IGE ou la RRC. Cela contraste avec les résultats obtenus pour d'autres pays (Acciari et al., 2022; Chetty et al., 2014a; Corak, 2020).

Enfin, nous réalisons une analyse descriptive de la relation entre mobilité intergénérationnelle de revenus et mobilité géographique. Nous documentons des gains importants en termes de rang de revenu attendu pour les personnes géographiquement mobiles, légèrement décroissants avec le rang de revenu parental. Parmi les enfants issus de familles dans le décile inférieur, les personnes qui déménagent ont un rang attendu environ 5,6 centiles supérieur à celui des sédentaires, tandis que cette différence est d'environ 4,4 centiles pour les enfants issus de familles dans le décile supérieur. Ces gains sont en partie attribuables au fait que les personnes qui déménagent s'installent dans des départements à revenus plus élevés à l'âge adulte par rapport aux sédentaires, mais aussi au fait que les personnes qui déménagent atteignent des rangs locaux dans leur département à l'âge adulte qui sont plus éloignés du rang de leurs parents dans leur département d'origine. Les départements de destination se caractérisent en moyenne par des niveaux de revenu plus élevés que les départements d'origine uniquement aux extrémités de la distribution des revenus parentaux. Cependant, quel que soit le rang de revenu parental, les gains en mobilité ascendante absolue associés au fait de déménager dans un département à revenu plus élevé sont élevés et augmentent avec le revenu moyen dans le département de destination. Ces résultats combinent à la fois des effets d'auto-sélection et des effets causaux, que nous laissons à de futures recherches le soin de démêler.

Chapitre II

Être né de parents natifs ou immigrés est une caractéristique innée qui a constamment été mise en avant comme l'un des principaux déterminants des résultats socio-économiques.

[4]La « *Great Gatsby curve* » fait référence à la corrélation positive entre la persistance intergénérationnelle des revenus (définie par l'IGE) et l'inégalité des revenus (définie par l'indice de Gini) observée entre pays (Corak, 2013).

En théorie, dans une société à faible mobilité intergénérationnelle, le simple fait que les parents immigrés tendent à gagner moins que les natifs affaiblit mécaniquement les perspectives économiques de leurs enfants. En pratique, dans la plupart des pays d'Europe de l'Ouest on observe que les enfants d'immigrés sont encore moins bien lotis que les enfants de natifs même à niveaux de revenus parentaux donnés. La discrimination à l'embauche a largement été documentée comme l'une des raisons de cet écart conditionnel par les études de *correspondence testing*, mais d'autres facteurs peuvent aussi entrer en jeu. Dans cette analyse, j'examine le rôle de la ségrégation résidentielle en tant que déterminant potentiel de la persistance de cet écart, plus facile à quantifier de manière exhaustive et systématique, et sur lequel il est donc plus facile d'agir pour les décideurs politiques.

Les résultats montrent que, dans la plupart des cas, la prise en compte de la position des parents dans la distribution des revenus comble l'écart de rang de revenu entre les enfants d'immigrés et les enfants de natifs. Les fils d'immigrés d'Afrique du Nord, cependant, restent résolument plus bas que les natifs dans la distribution des revenus, même au sein du même décile de revenus parentaux. Dans une approche par variable instrumentale, je montre que les fils d'immigrés d'Afrique du Nord sont également le seul groupe, en plus des enfants de natifs, à subir un effet négatif de la ségrégation résidentielle sur le rang de revenu conditionnel. Ces résultats enrichissent la littérature en fournissant des preuves de l'effet de la ségrégation résidentielle sur les différences de mobilité intergénérationnelle entre les enfants d'immigrés et de natifs dans un contexte culturel et historique d'immigration différent de celui des États-Unis, plus représentatif de la plupart des pays d'Europe occidentale.

Je mène l'analyse sur un jeu de données administratives français qui combinant les informations du recensement tout au long de la vie, celles des données fiscales et des données employeurs-employés. Cela me permet d'observer le pourcentage d'immigrés où les individus ont grandi, aussi précisément qu'au niveau du bâtiment, leur propre origine migratoire, ainsi que leurs revenus détaillés à l'âge adulte. Cette fusion de sources de données administratives est disponible pour un échantillon pseudo-aléatoire de 1 % de la population, rassemblant environ 85 000 individus pour les cohortes de naissance 1972-1984 considérées. En suivant la méthodologie développée dans le premier chapitre, j'estime le rang de revenu des parents en fonction de leurs caractéristiques socio-démographiques détaillées observées dans les données du recensement de l'enfance des individus. Les tests de robustesse suggèrent que les inexactitudes dans les prédictions de rang de revenu, ainsi que les biais de cycle de vie et d'atténuation, sont modérés et homogènes entre les groupes d'origine.

En premier lieu, je décris la convergence globale du rang de revenu avec les natifs entre les immigrés de première génération et leurs enfants. La sur-représentation des immigrés au bas de la distribution des revenus est beaucoup plus prononcée chez les parents nés en Afrique du Nord, dont 50 % appartiennent au premier décile. Les enfants d'immigrés, en revanche, sont répartis de manière beaucoup plus équilibrée dans la distribution des revenus, en particulier ceux ayant des origines sud-européennes, car ils ont complètement rattrapé les descendants de natifs. Cependant, un écart significatif d'environ 10 centiles est observé pour les enfants d'immigrés d'Afrique du Nord.

La comparaison des rangs de revenus au sein du même décile de revenus parentaux augmente systématiquement le rang de revenu moyen des enfants d'immigrés par rapport aux enfants de natifs. Les enfants d'immigrés sud-européens obtiennent de meilleurs résultats que les natifs, conditionnellement aux revenus des parents, d'où l'absence d'écart inconditionnel malgré des différences importantes dans les revenus des parents. L'écart entre les filles de natifs et les filles de parents nord-africains se comble complètement en comparant au sein des déciles de revenu, mais pour les fils, un écart de 4 centiles persiste. Ces résultats sont très similaires à ce que Chetty et al. (2020) a documenté entre « races » aux États-Unis : un écart noir-blanc chez les hommes mais pas chez les femmes, et des taux de mobilité intergénérationnelle beaucoup plus similaires aux blancs parmi les individus hispaniques.

Cet écart persistant entre les fils d'immigrés d'Afrique du Nord et les fils de natifs ne semble pas principalement s'expliquer par des différences dans les niveaux d'éducation. En effet, les taux de diplomation de l'enseignement supérieur tendent à être plus élevés pour les enfants d'immigrés, conditionnellement aux rangs de revenu des parents, indépendamment du sexe et du groupe d'origine. Pour mieux comprendre la nature de l'écart conditionnel de rang de revenu, je distingue ce qui provient de la rémunération en elle-même de ce qui provient du nombre d'heures travaillées via deux spécifications alternatives. Premièrement, lorsque j'exclus les revenus non positifs, l'écart de rang de revenu conditionnel entre les enfants de natifs et les enfants d'immigrés d'Afrique du Nord se réduit. Deuxièmement, en termes de salaire horaire, il disparaît complètement. Cela suggère que l'écart restant provient de différences d'accès au marché du travail, tant à la marge extensive qu'intensive, malgré des niveaux d'éducation conditionnelle plus élevés en moyenne que ceux des natifs.

Je me concentre sur la ségrégation résidentielle comme facteur potentiel de cette réduction de l'accès au marché du travail. Je mesure les proportions d'immigrés dans les zones de résidence des individus à partir du recensement exhaustif de la population, que j'apparie ensuite au niveau de l'unité urbaine, du quartier et du bâtiment. La part moyenne d'immigrés dans le bâtiment de résidence est de 8 % pour les enfants de natifs, et de 26 % pour les enfants d'immigrés. La force de la relation entre le pourcentage d'immigrés à l'échelle locale et le rang de revenu des individus conditionnellement au rang des parents varie selon le niveau géographique considéré. C'est au niveau du quartier que cette relation est la plus fortement et systématiquement négative.

Je mesure la ségrégation résidentielle dans les unités urbaines à l'aide de l'indice de dissimilarité de Duncan, qui capture à quel point les immigrés sont inégalement répartis entre les quartiers. La ségrégation résidentielle tend à être plus faible dans le Sud et dans l'Est, et plus élevée dans le Nord et dans l'Ouest. Pour avoir une idée naïve du rôle joué par la ségrégation résidentielle dans la relation négative entre la part locale d'immigrés et le rang de revenu conditionnel, j'examine leurs relations conjointes. L'interaction entre la part d'immigrés et la ségrégation absorbe l'intégralité de l'effet négatif, laissant supposer que la ségrégation pourrait en fait être à l'origine la relation négative initialement attribuée à la proportion d'immigrés.

Pour isoler la composante causale de cette relation, j'applique une stratégie de variable instrumentale adaptée de Chyn et al. (2022), basée sur la manière dont les barrières géographiques divisent les unités urbaines. Plus précisément, je considère la combinaison des voies navigables, des routes et des chemins de fer pour calculer un indice capturant comment la superficie des unités urbaines est inégalement répartie entre les sous-unités générées par ces obstacles, tout en contrôlant leur longueur dans l'unité urbaine. L'idée sous-jacente est que, conditionnellement à la densité de ces éléments dans une unité urbaine donnée, la manière dont ils divisent l'espace peut favoriser ou non la ségrégation résidentielle sans affecter par ailleurs les perspectives de mobilité intergénérationnelle des enfants qui y résident.

Les résultats documentent un effet négatif de la ségrégation résidentielle sur le rang de revenu conditionnel au rang des parents, à la fois pour les fils de natifs et pour les fils d'immigrés d'Afrique du Nord. Une augmentation d'un écart-type de la ségrégation résidentielle réduit de 0,68 centile l'effet d'une augmentation d'un point de pourcentage de la proportion d'immigrés à l'échelle locale sur le rang de revenus des individus pour les fils de natifs, conditionnellement au rang des parents, en moyenne. Pour les fils d'immigrés d'Afrique du Nord, cet effet atteint $-1{,}9$ centiles de revenu. Aucun effet significatif n'est observé pour les enfants d'origine sud-européenne, et un effet plus faible et moins significatif est constaté pour les filles de natifs.

L'hypothèse sur laquelle reposent ces interprétations est que la manière dont les barrières géographiques divisent l'espace en plus ou moins de sous-unités n'a pas d'effet sur la mobilité intergénérationnelle autre que par la ségrégation résidentielle. La validité de cette hypothèse est remise en question par le fait que certaines des barrières considérées constituent des réseaux de transport, qui peuvent eux-mêmes favoriser la mobilité intergénérationnelle, comme cela a été démontré pour l'Argentine (Pérez, 2018) et pour l'Angleterre et le Pays de Galles (Costas-Fernández et al., 2020). En plus de contrôler pour la longueur de chaque élément dans les unités urbaines, je construis un indice capturant la mesure dans laquelle le réseau ferroviaire offre des opportunités sur le marché du travail en utilisant les salaires agrégés dans les unités urbaines directement reliées par le réseau ferroviaire, pondérés par leurs distances bilatérales. Les résultats sont robustes à l'inclusion de cet indice.

Ce chapitre contribue à deux principaux pans de la littérature. Premièrement, il se rapporte à la littérature sur la mobilité intergénérationnelle et l'intégration socio-économique des immigrés et de leurs enfants. Les perspectives de mobilité intergénérationnelle des enfants d'immigrés sont souvent moins favorables que celles des enfants de natifs. Cela a notamment été documenté pour l'Estonie (Kivi et al., 2021), les Pays-Bas (Van Elk et al., 2024), et la Suède (Bratu and Bolotnyy, 2023), mais pas pour le Danemark (Jensen and Manning, 2024) ni pour les États-Unis (Abramitzky et al., 2021). Pourtant, Mazumder (2014) et Chetty et al. (2020) trouvent une hétérogénéité de mobilité intergénérationnelle significative selon la« race » aux États-Unis. Comme il est beaucoup moins courant d'avoir des informations raciales dans les bases de données européennes, et en raison des contextes historiques d'immigration différents, il convient de garder à l'esprit que les variations dans le chevauchement entre race et origine immigrée récente peuvent mettre en péril les comparaisons internationales.

En France spécifiquement, la mobilité intergénérationnelle des immigrés de deuxième génération a notamment été étudiée sur la base de l'enquête Trajectoires & Origines menée par l'Insee et l'Ined (Beauchemin et al., 2016). En particulier, Beauchemin (2018) identifie l'origine migratoire comme un facteur clé d'hétérogénéité de mobilité socio-économique intergénérationnelle, et montre que les fils d'immigrés d'Afrique du Nord sont particulièrement désavantagés. Achard (2024) montre également que pour les enfants d'immigrés, les caractéristiques des parents sont moins prédictives que celles des grands-parents en raison du déclassement socio-économique transitoire des immigrés de première génération. Dans cette étude, je complète ces résultats en utilisant des données administratives pour documenter de manière plus détaillée les perspectives de mobilité intergénérationnelle des enfants d'immigrés et de natifs le long de la distribution des revenus parentaux.

Deuxièmement, ce chapitre contribue à la littérature sur la ségrégation résidentielle et les effets de quartier. Aux États-Unis, Andrews et al. (2017) montrent que la ségrégation raciale passée explique une partie importante de la variation de la mobilité intergénérationnelle documentée par Chetty et al. (2014b). En France, Weber et al. (2024) documentent un désavantage de quartier persistant chez les descendants d'immigrés non européens, et McAvay and Safi (2018) révèlent un risque plus élevé de désavantage spatial cumulatif pour les immigrés nord-africains et sub-sahariens. Concernant les effets de quartier, Hémet and Malgouyres (2018) montrent notamment qu'en France, la diversité au niveau du quartier en termes d'origine des parents est moins importante que la diversité en termes de nationalité pour les perspectives d'emploi. C'est dans ce contexte que ce chapitre entreprend de quantifier l'effet causal de la ségrégation résidentielle sur les différences de mobilité intergénérationnelle entre origines.

Chapitre III

L'amplification continue des flux migratoires soulève des questions cruciales sur les politiques d'intégration et de régulation les plus adaptées à mettre en œuvre. La naturalisation, en tant que dernière étape du point de vue légal du processus d'intégration pour les migrants, a toujours été un point central de ce débat. D'une part, la naturalisation peut être perçue comme un outil favorisant l'intégration des migrants. D'autre part, elle est perçue comme la récompense d'une intégration réussie. C'est ce dernier point de vue qui guide actuellement l'approche dominante en matière de politiques publiques. Les pays d'accueil imposent des coûts substantiels à l'acquisition de la citoyenneté afin de sélectionner les immigrés selon certaines caractéristiques.

Cependant, les analyses causales existantes restent limitées quant à la manière dont les migrants réagissent aux changements de coût de la naturalisation, ainsi que l'impact de la naturalisation sur l'intégration des descendants d'immigrés sur marché du travail. Cela s'explique par trois problématiques principales. Premièrement, la décision de naturalisation est endogène, engendrant un biais de sélection. Deuxièmement, les chocs exogènes dans les études existantes concernent souvent des cohortes encore trop jeunes pour que leurs résultats sur le marché du travail puissent être étudiés. Troisièmement, les études sur l'acquisition de la citoyenneté pour les immigrés de deuxième génération, qui reposent dans la plupart des cas sur des réformes affectant les individus tôt dans l'enfance, ne peuvent pas toujours distinguer clairement ses effets sur l'éducation de ses effets directs sur le marché du travail.

Dans ce chapitre, nous répondons à ces problématiques en nous appuyant sur deux aspects clés du contexte français. Premièrement, les individus nés en France de parents étrangers n'ont quasiment aucun coût à acquérir la citoyenneté française à 18 ans. Deuxièmement, dans les années 1990, le service militaire obligatoire rendait la naturalisation coûteuse pour les hommes étrangers. Associées entre elles, ces deux caractéristiques ont induit un arbitrage explicite à l'âge de 18 ans pour les hommes descendant d'immigrés, entre renoncer à la citoyenneté française ou bien effectuer leur service militaire. Dans ce contexte, nous exploitons l'abolition du service militaire obligatoire en 1997 pour les hommes nés après 1978 en tant que choc exogène dans le coût d'acquisition de la citoyenneté française pour les descendants d'immigrés. Nous utilisons des données administratives ainsi que des données d'enquête pour explorer comment cette réforme a affecté les comportements de naturalisation, ainsi que les répercussions potentielles sur les résultats sur le marché du travail.

Nos résultats montrent que l'abolition du service militaire a induit une forte augmentation des taux de naturalisation chez les hommes par rapport aux femmes. Cet effet est entièrement porté par les citoyens de l'Union Européenne (UE), le groupe d'immigrés de deuxième génération pour lequel l'acquisition de la citoyenneté française devrait théoriquement être la moins importante. Néanmoins, nous constatons que cette augmentation de la naturalisation a aussi induit une hausse significative de 1,7 point de pourcentage du taux l'emploi pour ce groupe.

Notre cadre d'analyse permet d'éliminer mécaniquement trois canaux explicatifs potentiels de cet effet. En effet, le fait que les *compliers* soient des citoyens de l'UE garantit que l'effet sur l'emploi ne découle ni du droit de résidence et de travail dans le pays d'accueil, ni de l'accès aux prestations sociales, ni de la stabilité accordée par la citoyenneté. Deux des mécanismes traditionnellement avancés par la littérature restent pertinents pour ce groupe : les barrières à l'entrée sur le marché du travail et la discrimination.

Nous montrons que l'augmentation de l'emploi s'accompagne d'une plus forte proportion d'emplois publics et d'une diminution de l'auto-emploi, soutenant ainsi l'hypothèse de l'accès au marché du travail. Nous supposons également que la discrimination à l'embauche contribue à cet effet, car nous observons des proportions significativement positives d'auto-déclaration de discrimination et de racisme, même parmi les immigrés de deuxième génération d'origine européenne. Nos tests de robustesse nous permettent également d'exclure la possibilité que ces effets soient directement dus à l'impact du service militaire.

Pour rationaliser la décision de naturalisation, nous proposons un cadre théorique au sein duquel les individus acquièrent la citoyenneté tant que les bénéfices dépassent les coûts. Nous considérons une fonction de coût qui décroit avec les compétences, ce qui est généralement le cas sous des politiques publiques telles que les tests de langue ou bien les conditions financières. Le modèle prédit que si les bénéfices sont homogènes, de tels coûts conduiraient à une auto-sélection des individus les plus qualifiés. Cependant, si les bénéfices sont hétérogènes, ces coûts peuvent exclure du processus les individus qualifiés avec de faibles bénéfices potentiels. Ce scénario s'applique aux contextes où les groupes les plus discriminés sur le marché du travail sont également les moins éduqués.

Dans le contexte français, nous nous attendons à ce que les citoyens de l'UE bénéficient le moins de la citoyenneté. En effet, contrairement aux individus d'autres nationalités de naissance, ils peuvent librement travailler et résider en France. Ainsi, ils sont les moins susceptibles de se naturaliser lorsqu'un service militaire obligatoire est en place, et les plus susceptibles de réagir à son abolition. En revanche, nous nous attendons à ce que les individus de nationalités typiquement discriminées sur le marché du travail, telles que les nationalités africaines, bénéficient le plus de la citoyenneté. Les coûts sont aussi probablement hétérogènes, car les jeunes hommes peu qualifiés étaient généralement affectés aux postes les plus pénibles (Maurin and Xenogiani, 2007). En conséquence, nous nous attendons à ce que l'abolition du service militaire obligatoire ait un impact plus important sur l'acquisition de la citoyenneté pour les individus peu éduqués à groupe de nationalité de naissance donné.

Pour tester ces hypothèses empiriquement, nous exploitons le fait que les femmes étaient exemptées du service militaire obligatoire, et qu'elles n'ont donc pas été affectées par son abolition. En utilisant une approche par différence-de-différences, nous comparons le taux de naturalisation des hommes et des femmes étrangers nés en France avant et après le 31 décembre 1978. Nous constatons qu'à l'abolition du service militaire, le taux de naturalisation des hommes est passé de 68,5 % à 78,9 %, tandis que celui des femmes est resté stable autour de 84 %. Cela suggère qu'environ un quart du déficit de naturalisation chez les jeunes hommes était dû au service militaire, et que son abolition a réduit de moitié l'écart avec les femmes.

Conformément à notre cadre théorique, les résultats montrent que cet effet est entièrement porté par les citoyens de l'UE à la naissance, pour lesquels les bénéfices de l'acquisition de la citoyenneté française sont moins élevés. Au sein de ce groupe, l'abolition de la conscription obligatoire a augmenté les taux de naturalisation de 11,9 points de pourcentage chez les hommes. Aucun effet significatif n'est observé pour d'autres nationalités de naissance, pour lesquelles le coût du service militaire n'est donc pas contraignant. Parmi les citoyens de l'UE, nous constatons que l'augmentation des taux de naturalisation est plus de 50 % plus importante pour les individus peu éduqués par rapport aux individus plus éduqués, soutenant ainsi l'hypothèse selon laquelle le coût du service militaire est moindre pour ces derniers.

Nous exploitons ensuite le fait que seuls les hommes citoyens de l'UE ont bénéficié d'une augmentation de la naturalisation pour étudier son effet sur leurs résultats sur le marché du travail. Plus précisément, nous utilisons chaque groupe non affecté dans une approche de double différence synthétique afin de mieux capturer l'évolution des résultats des hommes citoyens UE telle qu'elle aurait été en l'absence de l'abolition de la conscription obligatoire. Le groupe de contrôle synthétique reflète étroitement la tendance du taux d'emploi des hommes citoyens UE jusqu'à la réforme, après quoi le taux d'emploi dans le groupe traité dévie de sa trajectoire avec une augmentation de 1,7 point de pourcentage. Étant donné que cet effet est porté par 11,9 % des hommes citoyens UE qui ont réagi à la réforme, cela correspond à une augmentation de 14,5 points de pourcentage parmi les *compliers*. Nous montrons que cet effet positif sur l'emploi est principalement attribuable à une diminution de l'inactivité plutôt que du chômage.

Nous étudions deux mécanismes potentiels expliquant ces résultats. Premièrement, nous documentons une augmentation significative de la probabilité d'être employé dans le secteur public. Deuxièmement, nous constatons une diminution de l'auto-emploi chez les hommes citoyens UE par rapport aux groupes de contrôle, conformément à l'idée que l'acquisition de la citoyenneté élargit les opportunités sur le marché du travail pour les individus naturalisés.

Nous effectuons des tests de sensibilité démontrant que nos résultats sont robustes au choix des groupes de contrôle de la double différence synthétique, aux effets d'anticipation, aux effets d'équilibre général, à une attrition différentielle, et à la durée relative du service militaire dans le pays de nationalité d'origine. Aussi, nous évaluons le biais potentiel lié au fait que le service militaire pourrait directement affecter les perspectives d'éducation et sur le marché du travail. Dans le contexte français il a été montré que le service militaire n'avait soit aucun impact, soit un impact positif sur les résultats éducatifs, en adéquation avec les stratégies de dispense du service militaire, et sur les résultats professionnels (Maurin and Xenogiani, 2007; Mouganie, 2020). Nos résultats suggèrent que les niveaux d'éducation des descendants d'immigrés n'ont pas été impactés par le service militaire. Nous excluons également un impact direct potentiel sur les résultats professionnels étant donné l'absence d'effets sur le marché du travail pour les groupes de nationalité de naissance qui ont uniquement connu l'abolition du service militaire sans variations de taux de naturalisation. En outre, Mouganie (2020) documente qu'en France, le service militaire n'a eu aucun effet ou alors des effets positifs sur les résultats sur le marché du travail. Compte tenu de ces facteurs, notre étude pourrait, au mieux, sous-estimer l'impact réel de la naturalisation sur le marché du travail.

Ce chapitre contribue à trois différentes branches de la littérature. Premièrement, il offre un éclairage sur les effets de la naturalisation sur l'intégration sur le marché du travail des migrants de deuxième génération. La littérature associée s'est largement concentrée sur la première génération, établissant une corrélation positive entre la naturalisation et les résultats sur le marché du travail, à commencer par les travaux de Chiswick (1978). Une branche émergente de cette littérature a exploré le lien causal entre les deux, pour les immigrants et les réfugiés de première génération (Gathmann and Keller, 2018; Hainmueller et al., 2019; Govind, 2021; Fasani et al., 2023; Hainmueller et al., 2023). Nous contribuons à cette littérature en étudiant l'intégration professionnelle des immigrés de deuxième génération, touchant à la littérature sur le droit du sol qui s'est jusqu'à présent concentrée sur le niveau d'éducation (Felfe et al., 2020, 2021; Dahl et al., 2022). Nos résultats démontrent que même les populations qui pourraient avoir moins à gagner à se naturaliser, ici les descendants d'immigrés citoyens de l'UE, bénéficient d'une meilleure intégration économique grâce à la naturalisation.

Deuxièmement, ce chapitre contribue à la littérature sur l'acquisition de la citoyenneté. Diverses études ont exploré l'association entre la propension à se naturaliser et les caractéristiques des individus ou des pays d'origine, telles que l'âge à la migration, le genre, le niveau d'éducation et les conditions politiques dans le pays d'origine (Yang 1994; Chiswick and Miller 2009; Fougère and Safi 2009. Voir Gathmann and Garbers (2023) pour une revue détaillée de la littérature). De plus, les coûts d'acquisition de la citoyenneté, tels que les exigences de connaissances civiques, les frais de naturalisation et les restrictions à la double nationalité, ont montré qu'ils affectent directement l'acquisition, en particulier pour les individus peu éduqués et les citoyens de l'UE (Yasenov et al., 2019; Peters and Vink, 2023; Vink et al., 2021). Ce chapitre contribue à la littérature existante en formalisant l'acquisition de la citoyenneté dans un cadre théorique coût-bénéfice. Nous discutons des implications involontaires de l'augmentation des coûts de la naturalisation face à des bénéfices hétérogènes, faisant écho à des résultats récents de *backlash* potentiel des politiques d'intégration (Fouka, 2020; Dahl et al., 2022; Arendt et al., forthcoming).

Troisièmement, nous contribuons à la littérature sur les effets du service militaire. Les recherches existantes se sont principalement concentrées sur l'impact du service militaire sur des variables telles que l'éducation, l'emploi, les revenus, le comportement politique et la criminalité (par exemple, Angrist 1990; Bauer et al. 2012; Card and Cardoso 2012; Hubers and Webbink 2015; Hjalmarsson and Lindquist 2019; Savcic et al. 2023, et plus spécifiquement sur la France : Maurin and Xenogiani 2007; Fize and Louis-Sidois 2020; Mouganie 2020). À notre connaissance, nous sommes les premiers à étudier les effets du service militaire sur les non-citoyens, et plus spécifiquement ses implications sur leurs décisions de naturalisation.

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Chapter 1

Intergenerational income mobility in France: A comparative and geographic analysis

This chapter is based on a paper co-authored with Gustave Kenedi (London School of Economics).

Abstract

We provide new estimates of intergenerational income mobility in France for children born in the 1970s using rich administrative data. Since parents' incomes are not observed, we employ a two-sample two-stage least squares estimation. We show, using the Panel Study of Income Dynamics, that this method slightly underestimates rank-based measures of intergenerational persistence. Our results suggest that France is characterized by a strong persistence relative to other developed countries. 9.7% of children born to parents in the bottom 20% reach the top 20% in adulthood, four times less than children from the top 20%. We uncover substantial spatial variations in intergenerational mobility across departments, and a positive relationship between geographic mobility and intergenerational upward mobility. The expected income rank of individuals from the bottom of the parent income distribution who moved towards high-income departments is around the same as the expected income rank of individuals from the 75$^{\text{th}}$ percentile who stayed in their childhood department.

1.1 Introduction

To what extent is the income of individuals related to that of their parents? This question has seen renewed interest both in the general public and in academia as rising income inequality raised concerns about equality of opportunity. Examining this link is essential to understand whether children from different socio-economic backgrounds are afforded the same opportunities. It also matters for economic efficiency, as high persistence across generations may reflect an inefficient allocation of talents (so-called "Lost Einsteins"). Intergenerational persistence has now been estimated for a large number of countries, paving the way for insightful cross-country comparisons. Yet, much remains to be known for France, a country with relatively modest post-tax/transfers income inequality in international comparison and largely inexpensive higher education tuition fees.

The few existing studies for France only estimate the traditional intergenerational income elasticity (IGE), which captures the elasticity of child income with respect to parent income, and are based on small-sample surveys with self-reported incomes (Lefranc and Trannoy, 2005; Lefranc, 2018). Using a large sample combining census and tax returns data, we estimate two additional measures of intergenerational mobility: (i) the rank-rank correlation (RRC), increasingly prominent in the literature, which corresponds to the correlation between child and parent income percentile ranks, and (ii) transition matrices, which capture finer mobility patterns along the parent income distribution. While previous studies on France used self-reported labor earnings, we focus on household-level income measures. They provide a better depiction of one's economic resources and allow the inclusion of children raised by single mothers. Integrating these improvements from the "new" intergenerational mobility literature enables us to conduct a detailed international comparison to rank France relative to other advanced economies for which comparable estimates are available.

In addition, we investigate the spatial variations in intergenerational mobility across the 96 metropolitan French departments. Such subnational analyses, pioneered by Chetty et al. (2014), help shed light on the mechanisms that may underlie income persistence across generations. Importantly, they highlight that national level estimates provide an incomplete assessment of a country's intergenerational mobility. We make use of the panel dimension of our data to describe the geographic mobility patterns of individuals and study the relationship between geographic mobility and intergenerational mobility. We investigate the separate roles of moving to a higher-income department from that of climbing the income ladder within departments, conditional on parent income rank.

Our analysis is conducted on almost 65,000 children born between 1972 and 1981, and observed in the Permanent Demographic Sample (EDP). This rich administrative dataset allows us to implement the contributions discussed above and to convincingly address concerns related to lifecycle and attenuation bias (Haider and Solon, 2006; Black and Devereux, 2011; Nybom and Stuhler, 2017). Since parents' incomes are not observed, we use a two-sample two-stage least squares (TSTSLS) estimation which consists in predicting parents' incomes using other parents drawn from the same population but for whom income is observed (Björklund and Jäntti, 1997). This method has been employed previously to estimate the IGE in the French context (Lefranc and Trannoy, 2005; Lefranc, 2018) as well as in many other countries (Jerrim et al., 2016, Table A1).

While studies typically use education and/or occupation to predict parent income, we make use of the richness of our data to also include detailed demographic characteristics of parents (French nationality dummy, country of birth, household structure, and birth cohort), and characteristics of the municipality of residence (unemployment rate, share of single mothers, share of foreigners, population, and population density). Our results are largely insensitive to the set of predictors. Parent income is then defined as the average[1] of father and mother predicted mean pretax wage over ages 35-45, and child income as pretax household income averaged over the same age range between 2010 and 2016. These two income definitions represent the most comprehensive household-level income definitions available for either generation.

TSTSLS validation exercise. Using the United States' Panel Study of Income Dynamics (PSID), we find that TSTSLS slightly underestimates rank-based measures of intergenerational persistence relative to what would be obtained if parent income were observed (OLS). The downward bias relative to the OLS estimate for the RRC ranges from 11% when education is the only predictor, to around 3-5% once occupation is also included. Subnational TSTSLS estimates are also fairly close to their OLS counterparts, though they tend to deviate more when the number of observations is small. Our results highlight that in settings like ours, where parent income cannot be directly observed, rank-based measures of intergenerational mobility obtained with TSTSLS likely provide lower bounds that are reasonably close to the true estimates. These findings confirm those obtained in different settings and samples by Cortes-Orihuela et al. (2022) and Jacome et al. (2023). We find that this reasoning also applies to the transition matrix.

National results. Our main finding is that France exhibits relatively strong intergenerational income persistence compared to other developed countries. Our baseline estimate of the intergenerational elasticity in household income is 0.527, suggesting that on average, a 10% increase in parent income is associated with a 5.27% increase in child income. Put differently, if one's parents earn 10% more than the average of parents' incomes, then one is expected to preserve about 50% of that relative advantage. This estimate should be interpreted with caution considering our validation exercise suggests the TSTSLS IGE is significantly greater than the true estimate. Applying the correction factor we find, the IGE decreases to 0.396.

Moving to the rank-rank relationship, we find that the conditional expectation of child

[1] See Section 1.3.3 for an explanation for why we take the *average* rather than the *sum*.

income percentile rank with respect to parent income percentile rank is linear throughout most of the parent income distribution, with steeper relationships at the tails. Our baseline estimate of the rank-rank correlation is 0.303, implying that a 10 percentile increase in parent income rank is associated, on average, with a 3.03 percentile increase in child income rank. This estimate is of similar magnitude to that found for Italy (0.3; Acciari et al. (2022)), somewhat smaller than for the United States (0.341; Chetty et al. (2014)), and markedly greater than existing estimates for other advanced economies such as Sweden (0.197; Heidrich (2017)), Australia (0.215; Deutscher and Mazumder (2020)) or Canada (0.242; Corak (2020)). Applying the correction factor we find in the validation exercise gives an RRC of 0.314 which does not affect France's relative position.

Intergenerational persistence, as captured by the transition matrix, is strongest at the tails of the parent income distribution: 9.7% of children from the bottom 20% of the parent income distribution reach the top 20% as adults. This probability is almost 4 times greater for children born to parents in the top 20% (38.4%). In comparison, the probability for a child born to a family in the bottom 20% to reach the top 20% in adulthood is 7.5% in the United States (Chetty et al., 2014) and 12.3% in Australia (Deutscher and Mazumder, 2020). Moreover, persistence at the top becomes stronger and stronger as we zoom in on the right tail of the parent income distribution. As with the RRC, the validation exercise suggests these estimates represent upper (lower) bounds on mobility (persistence).

We show that our baseline results are robust to potential biases. Foremost, we evaluate how sensitive they are to the parent income prediction specification. In particular, we check whether varying the set of predictors or using non-parametric estimation methods influences our estimates. IGE estimates are overinflated when using only education as a predictor, while the RRC and transition matrices remain surprisingly stable regardless of the set of predictors used. Slightly improved prediction from using flexible models does not quantitatively alter our estimates. Moreover, we assess our estimates' sensitivity to the lifecycle and attenuation biases by varying the ages at which child and parent incomes are measured as well as the number of parent income observations used. Our baseline results do not appear to under- nor over-estimate intergenerational mobility due to measuring child and/or parent incomes too early or too late in the lifecycle nor because of averaging incomes over too few years.

Subnational results. We uncover substantial spatial variations in intergenerational mobility across departments, comparable to those observed across countries. We define individuals' location as their department of residence in 1990, when they are between 9 and 18 years old. Higher levels of mobility are typically found in the West of France, and lower levels in the North and South. While the IGEs range from 0.30 to 0.45 in departments in Brittany (West), they range from 0.42 to 0.70 in departments in Hauts-de-France (North). The distribution of department-level RRCs is tighter than that of IGEs, but displays very similar spatial patterns.

We also characterize departments' absolute upward mobility (AUM), defined as the expected income rank of children born to parents at the 25th percentile, which is obtained from the fitted values of the department-level rank-rank regression (Chetty et al., 2014). Absolute upward mobility ranges from the 36.8 in Pas-de-Calais (North) to 54.4 in Haute-Savoie (East). The Paris department stands out in terms of AUM (49.8) but exhibits around average intergenerational persistence levels in terms of IGE (0.51) and RRC (0.28). The cross-department correlation between the IGE and RRC is only 0.65, and −0.55 with AUM. This highlights the importance of using a variety of intergenerational mobility measures to characterize a country's income persistence across generations (Deutscher and Mazumder, forthcoming).

As a first step to understand the sources underlying these cross-department variations in intergenerational mobility, we undertake a simple correlational analysis. We find that absolute upward mobility exhibits much stronger relationships with department characteristics in general, than either the IGE or the RRC. This suggests that factors that affect absolute mobility might differ from those that affect relative mobility. The only characteristic consistently negatively correlated with intergenerational mobility is the unemployment rate. Intriguingly, we find no evidence of a within France "Great Gatsby Curve"[2] with respect to the IGE nor the RRC. This contrasts with findings from other countries (Acciari et al., 2022; Chetty et al., 2014; Corak, 2020).

Lastly, we conduct a descriptive analysis of the relationship between intergenerational income mobility and geographic mobility. We document important gains in expected income rank for movers, which are slightly decreasing in parent income rank. For children from families in the bottom decile, movers have an expected rank approximately 5.6 percentiles greater than stayers, while this difference is of roughly 4.4 percentiles for children from families in the top decile. These gains are partly attributable to movers locating in higher-income departments in adulthood relative to stayers, but also to movers reaching local ranks in their adulthood department that are further away from the rank of their parents in the childhood department. Destination departments are on average characterized by higher income levels than origin departments only at the tails of the parent income distribution. However, regardless of parent income rank, the absolute upward mobility gains associated with moving to a higher-income department appear to be large and increasing with average income in the destination department. All these findings combine self-selection and causal effects, and we leave the disentangling of these two channels for future research.

The rest of the article is organized as follows. Section 1.2 describes the intergenerational income mobility measures we estimate and the main sources of bias they are subject to. The data, the parent income prediction procedure and validation exercise, and the sample and variable definitions are presented in Section 1.3. Section 1.4 reports our baseline estimates at the national level, while Section 1.5 assesses their robustness to various sources of bias. In

[2]The "Great Gatsby Curve" refers to the positive correlation between intergenerational income persistence (defined by the IGE) and income inequality (defined by the Gini index) found across countries (Corak, 2013).

Section 1.6, we investigate the spatial variations in intergenerational income mobility, their correlation with local characteristics, and describe the relationship between geographic and intergenerational mobility. Section 1.7 concludes.

1.2 Measuring intergenerational mobility

Intergenerational income mobility can be characterized using a variety of statistics.[3] In this section we (i) describe the statistics we employ, and (ii) discuss the two major biases inherent to most intergenerational persistence estimators, namely lifecycle bias and attenuation bias.

1.2.1 Main measures

Intergenerational persistence measures primarily aim to characterize the joint distribution of children and their parents' lifetime incomes with a parsimonious set of practical statistics. We summarize intergenerational persistence using the following statistics.

Intergenerational income elasticity (IGE). The traditional intergenerational income elasticity is obtained by regressing children's log lifetime income on their parents' log lifetime income. An IGE of 0.4 implies that a 10% increase in parent income is associated, on average, with a 4% increase in child income. Importantly, this estimator is sensitive to differences in inequality across generations. This can be seen in the following equation, where y_p and y_c are parent and child log lifetime incomes:

$$\text{IGE} = \frac{\text{Cov}(y_c, y_p)}{\text{Var}(y_p)} = \text{Corr}(y_c, y_p) \times \frac{\text{SD}(y_c)}{\text{SD}(y_p)}. \tag{1.1}$$

The empirical literature has highlighted that IGEs are particularly sensitive to lifecycle and attenuation biases, sample selection criteria, non-linearities along the parent income distribution, income definitions, and to the treatment of negative/zero incomes (Couch and Lillard, 1998; Chetty et al., 2014; Landersø and Heckman, 2017; Helsø, 2021).

Rank-rank correlation (RRC). The increasingly popular rank-rank correlation is obtained by regressing children's percentile rank in lifetime income on their parents' percentile rank in lifetime income. A RRC of 0.4 means that a 10 percentile increase in parent rank is associated, on average, with a 4 percentile increase in child rank. Unlike the IGE, the RRC is unaffected by inequality levels in either generation. This can be seen in the following equation, where p_p and p_c are parent and child percentile ranks in their respective lifetime income distributions:

[3] See for example Corak (2020), where nine statistics of intergenerational mobility are put into perspective. More elaborate discussions on the properties of the different intergenerational mobility estimators can also be found in Black and Devereux (2011), Chetty et al. (2014), Nybom and Stuhler (2017), and Deutscher and Mazumder (forthcoming).

$$\text{RRC} = \frac{\text{Cov}(p_c, p_p)}{\text{Var}(p_p)} = \text{Corr}(p_c, p_p) \times \frac{\text{SD}(p_c)}{\text{SD}(p_p)} = \text{Corr}(p_c, p_p). \tag{1.2}$$

Consequently, the greater the degree of inequality in the child generation relative to the parent generation, the greater the IGE relative to the RRC. In addition, the same RRC in two countries with large differences in inequality would hide that in one country the distance between ranks in monetary terms is actually much larger than in the other. The RRC owes its recent popularity to its robustness to specification variations, common biases, and treatment of negative/zero incomes (Dahl and DeLeire, 2008; Chetty et al., 2014; Nybom and Stuhler, 2017).

Transition matrices. To get a finer picture, one can use transition matrices, which report the probability of ending up in a given quantile as an adult conditional on coming from a family in a given quantile. Typically, they are reported by quintile and are of particular interest to seize non-linearities in children mobility along the parent income distribution.

1.2.2 Main sources of bias

The vast majority of currently available data sources do not cover the whole lifetime of children's and/or parents' incomes, leading researchers to approximate lifetime income based on shorter time spans. This data limitation generates the following two fundamental biases, which we extensively investigate in Section 1.5.

Attenuation bias. A direct implication of relying on a limited number of income observations to approximate parent lifetime income is the attenuation bias arising from classical measurement error (Solon, 1992; Zimmerman, 1992). This leads to downward-biased estimates of intergenerational persistence. Mazumder (2005, 2016) and Nybom and Stuhler (2017) find that the attenuation bias can be very large for the IGE but affects the RRC only mildly, while O'Neill et al. (2007) show that it affects most the corner elements of the transition matrix. The common solution to lessen this bias is to average parent income over as many years as possible.

Lifecycle bias. The second common bias relates to the age at which child and parent incomes are observed (Grawe, 2006; Haider and Solon, 2006). In particular, lifecycle bias arises in the presence of heterogeneous age-income profiles, which is observed empirically as high lifetime income individuals tend to experience steeper earnings profiles than low lifetime income individuals. As such, observing child or parent incomes either too early or too late in the lifetime is likely to bias intergenerational persistence estimates. The IGE is particularly sensitive to lifecycle bias, especially if incomes are measured before age 35, while it affects the RRC only moderately so long as incomes are measured at least in the late 20s/early 30s. Just as for the attenuation bias, the corner elements of the transition matrix are most sensitive to lifecycle bias (Chetty et al., 2014; Nybom and Stuhler, 2016, 2017).

1.3 Data

We use data from the Permanent Demographic Sample (EDP), which combines several administrative data sources on individuals born on the first four days of October. We refer to individuals born on one of these days as *EDP individuals*. We describe below the most relevant details for each data source we use and provide additional technicalities in Appendix A.1.

Civil Registers. They contain information from birth certificates of EDP individuals and their children, including gender, date and place of birth, and parents' date and place of birth, nationality and occupation.

1990 Census. It contains socio-demographic information about EDP individuals and members of their household. Importantly, it reports parents' education level, occupation, and other demographic characteristics if EDP individuals live with their parents in 1990.

All Employee Panel. It gathers worker-year level information on all private (since 1967) and public (since 1988) sector employees in metropolitan France, except those in the agricultural sector. Prior to 2001, only individuals born on an even year are covered. Our results are robust to the late coverage of civil servants (see Appendix A.3.1).

Tax Returns. They provide tax information on incomes earned between 2010 and 2016 for individuals in dwellings where an EDP individual is known either from their income tax form or their main housing tax. Income variables are available both at the household level and at the individual level. An advantage of the information being gathered at the dwelling level is that household income is observed for all couples, regardless of whether they file their taxes jointly.

1.3.1 Parent income prediction

The measures of intergenerational mobility laid out in Section 1.2.1 cannot be estimated directly with our data since we do not observe parents' incomes. We therefore rely on the two-sample two-stage least squares (TSTSLS) strategy introduced by Björklund and Jäntti (1997), and previously used in the French context by Lefranc and Trannoy (2005) and Lefranc (2018), and in many other countries (Jerrim et al., 2016, Table A1). It consists in predicting individuals' parents' incomes from a sample of other parents whose incomes are observed using a set of common observed characteristics. We refer to these other parents as *synthetic* parents.

Let Z denote a set of characteristics observed both for parents and synthetic parents. Their log lifetime incomes y can be expressed as:

$$y_i = \beta Z_i + \varepsilon_i. \tag{1.3}$$

We estimate this first-stage equation by OLS on our sample of synthetic parents, and predict parents' log lifetime incomes using the resulting $\hat{\beta}$ as $\hat{y}_i = \hat{\beta} Z_i$. Z includes parents' (i) education (8 categories), (ii) 2-digit occupation (42 cat.; includes inactivity status), (iii) demographic characteristics (birth cohort, French nationality dummy, country of birth (6 cat.), and household structure (6 cat.)), and (iv) characteristics of the municipality of residence (unemployment rate, share of single mothers, share of foreigners, population, and population density). For the geographic analysis, we drop the municipality characteristics to ensure they do not spuriously drive any spatial patterns, though this has virtually no impact on the estimates. All characteristics are observed in the 1990 census. To reduce the potential for lifecycle and attenuation bias, synthetic parents' income is defined as average pretax wage between 35 and 45 with at least 2 income observations over this age range in the All Employee Panel. The model is estimated separately on synthetic mothers (adj. $R^2 = 0.37$) and fathers (adj. $R^2 = 0.36$). We extensively test the robustness of our baseline results to using more flexible models and to varying the set of first-stage regressors in Section 1.5.1.

Method validity. To assess how reliable TSTSLS estimates are relative to their OLS counterparts (i.e., using *observed* parent income), we need a dataset that includes parents' actual incomes as well as predictors of parents' incomes. Since such a dataset does not exist for France, we follow Jerrim et al. (2016), Bloise et al. (2021) and Jacome et al. (2023), and conduct a validation exercise using the United States' Panel Study of Income Dynamics.[4] We describe this analysis in detail in Appendix A.2. We provide comparisons both at the national level and, due to sample size constraints, by Census Bureau regions (Northeast, Midwest, South, and West). Our sample and definition choices aim to be as close as possible to our main analysis setting while at the same time maximizing sample size.

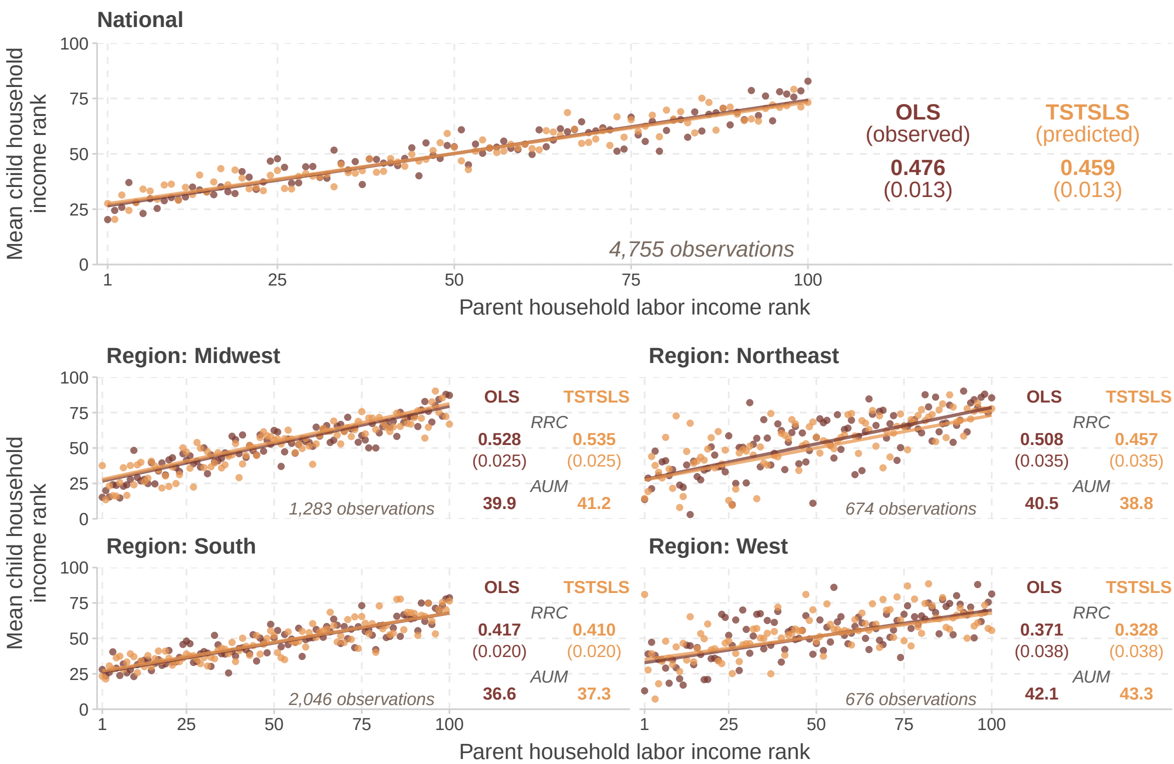
Specifically, our sample of children consists in individuals born between 1963 and 1988. We define parent income as the sum of father and mother mean labor income over ages 30-50, and child income as mean family total income over ages 30-50. The results are quantitatively similar when computing parent and child incomes over ages 35-45 as in the main analysis, despite the smaller sample size. We use education, 3-digit occupation (including inactivity status), birth year, race, and state of residence as first-stage predictors. These predictors are the closest we could find to those used in the main analysis.

Figure 1.1 presents the main results from our validation exercise. At the national level, the TSTSLS RRC estimate (0.459) is 4% smaller than the OLS estimate (0.476), a very moderate

[4] Acciari et al. (2022) and Cortes-Orihuela et al. (2022) also conduct validation exercises of TSTSLS using administrative data from Italy and Chile respectively.

difference. Moreover, and importantly, the TSTSLS estimate of the RRC appears to understate persistence, i.e., they provide an upper bound for intergenerational mobility, as also found by Cortes-Orihuela et al. (2022) and Jacome et al. (2023). The same applies for estimates of the transition matrix presented in Appendix Figure A.3. At the Census Region level, the RRC obtained by TSTSLS are again reasonably similar to those obtained by OLS, with a slightly larger underestimation for the Northeast and West regions where the number of observations is smaller. The same applies to absolute upward mobility, defined as the expected rank of children from families at the 25th percentile.

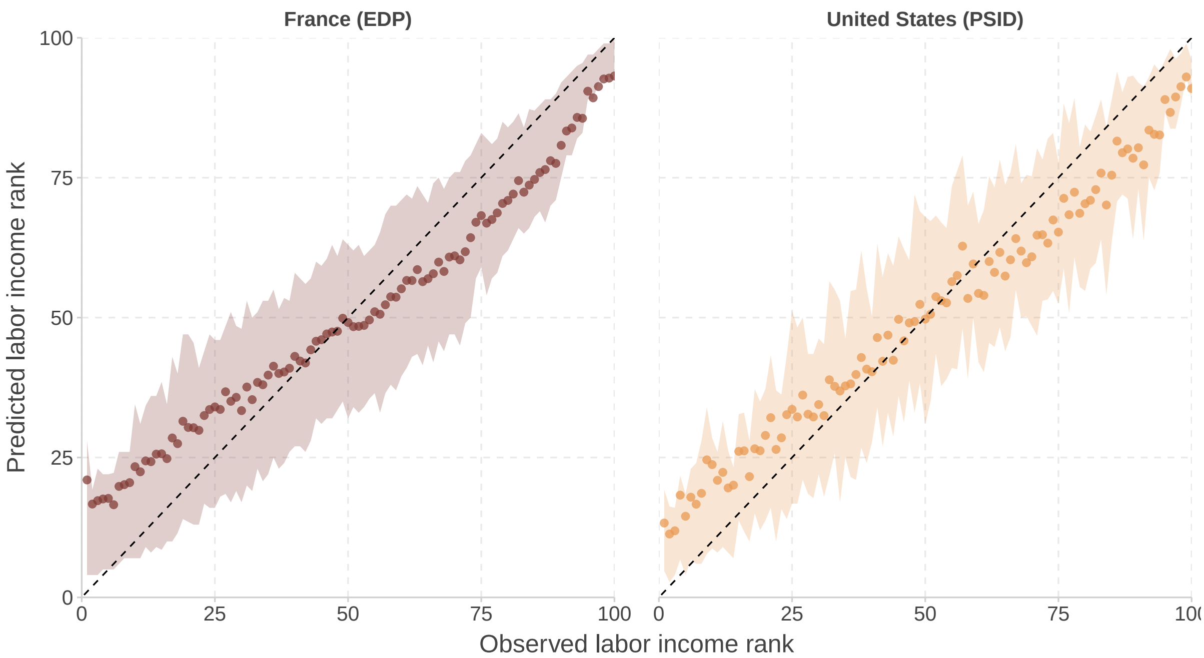
Figure 1.1: OLS vs. TSTSLS RRC - National and census regions in the United States



Notes: This figure presents rank-rank correlations obtained when parent income is observed (OLS) and when it is predicted using two-sample two-stage least squares (TSTSLS), at the national level and by Census Bureau Regions in the United States. They are computed on the Panel Study of Income Dynamics (PSID). The sample used is restricted to children born between 1963 and 1988 who are observed at least once as children in a family unit and at least once as a reference person or partner in a family unit over ages 30-50. Child income is the mean of family total income over ages 30-50. Parent income is the sum of father and mother (predicted) mean labor income over ages 30-50. For TSTSLS estimates, parent income is predicted separately for males and females using an OLS model including education (7 cat.; highest years of school completed), 3-digit occupation (334 cat.; most common occupation (incl. inactivity status) between 30 and 50 years old), parents' demographic characteristics in 1990 (birth cohort and race (5 cat.; most recent observation) and state fixed effects (most common state of residence between 30 and 50 years old). The fitted lines correspond to the regression line obtained on the microdata. We report coefficients and naive standard errors (in parenthesis) obtained from OLS regressions of child income rank on parent income rank with child cohort fixed effects, on the microdata for the full sample.

The TSTSLS RRC estimate is smaller than the true OLS estimate likely because parents from the very top (bottom) of the income distribution can only be mispositioned downwards (upwards) when using predicted incomes. Assuming a monotonic relationship between parents and child income ranks, this mechanically flattens the rank-rank relationship and biases the rank-rank correlation downwards. This can be seen in Figure 1.2, which shows the conditional expectation of out-of-sample predicted labor income rank with respected to observed labor income rank, as well as the interquartile range of the prediction. Indeed, percentile ranks tend to be overestimated at the bottom of the parents income distribution and underestimated at the top. We obtain very similar out-of-sample predictions in the EDP as in the PSID, suggesting we can reasonably apply the estimated TSTSLS biases of our validation exercise to the main analysis. Note that the IGE is sensitive to another bias because, all else equal, it is decreasing in the variance of parents' incomes (as highlighted in equation (1.1)). As such, since the distribution of predicted parent incomes is narrower than the true distribution, this puts an upwards pressure on the IGE.

Figure 1.2: Out-of-sample predicted labor income rank - France (EDP) and United States (PSID)



Notes: This figure presents the conditional expectation of out-of-sample predicted labor income rank with respect to observed labor income rank, for both the PSID validation exercise (United States - PSID) and our own parent income prediction (France - EDP). See Figure 1.1's notes for details on data, sample and income definitions for the PSID analysis, and Figure 1.3's note for details on our analysis (EDP).

Inference. Since we are in a two-stage setting, standard inference is inappropriate. Inoue and Solon (2010) derive an analytical formula for TSTSLS standard errors. However, their method cannot be applied in our setting as we use a non-standard transformation of the first-stage outcome variables. Indeed, because labor income is observed for synthetic parents *individually* but is not observed for their spouse, we can only estimate equation (1.3) on *individual* income. We then aggregate mother and father predicted incomes to obtain a measure of *household* income, which we use as the regressor in the second stage rather than using the fitted values from the first stage as is. We thus report bootstrapped standard errors for all individual-level regressions, which, for the same reason, cannot be clustered at the family level. Specifically, we draw one bootstrap sample for synthetic fathers and one for synthetic mothers separately. We then run the first-stage regression, and predict parent income on a bootstrap sample of children. We iterate this process 1,000 times. These bootstrapped standard errors are of the same order of magnitude though slightly larger than naive ones.

1.3.2 Sample definitions

Hereinafter we rely on the Permanent Demographic Sample (EDP) to estimate intergenerational persistence in France. Our samples of interest are defined as follows.

Sample of children. It consists of EDP individuals who are (i) born between 1972 and 1981 in metropolitan France,[5] (ii) observed with their parents in the 1990 census, (iii) whose parents are neither farmers nor in a liberal profession[6], and (iv) observed in the tax returns data at least once between 35 and 45 years old.[7] Restriction (i) is made to observe individuals with their parents in the 1990 census[8] and to have a reasonably large sample size for the subnational analysis. Restriction (ii) enables us to retrieve their parents' characteristics, and (iii) is due to the fact that farmers and liberal professions are not covered by the All Employee Panel from which we obtain synthetic parent income. Restriction (iv) aims to minimize lifecycle bias. The final sample contains 64,571 children.[9] Overall, they have very similar socio-economic characteristics as the representative sample of EDP individuals satisfying only restriction (i), except for under-representing children of farmers by definition, as shown in Appendix Section A.3.1.

Sample of synthetic parents. It is constructed such that synthetic parents come from the same overarching population as actual parents. It therefore consists of EDP individuals who (i) had at least one child born between 1972 and 1981 in metropolitan France, (ii) are observed in the 1990 census, (iii) are neither farmers nor in a liberal profession in 1990, and (iv) have at

[5] Metropolitan France refers to the part of France that is geographically in Europe.

[6] Liberal professions encompass activities that are not salaried, agricultural, commercial or artisanal, and carried out by self-employed service providers (e.g., lawyers, notaries, private doctors, etc.). 5.08% of EDP individuals satisfying (i) and (ii) have at least one parent who is a farmer and 2.41% have at least one parent who is in a liberal profession. As raised by Lefranc (2018), the fact that farmers tend to face relatively low incomes and a strong occupational inheritance (Lefranc et al., 2009) makes the exclusion of farmers likely to bias intergenerational persistence downwards.

[7] 6.73% of EDP individuals satisfying (i) and (ii) are not observed in the tax returns data between 35 and 45 years old.

[8] See Appendix Figure A.30 for the position in the family in the 1990 census by child birth cohort.

[9] See Appendix Table A.12 for the sample size at each additional restriction. Parent income cannot be predicted for 23 children because one of their parents has an occupation not represented in the sample of synthetic parents of the corresponding gender, hence the very slight discrepancy with this table.

least two pretax wage observations between 35 and 45 years old in the All Employee Panel.[10] As such our sample excludes individuals born in an odd year since they were not covered by the All Employee Panel prior to 2001. The final sample contains 31,423 synthetic parents.[11]

Descriptive statistics. Appendix Table A.17 provides statistics on our sample of synthetic parents and children. On average, fathers are around 42 in 1990 and mothers 39. This assures that we predict income based on observable characteristics measured sufficiently late in their lifecycle.

1.3.3 Variable definitions

The variables we use are constructed as follows. All incomes are expressed in 2015 euros, and are measured before taxes but after the deduction of employer- and employee-level payroll taxes.

Parent income. We define the income of one parent as predicted average pretax wage over ages 35 to 45. This income is predicted according to the methodology described in Section 1.3.1. We then compute income at the household level (regardless of marital status) by taking the average of father and mother predicted incomes if the child is observed with both parents in the 1990 census, and income of the only parent otherwise. We take the *average* of father and mother predicted incomes rather than the *sum* (the standard in the literature), to correct for the fact that otherwise single-headed households would be over-represented in the bottom of the income distribution (when using the sum, there are virtually no single-headed households above rank 50). Indeed, while in other studies parent income is typically observed repeatedly over several years, in our setting a parent observed as single in 1990 can by definition only be predicted their *individual* income for their entire lifetime even if their marital status actually changes later on. We refer to this income definition as parent household wage and use it as our main parent income measure. We also report results using father predicted income, which we refer to as father wage.

Child income. Our main measure of child income, computed from the tax returns, corresponds to the sum of labor earnings (wages and self-employment income), taxable and imputed non-taxable capital income[12], unemployment insurance, retirement, and alimony, at

[10] In Appendix Table A.13 we compare average characteristics of parents and synthetic parents. To ensure appropriate comparability of the two samples, no restriction on wage observations for synthetic parents or children is applied. Average characteristics are remarkably similar for most variables, even for 2-digit occupation (Appendix Table A.14), which confirms the assumption that actual and synthetic parents are random subsets of the same population.

[11] See Appendix Table A.15 for the sample size at each additional restriction.

[12] Financial incomes not subject to any tax reporting are predicted by the French National Institute of Statistics and Economic Studies (INSEE) from a model estimated on the *Enquête Patrimoine*. In particular, they predict capital income for seven financial products (various tax-exempt savings accounts and life insurance) using

the household level.[13] Just as for parents, a household is defined as individuals living in the same dwelling. To mitigate the potential for lifecycle bias, we average over 2010-2016 only for incomes declared when the individual is between 35 and 45 years old. We refer to this income definition as household income and use it as our main child income measure. We also report results using the following alternative child income definitions: (i) household wage, which is equivalent to the parent household wage definition, (ii) individual income, which we define as the sum of all individual-level incomes: labor earnings (wages and self-employment income), unemployment benefits, retirement, and alimony, and (iii) individual wage.

Income definition discussion. Our preferred parent and child income definitions represent the most comprehensive household-level income definitions possible for either generation. Defining incomes at the household level is important in order to (i) better capture the economic conditions of individuals and their parents, (ii) allow the inclusion of children raised by single mothers, and (iii) enable the analysis of daughters, whose labor incomes alone may not be an appropriate measure of their economic outcomes. These income definitions are not identical but the results are qualitatively similar when using the same income definition, household wage, for both children and parents.

Percentile ranks. We rank children within their birth cohort, and parents relative to other parents with children in the same birth cohort. To avoid individuals (in a given cohort) earning the same income (e.g., 0, or the minimum wage) being assigned different income ranks, we define the income rank of such individuals as the ceiling of the median income rank of individuals with that income level.[14]

1.4 Results at the national level

We start by analyzing intergenerational mobility at the national level. For our baseline results, we use data on children born on the first four days of October between 1972 and 1981 and measure parent income as household-level predicted average annual pretax wage over ages 35-45, and child income as pretax household income averaged over the same age range between 2010 and 2016. We include child birth cohort fixed effects in the log-log and rank-rank regressions.[15]

household-level observed characteristics (income, age, family situation, ...). Excluding this income source from our child income definition does not affect the results.

[13] Social benefits such as family allowances, social minima (e.g., RSA, disability benefits) and housing benefits are not included in our main measure of child income.

[14] For example, if there are 3.65% of children with zero income, their median rank is 2, and thus they are assigned a rank of 2. In our samples, 0.06% of children have negative or zero household income (see Appendix Table A.16), while no parent has negative or zero predicted wage.

[15] In practice, these fixed effects have virtually no influence on the coefficients of interest.

1.4.1 Intergenerational income elasticity (IGE)

Figure 1.3 panel A displays the conditional expectation of log child income with respect to log parent income. Children with negative or zero incomes are excluded. This is of minor importance when defining child income as household income as such cases are exceedingly rare. Nonetheless, we assess the influence of zero incomes in Appendix Figure A.24. The log-log CEF is pretty linear throughout the middle 80% of the parent income distribution, with some mild non-linearities at the tails.[16] This S-shaped relationship is also observed in the United States (e.g., Chetty et al. (2014)), Denmark (e.g., Helsø (2021)) or Sweden (e.g., Björklund et al. (2012)). It implies that the elasticity is not constant over the whole parent income distribution, with smaller magnitudes at the tails, and is sensitive to the inclusion or exclusion of parents at the tails of their income distribution.[17]

Our baseline IGE estimate is 0.527, meaning that a 10% increase in parent income is associated, on average, with a roughly 5% increase in child income. This estimate should be interpreted with caution as our validation exercise presented in Section 1.3.1 suggests TSTSLS estimates of the IGE can be quite inflated relative to the true value. Thus this baseline IGE is not well-suited for international comparisons. Appendix Figure A.31 shows our estimates of the intergenerational income elasticity for every child and parent income definition, and for sons and daughters separately. Our father-son wage IGE estimate is relatively similar to existing ones for France despite important differences in methodology and data (see Appendix Table A.18). Intergenerational persistence estimates are larger for household income than for individual income or wage, which could be the result of assortative mating. IGEs are very similar when defining parent income as father wage, despite the fact that by construction, estimates based on father wage exclude children only observed with their mother in the 1990 census (about 10% of observations). The IGE is significantly lower for sons (0.478) than for daughters (0.577). This phenomenon is not systematic across countries, but is also observed in Germany (Bratberg et al., 2017) and the Netherlands (Carmichael et al., 2020), for instance.

1.4.2 Rank-rank correlation (RRC)

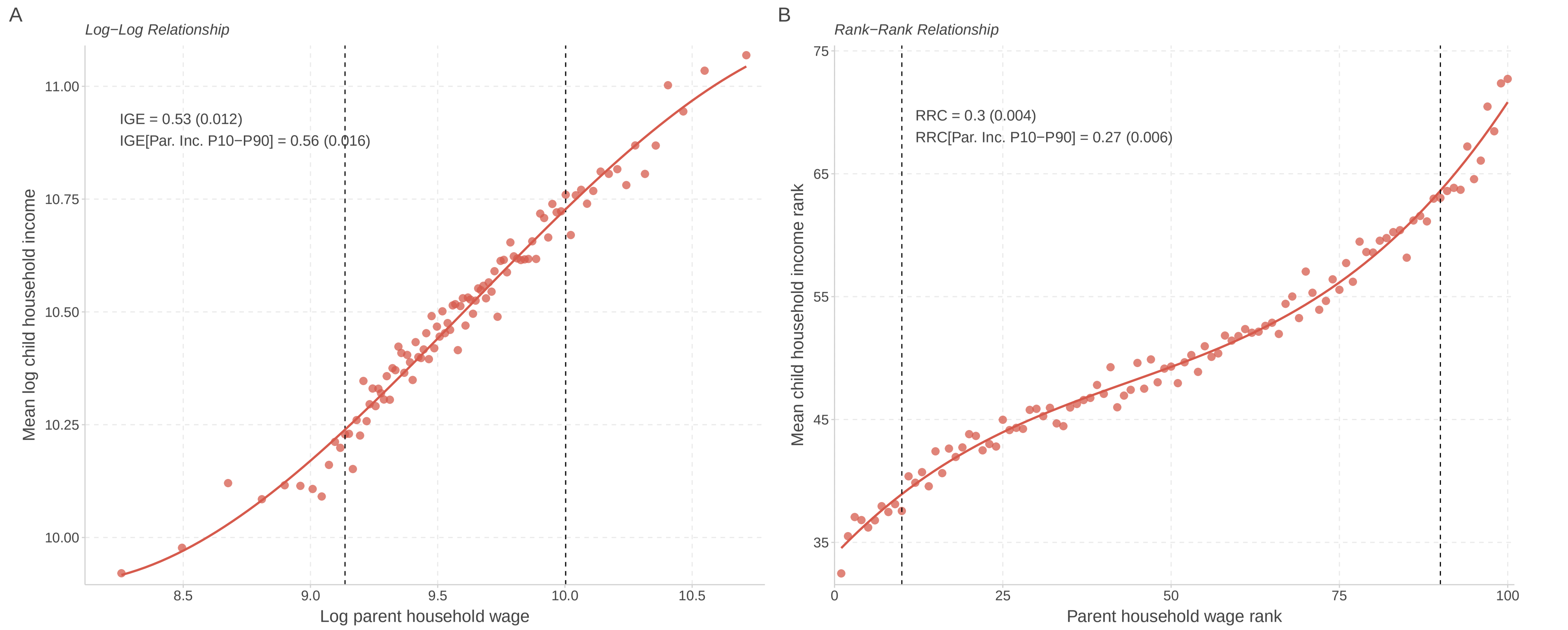
Figure 1.3 panel B plots the conditional expectation of child income rank with respect to parent income rank. It is relatively linear, with slight non-linearities at the tails as observed in many countries (Chetty et al., 2014; Bratberg et al., 2017; Helsø, 2021).

Our baseline estimate of the rank-rank correlation is 0.303, meaning that a 10 percentile increase in parent income rank is associated, on average, with a 3.03 percentile increase in child income rank. Note that this estimate corresponds to a lower bound, as the validation exercise suggests the TSTSLS methodology slightly underestimates the RRC. Applying the estimated

[16] Appendix Figure A.17 shows that these non-linearities are not driven by the set of first-stage predictors.

[17] Appendix Figures A.25a and A.25c show how trimming the top and bottom of the parent/child income distribution influences our estimates.

Figure 1.3: Conditional expectation functions for log-log and rank-rank relationships in France



Notes: This figure presents non-parametric binned scatter plots of the relationship between log child income and log parent income (panel A), and child income rank and parent income rank (panel B) in France. It is computed on the Permanent Demographic Sample, a dataset of individuals born on the first four days of October. The sample used is restricted to children born between 1972 and 1981. Child income is the mean of 2010–2016 household income (with age restricted to 35-45). Parent income is the sum of each parent predicted wage divided by the number of parents. Parent income is predicted separately for males and females using an OLS model including parents' education (8 cat.), 2-digit occupation (42 cat.), demographic characteristics in 1990 (birth cohort, French nationality dummy, country of birth (6 categories), and household structure (6 cat.)) and characteristics of the municipality they lived in in 1990 (unemployment rate, share of single mothers, share of foreigners, population, and population density). These municipality characteristics are excluded for the geographic analysis. It is estimated on a sample of synthetic parents whose average wage at ages 35-45 (at least 2 income observations) is used as the dependent variable. Incomes are in 2015 euros. To construct panel A, children with negative or zero incomes are excluded (0.06% of the sample) and we bin parent incomes into 100 equal-sized bins and plot mean log child income versus mean log parent income within each bin. To construct panel B, children are ranked relative to other children in the same birth cohort while parents are ranked relative to other parents with children in the same birth cohort. We then plot mean child income rank versus parent income rank. The dashed lines represent the 10^{th} and 90^{th} percentiles of parents' income. We report coefficients and bootstrapped standard errors (in parenthesis) obtained from OLS regressions of log child income on log parent income (panel A) and child income rank on parent income rank (panel B), both with child cohort fixed effects, on the microdata for the full sample and for parents between the 10^{th} and 90^{th} percentiles. The fitted line is a 3^{rd} order polynomial fit through the conditional expectations.

correction factor of 3.7% leads to a corrected baseline RRC coefficient of 0.314. Appendix Figure A.32 shows our baseline estimates of the rank-rank correlation for every child and parent income definition, and for sons and daughters separately. The estimates are slightly higher for daughters (0.310) than for sons (0.296), and are also slightly higher when defining parent income as household wage rather than as father wage. The estimates are smaller when defining child income as household wage or individual income and smallest when using individual wage, a pattern observed in other countries (Chetty et al., 2014; Deutscher and Mazumder, 2020; Landersø and Heckman, 2017), again possibly due to assortative mating.

To the best of our knowledge, this is the first time the RRC is estimated for France.[18] In Table 1.1 we compare RRC estimates for countries for which estimates exist (see Appendix Figure A.34 for a visual representation). To enable comparability we only keep studies which pool sons and daughters together, define parent income at the household level and use comprehensive income definitions. Note that for child income some studies only observe *individual* rather than *household* income which might result in lower RRC estimates (as we find for France, and Chetty et al. (2014) for the United States). Even though they are not directly comparable due to important differences in data and sample selection rules, we believe that it is a relevant exercise given the relative stability of the RRC to specification variations and common data limitations.

This international comparison suggests that (i) France exhibits strong persistence across generations in international comparison, given that it is the country with the second highest available RRC estimate behind the United States, and (ii) there is less variation across countries in the rank-rank slope than in the intergenerational elasticity, which is coherent with the fact that the RRC is not influenced by changes in inequality across generations, and is less sensitive to sample restrictions.

1.4.3 Transition matrices

The last measure of intergenerational income persistence we estimate is a quintile-by-quintile transition matrix, which documents the conditional probabilities of being in each income quintile as an adult given any parent income quintile. Figure 1.4 presents our baseline estimates of the transition matrix for France, along with available estimates for the United States (Chetty et al., 2014) and Australia (Deutscher and Mazumder, 2020). To the best of our knowledge, this is the first time transition matrices are estimated for France.[19]

[18] A recent report (in French) by Abbas and Sicsic (2022) now also provides rank-based intergenerational mobility estimates for France. They use the same data as us and their sample consists in individuals born in 1990 (i) who are still claimed as dependent in their parents' tax return at age 20, (ii) whose parents' income can be observed around age 50, and (iii) whose individual income is observed at age 28 in their own tax return. They compare their results to ours and despite different sample definitions, when using the same income definition and measuring child income at the same age (i.e., 28), they find very similar results.

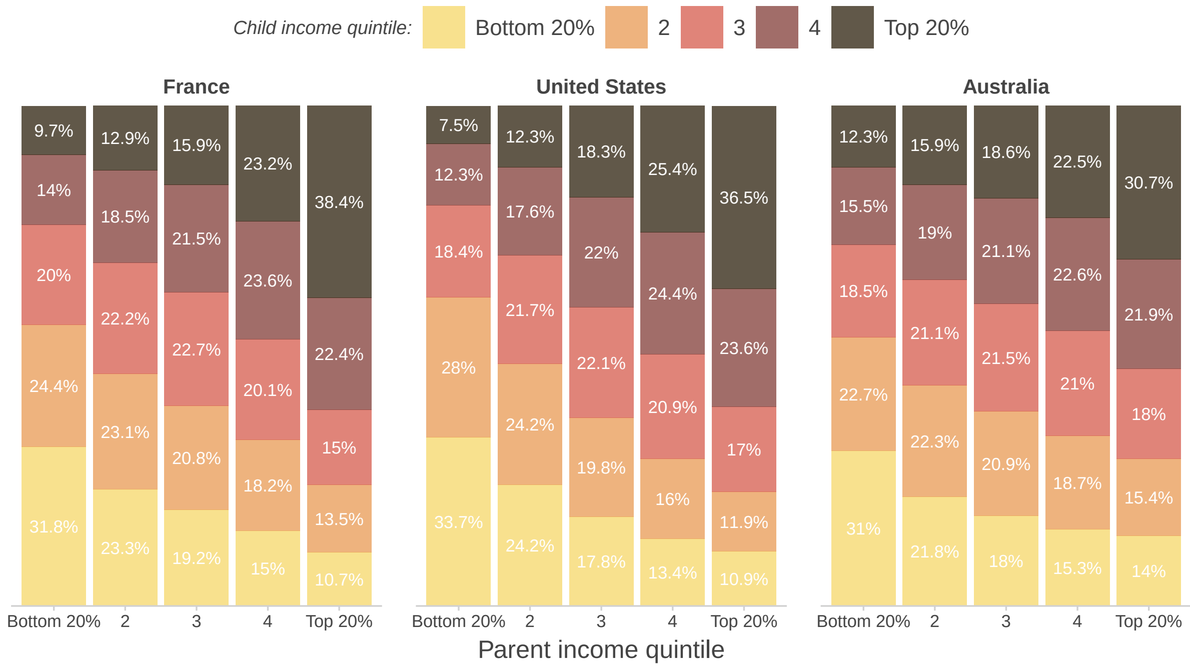
[19] Alesina et al. (2018) estimated father-son wage transition probabilities from the bottom quintile only, using the TSTSLS methodology and data from the *Formation et Qualification Professionnelle* survey for earlier cohorts (1963-1973).

Table 1.1: Rank-rank correlation in international comparison

Country	RRC ↓	# obs.	Data	Child Income Definition[1]	Child Cohort	Child Age or Year at Income Measurement	Parent Age or Year at Income Measurement	Source
Switzerland	0.14	667,047	Social Security Earnings Records	Average total pretax *individual* income	1967-1982	32-34	when child between 15-20	Kalambaden and Martınez (2021, Table 3)
Switzerland	0.14	923,262	Social Security Earnings Records	Average total pretax *individual* income	1967-1984	30-33	when child between 15-20	Chuard-Keller and Grassi (2021, Figure 1)
Spain	0.195	1,492,107	Atlas de Oportunidades	Total pretax *individual* income	1980-1986	2016	1998	Soria Espín (2022, Figure 1)
Sweden	0.197	778,484	SIMSAM database[2]	Average total pretax *individual* income	1968-1976	32-34	34-50	Heidrich (2017, Table 2)
Denmark	0.203	157,543	Danish register data	Average total pretax *family* income	1980-1982	2011-2012	1996-2000	Helsø (2021, Table 1)
Australia	0.215	1,025,800	Federal income tax returns	Average total pretax *family* income	1978-1982	2011-2015	1991-2001	Deutscher and Mazumder (2020, Table 2)
Sweden	0.215	252,745	35% random sample from admin. data	Average total pretax *household* income	1957-1964	1996-2007[3]	1978-1980	Bratberg et al. (2017, Table 3)
Norway	0.223	324,870	Full population admin. data	Average pretax *family* earnings	1957-1964	1996-2006	1978-1980	Bratberg et al. (2017, Table 3)
Canada	0.242	2,115,150	Intergenerational Income Data	Average total pretax *family* income	1963-1970	2004-2008	when child between 15-19	Corak (2020, Table 5)
Germany	0.245	1,128	German Socio-Economic Panel	Average total pretax *household* income	1957-1976	2001-2012	1984-1986	Bratberg et al. (2017, Table 3)
Denmark	0.253	≈ 410,000	Danish register data	Average total pretax *individual* income	1973-1979	2010-2012	when child between 7-15	Landersø and Heckman (2017, Table A6)
Denmark	0.257	205,625	Full population admin. data	Average total pretax *individual* income	1973-1975	2010-2012	when child between 7-15	Eriksen (2018, Table 3.2)
Italy	0.30[4]	1,719,483	Electronic database of Personal Income returns	Average total pretax *individual* income	1979-1983	2016-2018	1998-2000	Acciari et al. (2022, p.145)
France	0.303[5]	64,571	Permanent Demographic Sample	Parents: (predicted) *household* wage; Children: average total pretax *household* income	1972-1981	2010-2016 (between 35-45)	35-45	
United States	0.341	9,867,736	Federal income tax records, 1996-2012	Average total pretax *family* income	1980-1982	2011-2012	1996-2000	Chetty et al. (2014, Table 1)
United States	0.395	6,414	NLSY79	Average total pretax *family* income (self-reported)	1957-1964	1996-2008	1978-1980	Bratberg et al. (2017, Table 3)

Notes:
[1] The parent income definition is always at the family level.
[2] Swedish Initiative for Research on Microdata in the Social and Medical Sciences.
[3] Only even years.
[4] This estimate corresponds to the one when adjusting for lifecycle bias, incomplete coverage of taxpayers and tax evasion as reported on p.28. The baseline RRC estimate reported in Table 3 is 0.22.
[5] Assuming that the bias induced by the TSTSLS methodology is the same in France as in the United States, our validation exercise performed on the PSID suggests the OLS counterpart to our baseline estimate would equal to $0.303 \times \frac{0.476}{0.459} = 0.314$ (see Figure 1.1).

Figure 1.4: Baseline quintile transition matrix for different countries



Notes: The first panel of this figure presents our baseline intergenerational transition matrix estimates. Bootstrapped standard errors are presented in Appendix Figure A.33. See Figure 1.3's notes for details on data, sample and income definitions. Each cell documents the share of children belonging to the quintile indicated by the color legend among children born to parents whose income falls in the quintile indicated on the x-axis. We present these estimates along with those put forward by Chetty et al. (2014) for the United States (second panel) and Deutscher and Mazumder (2020) for Australia (third panel). While we rely on at most 11 income observations (7 on average) for parents and at most 7 income observations (5 on average) for children, Deutscher and Mazumder (2020) use 11 income observations for parents and 5 for children, and Chetty et al. (2014) use 5 income observations for parents and 2 for children.

We find that 9.7% of children born to parents in the bottom 20% reach the top 20% in their forties. This share is 7.5% in the United States and 12.3% in Australia. In comparison, 31.8% remain in the bottom 20% of the income distribution. Regarding children born to the top 20%, 38.4% remain at the top, while only 10.7% move down to the bottom of the income distribution, much less than in Australia (14%). As a reference point, in a society where an individual's income is completely independent of parent income, the probability of being in any quintile given a parent quintile would by definition be 20%. We analyze persistence at the top of the parent income distribution in more detail in Appendix Section A.3.5.

Note that among the corner elements of the transition matrix, the estimates of mobility (i.e., P(Child Top 20% | Parent Bot. 20%) and P(Child Bot. 20% | Parent Top 20%)) are likely to be upper bounds, while estimates of persistence (i.e., P(Child Bot. 20% | Parent Bot. 20%) and P(Child Top 20% | Parent Top 20%)) are likely to be lower bounds. This is because the potential measurement error in parent rank prediction induced by TSTSLS can only go in one direction for the bottom and top quintiles. Parents in the bottom 20% necessarily have a true rank in the bottom 20% or above, but not below, as ranks take positive values by definition. Reasonably

Table 1.2: Transition matrix in international comparison

Country	P(Child Top 20% | Parent Bot. 20%) ↓	P(Child Bot. 20% | Parent Bot. 20%)	P(Child Top 20% | Parent Top 20%)	Source
United States	7.5%	33.7%	36.5%	Chetty et al. (2014, Table 2)
Italy[1]	8.6%[2]	36.7%	27.8%	Acciari et al. (2022)
France	9.7%	31.8%	38.4%	
Denmark	10.7%	30.7%	34.8%	Eriksen (2018, Figure 3.3*)
Netherlands	11.3%	29.8%	33.1%	Carmichael et al. (2020, Table 1*)
Canada	11.4%	30.1%	32.3%	Corak (2020, Table 6)
Switzerland	11.9%	23.7%	30.3%	Chuard-Keller and Grassi (2021, Table 2)
Spain	12.3%	25.3%	33.3%	Soria Espín (2022, Table A.5)
Australia	12.3%	31%	30.7%	Deutscher and Mazumder (2020, Table 3)
Switzerland	12.8%	24.5%	28.8%	Kalambaden and Martınez (2021, Table 5)
Sweden[3]	15.7%	26.3%	34.5%	Heidrich (2017, Figure 10, Appendix B)

Notes: See Table 1.1 for details about samples and income definitions used in each study.
[1] As the authors point out, this paper's baseline estimates are likely to overestimate upward mobility and underestimate persistence at the bottom and at the top because of lifecycle bias, the omission of taxpayers and tax evasion. The reported P(Top 20% | Bottom 20%) here corresponds to the estimate accounting as best as possible for these three sources of bias. For the other two measures, we report the estimates correcting for missing tax returns and tax evasion obtained from the authors.
[2] Obtained by multiplying the "Q1Q5" estimate found in the last column of Table 14 by the ratio of the two rows in Table 11, i.e., $0.100 \times 0.099/0.115$.
[3] Child incomes are measured relatively early in the lifecycle (32-34 years old), thus these estimates may suffer from lifecycle bias (i.e., overestimating upward mobility and underestimating persistence). By comparison, the father-son P(Child Top 20% | Parent Bot. 20%) estimate in Nybom and Stuhler (2017, Figure 1, Panel D) is essentially 10%, a much lower estimate of upward mobility.
* The authors very kindly shared more detailed estimates than reported in their papers.

assuming that the probability of reaching the top 20% is increasing in parent income rank, our estimate of P(Child Top 20% | Parent Bot. 20%) is therefore likely to be an upper bound. In line with this intuition, the PSID validation exercise suggests that TSTSLS transition matrices overstate mobility relative to observed transition matrices (see Appendix Table A.4). The same reasoning can be applied to the other corner elements of the transition matrix.

In Table 1.2 we compare conditional probabilities of interest with those found for other developed countries. In France income persistence across generations is particularly strong, both at the top and at the bottom. While France does better than the United States when it comes to upward mobility from the bottom quintile (9.7% vs. 7.5%), a point we discuss in Section 1.4.4, it fares significantly worse than countries such as Canada (11.4%), Switzerland (11.9%) or Australia (12.3%). It also displays one of the strongest persistence at the bottom and at the top of the income distribution.

1.4.4 Discussion of baseline results

International comparison. Our findings confirm the conventional wisdom that France exhibits strong income persistence across generations relative to many OECD countries (OECD, 2018). This is true not only with respect to the IGE, which has been the main focus for cross-country comparisons in the literature (e.g., see Corak (2016)), but also for the RRC, and in terms of transition matrices. This raises the question of the underlying mechanisms. Indeed, one apparent puzzle is that various studies have found positive effects of government spending on intergenerational mobility (Mayer and Lopoo, 2008; Huang et al., 2021). Yet, despite significant government spending, France displays relatively little intergenerational mobility.

However, though the IGE and RRC estimates are fairly similar for France and the United States, the two countries differ in terms of the probability of reaching the top 20% conditional on having parents in the bottom 20%. Given the large dissimilarities in their higher education systems, part of the explanation could stem from differences in access to, and graduation from, higher education along the parent income distribution.

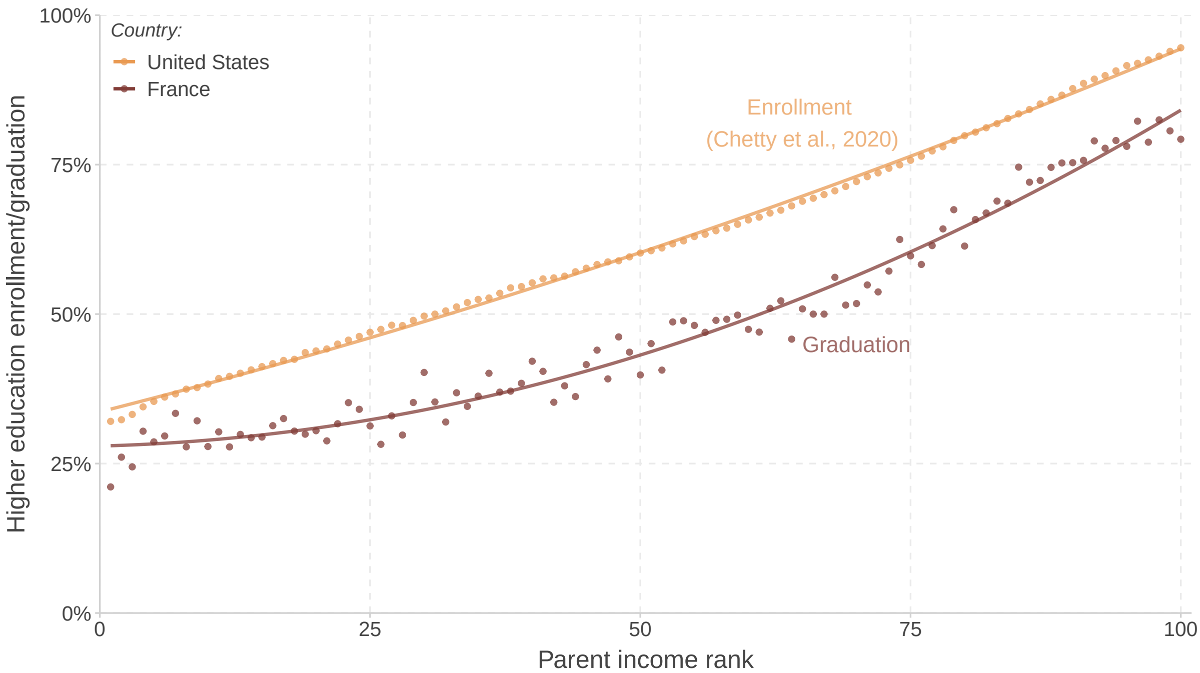
Access to and graduation from higher education. Using the yearly census surveys available since 2004 in the EDP, we can observe children's last obtained diploma when they are between 23 and 45.[20] Figure 1.5 compares higher education *graduation rates* in France with *enrollment rates* in the United States (defined by Chetty et al. (2020) as attending college at least at some point between ages 18-21) by parent income rank. To avoid capturing the direct effect of parent education (independent from parent income) on child higher education graduation, we use parent income ranks obtained when excluding parent education from the set of first-stage predictors. This has virtually no effect on the result. Graduation rates in France are lower than enrollment rates in the United States, which is expected considering that a sizable share of students who enroll in higher education eventually drops out. While the relationship between parent income rank and enrollment is linear in the United States, obtaining a higher education degree appears to be a convex function of parent income rank in France. In particular, it is flatter at the bottom of the distribution.[21] This convex relationship is all the more striking since children from low-income families are probably more likely to drop out from higher education, and therefore not earn a higher education degree.

This comparison does not allow us to assess directly whether higher education may explain the gap in upward mobility between France and the United States, since the relationship between college completion and parent income rank for the latter is not available. Using a French survey of roughly 6,000 18-24 year olds, Bonneau and Grobon (2022) find that enrollment rates in higher education by parent income rank are very similar in France compared to the United States. Therefore, if higher education were to explain part of the upward mobility gap observed between the two countries, it must necessarily be trough differences in dropouts rates and/or heterogeneous returns to higher education along the parent income distribution.

[20] We observe this information for 86% of the sample. The share of missing values is pretty well uniformly distributed along the parent income rank distribution.

[21] Appendix Figure A.35 documents the graduation rate for each cell of the quintile-by-quintile transition matrix. It shows that the convexity in the relationship between family background and graduation rate holds within child income quintile.

Figure 1.5: Graduation from/enrollment in higher education by parent income



Notes: This figure presents higher education graduation in France vs. enrollment rates in the United States (Chetty et al., 2020) by parent income rank. See Figure 1.3's notes for details on data, sample and income definitions. In this figure parent income ranks are computed without parent education in the set of first-stage predictors to avoid capturing the effect of parent education independent from that of parent income.

1.5 Robustness of baseline results

In addition to the method validity exercise presented in Section 1.3.1, we assess the sensitivity of our baseline results to the TSTSLS method by (i) varying the set of instruments, and (ii) relaxing parametric assumptions. Moreover, as discussed in Section 1.2.2, two statistical biases may affect our baseline estimates: lifecycle and attenuation bias. The former relates to heterogeneous lifecycle earnings profiles among parents and children, while the latter refers to classical measurement error in parent income. We therefore assess how our estimates vary with the age at which child and parent incomes are measured, and with the number of synthetic parent income observations used. We discuss additional potential biases (i.e., data coverage, treatment of zero incomes, and top and bottom income trimming) in Appendix A.3.

1.5.1 Two-sample two-stage least squares

First-Stage Predictors. We first estimate the IGE, RRC, and transition matrices using only education as the first-stage predictor. We then add successively to the set of first-stage predictors: parents' (i) 2-digit occupation, (ii) demographic characteristics, and (iii) municipality-level characteristics. Our baseline specification corresponds to the one including the full set of predictors. The results are shown in Appendix Figure A.17.

Overall, our estimates are largely insensitive to the set of first-stage regressors, except for the IGE which is significantly larger when using only education in the first-stage. For example, the RRC (IGE) when using only education is 0.284 (0.679) compared to 0.303 (0.527) in our baseline. The transition matrices are also mostly unchanged: when using only education the P(Top 20% | Bot. 20%) is 10.8% compared to 9.7% in our baseline. These results are consistent with our validation exercise using the PSID where we find that the TSTSLS RRC estimate increases slightly once (3-digit) occupation is included as a predictor and the transition matrices are largely unaffected by the set of first-stage regressors (see Appendix Table A.4).

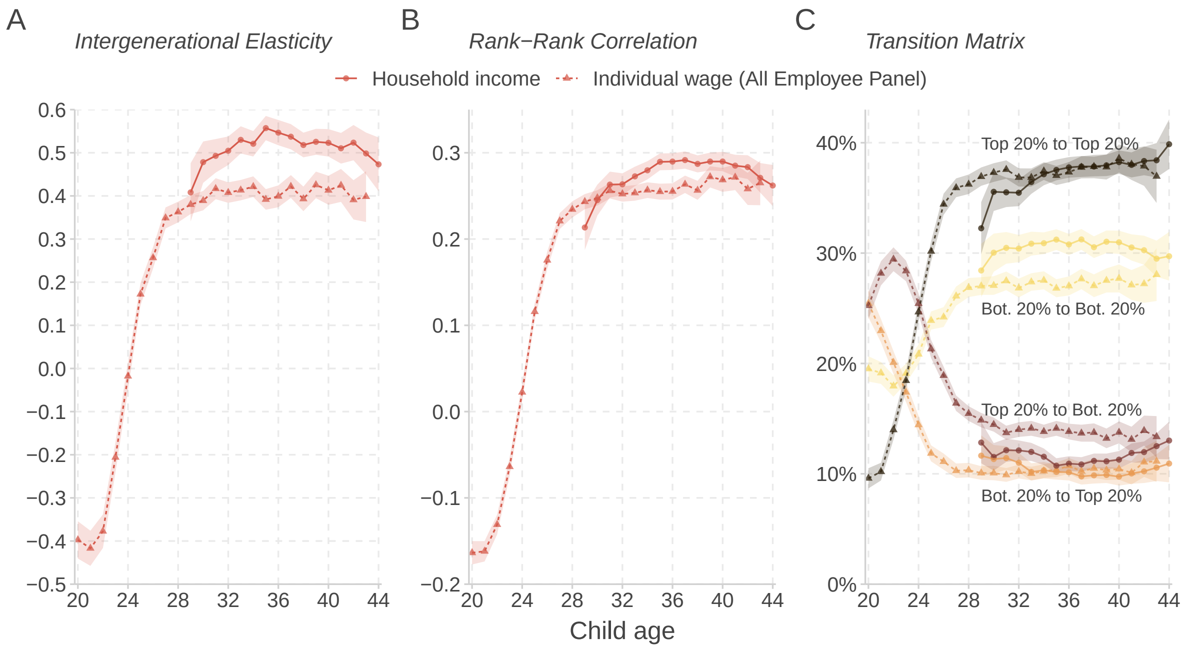
Functional form. We estimate the first-stage using the three following flexible methods: (i) generalized additive model (GAM), (ii) gradient boosted tree, and (iii) the ensemble method. The results are shown in Appendix Figure A.18. These more flexible models yield essentially identical estimates and they do not lead to gains in terms of (out-of-sample) mean squared error.

1.5.2 Lifecycle and attenuation bias

Child lifecycle bias. Figure 1.6 presents our estimates of intergenerational income mobility when varying the age at which child income is measured. In addition to household income from the tax returns data, we exploit the longer time series wage data provided by the All Employee Panel. Each point represents the estimate of the measure of intergenerational income mobility when measuring child income at a given age. For the transition matrix, we only present the analysis for the conditional probability of being in the top or bottom 20% for children born to parents in the top or bottom 20%. The broad pattern that emerges in Figure 1.6 is that the estimated persistence (mobility) increases (decreases) sharply when child incomes are measured early in the lifecycle and stabilizes roughly when child income is measured in their mid-thirties.[22]

[22]By construction, each age estimate is obtained from a different sample since we only measure child incomes in the tax returns data between 2010 and 2016, and in the All Employee Panel from 1967 to 2015 (though only for individuals born in even years before 2001). The observed slight decline in the IGE and RRC estimates when children are in their forties for household income appears to mostly reflect changes in the underlying cohort sample rather than a real decrease in the estimate (see Appendix Figure A.19 where we reproduce the All Employee Panel estimates keeping the sample of children constant).

Figure 1.6: Child lifecycle bias



Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates to changes in the age at which child income is measured. Shaded areas represent the 95% bootstrapped confidence intervals. See Figure 1.3's notes for details on data, sample and income definitions.

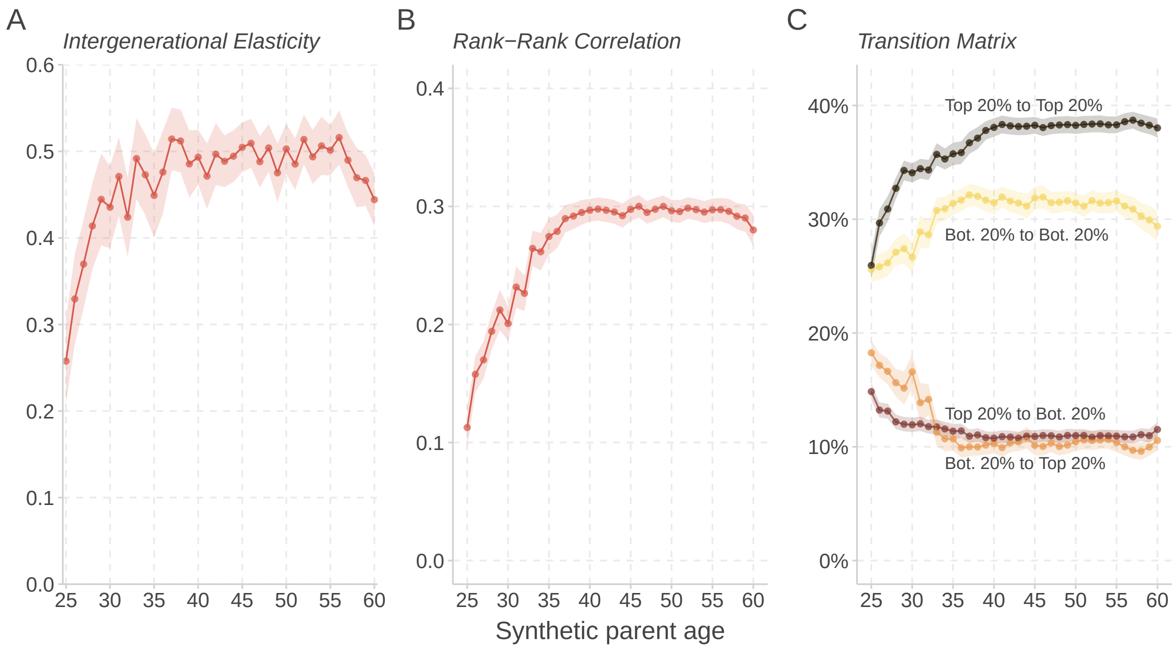
Parent lifecycle bias. We assess the sensitivity of our baseline estimates to varying the age at which parent income is measured. Since we predict parent income rather than observe it, we vary the age at which synthetic parent income is measured in the first-stage regression. Specifically, we run the first-stage regression (equation (1.3)) defining synthetic parent income at a given age between 25 and 60 years old. Figure 1.7 shows that the relationship between age at which parent income is measured and persistence is concave, strongly increasing between 25 and the late thirties and then stabilizing until the mid to late fifties. Relative to our baseline estimate, it does not appear that our choice of measuring synthetic parent income as the average between 35 and 45 years old is either too early or too late in the lifecycle.[23]

Attenuation bias. We evaluate the extent to which our baseline estimates are sensitive to the number of observations used to compute parent lifetime income. The main source of attenuation bias comes from measurement error in parent income.[24] Appendix Figure A.21 plots estimates of our persistence measures varying the number of synthetic parent income observations used in the first-stage regression from 1 to 11 (see details in Appendix Section A.3.3). The rank-based measures, whether the RRC or the transition matrix cells, are remarkably un-

[23]In Appendix Section A.3.3 we study how our measures of intergenerational persistence vary with the age at which child and synthetic parent income is measured jointly.

[24]We also check in Appendix Section A.3.3 the sensitivity of intergenerational mobility to the number of child income observations and confirm that it only plays a very minor role.

Figure 1.7: Parent lifecycle bias



Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates to changes in the age at which synthetic parent income is measured. Shaded areas represent the 95% bootstrapped confidence intervals. See Figure 1.3's notes for details on data, sample and income definitions.

altered by increasing the number of income observations over which synthetic parent income is averaged. However, the IGE increases gradually with the number of income observations, which largely rests on how mothers' incomes are predicted. In the context of TSTSLS estimation, this appears to be a strength of rank-based measures since it suggests that in cases where parent income is not observed, predicting it using only one synthetic parent income observation is likely to provide sufficiently accurate estimates. This is indeed what we find in our validation exercise, where the TSTSLS RRC bias is largely unchanged when increasing the number of parent income observations.

1.6 Geographic analysis

1.6.1 Heterogeneity across departments

A first step in understanding the sources of intergenerational mobility in France is to investigate where persistence is highest and lowest. We study the geographic variations of intergenerational mobility at the department level. Departments divide metropolitan France into 96 territories.[25] Departments have the advantage of covering the whole of metropolitan France,

[25] For practical reasons, we treat Corsica as a single department. Appendix Figure A.36 shows a map of French departments.

and their borders have not changed over the study period. In addition, considering finer geographic units such as commuting zones would imply dropping a sizable amount of areas due to insufficient sample size.

Children are assigned to their department of residence in 1990, when they were between 9 and 18 years old. This is the best proxy we have for the department they grew up in. To ensure our estimates are sufficiently reliable, we focus on the 85 departments with at least 200 observations.[26] Individuals are still ranked within the national income distribution.

Hereinafter we use parent income predicted without municipality characteristics in the first stage. This is to make sure that they do not spuriously drive any spatial patterns.[27] Moreover, we find that spatial variations in intergenerational mobility are not driven by differences in prediction accuracy of the first-stage across departments. Indeed, as shown in Appendix Table A.19, the department-level mean-squared errors of the first-stage predictions are not significantly related with department-level intergenerational mobility measures.

The statistics we use at the subnational level are (i) the IGE, (ii) the RRC, and (iii) the expected income rank for individuals whose parents locate at the 25th percentile, which we refer to as *absolute upward mobility* (AUM) following Chetty et al. (2014). We favor absolute upward mobility over specific cells of the transition matrix because of the size of our department samples. Indeed, while absolute upward mobility is estimated using all the observations in a given department, any cell of the quintile transition matrix is by construction estimated using only a fifth of these observations. Denoting $p_{c,d}$ the percentile income rank of children observed in department d during childhood, and $p_{p,d}$ the percentile income rank of their parents, local RRCs are obtained from the following OLS regression:

$$p_{c,d} = \alpha_d + RRC_d \times p_{p,d} + \varepsilon_d \tag{1.4}$$

The expected income rank for individuals whose parents locate at the 25th percentile then writes:

$$\text{AUM} := \mathbb{E}[p_{c,d} \mid p_{p,d} = 25] = \hat{\alpha_d} + R\hat{R}C_d \times 25 \tag{1.5}$$

Appendix Figure A.37 graphically illustrates how this intergenerational mobility measure is computed for the Nord department, the most populated one in 1990. The conditional expectation functions for the most populated departments are available in Appendix Figures A.38 and A.39. Even at the department level, it appears that the rank-rank relationship is well approximated by a linear function.

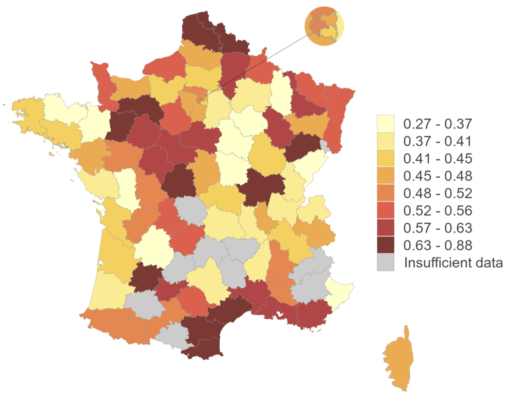
Geographic variations. Figure 1.8 depicts department-level intergenerational mobility as captured by the three estimators mentioned above. It reveals substantial variations, though

[26] The number of observations per department is reported in Appendix Table A.20.

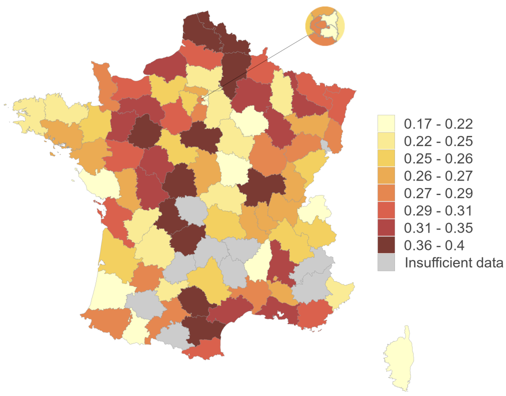
[27] The removal of municipality characteristics from the first stage does not alter our national estimates (see Appendix Figure A.17) nor the first-stage R^2. Moreover, the cross-department correlation with and without municipality characteristics is above 0.97 for all three intergenerational mobility measures (IGE, RRC, AUM).

not necessarily statistically significant likely due to a lack of statistical power.[28] The distribution of department-level RRCs ranges from 0.17 to 0.40 and is tighter than that of IGEs, which ranges from 0.27 to 0.88. Both vary across departments just as much as they vary across countries. The range of our estimates of absolute upward mobility, from rank 37 to rank 54, is almost identical to that observed in Italy using a comparable geographic unit (from 35 to 57 (Acciari et al., 2022)).

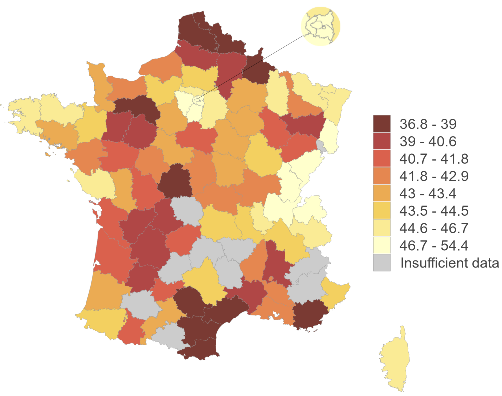
Figure 1.8: Spatial variations in intergenerational mobility



(a) Intergenerational elasticity



(b) Rank-rank correlation



(c) Absolute upward mobility

Notes: This figure presents department-level estimates of our intergenerational mobility measures. To compute local estimates, individuals are assigned to their department of residence in 1990, when they were between 9 and 18 years old. Departments with less than 200 observations are considered as having insufficient data. See Figure 1.3's notes for details on data, sample and income definitions.

Intergenerational persistence is particularly high in the North and in the South of France, and relatively low in the West. For instance, the IGEs range from 0.30 to 0.45 in departments in Brittany (West), from 0.42 to 0.70 in departments in Hauts-de-France (North), and from 0.63 to 0.77 in the former region of Languedoc-Roussillon (South). This pattern is observed

[28]Department-level estimates are reported in Appendix Table A.20. Department-level IGE, RRC and AUM are represented graphically with their confidence intervals in Appendix Figures A.40 to A.42.

not only in terms of relative mobility (IGE and RRC), but also in terms of absolute upward mobility. Indeed, while children with modest socio-economic backgrounds have relatively high expected income ranks in Brittany (AUM $\in$ (43.3; 44.7)), they tend to remain lower in the income distribution in Hauts-de-France (AUM $\in$ (36.8; 44.1)) and Languedoc-Roussillon (AUM $\in$ (36.9; 39.3)).

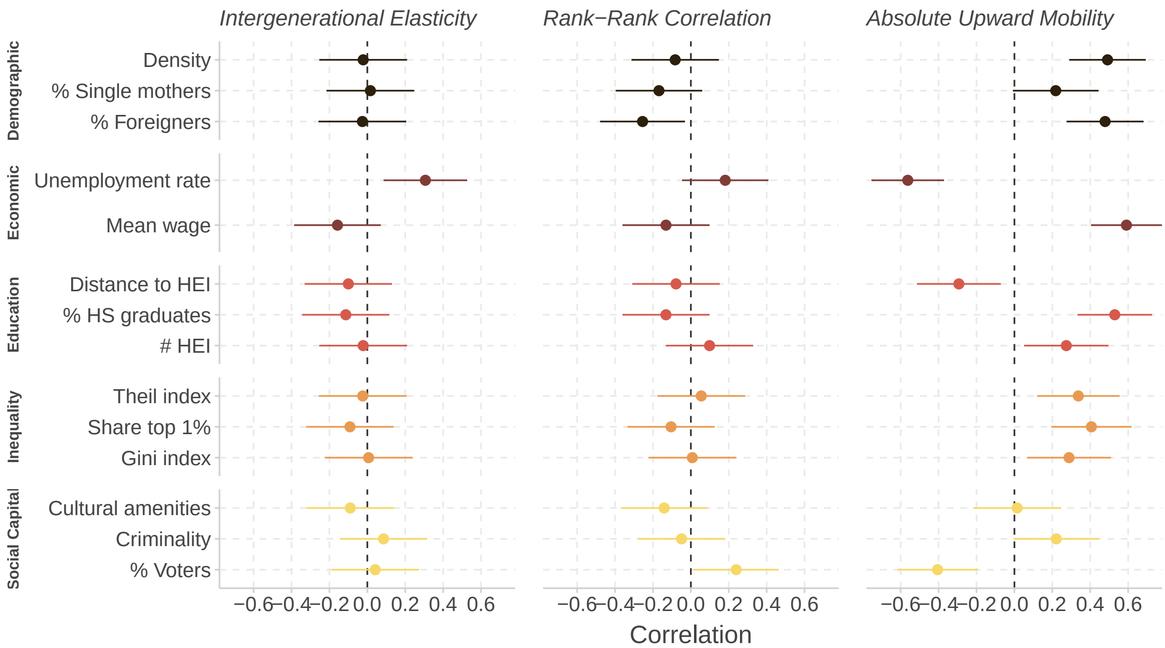
However, a high relative mobility is not systematically associated with a high absolute upward mobility. For instance, such a discrepancy is observed for the municipality-department of Paris, the third highest department in terms of AUM, but where intergenerational mobility levels in terms of IGE and RRC are close to the department-level average. The conditional expectation functions in Appendix Figure A.39 provide an explanation to this idiosyncrasy. They reveal that the Parisian CEF is both shifted upwards relative to other large departments, and flatter at the lower end of the parent income distribution. The combination of these two features results in relatively good prospects for children whose parents locate at the 25$^{\text{th}}$ percentile without implying particularly high relative mobility. The cross-department correlation between the IGE and RRC is 0.65, and is -0.55 with AUM (see Appendix Table A.21), which highlights the importance of using a variety of intergenerational mobility measures to characterize a country's income persistence across generations (Deutscher and Mazumder, forthcoming).

Correlation with local characteristics. To pin down potential sources of the spatial variations in intergenerational mobility, we explore the department characteristics that it might correlate with. We consider 14 variables, measured as close to 1990 as possible, classified into 5 groups: demographic, economic, inequality, education, and social capital variables. There are three main takeaways from this correlational analysis (additional details can be found in Appendix A.4).

First, the IGE appears to be only significantly related to the unemployment rate. This correlation is indeed striking visually when comparing the department-level unemployment rate in 1990, displayed in Appendix Figure A.43, with Figure 1.8a. Second, absolute upward mobility tends to exhibit much stronger relationships with department characteristics in general, than either the IGE or the RRC. This suggests that factors that affect absolute mobility might differ from those that affect relative mobility. A lasso analysis, detailed in Appendix A.4.3, yields similar insights.

Third, we find no evidence of a within France "Great Gatsby Curve", which refers to the positive correlation between intergenerational income persistence (defined by the IGE) and income inequality (defined by the Gini index) found across countries (Corak, 2013). The Gini index is significantly positively related to absolute upward mobility, the opposite sign one might expect if inequality is detrimental to intergenerational mobility. This contrasts with findings from Italy (Acciari et al., 2022) and North America (Chetty et al. (2014) for the United States and Corak (2020) for Canada).

Figure 1.9: Intergenerational mobility and department characteristics - Separate estimation



Notes: This figure presents the regression coefficient between department-level intergenerational mobility and department characteristics. Each coefficient is obtained from a separate regression. Both the department intergenerational mobility estimates and the characteristics are standardized, implying that the coefficients can be interpreted as correlations. Horizontal lines represent the 95% confidence intervals. See Figures 1.3 and 1.8's notes for details on data, sample and income definitions, and Appendix Table A.8 for definitions and sources of the department characteristics.

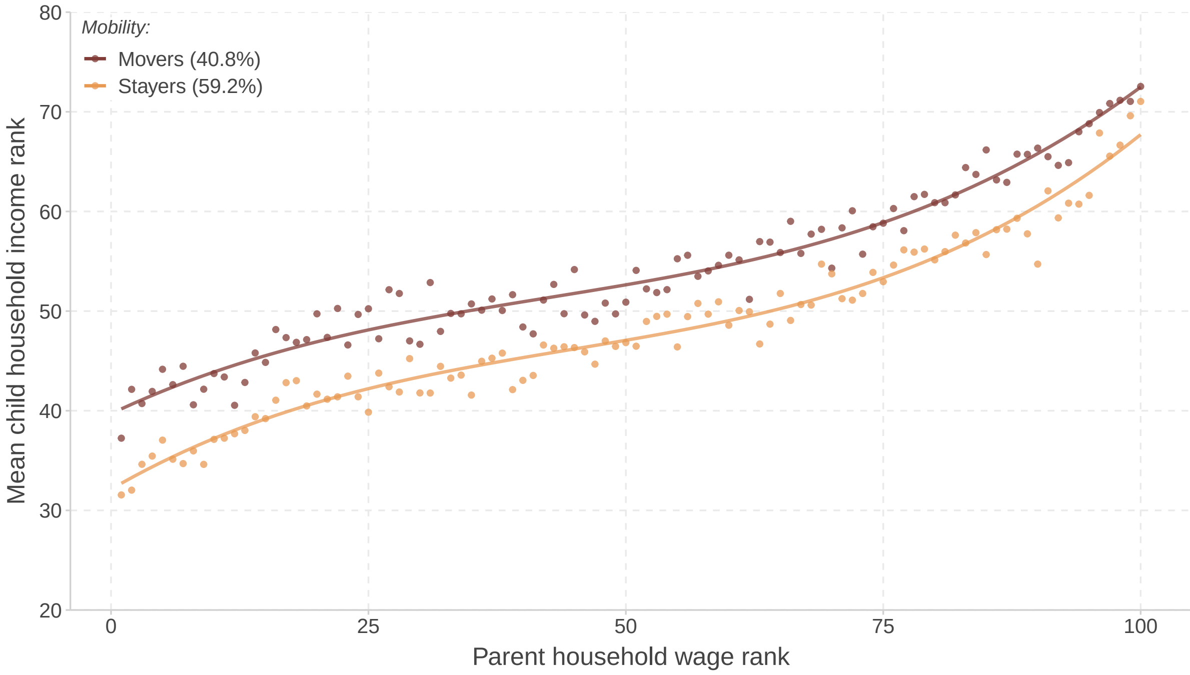
1.6.2 Geographic mobility

Few studies have explored the relationship between geographic mobility and intergenerational mobility.[29] We consider individuals as geographically mobile if their adulthood department of residence is different from their childhood department of residence. The childhood department of residence is observed in the 1990 census, when individuals were aged from 9 to 18 years old. The adulthood department of residence is the one indicated on individuals' tax return. If the individual has lived in several departments over 2010-2016, we consider the most common department of residence. In case of ties, we consider the most recent of the most common departments. According to this definition, 40.8% of individuals are geographically mobile. This share is relatively homogeneous across males (40.2%) and females (41.3%). The percentage of movers by parent household wage rank is presented in Appendix Figure A.44.

Intergenerational mobility gains from geographic mobility. Figure 1.10 shows the con-

[29] Soria Espín (2022) analyzes this relationship in Spain, but other existing studies rather exploit geographic mobility to estimate the causal impact of location on upward mobility (Chetty and Hendren, 2018; Laliberté, 2021).

Figure 1.10: Intergenerational mobility and geographic mobility



Notes: This figure represents the conditional expectation of child household income rank with respect to parent household wage rank separately for individuals whose adulthood department of residence is different or not from their childhood department of residence. Percentile ranks are computed according to the national income distribution, which implies that the share of movers and stayers is not constant throughout the parent income distribution. The childhood department of residence is observed in the 1990 census, when individuals were aged from 9 to 18 years old. The adulthood department of residence is the one indicated on individuals' tax return. If the individual has lived in several departments over 2010-2016, we consider the most represented department of residence. In case of ties, we consider the most recent of the most represented departments. See Figure 1.3's notes for details on data, sample and income definitions.

ditional expectation of child household income rank with respect to parent household wage rank for movers and stayers. The CEF is slightly flatter for movers than for stayers, and importantly, movers have systematically higher expected income ranks than stayers throughout the parent household wage rank distribution. The difference between the two CEFs is slightly decreasing in parent income and is particularly pronounced at the bottom of the distribution. This difference is the result of the combination of individuals self-selecting into migration and the causal effect of moving.

To characterize the relationship between intergenerational and geographic mobility, we estimate the following regression model:

$$p_{c,i} = \alpha + \beta p_{p,i} + \gamma \text{Mover}_i + \delta p_{p,i} \times \text{Mover}_i + X_i'\lambda + \varepsilon_i, \tag{1.6}$$

where $p_{c,i}$ is the household income rank of individual i, $p_{p,i}$ is individual i's parents' household wage rank, Mover_i is a binary variable taking the value 1 if individual i lives in a different department from the one they grew up in and 0 otherwise, and X_i is a set of control variables.

Table 1.3 reports the corresponding regression results.

Table 1.3: Intergenerational & geographic mobility

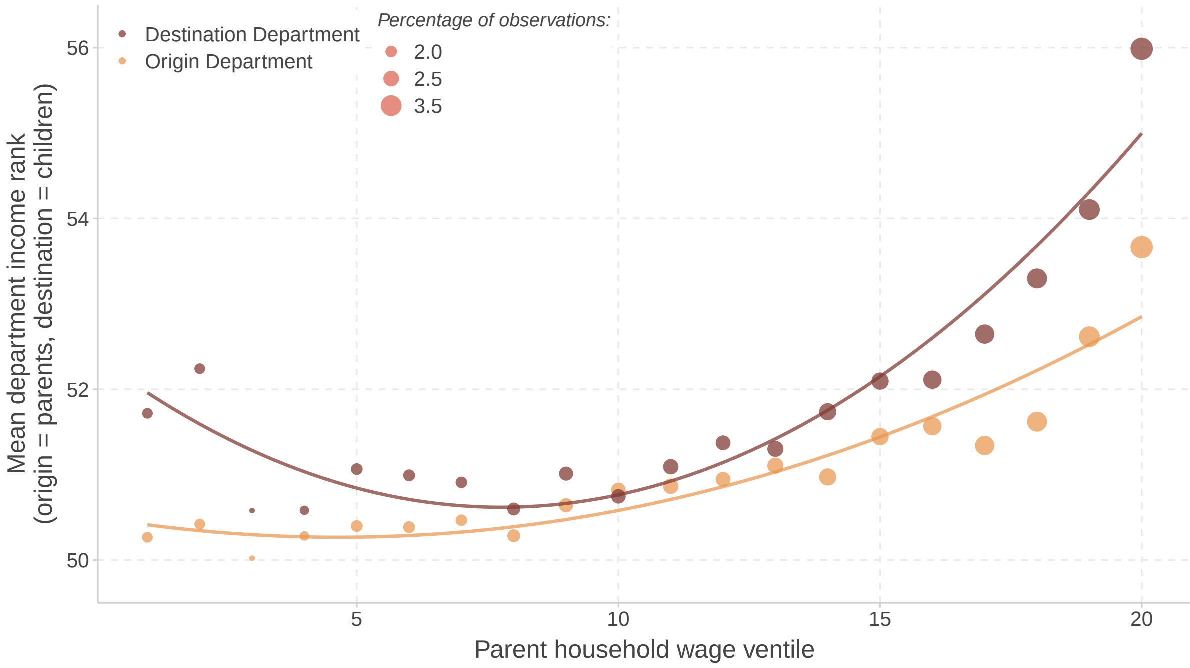
	Dependent variable: Child household income rank				
	(1)	(2)	(3)	(4)	(5)
Parent income rank ($\hat{\beta}$)	0.278***	0.278***	0.268***	0.163***	0.138***
	(0.005)	(0.005)	(0.005)	(0.008)	(0.017)
Mover ($\hat{\gamma}$)	5.836***	5.858***	5.539***	5.716***	5.681***
	(0.472)	(0.472)	(0.475)	(0.472)	(0.475)
Parent income rank × Mover ($\hat{\delta}$)	0.001	0.0003	0.001	−0.012	−0.013
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Constant	34.087***	33.780***	38.123***	29.195***	30.509***
	(0.258)	(0.274)	(1.228)	(1.659)	(1.782)
Birth cohort	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓
Department FE			✓	✓	✓
Parents' education				✓	✓
Parents' 2-digit occupation					✓
$\mathbb{E}[\hat{\gamma} + \hat{\delta} p_p] = \hat{\gamma} + \hat{\delta} \times 50.5$	5.89	5.87	5.59	5.11	5.02
$\mathbb{E}[\hat{\gamma} + \hat{\delta} p_p \| p_p = 25]$	5.86	5.86	5.56	5.42	5.36
$\mathbb{E}[\hat{\gamma} + \hat{\delta} p_p \| p_p = 75]$	5.91	5.91	5.61	4.82	4.71
Observations	64,571	64,571	64,571	64,571	64,571
Adjusted R^2	0.098	0.098	0.106	0.119	0.125

Notes: This table provides the estimates from regression child household income rank on their parents' income rank, a dummy variable indicating whether the individual is a mover, and the interaction between these two variables. Columns (2) to (5) progressively include control variables. See Figure 1.10 for details on variable and sample definitions. Bootstrapped standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Column (1) shows the estimates from equation (1.6). Living in a different department from one's childhood department is associated, on average, with a $\mathbb{E}[\hat{\gamma} + \hat{\delta} p_{p,i}] = 5.89$ percentile rank increase in the national household income distribution. The point estimate of the rank-rank slope is slightly lower for movers when controlling for parents characteristics (coefficient $\hat{\delta}$ col. (4)-(5)), but not statistically significantly so. In the last specification, the difference in expected income rank between movers and stayers is decreasing in parent income (5.36 at the 25^{th} percentile and 4.71 at the 75^{th} percentile).

The role of mobility toward richer departments at the aggregate level. There are several potential reasons for the better intergenerational mobility outcomes movers tend to experience. One explanation may be that movers simply migrate to departments where wages are higher. To investigate this channel, we compute two statistics: (i) the mean parent household wage rank in the origin department, and (ii) the mean child household income rank in the

Figure 1.11: Mean income rank of origin and destination departments of movers



Notes: This figure represents the conditional expectation of income rank with respect to parent household wage rank for movers, separately by origin and destination departments. Origin department mean income rank is computed as the average income rank of residents in the parent sample, while destination mean income rank is computed as the average income rank of residents in the child sample. See Figures 1.3 and 1.10's notes for details on data, sample and income definitions.

destination department. Figure 1.11 displays the average of these two statistics for movers for each ventile of the parent household wage rank distribution.

There are three takeaways from this figure. First, the difference in average income rank in the destination and origin departments is highest at the top and bottom of the parent income distribution. Second, these differences are relatively small, reaching at most 2 percentile ranks for the top ventile. Third, the origin and destination departments of movers from the middle of the parent income distribution have very similar average income ranks. Put in parallel with the slight monotonic decrease in the gains from geographic mobility along the parent income rank distribution, it seems that these gains are not only due to individuals moving to higher-income departments.

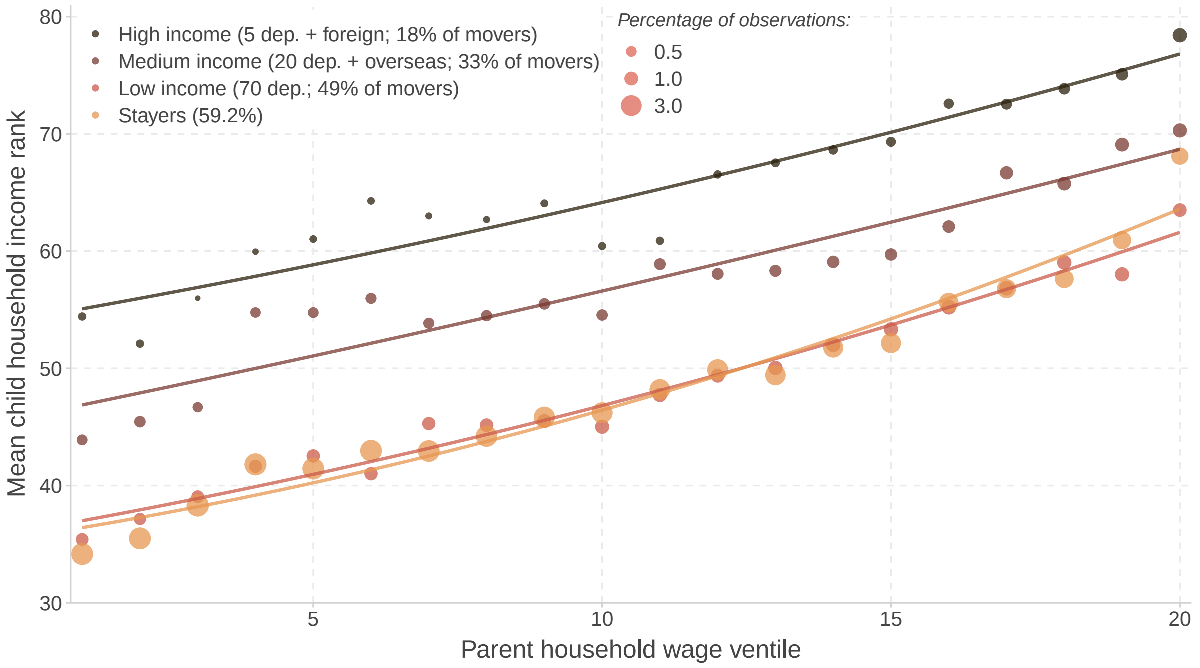
Another way to test this hypothesis consists in comparing the conditional expectation functions of movers and stayers ranked either at the *national* and *department* level. Indeed, ranking individuals at the national level allows individuals born to parents who earn the median income of their department to be upward mobile by earning the median income of a higher-income department in adulthood. This channel can be removed by ranking individuals and their parents within departments. When doing so, movers can only be more intergenerationally mobile than stayers if they reach income ranks in their adulthood department that are further away from the rank of their parents in their childhood department. Finding no expected gains associated with geographic mobility when ranking individuals according to their department income distribution would suggest that the expected increase in income rank associated with mobility is fully driven by movers ending up in higher-income departments, but reaching on expectation a local income rank in their destination department that is not further away from that of their parents, relative to stayers.

The regression results of equation (1.6) using percentile ranks computed at the department level rather than at the national level are reported in Appendix Table A.22 (Appendix Figure A.45 shows the corresponding conditional expectation functions). When considering ranks in the department distribution, the gap between the conditional expectation functions of movers and stayers shrinks but does not vanish completely. While the expected national-rank increase associated with mobility amounts to 5.89, it drops to 3.87 when considering local ranks. This suggests that the intergenerational mobility gains associated with geographic mobility are partly attributable to movers locating in higher-income departments in adulthood relative to stayers, but also to movers reaching local ranks in their adulthood department that are further away from the rank of their parents in the childhood department.

The role of mobility toward richer departments at the individual level. While geographic mobility patterns between low- and high-income departments only partially explain the gap between movers and stayers at the aggregate level, characteristics of the destination department may be decisive at the individual level. To investigate this hypothesis we classify destination departments into three groups according to the average income rank of their residents from the child sample: (i) *low-income*, destination departments with an average income rank below 50 (70 departments - 49% of movers), (ii) *medium-income*, those with an average income rank between 50 and 60 (20 departments and overseas departments - 33% of movers), and (iii) *high-income*, those with an average income rank above 60 (5 departments and foreign countries - 18% of movers). This high-income group of departments greatly overlaps with the Parisian region as it comprises Essonne, Hauts-de-Seine, Paris, and Yvelines.

Figure 1.12 shows the conditional expectation of child income rank with respect to parent income ventile for the three destination department categories and for stayers. Results of the corresponding regression are reported in Appendix Table A.23. Except for the top ventiles, the CEFs of movers by destination department category are virtually parallel. Movers thus experience similar levels of relative mobility regardless of the income category of their destination department. However, movers' absolute upward mobility increases with the average income of the destination department, such that the expected income rank of a mover from the bottom of the parent income distribution to a high-income department is around the same as the expected income rank of a stayer from the 75th percentile of the parental income distribution. Still, such transitions are the exception: most movers to high-income departments come from high-income families, while low-income movers go predominantly to low-

Figure 1.12: Mean child income rank by destination department mean income



Notes: This figure represents the conditional expectation of child household income rank with respect to parent household wage rank for stayers and for movers to departments of different mean income categories. Solid lines represent second-order polynomial fits. Low income destination departments are destination departments with an average income rank below 50, medium income are those with an average income rank between 50 and 60, and high income are those with an average income rank above 60. See Figures 1.3 and 1.10's notes for details on data, sample and income definitions.

or medium-income departments. Another noteworthy finding is that expected income ranks are essentially the same for movers to low-income departments as for stayers, highlighting the potential role of the destination department's characteristics in generating upward intergenerational mobility for movers. All these findings combine self-selection and causal effects, and we leave the disentangling of these two channels for future research.

1.7 Conclusion

France is an interesting case study for intergenerational income mobility considering its relatively modest income inequality and the specificity of its higher education system. Yet, it has been the focus of few studies due to important data limitations. We use administrative data to provide an overview of intergenerational income mobility in France for individuals born in 1972-1981. Relative to existing studies, the richness of these data enables us to apply two-sample two-stage least squares (TSTSLS) using a much larger set of individual characteristics, and to extensively assess the robustness of the resulting estimates. Using the Panel Study of Income Dynamics (PSID) we find that the TSTSLS methodology slightly underestimates rank-based measures of intergenerational persistence relative to what would be obtained if parent income was observed.

Moreover, we provide the first estimates of the rank-rank correlation and transition matrix for France, and conduct a comparative analysis with other countries for which such statistics are available. Our results reveal that France exhibits a relatively strong intergenerational income persistence at the national level. It ranks among the highest in OECD countries, with Italy and the United States, and far from Australia, Canada, and Scandinavian countries.

This high intergenerational income persistence at the national level hides substantial geographic heterogeneity across departments. We observe about as much variation across French departments as across countries. Intergenerational persistence appears to be particularly high in the North and South, and relatively low in the Western part of the country. Yet, only *absolute* mobility, as opposed to *relative* mobility, significantly correlates with local characteristics.

We also provide novel descriptive evidence on a new mechanism that could explain some features of intergenerational mobility: geographic mobility. We find that the difference in expected income ranks between geographically mobile individuals and stayers is large and slightly decreasing in parent income. This difference appears not to be solely due to individuals moving to higher income departments but to be also the result of individuals moving up the local income rank ladder. Destination departments are on average characterized by higher income levels than origin departments only at the tails of the parent income distribution. However, regardless of parent income rank, conditional on moving the absolute upward mobility gains associated with moving to a higher-income department appear to be large and increasing with average income in the destination department. Even though not causal, we believe that these descriptive findings constitute promising avenues for future research to better understand intergenerational income mobility.

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Chapter 2

Intergenerational mobility among children of immigrants and natives: The role of residential segregation

Abstract

I investigate the differences in intergenerational mobility between children born in France to native versus immigrant parents. For most origin groups, and systematically among daughters, income gaps with children of natives disappear when comparing individuals whose parents had the same income. Still, a gap persists for sons of immigrants from North Africa, despite higher rates of college graduation at the lower end of the parents' income distribution. The gap is lower among positive-income earners, and vanishes in terms of hourly wage, hinting at a labor market access mechanism. I investigate the role of residential segregation in this remaining gap using an instrumental variable approach. I estimate a spatial division index based on how geographical barriers partition the urban units individuals grew up in to isolate exogenous variations in segregation. Results suggest that residential segregation has a significantly negative effect on intergenerational mobility for sons of natives, and even more so for sons of North African immigrants. A marginally significant effect is found for daughters of natives as well, but no effect is observed among other groups.

2.1 Introduction

Being born to natives or immigrants is an inherited characteristic that has consistently been shown as one of the strongest determinants of socio-economic outcomes. In theory, in societies with low intergenerational mobility, the mere fact that immigrant parents tend to earn less than natives mechanically lowers their children's expected outcomes. In practice, in most western European countries, children of immigrants are shown to be still worse off when compared to children of natives with similar parental backgrounds. Discrimination has largely been documented as one driver of this conditional gap by the correspondence testing literature, but other factors may be at play. In this study, I use rich French administrative data to investigate the role of residential segregation, a potential driver of this remaining gap which is easier to quantify in a more exhaustive and systematic manner, and thus easier for policy makers to act on.

Results show that in most cases, accounting for parents' position in the income distribution closes the income rank gap between children of immigrants and children of natives. Sons of immigrants from North Africa, however, remain persistently lower than sons of natives in the income distribution, even within parents' income decile. In an instrumental variable approach, I show that sons of immigrants from North Africa are also the only group besides sons of natives to experience a negative effect of residential segregation on conditional income rank. These results extend the literature by providing evidence on the effect of residential segregation on intergenerational mobility differences between children of immigrants and natives in a different cultural and historical context of immigration than in the United States, more representative of most Western European countries.

I conduct the analysis on a French administrative dataset which combines lifetime census data, tax data, and employer-employee data. This allows me to observe the share of immigrants where individuals grew up in, down to the building level, their own immigration background, and their detailed earnings when adults. This merger of administrative data sources is available for a pseudo-random 1% sample of the population, which contains about 85,000 individuals for the birth cohorts considered, spanning from 1972 to 1984. Following Kenedi and Sirugue (2023), I estimate parents' income rank based on their detailed socio-demographics observed in individuals' childhood census data. Robustness checks suggest that inaccuracies in income rank predictions, as well as lifecycle and attenuation bias, are mild and homogeneous across origin groups.

To begin with, I describe the overall income rank convergence with natives between first-generation immigrants and their children. The over-representation of immigrants at the bottom of the income distribution is much more pronounced among parents born in North Africa, with 50% of them pertaining to the first decile. Children of immigrants, however, are distributed much more evenly in the income distribution, especially those with a South European background as they completely caught up with children of natives. Still, a significant gap of about 10 percentile ranks is observed for children of immigrants from North Africa.

Comparing income ranks within parents' income decile systematically raises the average income rank of children of immigrants relative to children of natives. Children of South European immigrants perform even better than natives conditional on parents' income, hence the absence of unconditional gap despite large differences in parents' income backgrounds. The gap between daughters of natives and daughters of North African parents closes down completely within income deciles, but for sons, a gap of 4 percentile income ranks persists. These results are very reminiscent to what Chetty et al. (2020) documented across race in the United States: a black-white gap among men but not women, and intergenerational mobility rates much more similar to that of whites among hispanic individuals.

This remaining gap between sons of immigrants from North Africa and sons of natives does not seem to be explained by differences in educational attainment. Indeed, higher education graduation rates tend to be larger for children of immigrants conditional on parents' income ranks, irrespective of gender and origin group. To better understand the nature of the conditional income-rank gap, I distinguish what comes from higher-paying jobs and what comes from more hours worked using two alternative specifications. First, when I exclude non-positive incomes, the conditional income rank gap between children of natives and children of immigrants from North Africa reduces. Second, when I use hourly wage instead of annual income, it vanishes completely. This suggests that the remaining gap stems from differences in labor market access both at the extensive and at the intensive margin, despite conditional higher education attainments that are higher than natives' on average.

I focus on residential segregation as a potential driver of this reduced labor market access. I measure shares of immigrants in individuals' areas of residence based on the exhaustive population census, which I then match at the urban unit, neighborhood, and building level. The average share of immigrants in one's building of residence is 8% for children of natives, and 26% for children of immigrants. The strength of the relationship between the local share of immigrants and individuals' income rank conditional on parents' rank varies depending on the geographical level considered. It is at the neighborhood level that this relationship is the most strongly and consistently negative.

I measure residential segregation in urban units using the Duncan dissimilarity index, which captures how unevenly immigrants are distributed across neighborhoods. Residential segregation tends to be lower in the South and in the East, and higher in the North and in the West. To get a naive sense of the role played by residential segregation in the negative relationship between the local share of immigrants and conditional income rank, I consider their relationships jointly. The interaction between the share of immigrants and segregation absorbs the entirety of the negative effect, hinting at the fact that segregation may actually mediate the negative relationship initially attributed to the share of immigrants.

To isolate the causal component of this relationship, I implement an instrumental variable strategy adapted from Chyn et al. (2022), based on how geographical barriers divide urban units. Specifically, I consider the combination of waterways, roadways, and railways to compute an index capturing how unevenly an urban unit's area is distributed across the sub-units generated by these features, controlling for their length in the urban unit. The underlying idea is that conditional on the density of such features in a given urban unit, the way they divide space can be more or less prone to residential segregation without otherwise affecting the intergenerational mobility prospects of resident children.

Baseline results document a negative effect of residential segregation on income rank conditional on parents' income rank for both sons of natives and sons of immigrants from North Africa. A one standard deviation increase in residential segregation reduces by 0.68 percentile rank the effect of a one percentage point increase in the local share of immigrants on individuals' income rank for sons of natives, conditional on parents' income rank, on average. For sons of immigrants from North Africa, this effect reaches -1.9 percentile income ranks. No significant effect is observed for children of South European origin, and a smaller and inconsistently significant effect is found for daughters of natives.

These interpretations rely on the assumption that the way in which these features divide space more or less equally into more or less sub-units has no effect on intergenerational mobility other than through residential segregation. The validity of this assumption is jeopardized by the fact that some of the barriers considered constitute transportation networks, which can themselves foster intergenerational mobility as shown for Argentina (Pérez, 2018) and England and Wales (Costas-Fernández et al., 2020). To address this issue, in addition to controlling for the length of each feature in urban units, I construct an index capturing the extent to which the railway network offers labor market opportunities using aggregate wages in urban units directly connected via the railway network, weighted by their bilateral distances. Results are robust to the inclusion of this index.

This paper contributes to two main strands of the literature. First, it relates to the literature on the intergenerational mobility and socio-economic integration of immigrants and their children. Intergenerational mobility prospects of children of immigrants are often shown to be less favorable than those of children of natives. This was notably documented for Estonia (Kivi et al., 2021), the Netherlands (Van Elk et al., 2024), and Sweden (Bratu and Bolotnyy, 2023), but not for Denmark (Jensen and Manning, 2024) or the United States (Abramitzky et al., 2021). However, Mazumder (2014) and Chetty et al. (2020) find significant heterogeneity in intergenerational mobility across race in the United States. Because it is much less common to have racial information in European datasets, and because of different historical contexts of immigration, it must be kept in mind that variation in the overlap between race and recent immigration background may hamper international comparisons.

In France specifically, the intergenerational mobility of second-generation immigrants has notably been studied based on the *Trajectoires & Origines* survey conducted by the Insee and Ined (Beauchemin et al., 2016). In particular, Beauchemin (2018) identifies migration background as a key factor of heterogeneity in intergenerational socio-economic mobility, and shows that sons of immigrants from North Africa are particularly disadvantaged. Achard (2024) also shows that for children of immigrants, parents' characteristics are less predictive than grandparents' characteristics due to the non-lasting socio-economic downgrading experienced by first-generation immigrants. In this study, I complement these descriptive findings using administrative data to document the intergenerational mobility prospects of children of immigrants and natives in more details along the parental income distribution.

Second, this paper contributes to the literature focusing on residential segregation and neighborhood effects. In the United States, Andrews et al. (2017) show that past racial segregation explains a significant part of the variation in intergenerational mobility documented by Chetty et al. (2014). In France, Weber et al. (2024) document a persistent neighborhood disadvantage among the offspring of non-European immigrants, and McAvay and Safi (2018) elicit a higher risk of cumulative spatial disadvantage for North African and Sub-Saharan African immigrants. Regarding neighborhood effects, Hémet and Malgouyres (2018) notably show that in France, diversity at the neighborhood level in terms of parents' origins matters less than diversity in terms of nationality for employment prospects. In this context, this paper endeavors to quantify the causal effect of residential segregation on intergenerational mobility differences across origins.

The remainder of the paper is structured as follows. Section 2.2 describes the data sources, sample, and main variables. Section 2.3 investigates the persistence of the income rank gap between natives and immigrants from the first to the second generation. Section 2.4 documents patterns of residential segregation and estimates its causal effect on the conditional income gap in an instrumental variable approach. Section 2.5 reports the main robustness checks, and Section 2.6 concludes.

2.2 Data

2.2.1 Permanent Demographic Sample

The Permanent Demographic Sample (EDP) is a large-scale French dataset that links individuals' socio-economic information, collected all along their life, to their parents' information. Since 1968, the EDP gathers data on individuals born during the first four days of October from various administrative sources.[1] In this analysis, I make use of EDP's census data, employer-employee panel data, and tax data.

Exhaustive population censuses were collected every 7 to 9 years in France until 1999. Since 2004, about 20% of dwellings are censused every year, such that any set of 5 consecutive yearly census surveys constitutes a complete census wave. Census information includes indi-

[1] Other birth dates were added to the scope of the EDP in 2006: the first four days of April and July, as well as January 2$^{\text{nd}}$ to 5$^{\text{th}}$. For individuals born during these days, administrative documents issued before 2006 were not retroactively added to the dataset. Thus, the EDP sample used throughout this analysis covers individuals born during the first four days of October exclusively.

viduals' socio-demographics, characteristics of where they live, but not their earnings. EDP's census data includes information on individuals born on the first 4 days of October, and some information on their household members. I use the 1990 census wave to observe childhood characteristics of EDP individuals born in metropolitan France between 1972 and 1984, and adulthood characteristics of their parents.[2]

The All Employee Panel gathers the labor earnings and job characteristics of employees until 2018. It does not include information on the self-employed nor on farmers. It covers the private sector since 1968, but public-sector jobs were progressively added to the database throughout the 1980s. Until 2001, only individuals born in an even year were included. I use this data source to measure parents' position in the income distribution.

Tax returns include detailed income information on all individuals known by the tax authorities either via an income tax form or via a housing tax form. Tax information is available in the EDP from 2010 to 2019. I use this data source to measure individuals' positions in the income distribution in the children's generation.

The baseline sample consists of all individuals born in metropolitan France during the first 4 days of October between 1972 and 1984, observed as a depend child to parents neither farmer nor self-employed in the 1990 census, and observed at least once in tax data between ages 35 and 45. It gathers 85,701 individuals. Appendix Table B.1 documents the evolution of the sample size after each restriction.

2.2.2 Variable definitions

Origins. I define individuals' origins based on their parents' places of birth as observed in the 1990 census. French data does not include information on race or ethnicity. Hereinafter, I use the word *immigrant* to refer to someone born abroad. There are two groups of emigration countries that are large enough to be considered separately from other origins: South Europe (Italy, Portugal, and Spain), and North Africa (Algeria, Morocco, and Tunisia). Given the under-representation and the heterogeneity of other origins, I gather all other birth countries in a third group of origins.

When an individual grew up with a lone parent, the birth country of the absent parent is usually not available in the data. In such cases, I use the country of birth of the lone parent to determine the origin group of the individual. I assign individuals with mixed origins to separate groups, following the classification detailed in Appendix Table B.2. The joint distribution of fathers' and mothers' places of birth is reported in Table 2.1.

84% of the sample have a mother born in France, and 70% have both parents born in France. 5% of individuals have both parents born in North Africa, and 2% both in South Europe. Mixed origins are relatively common, with 10% of individuals having one parent born in France and

[2]The sample is restricted to cohorts 1972 to 1984 because individuals born in earlier cohorts would be adults at the moment of the 1990 census, and individuals born in later cohorts would not be observed in tax data at age 35 or older.

Table 2.1: Distribution of parents' places of birth

	Mother's place of birth					
↓ Father's	France	North Africa	South Europe	Other	Absent	Total
France	70.14	1.98	1.05	1.23	1.03	75.43
North Africa	2.92	4.83	0.12	0.08	0.10	8.05
South Europe	1.49	0.10	2.30	0.04	0.04	3.96
Other	1.23	0.08	0.03	1.35	0.04	2.73
Absent	8.56	0.69	0.25	0.33	-	9.83
Total	84.34	7.68	3.74	3.04	1.20	100

Notes: This table shows the joint distribution of mothers' and fathers' places of birth in the baseline sample. It consists of all individuals born in metropolitan France during the first 4 days of October between 1972 and 1984, who are observed as a depend child in the 1990 census with parents neither farmer nor self-employed, and who are observed at least once in tax data (85,701 individuals). Parents' places of birth are observed in the 1990 census, when individuals were between 5 and 18 years old. North Africa includes Algeria, Morocco, and Tunisia, and South Europe includes Italy, Portugal, and Spain.

Reading: In 1990, 1.98% of children aged 5 to 18 who were born in France had a father born in France and a mother born in North Africa.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

the other born in a different country. Overall, 20% of the sample have a least one parent born abroad.

Income. I observe individuals' incomes in tax data, which is available from 2010 to 2019. Income is defined at the individual level as the sum of yearly wages and benefits (agricultural, industrial, commercial, non-commercial), averaged over ages 35 to 45. Robustness to the age window is assessed in Section 2.5.1. Incomes, as well as all the other monetary variables used in the analysis, are expressed in real 2015 euros.

Hourly wage. The only data source containing the number of hours worked is the All Employee Panel, where wages of employees are gathered. I define hourly wage as the annual pretax wage divided by the annual numbers of hours worked, averaged over ages 35 to 45. I consider only observations starting in 2010 to match with the period covered by tax data. By definition, hourly wage is available for wage earners only.[3]

Parents income. The structure of the data does not generally enable the direct link between earnings information from one generation to another, except in the rare cases where both parents were also born during the first four days of October. Still, the EDP has followed individuals' earnings within generations for long enough to cover both the parents' genera-

[3]About 8% of income earners in the sample did not earn any wages over the period.

tion and the children's generation. Following Kenedi and Sirugue (2023), I rely on the fact that predictors of parents' earnings are observed in the census survey collected in 1990 when individuals were aged between 5 and 18. More specifically, I predict separately fathers' earnings and mothers' earnings based on birth cohort, birth nationality, place of birth, education level, detailed occupation, household structure, and the average socio-economic characteristics in the municipality of residence. The predicted outcome is the annual pretax wage averaged over ages 35 to 45. Robustness to the age window is assessed in Section 2.5.1.

The prediction model is calibrated on parents born during one of the first four days of October, with at least one child born in metropolitan France between 1972 and 1984 who was still part of their household in the 1990 census. Only parents with at least two earnings observations in the All Employee Panel were kept to calibrate the model. Robustness to the minimum number of wage observations is assessed in Section 2.5.1. The adjusted R^2 amounts to 0.36 for both mothers and fathers. Appendix Figure B.1 shows the average out-of-sample income ventile predictions against actual income ventiles for each origin group.

Parameter values obtained from the first-stage regressions are applied to the characteristics of EDP individuals' parents observed in the 1990 census to obtain a prediction of parents' income. Because household structure is only observed in 1990, summing the incomes of both parents would over-penalize households that are transitorily single-headed. Thus, I take the average of the two predictions when an individual grew up with both parents, and the prediction of the lone parent otherwise.

Income quantiles. I characterize individuals' positions in the income distribution according to the income quantiles they fall into. I use either percentiles, ventiles, or deciles, depending on sample size. Children are ranked within their birth cohort, and parents are ranked within the birth cohort of their child.

Urban unit. A municipality, or a group of municipalities, forms an urban unit if it fulfills two criteria. First, it must form a contiguously built-up area with no distance greater than 200 meters between two constructions. Second, it must gather at least 2,000 inhabitants. Urban units are not bounded by borders of larger administrative units such as departments. Appendix Figure B.2 shows the map of urban units as of 1990. These 1,891 urban units gather 15% of all municipalities, and 74% of the population. The largest urban unit is that of Paris, which gathers 398 municipalities. Overall, most urban units are rather small. 90% of them are composed of less than 5 municipalities, and 50% of them consist of a single municipality. The distributions of the number of inhabitants across urban units and municipalities are shown in Figure B.3 Panels A and B.

Neighborhood. In 1990, municipalities of more than 5,000 inhabitants were divided into smaller spatial units called "Ilôts". These areas roughly correspond to the contemporaneous

"IRIS" statistical areas delineated by the French Institute for Statistics and Economic Studies (INSEE). For illustrative purposes, Appendix Figure B.4 shows which municipalities were divided into IRIS. Appendix Figure B.5 represents the "Ilôt" neighborhood division of three urban units. Neighborhoods typically contain from a few hundred inhabitants, in most municipalities, to a few thousand inhabitants, in large municipalities. All urban units are divided into neighborhoods. 92% of municipalities that are large enough to be divided into neighborhoods belong to an urban unit. The distribution of the number of inhabitants across neighborhoods is shown in Figure B.3 Panel C.

Building. The characterization of buildings is based on their postal addresses. Even though this typically matches with the material definition of a building, two separate constructions, adjoining or not, would be considered as the same building if they share a common address in which housing units are numbered jointly. About half of the population living in urban units reside in a building with multiple dwellings.

Building identifiers were collected in the exhaustive 1990 census, but they were not included in the linked EDP census data. Still, for 98% of the sample living in urban units, the neighborhood of residence, gender, and date of birth, are enough information to exactly match the building identifier. The distribution of the number of inhabitants across buildings is shown in Figure B.3 Panel D.

2.3 Intergenerational mobility gaps across origins

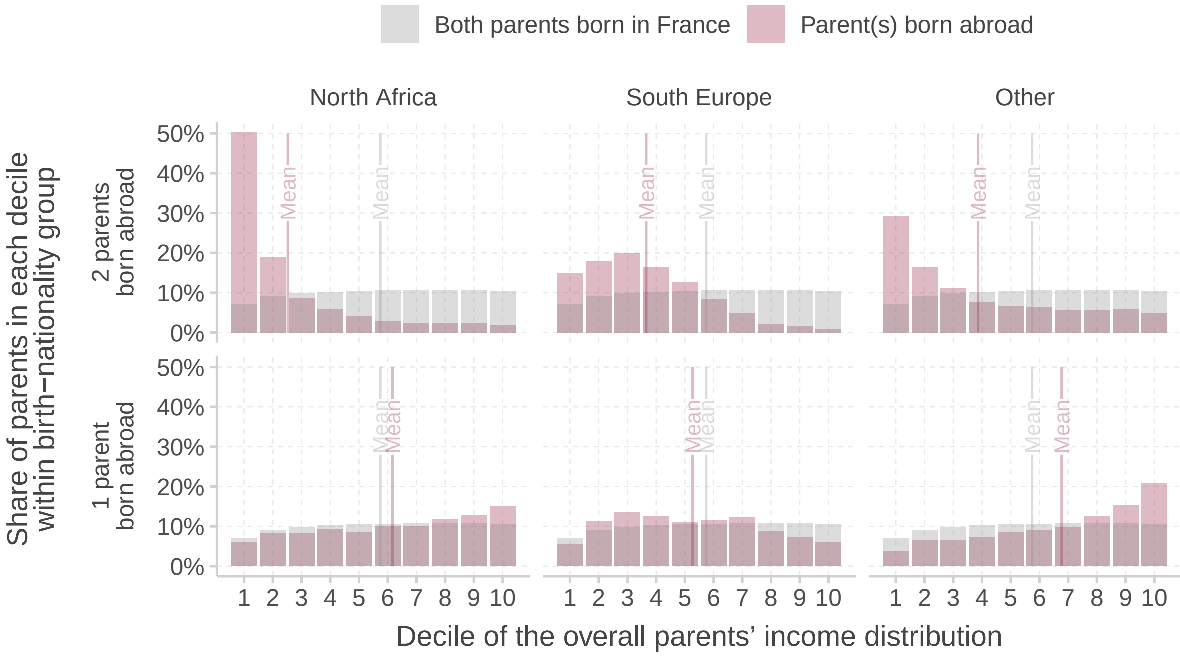
2.3.1 Parents' income background

This section documents the heterogeneity in parents' income background across origin groups. Figure 2.1 compares the distribution of parents' deciles in the overall income distribution among each origin group. If native parents and immigrant parents of each origin group were evenly positioned in the overall income distribution, there would be 10% of parents in each decile for every group.

The gray bars show the distribution of native parents' income deciles. Only 7% of native parents pertain to the first decile. They are also slightly under-represented in the second and third decile, and over-represented above. Native parents locate at the 58th percentile of the income distribution on average, as illustrated with the gray vertical line. The distribution of native parents' income deciles is replicated on every panel as a reference. On each panel, red bars show the distributions of income deciles among immigrant parents of each origin group. Vertical red lines show the average positions of parents from each origin group in the income distribution.

Parents born in North Africa are highly concentrated at the bottom of the income distribution. Half of them pertain to the first decile, and 78% of them pertain to the first 3 deciles.

Figure 2.1: Parents' positions in the national income distribution



Notes: This graph shows the distribution of parents' deciles in the overall income distribution across origin groups. Within each panel, the 10 bars of a given color sum to 1. Gray bars represent the distribution of native parents across income deciles. These are replicated on every panel as a reference. The gray vertical line corresponds to the average position of native parents in the overall income distribution. In each panel, red bars represent the distribution of parents' income deciles for the corresponding origin, and the red vertical line corresponds to their average position in the income distribution.

Reading: 50% of parents who were both born in North Africa pertain to the first decile of the national income distribution. 7% of parents who were both born in France pertain to this income decile.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

This results in a gap of 3 deciles between the average position of parents born in North Africa and the average position of natives. Parents born in South Europe are over-represented in the bottom half of the distribution as well, but they are much less concentrated in the very first deciles. Overall, 82% of them are located below the median, resulting in a gap of 2 deciles with natives.

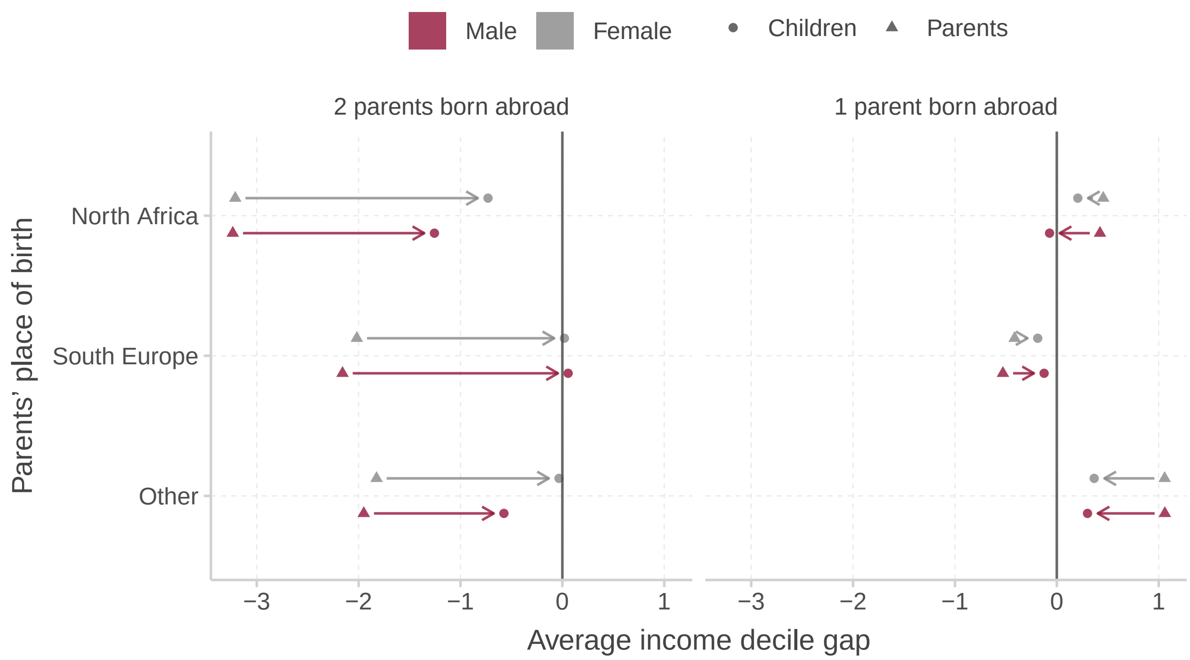
The top-right panel shows that parents with other foreign origins tend to be less under-represented within top deciles, but they still face an average income gap of about 2 deciles. Bottom panels indicate that when one of the two parents was born in France, parents' income positions in the overall income distribution are much closer to those of natives; generally even better, except when one of the parents was born in South Europe.

2.3.2 Unconditional income gap

In this section, I investigate the extent to which the income gap between immigrants and natives persists from the first generation to the second generation.

Figure 2.2 shows the evolution of the income-rank gap between the two generations for each origin group, separately for daughters and sons. Triangles represent the income-decile gaps between immigrant parents and native parents. Corresponding regression results, with parents' income decile regressed on the group of origin, are reported in Appendix Table B.3 Columns (1) and (4). Dots represent the income-decile gaps between children of immigrants and children of natives. Corresponding regression results, with children's income decile regressed on the group of origin, are reported in Appendix Table B.3 Columns (2) and (5). The left panel of the figure shows results for individuals whose parents were both born abroad, and the right panel shows results for individuals born to one native parent and to one immigrant parent. On both panels, a vertical solid line is placed at 0, where there would be no gap. Gaps located on the left-hand side of that threshold indicate that individuals of the corresponding origin tend to end up lower than natives in the income distribution.

Figure 2.2: Evolution of the income gap from the 1st to the 2nd generation



Notes: This graph shows the evolution of the immigrants' income gap from the 1st generation to the 2nd generation. Triangles show the gaps in average income decile between parents that were both born abroad and parents of each origin group. Dots show the gaps in average income decile between children of native parents and children of parents from each origin group. All the gaps are computed separately for second-generation males, in red, and for second-generation females, in gray. Within each gender and origin group, an arrow connects the gap in the first generation to the gap in the second generation. A vertical solid line is placed at 0, where there would be no gap. Gaps located on the left-hand side of that threshold indicate that individuals of the corresponding origin tend to end up lower than natives in the income distribution.

Reading: Among daughters of immigrants born in North Africa, parents' income decile is 3.2 deciles lower than among daughters of natives. The gap in average income decile between daughters of immigrants from North Africa and daughters of natives amounts to 0.7.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

As shown in Figure 2.1, the income-decile gaps between native parents and parents who were both born abroad range from 2 to 3 deciles. However, their children systematically end up much closer to each other in the income distribution of their generation. This is particularly true among daughters, as they fully converged in every group except North Africa. Sons of South European immigrants also caught up completely with natives, but a 12-percentile gap persists for sons of North-African immigrants. The distribution of income deciles in the overall income distribution across origin groups is shown in Appendix Figure B.6. It shows that groups that have converged in means have also converged in distributions, and that the remaining gap for sons of North-African immigrants is primarily driven by the first two deciles of the overall income distribution.

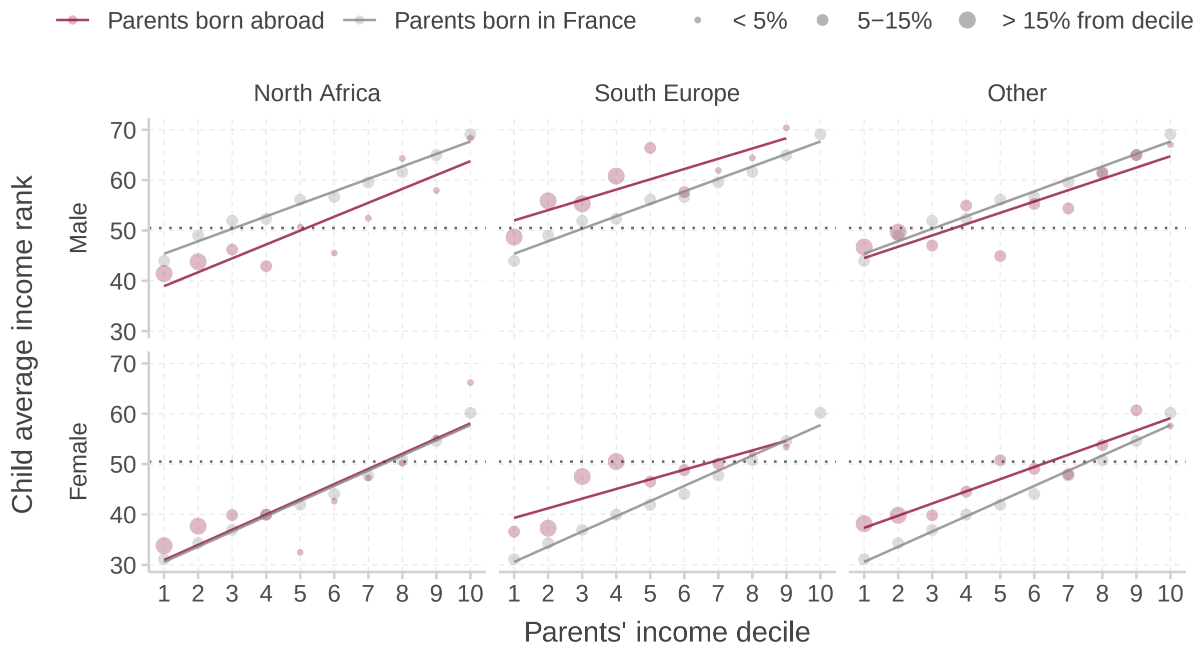
2.3.3 Conditional income gap

This section explores the extent to which the income rank gap between children of immigrants and children of natives can be explained by the initial differences in parents' positions in the income distribution.

Figure 2.3 shows the average percentile income rank of children from each parental income decile for each group of origin, separately for males and females. In each gender/origin panel, red dots show the conditional average outcomes among the corresponding groups of children of immigrants. Conditional average outcomes of natives are shown with gray dots as a reference. Straight lines represent the associated regression fits. Corresponding regression results are reported in Appendix Table B.4. Conditional average income rank gaps, obtained from regressing children's income percentile on the group of origin and on parents' income decile, are reported in Appendix Table B.3 Columns (3) and (6). The same figure with third-order polynomial fits instead of linear fits is presented in Appendix Figure B.7.

When comparing within parents' income decile, the average income rank of children of immigrants increases relative to that of children of natives for each origin group. For instance, the overall income gap between sons of natives and sons of immigrants from North Africa is reduced to 5 percentiles on average. The remaining gap is more pronounced in the lower part of the parents' income distribution, and is virtually closed at the top. The gap faced by daughters of North-African immigrants, however, almost vanishes along most of the parents' income distribution and even flips to a positive gap at the bottom. Children of South European origin tend to outperform children of natives conditional on parents' income decile. This is why no unconditional gap is observed for the second generation in Figure 2.2, despite the large differences in parents' income backgrounds documented in Figure 2.1. Appendix Figures B.8 and B.9 show the same figure for individuals born to one native parent and one immigrant parent with linear and polynomial fits. For them, virtually no difference in conditional income ranks relative to children of natives is observed.

Figure 2.3: Average income ranks across parents' income deciles



Notes: This figure shows the expected income rank among children from each parental income decile, separately for children of natives, in gray, and for children of parents of each origin group, in red. This figure is restricted to individuals whose parents were either both born in France or both born abroad. See Appendix Figure B.8 for the same results on children of one parent born in France and one parent born abroad. Average income ranks are estimated separately among sons (upper panels), and daughters (lower panels). For each gender, results for children of natives are replicated on each panel as a reference. Straight lines show the corresponding regression fits. Horizontal dotted lines represent the average income rank in the income distribution.

Reading: Sons of natives whose parents were located in the 3rd decile of the overall income distribution reach the 53rd percentile of the overall income distribution on average. Sons of parents born in North Africa located in the 3rd decile of the overall income distribution reach the 46th percentile of the overall income distribution on average.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

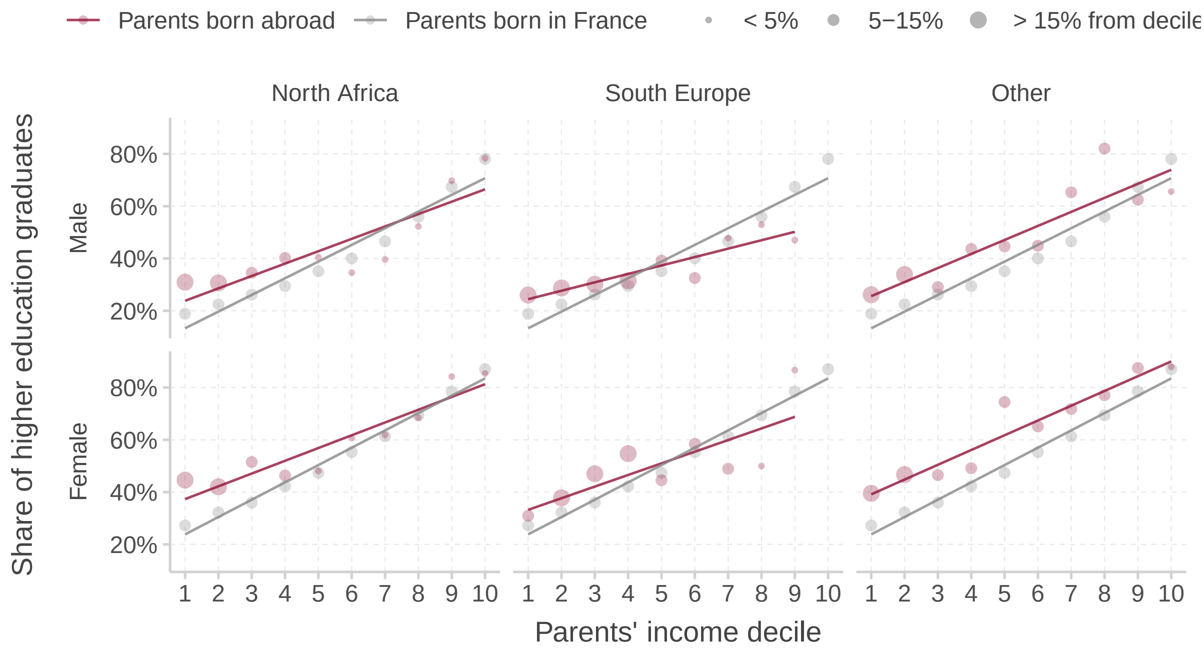
2.3.4 Conditional education gap

This section investigates conditional gaps in education levels as a natural candidate channel to the variations in the gaps in conditional income rank.

The highest diploma obtained by individuals is observed in annual census surveys for 89% of the sample. Figure 2.4 shows the share of children who graduated from higher education from each parental income decile, by gender and origin group. The same results for children born to one native and one immigrant parent are presented in Appendix Figure B.10. Alternative representations with polynomial fits are presented in Appendix Figures B.11 and B.12. Regression results are reported in Appendix Table B.5.

Figure 2.4 reveals a strong relationship between parents' income decile and children's educational attainment. While the share of higher education graduation varies from 20% to 40% among children from the first parental income decile, it ranges from 60% to 80% among

Figure 2.4: Higher education graduation rates across parents' income deciles



Notes: This figure shows the share of children who graduated from higher education from each parental income decile, separately for children of natives, in gray, and for children of parents of each origin group, in red. This figure is restricted to individuals whose parents were both born either in France or abroad. See Appendix Figure B.10 for the same results on children of one parent born in France and one parent born abroad. Average income ranks are estimated separately among sons (upper panels), and daughters (lower panels). For each gender, results for children of natives are replicated on each panel as a reference. Straight lines show the corresponding regression fits. Horizontal dotted lines represent the average income rank in the income distribution.

Reading: Among sons of natives whose parents were located in the 3rd decile of the overall income distribution, 26% graduated from higher education. Among sons of immigrant parents from North Africa who were located in the 3rd decile of the overall income distribution, 35% graduated from higher education.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

children from the top deciles of parental income. Both sons of immigrants and daughters of immigrants graduate from higher education to a larger extent than children of natives at the bottom of the parents income distribution. The higher the parents' income background, the lower the gap, and not much difference is observed at the top of the distribution.

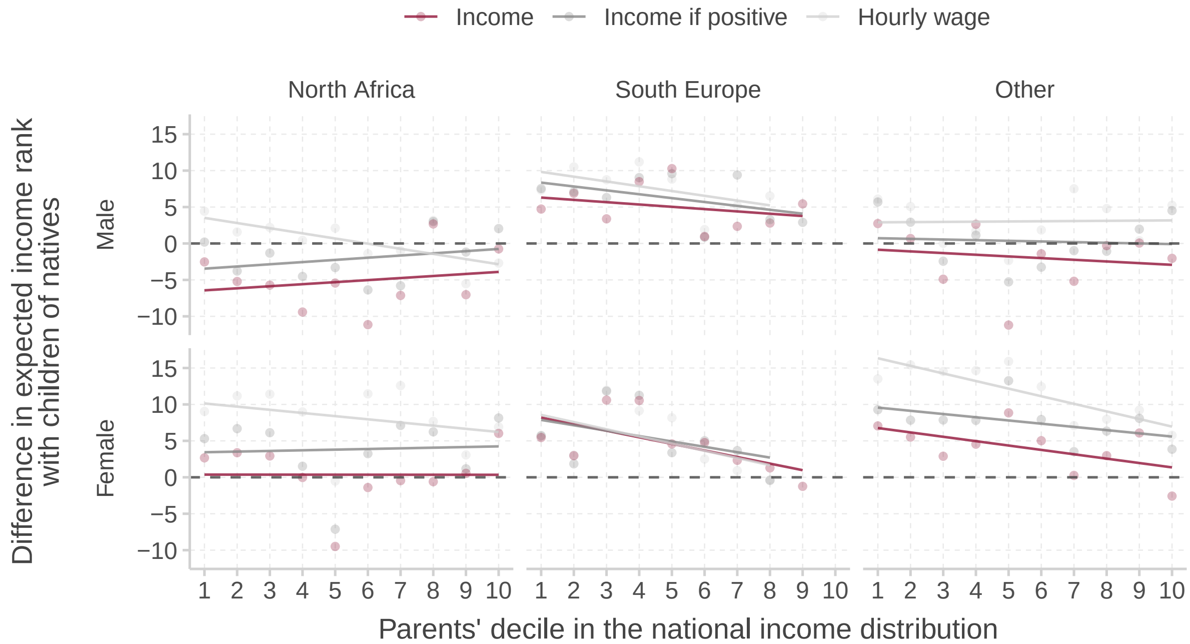
Interestingly, sons of immigrants from North Africa also graduate from higher education more than children of natives, to an extent that is comparable to other children of immigrants. Hence, education levels do not seem to explain the persistent conditional income gap observed only among sons of North African immigrants. Appendix Figures B.13 to B.16 show that this pattern holds for masters' graduation, although gaps with natives are much smaller.

Even though the quantity of education does not seem to be at play, I cannot evaluate whether the quality of education is a likely mechanism because no information on the field of the degree or on the higher education institution is available in the data.

2.3.5 Hours worked vs. hourly wage

To better understand the nature of the only remaining gap—that between sons of natives and sons of North-African immigrants—I compare the conditional income gaps using several income definitions in Figure 2.5. Specifically, I progressively isolate the contribution of hourly wage from that of hours worked shifting from the baseline conditional income-rank gap to the gap excluding zero incomes, and the gap in terms of hourly wage. The same figure with third-order polynomial fits instead of linear fits is presented in Appendix Figure B.17.

Figure 2.5: Income rank gaps with children of natives along the parents income distribution



Notes: This figure shows the gap in percentile ranks between children of natives and children of immigrants from each origin group for varying income definitions, separately for daughters and for sons. Parents' income deciles are represented on the x-axis and the corresponding average income-rank gaps between children of immigrants and natives are shown on the y-axis. The panel position indicates the gender/origin group, and the color of the dots indicates the income definition used to compute the income-rank gap. Straight lines represent the corresponding regression fits. A horizontal dashed line is placed at 0, where there would be no gap between children of natives and children of immigrants.

Reading: Among individuals whose parents pertain to the third decile of the income distribution, there is a 6.4 percentile income-rank gap between sons of North African immigrants and sons of natives. Computed on strictly positive income observations only, this gap shrinks to 1.7 percentiles. In terms of hourly wage, the average outcome is slightly larger for sons of North African immigrants, with a 2-percentile income-rank gap.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

The change in income definition has very different impacts on the conditional gap depending on the origin group. For children of immigrants from South Europe, changing the income definition makes virtually no difference. For children of North African immigrants, the restriction to strictly positive incomes raises the conditional expected income ranks with respect to children of natives. It raises even more when considering hourly wage instead, to the point that the gap gets completely closed for sons. Daughters of North-African parents, who experience virtually no conditional income gap, out-perform daughters of natives in terms of conditional hourly wage almost all along the parents' income distribution. Appendix Figures B.18 and B.19 show the same figure for individuals born to one native parent and to one immigrant parent with linear and polynomial fits. For them as well, shutting down the working time channel increases the conditional expected outcomes relative to children of natives.

2.4 The role of residential segregation

This section investigates residential segregation, as experienced during childhood, as a potential determinant of the remaining income gap between sons of natives and sons of immigrants from North Africa.

2.4.1 Share of immigrants

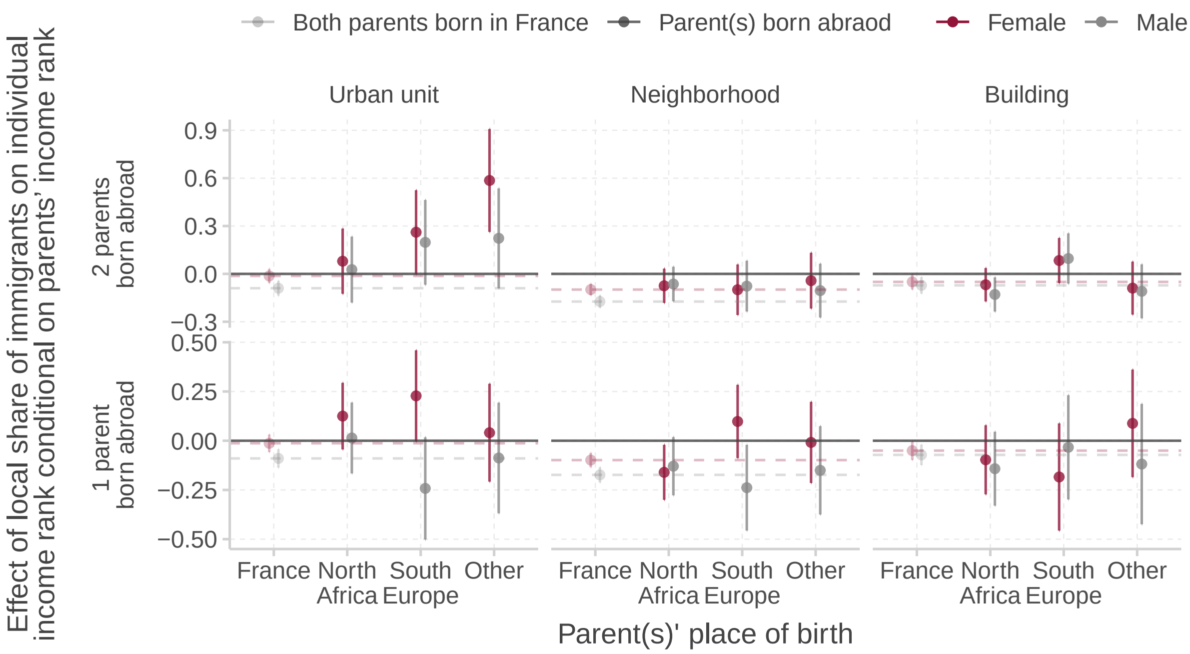
Before measuring residential segregation per se, I use the exhaustive 1990 census to compute the share of immigrants living in each urban unit, neighborhood, and building. Spatial variations in the share of immigrants across urban units are represented in Appendix Figure B.20.

Appendix Figure B.21 shows the average share of immigrants in individuals' area of residence for each origin group at each spatial level. Among children of natives, the average share of immigrants in the neighborhood of residence is 6%, and it is 8% in the building of residence. For children of immigrants, the average share of immigrants is 16% at the neighborhood level, and 26% at the building level. This pattern is homogeneous across groups of origins as long as both parents were born abroad. Children with one native parent and one immigrant parent tend to reside in areas where shares of immigrants are much closer to those that children of natives typically experience.

Note that the sample on which these shares are computed varies depending on the spatial unit considered. Shares at the urban unit level are computed on the whole sample. At the neighborhood level, individuals considered are those living in municipalities of at least 5,000 inhabitants, which are hence divided into statistical neighborhoods. I compute building-level statistics on individuals whose building of residence contains several housing units. Given that immigrants are more likely to reside in large cities, and also more likely to reside in apartments than in houses, the average share of immigrants increases mechanically as the spatial level considered gets smaller. To ensure that the observed patterns are not driven by compositional changes, I reproduce the figure using only individuals who live in buildings containing several housing units. Results, shown in Appendix Figure B.22, are almost identical.

To investigate the relationship between the local share of immigrants and the conditional income gap, I regress individuals' percentile income rank on the share of immigrants in their area of residence, controlling for parents' income rank. Figure 2.6 shows the corresponding coefficients, estimated separately by gender and origin group. The left, middle, and right panels respectively correspond to specifications where the share of immigrants is estimated at the urban unit, neighborhood, and building level. Upper panels show the results for individuals whose parents were both born abroad, and lower panels for children of one native and one immigrant parent. Results for children of two native parents are shown with a lighter color on each panel as a reference.

Figure 2.6: Relationship between conditional income rank and local share of immigrants



Notes: This figure shows the regression coefficients, and the corresponding 95% confidence intervals, obtained from regressing percentile income rank on the share of immigrants in the area of residence, controlling for parents' income rank. The regression is estimated separately for each gender and origin group, and considering three different spatial levels for the area of residence. Upper panels show results for children of two immigrant parents, and lower panels show results for children of one immigrant parent and one native parent. The column of the panel indicates the spatial level considered to compute the local share of immigrants. The x-axis indicates the region of origin of individuals' parent(s). Results for daughters are depicted in red, and results for sons are depicted in gray. Results for children of two native parents are represented in lighter colors with dots and horizontal dashed lines. A solid horizontal line is placed at 0, where there would be no relationship between the share of immigrants in the area of residence and individuals' income ranks conditional on their parents' income ranks.

Reading: Controlling for parental income rank, a 1 percentage-point increase in the share of immigrants in the building of residence is associated with a 0.13 percentile decrease in income rank for sons of natives. A barely smaller coefficient is observed for daughters of natives.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP, and *Full Population Census*, main sample, wave 1990, INSEE.

A one percentage point higher share of immigrants in the area of residence is associated with lower income ranks for children of natives conditional on parental income, by 0.10 to 0.25 percentiles depending on whether the area considered is the municipality, the neighborhood, or the building. This is coherent with the fact that lower-income individuals tend to locate in more affordable places, where immigrants are over-represented. The negative conditional relationship between local shares of immigrants and income ranks tends to be less pronounced for children of immigrants, and for daughters in general. Overall, the spatial level at which the share of immigrants has the largest negative relationship with conditional income rank is the neighborhood level.

2.4.2 Residential segregation

In this section, I estimate how the conditional relationship between income rank and the share of immigrants in the area of residence interacts with residential segregation.

I measure residential segregation between immigrants and natives across neighborhoods of a given urban unit with the Duncan dissimilarity index:

$$\mathrm{S}_U = \frac{1}{2}\sum_{n=1}^{N}\left|\frac{\mathrm{Imm}_n}{\mathrm{Imm}_U} - \frac{\mathrm{Nat}_n}{\mathrm{Nat}_U}\right|, \tag{2.1}$$

where Imm_n and Nat_n denote the number of immigrants and natives in neighborhood n, and Imm_U and Nat_U denote the total number of immigrants and natives in urban unit U. This index captures how unevenly immigrants and natives are distributed across neighborhoods, from 0 to 1. If the shares of immigrants and natives are respectively the same in each neighborhood, then the two fractions of Equation 2.1 would systematically be equal and the index would be 0. If neighborhoods contain either only natives or only immigrants, then both fractions would independently sum to 1 and the index would be 1.

I compute this index on the exhaustive 1990 census such that it captures the segregation levels experienced by individuals during their childhood. The spatial levels considered, urban units and neighborhoods, are defined as described in Section 2.2.2. Appendix Figure B.23 shows the spatial variations of this index across urban units. Residential segregation between immigrants and natives tends to be lower in the South and in the East, and higher in the North and in the West. Variations in the index are relatively large, both among small urban units and among large urban units (e.g., 0.15 in Toulouse and 0.37 in Le Havre). Overall, larger urban units tend to be more segregated than smaller urban units.

To get a sense of how residential segregation enters the conditional relationship between the share of immigrants in the area of residence and income ranks, I estimate the following regression:

$$\text{Income rank}_i = \alpha + \beta \mathrm{Imm}_n + \delta\frac{\mathrm{S}_U}{\mathrm{SD}(\mathrm{S}_U)} + \gamma \mathrm{Imm}_n \times \frac{\mathrm{S}_U}{\mathrm{SD}(\mathrm{S}_U)} + \eta \text{Parents' rank}_i + \varepsilon_i, \tag{2.2}$$

where Income rank_i denotes individual i's percentile income rank in the income distribution, Imm_n denotes the share of immigrants in childhood's neighborhood of residence n, S_U denotes the Duncan dissimilarity index between immigrants and natives in childhood's urban unit of residence U, and Parents' rank_i denotes individual i's parents' percentile income rank. I estimate this regression separately by gender and origin group.

Regression results are presented in Appendix Tables B.6 and B.7 for sons, and in Appendix Tables B.8 and B.9 for daughters. For children of natives, the previously documented negative conditional relationship between the share of immigrants in the neighborhood of residence and income rank is entirely absorbed by the interaction between the share of immigrants and residential segregation. In terms of magnitude, a one standard deviation increase in residential segregation is associated with a decrease of 0.1 percentile in the income rank variation expected from a 1 percentage point increase in the share of immigrants in the neighborhood of residence during childhood. However, no significant effect is observed for both sons and daughters of immigrants from South Europe and from North Africa. Naturally, no causal interpretation can be derived from these coefficients as the relationship is likely to be affected by confounding factors.

2.4.3 Instrumented relationship

This section proposes an instrumental variable approach to tackle the endogeneity of the relationship between residential segregation and income rank conditional on parents' income across origins.

Following Chyn et al. (2022), I construct a spatial division index based on how geographical barriers partition urban units. The underlying idea is that the way in which these spatial features divide space within urban units must impact residential segregation, but not intergenerational mobility across origins except through residential segregation.

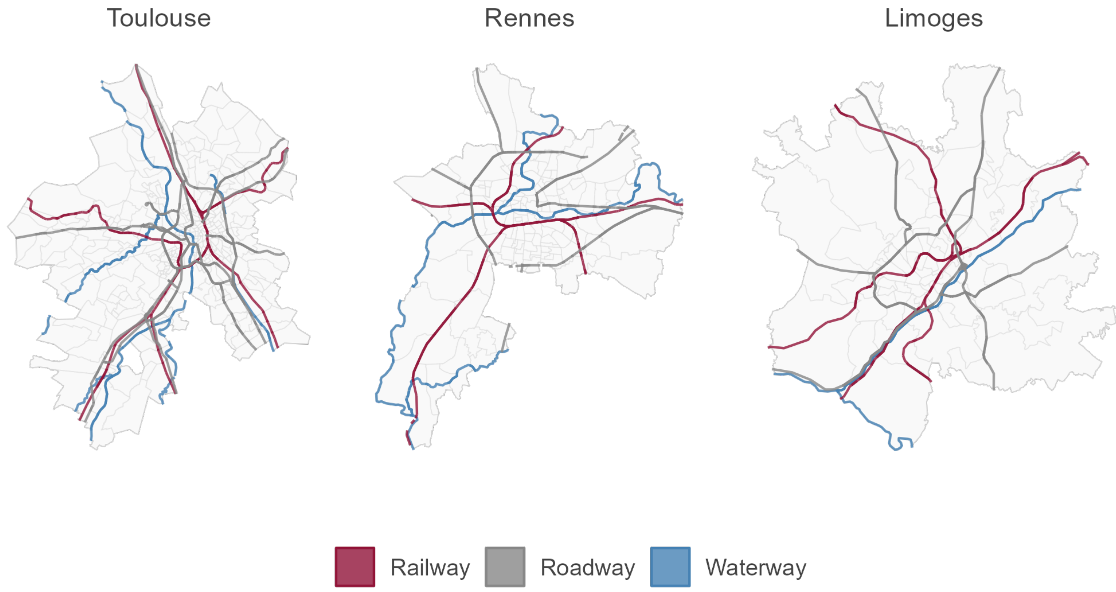
Specifically, I consider the combination of railways, waterways wider than 15 meters, and national highways. The data source I use for these three spatial features is *ROUTE 500*, produced by the French National Institute for the Geographic and Forest Information (IGN). I use the earliest version of this data source, produced in 1999, to limit the risk of using features that did not exist when residential segregation is measured, in 1990. A map of these spatial features for the whole territory is presented in Appendix Figure B.24. Figure 2.7 maps these spatial features at the local level for the urban units of Toulouse, Rennes, and Limoges.

The spatial division index writes as follows.

$$\text{Division index}_U = 1 - \sum_{s=1}^{S} \left(\frac{\text{area}_s}{\text{area}_U} \right)^2, \text{ with } \sum_{s=1}^{S} \text{area}_s = \text{area}_U, \tag{2.3}$$

where area_U is the total area in the urban unit, and area_s is the area in subdivision s. The index is designed to capture how the area in urban unit U is unequally distributed across

Figure 2.7: Spatial division features considered in three urban units



Notes: This figure shows the three geographical features considered for the computation of the spatial division index, in the urban units of Toulouse, Rennes, and Limoges. Railways are represented in red, national roadways are represented in dark gray, and waterways wider than 15 meters are represented in blue. Neighborhood borders are represented in light gray.

Source: *GEOFLA*®, wave 1997, IGN, and *Route 500*®, wave 1999, IGN, and *CONTOURS... IRIS*®, wave 2009, IGN-INSEE.

the smaller units s delimited by the geographic barriers considered. The index would be equal to 0 for an area which is not divided at all. The higher the number of divisions, and the more unequal the distribution of the total area between the subdivisions, the closer to 1 the index. The spatial distribution of the division index across urban units is represented in Appendix Figure B.25. The index varies substantially across urban units, and tends to be larger in more populated areas. Still, large variations are observed across urban units both below and above median population size, with inter-quartile ranges of 0.53 and 0.34 respectively. Unlike for residential segregation, no clear distinction is observed between the South-East and the North-West.

For this instrumental variable approach to be valid, it is necessary that the relationship between the spatial division index and residential segregation is strong enough. Table 2.2 shows the results of the regression of segregation index on the spatial division index.

Column (1) shows that a 1 point increase in the spatial division index is associated with a significant 0.088 point increase in the Duncan dissimilarity index. The F-statistic of the model amounts to 136. The relationship between the two variables is robust to the inclusion of controls for the total length of railway, roadway, and waterway in the urban unit, as shown

Table 2.2: First-stage relationship

	Segregation index				
	(1)	(2)	(3)	(4)	(5)
Spatial division index	0.088***	0.072***	0.082***	0.082***	0.072***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Constant	0.157***	0.158***	0.158***	0.158***	0.157***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Waterway length		✓			✓
Railway length			✓		✓
Roadway length				✓	✓
Obs. (Urban units)	1,891	1,891	1,891	1,891	1,891
R^2	0.07	0.08	0.07	0.07	0.08
F-stat.	136.6	82.2	72.9	72.4	41.6

Notes: This table shows the results of the regression of the Duncan dissimilarity index, defined in Equation 2.1 on the spatial division index, defined in Equation 2.3, at the urban unit level. Check marks indicate which of the variables listed in the middle section of the table are included as control variables in the specification of the corresponding column. Standard errors are reported in parentheses. Statistical significance is reported according to the following symbology. P-val.: $^{***} < 0.01 \leq^{**} < 0.05 \leq^{*} < 0.10$.

Reading: At the urban unit level, a 1 point increase in the spatial division index is associated with a 0.088 point increase in the Duncan dissimilarity index. This coefficient is statistically significant at the 99% confidence level.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP, *GEOFLA®*, wave 1997, IGN, and *Route 500®*, wave 1999, IGN.

in Columns (2) to (5). The relationship is represented graphically in Appendix Figure B.26.

For the instrumental approach to be valid, in addition to the relationship being sufficiently strong, there must be no effect of the spatial division index on individuals' income ranks conditional on their parents' income ranks across origins other than through residential segregation. Still, proximity to a railway network could have direct effects on intergenerational mobility, as shown for Argentina (Pérez, 2018) and England and Wales (Costas-Fernández et al., 2020). Hence, I control for the length of railways, roadways, and waterways in urban units. In doing so, I assume that holding the length of railways, roadways, and waterways constant, the way in which these features divide space more or less equally into more or less sub-units has no effect on intergenerational mobility other than through residential segregation. I perform further robustness checks on this issue in Section 2.5.2.

Table 2.3 shows the second-stage results of regression Equation 2.2 where the (standardized) Duncan dissimilarity index of residential segregation is instrumented with the (standardized) spatial division index, for males whose parents were either both born in France or both born abroad. The regression is estimated separately for each origin group, and separately for individuals who grew up in urban units with less than 50,000 inhabitants and individuals who grew up in larger urban units. Robustness to this threshold is examined is Section 2.5.3. About one third of all urban units pertains to the larger group, and it gathers about two thirds of the population. The same results for females are presented in Appendix Table B.10, and results for individuals born to one native parent and one immigrant parent are presented in Appendix Tables B.11 and B.12. Corresponding naive OLS regressions are shown in Appendix Tables B.13 to B.16.

Table 2.3: Instrumented effect of residential segregation on the conditional relationship between the local share of immigrants and income rank

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	-1.40	-0.88	2.90	1.68^{*}	5.16^{**}	-1.19
	(1.323)	(1.964)	(2.844)	(0.967)	(2.533)	(2.514)
$\widehat{\text{Segregation}}$	-11.33	1.39	49.21	4.91	44.94^{*}	-6.32
	(13.666)	(14.665)	(30.384)	(3.806)	(23.573)	(15.312)
%Imm. $\times$ $\widehat{\text{Seg.}}$	0.39	0.29	-1.14	-0.68^{**}	-1.90^{**}	0.39
	(0.401)	(0.645)	(0.983)	(0.349)	(0.905)	(0.946)
Parents' rank	0.26^{***}	0.29^{**}	0.13	0.25^{***}	0.27^{***}	0.17^{***}
	(0.017)	(0.135)	(0.166)	(0.011)	(0.065)	(0.056)
Constant	70.67^{**}	43.88	-51.95	29.44^{**}	-88.38	70.91
	(31.396)	(33.813)	(67.250)	(12.217)	(70.344)	(44.893)
Waterway length	✓	✓	✓	✓	✓	✓
Roadway length	✓	✓	✓	✓	✓	✓
Railway length	✓	✓	✓	✓	✓	✓
Observations	8,529	476	271	15,120	1,750	708
F-stat. Seg.	31.61	3.17	3.63	440.80	24.17	21.56
F-stat. Seg. $\times$ Imm.	420.21	3.91	14.97	265.55	24.19	13.33

Notes: This table shows the results of the regression of percentile income rank on (i) the share of immigrants in the urban unit the individual grew up in, (ii) the Duncan dissimilarity index of residential segregation instrumented by the spatial division index, and (iii) the interaction between these two variables, controlling for parents' income rank and progressively for the length of each geographical feature underlying the spatial division index in the urban unit: waterways, roadways, and railways. Standard errors are corrected for the two-stage approach and reported in parentheses. Each column shows the results of the estimation of this regression on a different subsample. Columns (1) to (3) are estimated on individuals who grew up in urban units with less than 50,000 inhabitants, and columns (4) to (6) are estimated on individuals who grew up in larger urban units. Columns (1) and (4) are estimated on individuals whose parents were both born in France, columns (2) and (5) in North Africa and columns (3) and (6) in South Europe. Statistical significance is reported according to the following symbology: P-val.: $^{***} < 0.01 \leq ^{**} < 0.05 \leq ^{*} < 0.10$.

Reading: Column (4) indicates that for individuals who grew up in large urban units and whose parents were both born in France, a one standard deviation increase in residential segregation, as instrumented by the spatial division index, induces a change of -0.69 percentile rank in the effect of the local share of immigrants on individuals' income rank conditional on their parents' income, on average. This coefficient is statistically different from 0 at the 95% confidence level.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

No significant effect of residential segregation is observed among individuals who grew up in smaller urban units. Among those who grew up in larger urban units, significantly negative effects are observed for sons of natives and for sons of immigrants from North Africa. Results suggest that on average, conditional on parents' income rank, a one standard deviation increase in residential segregation, as instrumented by the spatial division index, induces a change of -0.68 percentile rank in the effect of a one percentage point increase in the local share of immigrants on individuals' income rank for sons of natives. For sons of immigrants from North Africa, this effect reaches -1.9 percentile income ranks. However, no significant effect of residential segregation is observed for females of any origin, or for individuals with one native parent and one immigrant parent.

2.5 Robustness

2.5.1 Attenuation and lifecycle biases

This subsection investigates the most common sources of bias to intergenerational persistence measures: the attenuation bias and lifecycle bias. Results suggest that they are mild and homogeneous across origin groups.

In most countries, the measurement of intergenerational persistence is hampered by data limitations. While the objective is to quantify the association between parents' and children's *lifetime* earnings, earnings are generally only observed for a few years in the children's and/or in the parents' generation. This can bias intergenerational persistence estimates in two main ways. First, relying on few parents' earnings observations to proxy for lifetime income is prone to classical measurement errors, which mechanically attenuates persistence. Second, heterogeneous steepness in the age-earnings profiles along the parents' income distribution translates into different relationships between earnings observed at given ages and lifetime earnings.

I check for symptoms of attenuation bias in Appendix Figure B.27. It shows how the rank-rank slope between children's and parents' earnings evolves with the floor number of parents' earnings observations used to compute lifetime income, separately by gender and origin group. In most cases, estimates are very stable as long as parents' earnings are observed at least twice.[4] Males with South European origins tend to exhibit lower intergenerational persistence when parents' income is observed less than four times, but to a small and non-significant extent. As indicated in Section 2.2, I estimate the prediction model using parents whose earnings are observed at least twice.

I investigate lifecycle bias in children's earnings in Appendix Figure B.29. It shows how the

[4] Note that the sample size reduces as the restriction gets stricter. Appendix Figure B.28 shows the results obtained when using only parents whose earnings are observed at least 11 times, i.e., holding the sample constant. From two income observations onward, rank-rank slopes are even more stable on this subsample, providing additional support to the absence of an attenuation bias.

rank-rank slope between children's and parents' earnings evolves with the age at individuals' income observation, separately by gender and origin group. In line with what is typically found in the literature, the rank-rank slope tends to be lower when individuals' earnings are observed below age 30, and it stabilizes afterwards. Overall, estimates are quite stable in the age range used for baseline measures, between 35 and 45. This pattern is also observed for parents' earnings, as documented in Appendix Figure B.30.

2.5.2 Railroad network opportunities

To substantiate the plausibility of the exclusion restriction, this subsection proposes a more elaborate control for improved economic opportunities via the railroad network.

In baseline specifications, the identification strategy is based on the assumption that the spatial division index used to instrument for residential segregation has not impacted individuals' income ranks, conditional on their parents' income ranks across origins, other than through residential segregation. The main challenge to this hypothesis is the nature of the geographical barriers considered. Indeed, while they may cause immobility at a very local level, they can also foster geographic mobility toward more distant urban units and hence broaden the set of economic opportunities. This is particularly true for the railway network (Costas-Fernández et al., 2020; Pérez, 2018).

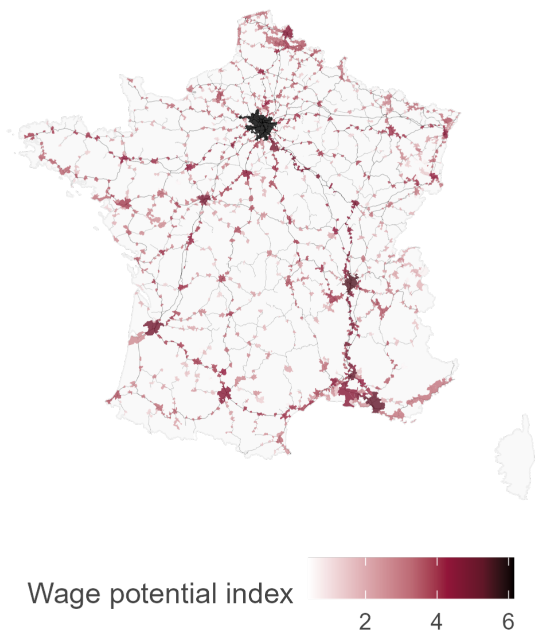
Baseline specifications include controls for the length of the geographical barriers in the urban unit, which proxies for the resulting connectivity, and indirectly for economic opportunities. To tackle this potential issue in a more direct fashion, I measure the set of economic opportunities brought by the railway network with the following market potential index:

$$\text{Wage Potential}_U = \log\left(\sum_{V\in\nu_U}\frac{\log\left(\sum_{i=1}^{N}\text{wage}_{i,V}\right)}{\log(\text{dist}_{UV})}\right), \tag{2.4}$$

where ν_U denotes the subset of urban units $V \neq U$ directly connected to U via railroad, $\text{wage}_{i,V}$ is the annual pretax wage of worker i in urban unit V, and dist_{UV} is the spherical distance between the centroids of urban units U and V. Figure 2.8 shows the spatial variations of this index along with the railroad network at the time.

I include this index of wage potential as an additional control to the baseline specification. Results are shown in Appendix Tables B.17 to B.20. The inclusion of this additional control yields the same pattern for males, with slightly stronger and more statistically significant effects. Specifically, the effect for sons of natives reaches -0.86 percentile ranks, and for sons of immigrants from North Africa it amounts to -1.92. The effect for daughters of natives becomes significant at -0.69 percentile income ranks, but results for children of immigrants from South Europe and children born to one immigrant parent and one native parent remain largely insignificant. Importantly, first-stage F-statistics remain of very similar magnitudes.

Figure 2.8: Wage potential index



Notes: This figure shows the spatial variation of the wage potential index across urban units along with the railroad network as of 1999. The wage potential index of a given urban unit is computed as the logged sum of all wages in all urban units directly connected it via the railway network, weighted by the logged centroid-to-centroid distances, as specified in Equation 2.4. The darker the red, the higher the wage potential of the urban unit.

Reading: The wage potential index in the urban unit of Bordeaux amounts to 4.9.

Source: *GEOFLA*®, wave 1997, IGN, and *Route 500*®, wave 1999, IGN, and *PANEL DADS*, INSEE

2.5.3 Population threshold

Results show that the size of the urban unit is a key dimension of the relationship between residential segregation and intergenerational mobility differences between children of natives and immigrants. This subsection investigates the sensitivity of the main effect to the population size of urban units kept in the group of larger urban units.

Baseline specifications in Table 2.3 distinguish small urban units from large urban units based on a cutoff set at 50,000 inhabitants, and show significant effects for large urban units only. Appendix Figure B.31 shows how these estimated effects vary with the value of the cutoff, separately by gender and origin group. Overall, coefficients appear to be relatively stable as long as the cutoff lies between 30,000 inhabitants and 60,000 inhabitants. The global absolute increase of the coefficient between 20,000 and 80,000 inhabitants corroborates that the effect tends to be larger for larger urban units, but no sharp population threshold emerges.

2.6 Conclusion

This analysis sheds light on heterogeneous intergenerational income mobility patterns among children born in France depending on their parents' immigration background. Immigrant parents from South Europe are over-represented in the bottom half of the income distribution, but their children, conditional on parents' income rank, overtake children of natives in the income distribution to the point that no unconditional income rank gap is observed. Immigrant parents from North Africa are over-represented in the first deciles of the income distribution, and while their daughters conditionally catch up with natives' daughters, their sons remain significantly lower ranked than sons of natives almost all along the parents' income distribution.

Results suggest that this heterogeneity does not stem from differences in conditional educational attainment or in hourly wage rates, but rather from a gap in labor market access. These findings notably resonate with the phenomenon of hiring discrimination towards Muslim sounding names put forward by the correspondence testing literature in France (Adida et al., 2010). This study focuses on an alternative potential mechanism, residential segregation, which has the interest of being easier to track, and hence, to act on.

In an instrumental variable setting, I show that residential segregation has no effect on conditional income rank for children of most origin groups, but that it hampers the intergenerational mobility prospects of children of natives, particularly sons, and even more those of sons of immigrants from North Africa. These effects tend to be stronger in larger urban units. Hence, results suggest that a durable reduction in residential segregation levels between natives and immigrants is a promising way to equalize intergenerational mobility prospects among future generations.

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Chapter 3

To become or not to become French? Conscription, citizenship and labor market outcomes

This chapter is based on a paper co-authored with Yajna Govind (Copenhagen Business School).

Abstract

We examine how changing the costs of acquiring citizenship translates into naturalization decisions for second-generation immigrants, and the effect of naturalization on their labor market outcomes. We exploit the abolition of mandatory military service in France as an exogenous reduction in the cost of citizenship for men. In line with the predictions of our theoretical framework, we find that the reform induced a jump in male naturalization rates, entirely driven by European Union citizens. Using a Synthetic Difference-in-Differences, we show that the probability of employment for EU males consequently increased by 1.7 percentage points, through a reduction in inactivity rather than unemployment. We provide suggestive evidence that this effect is mainly driven by an increase in public sector employment and a reduction in self-employment.

3.1 Introduction

The continuous growth of migration flows raises crucial questions on the most adapted integration and regulation policies to implement. Naturalization, which represents the final legal step in the integration process for migrants, has consistently been a focal point of this debate. On the one hand, naturalization may be seen as a policy tool to boost migrants' integration. On the other hand, it is considered a reward for a successful integration. The latter standpoint currently guides the dominant policy-making approach. Host countries impose substantial costs on citizenship acquisition, intending to screen immigrants.

However, there is limited causal evidence on how migrants respond to changes in the cost of naturalization, and on the impact of naturalization on the labor market integration of second-generation migrants. This is due to three main challenges. First, naturalization take-up is an endogenous decision that raises concerns of selection bias. Second, exogenous shocks in existing studies often impact cohorts that are still too young for their labor market outcomes to be studied. Third, studies on citizenship acquisition for second-generation immigrants, which in most cases rely on reforms impacting individuals at young ages, may not fully be able to disentangle its effects on education from its direct labor market effects.

In this paper, we overcome these challenges by relying on two key aspects of the French context. First, individuals born in France to foreign parents face almost no costs in acquiring French citizenship at the age of 18. Second, during the 1990s, compulsory conscription for male citizens made naturalization a costly choice for foreign men. Altogether, these two features induced a salient trade-off for second-generation men at the age of 18 between renouncing French citizenship and doing military service. In this context, we leverage the abolition of compulsory conscription in 1997 for men born after 1978 as an exogenous shock in the costs of acquiring French citizenship for children of immigrants. We exploit administrative and survey data to explore how this reform affected the take-up of citizenship, and the potential repercussions on labor market outcomes.

Our results show that the abolition of compulsory conscription induced a sharp increase in naturalization rates for males relative to females. This effect is entirely driven by European Union (EU) citizens, the group of second-generation immigrants for whom acquiring French citizenship should matter the least. Still, we find that the surge in naturalization induced a significant 1.7 percentage point increase in employment for this group.

Our setting has the advantage of mechanically shutting down three candidate channels to this effect. Indeed, the fact that compliers are EU citizens ensures that the employment effect does not stem from either the right to reside and work in the host country, the access to welfare benefits, or the stability granted by citizenship. Two of the classical potential mechanisms put forward in the literature remain relevant for this group: labor-market access restrictions, and discrimination.

We show that the increase in employment was accompanied by higher shares of public sector jobs and a departure from self-employment, supporting the labor-market access hypothesis. We postulate that hiring discrimination also contributes to this effect, as we observe significantly positive shares of self-reported discrimination and racism even among second-generation immigrants with European Union origins. We also rule out the possibility that these are driven by a direct impact of military service.

To rationalize the decision to naturalize, we introduce a theoretical framework in which individuals take up citizenship as long as the benefits exceed the costs. We consider a cost function that is decreasing with skills, which is typically the case of policies such as language tests or financial requirements. The model predicts that if benefits are homogeneous, such costs would screen the top of the skill distribution. However, if benefits are heterogeneous, such costs may screen the bottom of the skill distribution by excluding low-benefit high-skilled individuals. This scenario applies to settings where groups that are the most discriminated against on the labor market are also the lower educated.

In the French context, we expect European Union (EU) citizens to benefit the least from citizenship. Indeed, unlike individuals from other birth nationalities, they can freely work and reside in France. Thus, they are the least likely to take up citizenship under compulsory conscription, and the most likely to react to its abolition. On the contrary, we expect individuals from nationalities that are typically discriminated against in the labor market, such as African nationalities, to benefit the most from citizenship. Costs are likely to be heterogeneous as well because low-educated conscripts were typically assigned more strenuous positions (Maurin and Xenogiani, 2007). As a result, we expect the abolition of compulsory conscription to have a larger impact on take-up at the bottom of the education distribution within groups of birth nationalities.

To test these hypotheses empirically, we exploit the fact that women were exempt from compulsory conscription and therefore unaffected by its abolition. Using a Difference-in-Differences approach, we compare the naturalization rate of foreign men and women born in France before and after December 31st, 1978. We find that at the abolition of compulsory conscription, the naturalization rate of males increased from 68.5% to 78.9%, while the rate for females remained stable at around 84%. This suggests that almost a quarter of the missing citizenship take-up among young males was due to compulsory conscription and that its abolition halved the gap with women.

Consistent with our theoretical framework, results show that this effect is entirely driven by European Union citizens at birth, for whom the benefits of acquiring French citizenship are lower. Within this group, the abolition of compulsory conscription increased male naturalization rates by 11.9 percentage points. No significant effect is observed for other birth nationalities, for which the cost of military service is therefore not binding. Among EU citizens, we find that the increase in naturalization rates is more than 50% larger for low-educated individuals compared to high-educated individuals, supporting the hypothesis that the cost of military service is lower for the latter.

We then take advantage of the fact that only EU males experienced a jump in naturalization to study its effect on their labor market outcomes. Specifically, we exploit every unaffected group in a Synthetic Difference-in-Differences approach to best capture how the outcomes of EU males would have evolved absent the abolition of compulsory conscription. The synthetic control group closely mirrors the trend in the employment rate of EU males until the reform, after which the employment rate in the treated group diverged from its path with a 1.7 percentage point increase. Given that this effect is driven by 11.9% of EU males who reacted to the reform, it corresponds to a 14.5 percentage point increase among compliers. We show that this positive effect on employment is primarily attributable to a decrease in inactivity rather than in unemployment.

We explore two potential mechanisms explaining these results. First, we observe a significant increase in the probability of being employed in the public sector. Second, we find a decrease in self-employment for EU males relative to the control groups, in line with the idea that citizenship acquisition expands the set of labor market opportunities for naturalized individuals.

We conduct sensitivity checks showing that our results are robust to the set of control groups considered in our Synthetic Difference-in-Differences setting, to anticipation effects, to general equilibrium effects, to differential attrition, and to the relative length of military service in the origin country nationality. In addition, we address the concern that compulsory conscription might directly impact educational and labor market outcomes. Conscription has been shown to have either no impact or a positive impact on educational outcomes, in line with draft avoidance behavior, and on labor market outcomes in the French context (Maurin and Xenogiani, 2007; Mouganie, 2020). We provide evidence that second-generation immigrants' education levels were not impacted by compulsory conscription. We also rule out a potential direct effect on labor market outcomes given the absence of labor market effects for the birth nationality groups which only experienced the abolition of military service but no changes in naturalization take-up. In addition, Mouganie (2020) documents that in France, military service had either no effect or positive effects on labor market outcomes. Given these factors, our study may, if anything, underestimate the actual labor market impact of naturalization.

This paper relates and contributes to three different strands of the literature. First, it sheds light on the effects of acquiring citizenship on the labor market integration of second-generation migrants. The related literature has largely focused on first-generation, establishing a positive correlation between naturalization and labor market outcomes, starting with the work of Chiswick (1978). An emerging strand of this literature has explored the causal link between the two, for first-generation immigrants and refugees (Gathmann and Keller, 2018; Hainmueller et al., 2019; Govind, 2021; Fasani et al., 2023; Hainmueller et al., 2023). We contribute to this literature by studying the labor market integration of second-generation immigrants, touching upon the literature on birthright citizenship which has so far focused on educational outcomes (Felfe et al., 2020, 2021; Dahl et al., 2022). Our findings demonstrate that even populations who might have less to gain from naturalization, here second-generation EU citizens, experience improved economic integration from naturalization.

Second, this paper contributes to the literature on citizenship take-up. Various studies have explored the association between the propensity to naturalize and individuals' or origin countries' characteristics such as age at migration, gender, educational attainment, and political conditions in the home country (Yang 1994; Chiswick and Miller 2009; Fougère and Safi 2009. See Gathmann and Garbers (2023) for a detailed review of the literature). In addition, citizenship acquisition costs such as civic knowledge requirements, naturalization fees, and multiple citizenship restrictions, have been shown to directly affect take-up, especially that of low-educated individuals and EU citizens (Yasenov et al., 2019; Peters and Vink, 2023; Vink et al., 2021). This paper contributes to the existing literature by formalizing citizenship take-up with a cost-benefit theoretical framework. We discuss the unintended implications of increasing naturalization costs in the face of heterogeneous benefits, echoing recent evidence of potential backlash of integration policies (Fouka, 2020; Dahl et al., 2022; Arendt et al., forthcoming).

Third, we contribute to the literature on the effects of military service. Existing research has mainly focused on the impact of conscription on citizens' outcomes such as education, employment, earnings, political behavior, and crime (e.g., Angrist 1990; Bauer et al. 2012; Card and Cardoso 2012; Hubers and Webbink 2015; Hjalmarsson and Lindquist 2019; Savcic et al. 2023, and more specifically on France: Maurin and Xenogiani 2007; Fize and Louis-Sidois 2020; Mouganie 2020). To the best of our knowledge, we are the first to investigate the effects of military service on non-citizens, and more specifically its implications for their naturalization decisions.

The remainder of this paper is structured as follows. Section 3.2 provides background information on military service in France, the abolition of compulsory conscription, and the relevant naturalization process. Section 3.3 presents the data sources. Section 3.4 outlines our theoretical and empirical framework for studying citizenship take-up, and estimates the effect of the abolition of compulsory conscription on naturalization rates. Section 3.5 describes the empirical approach used to study the impact of naturalization on labor market outcomes, presents the results, and explores potential underlying mechanisms. Section 3.6 provides various robustness analyses, and Section 3.7 concludes.

3.2 Context of the reform

3.2.1 Military service

France has had an organized military service system since the end of the 18th century. All French men were obliged to enroll in military service at the age of majority, with a possibility to postpone conscription up to the age of 22. As of 1971, women could also participate in military service on a voluntary basis.

In the case of dual citizenship, the country in which a person was required to fulfill his military service obligation could vary depending on the existence or absence of bilateral agreements between France and the country of origin. Thirteen European countries signed on May 6th, 1963, and progressively applied a convention that stated that individuals with dual nationalities from these countries were required to fulfill their military obligations in the country of usual residence.[1] Similar conventions were also ratified bilaterally with Algeria, Israel, Switzerland, and Tunisia.[2]

With a decline in military needs, the length of conscription has frequently been shortened over time, down to a duration of 10 months in the 1990s. In February 1996, Jacques Chirac, the president of France at the time, declared his intention to abolish mandatory military service in order to professionalize it. The French government began discussing the reform of the military service in November 1996, and the law was voted and passed in October 1997. It aimed at progressively suspending compulsory military service, seeking to end it completely by 2003. During this transition period, the former regulations on military duty continued to apply to men born before 1979, even if they postponed their conscription, while those born after December 31st, 1978 were exempt from compulsory conscription. A decree published in June 2001 officially marked the end of mandatory military service for all in France.

3.2.2 Naturalization

Unlike the United States, most migrant-receiving countries have not adopted unconditional birthright citizenship, or the *jus soli* principle (right of soil). During our period of interest in France, children born on the French territory to two foreign parents were eligible to obtain French citizenship at the age of 18 with a light administrative procedure.[3,4] To do so, individuals must submit the required documents to the Court of Justice between the ages of 16 and 21.

[1] These 13 countries are Austria, Belgium, Denmark, France, Germany, Ireland, Italy, Luxembourg, Netherlands, Norway, Spain, Sweden, and the United Kingdom. See https://www.coe.int/en/web/conventions/full-list?module=signatures-by-treaty&treatynum=043 for an overview of the signature and ratification dates.

[2] The Franco-Algerian convention allows bi-nationals to choose the country in which they would like to complete their military service, irrespective of their place of residence. In Section 3.6.1, we ensure that our results are not driven by differences in hardship of the home country's military service.

[3] The majority of children obtain citizenship at age 18. They could also obtain it as early as 13 years of age if the child was born in France and has resided in the country since the age of 8, upon the request of parents. Above the age of 16, children can apply for French citizenship if they have resided in France for at least 5 years since the age of 11.

[4] The rules were changed between 1993 and 1998. The Pasqua Law of 1993 required children born in France to foreign parents to formally declare their wish to acquire French citizenship to obtain it. This reform should not affect our setting because it should not affect men and women differently. Indeed, we show that the naturalization rate of males and females did not evolve differentially at the moment of the reform. In addition, the cohorts that were most impacted by this reform were those born between 1974 and 1980, which does not coincide with our pre- and post-treatment distinction.

An estimated 10 to 15% of young adults did not acquire French citizenship in the 1980s (Massot, 1985). If they chose not to acquire French citizenship during that age window, they could not be reinstated due to the unfulfillment of military obligations (Spire and Thave, 1999) prior to its abolition. In all cases, they would be subject to the more stringent requirements faced by other foreigners if they subsequently wished to acquire French citizenship. These made the lack of take up at around 18 years old an almost definite choice.

3.2.3 Naturalization and conscription

Historically, naturalization policies have been tightly related to military needs. Indeed, the law of the 26$^{\text{th}}$ of June 1889, which established the automatic attribution of French citizenship to children of foreign parents born in France, can be traced back to demographic and military concerns (Massot, 1985). It was notably put in place to ensure that second-generation immigrant men would be liable to serve in the French military (Spire, 2005).

Conscription thus became an obligation associated with obtaining citizenship. The timing of the choice to naturalize coincided with the enrollment for military service, making the latter salient in the naturalization decision. In fact, it is estimated that around 90% of individuals who renounced citizenship among second-generation in the 1990s were men (Weil, 2002). This clearly suggests that military service was considered a cost of naturalization. Thus, the abolition of compulsory conscription in 1997 made naturalization less costly for second-generation men. Given the delay between the first presidential announcement in February 1996 and the adoption of the reform in October 1997, we document a slight anticipation effect and account for this in Section 3.6.

Second-generation men who obtained French nationality while retaining their origin countries' nationality, could usually fulfill their military service obligation in the country of usual residence. We address concerns that the naturalization take-up could be a function of the ease of the origin country's military service in a robustness check in Section 3.6. In addition, some European countries did not require their citizens who are born and still reside abroad to do military service altogether. For instance, Italy's citizenship law n° 555/1912 exempted Italian citizens residing abroad and who have not lived in Italy beyond their 16$^{\text{th}}$ birthday to fulfill military service obligations in Italy (Bussotti, 2016).

3.3 Data

3.3.1 French Population Census

We conduct the main analysis on the French Population Census, collected by the French National Institute for Statistics and Economic Studies (INSEE). Until 1999, it was exhaustive and collected every 6 to 9 years. Since 2004, it has been exhaustive for municipalities under 10,000 inhabitants and it has covered 40% of dwellings in municipalities that exceed this threshold. We thus weight all computations using the survey weights provided by INSEE. Under this new data collection procedure, 20% of individuals are surveyed each year such that each period of 5 consecutive yearly census surveys constitutes a complete survey wave.

We specifically rely on the 2014 survey wave, which gathers forms collected from 2012 to 2016, such that individuals born around January 1st 1979 are surveyed at age 35 on average. French citizenship take-up is thus observed relatively early in adulthood, and labor market outcomes are observed sufficiently late for individuals born after 1978 to have completed their education and entered the labor market. Note that the collection process of a complete survey wave over 5 years generates some age variation within birth cohort, which we can control for in the analysis.

The census notably collects information on individuals' age, gender, educational and labor market outcomes, marital status and nationality. For French citizens, both the current nationality and the birth nationality are observed. This allows us to distinguish individuals who acquired the French citizenship from those who were born French. For non-French citizens, the current nationality is observed.[5] The census does not contain information on dual nationalities.[6]

Information on family links between censused individuals is only available for 40% of the full sample. We exploit this feature of the data to study the outcomes of the spouse. Hereinafter, we refer to this subsample as the *complementary census sample*, while we refer to the full sample as the *main census sample*.

3.3.2 Permanent Demographic Sample

We rely on the Permanent Demographic Sample (EDP) to investigate attrition patterns and the timing of citizenship acquisition over the life course. The EDP is a panelized version of the census data for a subset of individuals born on specific dates during the year.

Specifically, it includes individuals born during the first 4 days of April, July, October, and from the 2nd to the 5th of January. Thus, the EDP covers about 4% of the population. Census information of EDP individuals is observed at variable ages and calendar years due to the rotating yearly collection procedure of the French population census. On average, individuals are observed once every 5 years in the EDP census data.

Census information of EDP individuals is also merge with data from the electoral register.

[5] For these individuals, we must assume that their birth nationality is identical to their current nationality. We test this assumption using the Permanent Demographic Sample, described in Section 3.3.2, which follows a sample of the population across census waves. In this sub-population, 98% of the foreigners born ± 10 years around the cutoff, i.e., from 1969 to 1998, and observed in both the 2014 census and in the 1990 census, have the same nationality in the two census waves.

[6] As long as an individual was born French, no information on other potential nationalities is available. For those who acquired French citizenship, a single birth nationality can be reported. For those who were not French when they were surveyed, a single current nationality is reported even if they had more than one nationality.

Table 3.1: Descriptive statistics of the main sample

	Male	Female	All
Age	35.05	34.59	34.81
Citizenship acquisition	72.18%	84.22%	78.37%
High school	55.12%	68.96%	62.24%
Higher education	35.34%	46.60%	41.13%
Birth nationality:			
European Union	51.45%	46.80%	49.06%
Other Europe	11.65%	11.93%	11.80%
Africa	31.00%	35.09%	33.11%
Other	5.90%	6.18%	6.04%
Observations	65,347	67,722	133,069

Notes: This table provides the average demographic characteristics of our sample of interest: individuals born in France without French citizenship from 1969 to 1988. European Union is defined as in 1996. A more detailed breakdown of the birth nationality composition of the sample is provided in Online Appendix Table C.1. Source: *French Population Census*, wave 2014, INSEE.

This is of particular interest because it contains the date at which individuals were added to the electoral register, which we can exploit to impute the age at which individuals acquired citizenship.

3.3.3 Sample definition

Our sample of interest consists of second-generation immigrants, i.e., individuals born in France without French citizenship. They represent 1.3% of the population among individuals born 10 years around the threshold, from 1969 to 1988. Table 3.1 shows the descriptive statistics of this population using the main census sample.

This sample comprises around 6,500 individuals per birth cohort, with about as many men as women. Individuals are observed at age 35 on average, and 78% of them acquired French citizenship. Females tend to be more educated than males, and they take up French citizenship more often. Almost half of the sample has a birth nationality that pertains to the European Union, and African birth nationalities represent a third of the sample. The shares of the most represented birth nationalities in our sample are presented in Online Appendix Table C.1.

3.4 Compulsory conscription and naturalization

3.4.1 Theoretical framework

General framework

We present a simple theoretical framework to describe the relationship between the costs associated with citizenship and the take-up of citizenship. We consider a simple setting where an individual i would take up citizenship as soon as the corresponding benefits B_i exceed the associated costs C_i. Individuals are characterized by their skill level S_i, distributed over the interval $[s^- \geq 0,\ s^+]$. Typically, costs imposed on citizenship such as language tests, financial requirements, or even simply administrative burden, are decreasing with skills. Thus, individuals' take-up decision can be modeled as:

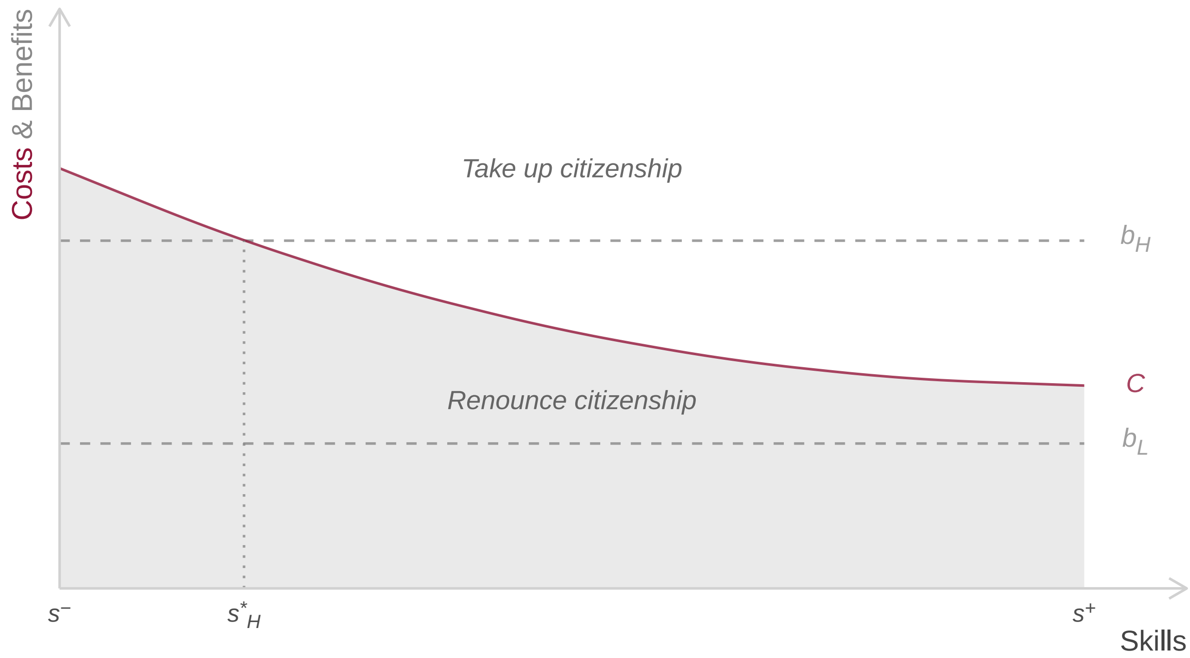
$$\text{Take-up}_i = \mathbb{1}\Big\{B_i \geq C(S_i)\Big\}, \text{ with } \frac{\partial C}{\partial S} < 0.$$

Homogeneous benefits. To begin with, consider that $B_i = b\,\forall i$. If $b > C(s^-)$, everyone would take up citizenship. If $b < C(s^+)$, no one would take up citizenship. For any intermediate value of b, there is a skill level s^* such that costs and benefits equalize: $b = C(s^*)$. In this situation, only individuals whose skill level S_i is greater than the threshold s^* would take up citizenship. Thus, when benefits are homogeneous, the higher the costs associated with citizenship, the higher the skills of individuals who take up citizenship.

Heterogeneous benefits. In most contexts, all individuals do not benefit from citizenship to the same extent. For instance, benefits may be larger for individuals whose birth nationality is discriminated against on the labor market. To account for that, let the benefits derived from citizenship vary with individuals' type $k \in (L, H)$ such that $b_L < b_H$. The higher the level of benefits b_k derived from citizenship, the lower the skill threshold s^*_k above which individuals would take up citizenship: $s^*_H < s^*_L$.

In this situation, if high-benefits individuals tend to be lower-skilled than low-benefits individuals, shifting the cost function upwards would not necessarily lead to select the top of the skill distribution. Consider for instance the case depicted in Figure 3.1. The cost function C is such that $C(s^+) > b_L$ screening out every high-skill low-benefits individual. Given that $s^*_H > s^-$, the lower end of the skill distribution of low-skill high-benefits individuals is also screened out. Overall, such a cost function would lead to an over-representation of the middle part of the skill distribution among individuals who acquire citizenship. A flatter cost function could even be such that $C(s^-) < b_H$, selecting every individual from the low-skill group but none from the high-skill group.

Figure 3.1: Citizenship take-up decision under heterogeneous benefits



Notes: This figure shows a schematic depiction of our theoretical framework. Costs and benefits of citizenship acquisition are represented on the y-axis. The skill level of individuals confronted with the choice of either taking-up or renouncing citizenship varies along the x-axis, from s^- to s^+. The solid curve C represents a typical cost associated with citizenship acquisition. Any individual whose level of benefits is below this curve would renounce citizenship. The horizontal dashed lines b_L and b_H represent benefits that individuals from the low-benefits group and the high-benefits group respectively derive from acquiring citizenship. As b_L lies below C over the whole $[s^*_H,\ s^*_L]$ interval, no one from the low-benefits group would take up citizenship. Individuals from the high-benefits group whose skill level is below s^*_H would not take up citizenship either. Only individuals from the high-benefits group whose skill level exceeds that threshold would take up citizenship.

Application to the French context

In the 1980s, 10 to 15% of individuals born in France to foreign parents did not take up citizenship despite the automatic acquisition process (Massot, 1985). A potential explanation to this phenomenon is that compulsory conscription disincentivized the offspring of immigrants to take up French citizenship.

The cost of doing military service is likely to decrease with education, which is reminiscent of actual naturalization policies whose costs are usually decreasing with skills. Indeed, less strenuous positions were typically assigned to more educated conscripts (Maurin and Xenogiani, 2007). In addition, lower educated individuals tend to be more liquidity constrained as they usually come from lower socio-economic backgrounds. They may need to work to earn money for them and their family, which is not compatible with military duty.[7] Military service may also have beneficial aspects, such as exposition to social diversity, or the possibility to obtain a driver's license for free (Avrillier et al., 2010). We assume that these

[7]The fact that higher-educated individuals would have higher forgone expected earnings increases the relative cost of military service at the top of the education distribution, but we do not expect this effect to be strong enough to reverse the sign of the relationship.

potential benefits are homogeneous and we implicitly include them in the costs, which must be interpreted as the *net* cost of military service. Given the automatic acquisition process, we consider that other costs are negligible in our framework.

Benefits of acquiring French citizenship notably include electoral rights and access to public sector jobs restricted to the French population.[8] A strong dimension of heterogeneity in the benefits of citizenship is the birth country. Since 1958, the Treaty of Rome allows European Union citizens to freely reside and work in any country of the EU regardless of their specific nationality. Individuals whose birth nationality does not pertain to the EU would thus derive larger benefits from French citizenship in terms of labor market access and residence rights. On top of that, individuals whose birth nationality is typically discriminated against in the labor market, such as African nationalities (Duguet et al., 2010; Adida et al., 2016; Primon and Simon, 2018; Govind and Santini, forthcoming), would benefit even more from French citizenship as a signal to employers.

In this context, the decision to take up citizenship can be modeled as follows.

$$\text{Take-up}_i = \mathbb{1}\Big\{ B\big(\underset{(+)}{\mathbb{1}\{\text{Birth nat}_i \notin \text{EU}\}}, \underset{(+)}{\mathbb{1}\{\text{Birth nat}_i \in \text{Africa}\}}\big) + \eta_i \geq \underset{(-)}{C(\text{Education}_i)} \Big\},$$

where η_i represents exogenous and normally distributed individual preferences for acquiring French citizenship that generate variation in the take-up decision for a given birth nationality and education level. Note that the cost of military service C only applies to males whose birth cohort was subject to compulsory conscription, and may not be binding to all birth nationality groups. We can draw three main hypotheses from this theoretical framework.

First, if the cost of military service is sufficiently large, the share of males born in France to foreign parents who acquired French citizenship should increase with the abolition of compulsory conscription, while that of females should remain constant.

Second, this increase should be larger for less educated men. The cost of military service is expected to be larger for them, and it is thus more likely to be binding.

Third, this increase should be larger for men who were born citizens of the European Union than for men born with other nationalities. The latter have larger benefits of acquiring French citizenship, especially individuals with an African nationality who are typically discriminated against on the labor market.

3.4.2 Empirical framework

We expect the abolition of compulsory conscription to reduce the disincentive for males born in France without French nationality to take up French citizenship at the age of 18. To test

[8]Note that since the law n° 91-715 of July 26, 1991, 80% of public sector employment are accessible to EU citizens.

this hypothesis we exploit the fact that women were not subject to compulsory conscription. Specifically, we compare the difference in naturalization rates for men born before and after 1979 to the difference in female naturalization rates in a Difference-in-Differences setting. Equation 3.1 displays the corresponding specification.

$$\text{Naturalization}_i = \alpha + \beta\text{Male}_i + \delta\text{Post}_i + \gamma(\text{Male}_i \times \text{Post}_i) + \varepsilon_i \tag{3.1}$$

Naturalization_i is a dummy variable taking the value 1 if the individual acquired French citizenship and 0 otherwise. Male_i is a dummy variable taking the value 1 for males and 0 for females. Post_i is a dummy variable taking the value 1 if the individual was born after 1978 and 0 otherwise, since only males born until 1978 were subject to compulsory conscription.

Our coefficient of interest is γ, which quantifies the difference in the naturalization rates for men born before and after 1979, net of the difference in the naturalization rates of women. According to the theoretical framework developed in Section 3.1, we expect γ to be positive. For γ to capture the causal effect of the abolition of compulsory conscription on naturalization rates, the difference in female naturalization rates between the two periods must be equal to the difference that would have been observed for males absent the abolition of compulsory conscription. For the parallel trend assumption to hold, the naturalization rate of females must not be directly affected by the reform, nor indirectly through their spouses. Also, no event concomitant to the reform must have affected the naturalization rates of men and women. We provide evidence supporting these two assumptions in Section 3.4.3.

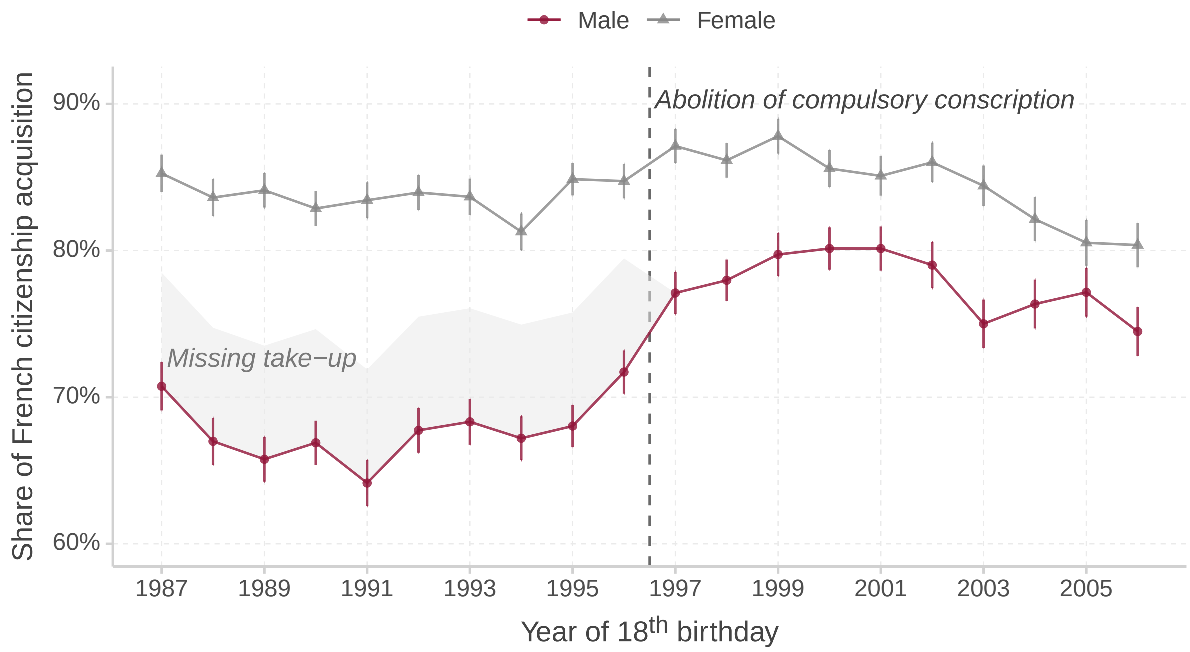
3.4.3 Results

Figure 3.2 shows the naturalization rates of individuals born in France without French citizenship by birth cohort and separately for men and women. Given that the trade-off between renouncing French citizenship and doing military service is faced at age 18, birth cohorts are labeled according to the year of 18$^{\text{th}}$ birthday on the x-axis. The vertical dashed line represents the moment of the abolition of compulsory conscription. The corresponding difference-in-differences regression results for a window of $\pm$ 5 years around the threshold are reported in Appendix Table C.2.

The naturalization rate of females remains stable at around 85% over the period. The fact that it never reached 100% even though females were not subject to compulsory conscription could be explained by factors such as exclusive nationalities, identities and sense of belonging, individual preferences, the intention not to stay in France, or not fulfilling the residence requirement. The naturalization rate of males, however, does increase markedly at the threshold. This is in line with the fact that the automatic acquisition of French citizenship was suddenly no longer tied to the cost of doing military service, which increased take-up.

We estimate the overall share of compliers with a Difference-in-Differences following Equation 3.1. It amounts to 7.7% and is represented graphically by the shaded area. Cor-

Figure 3.2: Naturalization rates of second-generation immigrants



Notes: This figure represents the share of French citizenship acquisition among individuals born in France without French citizenship, separately for males (red) and females (gray), for birth cohorts from 1969 to 1988. Vertical lines show the corresponding 95% confidence intervals. The x-axis is labeled according to the year of 18$^{\text{th}}$ birthday, which is when males born before 1979 must decide whether to do military service or to renounce French citizenship. The vertical dashed line represents the abolition of compulsory conscription. For males, after this point, citizenship acquisition is not tied to doing military service anymore. The shaded area represents the estimated fraction of missing citizenship take-up among young males caused by compulsory conscription. The height of the area is obtained from a Difference-in-Differences regression between males and females born 5 years before and after 1979, as specified in Equation 3.1. The corresponding regression results are shown in Appendix Table C.2. Source: *French Population Census*, wave 2014, INSEE.

responding regression results are reported in Appendix Table C.2. Thus, the abolition of compulsory conscription halved the initial gap of 15.2% in naturalization rates between males and females. As the take-up rate for males born before 1979 amounts to 68.5%, this result suggests that compulsory conscription was responsible for about a quarter of the missing take-up of French citizenship among young males before its abolition. In absolute terms, more than 4,500 young males born in France renounced French citizenship because of compulsory conscription during the decade leading to its abolition.

3.4.4 Assumptions validity

This result can only be interpreted as causal if the trend in the naturalization rate of females is a valid counterfactual for that of males. The fact that the two trends are parallel in the pre-period supports the validity of this assumption. The p-value associated with the 5-year pre-trend is 0.39. Still, this does not rule out that naturalization rates of males and females may have diverged absent the reform. We must ensure that the reform had no indirect impact on females, and that no concomitant event may confound its effect.

Absence of indirect effect on females

Even though females are not directly concerned by the abolition of compulsory conscription, those who married someone affected by the reform could in turn be affected through the possibility of acquiring citizenship after 4 years of marriage with a French person. We test this hypothesis by plotting the naturalization rate of females against the birth cohort of their husbands instead of their own. Because these computations require identifying family links between censused individuals, we rely on the complementary census sample instead of the main census sample.

Results are displayed in Online Appendix Figure C.1. Among women whose husband was born in France, we distinguish women whose husband was born French from women whose husband was born foreigner. In both cases, no discontinuity or kink in naturalization rates is observed between the two periods. This suggests that females were not indirectly affected by the reform through their spouses.

Absence of concomitant events

Any event differently affecting males' and females' naturalization rates simultaneously to the reform could threaten the validity of our estimates. To the best of our knowledge, no reform other than the abolition of compulsory conscription have affected differently males and females born before and after 1979. However, two events that may have impacted citizenship take-up occurred close to 1997, when the reform passed.

The first one is the inclusion of Austria, Finland, and Sweden in the European Union in 1995. After this change, citizens from these countries could work and reside freely in France, which lowered their incentive to acquire French citizenship. However, this should not affect males and females differently. In addition, these birth nationalities account for only 0.12% of our sample, so their exclusion cannot threaten the robustness of our result given its magnitude.

The second event is the Pasqua law, implemented in 1993 and abolished in 1998. It required individuals to formally declare their wish to acquire French citizenship to obtain it. Other than that, citizenship acquisition remained automatic. In Figure 3.2 we observe a slight drop in naturalization in 1993 for both men and women, which did not seem to induce any divergence between the two trends. The abolition of the Pasqua law in 1998, one year after the abolition of compulsory conscription, does not seem to have affected either of men's and women's trend in naturalization rate.

3.4.5 Heterogeneity

Following the theoretical framework outlined in Section 3.4.1, we then investigate the heterogeneity of this effect according to education and birth nationality group. Specifically, we distinguish individuals who studied up to high school from those who pursued higher education. Within these two groups, we also distinguish individuals whose birth nationality pertains to the European Union from other European nationalities, and from African nationalities.[9] The remaining nationalities are very heterogeneous and would form a too small group for their naturalization rates to be reliably computed by gender and birth cohort.

Figure 3.3 shows the evolution of naturalization rates by birth cohort for each of the 6 resulting groups, for males and females separately. Corresponding regression results are shown in Appendix Table C.3. Female naturalization rates are relatively stable over the period irrespective of the education level and of the group of birth nationality. Male naturalization rates are generally lower, and they do not follow the same trend depending on the group of birth nationality. The naturalization rate of males born with a non-EU nationality, either European or African, follow a very similar trend to that of females. For individuals born EU citizens, however, there is a sudden increase in naturalization rates at the moment of the reform. This clearly shows that the impact of the abolition of compulsory conscription is entirely driven by EU citizens. This is in line with the incentive scheme described in Section 3.4.1, as this group is the one for whom the benefits of acquiring French citizenship are expected to be the lowest.

Also, within European Union citizens at birth, we observe that the effect is more than 50% larger for the lower educated (12.7 pp., against 7.7 pp. for the higher educated). This is consistent with the idea that the cost of military service is much larger for the low-educated, and thus more likely to be binding for this subgroup.

3.5 Naturalization and labor market outcomes

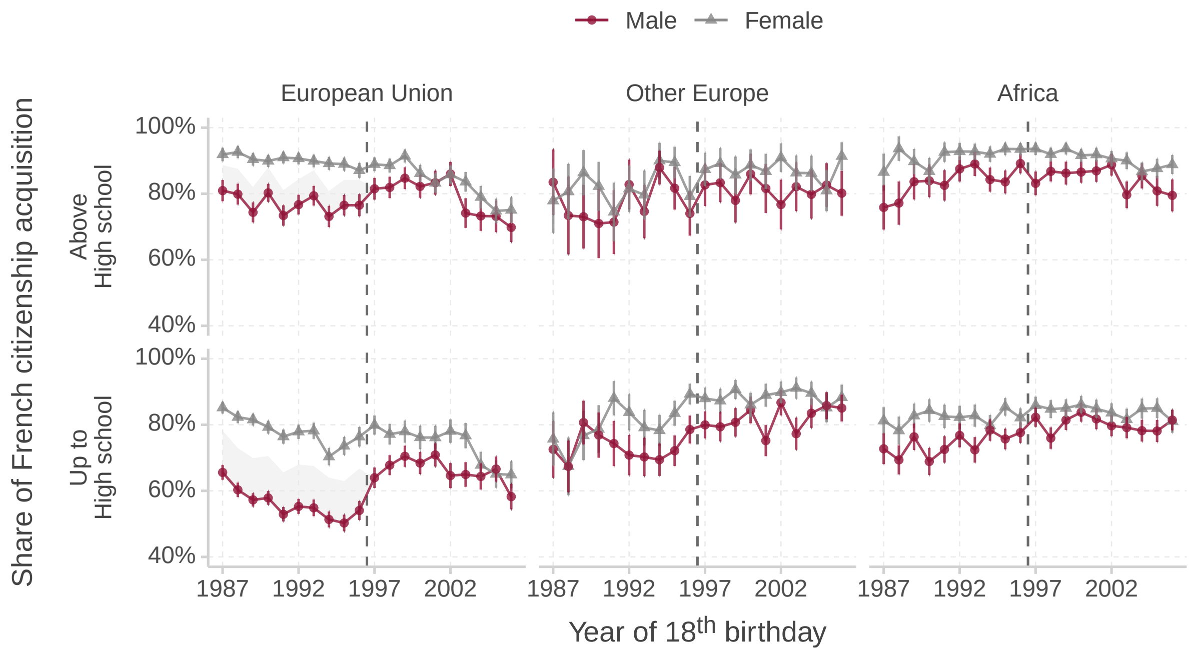
3.5.1 Empirical framework

We exploit the sudden increase in naturalization among males born in France without French citizenship to estimate the causal impact of naturalization on labor market outcomes. As shown above, the only group to react to the abolition of compulsory conscription was males born EU citizens, while other groups of birth nationalities remained unaffected by the reform. Thus, several potential control groups can be considered.

Females born EU citizens belong to the same group of birth nationalities as the treated group, but they may not share the same trend in labor market outcomes due to the gender difference. Males born without the EU citizenship have the same gender, but birth nationality differences may induce differences in trends. Still, these different potential control groups all

[9] We consider European Union as in 1996, i.e., Austria, Belgium, Denmark, Finland, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden, and the United Kingdom. Other European countries are grouped into the non-EU category. Because Austria, Finland, and Sweden entered the EU only two years prior to the reform, we provide robustness checks without these three countries in the Online Appendix Figure C.2 and Table C.4. Results remain virtually unaffected by the exclusion of these countries.

Figure 3.3: Naturalization rates by education and birth nationality groups



Notes: This figure represents the share of French citizenship acquisition among individuals born in France without French citizenship for 6 subgroups defined according to birth nationality (European Union, Other Europe, and Africa) and education (up to high school and above high school). European Union is defined as it was in 1996. In each panel, shares of citizenship acquisition are represented separately for males (red) and females (gray), for birth cohorts from 1969 to 1988. Vertical lines show the corresponding 95% confidence intervals. The x-axis is labeled according to the year of 18th birthday, which is when males born before 1979 must decide whether to do military service or to renounce French citizenship. Shaded areas represent the fraction of missing citizenship take-up among young males caused by compulsory conscription. The height of the area is obtained from Difference-in-Differences regressions between males and females born 5 years before and after 1979, estimated on the corresponding subgroups as specified in Equation 3.1. Regression results are reported in Appendix Table C.3. Significant effects are found only for individuals born citizens of the European Union. Source: *French Population Census*, wave 2014, INSEE.

have common characteristics with the treated group that are likely to induce co-movement in the outcomes of interest. It makes our empirical setting suited for the use of a Synthetic Difference-in-Differences strategy (Arkhangelsky et al., 2021).[10] This approach extends the Synthetic Control approach introduced by Abadie and Gardeazabal (2003) in two ways.

First, the inclusion of a group fixed effect allows us to weight control groups based on their demeaned trend instead of their absolute trend. Thus, a control group that is more distant from the treated group in absolute terms but whose time variations are similar can be attributed a higher weight.

Second, the Synthetic Difference-in-Differences approach introduces time weights for pre-treatment years in addition to group weights.[11] Time weights are computed to give more importance to time units for which the outcome levels of control groups are closer to their

[10]We cannot conveniently implement a Triple-Difference nor a Regression Discontinuity design in our setting due to the lack of statistical power provided by our sample.

[11]Note that group weights and time weights are computed independently from one another.

average in the post-period. This typically gives more importance to the fit of the most recent time units in the pre-period to prevent potential discrepancies right before the event from contaminating the estimated treatment effect.

The estimated model is as follows.

$$(\hat{\tau}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{g=1}^{G} \sum_{t=1}^{T} (Y_{gt} - \mu - \alpha_g - \beta_t - W_{gt}\tau)^2 \hat{\omega}_g \hat{\lambda}_t \right\}, \tag{3.2}$$

where the G groups are indexed by g, and the T birth cohorts are indexed by t. Y_{gt} denotes the average of a given labor market outcome among individuals from group g and birth cohort t. μ is a constant term, α_g a group fixed effect, and β_t is a time fixed effect. W_{gt} is a binary variable taking the value 1 for the treated group in post-reform time units, and the value 0 otherwise. $\hat{\omega}_g$ and $\hat{\lambda}_t$ are the vectors of group and time weights, computed following Arkhangelsky et al. (2021). Each of these weight vectors sums to 1 and can only take positive values. Note that with a single control group and two periods, weights do not matter and this approach boils down to the standard Difference-in-Differences specification. Similarly, the standard Synthetic Control specification can be obtained by omitting the group fixed effect α_g and the vector of time weights $\hat{\lambda}_t$.

Our set of control groups includes all female birth nationality groups, i.e., females born EU citizens, females born with a non-EU European nationality, and females born with an African nationality. It also includes male birth nationality groups for whom the cost of military service was not binding, i.e., males born with a non-EU European nationality and males born with an African nationality. To enlarge our set of control groups we also include females and males born abroad with a nationality outside the EU and who arrived in France early during childhood.[12] In particular, for comparability with individuals born in France from foreign parents, we consider children who arrived in France by the age of 6 to ensure that they were schooled in France from the beginning of primary school. We also exclude EU citizens from the group of males born abroad because even though they are not subject to the automatic acquisition of citizenship, the cost of doing military service may enter their decision to apply for citizenship.

The correct identification of the parameter of interest τ relies on the parallel trend assumption. Specifically, the trend followed by the synthetic control group, i.e., by the weighted average of the different control groups considered, must be the same as the trend that the treated group would have followed absent the shock. This requires (i) that the abolition of compulsory conscription must not have impacted the outcome of the treated group other than through the jump in naturalization it induced, (ii) that no concomitant event has impacted differently the

[12] In practice, low sample size prevents us from breaking down these two groups even further by groups of birth nationalities. We do not consider individuals born French as potential control groups because of the direct effect of compulsory conscription on education identified by Maurin and Xenogiani (2007). This issue is discussed in more detail in Section 3.5.3.

treated group and the control groups, and (iii) that none of the individual control groups were impacted by the reform, neither directly nor indirectly. We provide support for the validity of these assumptions in Section 3.5.3.

Under these assumptions, the parameter τ identifies an intention-to-treat (ITT) impact. Indeed, it captures the average effect on all EU males regardless of compliance status. The average treatment effect on the complier population, i.e., the local average treatment effect (LATE), can be obtained by dividing the ITT by the share of compliers. The share of compliers corresponds to the baseline Difference-in-Differences coefficient γ from Equation 3.1 estimated on EU citizens at birth. Regression results for individuals born within 5 years before or after 1978 are presented in Appendix Table C.5, with and without controls for age and education. The share of compliers amounts to 11.9%. Yearly Difference-in-Differences coefficients are shown in Figure C.3. The Synthetic Difference-in-Differences estimate of the share of compliers amounts to 10.7%. Corresponding results are shown in the Online Appendix Figure C.4 and Table C.6.

3.5.2 Results

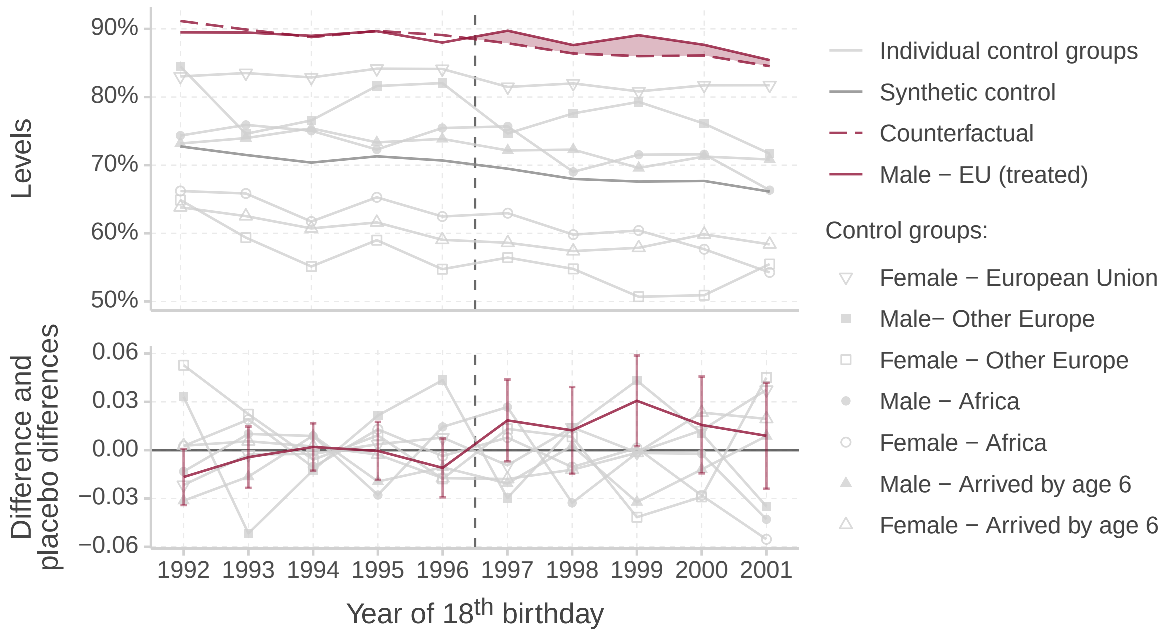
We implement the Synthetic Difference-in-Differences methodology to estimate the effect of the abolition of compulsory conscription on employment, unemployment, and inactivity for EU males through the jump in naturalization only them experienced.

Figure 3.4 provides a graphical representation of the results obtained for the probability of employment. The top panel displays the average share of employed individuals by birth cohort separately for each group. The solid red line corresponds to the evolution of the outcome of EU males, those who experienced a jump in their naturalization rates at the moment of the reform. The individual trends underlying that of the synthetic control group are reported in light gray separately for each control group.

Over the whole period, EU males have the most favorable employment outcomes. EU females do also have good employment outcomes relative to other groups. This highlights the importance of relying on the Synthetic Difference-in-Differences approach instead of the standard Synthetic Control in our setting. Indeed, while the latter would give virtually all the weight to EU females despite slightly converging trends, the former abstracts from the proximity in levels and it weights groups based on their demeaned trend. The resulting synthetic trend, computed following Arkhangelsky et al. (2021)'s methodology, and represented with a dark gray line, closely follows that of the treated group, with more stability than any other individual trend. This makes Synthetic Difference-in-Differences also more suited than standard Difference-in-Differences in our setting.

The dashed red line represents the counterfactual evolution of the treated group absent the reform. It is obtained by shifting the synthetic trend by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period.

Figure 3.4: Synthetic Difference-in-Differences - Employment



Notes: The top panel of this figure shows the share of employed individuals by birth cohort within each of the groups used in our Synthetic Difference-in-Differences approach. That of the treated group, male European Union citizens, is represented with a solid red line. That of each individual control group is represented with a gray line, whose markers indicate the corresponding group. The solid gray line shows the trend of the synthetic control group. The dashed red line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid red line, centered at the pre-treatment average difference weighted by the estimated time weights. Vertical lines show the corresponding 95% bootstrapped confidence intervals. The gray lines are placebo effects, each using one of the control groups as the treatment group instead of EU males. Corresponding regression results are reported in Table 3.2. Source: *French Population Census*, wave 2014, INSEE.

The vertical dashed line represents the implementation of the reform. Before the reform, the synthetic control group and the treated group share the same slightly decreasing but steady trend in employment. While the synthetic control group remains on the same path after the reform, a jump in employment is observed for the treated group.

The bottom panel displays the difference between the treated group and the synthetic control group with a solid red line. It is centered at the pre-treatment average difference, weighted by the time weights $\hat{\lambda}_t$. Vertical lines shows the corresponding bootstrapped 95% confidence intervals. The gray lines are placebo effects, each using one of the control groups as the treatment group instead of EU males. While all placebo differences in trends remain relatively gathered around 0, the actual difference in trends exhibits a clear upward shift at the moment of the reform.

Table 3.2 column (1) shows the baseline Synthetic Difference-in-Differences results on employment, controlling for age and education. Columns (2) and (3) report the results for unemployment and inactivity. Corresponding graphical representations are presented in Appendix Figures C.5 and C.6. The first row shows the intent to treat effect, estimated as specified in Equation 3.2 controlling for age and education. The second row documents the corresponding local average treatment effect. It is computed as the ITT divided by the share of compliers, which is estimated as specified in Equation 3.1 controlling for age and education. Bootstrapped standard errors are reported in parentheses.

Columns (1) indicates a significantly positive effect of naturalization on the probability of employment. The magnitude of the ITT amounts to 1.7 percentage points, which is substantive given that it is driven by the 12% of the treated group who actually acquired citizenship in response to the abolition of compulsory conscription. Indeed, the LATE indicates that the effect on compliers reaches 14.5 percentage points. Given that the probability of employment in the treatment group is around 90%, as can be seen from Figure 3.4, this suggests that among EU males, compliers have lower employment rates than average. This is in line with the fact that the lower educated are those who reacted the most to the reform, as observed in Figure 3.3.

Columns (2) and (3) suggest that this increase in the probability of employment is related to a greater extent to a reduction in inactivity than in unemployment. Indeed, while the reduction in inactivity reaches 8 percentage points for compliers, a non-significant 5.4 percentage point decrease is observed in unemployment.[13]

For each outcome, group weights are quite evenly distributed across individual control groups. No specific group stands out by having systematically high or low weights compared to other groups. Regarding time weights, earlier birth cohorts tend to be given less weight. This is what is typically obtained by design, as emphasized in Section 3.5.1.

3.5.3 Assumptions validity

For the estimated coefficients to capture the causal effect of naturalization, the parallel trend assumption must hold between the treated group and the synthetic control group. In our setting, its validity relies on three core hypotheses. First, military service must not have a direct impact on male labor market outcomes other than through naturalization. Second, female labor market outcomes must not be affected by the reform through the outcomes of their husbands. Third, military service must not be binding in the naturalization decision of immigrants arrived by age 6, which are included in our set of control groups. Evidence provided in this section suggest that none of these threats to identification is at play in our setting, supporting the validity of the parallel trend assumption.

[13] In a standard Difference-in-Differences setting the coefficients associated with the variables Employed, Unemployed, and Inactive would mechanically sum to 0, which is not generally the case with the Synthetic Difference-in-Differences approach. The ITT coefficients reported in Table 3.2 sum to 0.001, which is reasonably close to 0 given the magnitude of the estimates.

Table 3.2: Effect of naturalization on labor market outcomes

	Employed	Unemployed	Inactive
ITT ($\hat{\tau}$)	0.017**	−0.006	−0.010*
	(0.009)	(0.008)	(0.006)
LATE ($\hat{\tau}/\hat{\gamma}$)	0.145**	−0.054	−0.080*
	(0.079)	(0.069)	(0.052)
Group weights ($\hat{\omega}_g$)			
Female - European Union	0.092	0.109	0.152
Male - Other Europe	0.159	0.151	0.169
Female - Other Europe	0.167	0.162	0.172
Male - Africa	0.104	0.113	0.052
Female - Africa	0.146	0.155	0.150
Male - Arrived by age 6	0.181	0.144	0.172
Female - Arrived by age 6	0.152	0.166	0.133
Time weights ($\hat{\lambda}_t$)			
1992	0.000	0.000	0.120
1993	0.000	0.000	0.000
1994	0.556	0.382	0.788
1995	0.358	0.618	0.000
1996	0.086	0.000	0.091
Age	✓	✓	✓
Education	✓	✓	✓
Observations	112,468	112,468	112,468
Mean dep. var.	0.714	0.167	0.118
First-stage F-stat.		604	
Share of compliers ($\hat{\gamma}$)		11.91%	

Notes: This table reports the results of a Synthetic Difference-in-Differences which estimates the effect of the increase in the naturalization rates among EU males, induced by the abolition of compulsory conscription, on their employment status. Three binary employment statuses are considered as outcome variables: Employment, Unemployment, and Inactivity. The first row displays the Intention-to-treat effect, estimated with the Synthetic Difference-in-Differences approach described in Section 3.5.1. The second row shows the Local Average Effect, computed as the ITT divided by the share of compliers. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. *p<0.1; **p<0.05; ***p<0.01. The group and time weights computed following Arkhangelsky et al. (2021) are reported for each specification. Education and age are controlled for in each specification. The F-statistic of the first stage and the share of compliers estimated in Appendix Table C.5 are reported at the bottom of the table. Source: *French Population Census*, wave 2014, INSEE.

Absence of direct effect of military service on labor market outcomes

To include males whose birth nationality does not pertain to the EU in our set of control groups, we must ensure that there was no direct effect of military service on labor market outcomes for second-generation immigrants. This is crucial to attribute the effect of the reform entirely to the shock in naturalization rates it induced rather than to the abolition of military service in itself. To the best of our knowledge, no consensus has emerged on the direct effect of military service on labor market outcomes in the literature so far. Peacetime conscription is shown to have a negative effect on earnings in the Netherlands and in Denmark (Hubers and Webbink, 2015; Bingley et al., 2020), and either no effect or positive effects in Germany, Portugal, and France (Bauer et al., 2012; Card and Cardoso, 2012; Mouganie, 2020). Part of the heterogeneity in the evidence put forward is probably due to the large differences in the modalities of military services across countries.

To test whether military service had a direct effect on labor market outcomes, we compare the difference in employment trends by gender separately for second-generation immigrants born EU citizens and those born with other birth nationalities. Indeed, while EU males experienced both the abolition of military service and a jump in naturalization rates, non-EU males only experienced the abolition of military service. Appendix Figure C.7 shows the difference in employment rates between males and females per birth cohort, separately for those born EU citizens and for those born with other European nationalities or African nationalities. While no jump is observed between non-EU males and females, a clear deviation from the pre-trend is observed for EU citizens, with a magnitude comparable to that obtained with the Synthetic Difference-in-Differences.

This suggests that military service did not have a direct effect on labor market outcomes for second-generation immigrants who did not experience a jump in naturalization. As there is no reason for EU citizenship to affect how military service in itself impacts labor market outcomes, this absence of effect is likely to hold for EU citizens as well.

Absence of effect on female labor market outcomes

Females were not subject to compulsory conscription before the reform, so they were not directly affected by its abolition. Still, women whose spouse pertains to the treatment group could be subject to spillover effects. This could arise if there is assortative mating on citizenship status and if females tend to adapt their employment status to their husbands'.

To test this hypothesis, we document the evolution of females' outcomes according to the birth cohort of their husbands. Specifically, we compare the trend of females whose husband was born EU citizen to that of females whose husband was born with another nationality. We use the latter as a control group for the former because only EU citizens did react to the reform. Thus, for women whose husband is not an EU citizen, no change in labor market outcomes is expected with respect to whether their husband was born before or after the reform. For spouses of EU males, a sudden change in their labor market outcomes concomitant to the reform would indicate the presence of spillover effects.

Because these computations require identifying family links between censused individuals, we perform this exercise on the complementary census sample. Results are presented in

Online Appendix Figure C.8. Around the threshold, no differential trend is observed between females whose husband was born with an EU nationality and females whose husbands were not. This is suggestive evidence that women were not affected by the reform, even those whose husbands were.

Absence of effect on naturalization for immigrants arrived by age 6

Our results show that except males born EU citizens, no group of second-generation immigrants experienced a jump in naturalization rates at the moment of the reform. We include these groups in our set of control groups, to which we add males and females born abroad and who arrived in France by age 6. For them to be relevant control groups, their naturalization decision must not be affected by the abolition of compulsory conscription either.

Online Appendix Figure C.9 shows the naturalization rates by birth cohort for male and female immigrants who arrived by age 6, separately for those born EU and non-EU citizens. Similarly to what we observe for second-generation immigrants., the trend remains flat around the threshold for every group except EU males. This suggests that first-generation immigrants do account for the cost of doing military service when applying for French citizenship as well, and that this cost was not binding for non-EU immigrants. This supports the validity of including males and females born abroad and who arrived in France by age 6 in our set of control groups, as long as EU citizens are excluded from the male control group.

However, while second-generation immigrants must decide whether or not to take-up citizenship at the age of 18, first-generation immigrants who want to become French choose at which age they apply for citizenship. Given that the French military service was less intensive past certain age thresholds, we must ensure that the abolition of compulsory conscription did not entail a drop in the age of naturalization for first-generation immigrants. The year of naturalization is not available in census data, so we exploit the electoral information of the Permanent Demographic Sample and use the age at registration in the electoral register as a proxy for the age of naturalization.

As shown in Online Appendix Figure C.10, the trends in registration rates in the electoral lists reproduce closely the trends in naturalization rates depicted in Figure 3.2. We show the evolution of the age at registration in the electoral register by birth nationality groups and generation of immigration in Online Appendix Figure C.11. In this Figure we consider first generation immigrants arrived up to age 18 because the date of registration is not available for enough individuals among those arrived by age 6. The average age at registration of first-generation immigrants arrived by age 18 follows a stable path over the period, with no jump or deviation from the trend around the reform. This supports that males and females born abroad and who arrived in France by age 6 are suitable control groups in our setting.

3.5.4 Mechanisms

The fact that the sudden increase in citizenship acquisition was only experienced by EU citizens mechanically shuts down most of the mechanisms that may underlie its effect on labor-market outcomes. Indeed, none of the right to reside and work in France, the access to welfare benefits, or the stability granted by citizenship, could be at play here. In this section, we investigate the remaining two candidate mechanisms typically put forward in the literature: labor-market access restrictions, and discrimination.

Labor-market access

To get a better understanding of how naturalization fostered labor market outcomes, we analyze the effect of the shock on employment within certain types of occupations. Specifically, we focus on public sector jobs and on self-employment because a reaction in these segments of the labor market could be indicative of specific underlying mechanisms.

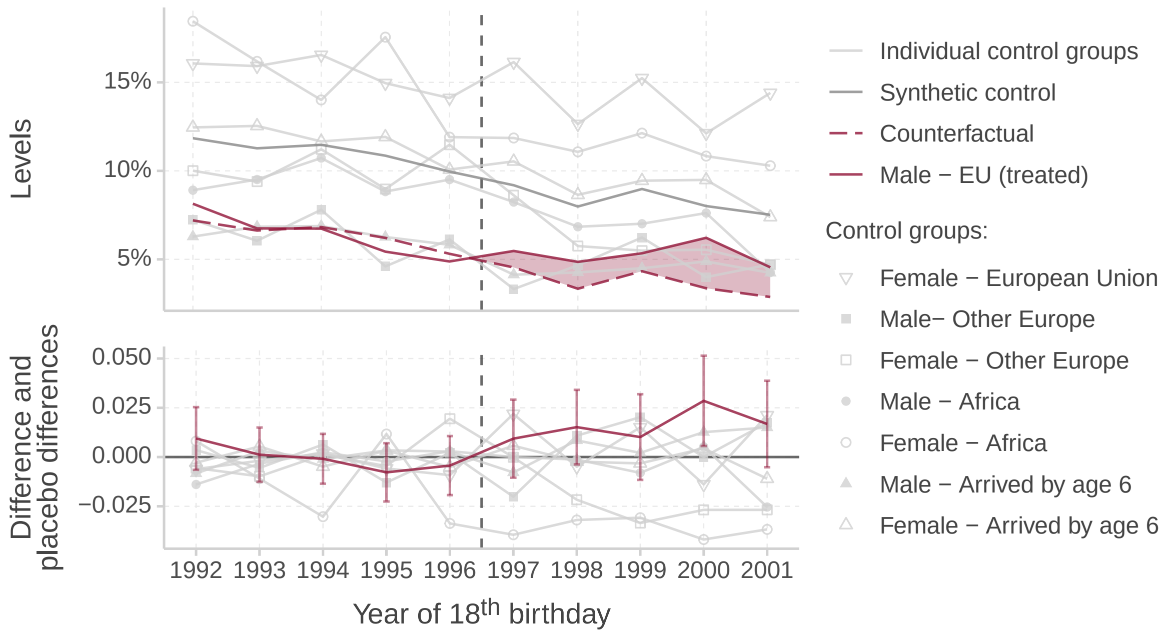
Given that part of the public sector is conditional on having French nationality, naturalization mechanically expands the labor market access of foreigners, providing more outside options. We investigate whether this played a role in the positive employment effect of naturalization. To do so, we estimate a Synthetic Difference-in-Differences as described in Section 3.5.1 on the share of individuals working in the public sector. Results are represented graphically in Figure 3.5. The corresponding regression results are documented in Online Appendix Table C.7.

Results suggest that the 11.9 percentage point increase in the naturalization rates of EU citizens induced a 1.6 percentage point increase in their probability of being employed in the public sector. This corresponds to an 13.3 percentage point increase for the group of compliers. The direction of this effect is coherent with the fact that some public sector jobs require French citizenship. However, the magnitude of the effect is too large to be entirely attributed to these specific positions. This hints at the fact that citizenship is a particularly important criterion in the hiring decision of public sector recruiters.

If the acquisition of citizenship mitigates hiring discrimination, it may provide individuals with more and better labor market opportunities. These new alternative options should reduce the prevalence of more precarious and unstable positions such as self-employment. To test this hypothesis, we estimate a Synthetic Difference-in-Differences on the share of self-employed individuals. Results are represented graphically in Figure 3.6. The corresponding regression results are reported in Online Appendix Table C.8.

We observe a decreasing trend in self-employment for the treated group, accentuated after the reform. Even though barely significant, this pattern is in line with the idea that citizenship acquisition could indeed constitute a stepping stone to depart from self-employment towards other types of occupations that may offer more stability on the labor market. This decrease in self-employment could also hint at the fact that citizenship acquisition may lead to a tran-

Figure 3.5: Synthetic Difference-in-Differences - Public job



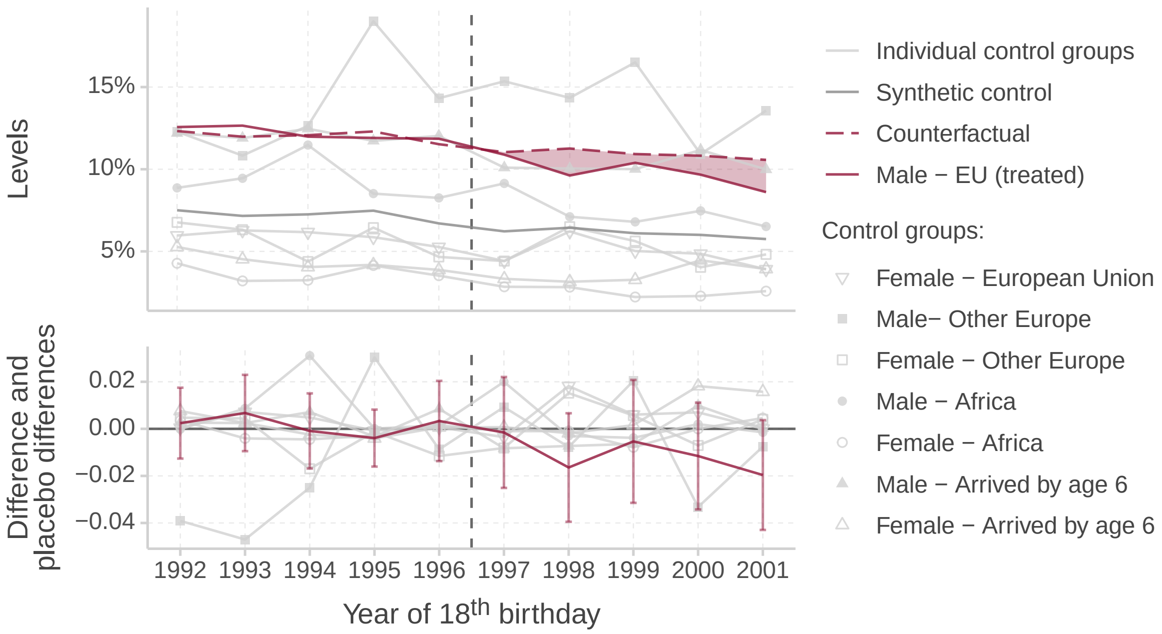
Notes: The top panel of this figure shows the share of individuals working in the public sector by birth cohort within each of the groups used in our Synthetic Difference-in-Differences approach. That of the treated group, male European Union citizens, is represented with a solid red line. That of each individual control group is represented with a gray line, whose markers indicate the corresponding group. The solid gray line shows the trend of the synthetic control group. The dashed red line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid red line, centered at the pre-treatment average difference weighted by the estimated time weights. Vertical lines show the corresponding 95% bootstrapped confidence intervals. The gray lines are placebo effects, each using one of the control groups as the treatment group instead of EU males. Corresponding regression results are reported in Online Appendix Table C.7. Source: *French Population Census*, wave 2014, INSEE.

sition from the informal to the formal sector of the economy. This hypothesis is all the more credible given that citizenship acquisition is associated with a significant decrease in inactivity (Govind, 2021), which would typically be the employment status reported by informal workers.

Labor-market discrimination

Since the positive labor market effects cannot be fully explained by access to the public sector, in this section we discuss whether labor market discrimination could partly channel the effect of naturalization on employment. To do so, we document the extent to which second-generation EU citizens at birth experience discrimination in France. We exploit the Trajectory and Origin survey data which is a nationally representative survey with a focus on individuals with a migration history. It contains rich and detailed information on the origins and experience of racism and discrimination for a small sample of the immigrant population in France.

Figure 3.6: Synthetic Difference-in-Differences - Self-employed



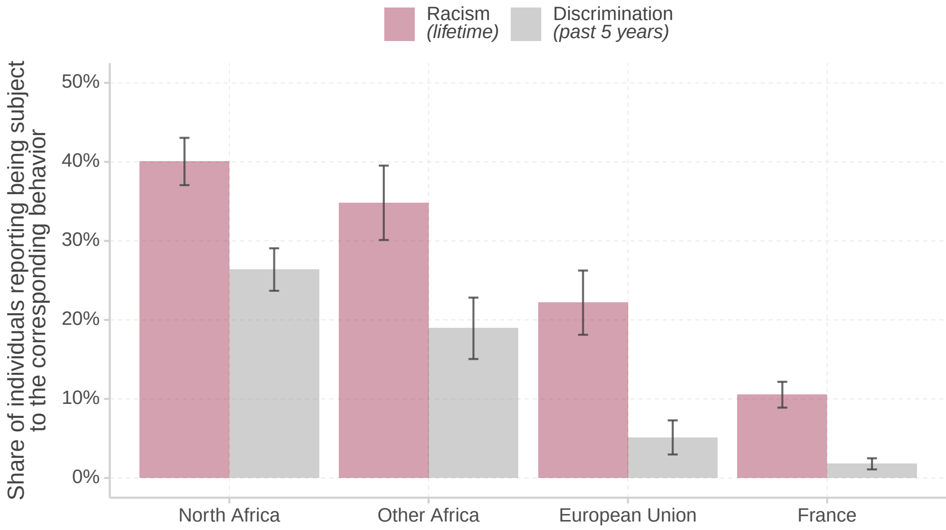
Notes: The top panel of this figure shows the share of self-employed individuals by birth cohort within each of the groups used in our Synthetic Difference-in-Differences approach. That of the treated group, male European Union citizens, is represented with a solid red line. That of each individual control group is represented with a gray line, whose markers indicate the corresponding group. The solid gray line shows the trend of the synthetic control group. The dashed red line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid red line, centered at the pre-treatment average difference weighted by the estimated time weights. Vertical lines show the corresponding 95% bootstrapped confidence intervals. The gray lines are placebo effects, each using one of the control groups as the treatment group instead of EU males. Corresponding regression results are reported in Online Appendix Table C.8. Source: *French Population Census*, wave 2014, INSEE.

In Figure 3.7, we estimate the share of reported discrimination and racism experienced for children of immigrants from different origin groups.

We see that, despite being less discriminated against than Africans, EU citizens born in France report having experienced discrimination and racism in France significantly more often than individuals born in France with no migration history. This suggests that this margin is relevant for second-generation EU citizens and that obtaining French nationality might play a role in reducing the discrimination they experience. Indeed, nationality could be seen as a signal of integration by the employer. In France, it is common for individuals of foreign origins to signal French nationality on their CV, as typically recommended in CV guides in the 1990s.[14]

[14]See for instance Duhamel and Lachenaud (1999).

Figure 3.7: Share of reported racism and discrimination by group of birth nationalities



Notes: Red bars represent the share of individuals reporting that they have been subject to racist insults, remarks, or behaviors in France during their lives because of their origins or nationalities. The gray bars represent the share of individuals reporting being subject to unfair treatment or discrimination related to their origins or nationalities over the past five years. Vertical lines represent the corresponding 95% confidence intervals. Estimates are computed within each of the origin groups reported along the x-axis. The sample consists solely of individuals born in France. Source: *Trajectories and Origins* wave 2 (2019-2020), INED-INSEE.

3.6 Robustness

3.6.1 Military service in the country of origin

We show in Section 3.4 that only individuals born citizens of the European Union react to the abolition of compulsory conscription. This is in line with the fact that their benefits of acquiring French citizenship are lower than those of other nationality groups, for which the cost of military service was not binding under compulsory conscription.

Yet, this heterogeneity could also arise from variations in the cost of military service across birth nationality groups. Indeed, for an individual whose country of birth nationality has a military service that is longer or harder than the French one, the French military service would not constitute a comparatively large cost.

We thus distinguish individuals according to whether military service in their country of birth nationality in 1996 was longer or shorter than the French military service, within the three groups of birth nationality. Appendix Figure C.12 shows the heterogeneity in the trend in male and female naturalization rates along these two dimensions. The effect of the abolition of compulsory conscription does not appear to vary within nationality groups depending on the relative length of the military service. Only European Union citizens at birth do react to the reform, regardless of the relative length of the French military service. This supports the idea that it is indeed a benefits imbalance that drives the heterogeneity across birth nationality groups.

3.6.2 Anticipation of the reform

Even though the reform did not exempt men born before the 1st of January 1979 from doing military service, it became easier to avoid doing military service after the announcement of the reform.[15] Many of the French men who were born in the last cohorts subject to compulsory conscription, and who postponed their military service to pursue higher education, have actually never fulfilled their military duty.

In our case, anticipation could lead individuals born before 1979, who would not have taken up French citizenship absent the announcement, to actually take up French citizenship because they anticipate that they will be able to avoid military service. This type of behavior would bias our results because these individuals would pertain to cohorts considered unaffected while actually being treated.

To investigate whether this is an issue in our setting, we show the naturalization rates of males and females born EU citizens by month of birth instead of year of birth in Online Appendix Figure C.15. Due to the amount of noise in the monthly trend, it is difficult to confidently identify from which specific birth month individuals react. Still, there is a clear anticipation phenomenon of two to four months. Anticipation thus starts relatively late given that presidential announcements were made seven and ten months before the effective abolition of compulsory conscription.

It is particularly important to alleviate anticipation effects when using a Synthetic Difference-in-Differences approach because the weighting of time units tends to give more importance to more recent periods, which are the ones potentially subject to anticipation. We thus test the robustness of our result to the exclusion of the 1978 birth cohort, with and without time weights. The resulting ITT coefficients are presented in Online Appendix Table C.9.

ITT effects tend to be slightly smaller when using time weights, which is usually the case because they tend to fit more closely on recent values of the pre-trend, and thus avoid the propagation of slight wedges at the end of the pre-period to the estimated difference in the post-period. Still, results are robust to the exclusion of the 1978 cohort in terms of magnitude and statistical significance, both with and without time weights. The same pattern is observed for LATE coefficients, presented in Online Appendix Table C.11.

3.6.3 Differential attrition

All along the analysis we implicitly assume that second-generation immigrants would stay in France in adulthood in the same proportions among cohorts turning 18 before and after the abolition of compulsory conscription. If compulsory conscription influences individuals' migration decisions, differential attrition around the threshold could induce a selection bias.

To investigate this potential issue we make use of the Permanent Demographic Sample,

[15] Fize and Louis-Sidois (2020) document this issue in more detail using data from the archives of the French Ministry of Defense.

which follows a subset of the main population census across census waves. Appendix Figure C.14 shows the probability for individuals born in France without French citizenship to be observed in the 2014 census wave, i.e., in our main sample, conditional on being observed in the 1990 (top panel) or 1999 (bottom panel) census wave by gender and birth cohort. We separate individuals who were born EU citizens from other birth nationalities.

Results reveal no differential attrition around the threshold. Males' and females' probabilities to be observed in the 2014 census wave conditional on being observed in either the 1990 or the 1999 census wave follow a very stable trend over the period. This holds both for individuals whose birth nationality pertains to the EU and for other second-generation immigrants.

3.6.4 Absence of direct effect of military service on education

Results on the heterogeneous effects of the abolition of compulsory on citizenship take-up across education levels implicitly assume that conscription does not directly impact education choices. However, Maurin and Xenogiani (2007) show that compulsory conscription in France incentivized young males to pursue higher education in order to postpone military service. Hence, the abolition of compulsory conscription had a direct negative effect on males' education compared to females' because education was no longer a way to avoid military service. However, the incentive scheme faced by individuals born in France without French citizenship is not equivalent to that faced by individuals born French. Indeed, most French citizens have no other option than to do military service because they do not have a second nationality to rely on.

To test whether the effect identified by Maurin and Xenogiani (2007, Figure 2) applies to our setting, we reproduce their result as closely as possible with our data.[16] Appendix Figure C.13 shows the difference between the proportion of graduates in a given birth cohort and the proportion of graduates in the 1974 birth cohort, within gender. We report this separately for individuals born French and for the three groups of birth nationality of second-generation immigrants.

Similarly to Maurin and Xenogiani (2007), we observe that for French-born individuals, the divergence in the growth of the share of graduates between males and females is more marked from 1978 onward than for the previous birth cohort.[17] However, we do not observe

[16] Maurin and Xenogiani (2007) use the French Labor Force Surveys 1991-2002 to show the changes in the proportion of men and women aged 17 to 23 still in school across birth cohorts. We cannot use the exact same variable definition because of differences in data collection periodicity. Alternatively, we consider two variables: the proportion of individuals who completed high school, and the proportion of individuals who pursued higher education.

[17] In our case, however, results indicate that the divergence did not begin in 1978 but started slightly and progressively from the beginning of the study period. This difference with what is put forward in Maurin and Xenogiani (2007) may be due to the difference in the variable definition or to sampling variation between the two data sources. Notably, much more stable trends are obtained from the population census than from the labor force survey, probably because of its much larger sample size.

this pattern for either of the three birth nationality groups of second-generation immigrants. This suggests that while compulsory conscription did incentivize French-born males to pursue higher education, second-generation immigrants did not adopt this draft-avoidance behavior. This could be explained by the fact that second-generation immigrant males could adjust on the citizenship margin at the age of 18 via their birth nationality.

3.6.5 General equilibrium effects

The fact that Maurin and Xenogiani (2007) identify a negative effect of the abolition of compulsory conscription on education for the overall male population raises an issue of general equilibrium effect. Indeed, if individuals born after the reform in the majority group become less educated due to the abolition of compulsory conscription, this could constitute a shock on the labor market for other males in these birth cohorts. As the rates of high school graduation and higher education remained quite stable for second-generation male immigrants, their relative education level increased.

To test this hypothesis, we control for the share of natives in individuals' employment zone.[18] Results are presented in Online Appendix Table C.10. Controlling for the share of natives in the employment zone leaves our estimates virtually unchanged. This absence of a general equilibrium effect could be due to non-substitutability between the two groups resulting from sectoral labor segregation, to labor market frictions in the matching process, or to any other departure from neo-classical settings.

3.6.6 Control group combinations

We provide a comprehensive assessment of the role that the set of control groups used to generate the synthetic control group has on our results in Appendix Figures C.16 to C.20.

Each row of the tile plot on the left panel corresponds to a given set of control group. A tile whose color lies on the gradient in the legend indicates that the control group of the corresponding column was included in the computation of the synthetic control group, while a gray tile indicates that it has been excluded. There are 127 rows, one for each of the possible combinations of control groups formed by sets of 1 control group to 7 control groups. The specification which includes the whole set of 7 control groups is our baseline specification.

Specifications are sorted by the value of the corresponding ITT coefficient reported on the scatter plot on the right panel. The color of each dot indicates the p-value of the pre-trend. To obtain this value, we compute the difference between the average outcome in the treated group and that in the synthetic control group, and regress it on the birth cohort for the years before the reform. The p-value is then computed from a standard two-sided t-test on the slope coefficient. The vertical solid line represents our baseline coefficient, and a vertical dashed line

[18]The employment zone is a geographical unit whose borders are based on the share of individuals who both work and live in the zone. It is computed with the algorithm LabourMarketAreas, available in OpenAccess.

is placed at 0. The kernel density of this distribution of 127 coefficients is shown on the top panel.

There are two main takeaways from these figures. First, pre-trends are quite well captured as long as there are enough control groups in the set. Second, when the set of control groups is large enough to allow the synthetic control group to capture the pre-trend, resulting estimates remain relatively close to our baseline.

3.7 Conclusion

This paper aims to shed light on the effect of policies that change naturalization costs on second-generation immigrants' naturalization decisions, and on the causal effect of naturalization on labor market outcomes. We rely on the fact that citizenship is automatically granted to individuals born in France to foreign parents at the age of 18 and that the obligation for citizens to undergo military service makes naturalization a costly choice. We exploit the abolition of compulsory military service in France as an exogenous shock that reduced the cost of naturalization for second-generation men while not impacting women.

We show that the decrease in the cost of naturalization led to a sudden increase in naturalization rates. In line with the theoretical framework developed to rationalize take-up decisions, this increase is entirely driven by individuals with the lowest expected benefits: individuals born European Union citizens who can already work and reside in France. They take up citizenship less often when it is tied to doing military service, while this cost is not binding for other birth nationality groups. Additionally, we show that there is a skill gradient in the cost of military service, with the low-educated reacting more to the reform. This feature is reminiscent of policies that are explicitly designed to affect naturalization rates such as language tests or financial requirements.

The fact that EU males are the only group to react to the reform allows us to exploit the variety of unaffected gender and birth nationality groups in a Synthetic Difference-in-Differences approach to study the causal impact of this rise in naturalization rates on labor market outcomes. Our results show positive labor market effects, with an increase in employment and a reduction in inactivity for EU citizens. We find that this employment effect is accompanied by an increase in public sector employment and by a decrease in self-employment.

Altogether, these results provide valuable insights both on the citizenship take-up decision of children of immigrants, and on the gains from naturalization for those who would have renounced French citizenship had it entailed higher costs. In particular, our findings illustrate how policies that affect naturalization costs heterogeneously across immigrants can unintentionally divest specific groups of improved labor market prospects.

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Appendix A

Intergenerational income mobility in France: A comparative and geographic analysis

A.1 Data details

The Permanent Demographic Sample (EDP) is a panel of individuals which the French statistical office, INSEE, started in 1968.[1] It combines several administrative data sources on individuals born on the first four days of October.[2] Individuals born on one of these days are called EDP individuals. The EDP gathers data from 5 administrative sources: (i) civil registers since 1968; (ii) population censuses since 1968 (exhaustive in 1968, 1975, 1982, 1990 and 1999, and yearly rotating 20% random samples since 2004); (iii) the electoral register since 1990; (iv) the *All Employee Panel* since 1967; and (v) tax returns since fiscal year 2011.

Each time an individual born on the first four days of October appears in one of these administrative datasets, the information contained in it is added to their individual identifier in the EDP. Therefore all these datasets can be matched together using a common individual identifier. For our analysis we use data from civil registers, the 1990 census, the All Employee Panel and tax returns. We describe each data source in detail below.

Civil Registers. They contain information from birth certificates of EDP individuals and their children, as well as death and marriage certificates of EDP individuals, since 1968. We use birth certificates of EDP individuals and their children which include the child's gender, date and place of birth, and information on each parent including date and place of birth, nationality and occupation. There are no data breaks or missing certificates for the years under study (1972-1981).

1990 Census. It contains socio-demographic information about EDP individuals, as well as, though to a lesser extent, about members of their household. These include the individual's date and place of birth, nationality, education, occupation, marital status, household structure, dwelling characteristics, building when relevant, and municipality.

All Employee Panel. It combines two sources of data: the annual declarations of social

[1]The EDP user guide (in French) can be found here.

[2]The EDP selection criterion has progressively widened to include individuals born on the first days of January, April, and July. See Robert-Bobée and Gualbert (2021) for a detailed description of the dataset.

data (*déclarations annuelles des données sociales* - DADS) and data on central government employees (*fichiers de paie des agents de l'état* - FPE). All businesses are obliged to annually communicate the declarations of social data about their employees to a network of private organizations (*Unions de recouvrement des cotisations de sécurité sociale et d'allocations familiales* - URSSAF) coordinated by a government agency (*Agence centrale des organismes de sécurité sociale* - ACOSS). The All Employee Panel data are reported at the worker-year level, aggregated by INSEE from data at the worker-firm-year level. As such, annual pretax wage and annual hours worked correspond to the sum over all the individual's salaried activities. The job characteristics correspond to the year's "main" job, that is the job for which the pay period was the longest and, in case of a tie, the job with the highest wage.

Between 1967 and 2001, data is only available for individuals born on an even year. The scope of workers covered by the All Employee Panel has varied over time. Since 1967 in metropolitan France, all private sector employees, except those in the agricultural sectors, and including employees of public enterprises, are covered. The hospital public service is integrated in 1984, the state civil service and local authorities in 1988. France Télécom and La Poste employees appear only in 1988 as well. See Appendix A.3.1 for a robustness check to this public sector coverage evolution. The agricultural sector and overseas territories are included in 2002, and employees of private employers in 2009. Unemployment insurance is included from 2008 onwards. Lastly, because of increased workload due to the population censuses of 1982 and 1990, the All Employee Panel data were not compiled by INSEE in 1981, 1983 and 1990.

Tax Returns. They are compiled using housing and income tax forms filed for incomes earned from 2010 to 2016. In particular, household-level tax returns information is constructed based on dwellings where an EDP individual is known either from the income tax return or from the principal housing tax (*taxe d'habitation principale*). The location of the individual is that declared on January 1st of the fiscal declaration year. Income variables are available at the household-level as well as at the individual level. Since the information is gathered based on living in the same dwelling, household income is computed not only for couples who file their taxes jointly, but also for couples who live together, an increasingly common arrangement. This departs from existing studies based on tax returns data which can only assign households based on marital status (Chetty et al., 2014). The scope of fiscal households excludes individuals living in collective structures (retirements homes, religious communities, student accommodations, prisons, etc.) as well as those most in distress, who live in precarious housing (worker hostels, etc.) or are homeless.

A.2 PSID validation exercise

We use the Panel Study of Income Dynamics (PSID) to assess the extent to which OLS and two-sample two-stage least squares (TSTSLS) estimates of rank-based intergenerational mobility measures differ from one another. Our sample and definition choices aim to be as close as possible to our main analysis setting while at the same time maximizing sample size. Note also that for this reason we use all of the PSID, rather than only the nationally-representative Survey Research Center (SRC) component. The main conclusions of our baseline results are robust to using only the SRC sample or to using various weighing schemes as shown in Section A.2.7.

A.2.1 Sample definitions

Sample of children. It consists of individuals who are (i) born between 1963 and 1988, (ii) observed as children in a family unit at least once, and (iii) observed at least once as reference person or partner in a family unit between 30 and 50 years old. Restriction (i) enables us to identify parents, while restriction (ii) enables us to observe children's incomes. The final sample contains 5,655 children.[3]

Sample of parents. Following Chetty et al. (2014), for each child, we define parent(s) as the reference person and partner of the family unit in which the child is first observed.[4] We then follow these individuals' incomes over time.[5] As Chetty et al. (2014), for simplicity, we fix each child's parent assignment regardless of any potential subsequent changes to the child's family unit reference person and partner. The final sample contains 5,785 (unique) parents.

A.2.2 Variable definitions

All income variables are measured in 2019 dollars, adjusting for inflation using the consumer price index (CPI-U). Following Lee and Solon (2009) and Mazumder (2016), we exclude income observations obtained by "major assignment". We opt for larger age ranges than in our main analysis (30-50 vs 35-45) to increase our sample size. However, our baseline results are robust to averaging over 35-45 as in the main analysis (see Appendix Table A.5).

Parent income. We rely on two parent income definitions. First, as a benchmark, we measure parent income as total pretax income at the household level, which we label parent family income. Specifically, we define parent family income as the sum of taxable income of the family unit's reference person and partner, and total transfer income of the reference person and partner.[6] Taxable income is equal to the sum of reference person's labor income, the partner's labor income, income from assets, and net profit from farm or business. This measure enables us to obtain benchmark estimates that the TSTSLS estimation strategy is supposed to yield.

Second, since in TSTSLS strategies parent family income is rarely observed, we also define parent labor income as the sum of family unit's reference person and partner's individual labor incomes (money income from labor, including self-employment income).[7] This follows very closely the setting adopted in the main analysis.

[3]See Appendix Table A.2 for the sample size at each additional restriction.

[4]90% of individuals born in 1963-1988 are first observed as children in a family unit prior to age 18.

[5]Note that this differs from the following studies using the PSID: Lee and Solon (2009) (use the family taxable income in which the children find themselves between ages 15 and 17), Mazumder (2016) (uses the PSID's Family Identification Mapping System (FIMS) to identify fathers), Jerrim et al. (2016) (do not explain exactly how fathers are identified; to be precise, the authors write "[...] we only include sons whose father can be identified," (Jerrim et al., 2016, p.89)), and Bloise et al. (2021) (do not explain exactly how fathers are identified; to be precise, the authors write "we include only sons whose real fathers have at least five years of positive earnings [...]," (Bloise et al., 2021, p.650)).

[6]The accuracy of the family's taxable income is missing in 1993-1996 and in 2001-2019. Total transfers are missing in 1968 and 1969. Total transfers include aid to families with dependent children, supplemental security income, other welfare payments, social security payments, other retirement, pensions and annuities, unemployment pay, workmen's compensation, child support, help from relatives, and other transfer income.

[7]The accuracy of the reported value for the reference person is missing in 1994-1996. Moreover, for partners, there was a small change in income definition in 1994: total labor income became total labor income excluding farm and business income.

For both parent family income and parent labor income, we average income values over 30 and 50 years old. Specifically, we take the sum of the average for the father and the average for the mother if both parents are observed, and take the average of the only observed parent otherwise.

Child income. We define child income in the same way as parent family income, again averaging over income observations between 30 to 50 years old.

Adjustment for household size. When defining income variables we follow Chetty et al. (2014), and do not account for household size (i.e., whether there is also a partner in the family unit). This way of defining parent income mechanically hinders single-headed households, both parents and children.[8] We therefore show in Table A.5 results when dividing family income measures by the number of observed reference person and partner in that year.

Descriptive statistics Appendix Table A.1 displays some descriptive statistics for our sample of parents and children. Parents' incomes are observed at a slightly older age (39) than that of our children (34). In both cases, incomes are measured sufficiently late in the lifecycle to limit lifecycle bias.

Table A.1: Descriptive statistics

	N	Missing (%)	Mean	Std. Dev.	25th pctile	Median	75th pctile
Parents							
Family income (average 30-50 yrs old)	5,785	5.88	82,047	66,121	42,976	72,081	105,523
Number of family income observations	5,785	5.88	13	5	10	15	18
Mean age at family income obs.	5,785	5.88	39	3	38	39	40
Labor income (average 30-50 yrs old)	5,785	5.62	39,679	39,946	13,575	30,800	55,074
Number of labor income observations	5,785	5.62	14	5	10	15	19
Mean age at labor income obs.	5,785	5.62	39	3	38	39	40
Fraction single parents	20.19%						
Fraction female among single parents	92.21%						
Mother's age at child birth	3,135	0.00	25	5	21	25	29
Father age at child birth	2,650	0.00	28	6	24	28	32
Children							
Family income (average 30-50 yrs old)	5,655	3.02	80,539	75,072	34,936	64,092	104,517
Number of family income observations	5,655	3.02	5	3	2	4	8
Mean age at family income obs.	5,655	3.02	34	3	32	34	38
Fraction female	53.60%						

Notes: See Sections A.2.1 and A.2.2 for details on sample construction and income definitions. Missing income observations can also correspond to values obtained by 'major assignment'.

A.2.3 Benchmark estimates

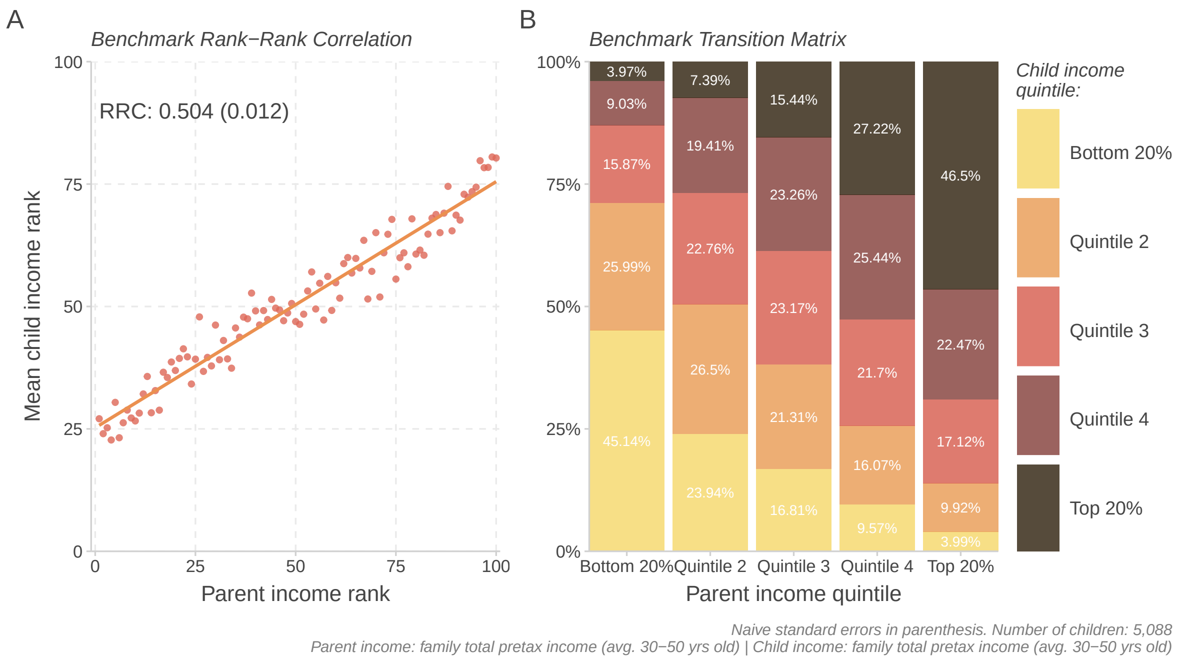
We first estimate the rank-rank correlation (RRC) and transition matrix using the family income definitions for both parents and children (results for the intergenerational income elasticity (IGE) are presented in Appendix Figure A.5). Recall that in the TSTSLS setting the parent income definition is parent labor income while we are actually interested in parent family income, a more comprehensive parent income measure. In theory, the extent to which

[8] Interestingly, this is an issue Raj Chetty alludes to in his conversation with Tyler Cowen in his 2017 *Conversations with Tyler* podcast episode. Indeed, Chetty noticed that daughters from affluent families in the Bay area have low *household* incomes but have very high *individual* incomes because they are significantly less likely to be married than if they had grown up somewhere else.

the additional incomes included in parent family income relative to parent labor income generate large rank reversals is ambiguous. Moreover, TSTSLS estimates necessitate restricting the analysis to the sample of children for whom parents' characteristics are observed (e.g., education and/or occupation, etc.). Such restrictions could potentially induce some biases relative to the statistic one is actually interested in measuring.

National results. Appendix Figure A.1 displays the benchmark RRC and transition matrix for the baseline parent and child income definitions. The baseline RRC is 0.504, compared to 0.34 found in Chetty et al. (2014). Such a high RRC likely reflects the fact that the PSID contains oversamples of disadvantaged families (see Appendix Figure A.7 for estimates obtained only on the Survey Research Center (SRC) component of the PSID). The benchmark transition matrix confirms this intuition. The share of children from the bottom 20% who reach the top 20% in adulthood is 4%, close to half the share found by Chetty et al. (2014) (7.5% for children born in 1980-1982). Persistence at the bottom and top are also very strong at roughly 45%.

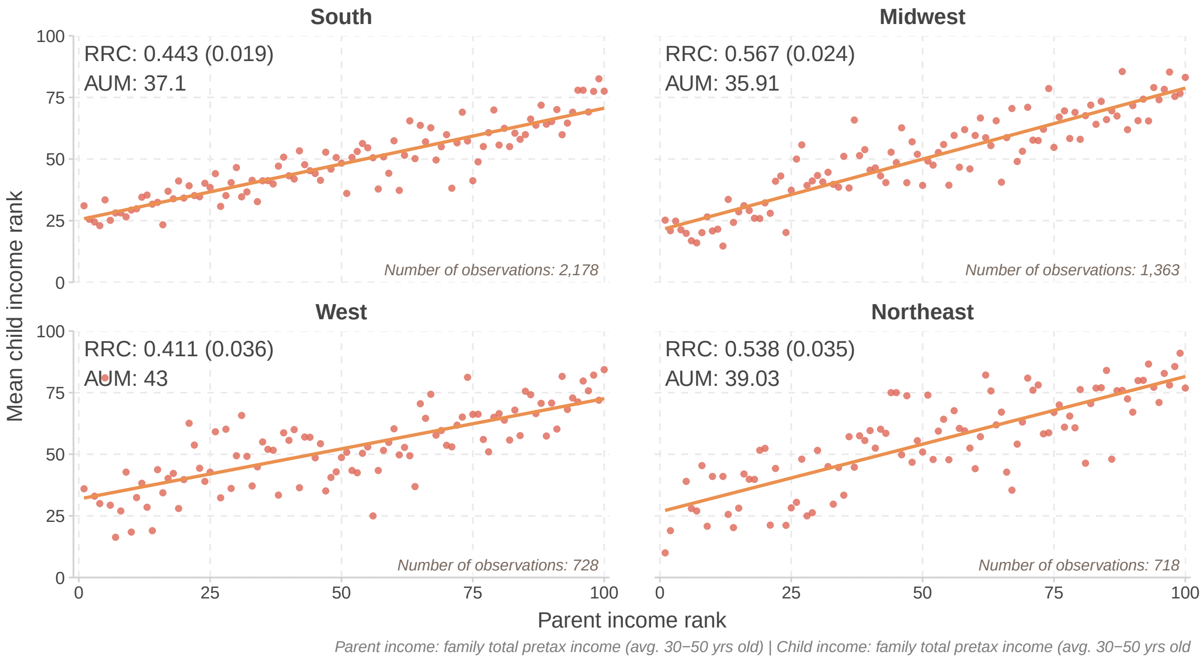
Figure A.1: Benchmark rank-rank correlation and transition matrix



Notes: This figure presents the rank-rank correlation (panel A) and the transition matrix (panel B). It is computed on the Panel Study of Income Dynamics (PSID). The sample used is restricted to children born between 1963 and 1988 who are observed at least once as children in a family unit and at least once as a reference person or partner in a family unit over ages 30-50. Child income is the mean of family total income over ages 30-50. Parent income is the sum of father and mother mean family total income over ages 30-50. In panel A, the fitted line is a linear fit through the conditional expectation. We report coefficients and naive standard errors (in parenthesis) obtained from OLS regressions of child income rank on parent income rank with child cohort fixed effects, on the microdata for the full sample.

Subnational results. Due to sample size constraints we explore geographic heterogeneity in intergenerational by Census Region (Northeast, Midwest, South, and West). Specifically, we define a child's Census Region as the most common region of residence until age 18 (included). Appendix Figure A.2 displays the benchmark RRC and absolute upward mobility (AUM) estimates by Census Region. AUM is defined as in Chetty et al. (2014) as the expected income rank for children at the 25$^{\text{th}}$ percentile of the parent income distribution.

Figure A.2: Benchmark rank-rank correlation and absolute upward mobility by census region



Notes: This figure presents Census Region-level estimates of the rank-rank correlation (RRC) and absolute upward mobility (AUM). To compute local estimates, individuals are assigned to their most common Census Region of residence until age 18 (included). See Appendix Figure A.1's notes for details on data, sample and income definitions.

A.2.4 OLS vs. TSTSLS comparison

We now turn to the comparison between estimates obtained with OLS and those obtained with TSTSLS. The PSID enables us to compare estimates of intergenerational mobility we obtain when observing parents' incomes and when predicting them using observable characteristics such as education and occupation. Since in the main analysis and in virtually all TSTSLS studies only parents' labor incomes or wages are observed, we define parents' income as individual labor income, while keeping in mind the benchmark estimates presented in the previous section. We follow the main analysis' definitions as closely as possible. We proceed in the following way.

Parent income prediction

Let Z denote a set of characteristics observed for parents. We can express their labor incomes y as $y_i = \beta Z_i + \epsilon_i$. We estimate this first-stage equation by OLS on our sample of parents, and predict out of sample using a 5-fold cross-validation approach. Specifically, we split the sample of parents in five random subsamples of equal size, and for each subsample we predict income using the first-stage estimated on the remaining four subsamples. As such all predicted incomes are conceptually made from a random sample of parents taken from the same population. We see these out-of-sample predictions as imitating very closely settings in which researchers do not observe the actual parents' incomes but observe the incomes of other parents taken from the same population (i.e., with children born in the same years).

We define parent income y as log mean (individual) labor income over ages 30 to 50. Once we have predicted labor incomes for children's father and/or mother, we compute a measure of labor income at the household level as the sum of father and mother predicted labor incomes if we have identified two parents, and predicted labor income of the only parent otherwise.[9] We display parents' (out-of-sample) predicted labor incomes against observed labor incomes in Appendix Section A.2.7.

For our baseline results, we define Z in the most similar way as possible as to our paper. Specifically, Z includes (i) education (7 categories; highest years of school completed), (ii) 3-digit occupation (334 cat.; most common occupation, including inactivity status, between 30 and 50 years old), (iii) demographic characteristics (birth cohort, race (5 cat.; most recent observation)), and (iv) state fixed effects (most common state of residence between 30 and 50 years old). The precise details of the construction of each of these variables are described in Appendix Section A.2.6. This set of predictors departs from the ones used in the main analysis because (i) we were unable to find a cross-walk between the 3-digit classification and a 2-digit classification, (ii) nationality is not available in the PSID, and (iii) country of birth is not available in the PSID. We replaced these variables with race. In Appendix Table A.4 we present results when incrementally including these predictors and find that the TSTSLS bias stabilizes once occupation is included.

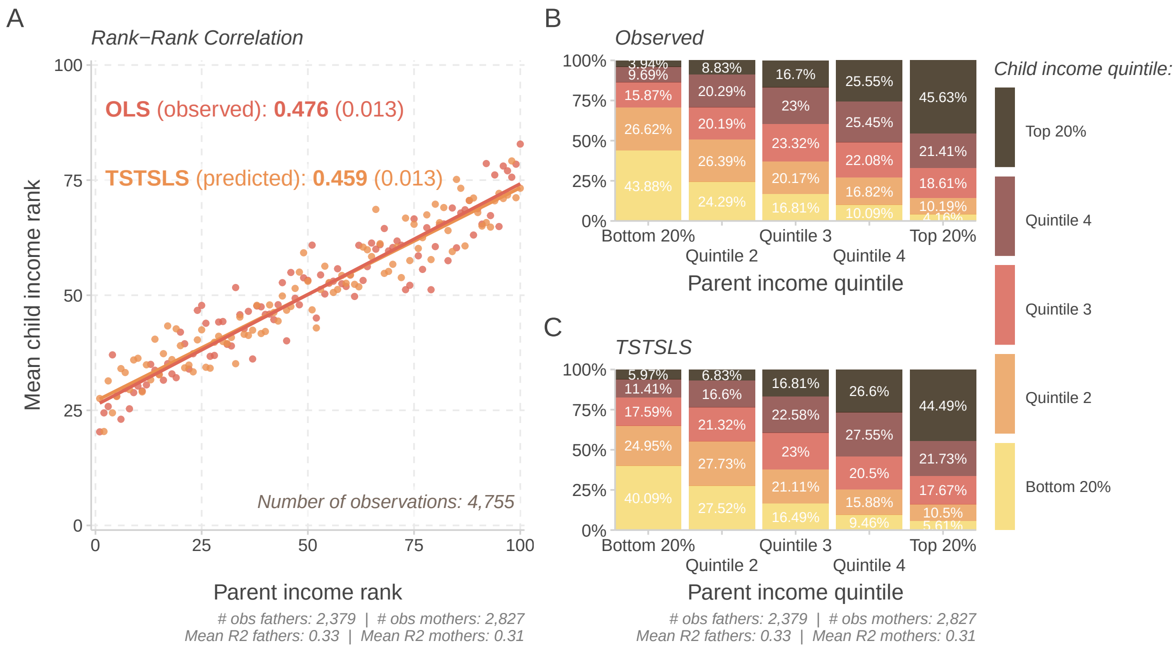
National results

Appendix Figure A.3 presents the main results from our validation exercise. Our TSTSLS estimate of the RRC is 0.459. On the exact same sample the OLS estimate is 0.476. Our benchmark RRC from the previous section was 0.504. The TSTSLS estimate is therefore roughly 4% smaller than the OLS estimate on the same sample, and 9% smaller than the benchmark OLS estimate (defining parent income as parent family income). These differences are quite small relative to the large differences in RRC estimates observed across countries (as well as within country across studies). Moreover, and importantly, the TSTSLS estimate appears to *understate* persistence, suggesting it provides a lower bound for intergenerational persistence.

The TSTSLS estimates for the transition matrix also appear to represent upper bounds on intergenerational (upward) mobility. The P(Top 20% | Bot. 20%) is roughly 6% in the TSTSLS case and 4% in the OLS case (4% as well in the benchmark), P(Bot. 20% | Bot. 20%) is 40% vs. 44% (45%) and the P(Top 20% | Top 20%) is 44% vs. 46% (47%). In Appendix A.2.7 we show that the TSTSLS bias of the RRC is largely unaffected by the number of parent income observations used. Moreover, Table A.5 shows our results are qualitatively robust to (i) using nationally-representative Survey Research Center (SRC) sample of the PSID, (ii) computing parent and child incomes over ages 35-45 as in the main analysis, (iii) dropping income observations equal to zero when computing parent and child incomes, and (iv) accounting for household size in the income definitions (additional details in Section A.2.7). Moreover, Table A.6 shows that using the longitudinal or cross-sectional weights moderately increases the TSTSLS RRC downward bias.

[9] Results when dividing by the number of parents are presented in Appendix Table A.5 (col. (5)).

Figure A.3: OLS vs. TSTSLS RRC and transition matrix

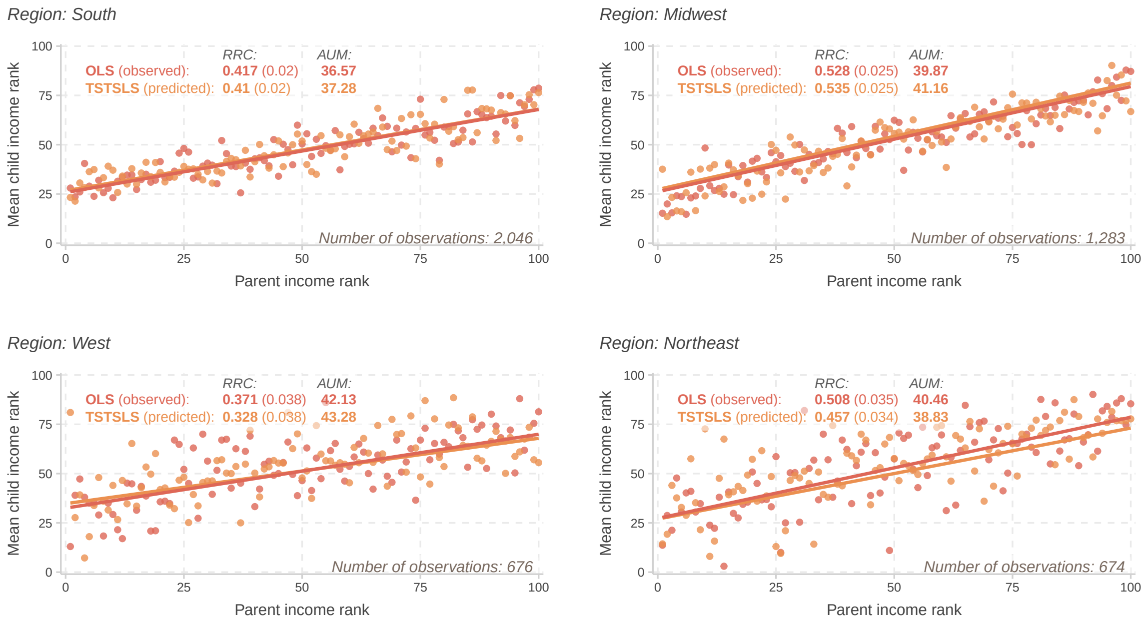


Notes: This figure presents the rank-rank correlation (panel A) and the transition matrix (panels B and C) obtained when parent income is observed (OLS/observed) and when it is predicted using two-sample two-stage least squares (TSTSLS). It is computed on the Panel Study of Income Dynamics (PSID). The sample used is restricted to children born between 1963 and 1988 who are observed at least once as children in a family unit and at least once as a reference person or partner in a family unit over ages 30-50. Child income is the mean of family total income over ages 30-50. Parent income is the sum of father and mother (predicted) mean labor income over ages 30-50. For TSTSLS estimates, parent income is predicted separately for males and females using an OLS model including education (7 cat.; highest years of school completed), 3-digit occupation (334 cat.; most common occupation (incl. inactivity status) between 30 and 50 years old), demographic characteristics in 1990 (birth cohort and race (5 cat.; most recent observation) and state fixed effects (most common state of residence between 30 and 50 years old). In panel A, the fitted line is a linear fit through the microdata. We report coefficients and naive standard errors (in parenthesis) obtained from OLS regressions of child income rank on parent income rank with child cohort fixed effects, on the microdata for the full sample.

Regional results

Appendix Figure A.4 shows the results obtained by Census Region. The RRC obtained by TSTSLS is remarkably similar to that obtained by OLS, with a slight underestimation for the Northeast and West regions. The same applies to the AUM which again is very similar in the TSTSLS setting relative to the OLS case (and the benchmarks). Compared to the benchmark estimates presented in the previous section, differences in RRCs are a bit larger but the rank-ordering of regions is preserved.

Figure A.4: OLS vs. TSTSLS RRC and AUM



Notes: This figure presents Census Region-level estimates of the rank-rank correlation (RRC) and absolute upward mobility (AUM). To compute local estimates, individuals are assigned to their most common Census Region of residence until age 18 (included). See Appendix Figure A.3's notes for details on data, sample and income definitions.

A.2.5 Discussion

Overall, the results presented in this analysis suggest that using TSTSLS for rank-based measures of intergenerational mobility leads to reasonably close estimates relative to OLS estimates, both at the national and subnational levels. Specifically TSTSLS estimates appear to slightly underestimate intergenerational persistence, from 4% to 10% depending on the set of predictors (see Appendix Section A.2.7 for all results when varying the set of first-stage predictors). Moreover, they seem to represent lower bounds for intergenerational persistence (i.e., upper bounds for mobility). In Appendix Table A.5, we show these findings are also robust to dropping income observations equal to 0, as well as to accounting for the number of reference person and partner when defining incomes for children and parents.

A.2.6 Details on samples and variable definitions

Sample construction details

Table A.2: Sample size at each restriction

	# obs.	%
Raw sample	82,573	100
+ born 1963-1988	30,186	36.56
+ observed at least once as child in a family unit	18,612	61.66
+ observed at least once as head/spouse 30-50	5,655	30.38
+ at least one family total income observation 30-50	5,484	96.98
+ at least one observation for parent total income observation 30-50	5,088	92.78

Notes: child and parent income observations exclude those obtained by "major assignment".

Details on variable constructions

Age: since prior to 1983, only age (rather than birth year) was reported, we use the following rule to obtain individuals' birth year: (i) if at least 1 birth year value: most common value; (ii) otherwise: most common value obtained from year - age (by definition this will equal birth year or birth year + 1).

Parent education: maximum grade completed over all observations, and classified following Jerrim et al. (2016) / PSID classification of grades into education levels.[10]
Categories: Grades 1-5, Grades 6-8, Grades 9-11, Grade 12 (HS completion), Some college / associate degree (grades 13-15), College degree (grade 16), Advanced college degree (grade 17).

Parent occupation: most common 3-digit occupation (1970 classification) or detailed inactivity status between 30 and 50 years old.[11] Occupation variables with a consistent classification are available for all individuals between 1981 and 2001, and are only available for a selected sample of PSID heads and wives/"wives"[12] between 1968 and 1980. In order to prevent bias from focusing only on employed parents[13], we use information from employment status variables from 1981 onwards.[14]
Categories: 441 3-digit occupations + 5 detailed inactivity status (Unemployed, Housewife,

[10]Note the grade completed variable is missing for 1969.

[11]In cases where an individual has several most common occupations, we assign the one for which the individual is the oldest on average, and choose one at random if average age is the same.

[12]Criteria: (i) original sample Heads and Wives/"Wives still living by 1992 who reported main jobs in at least three waves during the period 1968-1992, with at least one of those reports prior to 1980; and (ii) additionally, original sample Heads and Wives/"Wives" who had reported at least one main job between 1968 and 1980 but were known to have died by 1992. Those who were still living but had reported only one or two jobs during the period of interest were excluded, as were all nonsample Heads and Wives/"Wives".

[13]By definition, occupations are only available for employed individuals.

[14]Employment status is only available for heads between 1968 and 1978; from 1979 onwards, it is available for heads and wives/"wives". To prevent any bias, employment status is used only after 1980, i.e., when occupation is not restricted to a selected sample.

Student, Retired/Permanently disabled, Other).

Parent race: most recent race observation.
Categories: White, African American, Asian/Pacific Islander, Native American, Other.

Parent region: most common state between 30 and 50 years old.

Child region: most common state between 0 and 18 years old.

Table A.3: Detail of variables used in each year

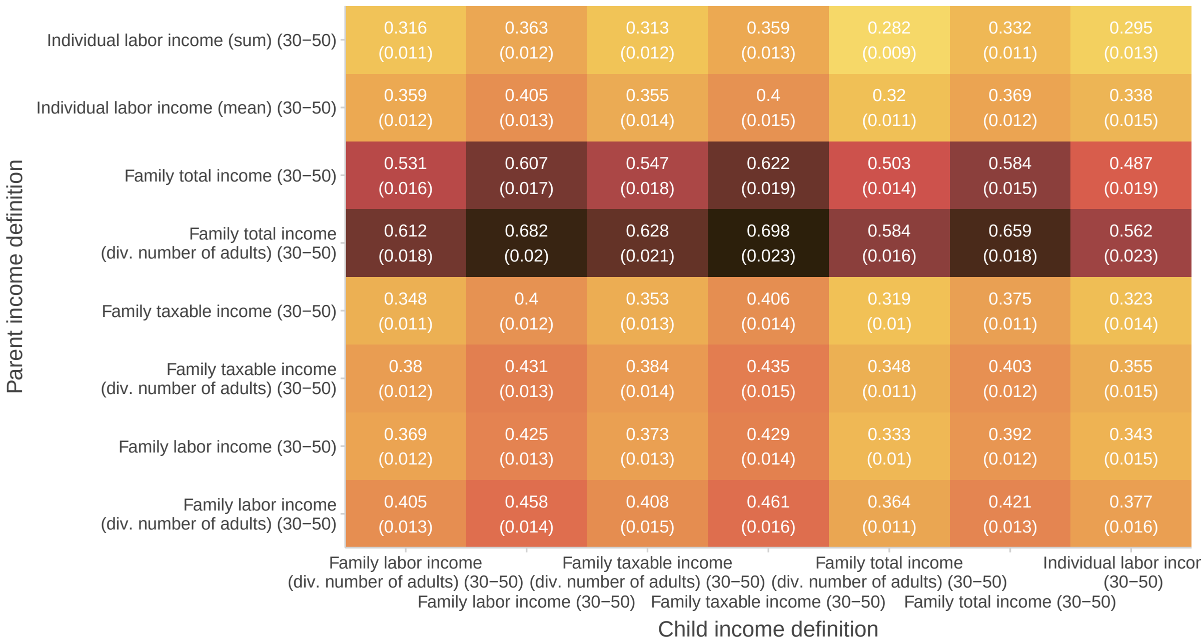
								Occupation			Race			Labor income						
Year	Int. # (ind. file)	Int. # (fam. files)	Sequence #	Rel. to ref. person	Age	Birth year	Education	Ref. person	Partner	Employment status	Ref. person	Partner	State of res.	Ref. person	Accu.	Partner	Accu.	Tax. inc.	Accu. tax. inc.	Total transfers
1968	ER30001	V3	-	ER30003	ER30004	-	ER30010	V197_A	V243_A	-	V181	-	V93	V74	V85	V75	V86	V76	V87	-
1969	ER30020	V442	ER30021	ER30022	ER30023	-	-	V640_A	V609_A	-	V801	-	V537	V514	V515	V516	V517	V518	V519	-
1970	ER30043	V1102	ER30044	ER30045	ER30046	-	ER30052	V1279_A	V1367_A	-	V1490	-	V1103	V1196	V1192	V1198	V1199	V1205	V1206	V1220
1971	ER30067	V1802	ER30068	ER30069	ER30070	-	ER30076	V1984_A	V2074_A	-	V2202	-	V1803	V1897	V1893	V1899	V1900	V1906	V1907	V1922
1972	ER30091	V2402	ER30092	ER30093	ER30094	-	ER30100	V2582_A	V2672_A	-	V2828	-	V2403	V2498	V2494	V2500	V2501	V2507	V2508	V2523
1973	ER30117	V3002	ER30118	ER30119	ER30120	-	ER30126	V3115_A	V3183_A	-	V3300	-	V3003	V3051	V3047	V3053	V3054	V3060	V3061	V3076
1974	ER30138	V3402	ER30139	ER30140	ER30141	-	ER30147	V3968_A	V3601_A	-	V3720	-	V3403	V3463	V3459	V3465	V3466	V3472	V3473	V3488
1975	ER30160	V3802	ER30161	ER30162	ER30163	-	ER30169	V4459_A	V4055_A	-	V4204	-	V3803	V3863	V3859	V3865	V3866	V3872	V3873	V3889
1976	ER30188	V4302	ER30189	ER30190	ER30191	-	ER30197	V5374_A	V4605_A	-	V5096	-	V4303	V5031	V4374	V4379	V4380	V4386	V4387	V4404
1977	ER30217	V5202	ER30218	ER30219	ER30220	-	ER30226	V5873_A	V5507_A	-	V5662	-	V5203	V5627	V5284	V5289	V5290	V5297	V5298	V5316
1978	ER30246	V5702	ER30247	ER30248	ER30249	-	ER30255	V6497_A	V6039_A	-	V6209	-	V5703	V6174	V5783	V5788	V5789	V5796	V5797	V5815
1979	ER30283	V6302	ER30284	ER30285	ER30286	-	ER30296	V7100_A	V6596_A	ER30293	V6802	-	V6303	V6767	V6392	V6398	V6399	V6408	V6407	V6426
1980	ER30313	V6902	ER30314	ER30315	ER30316	-	ER30326	V3529	V7198_A	ER30323	V7447	-	V6903	V7413	V6982	V6988	V6989	V6998	V6997	V7016
1981	ER30343	V7502	ER30344	ER30345	ER30346	-	ER30356	V7712	V7885	ER30353	V8099	-	V7503	V8066	V7574	V7580	V7581	V7590	V7589	V7608
1982	ER30373	V8202	ER30374	ER30375	ER30376	-	ER30384	V8380	V8544	ER30382	V8723	-	V8203	V8690	V8266	V8273	V8274	V8283	V8282	V8301
1983	ER30399	V8802	ER30400	ER30401	-	ER30404	ER30413	V9011	V9194	ER30411	V9408	-	V8803	V9376	V8874	V8881	V8882	V8891	V8890	V8909
1984	ER30429	V10002	ER30430	ER30431	-	ER30434	ER30443	V10460	V10678	ER30441	V11055	-	V10003	V11023	V10257	V10263	V10264	V10277	V10276	V10305
1985	ER30463	V11102	ER30464	ER30465	-	ER30468	ER30478	V11651	V12014	ER30474	V11938	V12293	V11103	V12372	V11398	V11404	V11405	V11419	V11418	V11461
1986	ER30498	V12502	ER30499	ER30500	-	ER30503	ER30513	V13054	V13233	ER30509	V13565	V13500	V12503	V13624	V12797	V12803	V12804	V12818	V12817	V12868
1987	ER30535	V13702	ER30536	ER30537	-	ER30540	ER30549	V14154	V14329	ER30545	V14612	V14547	V13703	V14671	V13899	V13905	V13906	V13920	V13919	V13970
1988	ER30570	V14802	ER30571	ER30572	-	ER30575	ER30584	V15162	V15464	ER30580	V16086	V16021	V14803	V16145	V14914	V14920	V14921	V14935	V14934	V14985
1989	ER30606	V16302	ER30607	ER30608	-	ER30611	ER30620	V16663	V16982	ER30616	V17483	V17418	V16303	V17534	V16414	V16420	V16421	V16435	V16434	V16485
1990	ER30642	V17702	ER30643	ER30644	-	ER30647	ER30657	V18101	V18403	ER30653	V18814	V18749	V17703	V18878	V17830	V17836	V17837	V17851	V17850	V17901
1991	ER30689	V19002	ER30690	ER30691	-	ER30694	ER30703	V19401	V19703	ER30699	V20114	V20049	V19003	V20178	V19130	V19136	V19137	V19151	V19150	V19201
1992	ER30733	V20302	ER30734	ER30735	-	ER30738	ER30748	V20701	V21003	ER30744	V21420	V21355	V20303	V21484	V20430	V20436	V20437	V20451	V20450	V20501
1993	ER30806	V21602	ER30807	ER30808	-	ER30811	ER30820	V22456	V22809	ER30816	V23276	V23212	V21603	V23323	V21740	V23324	-	V21959	-	V22366
1994	ER33101	ER2002	ER33102	ER33103	-	ER33106	ER33115	ER4017	ER4048	ER33111	ER3944	ER3883	ER4156	ER4140	-	ER4144	ER4145	ER4146	-	ER4147
1995	ER33201	ER5002	ER33202	ER33203	-	ER33206	ER33215	ER6857	ER6888	ER33211	ER6814	ER6753	ER6996	ER6980	-	ER6984	ER6985	ER6986	-	ER6987
1996	ER33301	ER7002	ER33302	ER33303	-	ER33306	ER33315	ER9108	ER9139	ER33311	ER9060	ER8999	ER9247	ER9231	-	ER9235	ER9236	ER9237	-	ER9238
1997	ER33401	ER10002	ER33402	ER33403	-	ER33406	ER33415	ER12085	ER12116	ER33411	ER11848	ER11760	ER12221	ER12080	ER12081	ER12082	-	ER12069	ER12070	ER12071
1999	ER33501	ER13002	ER33502	ER33503	-	ER33506	ER33516	ER13215	ER13727	ER33512	ER15928	ER15836	ER13004	ER16463	ER16464	ER16465	-	ER16452	ER16453	ER16454
2001	ER33601	ER17002	ER33602	ER33603	-	ER33606	ER33616	ER17226	ER17796	ER33612	ER19989	ER19897	ER17004	ER20443	ER18562	ER20447	ER20448	ER20449	-	ER20450
2003	ER33701	ER21002	ER33702	ER33703	-	ER33706	ER33716	-	-	ER33712	ER23426	ER23334	ER21003	ER24116	ER21930	ER24135	ER24136	ER24100	-	ER24101
2005	ER33801	ER25002	ER33802	ER33803	-	ER33806	ER33817	-	-	ER33813	ER27393	ER27297	ER25003	ER27931	ER25911	ER27943	ER27944	ER27953	-	ER28002
2007	ER33901	ER36002	ER33902	ER33903	-	ER33906	ER33917	-	-	ER33913	ER40565	ER40472	ER36003	ER40921	ER36929	ER40933	ER40934	ER40943	-	ER40992
2009	ER34001	ER42002	ER34002	ER34003	-	ER34006	ER34020	-	-	ER34016	ER46543	ER46449	ER42003	ER46829	ER42920	ER46841	ER46842	ER46851	-	ER46900
2011	ER34101	ER47302	ER34102	ER34103	-	ER34106	ER34119	-	-	ER34116	ER51904	ER51810	ER47303	ER52237	ER48242	ER52249	ER52250	ER52259	-	ER52308
2013	ER34201	ER53002	ER34202	ER34203	-	ER34206	ER34230	-	-	ER34216	ER57659	ER57549	ER53003	ER58038	ER53936	ER58050	ER58051	ER58060	-	ER58117
2015	ER34301	ER60002	ER34302	ER34303	-	ER34307	ER34349	-	-	ER34317	ER64810	ER64671	ER60003	ER65216	ER60995	ER65244	-	ER65253	-	ER65314
2017	ER34501	ER66002	ER34502	ER34503	-	ER34506	ER34548	-	-	ER34516	ER70882	ER70744	ER66003	ER71293	ER67047	ER71321	-	ER71330	-	ER71391
2019	ER34701	ER72002	ER34702	ER34703	-	ER34706	ER34752	-	-	ER34716	ER76897	ER76752	ER72003	ER77315	ER73070	ER77343	-	ER77352	-	ER77413

Notes: Int. = interview. Ind. = individual. Fam. = family. Rel. = relation. Ref. = reference. Res. = residence. Accu. = accuracy. Tax. inc. = taxable income.

A.2.7 Additional results

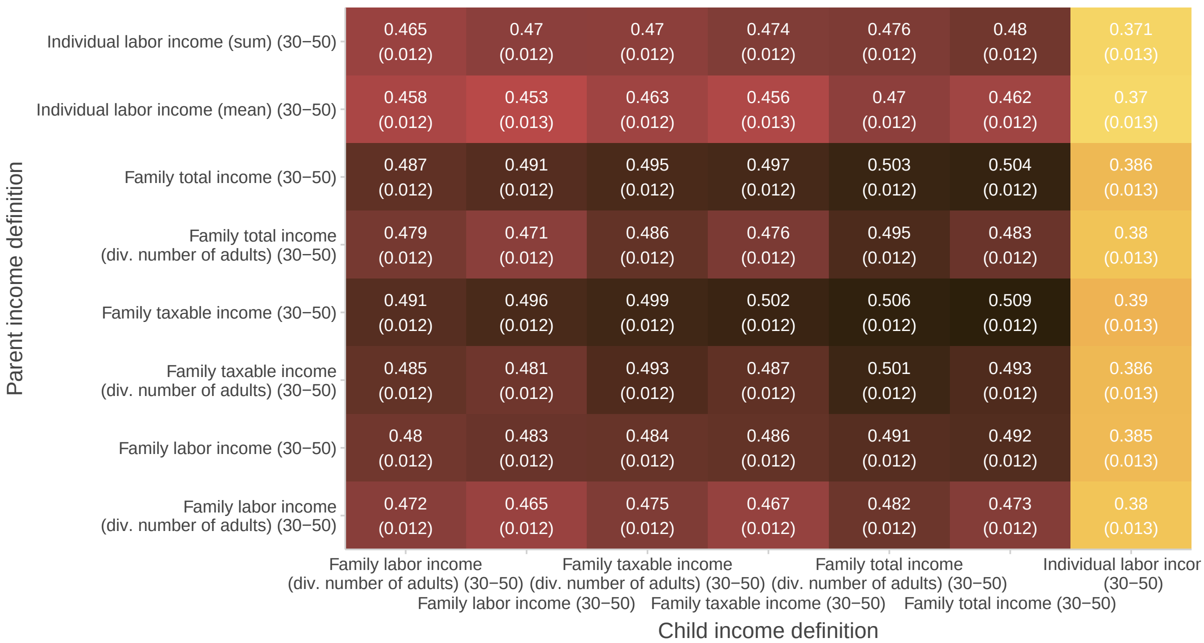
All benchmark estimates

Figure A.5: Benchmark IGEs for all income definitions



Naive standard errors in parenthesis. Number of children varies by income defintion since the number of negative or zero incomes varies.

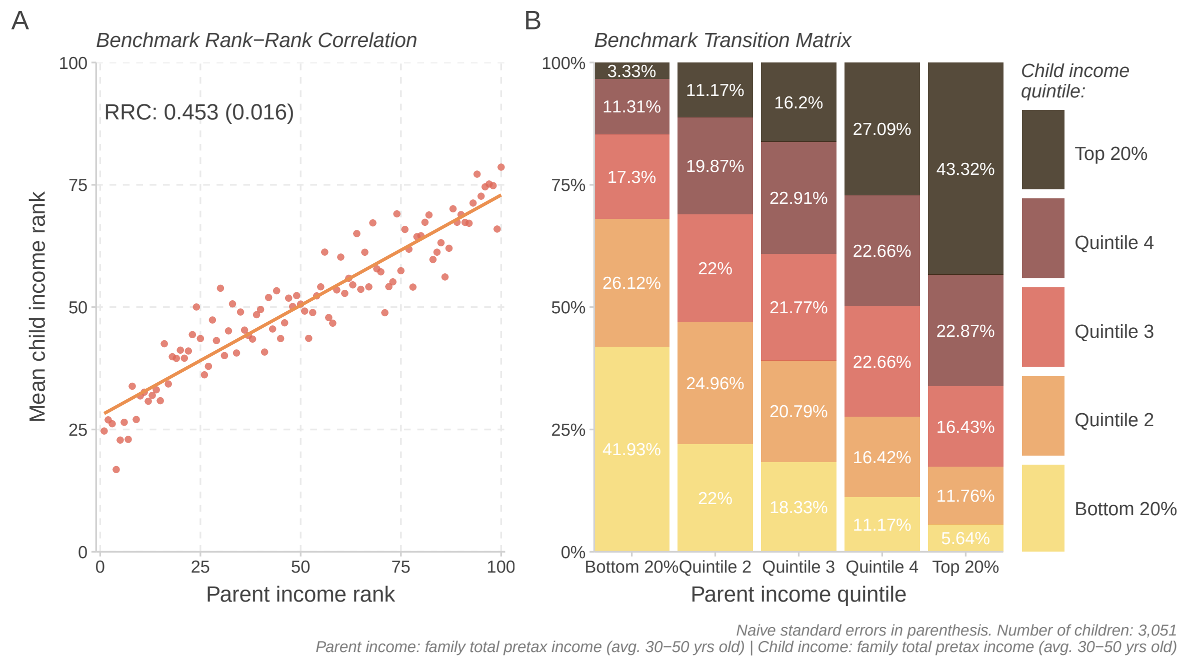
Figure A.6: Benchmark RRCs for all income definitions



Naive standard errors in parenthesis. Number of children: 5,088.

Only SRC sample

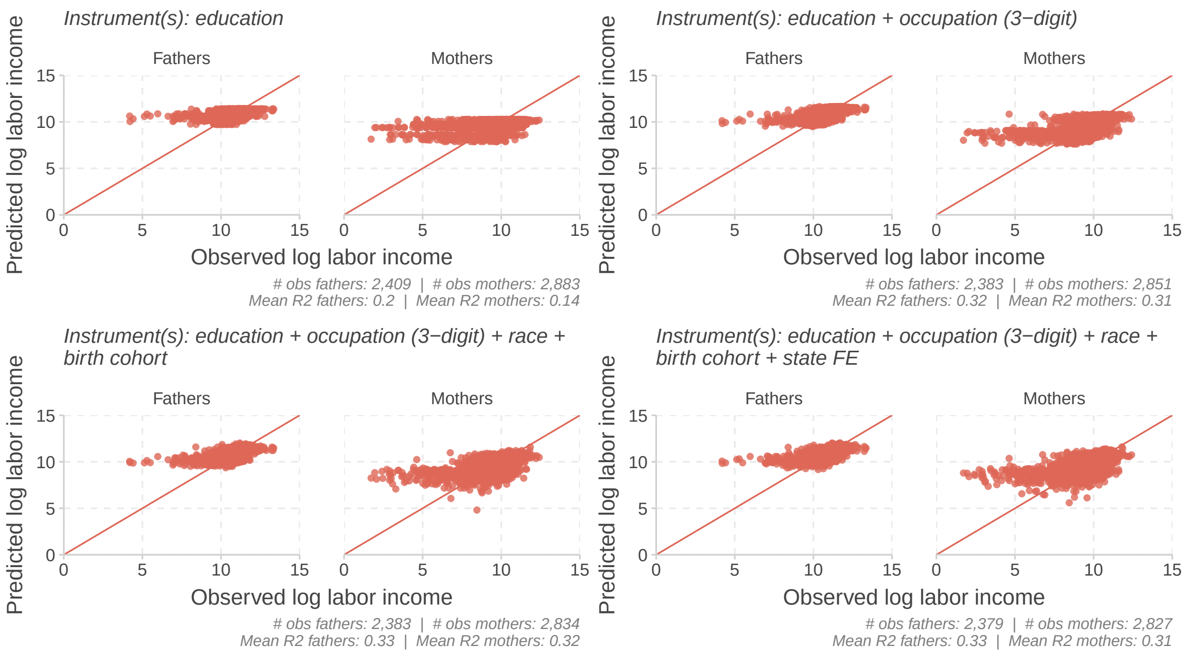
Figure A.7: Benchmark rank-rank correlation and transition matrix



Notes: This figure presents the rank-rank correlation (panel A) and the transition matrix (panel B), computed on the Panel Study of Income Dynamics (PSID)'s representative Survey Research Center sample. See Appendix Figure A.1's notes for details on data, sample and income definitions.

Baseline predictions

Figure A.8: Observed vs. (out-of-sample) predicted *individual* labor income

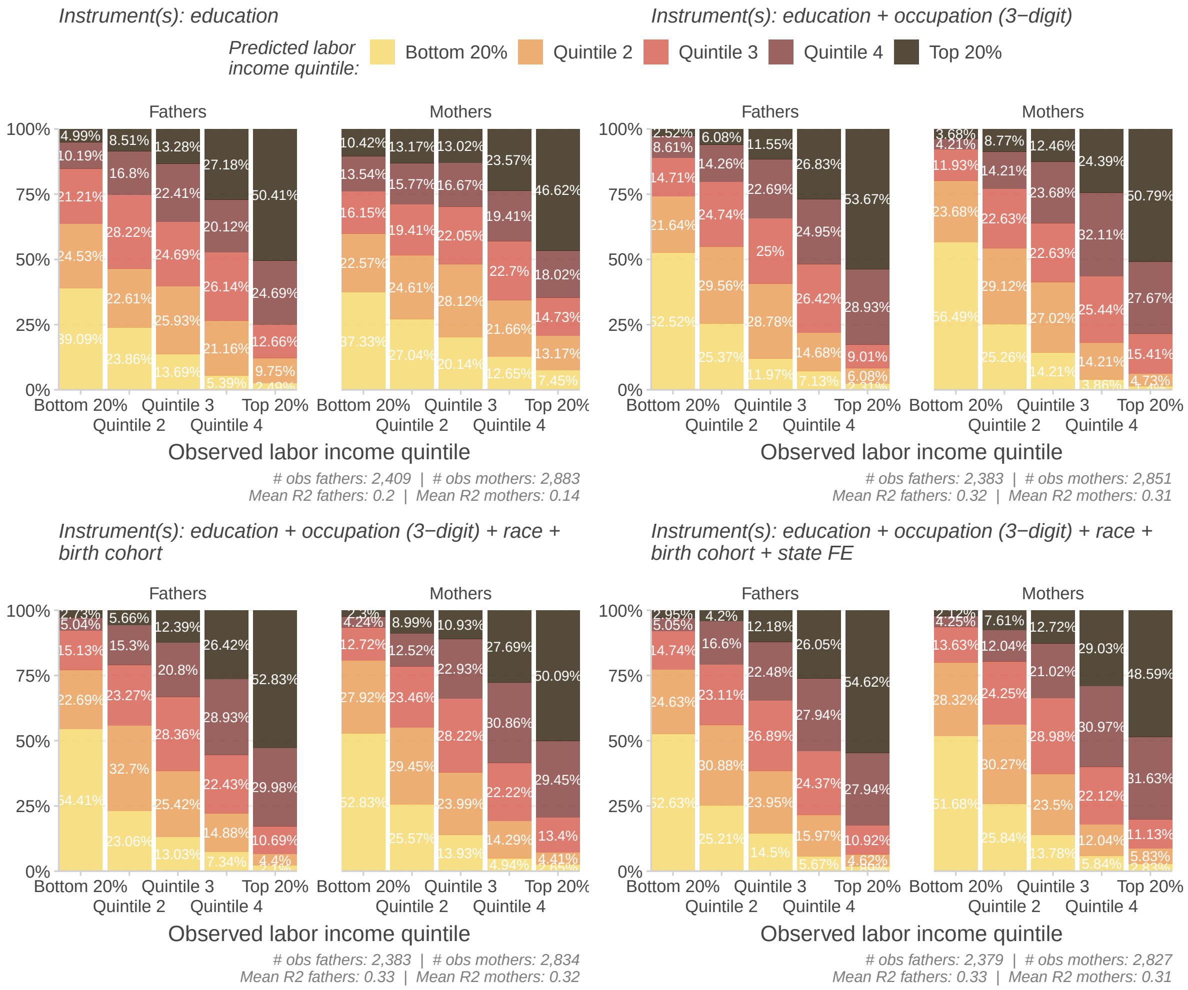


Notes: This figure presents observed *individual* log labor income and out-of-sample predicted *individual* log labor income for fathers and mothers depending on variables used in the first-stage prediction. The red line corresponds to the 45 degree line. See Appendix Figure A.3's notes for details on data, sample and income definitions.

Figure A.9: Observed vs. (out-of-sample) predicted *individual* labor income rank

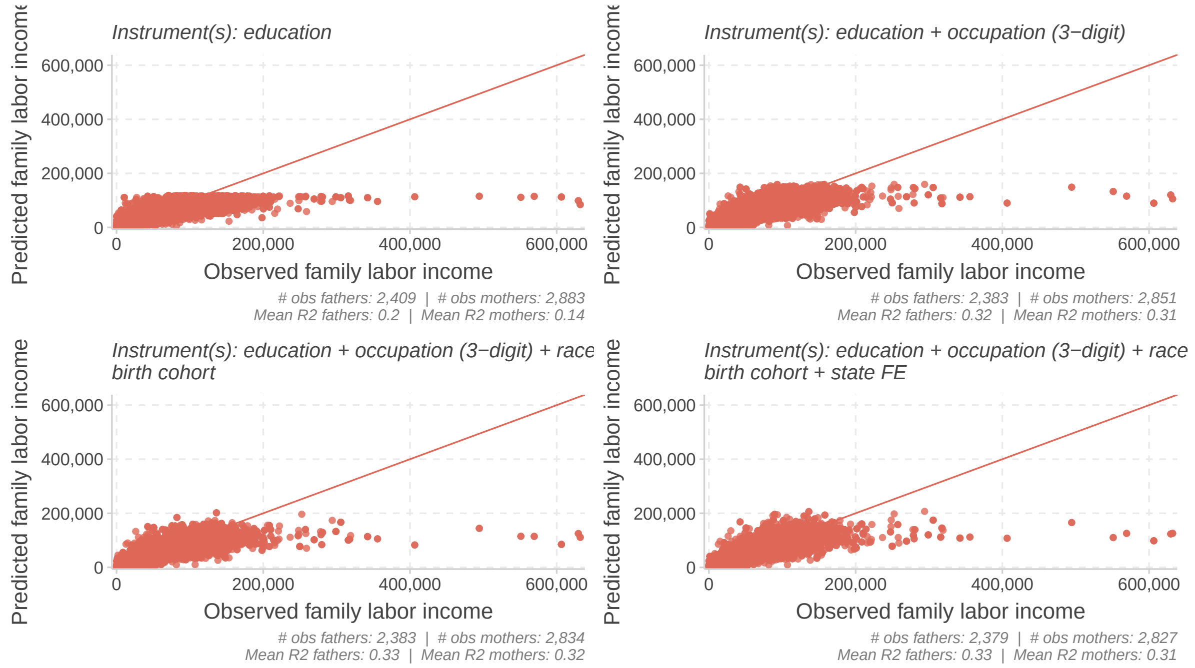
Notes: This figure presents the conditional expectation of out-of-sample predicted *individual* labor income rank, as a function of observed *individual* labor income rank, for fathers and mothers, depending on variables used in the first-stage prediction. The red line corresponds to the 45 degree line, while the red shaded area corresponds to the interquartile range. See Appendix Figure A.3's notes for details on data, sample and income definitions.

Figure A.10: Observed vs. (out-of-sample) predicted *individual* labor income quintile



Notes: This figure presents the quintile-by-quintile out-of-sample predicted *individual* labor income quintile by observed *individual* labor income quintile, for fathers and mothers, depending on variables used in the first-stage prediction. Each cell documents the share of out-of-sample labor income predictions belonging to the quintile indicated by the color legend among observed labor incomes falling in the quintile indicated on the x-axis. They are computed separately for father and mother, and depending on variables used in the first-stage prediction. See Appendix Figure A.3's notes for details on data, sample and income definitions.

Figure A.11: Observed vs. (out-of-sample) predicted *family* labor income

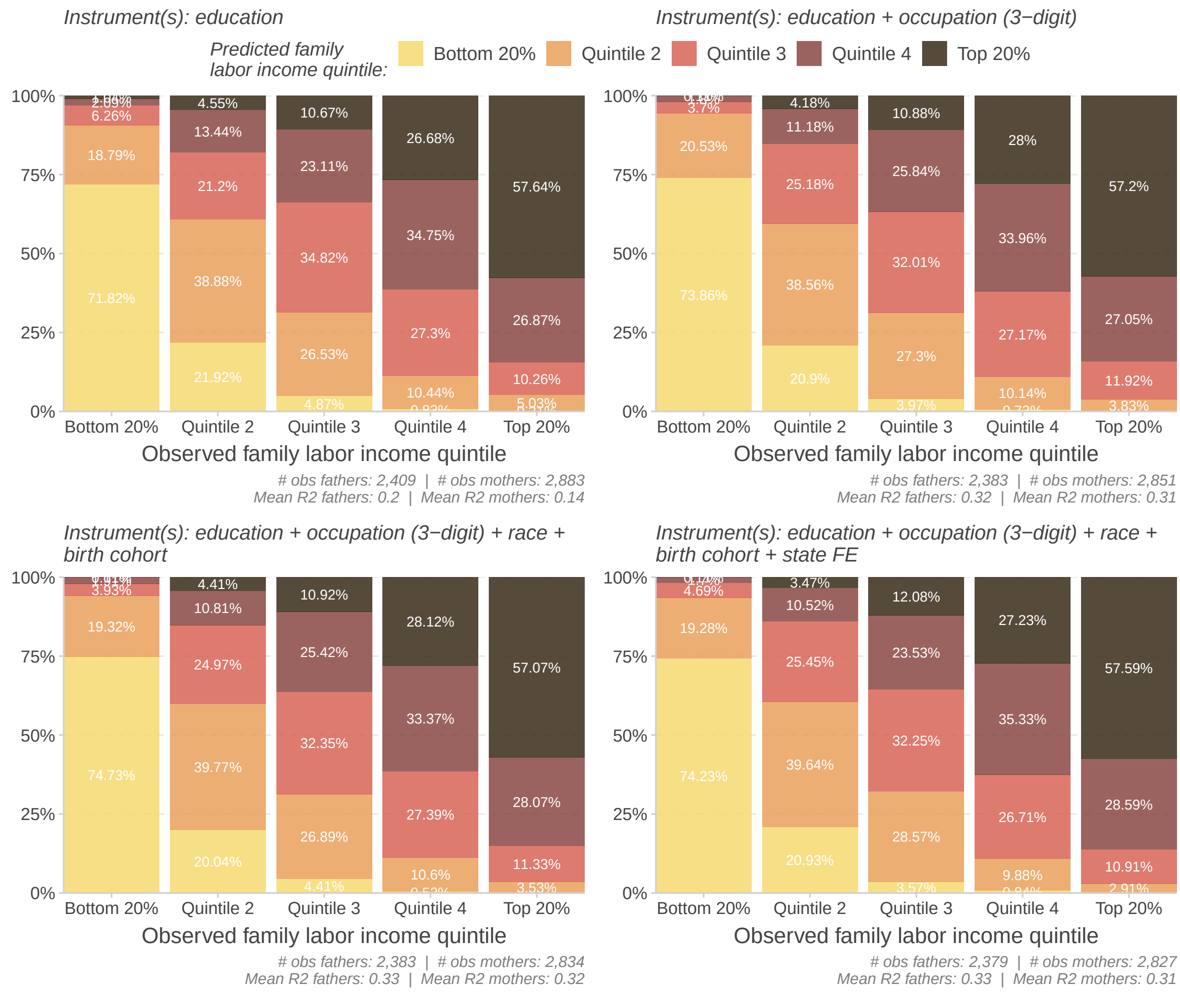


Notes: This figure presents observed *family* log labor income and out-of-sample predicted *family* log labor income for fathers and mothers depending on variables used in the first-stage prediction. The red line corresponds to the 45 degree line. See Appendix Figure A.3's notes for details on data, sample and income definitions.

Figure A.12: Observed vs. (out-of-sample) predicted *family* labor income rank

Notes: This figure presents the conditional expectation of out-of-sample predicted *family* labor income rank, as a function of observed *family* labor income rank, for fathers and mothers, depending on variables used in the first-stage prediction. The red line corresponds to the 45 degree line, while the red shaded area corresponds to the interquartile range. See Appendix Figure A.3's notes for details on data, sample and income definitions.

Figure A.13: Observed vs. (out-of-sample) predicted *family* labor income rank



Notes: This figure presents the quintile-by-quintile out-of-sample predicted *family* labor income quintile by observed *family* labor income quintile, for fathers and mothers, depending on variables used in the first-stage prediction. Each cell documents the share of out-of-sample labor income predictions belonging to the quintile indicated by the color legend among observed labor incomes falling in the quintile indicated on the x-axis. They are computed separately for father and mother, and depending on variables used in the first-stage prediction. See Appendix Figure A.3's notes for details on data, sample and income definitions.

Alternative first-stage predictors

Table A.4: Comparison for different sets of predictors

	Education (1)	+ occupation (3-digit) (2)	+ race + birth cohort (3)	+ state FE (4)
Panel A. Intergenerational Elasticity (IGE)				
Observed parent income (OLS)	0.335	0.334	0.334	0.334
	(0.011)	(0.011)	(0.011)	(0.011)
Predicted parent income (TSTSLS)	0.464	0.431	0.449	0.445
	(0.017)	(0.014)	(0.014)	(0.014)
Percentage diff. TSTSLS vs OLS	-27.76%	-22.57%	-25.73%	-24.82%
Number of observations	4,805	4,755	4,737	4,730
Panel B. Rank-Rank Correlation (RRC)				
Observed parent income (OLS)	0.476	0.475	0.476	0.476
	(0.013)	(0.013)	(0.013)	(0.013)
Predicted parent income (TSTSLS)	0.43	0.453	0.461	0.459
	(0.013)	(0.013)	(0.013)	(0.013)
Percentage diff. TSTSLS vs OLS	10.53%	4.9%	3.22%	3.85%
Number of observations	4,832	4,780	4,762	4,755
Panel C. Transition Matrix				
P(Bottom 20% | Bottom 20%) (OLS)	43.95%	43.7%	43.84%	43.88%
P(Bottom 20% | Bottom 20%) (TSTSLS)	37.83%	39.77%	40.66%	40.09%
P(Bottom 20% | Top 20%) (OLS)	4.51%	4.46%	4.26%	4.16%
P(Bottom 20% | Top 20%) (TSTSLS)	4.81%	5.18%	5.39%	5.61%
P(Top 20% | Bottom 20%) (OLS)	3.97%	3.92%	3.93%	3.94%
P(Top 20% | Bottom 20%) (TSTSLS)	5.22%	5.83%	5.63%	5.97%
P(Top 20% | Top 20%) (OLS)	45.54%	45.49%	45.53%	45.63%
P(Top 20% | Top 20%) (TSTSLS)	44.22%	43.32%	43.36%	44.49%

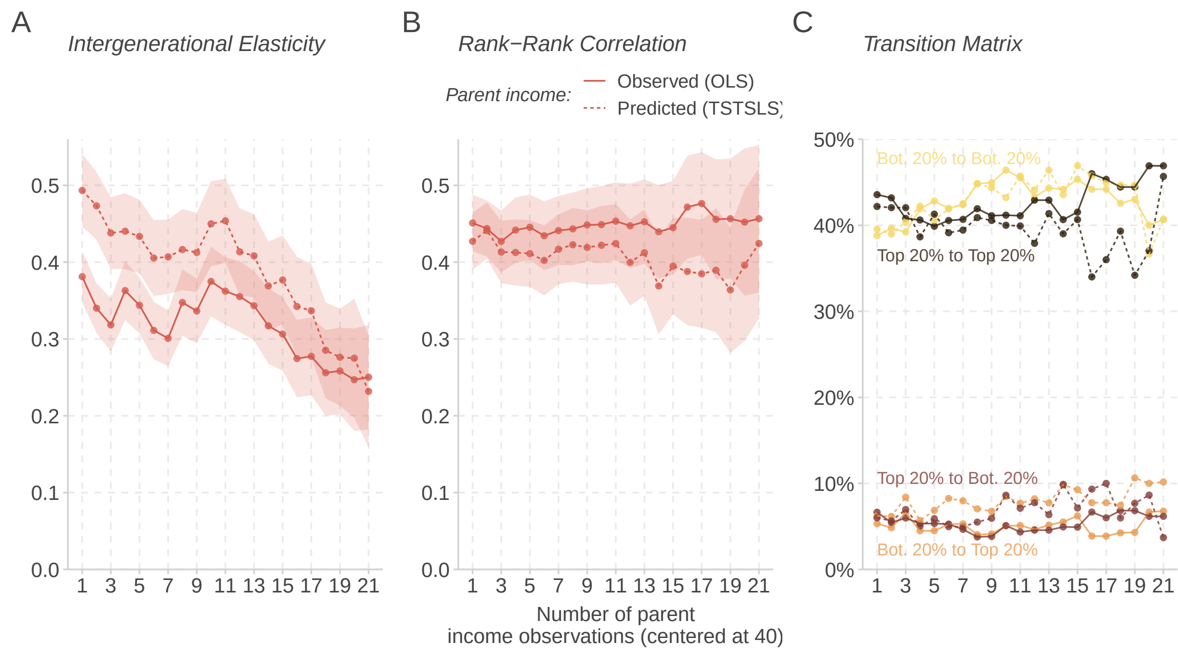
Notes:

Alternative samples and definitions

In Table A.5 we check the robustness of our baseline results to changes in estimation samples and income definitions. Specifically, we report results for the following changes: (i) using only the nationally-representative Survey Research Center (SRC) sample (col. 2), (ii) restricting the age range over which child and parent incomes are averaged to 35-45 years old in our main analysis, (iii) dropping parent and child income observations equal to zero when computing average incomes[15], (iv) accounting for household size when defining parent and child incomes (see discussion in A.2.2). Our baseline results are reported in column 1.

Attenuation bias

Figure A.14: OLS vs. TSTSLS estimates - Varying number of parent income observations



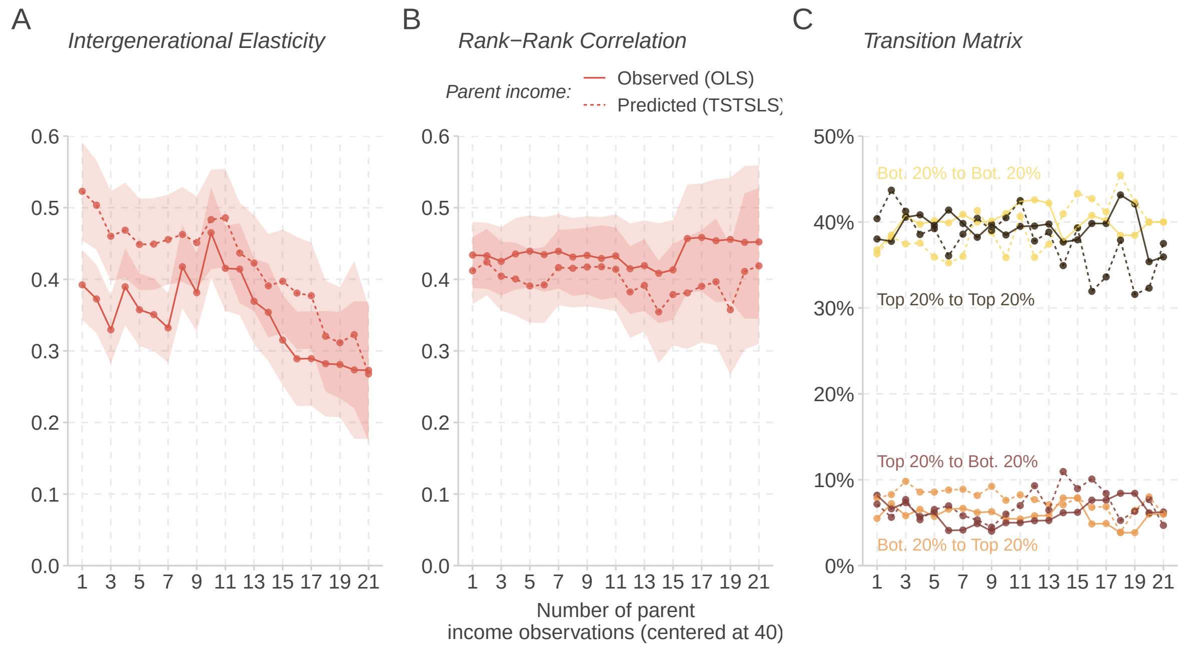
Notes: This figure presents the IGE, RRC and transition matrix cells obtained when parent income is observed (OLS) and when it is predicted using two-sample two-stage least squares (TSTSLS), for different number of parent income observations. To control for the potential effect of lifecycle bias, we center parent incomes around age 40. Thus one observation means parent income is equal to income at age 40, two observations means parent income is equal to income averaged over age 39 and age 41, three observations means parent income is equal to income averaged over age 39 to age 41, etc. See Appendix Figure A.3's notes for details on data, sample and income definitions.

[15] According to Mazumder (2016, p.101): "In the PSID, the household head is recorded as having zero labor income if their income was actually zero or if their labor income is missing, so one cannot cleanly distinguish true zeroes with labor income."

Table A.5: Robustness of baseline results

	Baseline Estimates	Only SRC Sample	35-45 Income Age Range	Dropping Zero Inc. Obs.	Accounting Household Size
	(1)	(2)	(3)	(4)	(5)
Panel A. National - Intergenerational Elasticity (IGE)					
Observed parent income (OLS)	0.334	0.369	0.363	0.414	0.324
	(0.011)	(0.018)	(0.016)	(0.013)	(0.011)
Predicted parent income (TSTSLS)	0.445	0.418	0.475	0.53	0.485
	(0.014)	(0.022)	(0.021)	(0.017)	(0.016)
Percentage diff. TSTSLS vs OLS	-24.82%	-11.72%	-23.53%	-21.9%	-33.21%
Number of observations	4,730	2,892	2,882	4,732	4,730
Panel B. National - Rank-Rank Correlation (RRC)					
Observed parent income (OLS)	0.476	0.409	0.464	0.47	0.466
	(0.013)	(0.017)	(0.017)	(0.013)	(0.013)
Predicted parent income (TSTSLS)	0.459	0.364	0.448	0.463	0.435
	(0.013)	(0.017)	(0.017)	(0.013)	(0.013)
Percentage diff. TSTSLS vs OLS	3.85%	12.38%	3.56%	1.7%	7.16%
Number of observations	4,755	2,903	2,903	4,732	4,755
Panel C. Region: Midwest					
RRC - OLS	0.528	0.417	0.506	0.523	0.509
	(0.025)	(0.03)	(0.031)	(0.025)	(0.025)
AUM - OLS	39.87	43.94	38.49	39.65	37.19
RRC - TSTSLS	0.535	0.37	0.506	0.521	0.497
	(0.025)	(0.032)	(0.032)	(0.025)	(0.026)
AUM - TSTSLS	41.16	46.85	40.29	41.43	39.01
RRC percentage diff. TSTSLS vs OLS	-1.22%	12.49%	0.02%	0.32%	2.52%
AUM percentage diff. TSTSLS vs OLS	-3.15%	-6.22%	-4.47%	-4.31%	-4.67%
Number of observations	1,283	980	834	1,277	1,283
Panel D. Region: Northeast					
RRC - OLS	0.508	0.429	0.457	0.503	0.52
	(0.035)	(0.04)	(0.048)	(0.035)	(0.036)
AUM - OLS	40.46	41.91	46.3	40.23	44.23
RRC - TSTSLS	0.457	0.35	0.377	0.459	0.46
	(0.034)	(0.039)	(0.045)	(0.033)	(0.035)
AUM - TSTSLS	38.83	41.49	46.9	37.85	42.34
RRC percentage diff. TSTSLS vs OLS	11.25%	22.4%	21.26%	9.6%	13.13%
AUM percentage diff. TSTSLS vs OLS	4.19%	1%	-1.28%	6.3%	4.44%
Number of observations	674	538	406	669	674
Panel E. Region: South					
RRC - OLS	0.417	0.398	0.423	0.414	0.413
	(0.02)	(0.03)	(0.025)	(0.02)	(0.02)
AUM - OLS	36.57	36.19	35.4	37.17	36.71
RRC - TSTSLS	0.41	0.401	0.421	0.42	0.386
	(0.02)	(0.03)	(0.026)	(0.021)	(0.02)
AUM - TSTSLS	37.28	35.01	35.34	37.47	38.12
RRC percentage diff. TSTSLS vs OLS	1.72%	-0.75%	0.47%	-1.5%	7.03%
AUM percentage diff. TSTSLS vs OLS	-1.92%	3.37%	0.15%	-0.8%	-3.71%
Number of observations	2,046	885	1,242	2,036	2,046
Panel F. Region: West					
RRC - OLS	0.371	0.299	0.321	0.357	0.353
	(0.038)	(0.047)	(0.053)	(0.037)	(0.037)
AUM - OLS	42.13	40.03	45.57	42.11	42.68
RRC - TSTSLS	0.328	0.232	0.303	0.344	0.303
	(0.038)	(0.046)	(0.052)	(0.038)	(0.038)
AUM - TSTSLS	43.28	43.43	46.18	42.98	43.66
RRC percentage diff. TSTSLS vs OLS	12.97%	28.89%	5.93%	3.8%	16.65%
AUM percentage diff. TSTSLS vs OLS	-2.66%	-7.84%	-1.31%	-2.02%	-2.23%
Number of observations	676	488	376	674	676

Figure A.15: OLS vs. TSTSLS estimates - Varying number of parent income observations - Child income mean 37-43



Notes: This figure presents the IGE, RRC and transition matrix cells obtained when parent income is observed (OLS) and when it is predicted using two-sample two-stage least squares (TSTSLS), for different number of parent income observations and when child income is defined over ages 37-43. To control for the potential effect of lifecycle bias, we center parent incomes around age 40. Thus one observation means parent income is equal to income at age 40, two observations means parent income is equal to income averaged over age 39 and age 41, three observations means parent income is equal to income averaged over age 39 to age 41, etc. See Appendix Figure A.3's notes for details on data, sample and income definitions.

Sampling weights

As is well-known, the PSID is not a nationally representative sample. In particular, the Survey of Economic Opportunity (SEO) component of the PSID oversamples low-income households but suffers from various sampling issues (see footnote 4 in Lee and Solon (2009)). In our baseline results, we opted to use all of the PSID because (i) our goal was to compare OLS to TSTSLS estimates rather than obtain the best OLS estimate, and (ii) the additional sample size allows us to compare OLS and TSTSLS estimates at the regional level. However, one may wish to know how our exercise performs for a nationally-representative sample. Table A.6 compares our baseline results with estimates obtained from four different specifications: (i) using only the PSID's nationally representative Survey Research Center (SRC) sample, (ii) using all of the PSID with three different kinds of weights, all measured in the child's last income observation year: (i) the family longitudinal weights, (ii) the individual longitudinal weights, and (iii) the individual cross-sectional weights (only available from 1997 onwards).

Overall, our baseline estimates have the smallest differences between TSTSLS and OLS. The OLS RRC is roughly 4% larger than the TSTSLS RRC in baseline, while it 12% when using only the SRC sample, 11% when using the family longitudinal weights, 9% when using individual longitudinal weights, and 12% when using the individual cross-sectional weights. Regarding the regional estimates, as with the baseline results, the relative difference between TSTSLS and OLS estimates largely reflect sample size: estimates for the Midwest and the South are quite close across specifications (a bit less so for the Midwest when using the SRC sample), while the differences become more pronounced for the Northeast and the West, for which the sample size is more limited. It should be noted that across regions and specifications, the TSTSLS estimates of the AUM are surprisingly close to their OLS counterparts.

A.3 Additional robustness

This Appendix provides additional robustness checks to those presented in the body of the paper.

A.3.1 Sensitivity to data coverage

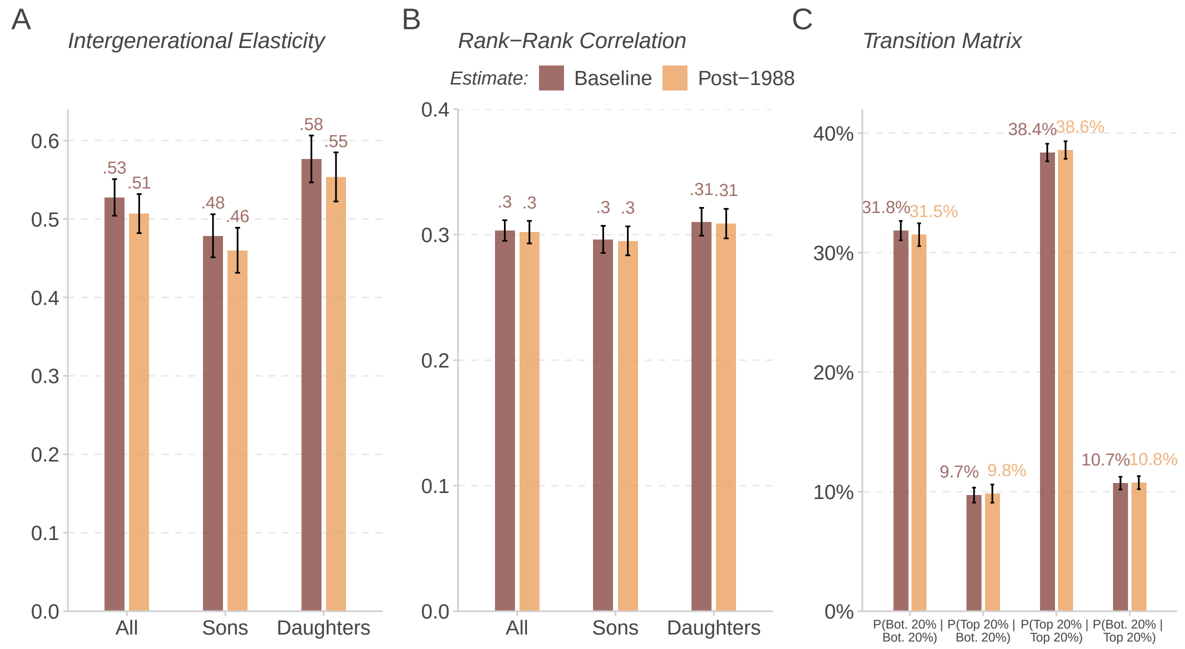
Civil servants

We ensure our results are not affected by the fact that civil servants are only observed from 1988 onwards by estimating the first-stage regression computing synthetic parents' on post-1988 wages only, still restricting to when they are between 35 ad 45 years old. Appendix Figure A.16 displays the results from this check. The results are largely unaffected.

Table A.6: Comparison between baseline results and weighted results

	Baseline Estimates	Only SRC Sample	*Weights in Last Child Income Observation Year* Family Longitudinal	Individual Longitudinal	Individual Cross-Sectional
	(1)	(2)	(3)	(4)	(5)
Panel A. National - Intergenerational Elasticity (IGE)					
Observed parent income (OLS)	0.334	0.369	0.372	0.377	0.369
	(0.011)	(0.018)	(0.012)	(0.012)	(0.013)
Predicted parent income (TSTSLS)	0.445	0.418	0.432	0.428	0.411
	(0.014)	(0.022)	(0.014)	(0.014)	(0.015)
Percentage diff. TSTSLS vs OLS	-24.82%	-11.72%	-13.89%	-11.91%	-10.23%
Number of observations	4,730	2,892	4,730	4,730	4,588
Panel B. National - Rank-Rank Correlation (RRC)					
Observed parent income (OLS)	0.476	0.409	0.458	0.456	0.437
	(0.013)	(0.017)	(0.013)	(0.013)	(0.014)
Predicted parent income (TSTSLS)	0.459	0.364	0.413	0.418	0.39
	(0.013)	(0.017)	(0.013)	(0.013)	(0.014)
Percentage diff. TSTSLS vs OLS	3.85%	12.38%	10.91%	9.09%	12.07%
Number of observations	4,755	2,903	4,755	4,755	4,612
Panel C. Region: Midwest					
RRC - OLS	0.528	0.417	0.45	0.456	0.447
	(0.025)	(0.03)	(0.026)	(0.026)	(0.027)
AUM - OLS	39.87	43.94	48.2	48.81	51.49
RRC - TSTSLS	0.535	0.37	0.427	0.433	0.444
	(0.025)	(0.032)	(0.027)	(0.027)	(0.028)
AUM - TSTSLS	41.16	46.85	50.13	50.4	53.01
RRC percentage diff. TSTSLS vs OLS	-1.22%	12.49%	5.48%	5.25%	0.66%
AUM percentage diff. TSTSLS vs OLS	-3.15%	-6.22%	-3.85%	-3.15%	-2.87%
Number of observations	1,283	980	1,283	1,283	1,265
Panel D. Region: Northeast					
RRC - OLS	0.508	0.429	0.497	0.494	0.487
	(0.035)	(0.04)	(0.036)	(0.036)	(0.037)
AUM - OLS	40.46	41.91	37.99	42.71	39.76
RRC - TSTSLS	0.457	0.35	0.443	0.441	0.42
	(0.034)	(0.039)	(0.035)	(0.035)	(0.037)
AUM - TSTSLS	38.83	41.49	37.09	41.41	40.1
RRC percentage diff. TSTSLS vs OLS	11.25%	22.4%	12.24%	11.99%	15.77%
AUM percentage diff. TSTSLS vs OLS	4.19%	1%	2.42%	3.14%	-0.84%
Number of observations	674	538	674	674	650
Panel E. Region: South					
RRC - OLS	0.417	0.398	0.447	0.428	0.421
	(0.02)	(0.03)	(0.019)	(0.019)	(0.02)
AUM - OLS	36.57	36.19	36.69	43.12	40.85
RRC - TSTSLS	0.41	0.401	0.437	0.423	0.406
	(0.02)	(0.03)	(0.019)	(0.019)	(0.02)
AUM - TSTSLS	37.28	35.01	37.31	43.2	41.35
RRC percentage diff. TSTSLS vs OLS	1.72%	-0.75%	2.29%	1.25%	3.79%
AUM percentage diff. TSTSLS vs OLS	-1.92%	3.37%	-1.64%	-0.2%	-1.19%
Number of observations	2,046	885	2,046	2,046	1,970
Panel F. Region: West					
RRC - OLS	0.371	0.299	0.363	0.385	0.336
	(0.038)	(0.047)	(0.038)	(0.039)	(0.04)
AUM - OLS	42.13	40.03	40.98	43.83	47.9
RRC - TSTSLS	0.328	0.232	0.264	0.301	0.231
	(0.038)	(0.046)	(0.036)	(0.038)	(0.038)
AUM - TSTSLS	43.28	43.43	44.76	46.89	52.58
RRC percentage diff. TSTSLS vs OLS	12.97%	28.89%	37.34%	27.62%	45.79%
AUM percentage diff. TSTSLS vs OLS	-2.66%	-7.84%	-8.43%	-6.53%	-8.9%
Number of observations	676	488	676	676	651

Figure A.16: Robustness of baseline estimates to computing synthetic parent incomes only on post-1988 data



Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates to computing synthetic parents' incomes only on post-1988 data. The All Employee Panel from which synthetic parents' wages are observed did not cover civil servants prior to 1988 (see Appendix Section A.1 for details). The graph presents the baseline estimates (Baseline) to those obtained when synthetic parent incomes are defined as average wage between 35-45 using only post-1988 wages (Post-1988). Vertical lines represent the 95% bootstrapped confidence intervals. All results pertain to parent and child incomes being defined at the household level. The results for the transition matrix correspond to the sample pooling sons and daughters. See Section 3.3 for details on data, sample and income definitions.

Comparison with population statistics

Since the sample selection of the EDP is (virtually) random (individuals born on the first four days of October), we can have a good idea of how our baseline sample compares with the French population by comparing its average characteristics to those of the completely unrestricted EDP sample for the same birth cohorts (1972-1981).

To obtain characteristics on parents (other than from the 1990 census), we rely on individuals' birth-certificates information from the EDP civil registry data. We compare the birth-certificate information (e.g., gender, parents' age at birth, single parenthood, parents' occupation at birth) for all EDP individuals born in 1972-1981 in metropolitan France and for our sample of children. Note that the resulting statistics are subject to the imperfections of birth-certificate data, notably regarding non-random missing information for fathers. Table A.7 displays the statistics for both samples. Overall, our sample of children is very similar to the unrestricted EDP sample, except for a higher probability of being in the fiscal data (91% vs. 100%, by construction) and a lower likelihood of having a father who is a farmer. The household income distributions are very similar.

Table A.7: Average characteristics of overall population vs. sample

Characteristic	Population	Sample	Diff.
Females	49.14%	49.77%	0.63
Parent demographics			
Mother age at birth	26	25.89	-0.11
Father age at birth	28.91	28.65	-0.26
Mother born French	90.07%	91.92%	1.85
Father born French	88.12%	90.15%	2.03
Single mothers	4.98%	4.42%	-0.56
Missing parents info.	2.24%	1.75%	-0.49
Father 1-digit occupation at child birth			
Missing father info.	9.4%	8.25%	-1.15
1. Farmers	3.41%	0.64%	-2.77
2. Craftsmen, salespeople, and heads of businesses	3.95%	3.96%	0.01
3. Managerial and professional occupations	7.14%	5.98%	-1.16
4. Intermediate professions	13.58%	14.63%	1.05
5. Employees	14.58%	16%	1.42
6. Blue collar workers	46.46%	49.4%	2.94
7. Retirees	0.03%	0.02%	-0.01
8. Other with no professional activity	1.45%	1.11%	-0.34
Mother 1-digit occupation at child birth			
Missing mother info.	5.34%	4.69%	-0.65
1. Farmers	0.83%	0.11%	-0.72
2. Craftsmen, salespeople, and heads of businesses	0.91%	0.85%	-0.06
3. Managerial and professional occupations	2.08%	1.62%	-0.46
4. Intermediate professions	8.92%	9.19%	0.27
5. Employees	26.19%	28.33%	2.14
6. Blue collar workers	11.2%	12%	0.8
7. Retirees	0.02%	0.02%	0
8. Other with no professional activity	44.51%	43.2%	-1.31
All Employee Panel (AEP) information in adulthood, 1968-2015, age 35-45			
Observed in AEP	72.83%	78.67%	5.84
Mean number of obs. in AEP	2.9	3.15	0.25
Q1 individual wage (AEP)	12,671	13,179	508
Mean individual wage (AEP)	21,538	21,666	128
Med. individual wage (AEP)	19,528	19,726	198
Q3 individual wage (AEP)	26,623	26,723	100
Tax information in adulthood, 2010-2016, age 35-45			
Observed in tax data	90.92%	100%	9.08
Mean number of obs. in tax data	4.23	4.65	0.42
Q1 household income (tax)	27,339	27,696	357
Mean household income (tax)	46,858	46,598	-260
Med. household income (tax)	41,220	41,418	198
Q3 household income (tax)	56,630	56,481	-149
N	83,009	64,571	

Notes: Comparison of birth-certificate information on the full EDP sample vs. the study sample. See Section 1.3.2 for details on construction of the study sample.

A.3.2 Alternative first-stage estimation

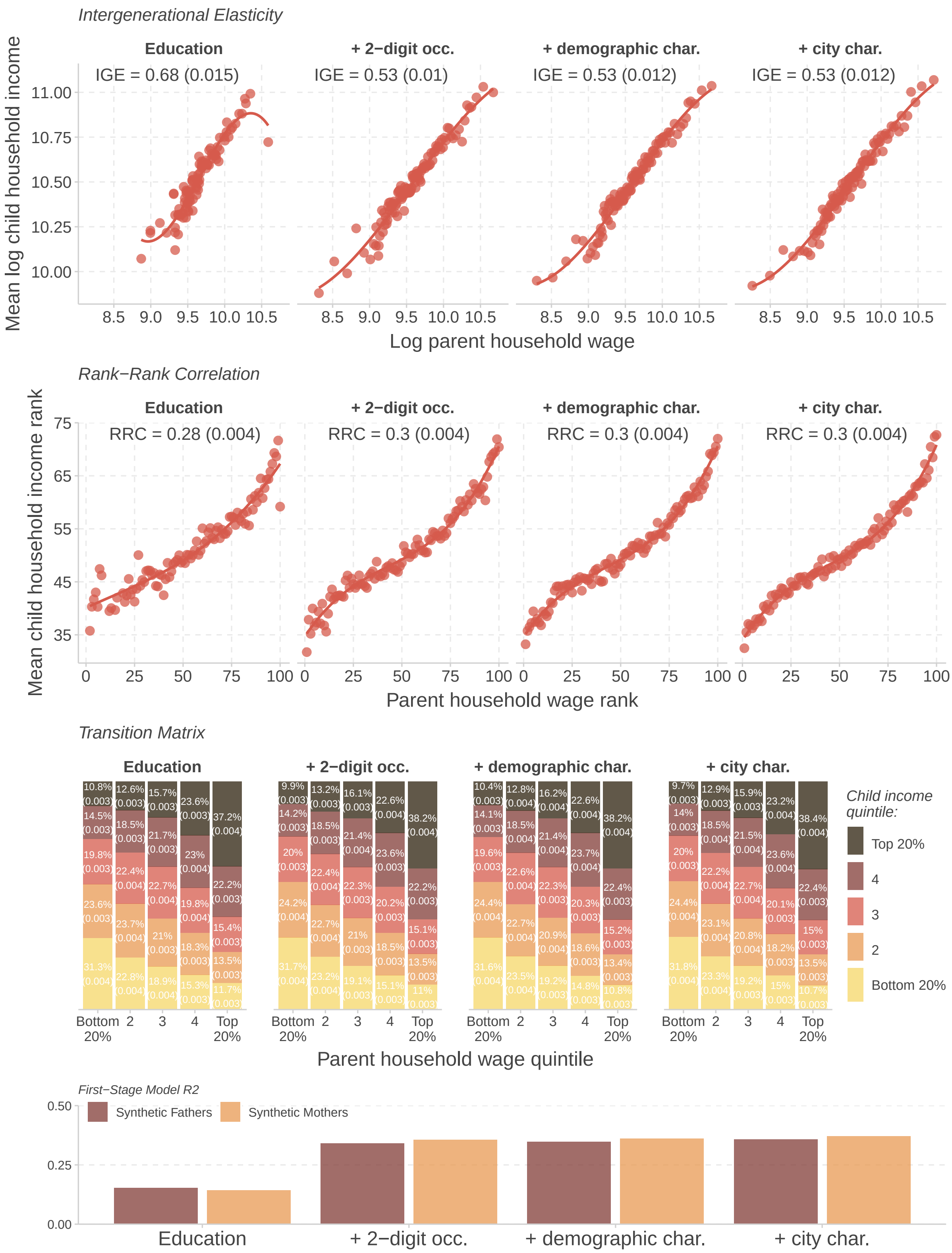
The parent income predictions we use to palliate French data limitations are central to our analysis. It is of primary importance that the first stage of the two-step strategy we rely on is reliable. We make sure that this first stage does not spuriously drive the results in one way or another by evaluating its sensitivity to varying the set of instruments and to relaxing parametric assumptions.

Set of first-stage predictors

The most important dimension to consider is the set of variables included in the first stage, notably because it has been shown that inadequate instruments could yield inconsistent estimates (Jerrim et al., 2016). Appendix Figure A.17 documents the sensitivity of IGE, RRC and transition matrix estimates to the set of predictors used in the first-stage estimation. We estimate them when adding each of the following predictors sequentially (all measured in 1990): education (8 categories), 2-digit occupation (42 cat.), a group of demographic characteristics (age, French nationality dummy, country of birth (6 cat.), and household structure (6 cat.)) and a group of municipality-level characteristics (unemployment rate, share of single mothers, share of foreigners, population, and population density). Since relying on a single variable with less than 100 categories induces some income values to span over several percentiles, parents with a given predicted income are attributed the average rank of individuals earning that level of income. Lastly, we also report the adjusted R^2, computed as the average from 5-fold cross-validation.

We find that the IGE is 0.68 when using only education as the first-stage predictor, consistent with a point already made in the literature that using only education as a predictor is likely to yield inflated estimates of the IGE. Once 2-digit occupation is included in the first-stage, adding other demographic or municipality-level characteristics has no effect on the estimates. Indeed, as can be seen from the R^2, most of the predictive power actually comes from the 2-digit occupation variable. The RRC appears remarkably unchanged by the set of first-stage predictors used, at 0.28 with only education and 0.30 with all variables. This appears once more to be a strength of the RRC in the TSTSLS context.

Figure A.17: Robustness of baseline estimates to different first-stage predictors



Notes: This figure assesses the robustness of our baseline IGE, RRC and transition matrix estimates to variations in the set of first-stage predictors. Parent income is predicted separately for fathers and mothers using a set a of instruments that vary along the x-axis. We report the corresponding CEFs, along with the point estimates and the bootstrapped standard error in parenthesis. The bottom panel of the figure reports separately for synthetic fathers and mothers the R^2 associated with each first stage. See Figure 1.3's notes for details on data, sample and income definitions.

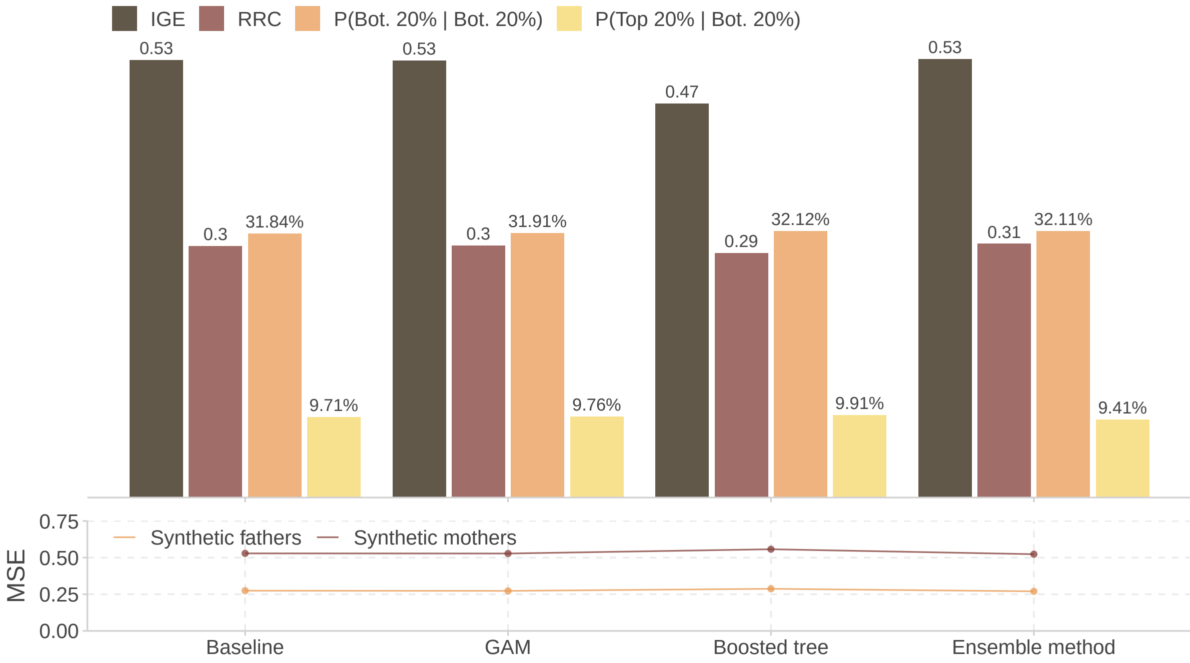
Flexible models

We make use of semi- and non-parametric models to elicit potential misspecifications in the first stage. The baseline specification of the first stage is of the form $y = \beta X + \varepsilon$, where y is the log of parent lifetime income and X is a set of k predictors. OLS would not account for interactions between predictors nor for non-linearities in the relationship between X and y unless they are explicitly modeled. Fully non-parametric methods of the form $y = m(X) + \varepsilon$ would capture both interactions and non-linearities that may help reduce the out-of-sample MSE. Obtaining a lower MSE and significantly different second-stage estimates with non-parametric models than with OLS would suggest that non-modeled non-linearities, interactions, or both, influence the resulting intergenerational mobility estimates.

We implement this test using three machine learning methods: (i) a generalized additive model (GAM) of the form $y = m_1(x_1) + m_2(x_2) + ... + m_k(x_k) + \varepsilon$ which accounts for non-linearities but not for interactions unless explicitly specified, (ii) a gradient boosted regression tree, that is a high-dimensional combination of sequentially grown regression trees, and (iii) the ensemble method, which consists in taking the average of the predictions from each model weighted in a way that minimizes the out-of-sample MSE.

Appendix Figure A.18 compares the intergenerational mobility estimates and out-of-sample MSE resulting from these three methods using our baseline child and parent income definitions. We do not observe significant differences in MSE between the different prediction methods. The resulting mobility estimates are virtually the same for OLS, GAM and the ensemble method, and slightly smaller for boosted trees. This suggests that conditional on the set of predictors we use, using more flexible estimation methods does not lead to better income predictions and different estimates than using an additive OLS specification.

Figure A.18: Robustness to machine learning prediction



Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates to increasingly flexible first-stage prediction models. Each bar represents the magnitude of the estimate of the corresponding color estimated using the first-stage model indicated on the x-axis. The first set of estimates are the baseline estimates obtained using OLS. The three other sets are obtained using increasingly flexible models: generalized additive models (GAM), gradient boosted regression trees, and the ensemble method. The connected dots represent the average out-of-sample MSEs of the associated prediction models, estimated using 5-fold cross-validation. See Figure 1.3's notes for details on data, sample and income definitions.

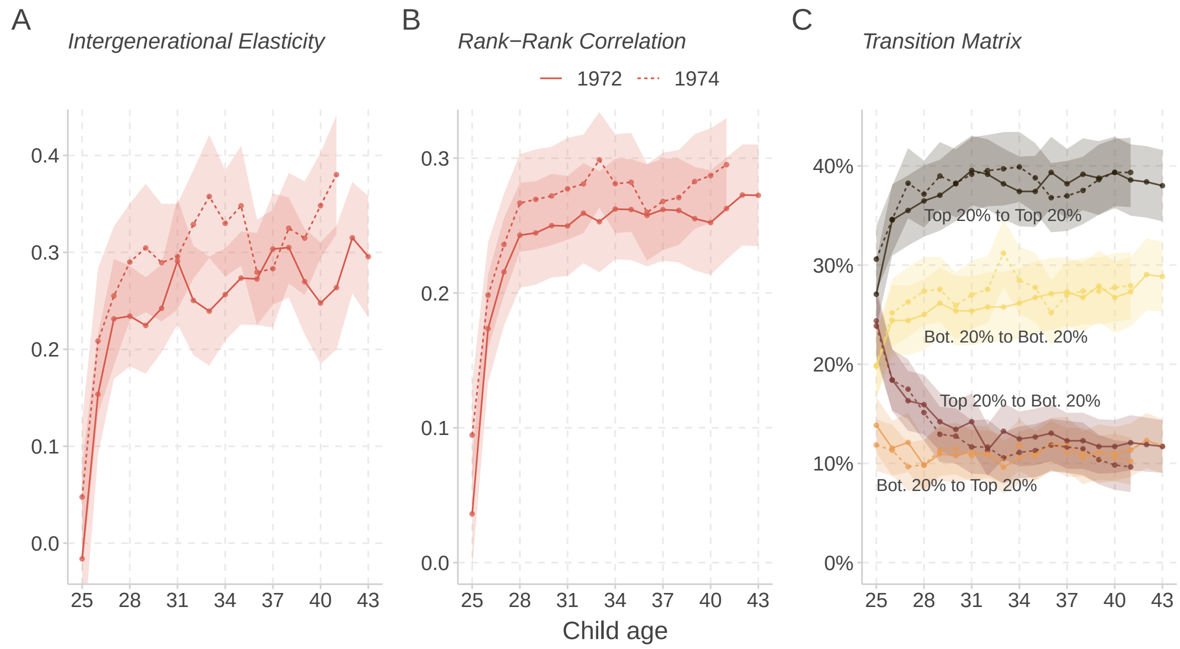
A.3.3 Lifecycle and attenuation bias

Child lifecycle bias - Constant sample of children

To overcome the issue related to changes in Figure 1.6's underlying sample of children, we reproduce the individual wage estimates using the All Employee Panel keeping the sample of children constant. To do so we restrict to children born in 1972 and 1974[16] for whom wages are observed every year between 25 and 43 years old and 25 and 41 years old respectively. Appendix Figure A.19 displays the results. Since the sample is kept constant throughout, the coefficients can be compared to one another and the change in magnitude can only be driven by the age at which child income is measured rather than sample composition. As in Figure 1.6, we find that measuring child income prior to the mid-thirties seriously underestimates the IGE (panel A) and RRC (panel B), and overestimates (underestimates) bottom or top mobility (persistence) (panel C).

[16] We cannot include the 1973 cohort as the All Employee Panel income data are only available for individuals born an even year before 2001. This choice of cohorts is done to be able to measure their incomes after they are 40 years old.

Figure A.19: Child lifecycle bias - 1972 and 1974 cohorts - Constant sample

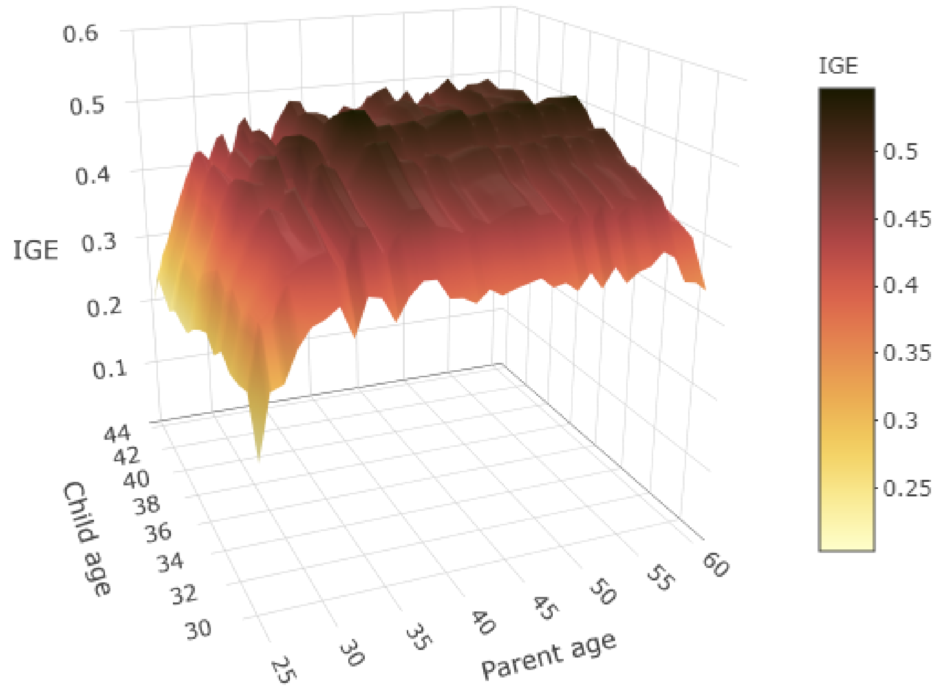


Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates presented in Figures 1.3 and 1.4 to changes in the age at which child income is measured, for children born in 1972 (solid line) and 1974 (dashed line). For both birth cohorts the sample is kept constant, that is only children with wages observed in the All Employee Panel at each age between 25 and 43 years old are retained. Shaded areas represent the 95% bootstrapped confidence interval. See Sections 3.3 and 1.4.4 for for details on data, sample and income definitions.

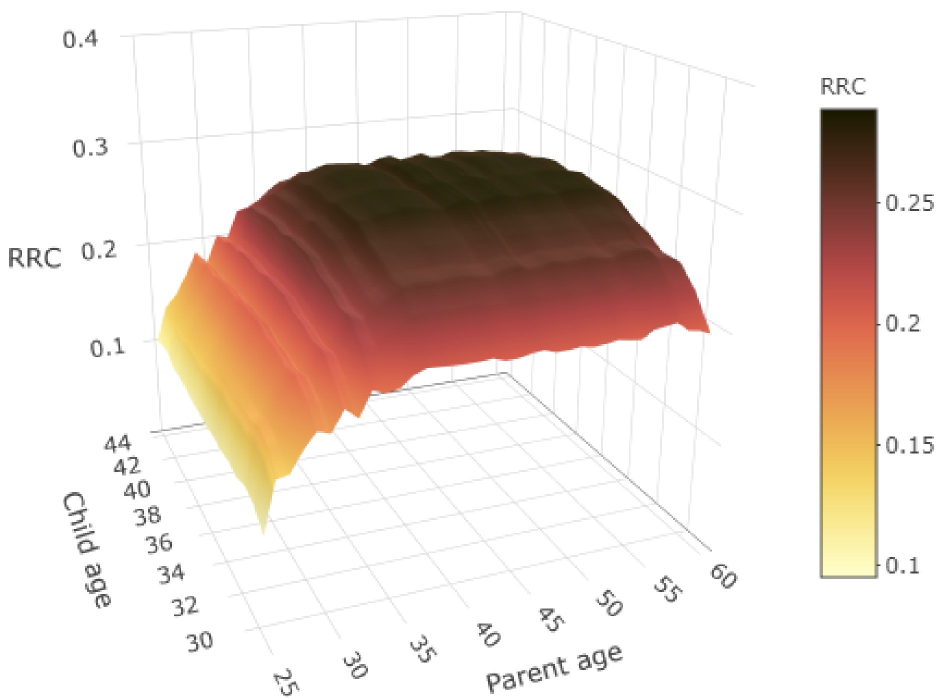
Child and parent lifecycle bias jointly

Child and parent lifecycle bias are typically assessed independently, as we do in the main body of the article. Yet they influence one another and it is instructive to estimate our measures of intergenerational persistence for each possible combination of synthetic parent and child age. Appendix Figure A.20 shows such estimates when child income is measured between ages 30 and 44, and synthetic parent income between ages 28 and 60.

Figure A.20: Child and parent lifecycle bias



(a) Intergenerational elasticity



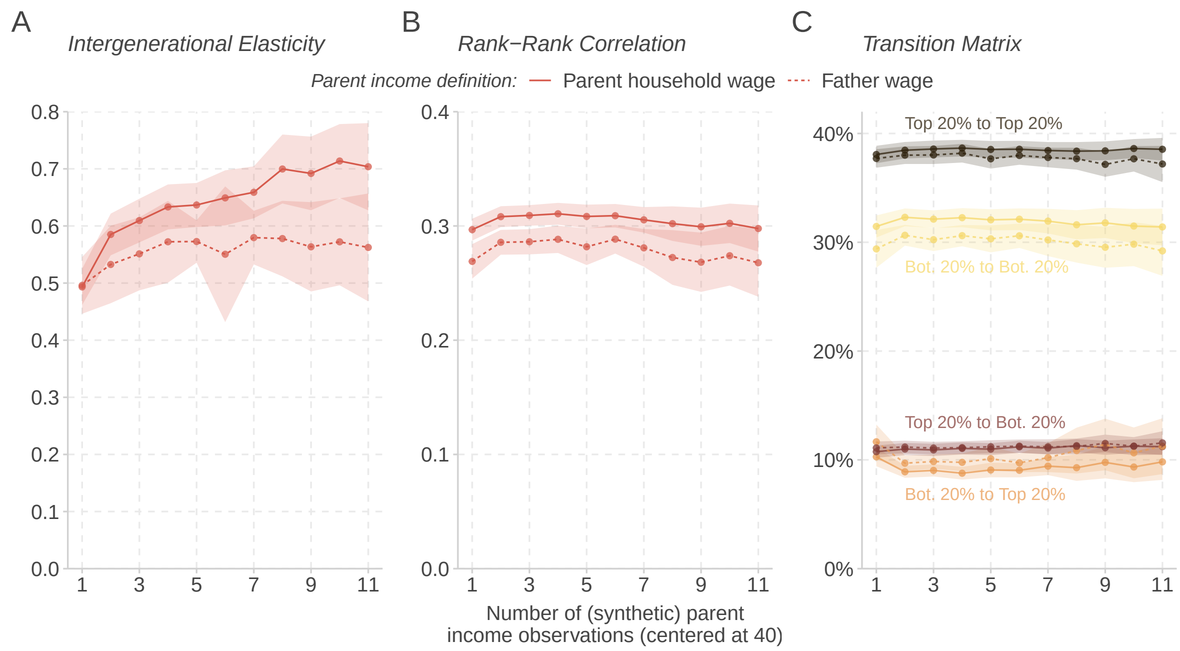
(b) Rank-rank correlation

Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates presented in Figure 1.3 to changes in the age at which child and synthetic parent incomes are measured. The sample of children and synthetic parents varies across ages. See Sections Figure 1.3's notes for for details on data, sample and income definitions.

Parent attenuation bias

Figure A.21 plots estimates of our persistence measures varying the number of synthetic parent income observations used in the first-stage regression from 1 to 11. To control for the potential effect of lifecycle bias we center the age at which synthetic parent income is measured at 40 years old. In other words, one income observation corresponds to income at age 40, two income observations corresponds to average income at ages 39 and 41, three income observations to average income between 39 and 41, and so on. Therefore, 11 income observations corresponds to the average between 35 and 45 years old. The sample of synthetic parents over which income is predicted varies for each estimate depending on how many synthetic parents had incomes observed each year in the required age range. We report results both for parent household wage and father wage.

Figure A.21: Attenuation bias



Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates to the number of income observations used to predict parent income. While varying the number of parent income observations, we center the age range at 40 to control for lifecycle bias. Shaded areas represent the 95% bootstrapped confidence interval. See Figure 1.3's notes for details on data, sample and income definitions.

These results suggest that attenuation bias might affect our baseline IGE (panel A) but not our other estimates of intergenerational mobility. Indeed when defining parent income at the household level, the IGE increases from 0.5 when using only one income observation to 0.7 when averaging over 11 income observations (i.e., between 35 and 45). It is important to highlight that almost all of this change is driven by how mothers' incomes are predicted. Indeed when looking at the father-child IGE, the estimate does not increase so markedly and stabilizes around 2 or 3 income observations, consistent with the idea that the two-stage procedure employed drastically shrinks the transitory component of annual income, and in large contrast with what is typically found when parent income is actually observed (Mazumder, 2005). Indeed, since we are already predicting parent income based on observable characteristics, and thus in a sense reducing year-to-year income volatility, averaging over more years does not affect the estimate much.

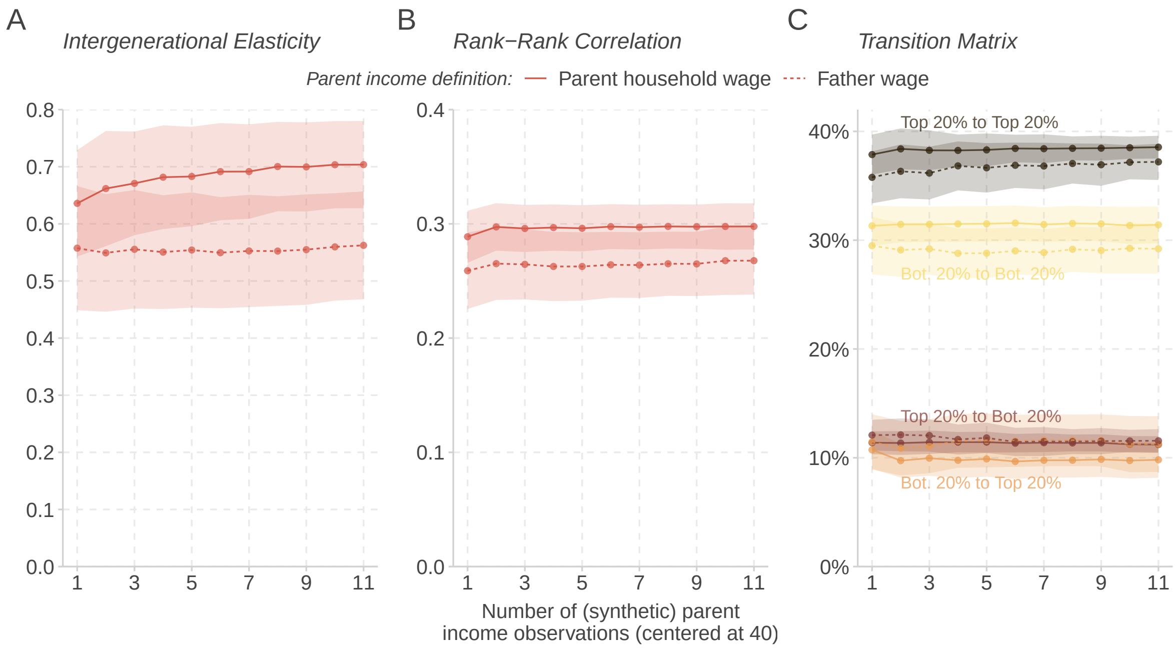
How one interprets the results based on parent household wage depends on one's prior as to how to best predict mothers' incomes. Our view is that predicting mothers' incomes only on the subsample of synthetic mothers with observed wages in all years between 35 and 45 years old biases the underlying sample considering the uneven labor force participation of women at the time. We believe our choice of restricting our sample of synthetic parents to those with at least two income observations between ages 35 and 45 is reasonable.

Constant sample. We check whether the lack of change in intergenerational mobility measures with the number of synthetic parent income observations observed in Figure A.21 could be due to the fact that the sample of synthetic parents varies throughout. We replicate those estimates restricting the sample of synthetic parents to those with all 11 income observations between 35 and 45 years old and estimating the intergenerational mobility measures by varying the number of income observations averaged in the first-stage regression (centered around 40 years old again). To do so, we impute wages in 1981, 1983 and 1990, for which the data are not available,[17] using the average wage between the previous and subsequent year only if both wages are observed. This enables us to have a consistent sample and increase the number of synthetic parents on which the predictions can be done.

Appendix Figure A.22 displays the results from this sensitivity analysis. The increase in the parent household wage IGE is much less marked, increasing from 0.636 when using one income observation to 0.704 when using all 11 observations (panel A). Our interpretation of this relatively modest increase is that averaging over at least 2 income observations as we do for our baseline estimate should suffice to not suffer from attenuation bias. Note that what matters in this figures is not how different the estimates are from our baseline estimate but rather the extent to which they vary with the number of synthetic parent income observations used. Indeed, the difference between our baseline IGE estimate and the estimates obtained are driven by the fact that the sample of synthetic parents for whom we observe all incomes between 35 and 45 years old is a highly non-representative sample, especially when it comes to mothers. In fact, we do not find any attenuation bias when restricting our analysis to fathers, suggesting all the variation in the IGE can be accounted for by changes in mothers' incomes predictions. As with the varying synthetic parent sample estimates, rank-based intergenerational mobility measures are significantly less sensitive to averaging over more income years, and the estimates found are very close to our baseline ones (panels B and C).

[17] As explained in Appendix A.1, the 1982 and 1990 population censuses generated an extra workload which prevented INSEE from compiling the All Employee Panel data for these years.

Figure A.22: Parent attenuation bias - Constant sample of synthetic parents



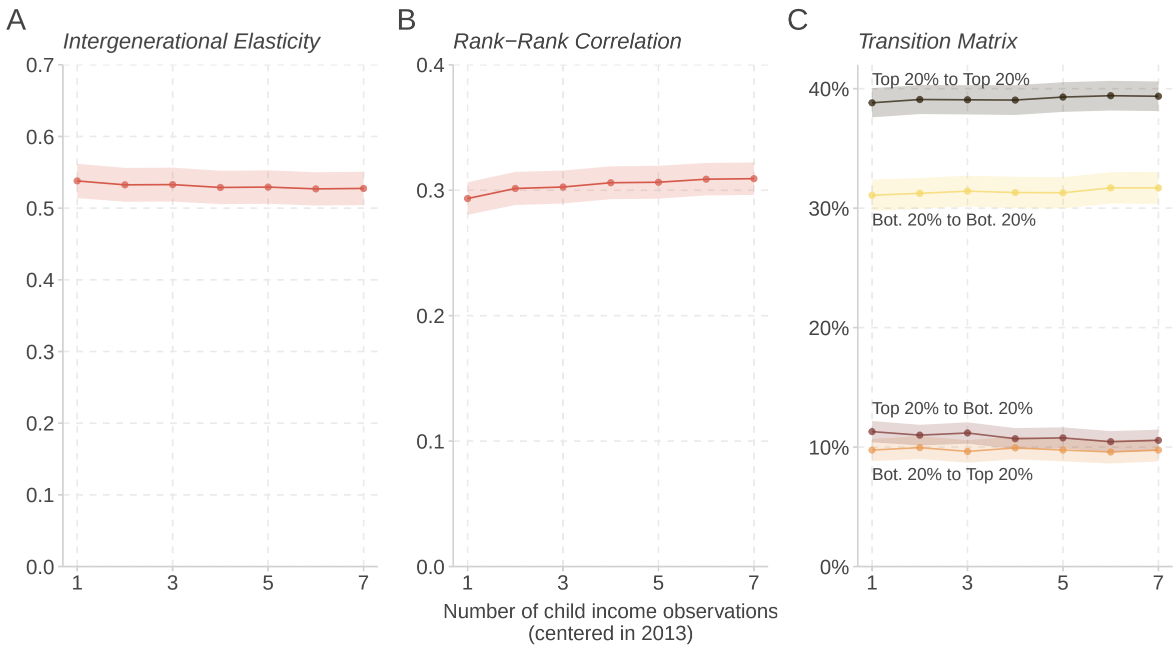
Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates to the number of income observations used to predict parent income, keeping the sample of synthetic parents constant. The sample of synthetic parents is thus restricted to those with all 11 income observations between 35 and 45 years old. While varying the number of parent income observations, we center the age range at 40 to control for lifecycle bias. Shaded areas represent the 95% bootstrapped confidence interval. See Figure 1.3's notes for details on data, sample and income definitions.

Child attenuation "bias"

Appendix Figure A.23 plots estimates of our persistence measures varying the number of child income observations from 1 to 7 between 35 and 45 years old, keeping the sample of children constant[18] (i.e. keeping only children with 7 household income observations). Due to this restriction only cohorts born between 1972 and 1975 are kept. Without this restriction, the value reported for 1 income observation would correspond to our baseline estimate. In the same way as for parents, we control for lifecycle bias by centering the year in which child income is measured to 2013. In other words, one child income observation corresponds to income measured in 2013, two income observations corresponds to the average between 2012 incomes and 2014 incomes, three to average income between 2012 and 2014, etc. The results suggest that estimates are largely unaffected by increasing the number of child income observations.

[18]The sample varies ever so slightly for the IGE due to the number of negative or 0 incomes changing between years.

Figure A.23: Sensitivity to number of child income observations - Constant sample



Notes: This figure presents estimates of our persistence measures varying the number of child income observations from 1 to 7 between 35 and 45 years old, keeping the sample of children constant, i.e. keeping only children with 7 household income observations. (The sample varies ever so slightly for the IGE due to the number of negative or 0 incomes varying between years.) Due to this restriction only cohorts born between 1972 and 1975 are kept. Without this restriction, the value reported for 1 income observation would equal our baseline estimate. We control for lifecycle bias by centering the year in which child income is measured to 2013. In other words, one child income observation corresponds to income measured in 2013, two income observations corresponds to the average of 2012 and 2014, three to average income between 2012 and 2014, etc. Shaded areas represent the 95% bootstrapped confidence interval. See Figure 1.3's notes for for details on data, sample and income definitions.

A.3.4 Sensitivity to income distribution tails

Our baseline estimates may be sensitive to two main sample selection choices when it comes to the income distributions of parent and children: (i) how children with negative or zero incomes are treated; and (ii) how the top and bottom tails of both the parent and child income distributions are dealt with.

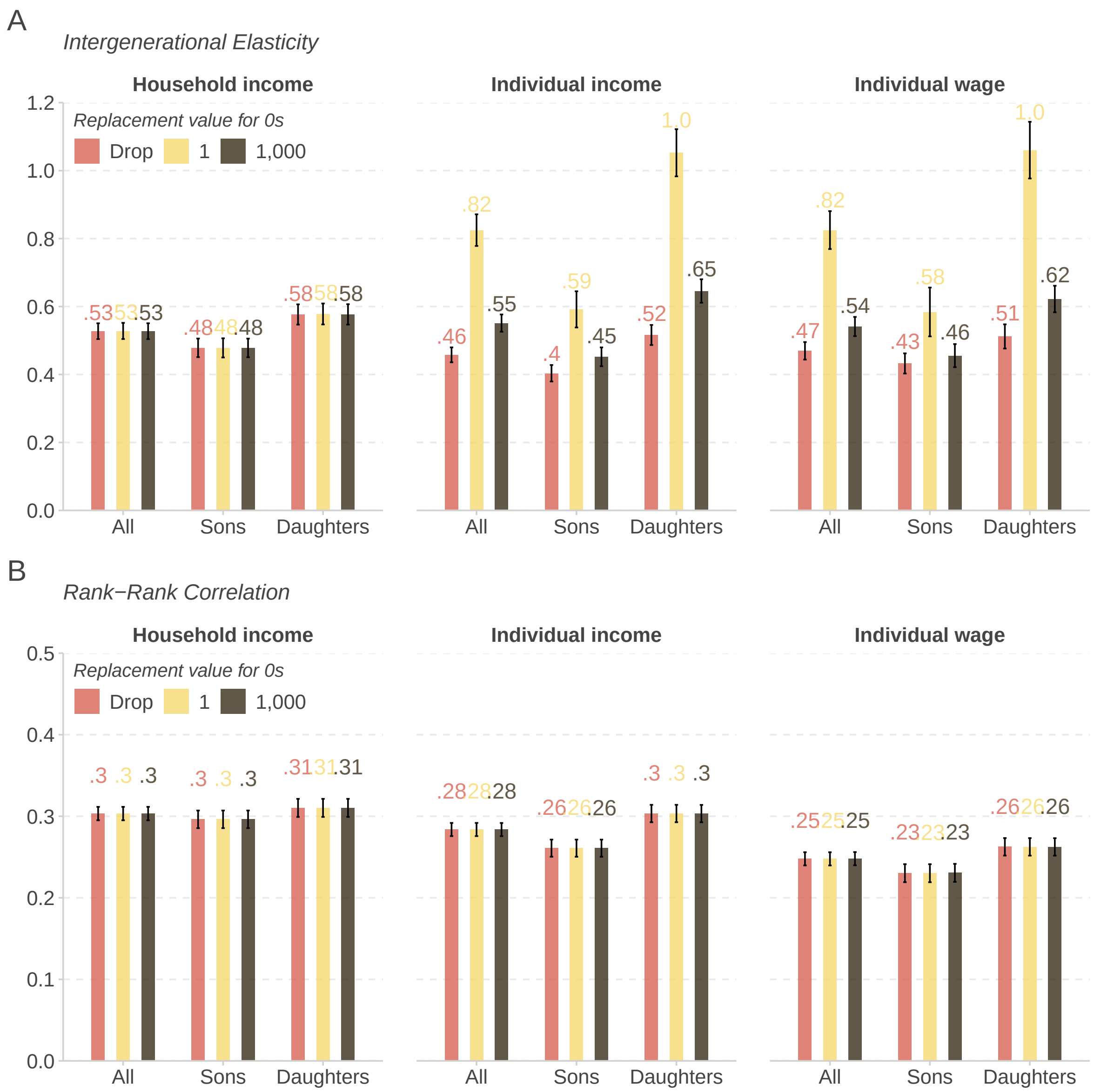
Treatment of zeros

The first issue is particularly salient for the estimation of the intergenerational income elasticity due to the impossibility of taking the log of zero.[19] Many researchers simply discard such observations since they are likely not representative of lifetime income. Though this may potentially be the case if only short income time spans are available, we nonetheless evaluate how our baseline estimates of both the IGE and the RRC when replacing negative or zero child income values by 1 or 1,000 euros.

[19]Various methods have been proposed to overcome this issue. Bellégo et al. (2021) describe such methods and propose a novel solution that can be applied to a variety of cases.

Appendix Figure A.24 shows estimates for the IGE and RRC when replacing income of children reporting negative or zero incomes by 1 euro or 1,000 euros, for different child income definitions. For our primary child income definition, household income, the estimates do not change due to there being very few children with negative or zero household income. However, for child income defined at the individual-level, for which the share of negative or zero incomes is more important, the IGE becomes highly sensitive to the recoding of such observations while the RRC remains unchanged. For example, for individual child income, the IGE is 0.46 when zeros are dropped and 0.82 when they are recoded to 1 and 0.55 when recoded to 1,000. The RRC is entirely insensitive to such recoding as ranks are not altered by it.

Figure A.24: Sensitivity to different zero child income replacement values



Notes: This figure assesses the robustness of our baseline IGE and RRC estimates to replacing incomes of children reporting negative or zero incomes by 1 euro or 1,000 euros, for different child income definitions. Vertical lines represent the 95% bootstrapped confidence intervals. See Section 3.3 for for details on data, sample and income definitions.

Top and bottom trimming

The second issue relates to the treatment of top and bottom earners in both the parent and child income distributions. For the parent income distribution the choice can both be made in the prediction stage and in the second stage. Specifically, we assess how the IGE and RRC vary when trimming the top and/or bottom 1% to 5% and 10%. Appendix Figure A.25 displays the results of this sensitivity check. There are three main takeaways.

First, the IGE is significantly more sensitive to small changes in parent or child income distributions while the RRC remains relatively stable. For example, removing the top and bottom

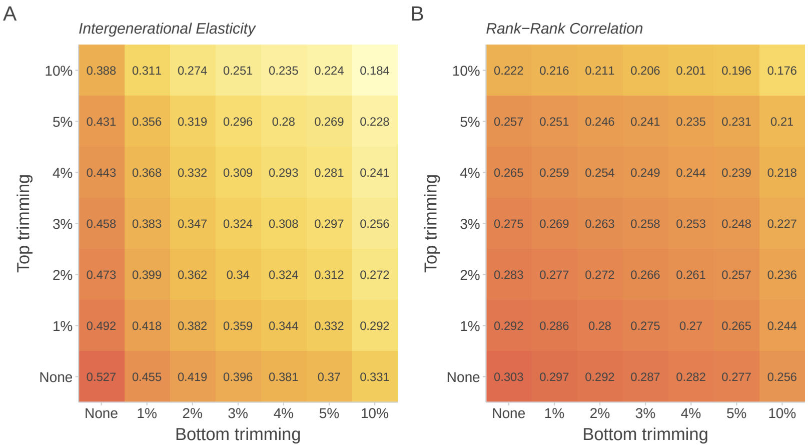
1% of child incomes decreases the IGE from 0.527 to 0.418 while the RRC only decreases from 0.303 to 0.286. It does not seem desirable that a measure of intergenerational mobility should be so sensitive to excluding just 2% of children. Mathematically it can be linked to changes in the dispersion of the distribution of child incomes but conceptually it seems difficult to defend such responsiveness to minor sample changes.

Second, the IGE is quite strongly influenced by minor trimming in the first-stage prediction sample. For example, excluding the bottom and top 2% of synthetic parent incomes leads to an IGE of 0.6. Such exclusions are not uncommon in the literature though their relevance is unclear.[20] Meanwhile the RRC is once more remarkably untouched by first-stage parent income exclusions. In fact excluding the bottom and top 10% of synthetic parent incomes decreases the RRC to 0.301 (from 0.303). This appears to be an additional benefit of estimating the RRC when using with the TSTSLS method. Note that trimming the first-stage prediction sample does lead to increased out-of-sample MSE, as shown in Appendix Figure A.26.

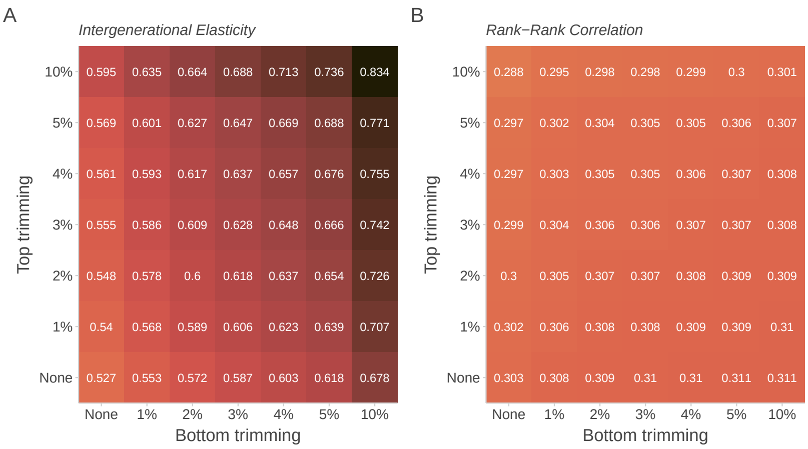
Third, for second-stage parent income trimming, the effects are relatively mild for both intergenerational mobility measures. This is very likely a consequence of the two-stage procedure which reduces the variance in parent incomes.

[20] For example, Barbieri et al. (2020) exclude the top and bottom 1% of their sons and synthetic fathers' incomes.

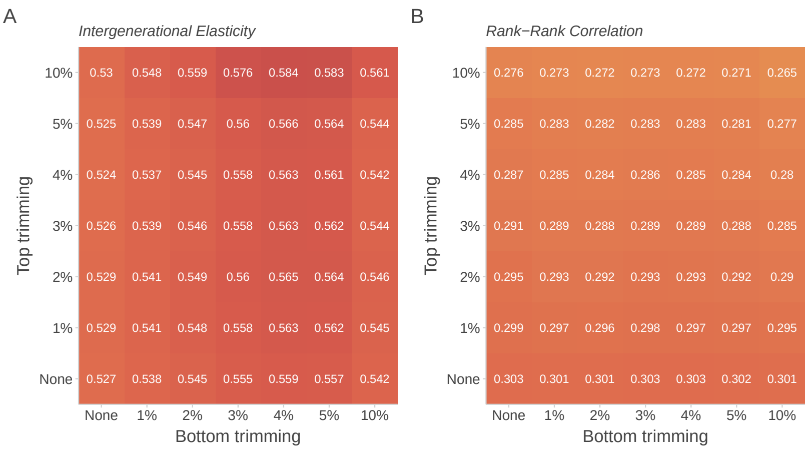
Figure A.25: Sensitivity to child and parent income distributions trimming



(a) Child income trimming



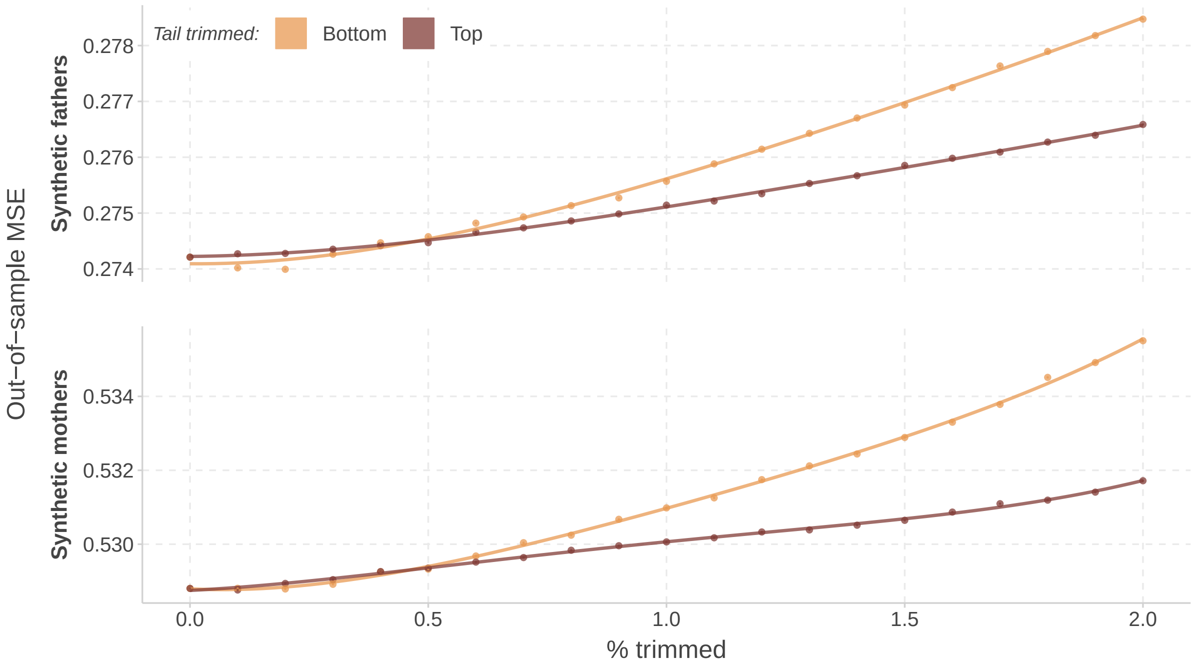
(b) First-stage synthetic parent income trimming



(c) Second-stage parent income trimming

Notes: This figure assesses the robustness of our baseline intergenerational income mobility estimates presented to trimming the tails of the parent and child income distributions. Each cell displays the value of the corresponding intergenerational mobility measure obtained after trimming the income distribution of the corresponding sample by the fraction indicated on the x-axis at the bottom and by that indicated on the y-axis at the top. See Figure 1.3's notes for details on data, sample and income definitions.

Figure A.26: Out-of-sample MSE as a function of top and bottom trimming



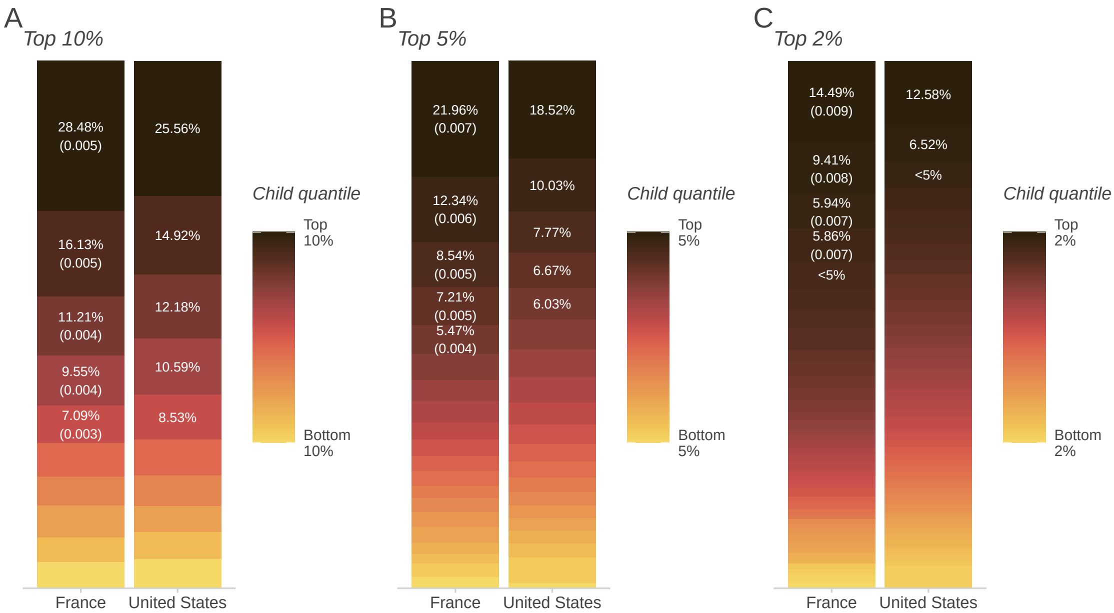
Notes: This figure plots the out-of-sample MSE as a function of trimming various shares of the tails of synthetic parents' income distribution. Our-of-sample MSEs correspond to the average MSE obtained from 5-fold cross-validation. See Sections 1.3.1 and 1.3.2 for details on the exact model being estimates and sample construction.

A.3.5 Transition probabilities at the top

To analyze persistence at the top of the parent income distribution, we estimate transition matrices for the top 10%, top 5% and top 2% of parent incomes and compare our results with those from the United States.[21] We estimate the likelihood of remaining in the top 10% to be about 28% in France close to the United States figure of 26%. This statistic is almost 3 times larger than would be observed in a world where child income is unrelated to parent income (i.e., 10%). This persistence at the top gets stronger as we zoom into the top 5% (22% remaining in top 5%) and top 2% (14% remaining in top 2%). The ratio of observed persistence to counterfactual world with no link between incomes increases with parent income rank in the distribution. This suggests that mechanisms of intergenerational persistence at the top of the parent income distribution might differ from those at play for the rest of the distribution.

[21] We use the detailed percentile-by-percentile estimates provided in the online appendix of Chetty et al. (2014).

Figure A.27: Top parent income quantiles transition matrices in France and United States



Notes: This figure presents intergenerational transition matrix estimates for children coming from families in the top 10% (panel A), top 5% (panel B) and top 2% (panel C) of the parent income distribution, with bootstrapped standard errors in parentheses. We compare the transition probabilities we obtain for France with those computed by Chetty et al. (2014) for the United States. Each cell corresponds to the percentage of children in a given income quantile who have parents in a given parent income quantile. See Section 3.3 for for details on data, sample and income definitions.

A.4 Correlation with local characteristics

A.4.1 Definitions and data sources

Appendix Table A.8 displays the variables used in the correlational analysis presented in Section 1.6 (subsection *Correlation with Local Characteristics*). We measure these variables as close to 1990 as possible so as to reflect the environment individuals grew up in.

A.4.2 Simple regression analysis

We start by regressing department-level intergenerational mobility estimates on each of these variables in separate regressions. Both the department intergenerational mobility estimates and the characteristics are standardized, implying that the coefficients can be interpreted as correlations. Results are presented in Appendix Tables A.9 to A.11 and summarized in Figure 1.9. Note that for the IGE and RRC, a positive coefficient implies the characteristic is positively correlated with intergenerational *persistence* (i.e., negatively correlated with intergenerational *mobility*), while for absolute upward mobility a positive coefficient implies the characteristic is positively correlated with higher incomes for children born to low-income families.

Appendix Figure A.28 provides a potential explanation for the results of the correlational analysis by documenting the correlation between all department characteristics. The 14 variables considered are for the most part quite strongly correlated with one another, both within and between variable groups. For instance, the Gini index is highly correlated with other in-

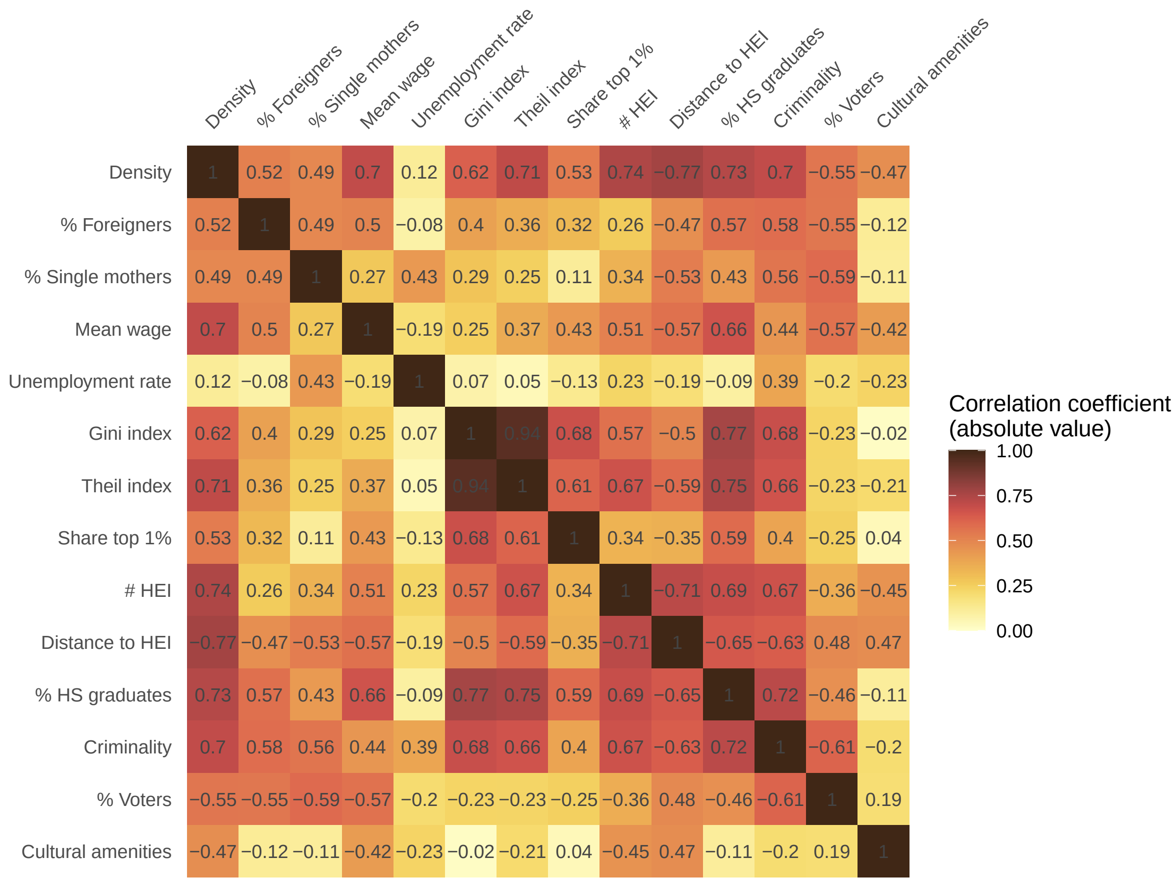
Table A.8: Definitions and sources of department characteristics

Variable	Definition	Source
Demographic		
Density	Log number of inhabitants per square meter	1990 BDCOM[1]
% Foreigner	Share without French nationality	1990 Census
% Single mothers	Share of single mothers in the adult population ($\geq$ 18)	1990 Census
Economic		
Mean wage	Log average wage	1996 DADS Panel
% Unemployed	Unemployment rate	1990 Census
Inequality		
Gini index	Gini index of wage inequality	1996 DADS Panel
Theil index	Theil index of spatial wage segregation	1996 DADS Panel
Share top 1%	Share of total wage accrued by the top 1% of wage earners	1996 DADS Panel
Education		
# HEI	Number of higher education institutions	2007 BPE[2]
Distance to HEI	Average distance to the closest public higher education institution	2007 BPE[2]
% HS graduates	Share of high-school graduates in adult population ($\geq$ 18)	1990 Census
Social capital		
Cultural amenities	Number of cinemas and museums per capita	2007 BPE[2], Min. de la Culture
Crime	Number of offenses and crimes per capita	Min. de l'Intérieur
% Voters	Participation rate to the first round of the 1995 presidential election	Min. de l'Intérieur

Notes:
[1] Base de données communales du recensement de la population (BDCOM) - 1990, INSEE (producteur), ADISP (diffuseur) - doi:10.13144/lil-0363
[2] Base permanente des équipements (BPE) - 2007, INSEE (producteur), PROGEDO-ADISP (diffuseur) - doi:10.13144/lil-0423

equality measures, but also with population density and the share of high school graduates, two variables whose relationship with absolute upward mobility is positive.

Figure A.28: Correlation between department characteristics



Notes: This figure presents the correlation coefficient between all department characteristics considered, defined as follows. See Appendix Table A.8 for definitions and sources of the department characteristics.

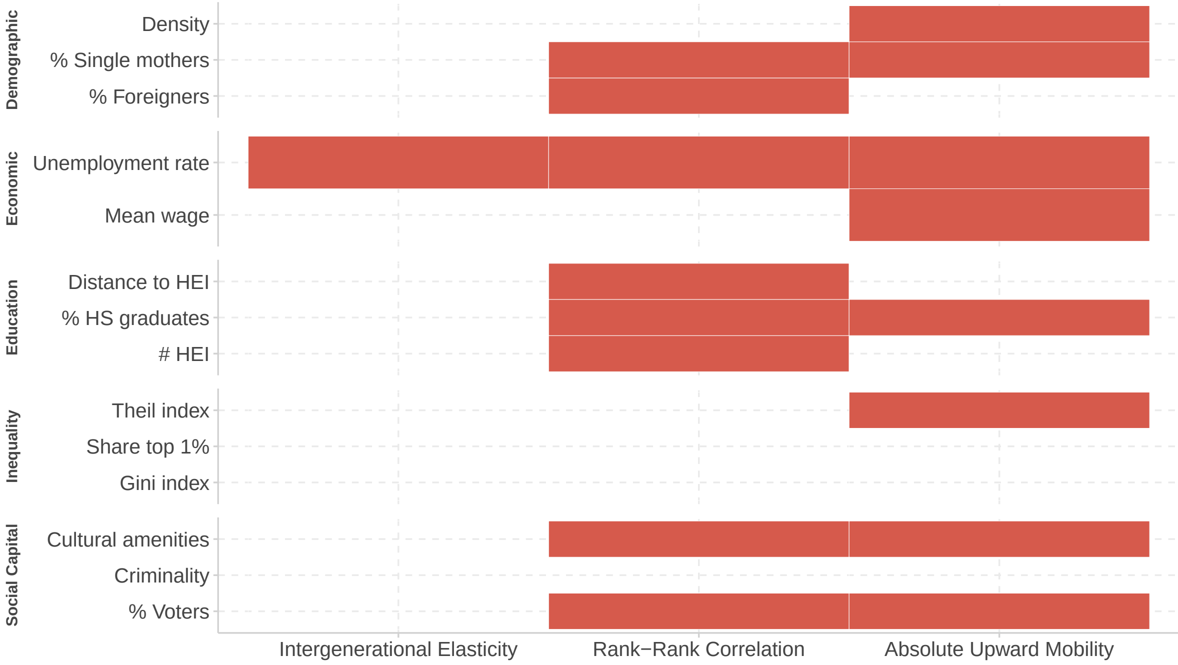
A.4.3 Lasso analysis

Considering the strong correlation across department characteristics, we estimate lasso regressions in order to identify the characteristics that are most strongly associated with intergenerational mobility. The result of this analysis is presented in Appendix Figure A.29.

The lasso analysis does not change the picture much. For the IGE, only the unemployment rate is picked up, as was the case in the univariate setting. For the RRC, the lasso maintains some demographic characteristics (% of single mothers and % foreigners), the unemployment rate, all three education variables, and two measures of social capital (cultural amenities and % voters). Again, these results are largely in line with what was observed in the univariate regressions. Lastly, for absolute upward mobility roughly the same characteristics that were significant in the simple regression analysis are kept except importantly for the Gini index.

Though the relationships we document between intergenerational mobility and department characteristics are overall pretty intuitive, these descriptive relationships cannot distinguish a potential causal effect of place from sorting. We leave this causal assessment to future studies.

Figure A.29: Department characteristics kept by Lasso



Notes: This figure presents the department characteristics kept by the lasso regression. See Appendix Table A.8 for definitions and sources of the department characteristics.

Table A.9: Correlation between intergenerational elasticity and department characteristics

	Dependent variable: Intergenerational Elasticity													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Density	−0.022													
	(0.110)													
% Single mothers		0.016												
		(0.110)												
% Foreigners			−0.026											
			(0.110)											
Unemployment rate				0.306***										
				(0.104)										
Mean wage					−0.157									
					(0.108)									
Distance to HEI						−0.100								
						(0.109)								
% HS graduates							−0.114							
							(0.109)							
# HEI								−0.022						
								(0.110)						
Theil index									−0.024					
									(0.110)					
Share top 1%										−0.092				
										(0.109)				
Gini index											0.007			
											(0.110)			
Cultural amenities												−0.090		
												(0.109)		
Crime													0.086	
													(0.109)	
% Voters														0.042
														(0.110)
Intercept	4.124***	4.008***	4.183***	2.512***	18.162*	4.374***	4.554***	4.218***	4.397***	4.954***	4.000*	4.386***	3.911***	2.943
	(0.135)	(0.893)	(0.213)	(0.565)	(9.654)	(0.277)	(0.412)	(0.406)	(1.169)	(0.977)	(2.287)	(0.318)	(0.312)	(3.152)
Observations	85	85	85	85	85	85	85	85	85	85	85	85	85	85
R^2	0.0005	0.0003	0.001	0.094	0.025	0.010	0.013	0.0005	0.001	0.008	0.00005	0.008	0.007	0.002

Notes: All variables are standardized such that the regression coefficient corresponds to the correlation. See Appendix Table A.8 for variable definitions and data sources. Standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A.10: Correlation between rank-rank correlation and department characteristics

	Dependent variable: Rank-Rank Correlation													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Density	−0.083													
	(0.109)													
% Single mothers		−0.168												
		(0.108)												
% Foreigners			−0.255**											
			(0.106)											
Unemployment rate				0.181*										
				(0.108)										
Mean wage					−0.131									
					(0.109)									
Distance to HEI						−0.078								
						(0.109)								
% HS graduates							−0.131							
							(0.109)							
# HEI								0.099						
								(0.109)						
Theil index									0.055					
									(0.110)					
Share top 1%										−0.105				
										(0.109)				
Gini index											0.008			
											(0.110)			
Cultural amenities												−0.141		
												(0.109)		
Crime													−0.049	
													(0.110)	
% Voters														0.239**
														(0.107)
Intercept	5.197***	6.617***	5.683***	4.294***	16.940*	5.440***	5.736***	4.906***	4.675***	6.186***	5.097**	5.642***	5.389***	−1.612
	(0.135)	(0.881)	(0.206)	(0.583)	(9.692)	(0.278)	(0.411)	(0.404)	(1.167)	(0.976)	(2.287)	(0.316)	(0.312)	(3.064)
Observations	85	85	85	85	85	85	85	85	85	85	85	85	85	85
R^2	0.007	0.028	0.065	0.033	0.017	0.006	0.017	0.010	0.003	0.011	0.0001	0.020	0.002	0.057

Notes: All variables are standardized such that the regression coefficient corresponds to the correlation. See Appendix Table A.8 for variable definitions and data sources. Standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

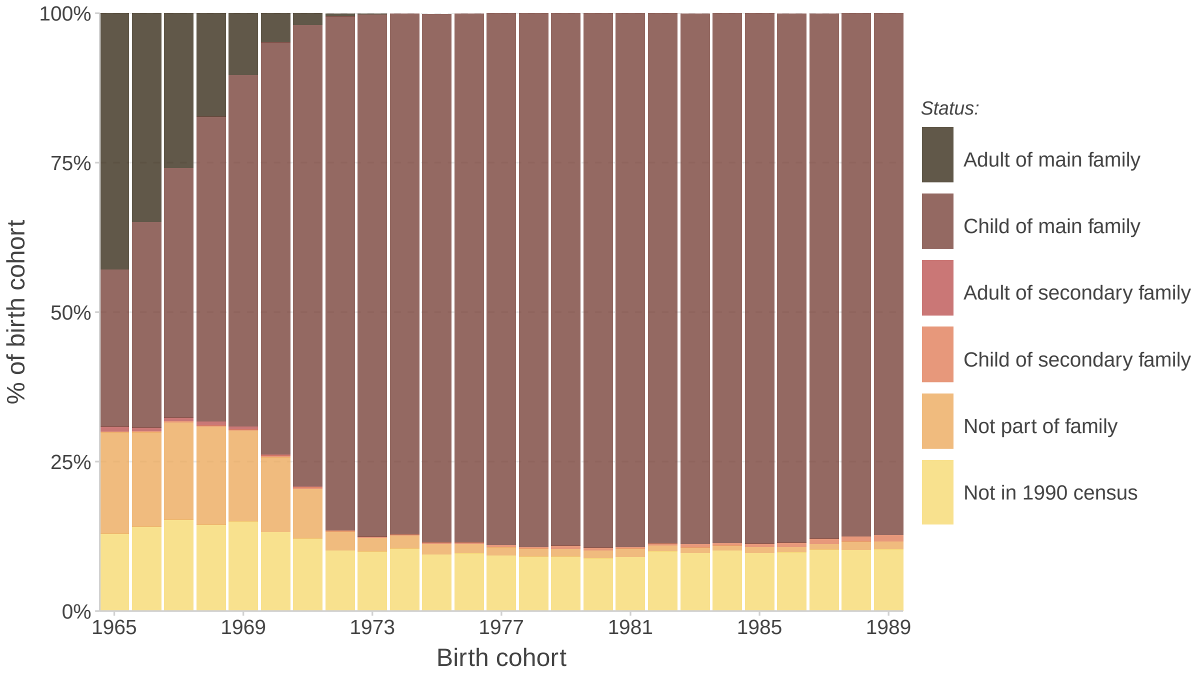
Table A.11: Correlation between absolute upward mobility and department characteristics

	Dependent variable: Absolute Upward Mobility													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Density	0.491^{***}													
	(0.096)													
% Single mothers		0.218^{**}												
		(0.107)												
% Foreigners			0.478^{***}											
			(0.096)											
Unemployment rate				-0.563^{***}										
				(0.091)										
Mean wage					0.591^{***}									
					(0.089)									
Distance to HEI						-0.293^{***}								
						(0.105)								
% HS graduates							0.530^{***}							
							(0.093)							
# HEI								0.274^{**}						
								(0.106)						
Theil index									0.337^{***}					
									(0.103)					
Share top 1%										0.406^{***}				
										(0.100)				
Gini index											0.288^{***}			
											(0.105)			
Cultural amenities												0.015		
												(0.110)		
Crime													0.221^{**}	
													(0.107)	
% Voters														-0.405^{***}
														(0.100)
Intercept	13.426^{***}	11.305^{***}	12.268^{***}	16.055^{***}	-39.580^{***}	13.751^{***}	11.138^{***}	12.092^{***}	9.488^{***}	9.459^{***}	7.068^{***}	13.026^{***}	12.476^{***}	24.709^{***}
	(0.118)	(0.872)	(0.187)	(0.490)	(7.885)	(0.266)	(0.352)	(0.390)	(1.100)	(0.896)	(2.190)	(0.319)	(0.305)	(2.884)
Observations	85	85	85	85	85	85	85	85	85	85	85	85	85	85
R^2	0.241	0.048	0.229	0.317	0.349	0.086	0.280	0.075	0.114	0.165	0.083	0.0002	0.049	0.164

Notes: All variables are standardized such that the regression coefficient corresponds to the correlation. See Appendix Table A.8 for variable definitions and data sources. Standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

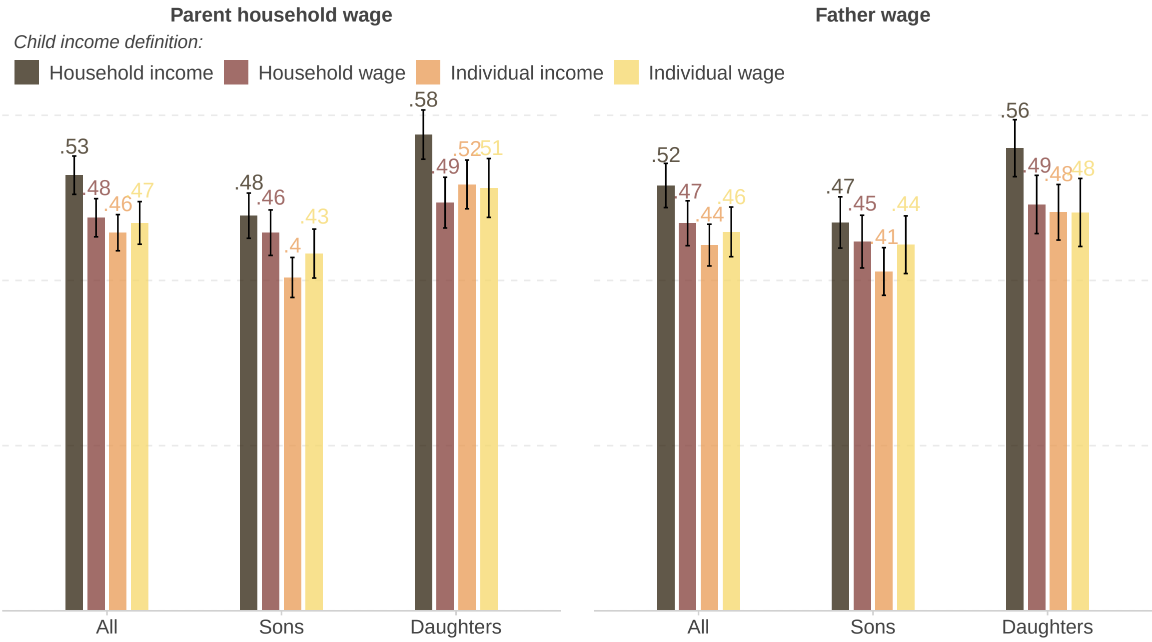
A.5 Additional Figures

Figure A.30: Family position in 1990 census by child birth cohort



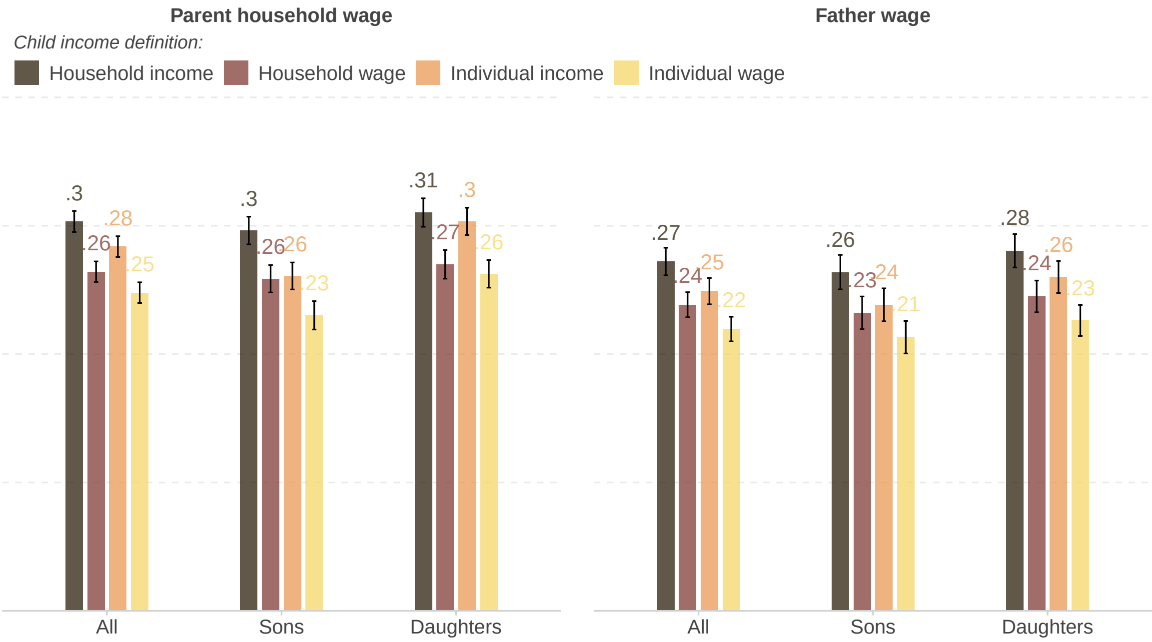
Notes: This figure presents the family position of EDP individuals in the 1990 census by birth cohort. The sample is restricted to EDP individuals born in metropolitan France.

Figure A.31: Baseline IGE estimates for different child and parent income definitions



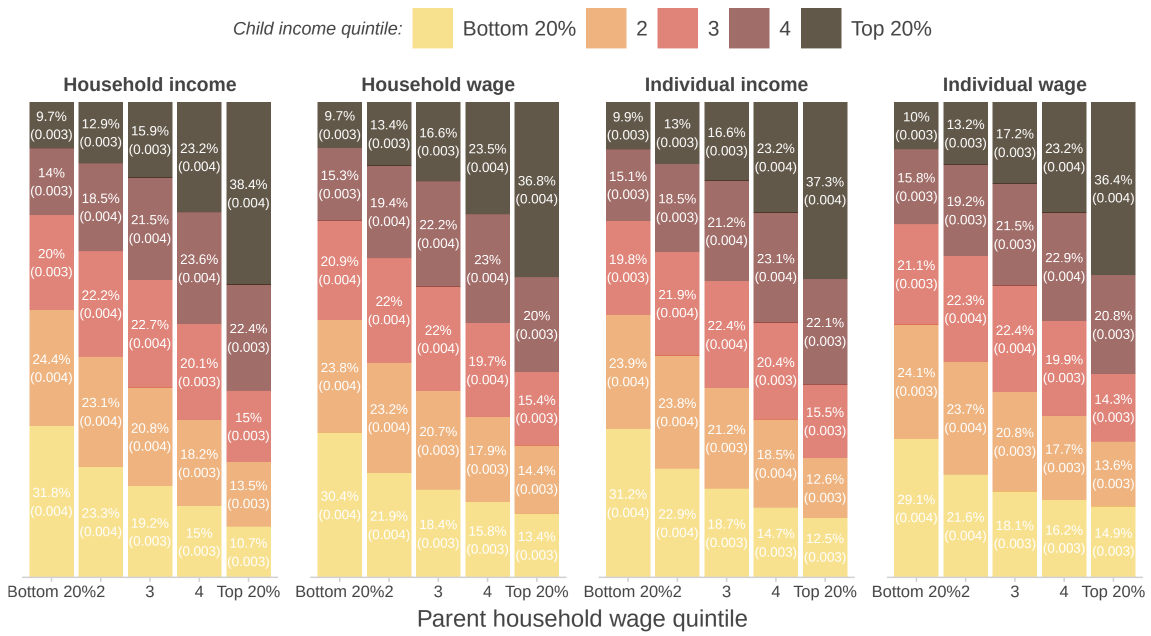
Notes: This figure presents our baseline intergenerational income elasticity estimates for various parent and child income definitions. Each bar represents the coefficient of an OLS regression of child income on parent income, for the entire sample (All) and for sons and daughters separately. Error bars represent the 95% bootstrapped confidence intervals. See Section 3.3 for for details on data, sample and income definitions.

Figure A.32: Baseline RRC estimates for different child and parent income definitions



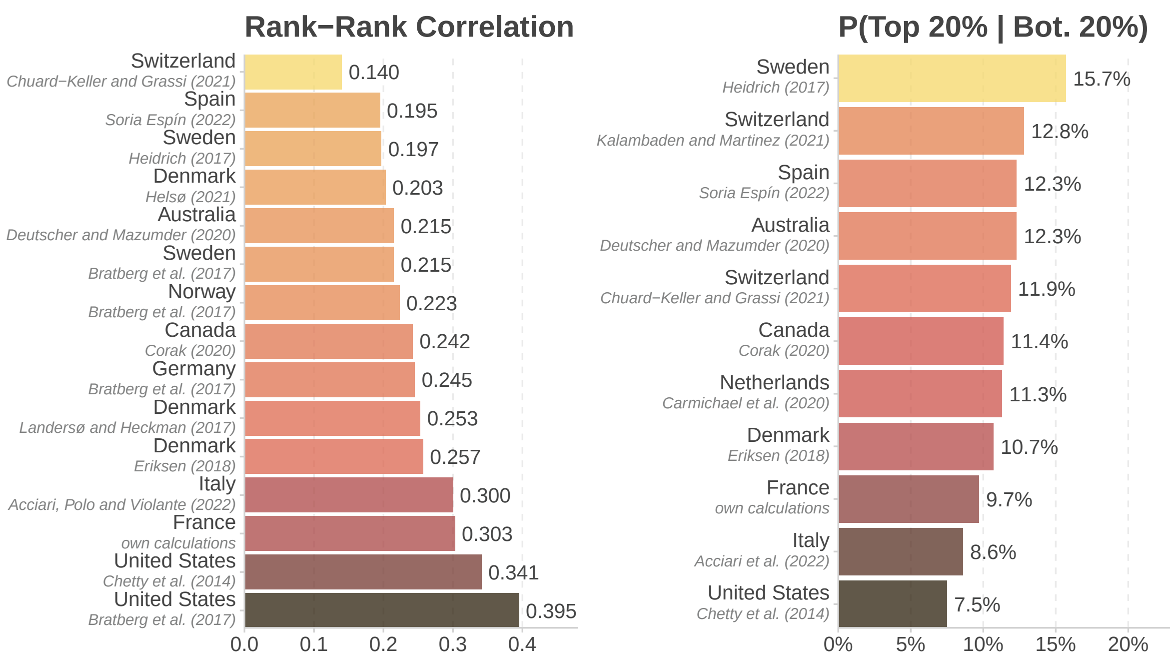
Notes: This figure presents our baseline intergenerational rank-rank correlation estimates for various parent and child income definitions. Each bar represents the coefficient of an OLS regression of child income rank on parent income rank, for the entire sample (All) and for sons and daughters separately. Error bars represent the 95% bootstrapped confidence intervals. See Section 3.3 for for details on data, sample and income definitions.

Figure A.33: Baseline quintile transition matrix for different child income definitions



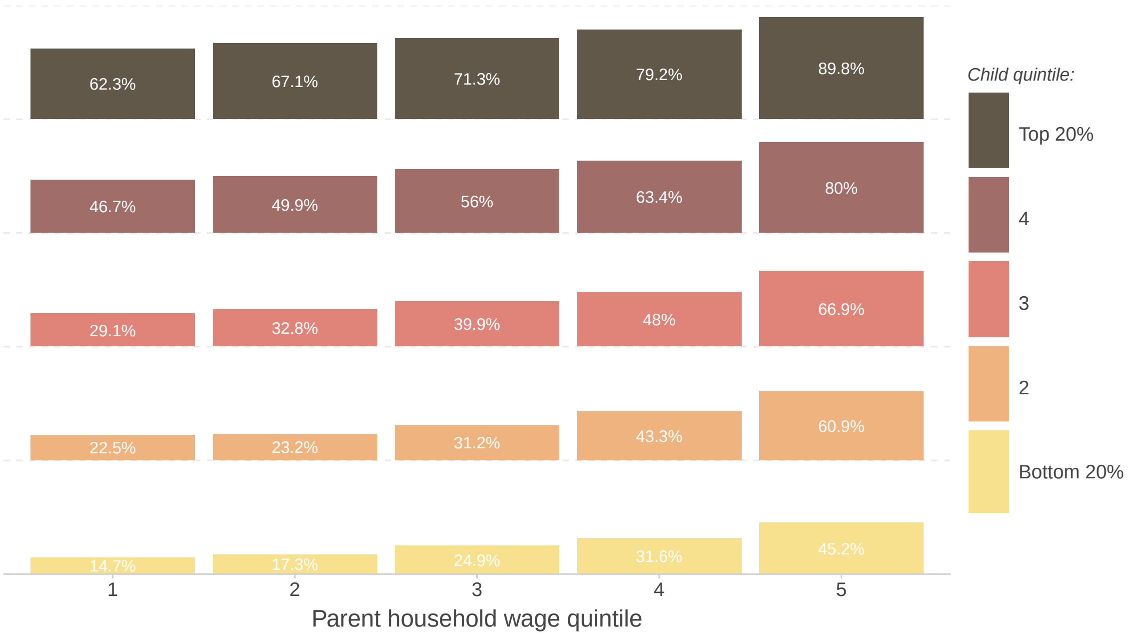
Notes: This figure presents our baseline intergenerational transition matrix estimates for various child income definitions, with bootstrapped standard errors in parentheses. Each cell corresponds to the percentage of children in a given income quintile among children who have parents in a given parent income quintile. See Section 3.3 for for details on data, sample and income definitions.

Figure A.34: Rank-rank correlation and upward mobility in international comparison



Notes: This figure represents the international comparisons in rank-rank correlation and transition matrix cells presented in Tables 1.1 and 1.2.

Figure A.35: Higher education graduation by quintile transition matrix cell



Notes: This figure presents the percentage of children graduating from higher education in each cell of the quintile transition matrix. Each cell corresponds to the percentage of children in a given income quintile coming from a family in a given parent income quintile who have a higher education diploma. See Sections 3.3 and 1.4.4 for for details on data, sample and income definitions. In this figure parent income ranks are computed without parent education in the set of first-stage predictors to avoid capturing the effect of parent education independent from that of parent income.

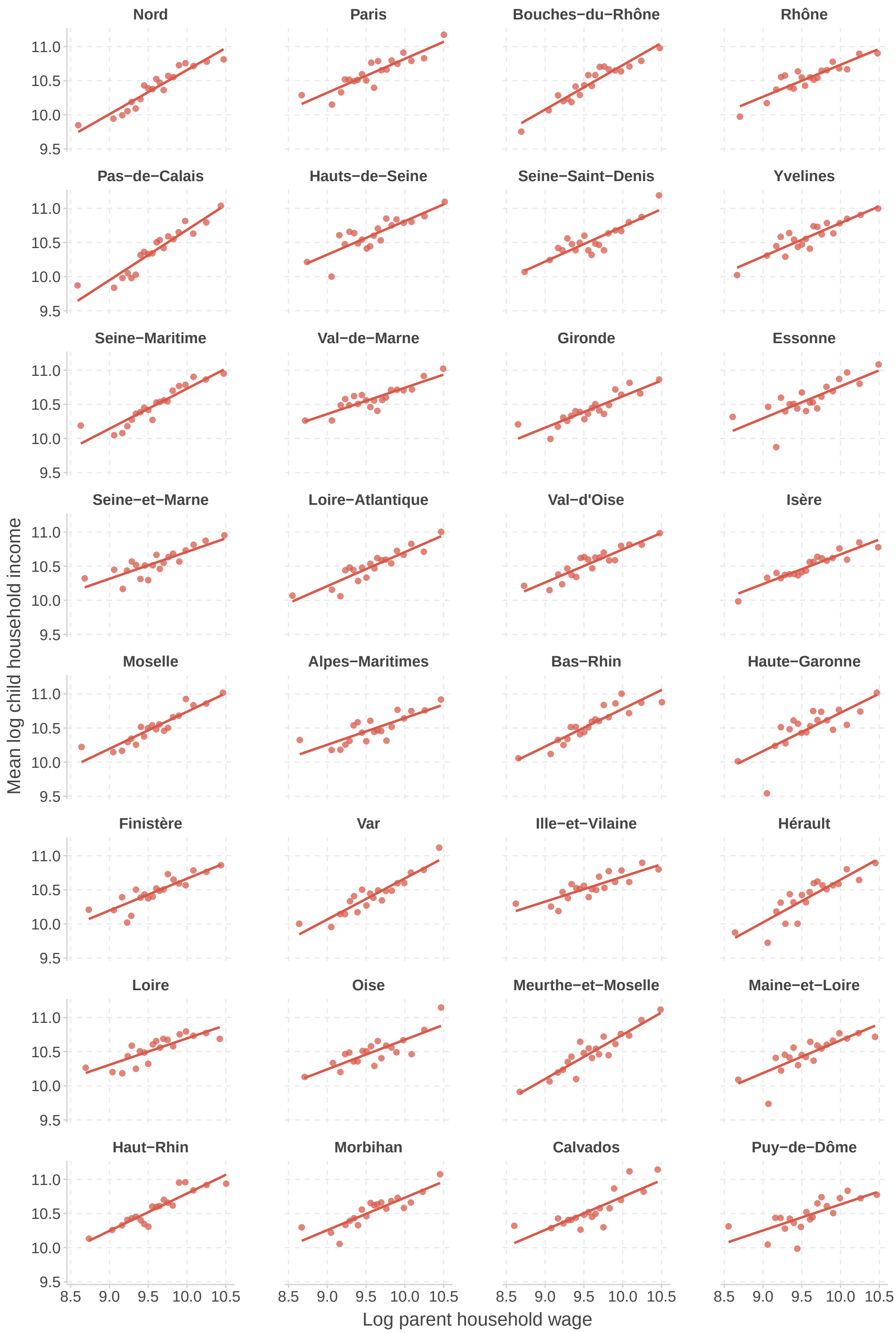
Figure A.36: French departments

Notes: This figure represents the 96 metropolitan French departments. The borders of these departments have not changed over the study period. For convenience, we treat Corsica (*Haute Corse* and *Corse du Sude*) as a single department.

Figure A.37: Illustration of absolute upward mobility for the *Nord* department

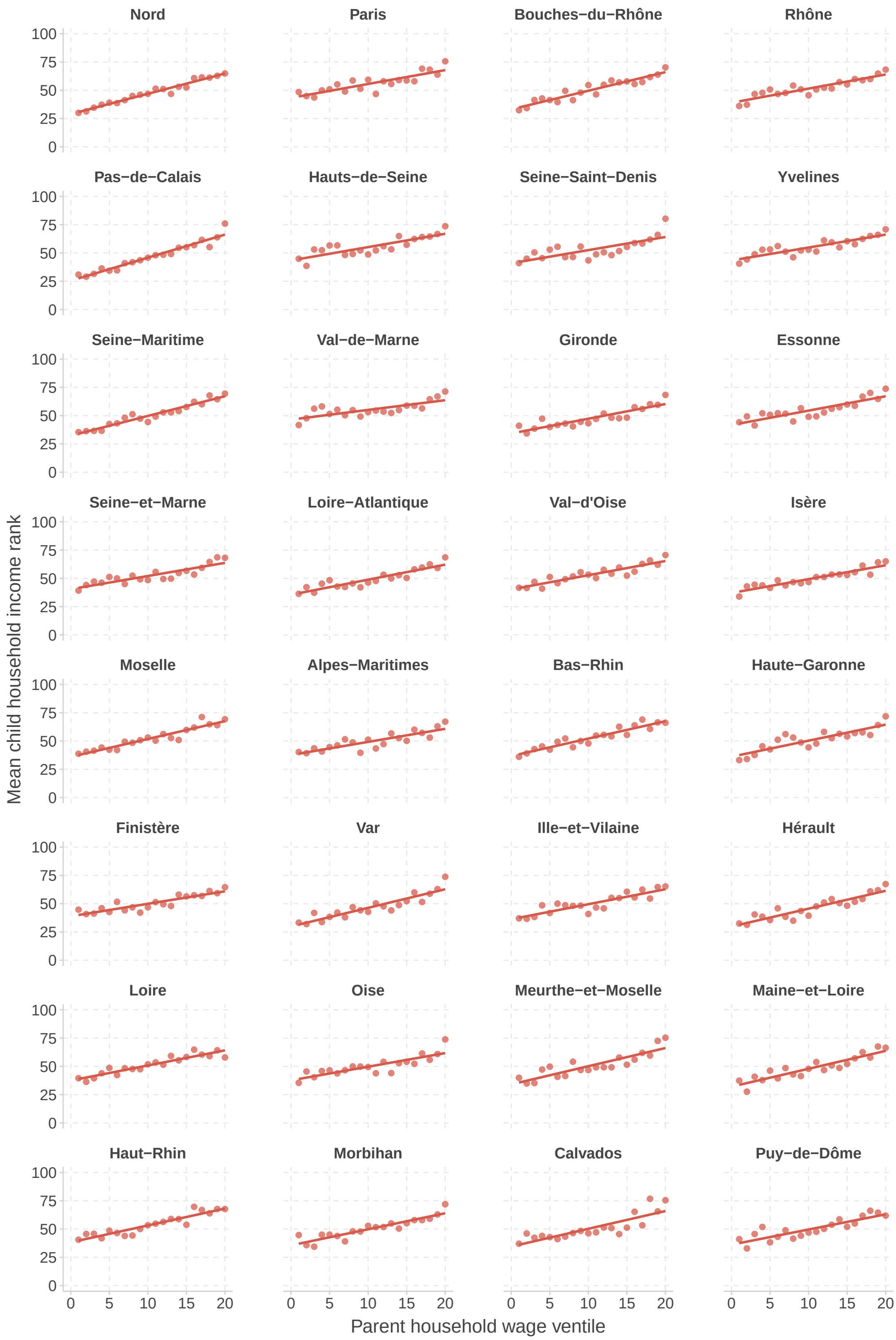
Notes: This figure presents a non-parametric binned scatter plot of the relationship between child income rank and parent income rank for individuals who grew up in the *Nord* department. The dashed line shows the expected income rank, here 38.7 (bootstrapped standard error = 0.54), for children whose parents locate at the 25^{th} percentile. The orange line is a linear regression fit through the conditional expectation. See Figure 1.3's notes for details on data, sample and income definitions.

Figure A.38: Department-level log-log relationships



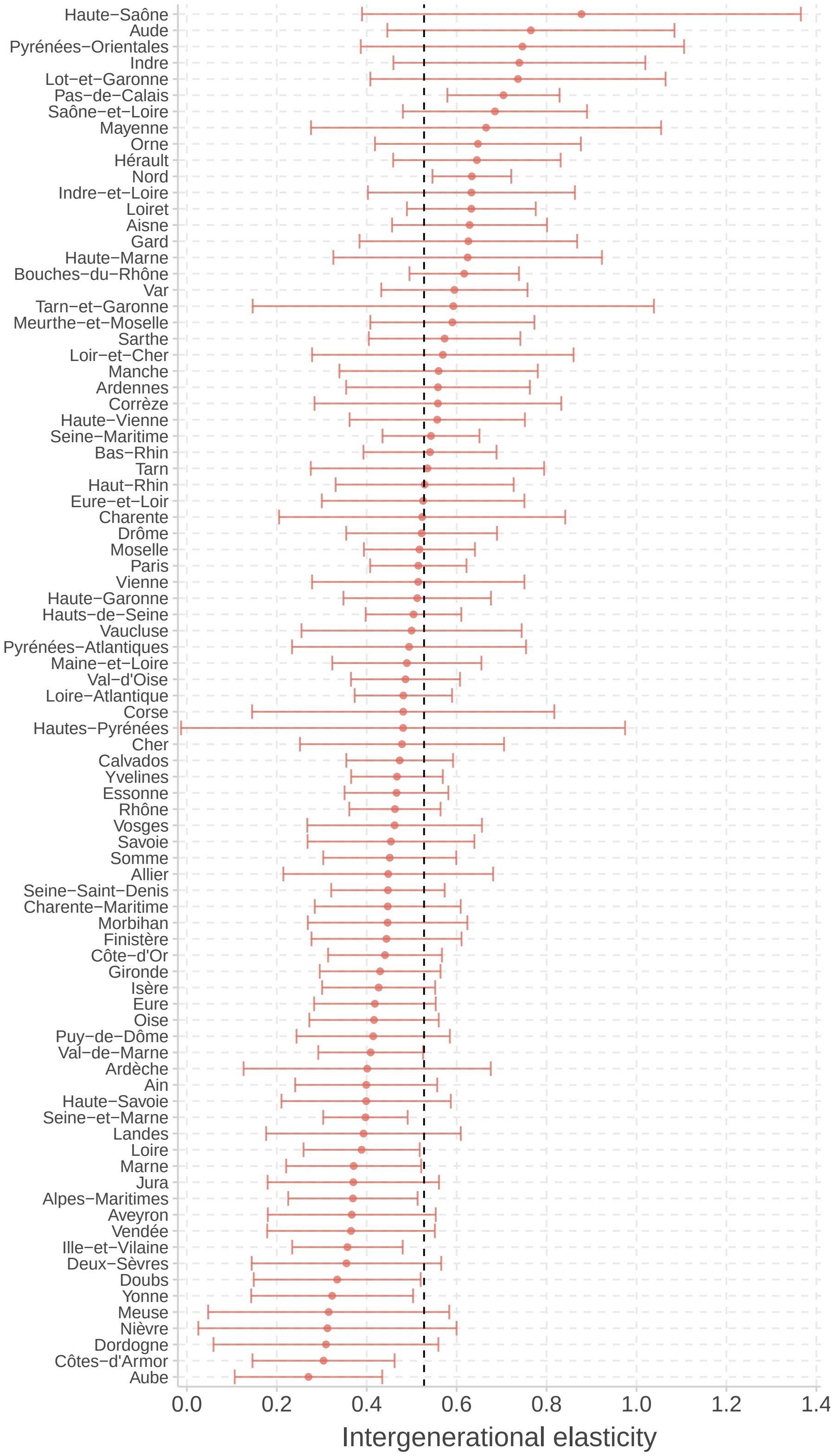
Notes: This figure presents the non-parametric binned scatter plot of the relationship between child log income and parent log income separately for each childhood department. The childhood department is that observed in 1990, i.e., when individuals were between 9 and 18 years old. See Figure 1.3's notes for details on data, sample and income definitions.

Figure A.39: Department-level rank-rank relationships



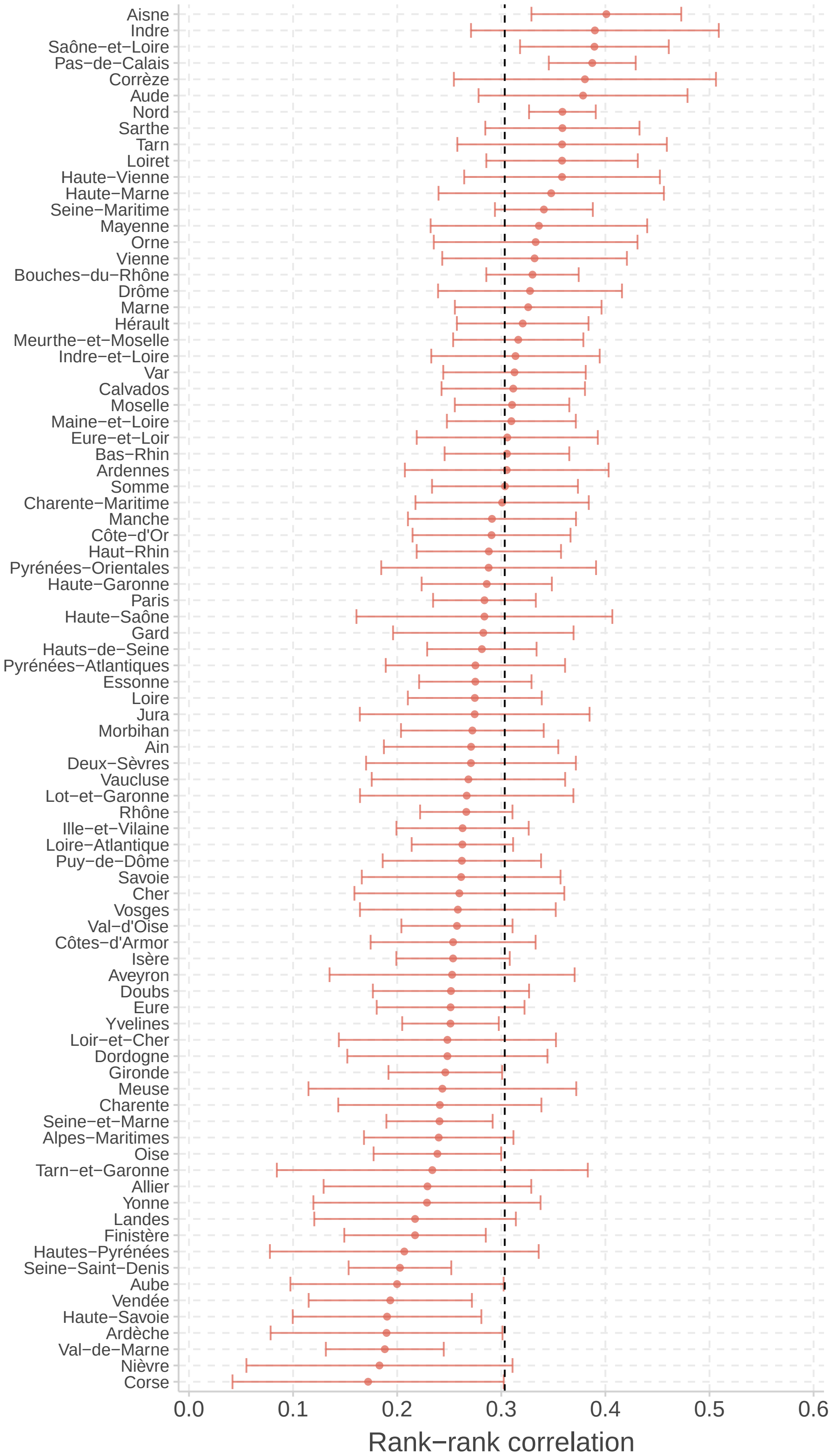
Notes: This figure presents the non-parametric binned scatter plot of the relationship between child income rank and parent income rank separately for each childhood department. The childhood department is that observed in 1990, i.e., when individuals were between 9 and 18 years old. See Figure 1.3's notes for details on data, sample and income definitions.

Figure A.40: Department-level intergenerational elasticities



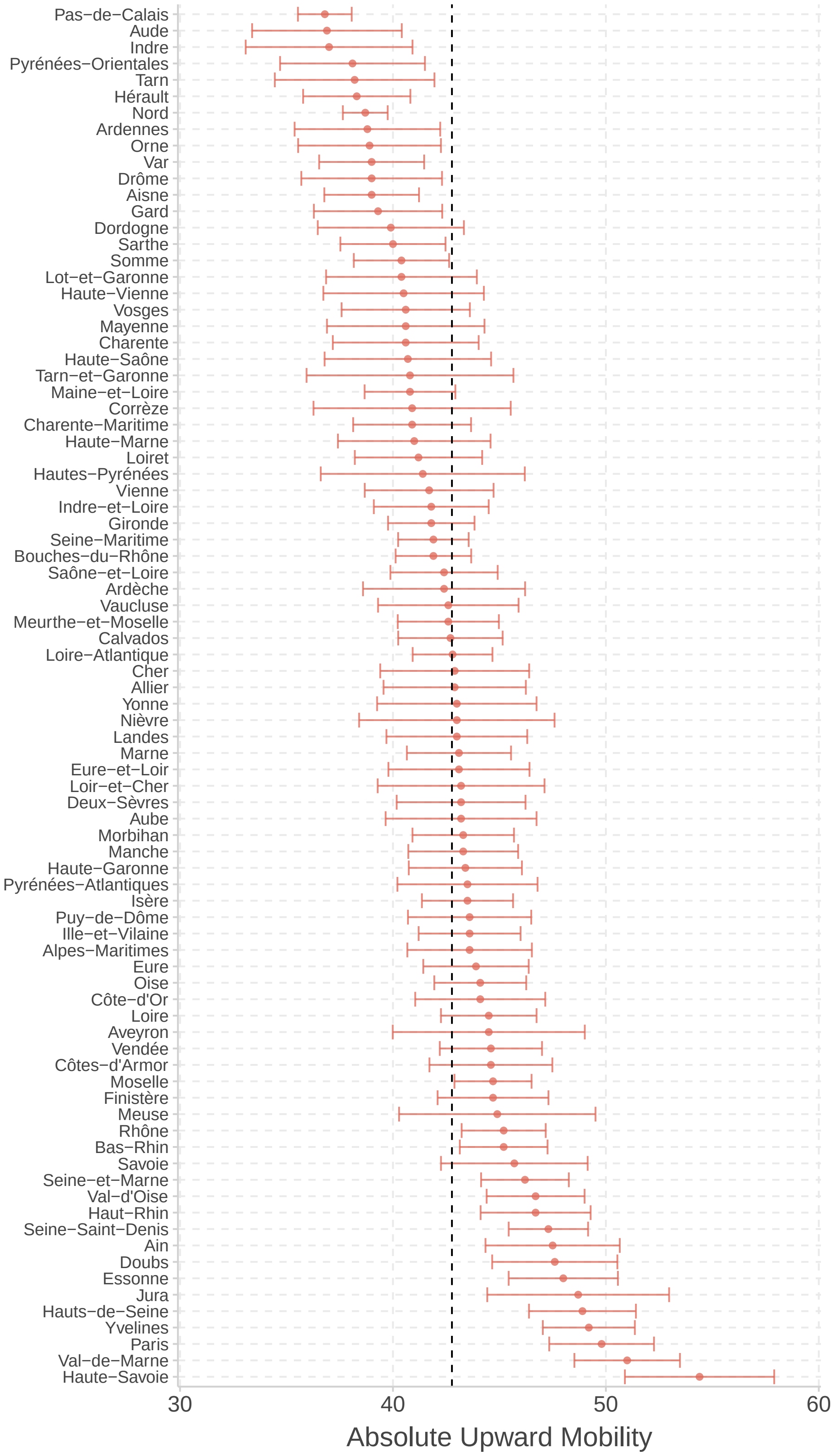
Notes: This figure presents the intergenerational elasticity in household income and its 95% bootstrapped confidence interval, estimated separately for each childhood department with more than 200 observations. The childhood department is that observed in 1990, i.e., when individuals were between 9 and 18 years old. The dashed black line represents the national estimate. See Figure 1.3's notes for details on data, sample and income definitions.

Figure A.41: Department-level rank-rank correlations



Notes: This figure presents the rank-rank correlation in household income and its 95% bootstrapped confidence interval, estimated separately for each childhood department with more than 200 observations. The childhood department is that observed in 1990, i.e., when individuals were between 9 and 18 years old. The dashed black line represents the national estimate. See Figure 1.3's notes for details on data, sample and income definitions.

Figure A.42: Department-level absolute upward mobility



Notes: This figure presents the absolute upward mobility in household income ranks and its 95% bootstrapped confidence interval, estimated separately for each childhood department with more than 200 observations. The childhood department is that observed in 1990, i.e., when individuals were between 9 and 18 years old. The dashed black line represents the national estimate. See Figure 1.3's notes for details on data, sample and income definitions.

Figure A.43: Department-level unemployment rate in 1990

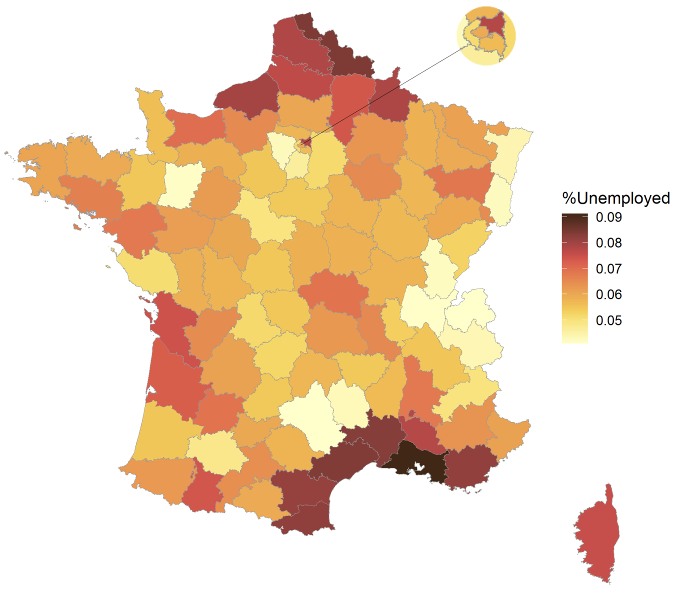


Figure A.44: Geographic mobility by parent household wage rank

Notes: This figure presents the percentage of movers by parent income rank. Movers are defined as individuals whose adulthood department of residence is different from that of their childhood. See Figure 1.3 and 1.10's notes for details on data, sample and income definitions.

Figure A.45: Intergenerational mobility and geographic mobility - Department ranks

Notes: This figure represents the conditional expectation function of child household income rank with respect to parent household wage rank separately for individuals whose adulthood department of residence is different or not from their childhood department of residence. Percentile ranks are computed according to the local department income distribution. Parents are ranked within their department of residence in 1990 while children are ranked within their adulthood department. See Figures 1.3 and 1.10's notes for details on data, sample and income definitions.

A.6 Additional Tables

Table A.12: Child sample construction

Birth Cohort	Born in Metropolitan France	+ Live with parents in 1990 census	+ At least one obs. in tax returns data (each inc. def.)	+ At least one obs. in tax returns data at 35-45	+ No parent in occupation 1 or 31
1972	9,083	7,946	7,515	7,515	7,015
1973	8,647	7,670	7,263	7,263	6,726
1974	8,704	7,713	7,294	7,294	6,758
1975	7,334	6,565	6,230	6,230	5,818
1976	7,762	6,963	6,567	6,547	6,100
1977	7,972	7,175	6,823	6,763	6,319
1978	7,755	7,000	6,691	6,585	6,136
1979	8,473	7,620	7,280	7,102	6,644
1980	8,822	7,965	7,559	7,239	6,774
1981	8,457	7,631	7,267	6,716	6,304
1972-1981	83,009	74,248	70,489	69,254	64,594

Notes: This table displays the number of observation for each child birth year cohort and the entire sample, at each sample restriction. Note that since parent income cannot be predicted for 23 children because one of their parents have an occupation not represented in the sample of synthetic parents of the corresponding gender, the actual sample size on which estimates are computed when using parent household wage as the parent income definition is 64,571 (i.e., 64,594 - 23).

Table A.13: Average characteristics of actual and synthetic parents

Characteristic	Synthetic Parents	Actual Parents
Females	53.42%	52.26%
Age in 1990	41.22%	40.74%
Born French	89.95%	88.36%
1-digit occupation		
1. Farmers	3.72%	3.47%
2. Craftsmen, salespeople, and heads of businesses	6.98%	6.77%
3. Managerial and professional occupations	9.76%	9.35%
4. Intermediate professions	15.48%	15.35%
5. Employees	20.76%	20.39%
6. Blue collar workers	23.19%	24.6%
7. Retirees	1.30%	1.32%
8. Other with no professional activity	18.81%	18.76%
Education		
No diploma	22.45%	23.8%
Primary education	19.38%	18.93%
BEPC	7.99%	8.18%
CAP	20.76%	19.91%
BEP	4.95%	5.00%
High school diploma	11.64%	11.47%
Bachelor or technical degree	6.08%	6.18%
Masters or PhD	6.75%	6.52%
Country of birth		
France	86.18%	84.81%
Maghreb	6.62%	8.03%
Other Africa	0.55%	0.73%
South Europe	3.32%	3.33%
Other Europe	2.33%	2.17%
Rest of the world	1.00%	0.94%
Family structure		
Single fathers	0.93%	0.72%
Single mothers	5.58%	5.25%
Both spouses active	58.73%	58.28%
Mother inactive	31.35%	32.32%
Father inactive	1.38%	1.38%
Both spouses inactive	2.03%	2.06%
Municipality characteristics		
Log population	7.83	7.85
Log density	0.46	0.49
% foreigners	2.31%	2.33%
Unemployment rate	6.22%	6.25%
% single mothers	6.3%	6.4%
N	134,572	140,136

Notes: See Section 1.3.2 for details on construction of samples. These statistics are computed before applying any income observation restrictions.

Table A.14: Share of actual and synthetic parents by 2-digit occupation

2-digit occupation	Synthetic Parents	Actual Parents
Farmers with small farm	0.92%	0.84%
Farmers with medium-sized farm	1.22%	1.19%
Farmers with large farm	1.58%	1.44%
Craftsmen	3.62%	3.57%
Trade workers and related	2.62%	2.50%
Heads of company with $\geq$ 10 employees	0.73%	0.70%
Liberal profession	1.38%	1.32%
Public sector executives	1.07%	1.05%
Professors and scientific professions	2.12%	1.97%
Information, arts, and entertainment professions	0.32%	0.31%
Administrative executives and sales representatives	2.72%	2.66%
Engineers, technical executives	2.16%	2.05%
Teachers and related	2.64%	2.57%
Intermediate health and social work professions	2.48%	2.62%
Clerk, religious	0.01%	0.01%
Intermediate administrative professions of the public sector	1.54%	1.41%
Intermediate administrative professions and salesmen	4.06%	4.03%
Technicians	2.30%	2.29%
Foremen, supervisors	2.44%	2.42%
Civil servants	6.74%	6.69%
Police and military officers	1.27%	1.35%
Company administrative employees	6.92%	6.70%
Trade employees	2.24%	2.16%
Personal service workers	3.58%	3.49%
Skilled industrial workers	5.82%	6.14%
Skilled crafts workers	4.60%	4.83%
Drivers	2.19%	2.39%
Skilled handling, storing and transport workers	1.41%	1.47%
Unskilled industrial workers	6.19%	6.67%
Unskilled crafts workers	2.32%	2.42%
Agricultural workers	0.66%	0.69%
Former farmers	0.09%	0.07%
Former craftsmen, salespeople, and heads of businesses	0.10%	0.08%
Former managerial and professional occupation	0.09%	0.10%
Former intermediate professions	0.19%	0.17%
Former employees	0.33%	0.30%
Former blue collar workers	0.51%	0.60%
Unemployed who have never worked	0.36%	0.38%
Military contingent	0.00%	0.00%
Students $\geq$ 15 yrs old	0.10%	0.04%
Other inactive $\leq$ 60 yrs old	18.24%	18.20%
Other inactive $\geq$ 60 yrs old	0.10%	0.12%
N	$134,572$	$140,136$

Notes: See Table A.13's notes for sample construction.

Table A.15: Synthetic parents sample construction

Gender	At least one child born in Metrop. France 1972-1981	+ Observed in 1990 Census	+ Born even year	+ At least two obs. at 35-45 in All Employee Panel	+ Not in occupation 1 or 31
Fathers	49,746	43,851	22,227	16,699	16,450
Mothers	52,904	48,097	24,297	15,104	14,973
All	102,650	91,948	46,524	31,803	31,423

Table A.16: Number of observations by child and parent income definitions

Child income definition	Parent income definition	Number of observations	0 child incomes (N.)	0 child incomes (%)	Negative child incomes (N.)	Negative child incomes (%)
Household income	Family income	64,571	0	0	41	0.06
Household income	Father income	57,902	0	0	35	0.06
Household wage	Family income	64,571	1976	3.06	0	0
Household wage	Father income	57,902	1690	2.92	0	0
Individual income	Family income	64,571	2479	3.84	68	0.11
Individual income	Father income	57,902	2162	3.73	60	0.1
Labor income	Family income	64,571	4990	7.73	0	0
Labor income	Father income	57,902	4376	7.56	0	0

Table A.17: Descriptive statistics

	N	Missing (%)	Mean	Std. Dev.	25^{th} pctile	Median	75^{th} pctile
Synthetic Parents							
Synthetic father income (35-45 yrs old)	16,450	0	25,902	17,265	16,251	21,966	30,427
Number of syn. father income observations	16,450	0	7.66	2.42	6	8	9
Synthetic mother income (35-45 yrs old)	14,973	0	15,167	10,143	7,496	14,140	21,027
Number of syn. mother income observations	14,973	0	6.95	2.84	5	7	9
Parents							
Fraction single parents in 1990	11.72%						
Fraction female among single parents	88.3%						
Father age at child's birth	64,594	10.35	28.48	6.08	24	28	31
Mother age at child's birth	64,594	1.37	25.89	5.15	22	25	29
Father age in 1990	64,594	10.35	41.98	6.61	38	41	45
Mother age in 1990	64,594	1.37	39.42	5.81	35	39	43
Children							
Household income (average 2010-16)	64,594	0	46,599	38,371	27,696	41,417	56,481
Household wage (average 2010-16)	64,594	0	38,460	30,184	20,812	35,205	50,096
Individual income (average 2010-16)	64,594	0	23,512	20,471	14,375	21,159	28,737
Labor income (average 2010-16)	64,594	0	21092	19,120	10,067	19,877	27,487
Fraction female	49.77%						

Notes: See Sections 1.3.2 and 1.3.3 for details on sample construction and income definitions.

Table A.18: Comparison with existing father-son IGE estimates for France

	Intergenerational Elasticity	First-Stage Instruments	Data	Income Definitions	Child Age
Lefranc and Trannoy (2005)	0.4-0.438[1]	Education (8 cat.) + occupation (7 cat.)	FQP	labor earnings (excl. UI)[2]	30-40
Lefranc (2018)	0.577[3]	Education (6 cat.)	FQP	labor earnings (excl. UI)[2]	28-32
EqualChances.org	0.357	Education (3 cat.) + occupation (9 cat.)	Synthetic fathers: ECHP Sons: EU-SILC	net personal employee income	-
Our estimate	**0.443**				

Notes: FQP = Formation-Qualification-Profession; ECHP = European Community Household Panel; EU-SILC = European Union Statistics on Income and Living Conditions

[1] Estimates taken from Table I, Panel A, cols. (1)-(4), p.65.
[2] Only salaried workers.
[3] Estimates taken from Table 2, 1971-75, col. (2), p.823.

Table A.19: Department-level MSEs and measures of intergenerational income mobility

	IGE	RRC	AUM
First-stage MSE	-0.160	-0.088	1.400
	(0.127)	(0.056)	(3.487)
Constant	0.565^{***}	0.318^{***}	42.370^{***}
	(0.053)	(0.024)	(1.465)
Observations	85	85	85
R^2	0.019	0.029	0.002

Notes: Standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A.20: Department-level intergenerational mobility estimates

	Department	Observations	IGE	RRC	AUM
01	Ain	535	0.4	0.27	47.5
02	Aisne	735	0.63	0.4	39
03	Allier	365	0.45	0.23	42.9
04	Alpes-de-Haute-Provence	141	*	*	*
05	Hautes-Alpes	112	*	*	*
06	Alpse-Maritimes	773	0.37	0.24	43.6
07	Ardèche	313	0.4	0.19	42.4
08	Ardennes	376	0.56	0.31	38.8
09	Ariège	121	*	*	*
10	Aube	361	0.27	0.2	43.2
11	Aude	274	0.77	0.38	36.9
12	Aveyron	243	0.37	0.25	44.5
13	Bouches-du-Rhône	1,795	0.62	0.33	41.9
14	Calvados	781	0.47	0.31	42.7
15	Cantal	164	*	*	*
16	Charente	374	0.52	0.24	40.6
17	Charente-Maritime	559	0.45	0.3	40.9
18	Cher	370	0.48	0.26	42.9
19	Corrèze	219	0.56	0.38	40.9
20	Corse	236	0.48	0.17	45.6
21	Côte-d'Or	549	0.44	0.29	44.1
22	Côtes-d'Armor	590	0.3	0.25	44.6
23	Creuse	102	*	*	*
24	Dordogne	337	0.31	0.25	39.9
25	Doubs	635	0.33	0.25	47.6
26	Drôme	435	0.52	0.33	39
27	Eure	738	0.42	0.25	43.9
28	Eure-et-Loire	505	0.53	0.31	43.1
29	Finistère	979	0.44	0.22	44.7
30	Gard	577	0.63	0.28	39.3
31	Haute-Garonne	949	0.51	0.29	43.4
32	Gers	136	*	*	*
33	Gironde	1,304	0.43	0.25	41.8
34	Hérault	788	0.65	0.32	38.3
35	Ille-et-Vilaine	1,036	0.36	0.26	43.6
36	Indre	235	0.74	0.39	37
37	Indre-et-Loire	597	0.63	0.31	41.8
38	Isère	1,217	0.43	0.25	43.5
39	Jura	269	0.37	0.27	48.7
40	Landes	326	0.39	0.22	43
41	Loir-et-Cher	357	0.57	0.25	43.2
42	Loire	901	0.39	0.27	44.5
43	Haute-Loire	194	*	*	*
44	Loire-Atlantique	1,467	0.48	0.26	42.8

Notes: * Insufficient number of observations (< 200).

Table A.20: Department-level intergenerational mobility estimates *(continued)*

	Department	Observations	IGE	RRC	AUM
45	Loiret	706	0.63	0.36	41.2
46	Lot	137	*	*	*
47	Lot-et-Garonne	319	0.74	0.27	40.4
48	Lozère	63	*	*	*
49	Maine-et-Loire	931	0.49	0.31	40.8
50	Manche	566	0.56	0.29	43.3
51	Marne	676	0.37	0.33	43.1
52	Haute-Marne	263	0.62	0.35	41
53	Mayenne	329	0.67	0.34	40.6
54	Meurthe-et-Moselle	862	0.59	0.32	42.6
55	Meuse	238	0.32	0.24	44.9
56	Morbihan	778	0.45	0.27	43.3
57	Moselle	1,274	0.52	0.31	44.7
58	Nièvre	251	0.31	0.18	43
59	Nord	3,668	0.63	0.36	38.7
60	Oise	1,008	0.42	0.24	44.1
61	Orne	357	0.65	0.33	38.9
62	Pas-de-Calais	2,145	0.7	0.39	36.8
63	Puy-de-Dôme	664	0.41	0.26	43.6
64	Pyrénées-Atlantiques	571	0.49	0.28	43.5
65	Hautes-Pyrénées	209	0.48	0.21	41.4
66	Pyrénées-Orientales	356	0.75	0.29	38.1
67	Bas-Rhin	1,033	0.54	0.31	45.2
68	Haut-Rhin	792	0.53	0.29	46.7
69	Rhône	1,583	0.46	0.27	45.2
70	Haute-Saône	273	0.88	0.28	40.7
71	Saône-et-Loire	661	0.69	0.39	42.4
72	Sarthe	635	0.57	0.36	40
73	Savoie	430	0.45	0.26	45.7
74	Haute-Savoie	629	0.4	0.19	54.4
75	Paris	1,352	0.51	0.28	49.8
76	Seine-Maritime	1,547	0.54	0.34	41.9
77	Seine-et-Marne	1,529	0.4	0.24	46.2
78	Yvelines	1,645	0.47	0.25	49.2
79	Deux-Sèvres	376	0.35	0.27	43.2
80	Somme	737	0.45	0.3	40.4
81	Tarn	354	0.54	0.36	38.2
82	Tarn-et-Garonne	202	0.59	0.23	40.8
83	Var	773	0.59	0.31	39
84	Vaucluse	468	0.5	0.27	42.6
85	Vendée	627	0.37	0.19	44.6
86	Vienne	464	0.51	0.33	41.7
87	Haute-Vienne	357	0.56	0.36	40.5
88	Vosges	504	0.46	0.26	40.6

Notes: * Insufficient number of observations (< 200).

Table A.20: Department-level intergenerational mobility estimates *(continued)*

	Department	Observations	IGE	RRC	AUM
89	Yonne	388	0.32	0.23	43
90	Territoire de Belfort	172	*	*	*
91	Essonne	1,302	0.47	0.28	48
92	Hauts-de-Seine	1,248	0.5	0.28	48.9
93	Seine-Saint-Denis	1,495	0.45	0.2	47.3
94	Val-de-Marne	1,188	0.41	0.19	51
95	Val-d'Oise	1,366	0.49	0.26	46.7

Notes: * Insufficient number of observations (< 200).

Table A.21: Correlation between department-level intergenerational mobility measures

Child income definition	IGE-RRC	RRC-AUM	IGE-AUM
Household income	0.65	−0.57	−0.55
Individual income	0.72	−0.55	−0.45
Individual wage	0.70	−0.41	−0.26

Notes: See Figure 1.8 for corresponding maps.

Table A.22: Intergenerational & geographic mobility - Department ranks

	Dependent variable: Child household income rank				
	(1)	(2)	(3)	(4)	(5)
Parents income rank	0.259***	0.259***	0.258***	0.162***	0.135***
	(0.005)	(0.005)	(0.005)	(0.007)	(0.012)
Mover ($\hat{\gamma}$)	4.572***	4.591***	4.897***	4.926***	4.883***
	(0.472)	(0.471)	(0.478)	(0.477)	(0.478)
Parents income rank × Mover ($\hat{\delta}$)	−0.014*	−0.014*	−0.016**	−0.026***	−0.027***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Constant	36.401***	36.137***	35.574***	26.815***	28.162***
	(0.265)	(0.279)	(1.125)	(1.570)	(1.620)
Birth cohort	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓
Department FE			✓	✓	✓
Parents' education				✓	✓
Parents' 2-digit occupation					✓
$\mathbb{E}[\hat{\gamma} + \hat{\delta} p_{p,i}] = \hat{\gamma} + \hat{\delta} \times 50.5$	3.87	3.88	4.09	3.61	3.52
$\mathbb{E}[\hat{\gamma} + \hat{\delta} p_p \mid p_p = 25]$	4.22	4.24	4.50	4.28	4.21
$\mathbb{E}[\hat{\gamma} + \hat{\delta} p_p \mid p_p = 75]$	3.52	3.54	3.70	2.98	2.86
Observations	64,571	64,571	64,571	64,571	64,571
Adjusted R^2	0.074	0.074	0.077	0.089	0.095

Notes: Bootstrapped standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A.23: Intergenerational mobility and income level in the destination department

	Dependent variable: Child household income rank				
	(1)	(2)	(3)	(4)	(5)
Parents income rank	0.278***	0.278***	0.271***	0.171***	0.153***
	(0.005)	(0.005)	(0.005)	(0.008)	(0.017)
Destination department (ref.: stayers)					
Low-income	0.902	0.923	1.046*	0.852	0.685
	(0.618)	(0.618)	(0.616)	(0.616)	(0.616)
Medium-income	11.355***	11.373***	10.846***	11.045***	11.027***
	(0.951)	(0.952)	(0.945)	(0.948)	(0.950)
High-income	18.819***	18.839***	18.265***	18.465***	18.567***
	(1.224)	(1.224)	(1.247)	(1.260)	(1.258)
Parents income rank × Low-income	−0.019*	−0.019*	−0.017	−0.020*	−0.018
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Parents income rank × Medium-income inc	−0.042***	−0.042***	−0.038***	−0.051***	−0.052***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Parents income rank × High-income	−0.035**	−0.035**	−0.035**	−0.054***	−0.058***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Constant	34.143***	33.860***	37.460***	28.392***	29.369***
	(0.261)	(0.277)	(1.213)	(1.655)	(1.779)
Birth cohort	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓
Department FE			✓	✓	✓
Parents' education				✓	✓
Parents' 2-digit occupation					✓
Observations	64,571	64,571	64,571	64,571	64,571
Adjusted R^2	0.118	0.118	0.124	0.135	0.142

Notes: Bootstrapped standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

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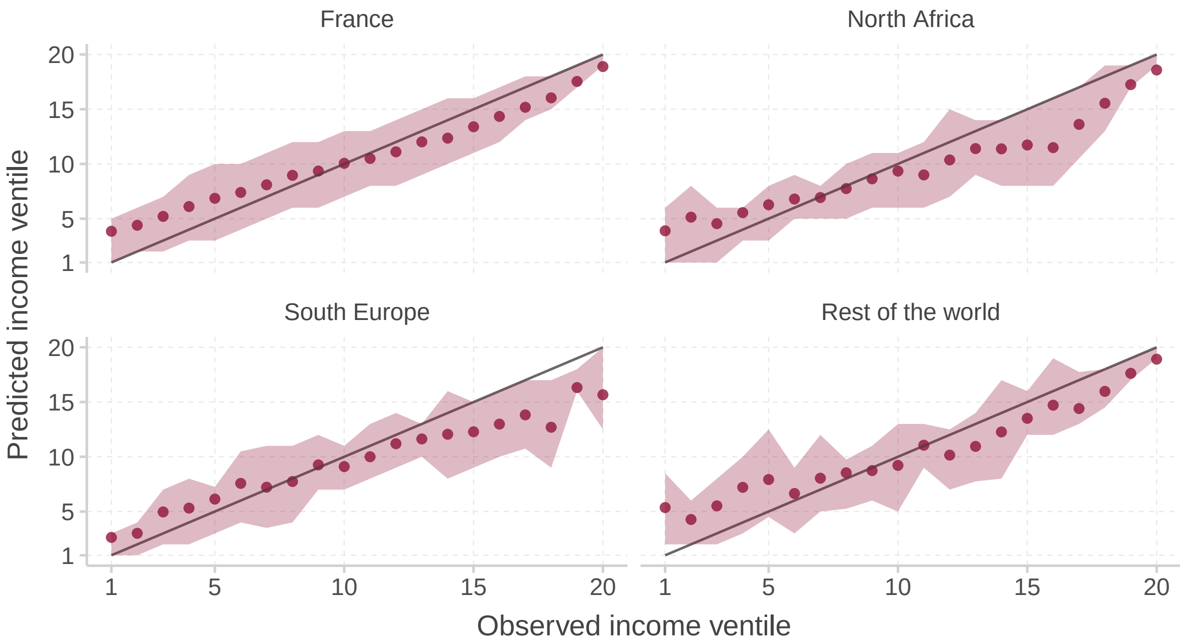
Appendix B

Intergenerational mobility among children of immigrants and natives: The role of residential segregation

B.1 Additional Figures

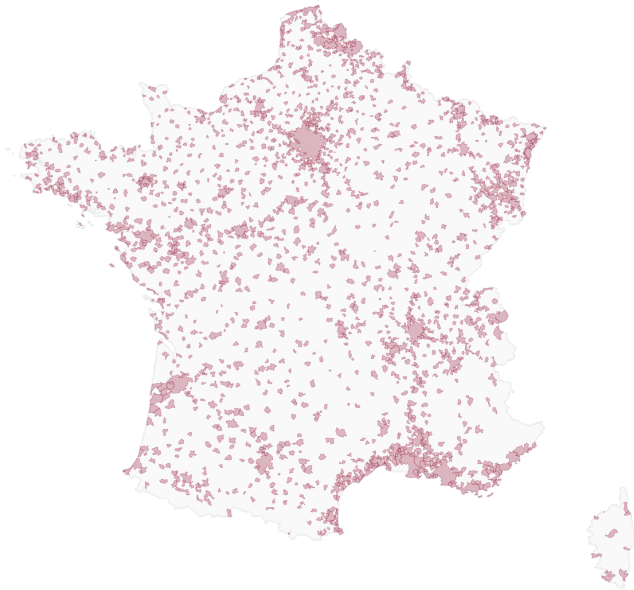
Figure B.1: Out-of-sample parents' income ventile prediction



Notes: 5-fold out of sample prediction. Q1 - mean - Q3.

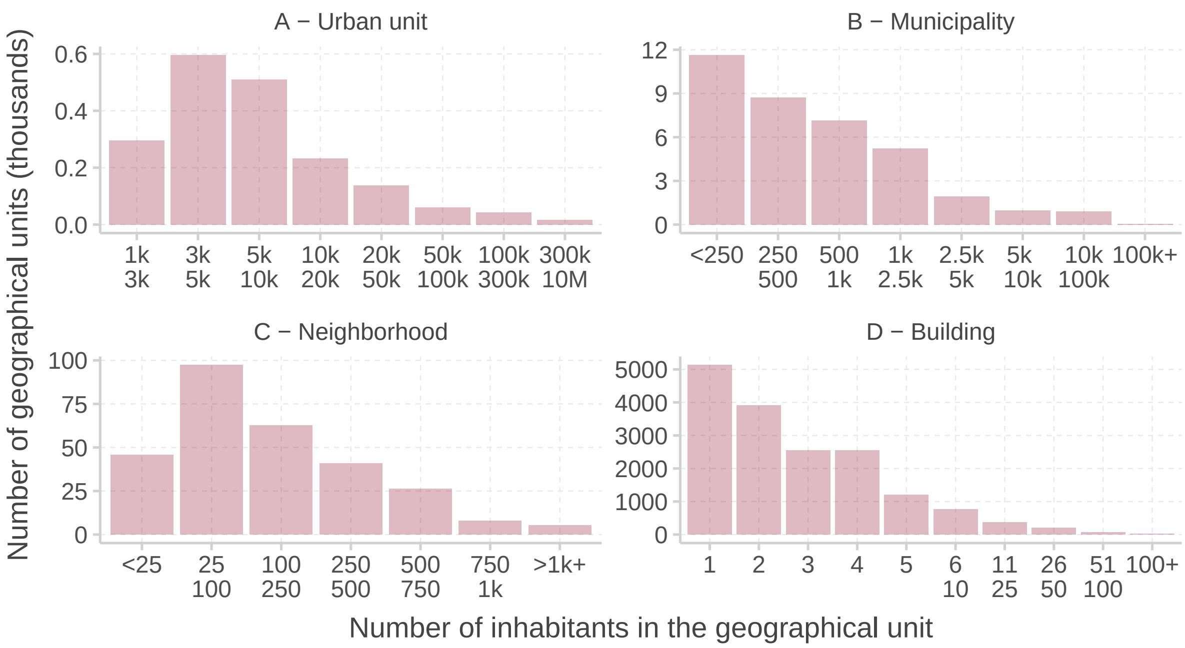
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.2: Urban units



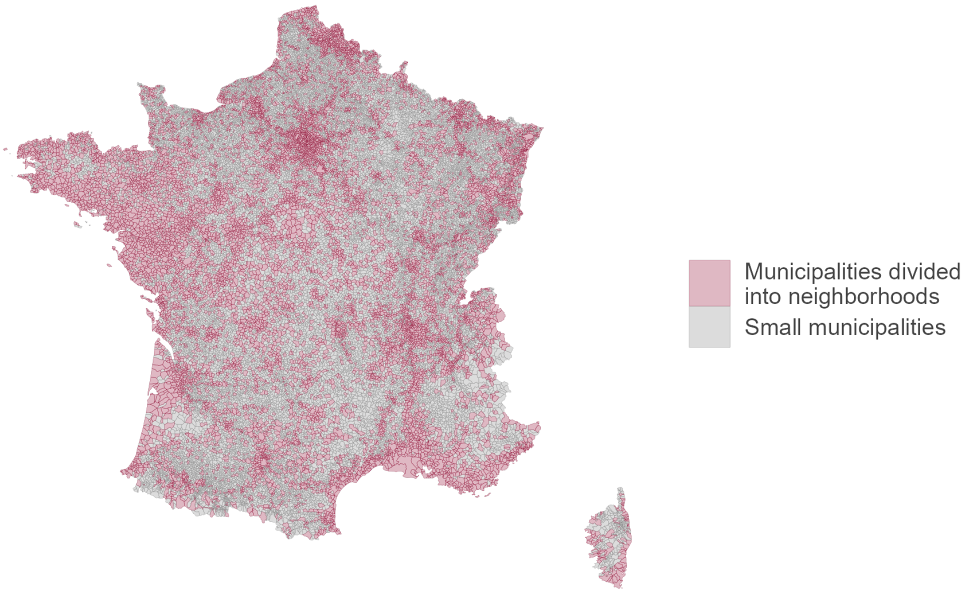
Source: *Full Population Census*, main sample, wave 1990, INSEE, and *GEOFLA*®, wave 1997, IGN.

Figure B.3: Distributions of inhabitants across geographical units



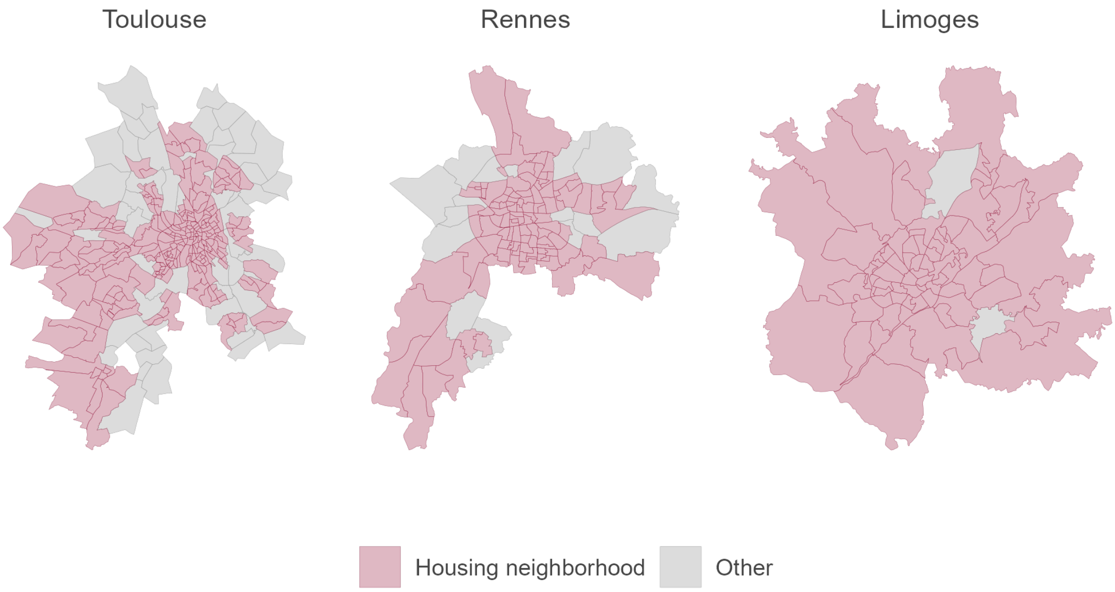
Source: *Full Population Census*, main sample, wave 1990, INSEE

Figure B.4: Municipalities divided into IRIS neighborhoods



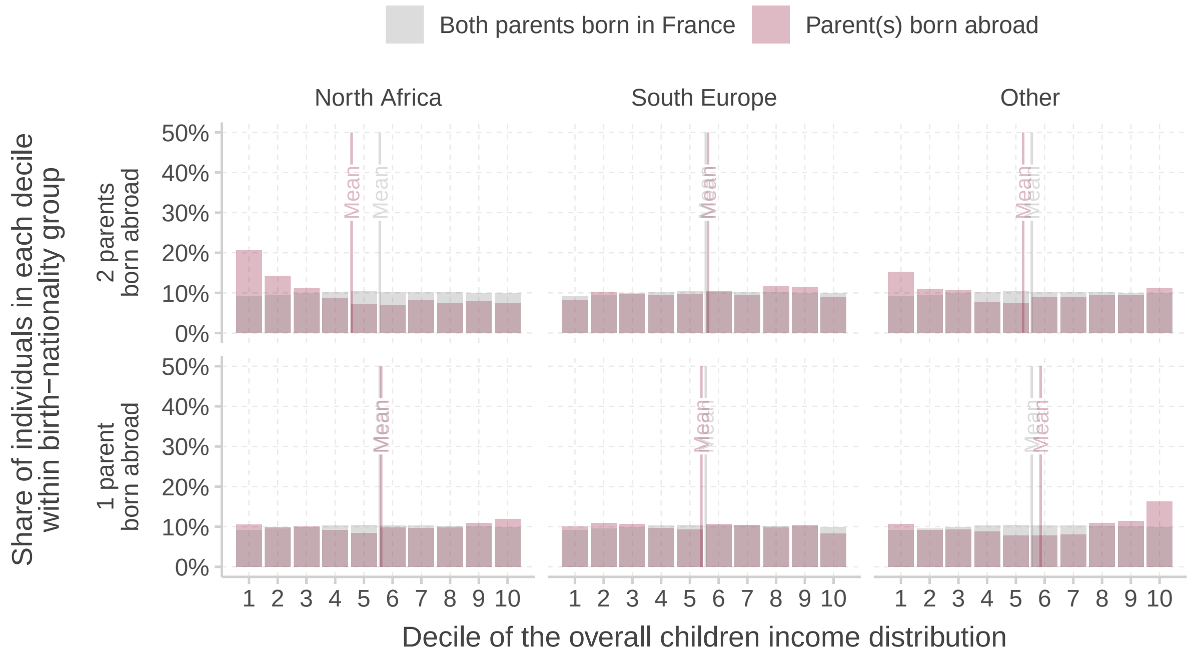
Source: *Full Population Census*, main sample, wave 1990, INSEE, and *GEOFLA®*, wave 1997, IGN.

Figure B.5: IRIS neighborhoods within urban units



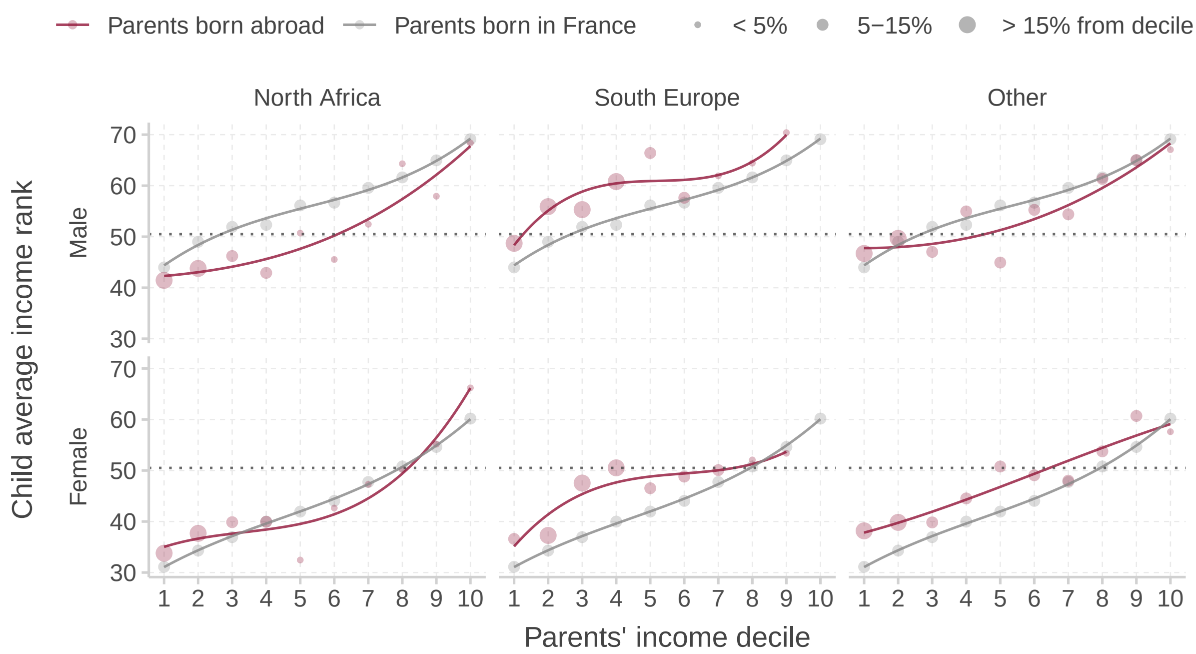
Source: *GEOFLA®*, wave 1997, IGN, and *CONTOURS... IRIS®*, wave 2009, IGN-INSEE.

Figure B.6: Positions in the national income distribution across origins



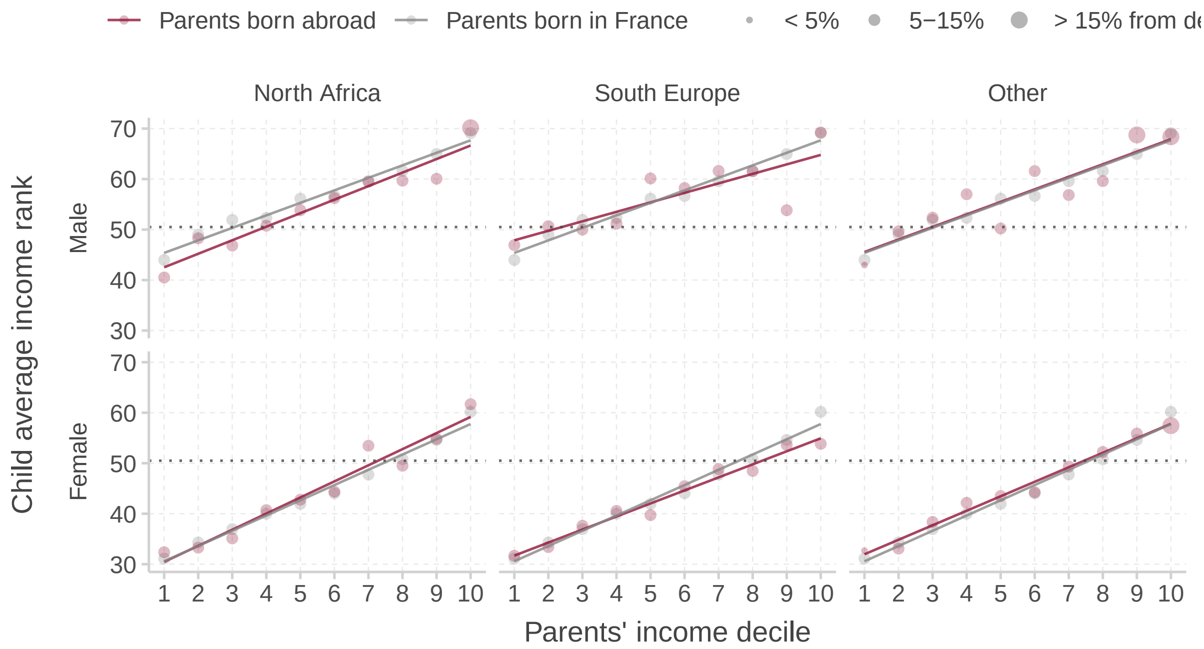
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.7: Average income rank across parents' income deciles - 3rd-order polynomial fit



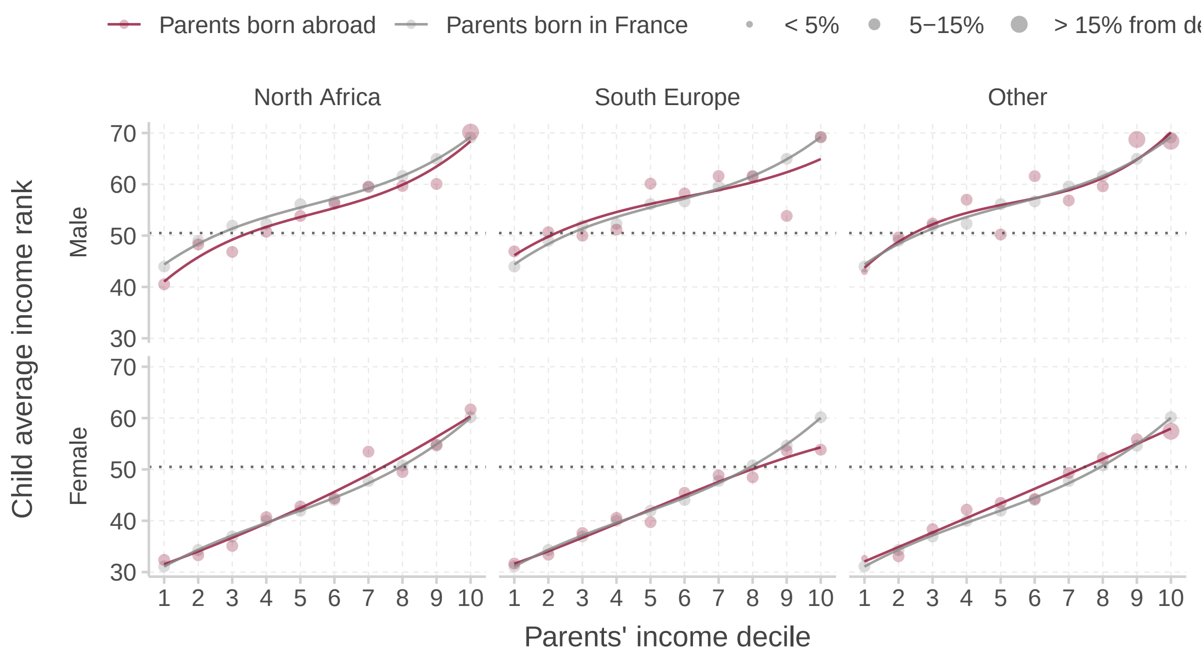
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.8: Average income rank across parents' income deciles - Mixed couple parents



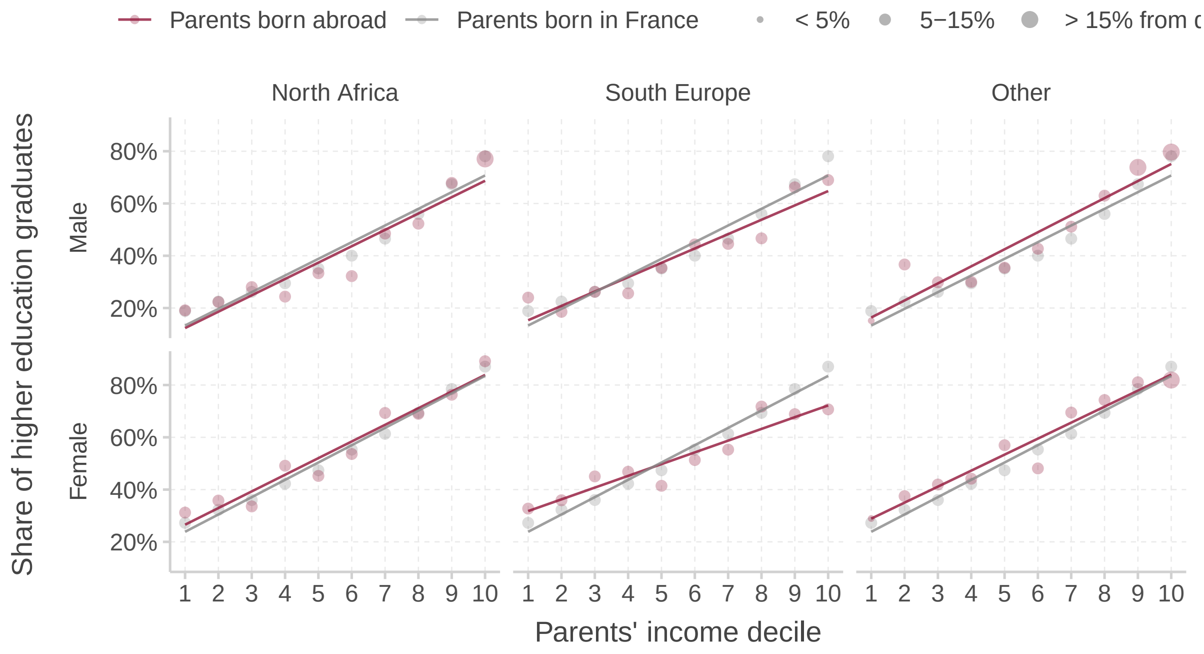
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.9: Average income rank across parents' income deciles - Mixed couple parents - 3rd-order polynomial fit



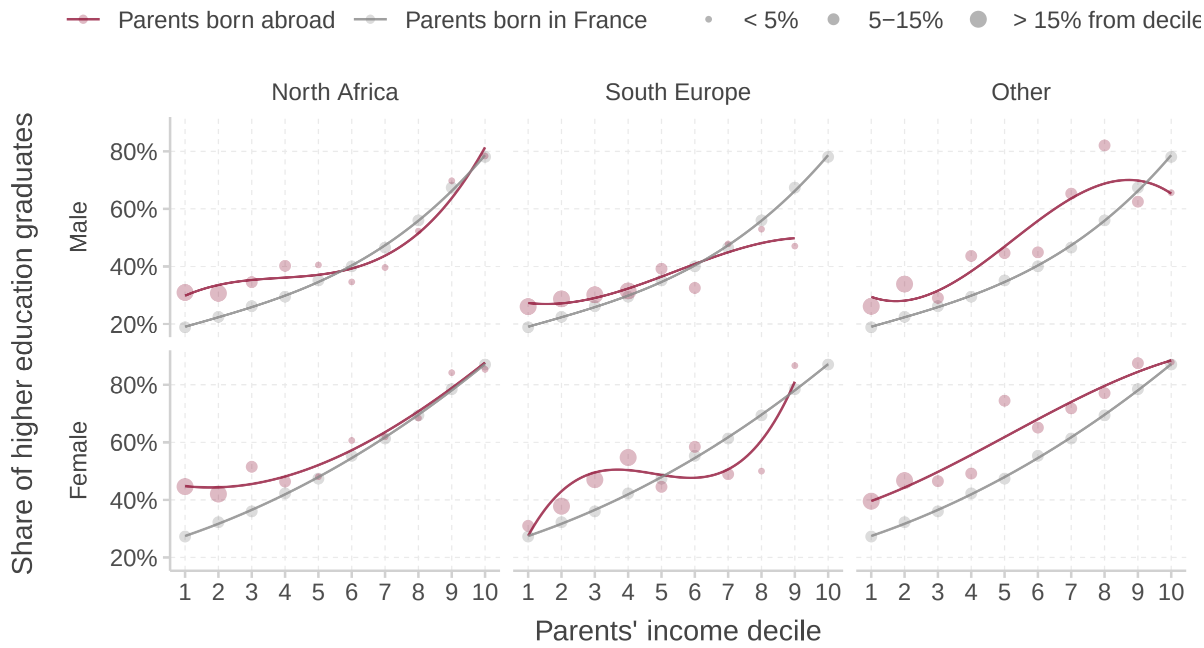
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.10: Higher education graduation rate across parents' income deciles - Mixed couple parents



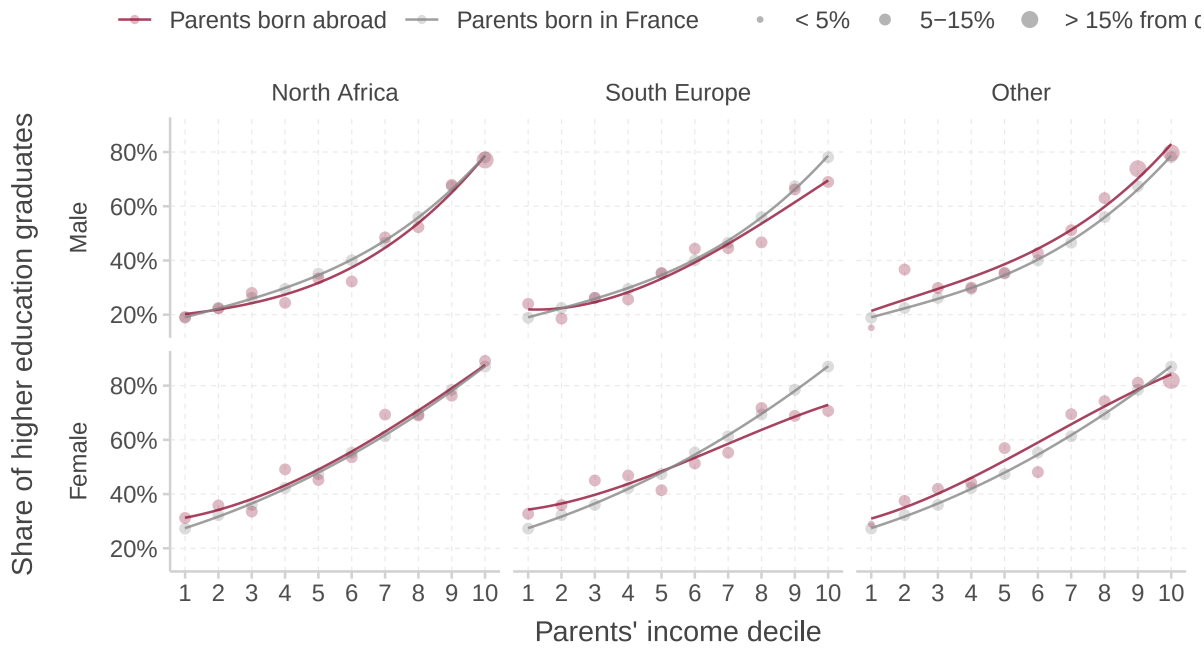
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.11: Higher education graduation rate across parents' income deciles - Polynomial fit



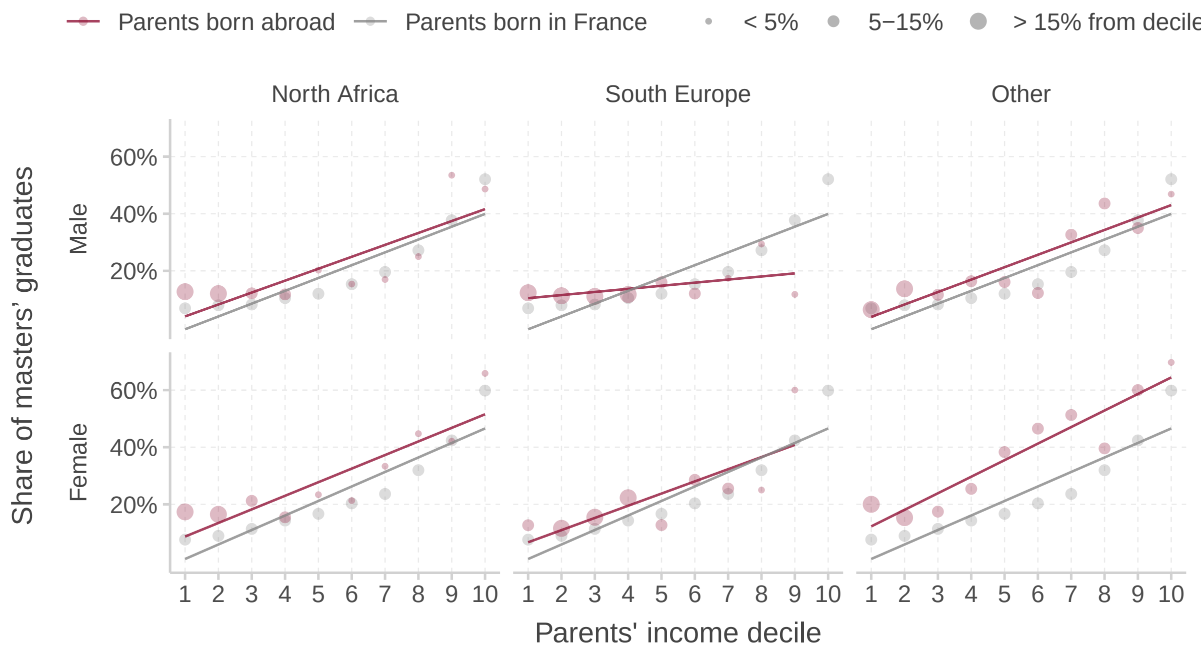
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.12: Higher education graduation rate across parents' income deciles - Mixed couple parents - Polynomial fit



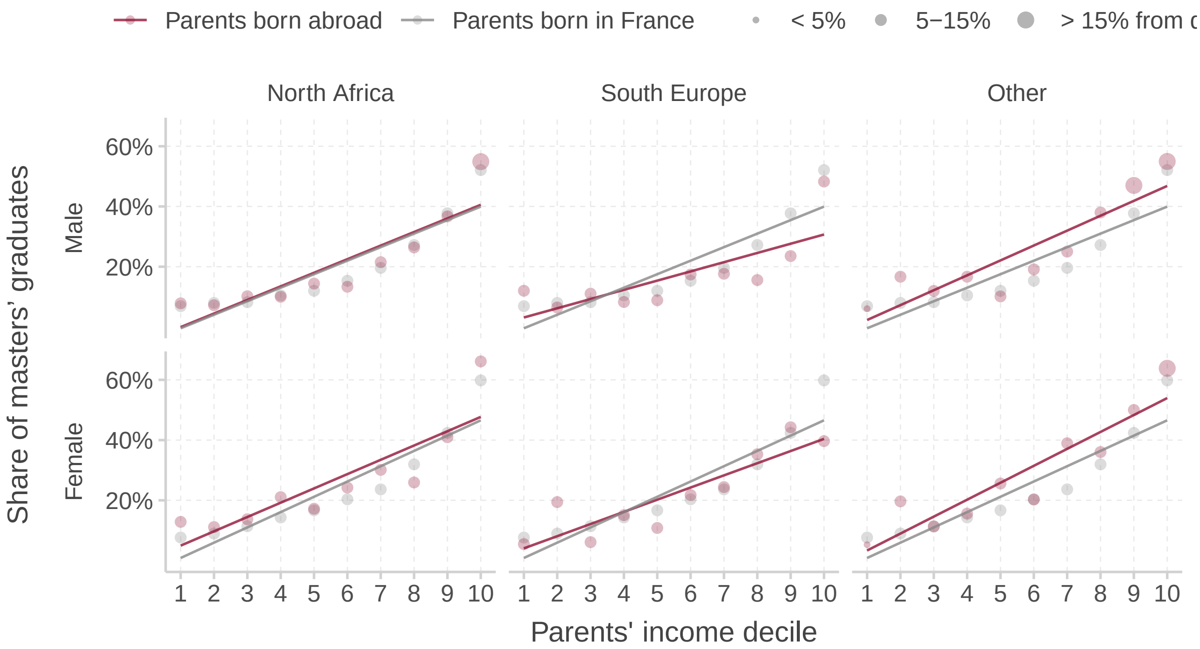
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.13: Masters' graduation rate across parents' income deciles



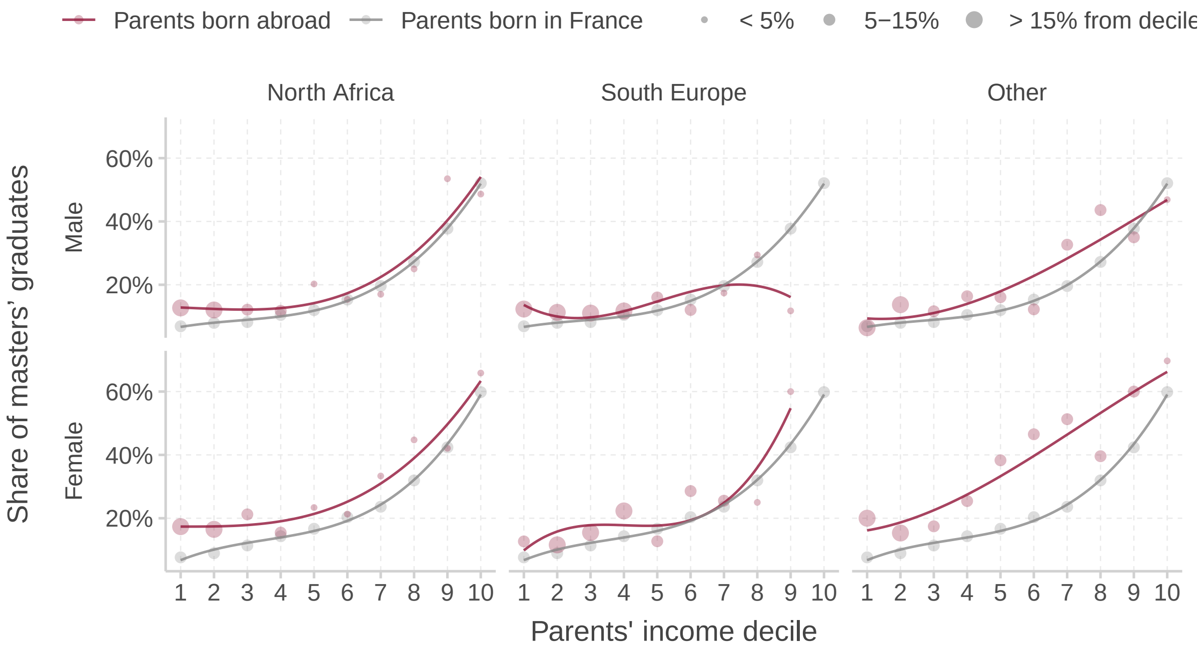
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.14: Masters' graduation rate across parents' income deciles - Mixed couple parents



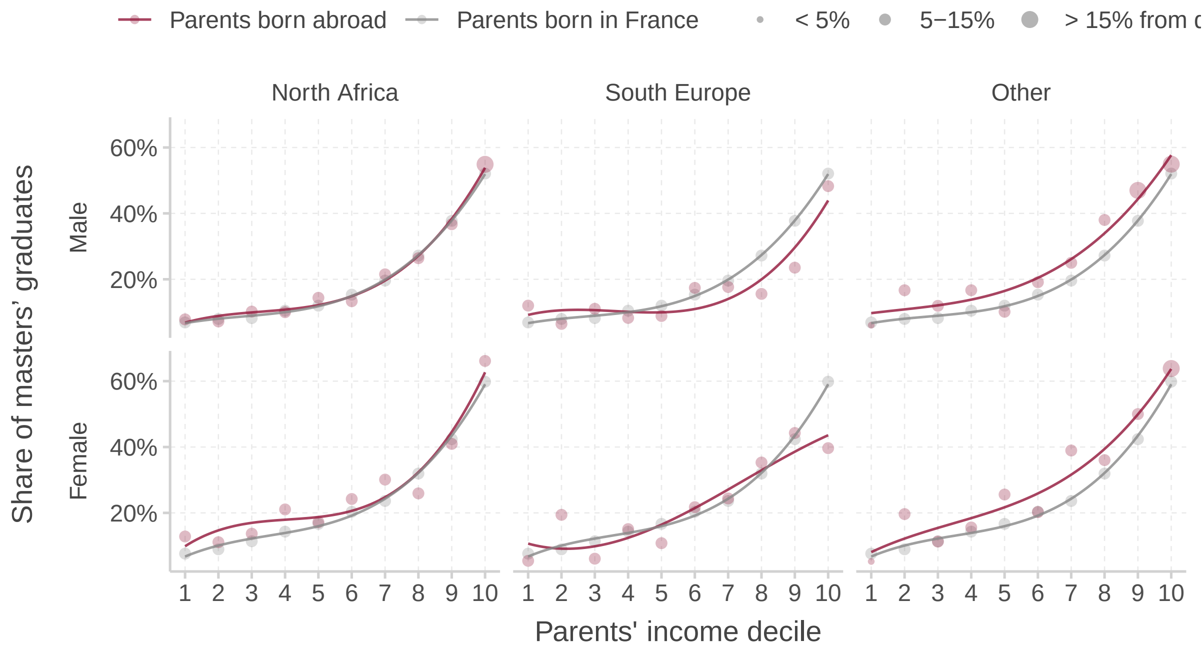
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.15: Masters' graduation rate across parents' income deciles - Polynomial fit



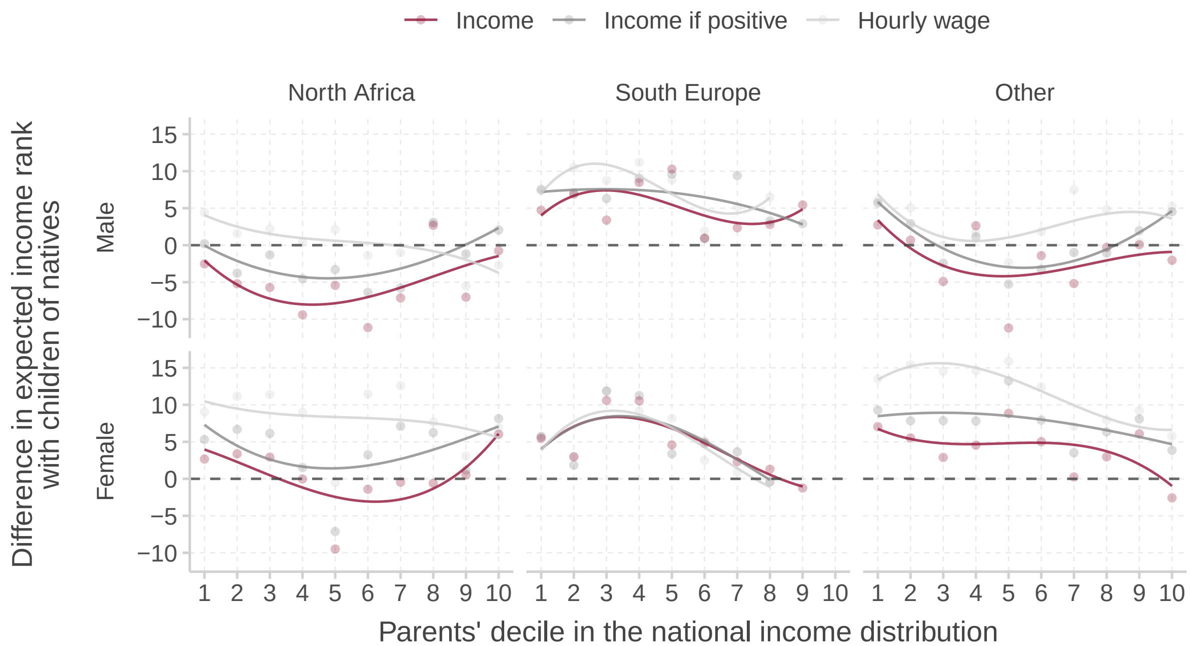
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.16: Masters' graduation rate across parents' income deciles - Mixed couple parents - Polynomial fit



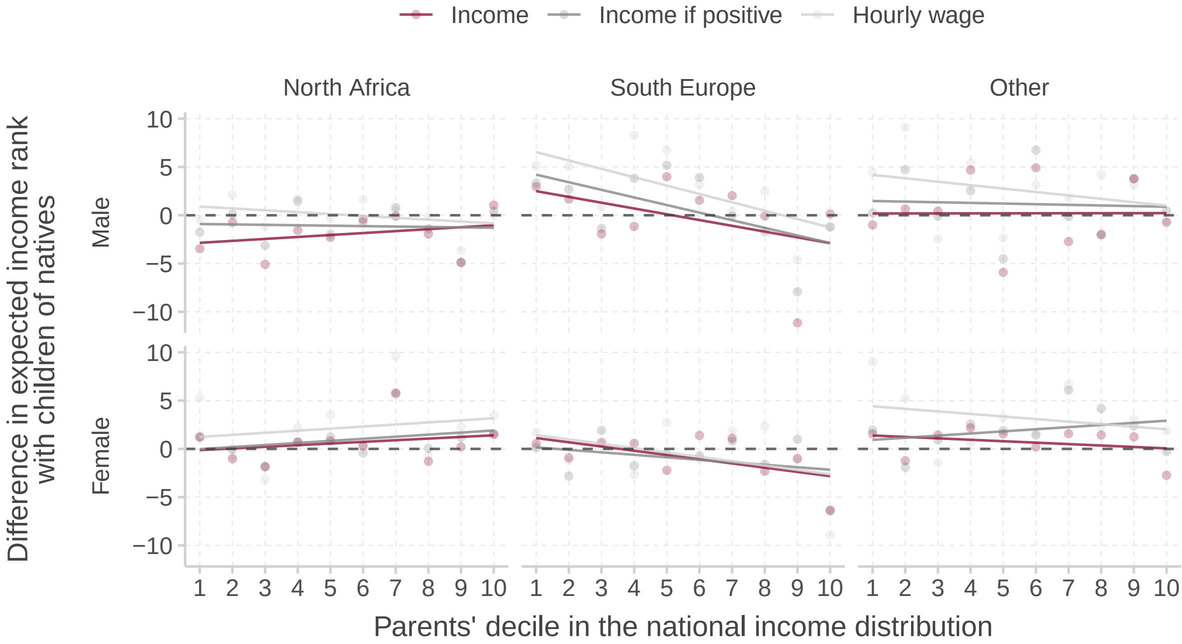
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.17: Income rank gap with children of natives along the parents income distribution - 3rd-order polynomial fit



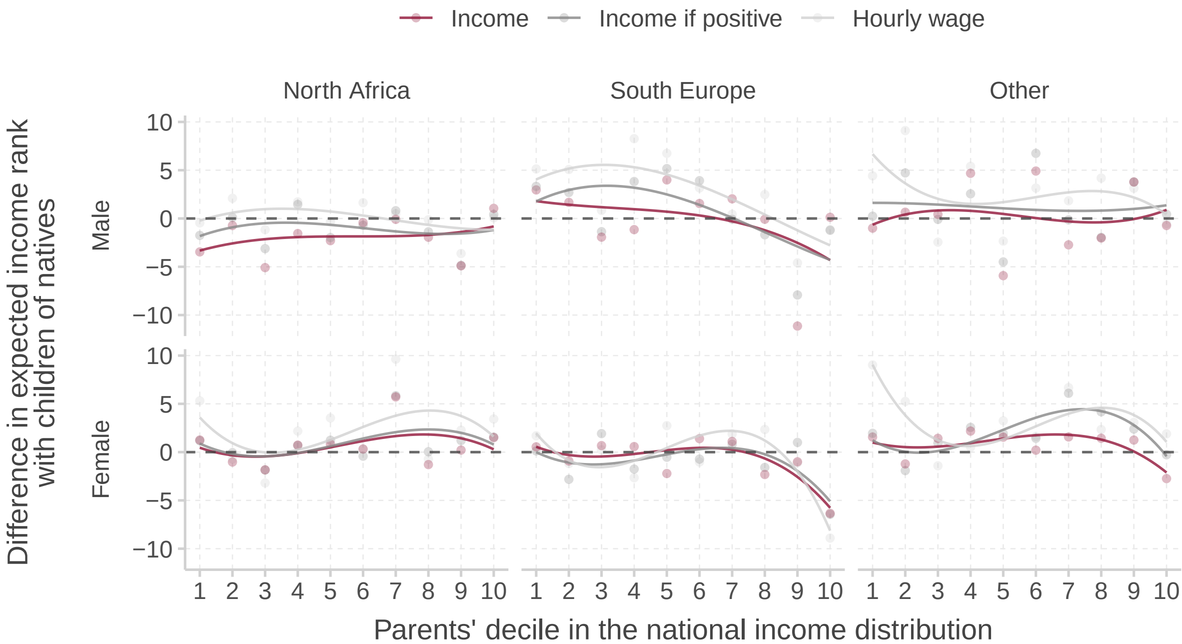
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.18: Income rank gap with children of natives along the parents income distribution - Mixed couple parents



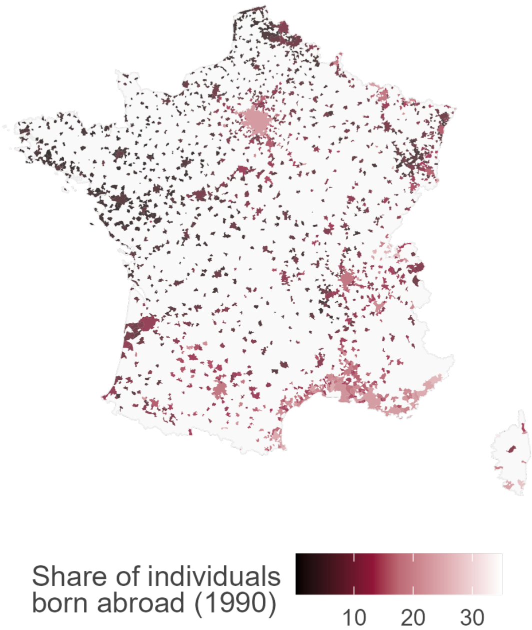
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.19: Income rank gap with children of natives along the parents income distribution - Mixed couple parents - 3rd-order polynomial fit



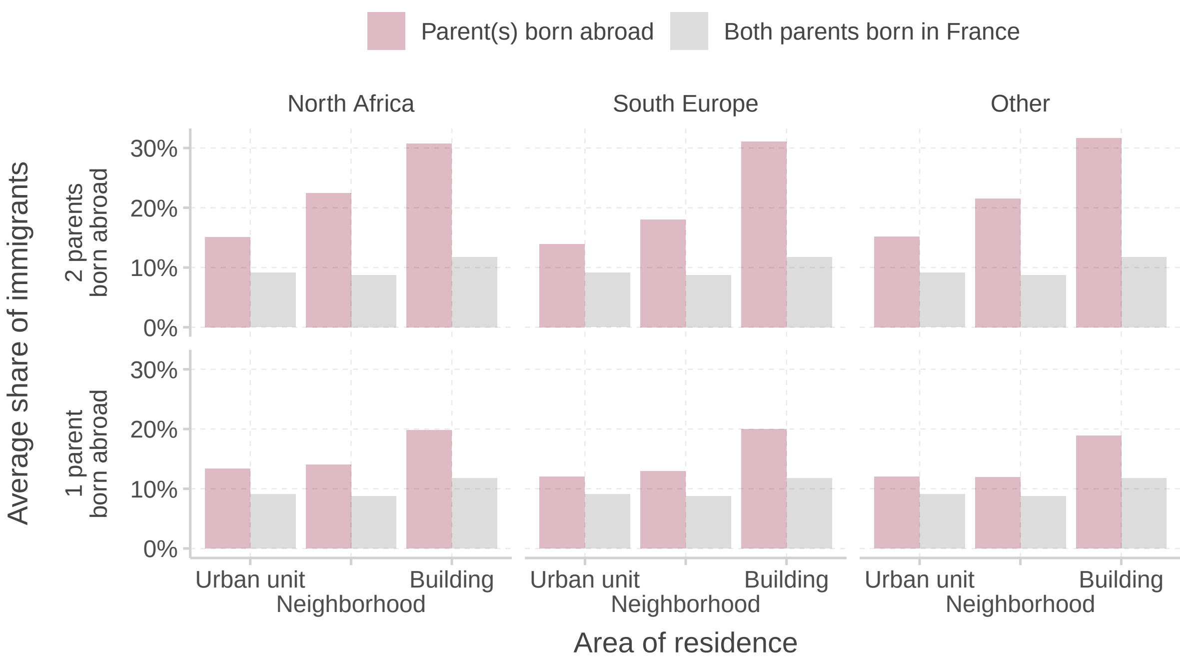
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.20: Share of immigrants across urban units in 1990



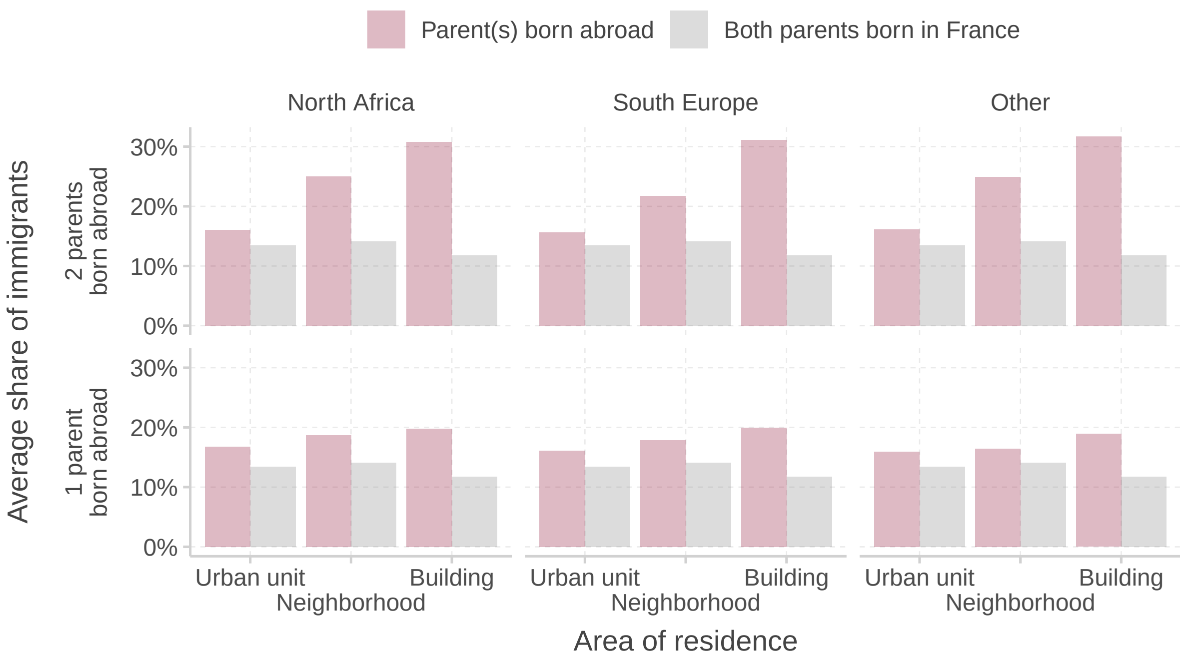
Source: *Full Population Census*, main sample, wave 1990, INSEE, and *GEOFLA®*, wave 1997, IGN.

Figure B.21: Share of immigrants by geographical unit of residence



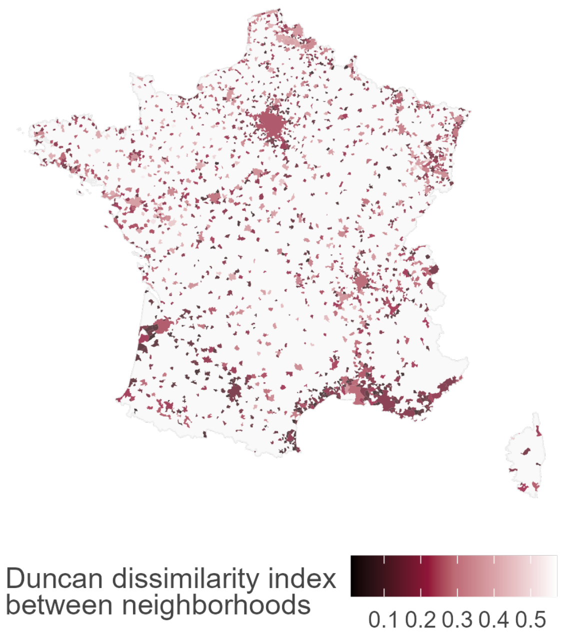
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP, and *Full Population Census*, main sample, wave 1990, INSEE.

Figure B.22: Share of immigrants by geographical unit of residence (constant sample)



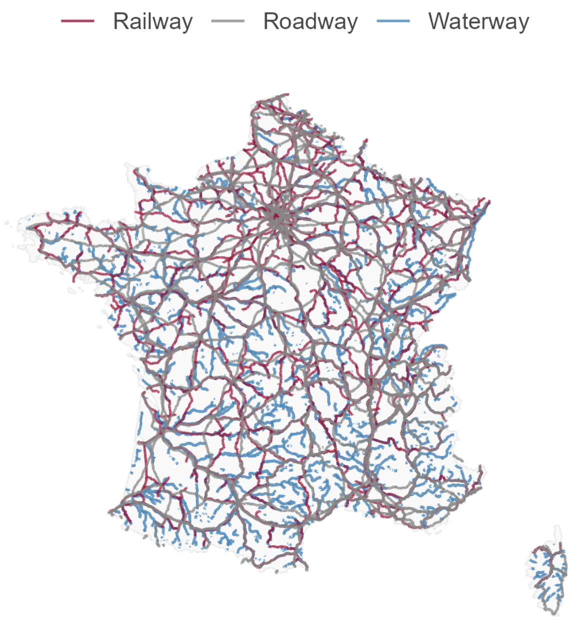
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP, and *Full Population Census*, main sample, wave 1990, INSEE.

Figure B.23: Segregation between neighborhoods across urban units in 1990



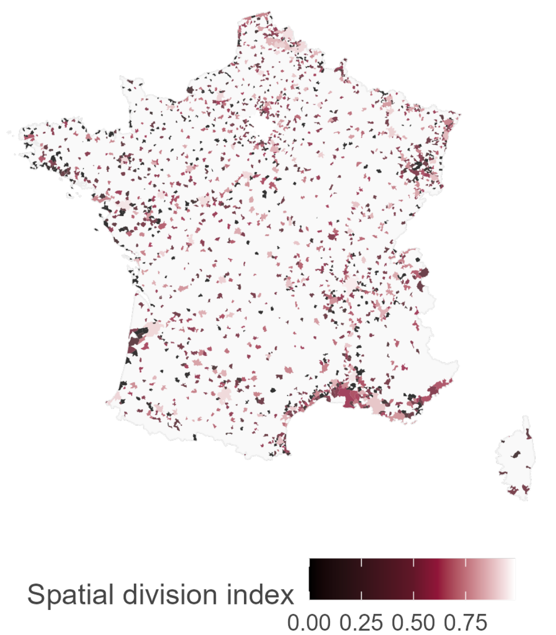
Source: *Full Population Census*, main sample, wave 1990, INSEE, and *GEOFLA*®, wave 1997, IGN.

Figure B.24: Geographical features considered for the instrument definition



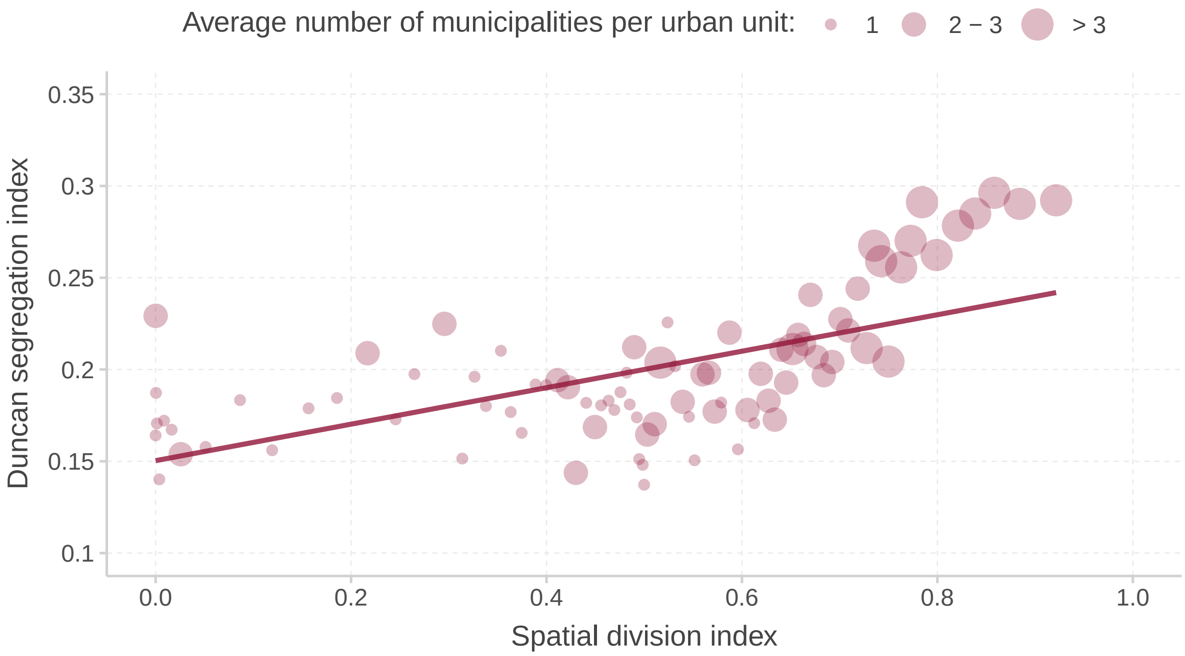
Source: *GEOFLA*®, wave 1997, IGN, and *Route 500*®, wave 1999, IGN.

Figure B.25: Spatial division index across urban units



Source: *GEOFLA*®, wave 1997, IGN, and *Route 500*®, wave 1999, IGN.

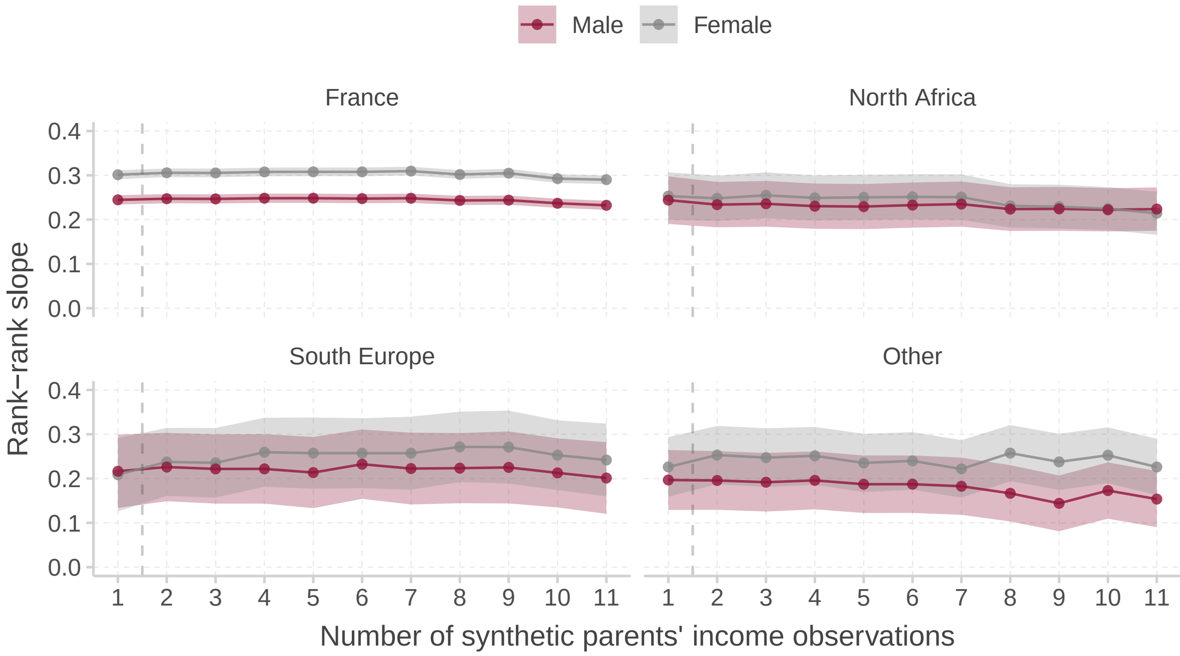
Figure B.26: Conditional expectation function of the first-stage relationship



Notes: For each centile of the spatial division index distribution, it shows the average average spatial division index on the x-axis and the average segregation index on the y-axis. The size of the dot indicates the size group of the urban unit, from 1 municipality to more than 3 municipalities on average. The straight line represents the corresponding regression fit.

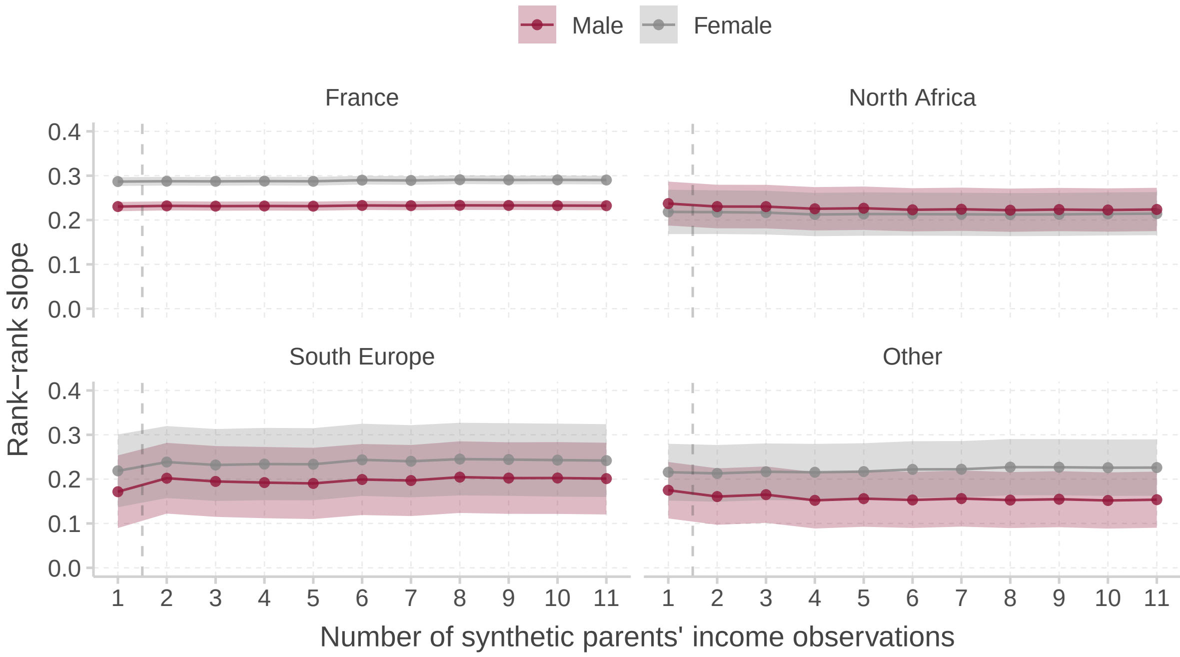
Source: *Full Population Census*, main sample, wave 1990, INSEE, *Route 500®*, wave 1999, IGN, and *GEOFLA®*, wave 1997, IGN.

Figure B.27: Attenuation bias - Parents' number of income observations



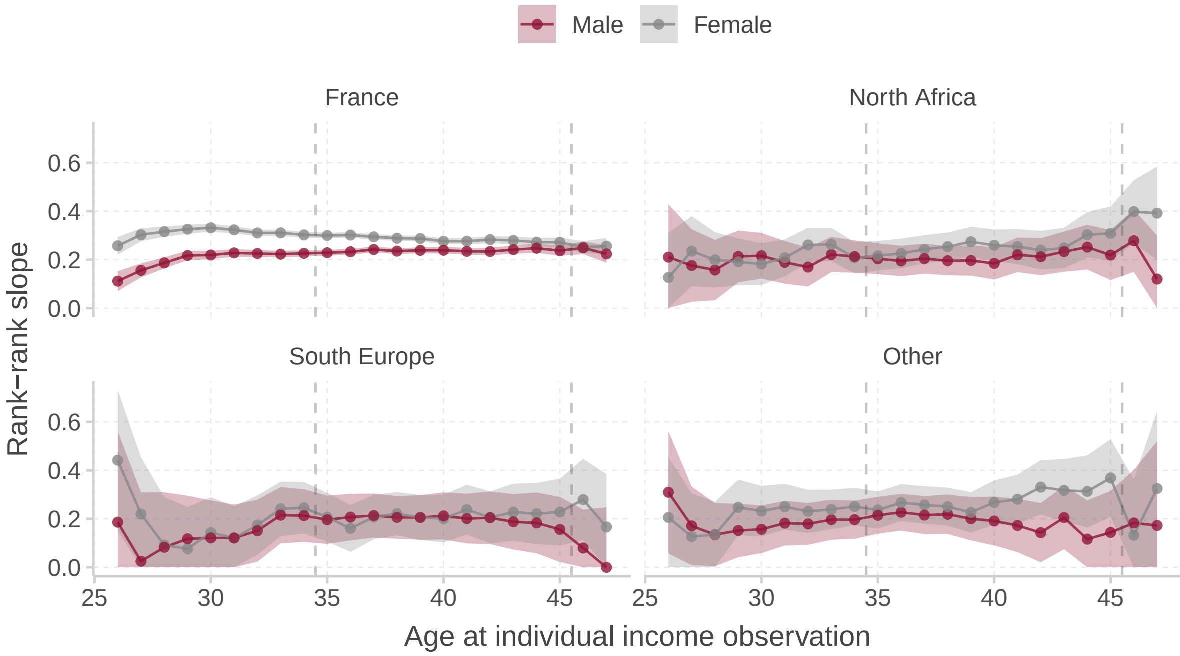
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.28: Attenuation bias - constant sample



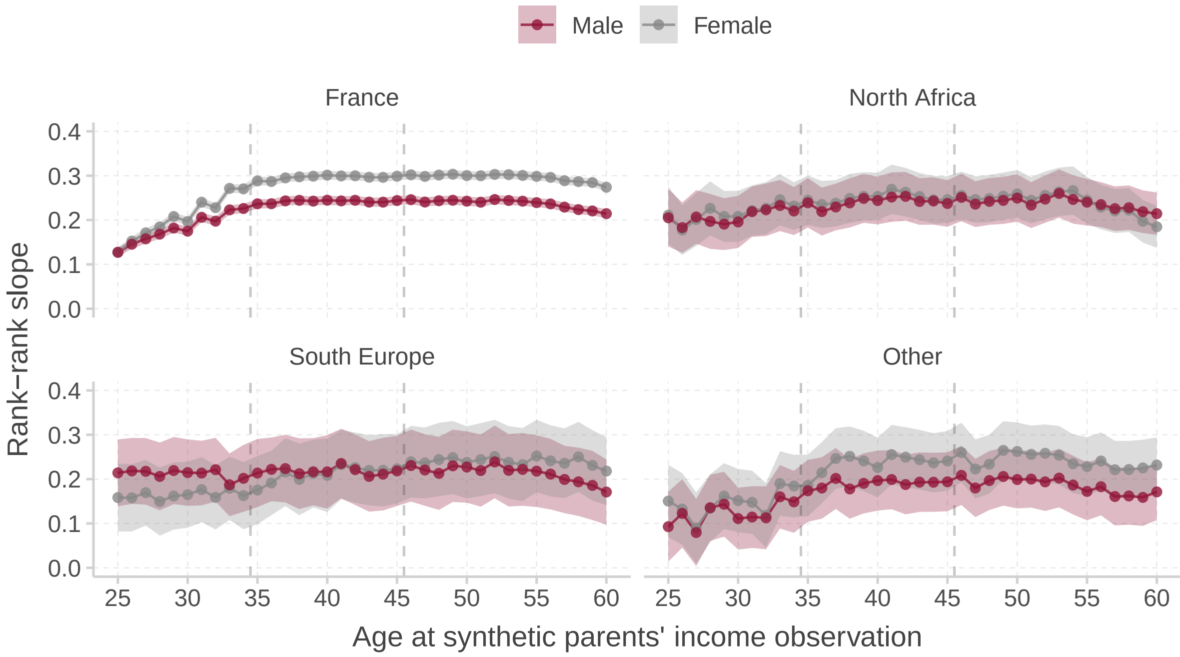
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.29: Lifecycle bias



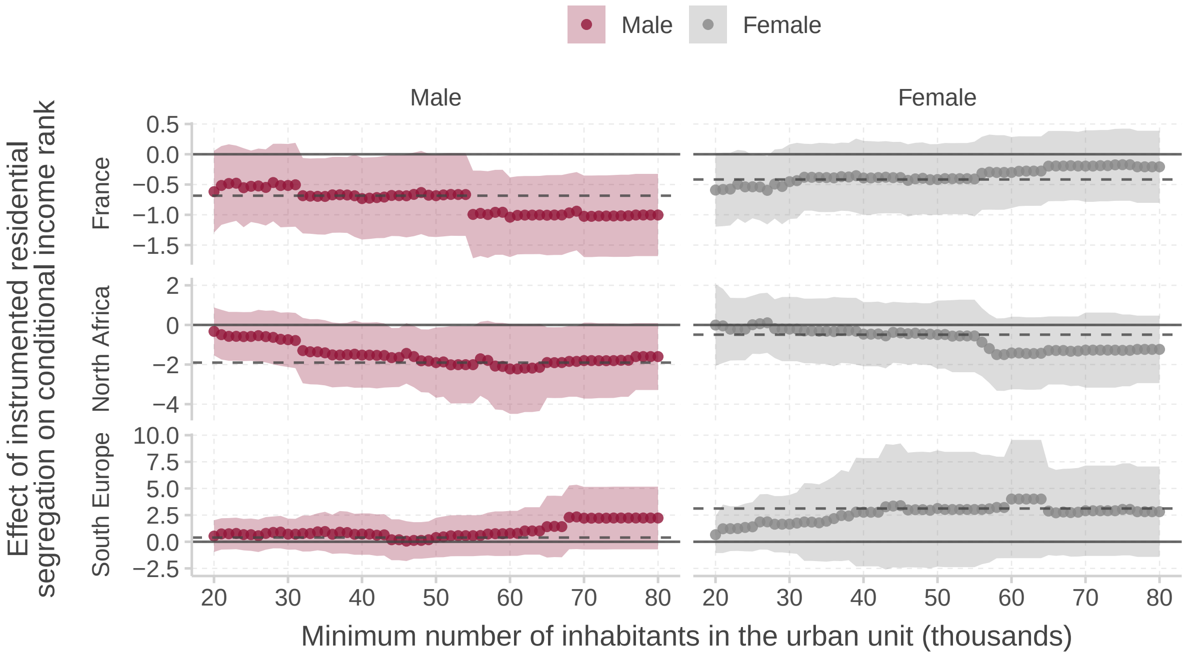
Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.30: Lifecycle bias - Parents' age



Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Figure B.31: Effect on large urban unit depending on the population threshold



Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Route 500®*, wave 1999, IGN, *GEOFLA®*, wave 1997, IGN, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

B.2 Additional Tables

Table B.1: Sample selection

	Born 72-84	Born France	Census 1990	As a child	Emp. par.	Tax data
	(1)	(2)	(3)	(4)	(5)	(6)
Female	296,227	211,407	49,250	47,745	44,692	42,425
Male	307,688	221,208	51,616	50,067	46,754	43,276
Total	603,915	432,615	100,866	97,812	91,446	85,701

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.2: Origin groups definition

	Mother's place of birth				
↓ Father's	France	North Africa	South Europe	Other	Absent
France	France	Mixed	Mixed	Mixed	France
North Africa	Mixed	North Africa	Other	Other	North Africa
South Europe	Mixed	Other	South Europe	Other	South Europe
Other	Mixed	Other	Other	Other	Other
Absent	France	North Africa	South Europe	Other	

Notes: South Europe includes Spain, Portugal, and Italy. North Africa includes Morocco, Algeria, and Tunisia.

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.3: Income rank gaps for the 1st and 2nd generation

	Males			Females		
	Par. rank	Individual rank		Par. rank	Individual rank	
	(1)	(2)	(3)	(4)	(5)	(6)
Both parents born abroad						
North Africa	−32.35***	−12.56***	−4.64***	−32.11***	−7.30***	2.29***
	(0.573)	(0.598)	(0.603)	(0.587)	(0.587)	(0.580)
South Europe	−21.58***	0.56	5.85***	−20.18***	0.20	6.22***
	(0.817)	(0.852)	(0.835)	(0.869)	(0.868)	(0.833)
Other	−19.49***	−5.74***	−0.96	−18.24***	−0.33	5.12***
	(0.908)	(0.948)	(0.927)	(0.922)	(0.921)	(0.883)
One parent born abroad						
North Africa	4.24***	−0.71	−1.75***	4.56***	2.06***	0.70
	(0.620)	(0.647)	(0.630)	(0.618)	(0.617)	(0.590)
South Europe	−5.29***	−1.25	0.05	−4.14***	−1.88**	−0.64
	(0.840)	(0.877)	(0.853)	(0.854)	(0.854)	(0.815)
Other	10.60***	3.01***	0.42	10.58***	3.67***	0.51
	(0.864)	(0.902)	(0.878)	(0.855)	(0.855)	(0.817)
Constant	53.20***	57.21***	44.18***	52.62***	44.77***	29.06***
	(0.148)	(0.155)	(0.300)	(0.150)	(0.150)	(0.282)
Parents' rank			✓			✓
Observations	43,276	43,276	43,276	42,425	42,425	42,425
R^2	0.094	0.011	0.066	0.088	0.005	0.094

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.4: Relationship between parents' and children's income rank across origins

	Child rank	
	Male	Female
	(1)	(2)
Parents' rank	0.25***	0.30***
	(0.005)	(0.005)
Both parents born abroad		
North Africa	−4.36***	3.62***
	(0.824)	(0.793)
South Europe	6.62***	8.93***
	(1.542)	(1.557)
Other	0.78	7.35***
	(1.432)	(1.376)
Parents' rank × North Africa	−0.01	−0.06**
	(0.025)	(0.025)
Parents' rank × South Europe	−0.02	−0.08**
	(0.040)	(0.040)
Parents' rank × Other	−0.05	−0.06**
	(0.032)	(0.030)
One parent born abroad		
North Africa	−3.30**	−0.53
	(1.410)	(1.317)
South Europe	2.75	1.34
	(1.813)	(1.756)
Other	−0.04	1.28
	(2.198)	(2.009)
Parents' rank × North Africa	0.03	0.02
	(0.022)	(0.021)
Parents' rank × South Europe	−0.06*	−0.04
	(0.033)	(0.032)
Parents' rank × Other	0.01	−0.01
	(0.032)	(0.029)
Constant	44.09***	28.81***
	(0.323)	(0.304)
Observations	43,276	42,425
R^2	0.066	0.094

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.5: Relationship between parents' income rank and children's higher education graduation across origins

	Child rank	
	Male	Female
	(1)	(2)
Parents' rank	0.006***	0.007***
	(0.0001)	(0.0001)
Both parents born abroad		
North Africa	0.182***	0.204***
	(0.0155)	(0.0156)
South Europe	0.137***	0.122***
	(0.0278)	(0.0291)
Other	0.138***	0.163***
	(0.0268)	(0.0266)
Parents' rank × North Africa	−0.003***	−0.003***
	(0.0005)	(0.0005)
Parents' rank × South Europe	−0.003***	−0.002***
	(0.0007)	(0.0008)
Parents' rank × Other	−0.001*	−0.001*
	(0.0006)	(0.0006)
One parent born abroad		
North Africa	−0.026	0.024
	(0.0251)	(0.0250)
South Europe	0.019	0.097***
	(0.0314)	(0.0326)
Other	0.020	0.055
	(0.0385)	(0.0379)
Parents' rank × North Africa	0.000	−0.000
	(0.0004)	(0.0004)
Parents' rank × South Europe	−0.001	−0.002***
	(0.0006)	(0.0006)
Parents' rank × Other	0.000	−0.001
	(0.0006)	(0.0006)
Constant	0.089***	0.197***
	(0.0056)	(0.0056)
Observations	38,202	38,201
R^2	0.127	0.131

Source: *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.6: Relationship between local share of immigrants and conditional income rank - Residential segregation heterogeneity - Both parents born abroad - Males

	Percentile income rank			
	France	North Africa	South Europe	Other
	(1)	(2)	(3)	(4)
%Immigrants in nbhd.	0.03	−0.00	−0.14	−0.50
	(0.085)	(0.232)	(0.419)	(0.357)
Segregation across nbhd.	−0.09	0.14	−0.42	−4.07
	(0.324)	(1.872)	(2.546)	(2.920)
%Immigrants × Segregation	−0.07**	−0.02	0.02	0.14
	(0.031)	(0.082)	(0.152)	(0.122)
Constant	44.27***	40.91***	54.15***	59.46***
	(1.041)	(5.531)	(7.242)	(9.115)
Parents' rank	✓	✓	✓	✓
Observations	23,649	2,226	979	788
R^2	0.069	0.031	0.024	0.045

Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.7: Relationship between local share of immigrants and conditional income rank - Residential segregation heterogeneity - One parent born abroad - Males

	Percentile income rank			
	France	North Africa	South Europe	Other
	(1)	(2)	(3)	(4)
%Immigrants in nbhd.	0.03	0.03	−0.13	0.67
	(0.085)	(0.307)	(0.489)	(0.489)
Segregation across nbhd.	−0.09	−0.58	0.89	3.24
	(0.324)	(1.728)	(2.421)	(2.350)
%Immigrants × Segregation	−0.07**	−0.05	−0.04	−0.34*
	(0.031)	(0.113)	(0.185)	(0.184)
Constant	44.27***	43.57***	46.71***	35.65***
	(1.041)	(5.233)	(7.146)	(7.447)
Parents' rank	✓	✓	✓	✓
Observations	23,649	1,648	831	799
R^2	0.069	0.072	0.041	0.070

Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.8: Relationship between local share of immigrants and conditional income rank - Residential segregation heterogeneity - Both parents born abroad - Females

	Percentile income rank			
	France	North Africa	South Europe	Other
	(1)	(2)	(3)	(4)
%Immigrants in nbhd.	0.13	-0.46^{**}	0.44	1.16^{***}
	(0.078)	(0.231)	(0.396)	(0.404)
Segregation across nbhd.	0.08	-3.24^{*}	3.68	4.03
	(0.304)	(1.873)	(2.601)	(3.035)
%Immigrants × Segregation	-0.08^{***}	0.14^{*}	-0.20	-0.43^{***}
	(0.028)	(0.082)	(0.143)	(0.141)
Constant	28.21^{***}	42.95^{***}	29.62^{***}	25.44^{***}
	(0.985)	(5.473)	(7.417)	(9.068)
Parents' rank	✓	✓	✓	✓
Observations	23,288	2,144	846	783
R^2	0.107	0.041	0.035	0.070

Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.9: Relationship between local share of immigrants and conditional income rank - Residential segregation heterogeneity - One parent born abroad - Females

	Percentile income rank			
	France	North Africa	South Europe	Other
	(1)	(2)	(3)	(4)
%Immigrants in nbhd.	0.13	0.42	0.27	0.40
	(0.078)	(0.291)	(0.442)	(0.413)
Segregation across nbhd.	0.08	2.02	0.55	2.28
	(0.304)	(1.636)	(2.336)	(2.037)
%Immigrants × Segregation	-0.08^{***}	-0.23^{**}	-0.07	-0.17
	(0.028)	(0.113)	(0.167)	(0.153)
Constant	28.21^{***}	25.24^{***}	24.97^{***}	23.30^{***}
	(0.985)	(4.908)	(6.876)	(6.401)
Parents' rank	✓	✓	✓	✓
Observations	23,288	1,671	799	793
R^2	0.107	0.121	0.084	0.083

Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.10: Instrumented effect of residential segregation on the relationship between local share of immigrants and conditional income rank - Both parents born abroad - Females

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	-3.23	-1.87	-2.09	1.03	1.15	-8.58
	(2.814)	(1.372)	(4.343)	(0.848)	(2.448)	(7.493)
$\widehat{\text{Segregation}}$	-43.84	-20.62	14.96	4.28	7.03	-28.76
	(34.780)	(14.908)	(27.972)	(3.753)	(22.194)	(38.264)
$\widehat{\text{\%Imm.} \times \text{Seg.}}$	0.78	0.67	0.45	-0.42	-0.49	3.12
	(0.782)	(0.504)	(1.425)	(0.299)	(0.877)	(2.780)
Parents' rank	0.24^{***}	0.15^{*}	0.37^{***}	0.31^{***}	0.22^{***}	0.07
	(0.039)	(0.085)	(0.109)	(0.010)	(0.056)	(0.073)
Constant	130.97	81.40^{**}	15.67	14.12	14.14	114.12
	(80.114)	(33.665)	(64.503)	(12.202)	(65.931)	(105.231)
Waterway length	✓	✓	✓	✓	✓	✓
Roadway length	✓	✓	✓	✓	✓	✓
Railway length	✓	✓	✓	✓	✓	✓
Observations	8,392	445	241	14,896	1,699	605
F-stat. Seg.	31.61	3.17	3.63	440.80	24.17	21.56
F-stat. Seg. × Imm.	420.21	3.91	14.97	265.55	24.19	13.33

Source: *Full Population Census*, main sample, wave 1990, INSEE, *Route 500*®, wave 1999, IGN, *GEOFLA*®, wave 1997, IGN, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.11: Instrumented effect of residential segregation on the relationship between local share of immigrants and conditional income rank - One parent born abroad - Males

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	-1.40	1.12	-0.20	1.68^{*}	2.52	8.98
	(1.323)	(1.334)	(1.420)	(0.967)	(3.245)	(8.479)
$\widehat{\text{Segregation}}$	-11.33	19.64	3.15	4.91	14.88	59.08
	(13.666)	(15.811)	(20.599)	(3.806)	(20.232)	(53.999)
$\widehat{\text{\%Imm.} \times \text{Seg.}}$	0.39	-0.37	-0.05	-0.68^{**}	-0.98	-3.25
	(0.401)	(0.470)	(0.604)	(0.349)	(1.188)	(3.061)
Parents' rank	0.26^{***}	0.23^{***}	0.21^{***}	0.25^{***}	0.30^{***}	0.23^{***}
	(0.017)	(0.068)	(0.074)	(0.011)	(0.033)	(0.063)
Constant	70.67^{**}	7.56	44.60	29.44^{**}	-4.06	-131.53
	(31.396)	(32.827)	(34.947)	(12.217)	(60.057)	(161.196)
Waterway length	✓	✓	✓	✓	✓	✓
Roadway length	✓	✓	✓	✓	✓	✓
Railway length	✓	✓	✓	✓	✓	✓
Observations	8,529	419	328	15,120	1,229	503
F-stat. Seg.	31.61	3.17	3.63	440.80	24.17	21.56
F-stat. Seg. × Imm.	420.21	3.91	14.97	265.55	24.19	13.33

Source: *Full Population Census*, main sample, wave 1990, INSEE, *Route 500*®, wave 1999, IGN, *GEOFLA*®, wave 1997, IGN, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.12: Instrumented effect of residential segregation on the relationship between local share of immigrants and conditional income rank - One parent born abroad - Females

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	-3.23	2.24	0.84	1.03	5.25	-3.96
	(2.814)	(1.447)	(1.712)	(0.848)	(5.390)	(4.333)
$\widehat{\text{Segregation}}$	-43.84	13.76	12.79	4.28	26.26	-38.64
	(34.780)	(29.873)	(14.498)	(3.753)	(37.700)	(34.038)
$\widehat{\%\text{Imm.} \times \text{Seg.}}$	0.78	-1.16	-0.29	-0.42	-2.14	1.34
	(0.782)	(0.756)	(0.711)	(0.299)	(1.997)	(1.539)
Parents' rank	0.24^{***}	0.33^{***}	0.29^{***}	0.31^{***}	0.29^{***}	0.27^{***}
	(0.039)	(0.081)	(0.074)	(0.010)	(0.036)	(0.053)
Constant	130.97	-0.51	-2.18	14.12	-41.06	145.72
	(80.114)	(58.063)	(28.681)	(12.202)	(110.687)	(102.631)
Waterway length	✓	✓	✓	✓	✓	✓
Roadway length	✓	✓	✓	✓	✓	✓
Railway length	✓	✓	✓	✓	✓	✓
Observations	8,392	402	258	14,896	1,269	541
F-stat. Seg.	31.61	3.17	3.63	440.80	24.17	21.56
F-stat. Seg. × Imm.	420.21	3.91	14.97	265.55	24.19	13.33

Source: *Full Population Census*, main sample, wave 1990, INSEE, *Route 500*®, wave 1999, IGN, *GEOFLA*®, wave 1997, IGN, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.13: OLS relationship between residential segregation on the relationship between local share of immigrants and conditional income rank - Both parents born abroad - Males

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	−0.17	−0.18	−0.73	0.14	0.04	−0.31
	(0.119)	(0.421)	(0.649)	(0.135)	(0.318)	(0.670)
Segregation	0.10	−0.86	−0.38	−0.52	0.03	−6.02
	(0.410)	(2.738)	(3.230)	(0.649)	(2.800)	(4.748)
%Imm. × Seg.	0.01	0.07	0.17	−0.12**	−0.06	0.07
	(0.043)	(0.137)	(0.208)	(0.047)	(0.112)	(0.246)
Parents' rank	0.27***	0.26***	0.26***	0.26***	0.23***	0.18***
	(0.011)	(0.071)	(0.095)	(0.008)	(0.034)	(0.052)
Constant	44.26***	41.09***	55.13***	45.59***	42.57***	72.02***
	(1.262)	(8.136)	(9.817)	(2.256)	(8.461)	(13.739)
Observations	8,529	476	271	15,120	1,750	708
R^2	0.067	0.029	0.038	0.071	0.033	0.027

Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.14: OLS relationship between residential segregation on the relationship between local share of immigrants and conditional income rank - Both parents born abroad - Females

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	0.07	−0.83**	−0.53	−0.07	−0.55*	0.70
	(0.110)	(0.409)	(0.568)	(0.124)	(0.316)	(0.624)
Segregation	0.77**	−3.72	0.29	−1.87***	−4.70*	2.84
	(0.386)	(2.781)	(3.334)	(0.617)	(2.748)	(4.327)
%Imm. × Seg.	−0.09**	0.23*	0.09	−0.02	0.16	−0.32
	(0.039)	(0.136)	(0.187)	(0.043)	(0.111)	(0.227)
Parents' rank	0.28***	0.16**	0.37***	0.33***	0.25***	0.17***
	(0.010)	(0.080)	(0.087)	(0.008)	(0.032)	(0.053)
Constant	28.56***	45.84***	34.03***	33.94***	47.87***	35.92***
	(1.197)	(7.828)	(9.493)	(2.159)	(8.297)	(12.715)
Observations	8,392	445	241	14,896	1,699	605
R^2	0.086	0.021	0.084	0.118	0.047	0.025

Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.15: OLS relationship between residential segregation on the relationship between local share of immigrants and conditional income rank - One parent born abroad - Males

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	−0.17	0.21	−0.45	0.14	0.04	0.72
	(0.119)	(0.495)	(0.644)	(0.135)	(0.428)	(0.778)
Segregation	0.10	0.38	0.95	−0.52	−0.83	6.12
	(0.410)	(2.522)	(3.028)	(0.649)	(2.638)	(4.545)
%Imm. × Seg.	0.01	−0.09	0.03	-0.12^{**}	−0.06	−0.28
	(0.043)	(0.175)	(0.248)	(0.047)	(0.154)	(0.284)
Parents' rank	0.27^{***}	0.18^{***}	0.20^{***}	0.26^{***}	0.30^{***}	0.25^{***}
	(0.011)	(0.051)	(0.062)	(0.008)	(0.030)	(0.052)
Constant	44.26^{***}	45.97^{***}	51.36^{***}	45.59^{***}	42.34^{***}	25.23^{*}
	(1.262)	(7.466)	(8.735)	(2.256)	(8.308)	(14.229)
Observations	8,529	419	328	15,120	1,229	503
R^2	0.067	0.033	0.046	0.071	0.087	0.048

Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.16: OLS relationship between residential segregation on the relationship between local share of immigrants and conditional income rank - One parent born abroad - Females

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	0.07	−0.12	−0.40	−0.07	0.53	0.19
	(0.110)	(0.479)	(0.613)	(0.124)	(0.393)	(0.653)
Segregation	0.77^{**}	1.58	−1.21	-1.87^{***}	1.66	−3.64
	(0.386)	(2.392)	(3.150)	(0.617)	(2.486)	(3.891)
%Imm. × Seg.	-0.09^{**}	−0.15	0.21	−0.02	-0.26^{*}	−0.09
	(0.039)	(0.185)	(0.241)	(0.043)	(0.147)	(0.238)
Parents' rank	0.28^{***}	0.33^{***}	0.27^{***}	0.33^{***}	0.33^{***}	0.31^{***}
	(0.010)	(0.048)	(0.065)	(0.008)	(0.028)	(0.044)
Constant	28.56^{***}	29.89^{***}	28.93^{***}	33.94^{***}	26.08^{***}	39.70^{***}
	(1.197)	(7.125)	(8.894)	(2.159)	(7.734)	(11.903)
Observations	8,392	402	258	14,896	1,269	541
R^2	0.086	0.136	0.066	0.118	0.118	0.097

Source: *Full Population Census*, main sample, wave 1990, INSEE, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.17: Instrumented effect of residential segregation on the relationship between local share of immigrants and conditional income rank - Both parents born abroad - Males - Control for railroad wage potential

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	−220.90	6.43	7.73	2.12*	5.14**	1.52
	(7846.415)	(7.644)	(9.109)	(1.090)	(2.395)	(2.799)
$\widehat{\text{Segregation}}$	−2093.08	27.64	98.13	5.23	36.48*	−8.19
	(74350.871)	(37.041)	(111.057)	(4.004)	(20.666)	(15.371)
$\widehat{\text{\%Imm.} \times \text{Seg.}}$	69.83	−2.06	−2.89	−0.86**	−1.92**	−0.65
	(2482.162)	(2.468)	(3.285)	(0.395)	(0.860)	(1.067)
Parents' rank	−1.14	0.15	0.15	0.25***	0.23***	0.15***
	(49.777)	(0.151)	(0.310)	(0.011)	(0.060)	(0.058)
Constant	5101.03	−41.40	−193.21	27.17*	−64.95	63.93
	(179583.275)	(107.983)	(277.914)	(13.929)	(65.005)	(51.191)
Waterway length	✓	✓	✓	✓	✓	✓
Roadway length	✓	✓	✓	✓	✓	✓
Railway length	✓	✓	✓	✓	✓	✓
Wage potential	✓	✓	✓	✓	✓	✓
Observations	6,571	402	218	15,054	1,741	705
F-stat. Seg.	23.78	3.22	1.18	523.59	29.58	15.02
F-stat. Seg. × Imm.	230.12	1.73	5.99	259.96	24.58	7.22

Source: *Full Population Census*, main sample, wave 1990, INSEE, *Route 500®*, wave 1999, IGN, *GEOFLA®*, wave 1997, IGN, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.18: Instrumented effect of residential segregation on the relationship between local share of immigrants and conditional income rank - Both parents born abroad - Females - Control for railroad wage potential

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	11.88	−1.36	0.34	1.78*	1.62	−8.30
	(19.444)	(2.610)	(3.498)	(0.970)	(2.657)	(6.615)
$\widehat{\text{Segregation}}$	120.80	−8.20	20.05	6.71*	11.19	−24.69
	(222.680)	(18.845)	(19.973)	(3.939)	(23.116)	(29.477)
$\widehat{\text{\%Imm.} \times \text{Seg.}}$	−3.70	0.43	−0.28	−0.69**	−0.66	3.02
	(5.765)	(0.888)	(1.143)	(0.344)	(0.953)	(2.467)
Parents' rank	0.37*	0.13	0.34***	0.31***	0.22***	0.07
	(0.209)	(0.096)	(0.107)	(0.010)	(0.052)	(0.073)
Constant	−266.41	51.47	−10.26	1.80	2.31	105.13
	(540.100)	(48.864)	(52.660)	(13.835)	(73.217)	(81.542)
Waterway length	✓	✓	✓	✓	✓	✓
Roadway length	✓	✓	✓	✓	✓	✓
Railway length	✓	✓	✓	✓	✓	✓
Wage potential	✓	✓	✓	✓	✓	✓
Observations	6,492	357	204	14,832	1,694	602
F-stat. Seg.	23.78	3.22	1.18	523.59	29.58	15.02
F-stat. Seg. × Imm.	230.12	1.73	5.99	259.96	24.58	7.22

Source: *Full Population Census*, main sample, wave 1990, INSEE, *Route 500*®, wave 1999, IGN, *GEOFLA*®, wave 1997, IGN, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.19: Instrumented effect of residential segregation on the relationship between local share of immigrants and conditional income rank - One parent born abroad - Males - Control for railroad wage potential

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	−220.90	1.23	0.66	2.12*	5.31	19.45
	(7846.415)	(2.636)	(12.033)	(1.090)	(4.779)	(35.586)
$\widehat{\text{Segregation}}$	−2093.08	38.67	7.88	5.23	28.18	133.87
	(74350.871)	(43.974)	(24.267)	(4.004)	(26.752)	(248.875)
$\widehat{\%\text{Imm.} \times \text{Seg.}}$	69.83	−0.43	−0.33	−0.86**	−2.04	−6.95
	(2482.162)	(0.915)	(4.797)	(0.395)	(1.775)	(12.606)
Parents' rank	−1.14	0.31**	0.24**	0.25***	0.29***	0.27*
	(49.777)	(0.122)	(0.102)	(0.011)	(0.035)	(0.143)
Constant	5101.03	−33.00	27.07	27.17*	−52.88	−379.46
	(179583.275)	(97.912)	(82.750)	(13.929)	(86.896)	(773.855)
Waterway length	✓	✓	✓	✓	✓	✓
Roadway length	✓	✓	✓	✓	✓	✓
Railway length	✓	✓	✓	✓	✓	✓
Wage potential	✓	✓	✓	✓	✓	✓
Observations	6,571	323	259	15,054	1,221	498
F-stat. Seg.	23.78	3.22	1.18	523.59	29.58	15.02
F-stat. Seg. × Imm.	230.12	1.73	5.99	259.96	24.58	7.22

Source: *Full Population Census*, main sample, wave 1990, INSEE, *Route 500*®, wave 1999, IGN, *GEOFLA*®, wave 1997, IGN, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Table B.20: Instrumented effect of residential segregation on the relationship between local share of immigrants and conditional income rank - One parent born abroad - Females - Control for railroad wage potential

	Small urban units			Large urban units		
	France	North Afr.	South Eu.	France	North Afr.	South Eu.
	(1)	(2)	(3)	(4)	(5)	(6)
%Immigrants	11.88	6.30	-1.20	1.78^{*}	6.07	-3.14
	(19.444)	(3.965)	(2.694)	(0.970)	(7.722)	(3.857)
$\widehat{\text{Segregation}}$	120.80	7.74	-15.97	6.71^{*}	31.64	-28.11
	(222.680)	(23.051)	(23.599)	(3.939)	(50.251)	(27.942)
$\widehat{\%\text{Imm.} \times \text{Seg.}}$	-3.70	-2.99^{*}	0.49	-0.69^{**}	-2.45	1.09
	(5.765)	(1.809)	(1.079)	(0.344)	(2.904)	(1.387)
Parents' rank	0.37^{*}	0.28^{***}	0.24^{**}	0.31^{***}	0.29^{***}	0.28^{***}
	(0.209)	(0.087)	(0.117)	(0.010)	(0.033)	(0.050)
Constant	-266.41	-5.94	60.22	1.80	-58.34	120.34
	(540.100)	(49.588)	(55.279)	(13.835)	(160.319)	(86.360)
Waterway length	✓	✓	✓	✓	✓	✓
Roadway length	✓	✓	✓	✓	✓	✓
Railway length	✓	✓	✓	✓	✓	✓
Wage potential	✓	✓	✓	✓	✓	✓
Observations	6,492	314	196	14,832	1,258	541
F-stat. Seg.	23.78	3.22	1.18	523.59	29.58	15.02
F-stat. Seg. × Imm.	230.12	1.73	5.99	259.96	24.58	7.22

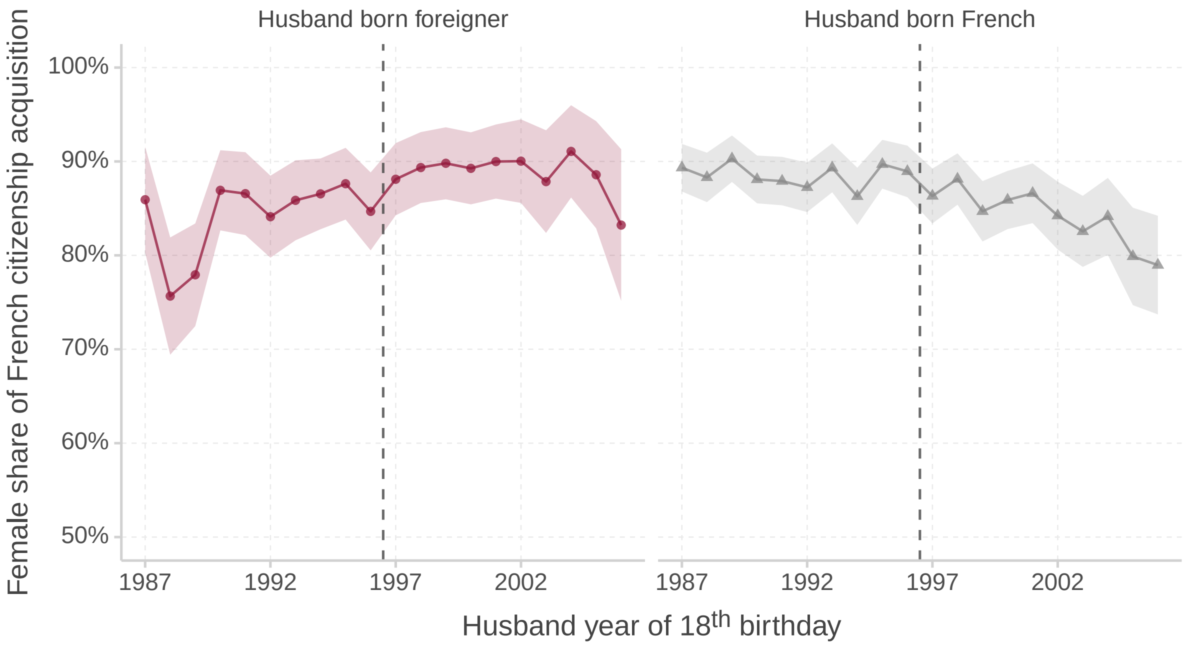
Full Population Census, main sample, wave 1990, INSEE, *Route 500*®, wave 1999, IGN, *GEOFLA*®, wave 1997, IGN, and *Permanent Demographic Sample*, wave 2020, INSEE-DGFiP.

Appendix C

To become or not to become French? Conscription, citizenship and labor market outcomes

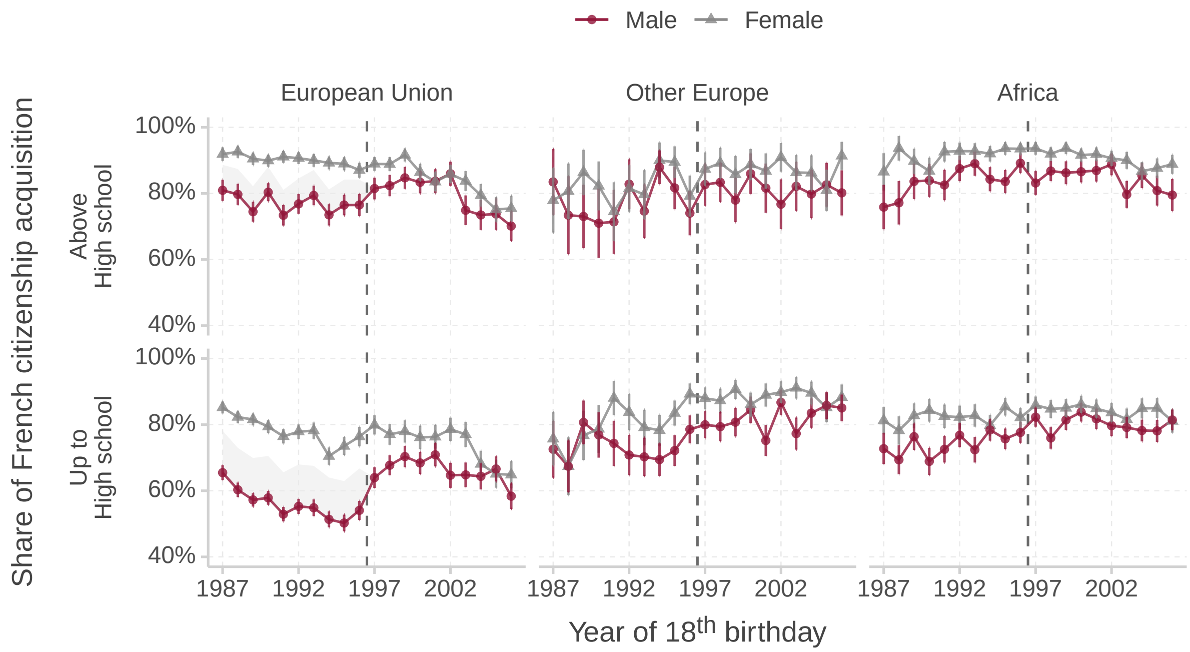
C.1 Additional Figures

Figure C.1: Female naturalization rates by their husband's birth cohort



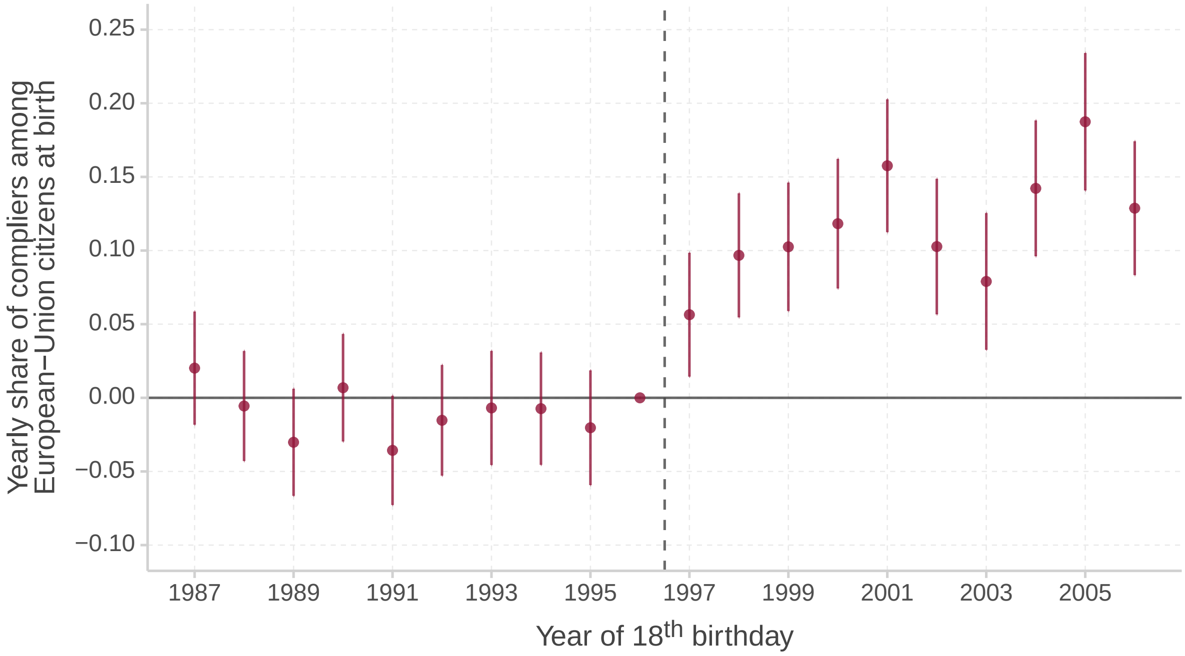
Notes: This figure represents the share of French citizenship acquisition among women born in France without French citizenship according to the birth cohort of their husband. The sample is restricted to women whose husband was born in France, and results are presented separately for those whose husband was born French (left panel) and those whose husband was born foreigner (right panel). The x-axis is labeled according to the husband's year of 18$^{\text{th}}$ birthday, which is when those born before 1979 must decide whether to do military service or to renounce French citizenship. The vertical dashed line represents the abolition of compulsory conscription. 95% confidence intervals are represented with light ribbons. Source: *French Population Census*, wave 2014 - complementary census sample, INSEE.

Figure C.2: Naturalization rates by education and birth nationality groups - Excluding Austrian, Finnish, and Swedish birth nationalities



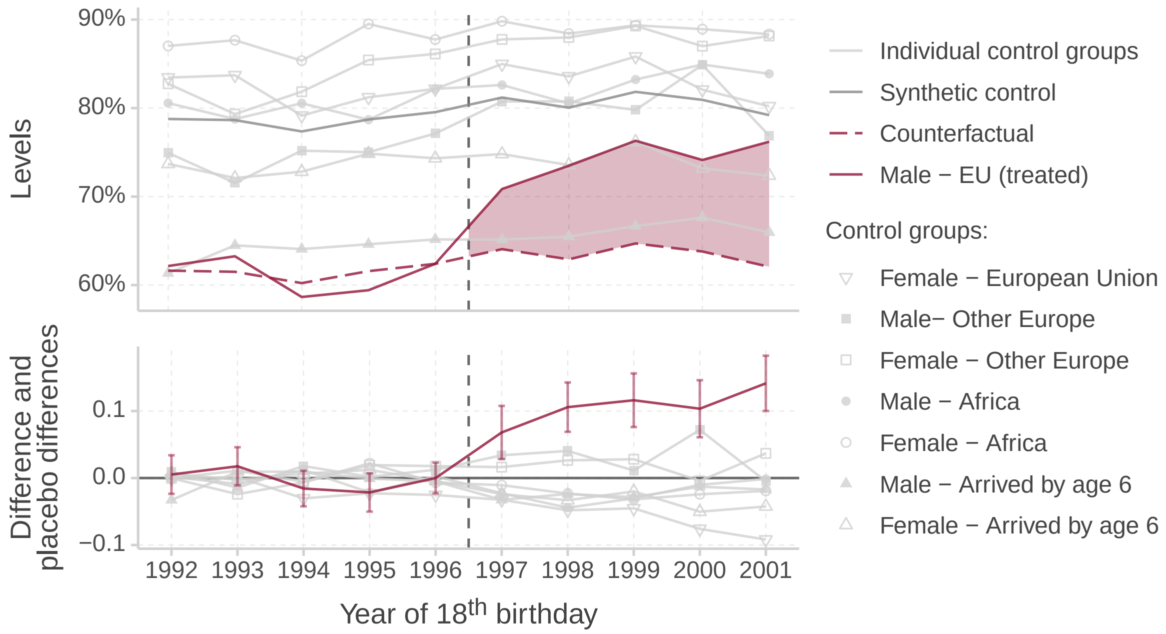
Notes: This figure represents the share of French citizenship acquisition among individuals born in France without French citizenship for 6 subgroups defined according to birth nationality (European Union, Other Europe, and Africa) and education (up to high school and above high school). European Union is defined as it was in 1994, before the inclusion of Austria, Finland, and Sweden. In each panel, shares of citizenship acquisition are represented separately for males (red) and females (gray), and for birth cohorts from 1969 to 1988. Vertical lines show the corresponding 95% confidence intervals. The x-axis is labeled according to the year of 18th birthday, which is when males born before 1979 must decide whether to do military service or to renounce French citizenship. Shaded areas represent the fraction of missing citizenship take-up among young males caused by compulsory conscription. The height of the area is obtained from Difference-in-Differences regressions between males and females born 5 years before and after 1979, estimated on the corresponding subgroups as specified in Equation 3.1. Regression results are reported in Appendix Table C.3. A significant effects are found only for individuals born citizens of the European Union. Source: *French Population Census*, wave 2014, INSEE.

Figure C.3: Naturalization rates of EU second-generation immigrants



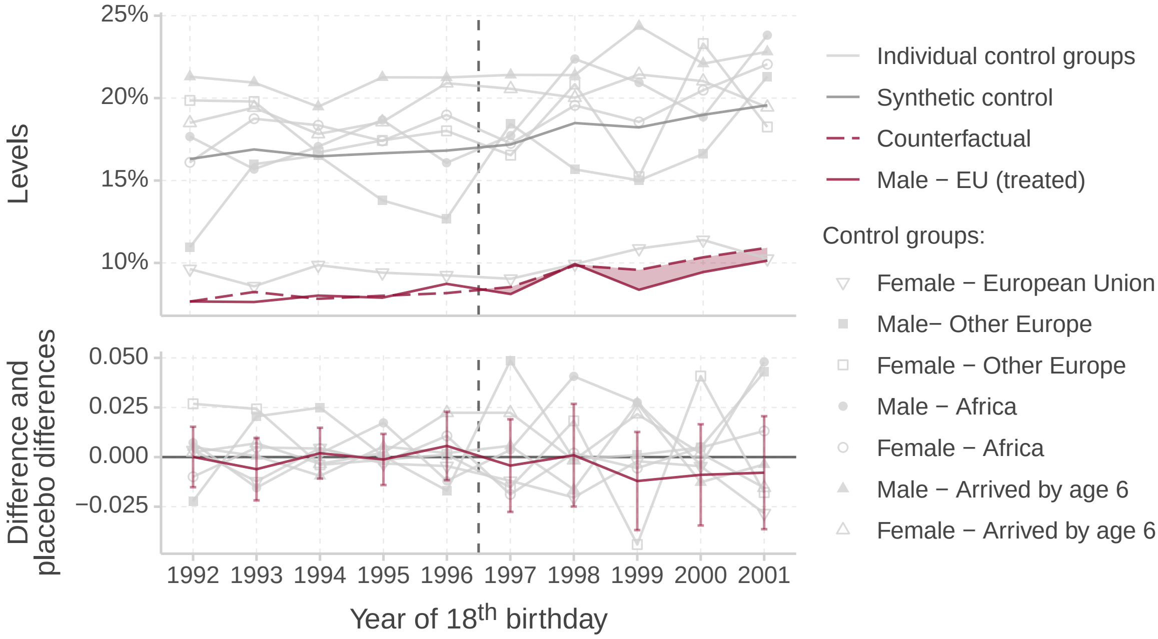
Notes: This figures shows regression results which document the evolution of the difference in French citizenship acquisition rates between males and females among those born in France with a non-French European Union citizenship. European Union is defined as it was in 1996. Coefficients are obtained from the regression of French citizenship acquisition on gender, birth cohort, and their interaction. The 1978 birth cohort is used as the reference category because it is the last cohort which was not subject to compulsory conscription. Females are used as the reference category because they were not subject to the abolition of compulsory conscription. Source: *French Population Census*, wave 2014, INSEE.

Figure C.4: Synthetic Difference-in-Differences - Naturalization



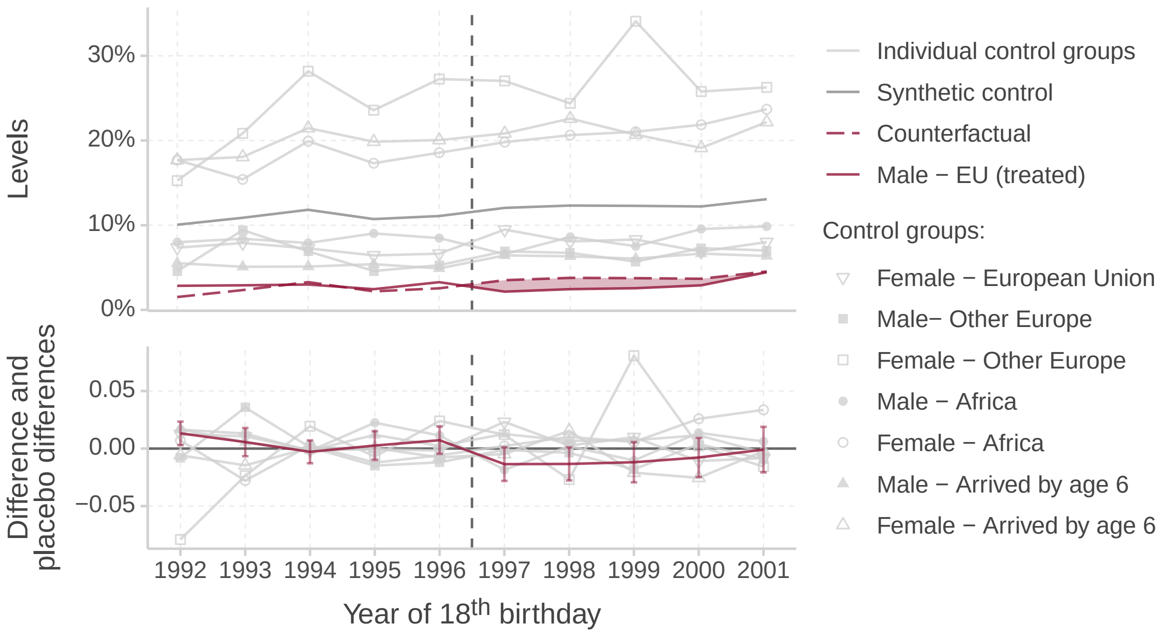
Notes: The top panel of this figure shows the share of individuals who acquired French citizenship by birth cohort within each of the groups used in our Synthetic Difference-in-Differences identification strategy. That of the treated group, male European Union citizens, is represented as a solid red line. That of each individual control group is represented with a gray line, whose markers indicate the corresponding group. The solid gray line shows the trend of the synthetic control group. The dashed red line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid red line, centered at the pre-treatment average difference weighted by the estimated time weights. Vertical lines show the corresponding 95% bootstrapped confidence intervals. The gray lines are placebo effects, each using one of the control groups as the treatment group instead of EU males. Corresponding regression results are reported in Table C.8. Source: *French Population Census*, wave 2014, INSEE.

Figure C.5: Synthetic Difference-in-Differences - Unemployed



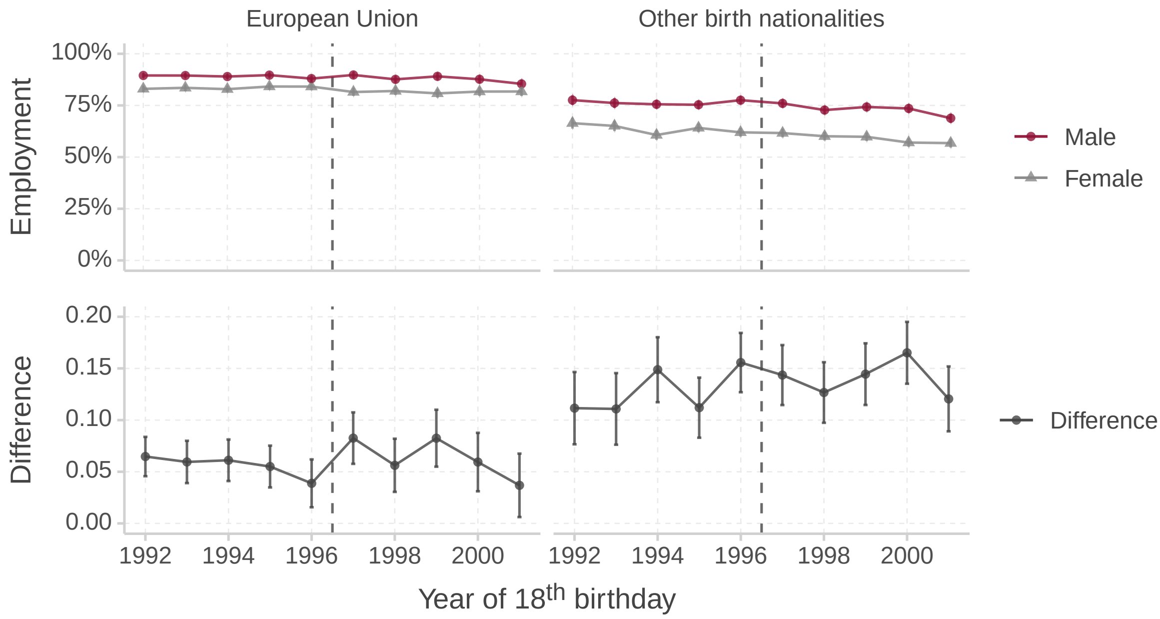
Notes: The top panel of this figure shows the share of unemployed individuals by birth cohort within each of the groups used in our Synthetic Difference-in-Differences identification strategy. That of the treated group, male European Union citizens, is represented as a solid red line. That of each individual control group is represented with a gray line, whose markers indicate the corresponding group. The solid gray line shows the trend of the synthetic control group. The dashed red line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid red line, centered at the pre-treatment average difference weighted by the estimated time weights. Vertical lines show the corresponding 95% bootstrapped confidence intervals. The gray lines are placebo effects, each using one of the control groups as the treatment group instead of EU males. Corresponding regression results are reported in Table C.8. Source: *French Population Census*, wave 2014, INSEE.

Figure C.6: Synthetic Difference-in-Differences - Inactive



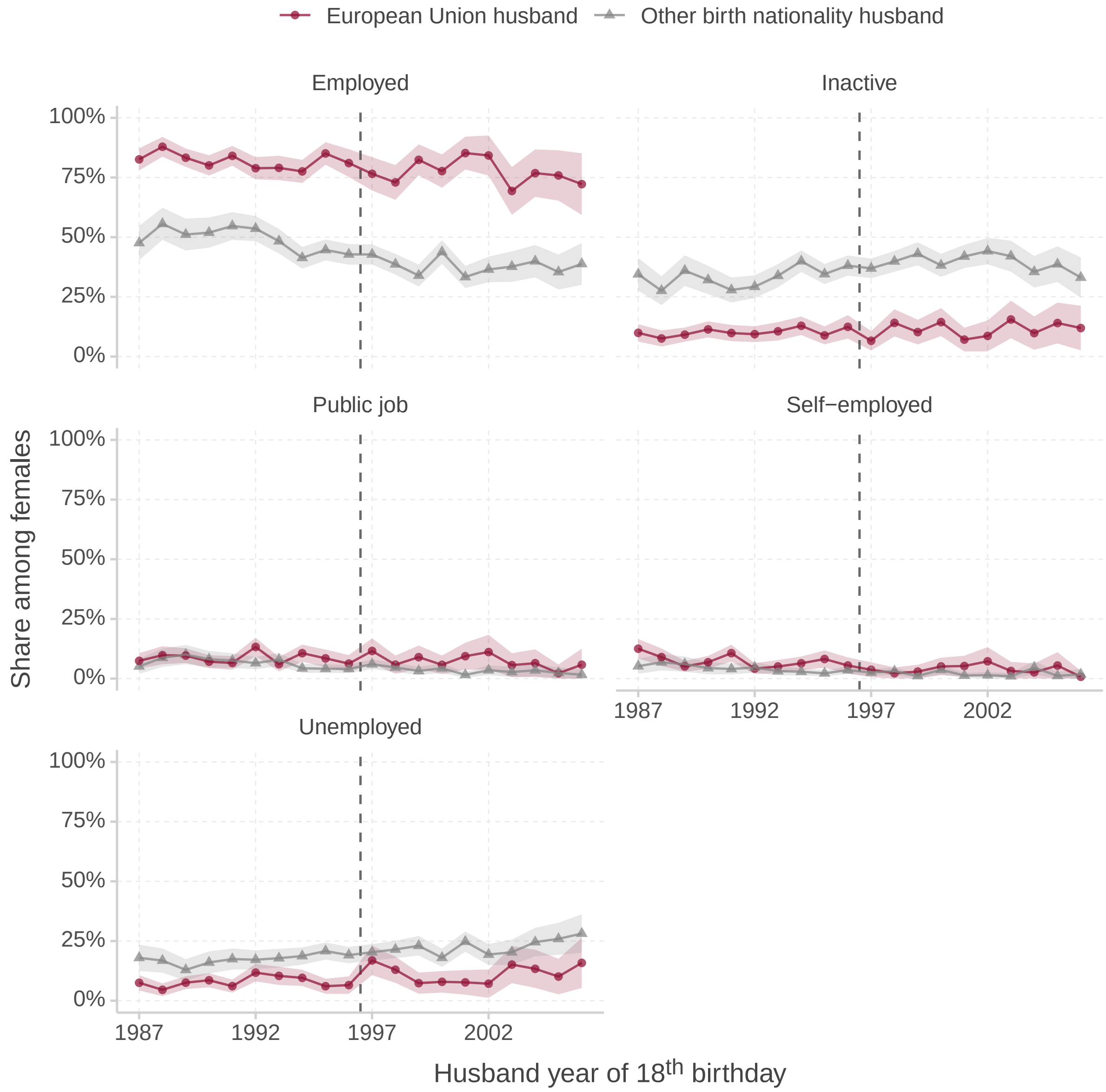
Notes: The top panel of this figure shows the share of inactive individuals by birth cohort within each of the groups used in our Synthetic Difference-in-Differences identification strategy. That of the treated group, male European Union citizens, is represented as a solid red line. That of each individual control group is represented with a gray line, whose markers indicate the corresponding group. The solid gray line shows the trend of the synthetic control group. The dashed red line represents the counterfactual trend of the treated group. It corresponds to the trend of the synthetic control group shifted by the average difference between the trend of the treatment group and that of the synthetic control group in the pre-period. The bottom panel displays the difference between the treated group and the synthetic control group with a solid red line, centered at the pre-treatment average difference weighted by the estimated time weights. Vertical lines show the corresponding 95% bootstrapped confidence intervals. The gray lines are placebo effects, each using one of the control groups as the treatment group instead of EU males. Corresponding regression results are reported in Table C.8. Source: *French Population Census*, wave 2014, INSEE.

Figure C.7: Difference in employment trends between males and females - EU vs. Non-EU citizens at birth



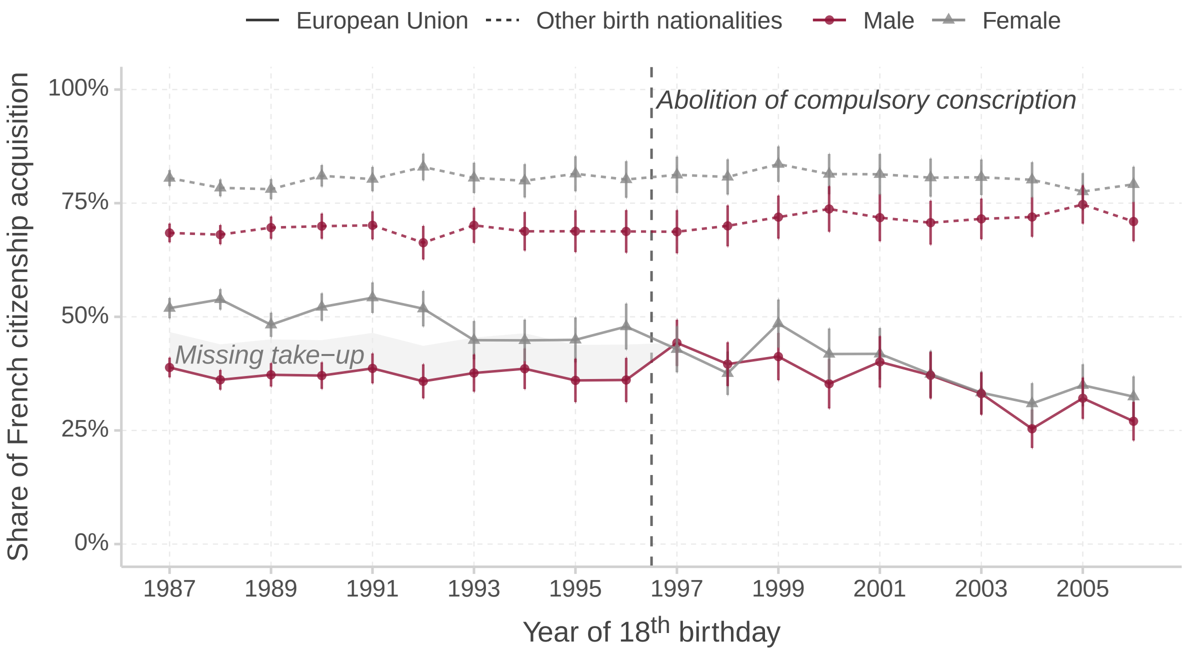
Notes: The top panel of this figure shows the share of employed males (red) and females (gray) by birth cohort among individuals born in France without French citizenship, separately for those born citizens of the European Union and those born with a nationality outside the EU. European Union is defined as it was in 1996. The x-axis is labeled according to the year of 18th birthday, which is when men born before 1979 must decide whether to do military service or to renounce French citizenship. The vertical dashed line represents the abolition of compulsory conscription. 95% confidence intervals are represented with vertical error bars. The bottom panel shows the difference in the share of employed individuals between males and females, by birth cohort and group of birth nationality. Source: *French Population Census*, wave 2014, INSEE.

Figure C.8: Female labor market outcomes by their husband's birth cohort



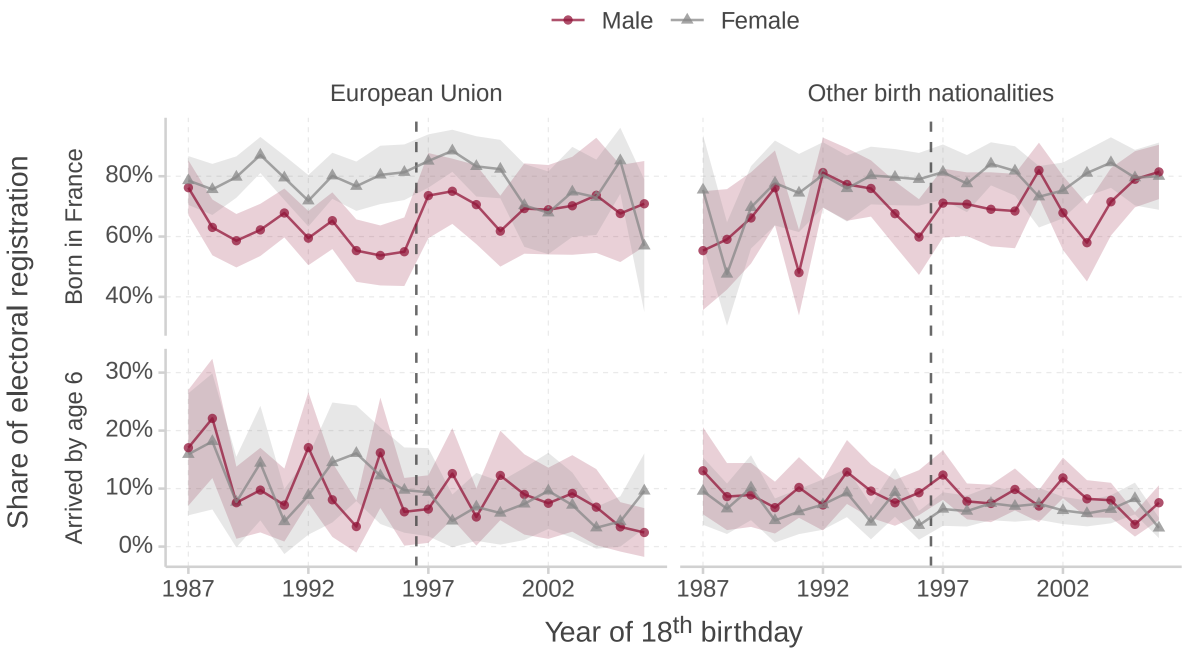
Notes: This figure represents average labor market outcomes of women born in France without French citizenship according to the birth cohort of their husband. The sample is restricted to women whose husband was born in France without French citizenship. Each panel corresponds to a given labor market outcome, and represents the share of women which pertains to the corresponding category, separately for those whose husband's birth citizenship pertains to the European Union (red) and for others (gray). European Union is defined as it was in 1996. The x-axis is labeled according to the husband's year of 18th birthday, which is when those born before 1979 must decide whether to do military service or to renounce French citizenship. The vertical dashed line represents the abolition of compulsory conscription. 95% confidence intervals are represented with light ribbons, bounded between 0 and 1. Source: *French Population Census*, wave 2014 - complementary census sample, INSEE.

Figure C.9: Naturalization rates of first-generation immigrants arrived by age 6



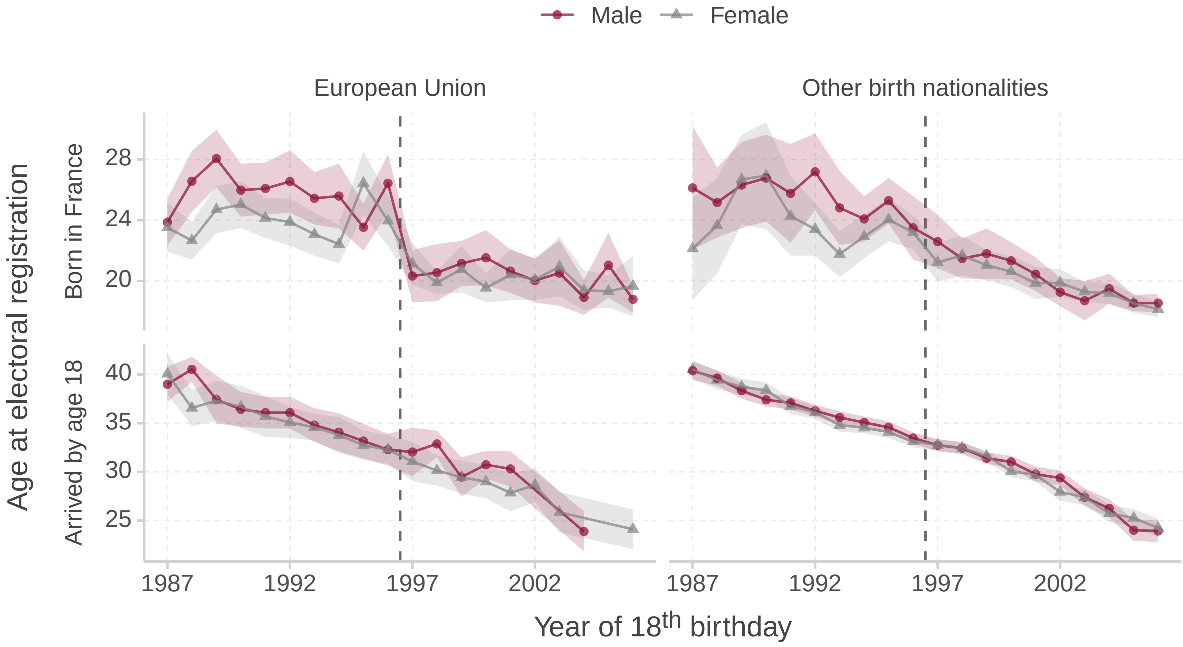
Notes: This figure represents the share of French citizenship acquisition among individuals born abroad and arrived in France by age 6 without French citizenship, separately for males (red) and females (gray), for those born with an European Union citizenship (solid lines) and other citizenship (dashed lines), for birth cohorts from 1969 to 1988. European Union is defined as it was in 1996. Vertical lines show the corresponding 95% confidence intervals. The x-axis is labeled according to the year of 18$^{\text{th}}$ birthday, when military service is tied to French citizenship acquisition for men. The vertical dashed line represents the abolition of compulsory conscription. For males, after this point, citizenship acquisition is not tied to doing military service anymore. The shaded area represents the estimated fraction of missing citizenship take-up caused by compulsory conscription among young males born citizens of the European Union without French citizenship, who were born abroad and who arrived in France by age 6. The height of the area is obtained from a Difference-in-Differences regression between males and females born 5 years before and after 1979. Source: *French Population Census*, wave 2014, INSEE.

Figure C.10: Registration in electoral list



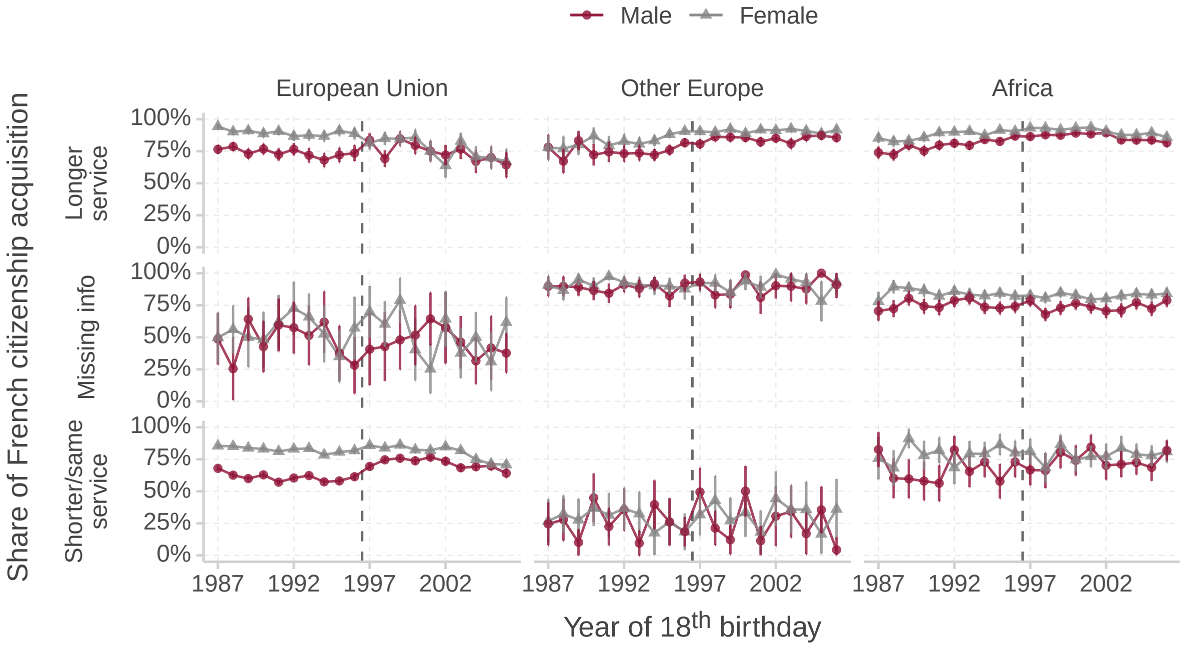
Notes: This figure shows the share of individuals registered in the electoral registers, as a proxy for citizenship acquisition, for 4 different subgroups defined according to place of birth (born France vs. born abroad and arrived in France by age 6) and group of birth citizenship (European Union citizenship vs. others). European Union defined as it was in 1996. In each panel, registration rates are represented separately for males (red) and females (gray), for each birth cohort from 1969 to 1988. The x-axis is labeled according to the year of 18$^{\text{th}}$ birthday, which is when males born before 1979 must decide whether to do military service or to renounce French citizenship. 95% confidence intervals are represented with light ribbons. Computations are made on the Permanent Demographic Sample, which covers about 4% of the population. Source: *Permanent Demographic Sample*, INSEE-DGFiP.

Figure C.11: Age at registration in electoral list



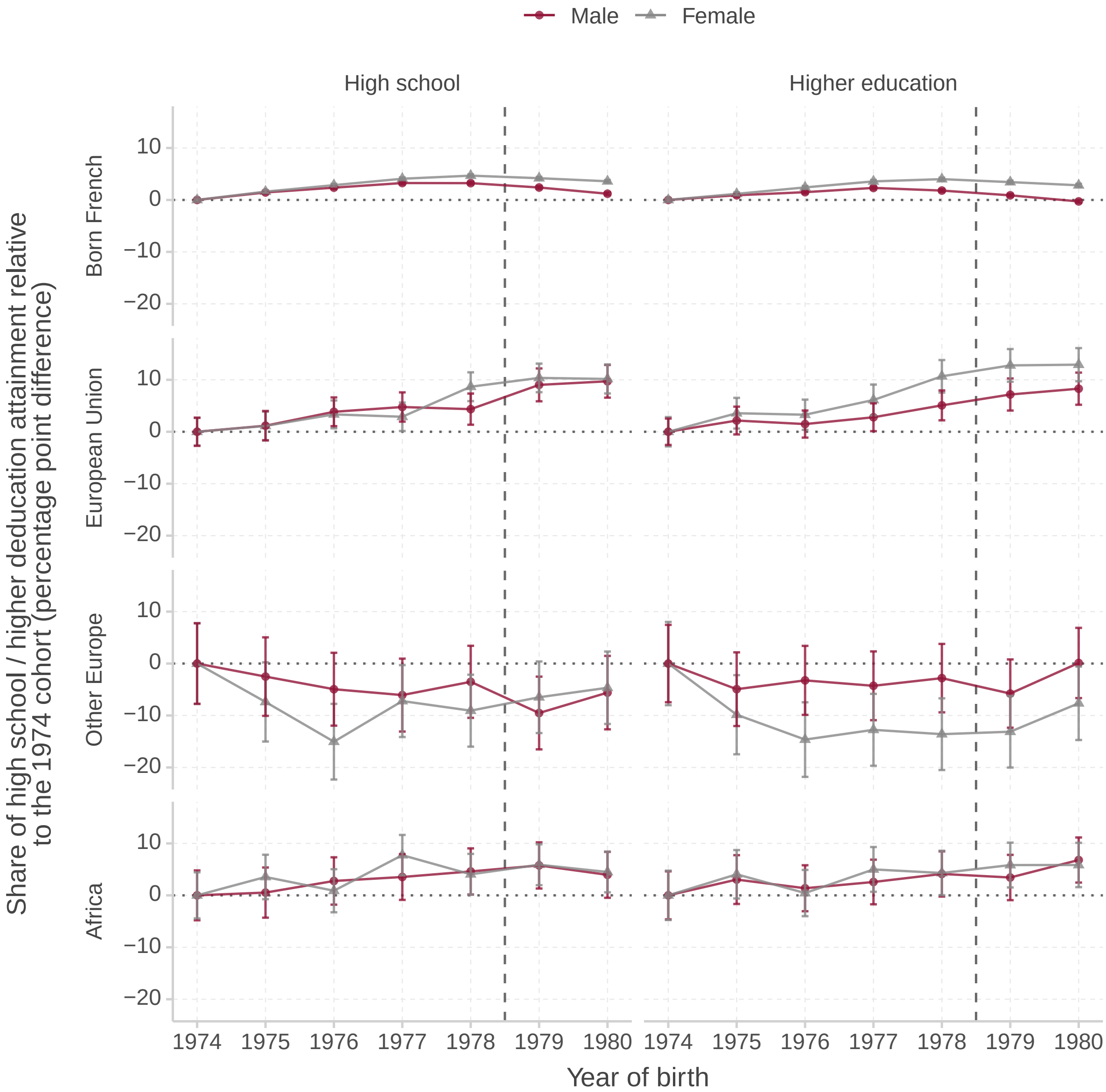
Notes: This figure shows the average age at registration in the electoral registers, as a proxy for age at acquisition of citizenship, for 4 different subgroups defined according to place of birth (born France vs. born abroad and arrived in France by age 18) and group of birth citizenship (European Union citizenship vs. others). European Union defined as it was in 1996. In each panel, registration rates are represented separately for males (red) and females (gray), for each birth cohort from 1979 to 1988. The x-axis is labeled according to the year of 18$^{\text{th}}$ birthday, which is when males born before 1979 must decide whether to do military service or to renounce French citizenship. 95% confidence intervals are represented with light ribbons. Computations are made on the Permanent Demographic Sample, which covers about 4% of the population. Missing information for some groups in specific years is due to insufficient sample size. Source: *Permanent Demographic Sample*, INSEE-DGFiP.

Figure C.12: Naturalization rates by duration of the military service of birth nationality



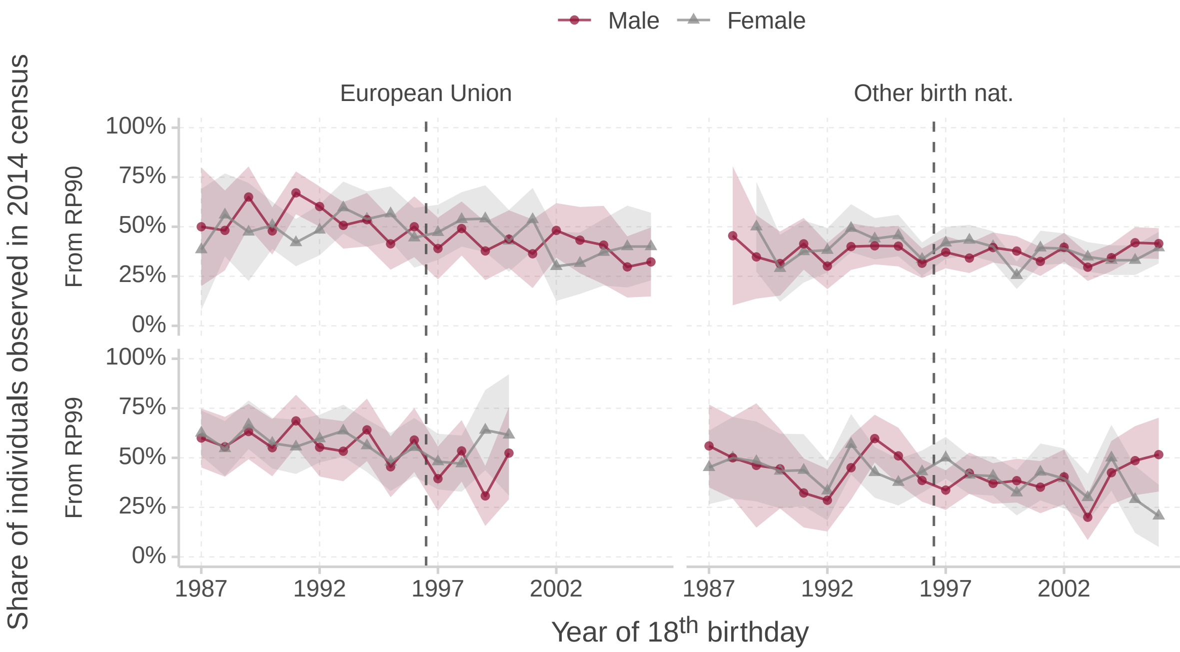
Notes: This figure represents the share of French citizenship acquisition among individuals born in France without French citizenship for 9 subgroups defined by birth nationality (European Union, Other Europe, and Africa) and military service duration of in the country of birth nationality (longer vs. shorter than the French military service, or unknown). European Union is defined as it was in 1996. In each panel, shares of citizenship acquisition are represented separately for males (red) and females (gray), for birth cohorts from 1969 to 1988. Vertical lines show the corresponding 95% confidence intervals, bounded between 0 and 1. The x-axis is labeled according to the year of 18th birthday, which is when males born before 1979 must decide whether to do military service or to renounce French citizenship. Source: *French Population Census*, wave 2014, INSEE.

Figure C.13: Education rates relative to the 1974 birth cohort



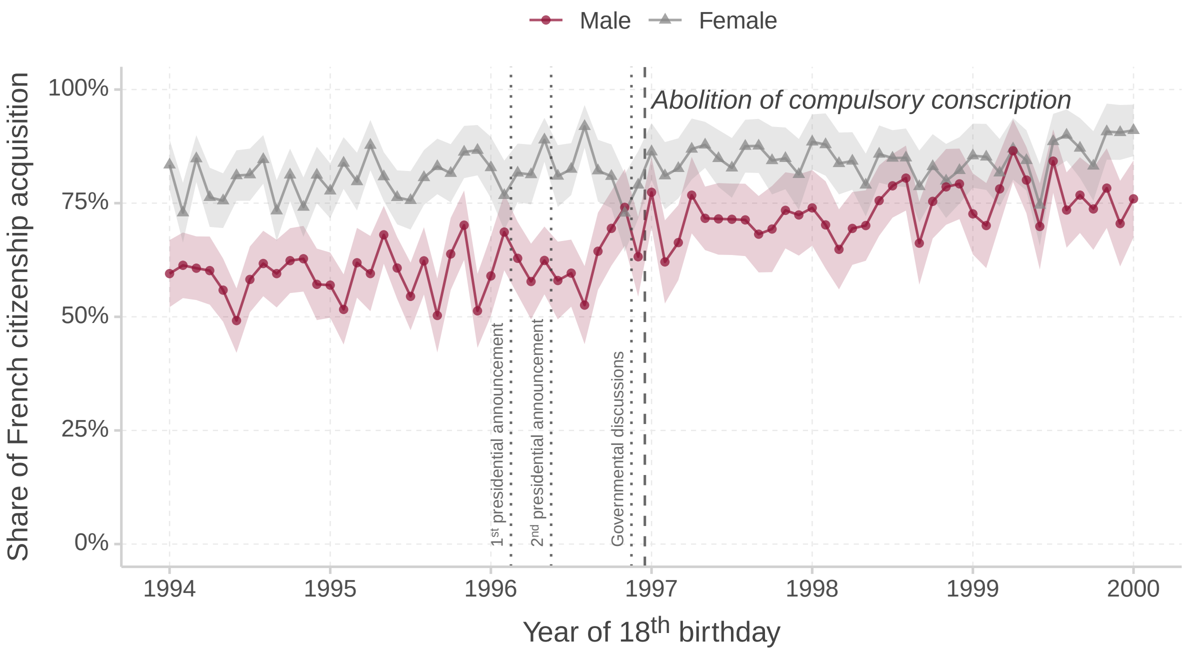
Notes: This figure represents the share of educational attainment -high school on the left panel and higher education on the right panel- for individuals born in France separately by gender, birth cohort, and for four birth nationality groups: French nationality, European Union nationalities, other European nationalities, and African nationalities. European Union is defined as it was in 1996. Each point represents the difference between the share of a given educational attainment in the corresponding birth cohort and in the 1974 birth cohort, following Maurin and Xenogiani (2007). Vertical bars represent the 95% confidence intervals, and the vertical dashed lines represent the moment of the abolition of compulsory conscription. Source: *French Population Census*, wave 2014, INSEE.

Figure C.14: Rates of presence in the 2014 census among 1990 and 1999 census populations



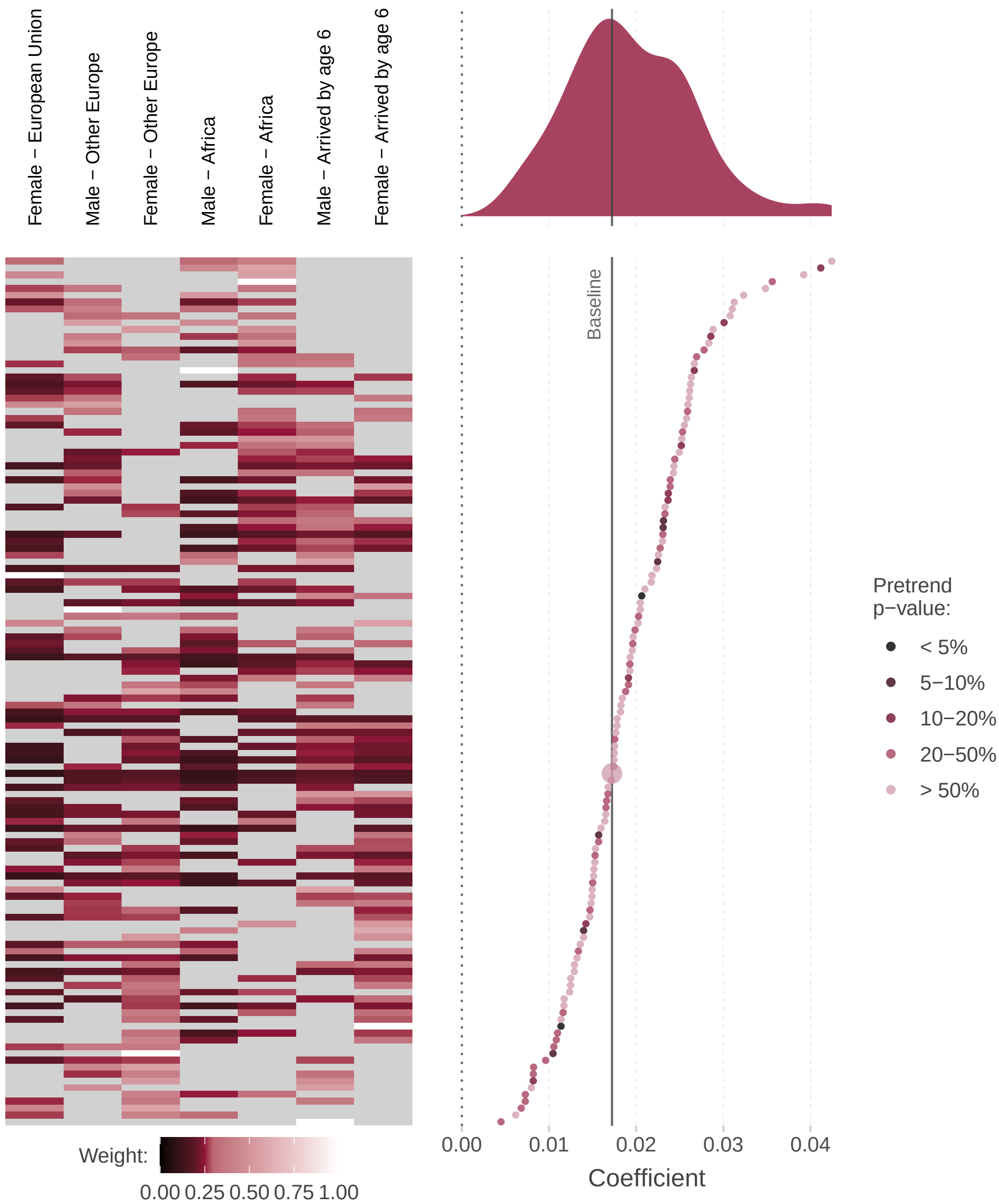
Notes: This figure represents the share of individual observed in the 2014 census wave among individuals observed in the 1990 census wave (top panel) and in the 1999 census wave (lower panel), separately for males (red) and females (gray) and for birth cohorts from 1969 to 1988. The x-axis is labeled according to the year of 18$^{\text{th}}$ birthday, which is when males born before 1979 must decide whether to do military service or to take up French citizenship. The vertical dashed line represents the abolition of compulsory conscription. Computations are made on the Permanent Demographic Sample, in which census information for the 1990 and 1999 waves is available for individuals born during the first 4 days of October. Missing information for some groups in specific years is due to insufficient sample size. Source: *Permanent Demographic Sample*, INSEE-DGFiP.

Figure C.15: Anticipation of the reform - Naturalization rates by month of birth



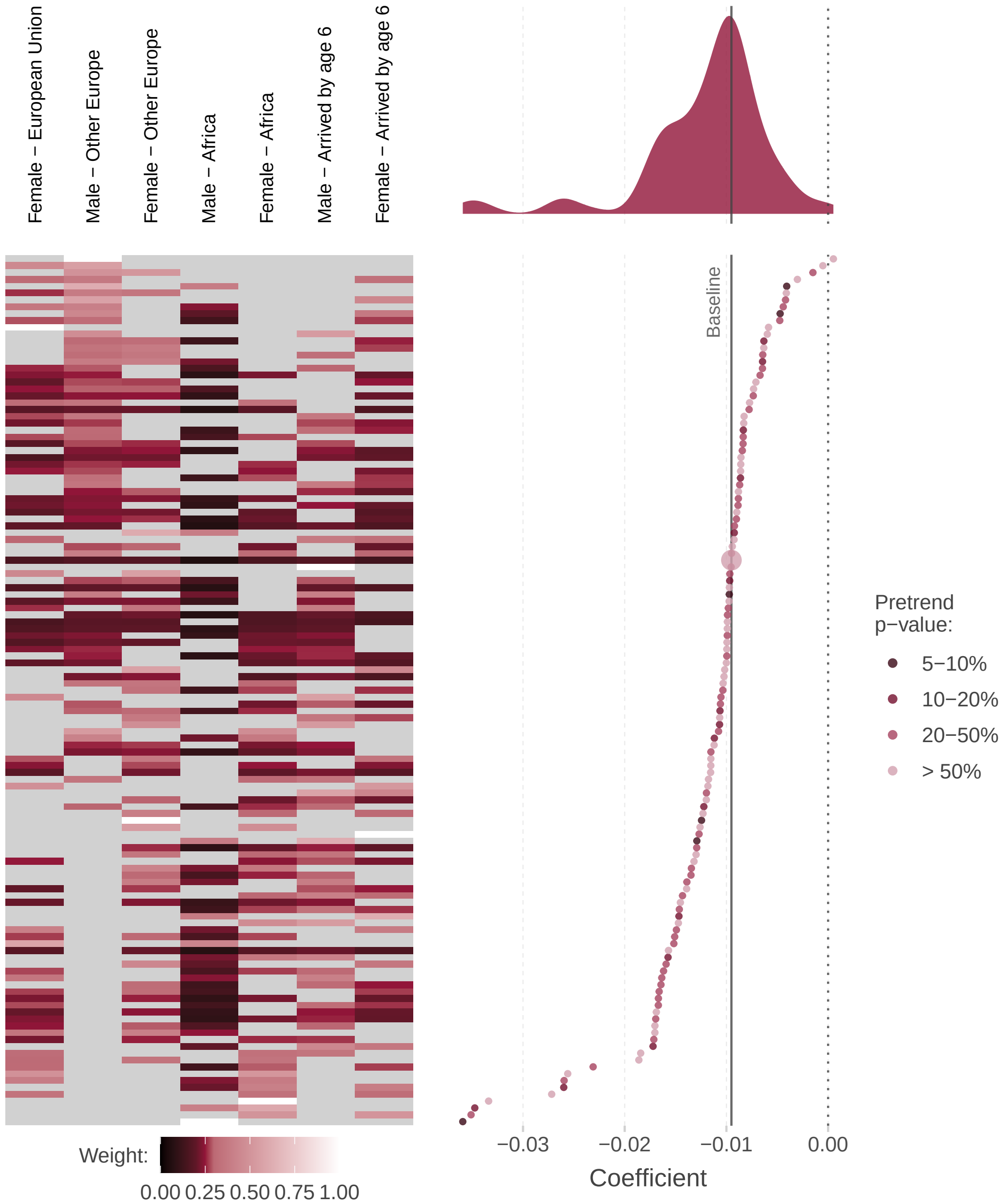
Notes: This figure represents the share of French citizenship acquisition among individuals born in France without French citizenship, separately for males (red) and females (gray), for each month of birth from January 1976 to January 1982. The x-axis is labeled according to the month of 18$^{\text{th}}$ birthday. 95% confidence intervals are represented with light ribbons. The vertical dashed line represents the abolition of compulsory conscription. For males, after this point, citizenship acquisition is not tied to doing military service anymore. The vertical dotted lines represent milestones in the implementation of the reform, from the 1$^{\text{st}}$ presidential announcement to the final governmental discussions. Source: *French Population Census*, wave 2014, INSEE.

Figure C.16: Employment ITT - Every combination of control groups



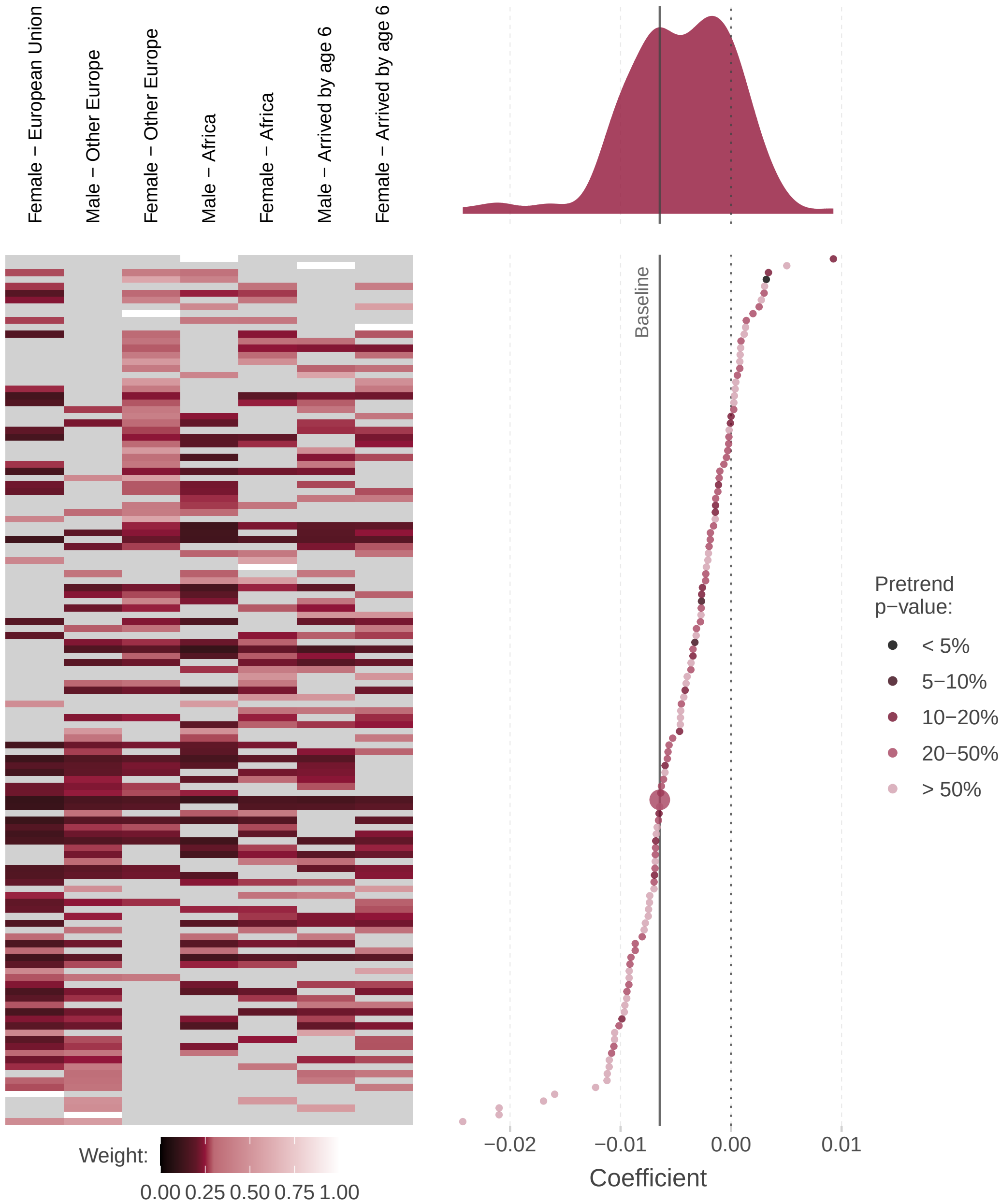
Notes: This figure shows the results of Synthetic Difference-in-Differences estimations of the effect of the increase in the naturalization rates among EU males, induced by the abolition of compulsory conscription, on the share of employed individuals. Specifically, it shows the ITT effect on the share of employed individuals estimated for each of the 127 possible combinations formed by sets of 1 to 7 control groups to generate the synthetic control group. Each row of the tile plot corresponds to a given estimation. A tile whose color lies on the gradient in the legend indicates that the control group of the corresponding column was included in the computation of the synthetic control group, while a gray tile indicates that it has been excluded. The specification which includes the whole set of 7 control groups is our baseline specification. Specifications are sorted by the value of the corresponding ITT coefficient reported on the scatter plot on the right panel. The color of each dot indicates the p-value of the pre-trend. To obtain this value, we compute the difference between the average outcome in the treated group and that in the synthetic control group, and regress it on the birth cohort for the years before the reform. The p-value is then computed from a standard two-sided t-test on the slope coefficient. The vertical solid line represents our baseline coefficient, and a vertical dashed line is placed at 0. The kernel density of this distribution of 127 coefficients is shown on the top panel. Source: *French Population Census*, wave 2014, INSEE.

Figure C.17: Inactive ITT - Every combination of control groups



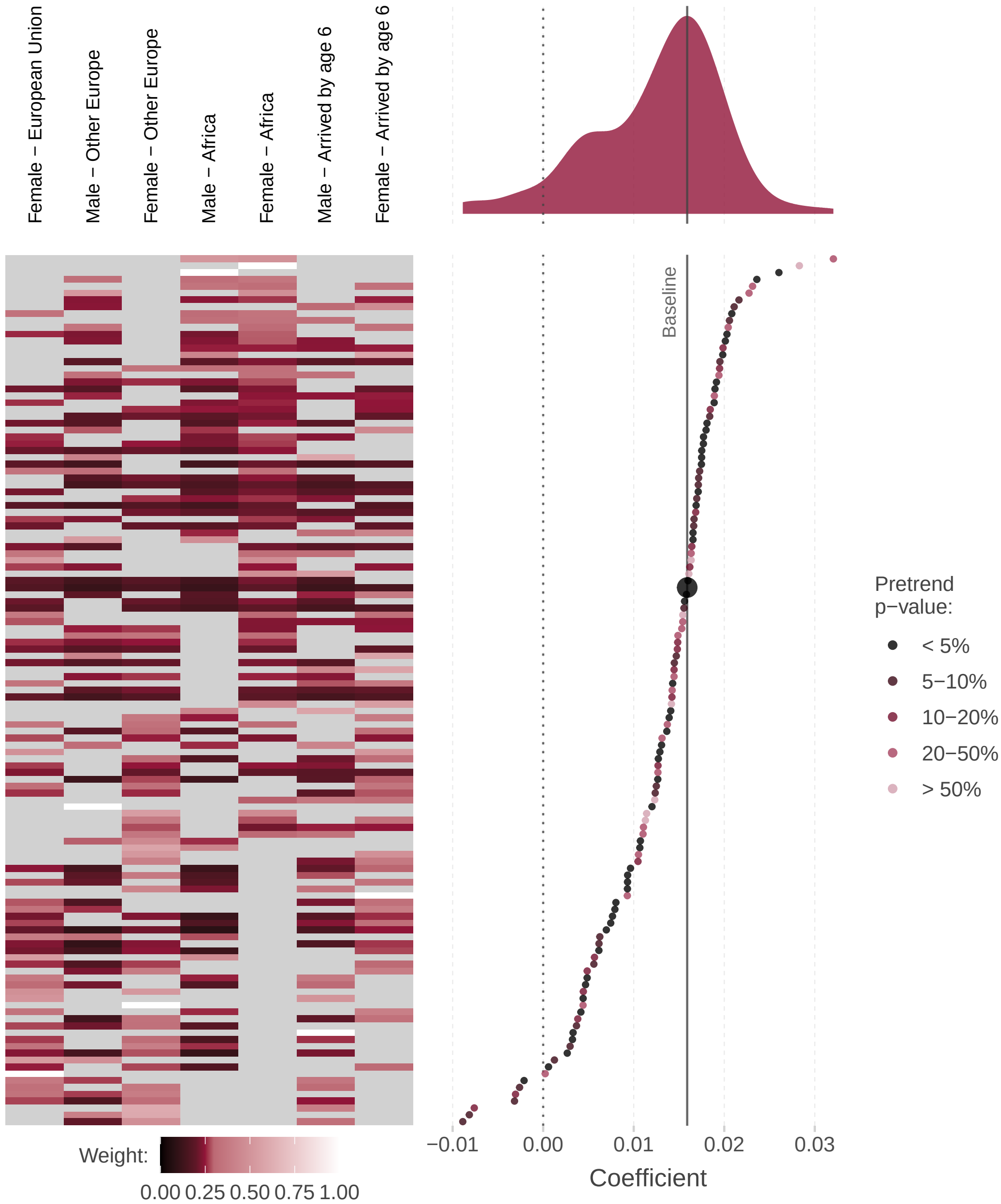
Notes: This figure shows the results of Synthetic Difference-in-Differences estimations of the effect of the increase in the naturalization rates among EU males, induced by the abolition of compulsory conscription, on the share of inactive individuals. Specifically, it shows the ITT effect on the share of inactive individuals estimated for each of the 127 possible combinations formed by sets of 1 to 7 control groups to generate the synthetic control group. Each row of the tile plot corresponds to a given estimation. A tile whose color lies on the gradient in the legend indicates that the control group of the corresponding column was included in the computation of the synthetic control group, while a gray tile indicates that it has been excluded. The specification which includes the whole set of 7 control groups is our baseline specification. Specifications are sorted by the value of the corresponding ITT coefficient reported on the scatter plot on the right panel. The color of each dot indicates the p-value of the pre-trend. To obtain this value, we compute the difference between the average outcome in the treated group and that in the synthetic control group, and regress it on the birth cohort for the years before the reform. The p-value is then computed from a standard two-sided t-test on the slope coefficient. The vertical solid line represents our baseline coefficient, and a vertical dashed line is placed at 0. The kernel density of this distribution of 127 coefficients is shown on the top panel. Source: *French Population Census*, wave 2014, INSEE.

Figure C.18: Unemployed ITT - Every combination of control groups



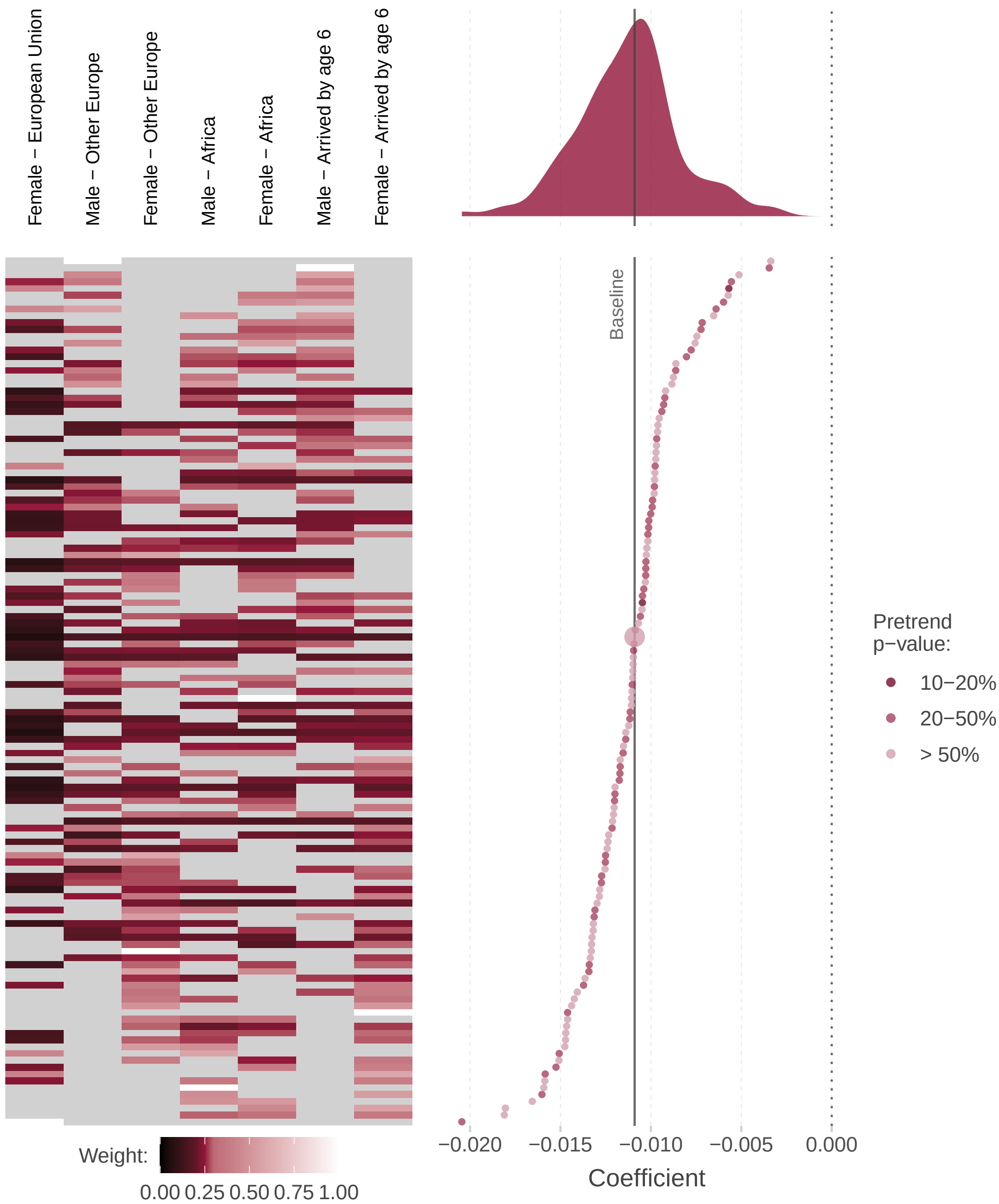
Notes: This figure shows the results of Synthetic Difference-in-Differences estimations of the effect of the increase in the naturalization rates among EU males, induced by the abolition of compulsory conscription, on the share of unemployed individuals. Specifically, it shows the ITT effect on the share of unemployed individuals estimated for each of the 127 possible combinations formed by sets of 1 to 7 control groups to generate the synthetic control group. Each row of the tile plot corresponds to a given estimation. A tile whose color lies on the gradient in the legend indicates that the control group of the corresponding column was included in the computation of the synthetic control group, while a gray tile indicates that it has been excluded. The specification which includes the whole set of 7 control groups is our baseline specification. Specifications are sorted by the value of the corresponding ITT coefficient reported on the scatter plot on the right panel. The color of each dot indicates the p-value of the pre-trend. To obtain this value, we compute the difference between the average outcome in the treated group and that in the synthetic control group, and regress it on the birth cohort for the years before the reform. The p-value is then computed from a standard two-sided t-test on the slope coefficient. The vertical solid line represents our baseline coefficient, and a vertical dashed line is placed at 0. The kernel density of this distribution of 127 coefficients is shown on the top panel. Source: *French Population Census*, wave 2014, INSEE.

Figure C.19: Public job ITT - Every combination of control groups



Notes: This figure shows the results of Synthetic Difference-in-Differences estimations of the effect of the increase in the naturalization rates among EU males, induced by the abolition of compulsory conscription, on public-sector employment. Specifically, it shows the ITT effect on the share of public-sector employment estimated for each of the 127 possible combinations formed by sets of 1 to 7 control groups to generate the synthetic control group. Each row of the tile plot corresponds to a given estimation. A tile whose color lies on the gradient in the legend indicates that the control group of the corresponding column was included in the computation of the synthetic control group, while a gray tile indicates that it has been excluded. The specification which includes the whole set of 7 control groups is our baseline specification. Specifications are sorted by the value of the corresponding ITT coefficient reported on the scatter plot on the right panel. The color of each dot indicates the p-value of the pre-trend. To obtain this value, we compute the difference between the average outcome in the treated group and that in the synthetic control group, and regress it on the birth cohort for the years before the reform. The p-value is then computed from a standard two-sided t-test on the slope coefficient. The vertical solid line represents our baseline coefficient, and a vertical dashed line is placed at 0. The kernel density of this distribution of 127 coefficients is shown on the top panel. Source: *French Population Census*, wave 2014, INSEE.

Figure C.20: Self-employed ITT - Every combination of control groups



Notes: This figure shows the results of Synthetic Difference-in-Differences estimations of the effect of the increase in the naturalization rates among EU males, induced by the abolition of compulsory conscription, on self-employment. Specifically, it shows the ITT effect on the share of self-employment estimated for each of the 127 possible combinations formed by sets of 1 to 7 control groups to generate the synthetic control group. Each row of the tile plot corresponds to a given estimation. A tile whose color lies on the gradient in the legend indicates that the control group of the corresponding column was included in the computation of the synthetic control group, while a gray tile indicates that it has been excluded. The specification which includes the whole set of 7 control groups is our baseline specification. Specifications are sorted by the value of the corresponding ITT coefficient reported on the scatter plot on the right panel. The color of each dot indicates the p-value of the pre-trend. To obtain this value, we compute the difference between the average outcome in the treated group and that in the synthetic control group, and regress it on the birth cohort for the years before the reform. The p-value is then computed from a standard two-sided t-test on the slope coefficient. The vertical solid line represents our baseline coefficient, and a vertical dashed line is placed at 0. The kernel density of this distribution of 127 coefficients is shown on the top panel. Source: *French Population Census*, wave 2014, INSEE.

C.2 Additional Tables

Table C.1: Share of the most represented birth nationalities of individuals born in France without French citizenship

European Union		Other Europe		Africa		Other	
Portugal	33.69%	Turkey	7.05%	Morocco	15.97%	Cambodia	0.84%
Spain	6.34%	Serbia	2.01%	Tunisia	7.61%	Lao	0.72%
Italy	6.34%	Poland	0.72%	Algeria	4.07%	Vietnam	0.53%
Belgium	0.72%	Switzerland	0.66%	Cameroon	0.85%	Haiti	0.52%
UK	0.68%	Romania	0.32%	Congo	0.70%	China	0.51%
Other	1.29%	Other	1.03%	Other	3.89%	Other	2.92%
Total	49.06%	Total	11.80%	Total	33.11%	Total	6.04%

Notes: This table reports the birth nationality composition of our sample: individuals born in France without French citizenship from 1969 to 1988. Note that Algerians are under-represented in our sample because most of them acquired French citizenship at birth under the principle of the "double jus soli". Following the independence of Algeria, this principle applies to individuals born after January 1st 1963 to a parent born in Algeria before July 3rd 1962. European Union is defined as it was in 1996. Source: *French Population Census*, wave 2014, INSEE.

Table C.2: Effect of the abolition of compulsory conscription on naturalization

	French citizenship acquisition
Male	-0.152^{***}
	(0.004)
Post	0.027^{***}
	(0.004)
Post×Male	0.077^{***}
	(0.006)
Constant	0.837^{***}
	(0.003)
Observations	70,117
Mean dep. var.	0.794
R^2	0.029

Notes: This table shows the results of a Difference-in-Differences regression which captures the effect of the abolition of compulsory conscription on French citizenship acquisition. It is estimated on individuals born in France without French citizenship between 1974 and 1983. The dependent variable, French citizenship acquisition, is a dummy variable taking the value 1 if the individual acquired French citizenship and 0 otherwise. The variable Male is a dummy variable taking the value 1 for males and 0 for females. The variable Post is a dummy variable taking the value 1 for individuals born after 1978, i.e., for cohorts not subject to compulsory conscription, and 0 otherwise. Hence, the interaction Post × Male is also a dummy variable taking the value 1 for males born after 1978 and 0 otherwise. Standard errors are reported in parentheses, and significance level is reported according to the following symbology. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. Source: *French Population Census*, wave 2014, INSEE.

Table C.3: Effect of the abolition of compulsory conscription on naturalization by education and birth nationality group

	French citizenship acquisition					
	European Union		Other Europe		Africa	
	Low edu.	High edu.	Low edu.	High edu.	Low edu.	High edu.
Male	-0.222^{***}	-0.128^{***}	-0.107^{***}	-0.036^{*}	-0.062^{***}	-0.065^{***}
	(0.008)	(0.008)	(0.014)	(0.020)	(0.010)	(0.009)
Post	0.022^{**}	-0.014^{*}	0.050^{***}	0.036^{*}	0.028^{***}	-0.005
	(0.010)	(0.008)	(0.014)	(0.019)	(0.010)	(0.008)
Post×Male	0.127^{***}	0.077^{***}	0.024	-0.018	0.019	-0.001
	(0.014)	(0.012)	(0.019)	(0.028)	(0.014)	(0.012)
Constant	0.753^{***}	0.892^{***}	0.833^{***}	0.840^{***}	0.825^{***}	0.930^{***}
	(0.006)	(0.005)	(0.010)	(0.014)	(0.007)	(0.006)
Observations	20,220	14,539	6,383	2,883	12,554	10,016
Mean dep. var.	0.659	0.845	0.813	0.838	0.814	0.902
R^2	0.048	0.020	0.022	0.006	0.007	0.011
F-stat.	336	101	47	5	30	39

Notes: This table shows the results of Difference-in-Differences regressions which capture the effect of the abolition of compulsory conscription on French citizenship acquisition. It is estimated on individuals born in France without French citizenship between 1974 and 1983, separately for 6 groups defined according to birth nationality (European Union, Other Europe, and Africa) and education (up to high school and above high school). European Union is defined as it was in 1996. The dependent variable, French citizenship acquisition, is a dummy variable taking the value 1 if the individual acquired French citizenship and 0 otherwise. The variable Male is a dummy variable taking the value 1 for males and 0 for females. The variable Post is a dummy variable taking the value 1 for individuals born after 1978, i.e., for cohorts not subject to compulsory conscription, and 0 otherwise. Hence, the interaction Post × Male is also a dummy variable taking the value 1 for males born after 1978 and 0 otherwise. Standard errors are reported in parentheses, and significance level is reported according to the following symbology. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. Source: *French Population Census*, wave 2014, INSEE.

Table C.4: Effect of the abolition of compulsory conscription on naturalization by education and birth nationality group - Robustness with European Union as it was before and after the 1995 European Union expansion

	French citizenship acquisition			
	European Union as in 1996		European Union as in 1994	
	Low edu.	High edu.	Low edu.	High edu.
Male	-0.222^{***}	-0.128^{***}	-0.222^{***}	-0.127^{***}
	(0.008)	(0.008)	(0.008)	(0.008)
Post	0.022^{**}	-0.014^{*}	0.023^{**}	-0.012
	(0.010)	(0.008)	(0.010)	(0.008)
Post×Male	0.127^{***}	0.077^{***}	0.126^{***}	0.078^{***}
	(0.014)	(0.012)	(0.014)	(0.012)
Constant	0.753^{***}	0.892^{***}	0.753^{***}	0.892^{***}
	(0.006)	(0.005)	(0.006)	(0.005)
Observations	20,220	14,539	20,198	14,484
Mean dep. var.	0.659	0.845	0.659	0.847
R^2	0.048	0.020	0.048	0.020
F-stat.	336	101	336	100

Notes: This table shows the results of Difference-in-Differences regressions which capture the effect of the abolition of compulsory conscription on French citizenship acquisition. It is estimated on individuals born in France with a non-French European Union citizenship between 1974 and 1983, for two definitions of the European Union, before and after the inclusion of Austria, Finland and Sweden in 1995, and separately for individuals whose education level is up to high school and individuals whose education level is above high school. The dependent variable, French citizenship acquisition, is a dummy variable taking the value 1 if the individual acquired French citizenship and 0 otherwise. The variable Male is a dummy variable taking the value 1 for males and 0 for females. The variable Post is a dummy variable taking the value 1 for individuals born after 1978, i.e., for cohorts not subject to compulsory conscription, and 0 otherwise. Hence, the interaction Post × Male is also a dummy variable taking the value 1 for males born after 1978 and 0 otherwise. Standard errors are reported in parentheses, and significance level is reported according to the following symbology. *p<0.1; **p<0.05; ***p<0.01. Source: *French Population Census*, wave 2014, INSEE.

Table C.5: Effect of the abolition of compulsory conscription on naturalization on European Union citizens

	French citizenship acquisition		
	(1)	(2)	(3)
Male	-0.209^{***}	-0.209^{***}	-0.187^{***}
	(0.006)	(0.006)	(0.006)
Post	0.015^{**}	0.050^{***}	0.037^{***}
	(0.007)	(0.009)	(0.009)
Post×Male	0.115^{***}	0.114^{***}	0.119^{***}
	(0.009)	(0.009)	(0.009)
Age		0.007^{***}	0.007^{***}
		(0.001)	(0.001)
Higher Education			0.161^{***}
			(0.005)
Constant	0.820^{***}	0.559^{***}	0.464^{***}
	(0.004)	(0.043)	(0.042)
Observations	34,759	34,759	34,759
Mean dep. var.	0.741	0.741	0.741
R^2	0.047	0.048	0.080
F-stat.	566	434	604

Notes: This table shows the results of Difference-in-Differences regressions which capture the effect of the abolition of compulsory conscription on French citizenship acquisition, progressively including controls for age and education. It is estimated on individuals born in France with a non-French European Union citizenship between 1974 and 1983. European Union is defined as it was in 1996. The dependent variable, French citizenship acquisition, is a dummy variable taking the value 1 if the individual acquired French citizenship and 0 otherwise. The variable Male is a dummy variable taking the value 1 for males and 0 for females. The variable Post is a dummy variable taking the value 1 for individuals born after 1978, i.e., for cohorts not subject to compulsory conscription, and 0 otherwise. Hence, the interaction Post × Male is also a dummy variable taking the value 1 for males born after 1978 and 0 otherwise. Standard errors are reported in parentheses, and significance level is reported according to the following symbology. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. Source: *French Population Census*, wave 2014, INSEE.

Table C.6: Effect of the abolition of compulsory conscription on citizenship acquisition

	Citizenship acquisition
ITT ($\hat{\tau}$)	0.107^{***}
	(0.015)
Group weights ($\hat{\omega}_g$)	
Female - European Union	0.065
Male - Other Europe	0.180
Female - Other Europe	0.334
Male - Africa	0.015
Female - Africa	0.158
Male - Arrived by age 6	0.140
Female - Arrived by age 6	0.108
Time weights ($\hat{\lambda}_t$)	
1992	0.000
1993	0.000
1994	0.000
1995	0.000
1996	1.000
Age	✓
Education	✓
Observations	106,081
Mean dep. var.	0.753

Notes: This table reports the results of a Synthetic Difference-in-Differences which estimates the effect of the abolition of compulsory conscription on naturalization rates among EU males. The first row displays the Intention-to-treat effect, estimated with the Synthetic Difference-in-Differences approach described in Section 3.5.1. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. Group and time weights are computed following Arkhangelsky et al. (2021). Education and age are controlled for. Source: *French Population Census*, wave 2014, INSEE.

Table C.7: Effect of naturalization on public-sector jobs

	Public job
ITT ($\hat{\tau}$)	0.016**
	(0.007)
LATE ($\hat{\tau}/\hat{\gamma}$)	0.133**
	(0.063)
Group weights ($\hat{\omega}_g$)	
Female - European Union	0.166
Male - Other Europe	0.111
Female - Other Europe	0.153
Male - Africa	0.114
Female - Africa	0.188
Male - Arrived by age 6	0.120
Female - Arrived by age 6	0.148
Time weights ($\hat{\lambda}_t$)	
1992	0.000
1993	0.418
1994	0.582
1995	0.000
1996	0.000
Age	✓
Education	✓
Observations	106,081
Mean dep. var.	0.094
First-stage F-stat.	604
Share of compliers ($\hat{\gamma}$)	11.91%

Notes: This table reports the results of a Synthetic Difference-in-Differences which estimates the effect of the increase in the naturalization rates among EU males, induced by the abolition of compulsory conscription, on public-sector jobs. The first row displays the Intention-to-treat effect, estimated with the Synthetic Difference-in-Differences approach described in Section 3.5.1. The second row shows the Local Average Effect, computed as the ITT divided by the share of compliers. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. *p<0.1; **p<0.05; ***p<0.01. Group and time weights are computed following Arkhangelsky et al. (2021). Education and age are controlled for. The F-statistic of the first stage and the share of compliers estimated in Appendix Table C.5 are reported at the bottom of the table. Source: *French Population Census*, wave 2014, INSEE.

Table C.8: Effect of naturalization on self-employment

	Self-employed
ITT ($\hat{\tau}$)	-0.011^{*}
	(0.008)
LATE ($\hat{\tau}/\hat{\gamma}$)	-0.091^{*}
	(0.065)
Group weights ($\hat{\omega}_g$)	
Female - European Union	0.055
Male - Other Europe	0.152
Female - Other Europe	0.159
Male - Africa	0.162
Female - Africa	0.149
Male - Arrived by age 6	0.158
Female - Arrived by age 6	0.165
Time weights ($\hat{\lambda}_t$)	
1992	0.305
1993	0.185
1994	0.001
1995	0.507
1996	0.001
Age	✓
Education	✓
Observations	106,081
Mean dep. var.	0.074
First-stage F-stat.	604
Share of compliers ($\hat{\gamma}$)	11.91%

Notes: This table reports the results of a Synthetic Difference-in-Differences which estimates the effect of the increase in the naturalization rates among EU males, induced by the abolition of compulsory conscription, on self-employment. The first row displays the Intention-to-treat effect, estimated with the Synthetic Difference-in-Differences approach described in Section 3.5.1. The second row shows the Local Average Effect, computed as the ITT divided by the share of compliers. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. *p<0.1; **p<0.05; ***p<0.01. Group and time weights are computed following Arkhangelsky et al. (2021). Education and age are controlled for. The F-statistic of the first stage and the share of compliers estimated in Appendix Table C.5 are reported at the bottom of the table. Source: *French Population Census*, wave 2014, INSEE.

Table C.9: Robustness of the ITT to the exclusion of the 1978 birth cohort

	With time weights		Without time weights	
	Including $t-1$	Excluding $t-1$	Including $t-1$	Excluding $t-1$
Employed	0.017^{**}	0.016^{**}	0.023^{***}	0.021^{***}
	(0.009)	(0.009)	(0.007)	(0.008)
Unemployed	-0.006	-0.007	-0.007	-0.006
	(0.008)	(0.008)	(0.006)	(0.007)
Inactive	-0.010^{*}	-0.009^{*}	-0.015^{***}	-0.014^{***}
	(0.006)	(0.006)	(0.004)	(0.005)
Public job	0.016^{**}	0.016^{**}	0.016^{***}	0.015^{***}
	(0.007)	(0.008)	(0.005)	(0.006)
Self-employed	-0.011^{*}	-0.011^{*}	-0.012^{**}	-0.012^{**}
	(0.008)	(0.008)	(0.006)	(0.006)

Notes: This table reports the results of Synthetic Difference-in-Differences specifications which estimate the ITT effect of the increase in the naturalization rate of males born European Union citizens in France, induced by the abolition of compulsory conscription, on several labor market outcomes. The first two columns report the estimate for each outcome variable using the time weights as in Arkhangelsky et al. (2021), while the last two columns show the estimates resulting from an equal weighting of each birth cohort. The first column of each of these two sets includes all birth cohorts in the pre-period, while the second one is estimated without the 1978 birth cohort, i.e., the last one before the reform, which is potentially subject to anticipation effects. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. *p$<$0.1; **p$<$0.05; ***p$<$0.01. Source: *French Population Census*, wave 2014, INSEE.

Table C.10: Sensitivity of baseline results to the share of natives in the employment zone

	ITT		LATE	
	(1)	(2)	(3)	(4)
Employed	0.017**	0.017**	0.145**	0.145**
	(0.009)	(0.009)	(0.079)	(0.078)
Unemployed	−0.006	−0.006	−0.054	−0.054
	(0.008)	(0.008)	(0.069)	(0.067)
Inactive	−0.010*	−0.010*	−0.080*	−0.080*
	(0.006)	(0.006)	(0.052)	(0.051)
Public job	0.016**	0.016**	0.133**	0.134**
	(0.007)	(0.007)	(0.063)	(0.065)
Self-employed	−0.011*	−0.011*	−0.091*	−0.091*
	(0.008)	(0.008)	(0.065)	(0.066)
Age	✓	✓	✓	✓
Education	✓	✓	✓	✓
%Natives in ZE		✓		✓

Notes: This table reports the results of the ITT effect and LATE of the increase in the naturalization rate of males born European Union citizens in France, induced by the abolition of compulsory conscription, on several labor market outcomes, with and without controlling for the share of natives in individuals' employment zone. ITTs are estimated with Synthetic Difference-in-Differences. LATEs are computed as the ITT divided by the share of compliers, estimated with Difference-in-Differences. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. *p<0.1; **p<0.05; ***p<0.01. Source: *French Population Census*, wave 2014, INSEE.

Table C.11: Robustness of the LATE to the exclusion of the 1978 birth cohort

	With time weights		Without time weights	
	Including $t-1$	Excluding $t-1$	Including $t-1$	Excluding $t-1$
Employed	0.145^{**}	0.135^{**}	0.196^{***}	0.180^{***}
	(0.079)	(0.075)	(0.064)	(0.068)
Unemployed	-0.054	-0.055	-0.055	-0.046
	(0.069)	(0.068)	(0.053)	(0.061)
Inactive	-0.080^{*}	-0.072^{*}	-0.123^{***}	-0.116^{***}
	(0.052)	(0.049)	(0.039)	(0.041)
Public job	0.133^{**}	0.135^{**}	0.138^{***}	0.128^{***}
	(0.063)	(0.065)	(0.047)	(0.051)
Self-employed	-0.091^{*}	-0.091^{*}	-0.104^{**}	-0.099^{**}
	(0.065)	(0.066)	(0.053)	(0.056)

Notes: This table reports the results of the local average treatment effect of the increase in the naturalization rate of males born European Union citizens in France, induced by the abolition of compulsory conscription, on several labor market outcomes. LATEs are computed as the ITT, estimated with Synthetic Difference-in-Differences, divided by the share of compliers, estimated with Difference-in-Differences. The first two columns report the estimate for each outcome variable using the time weights as in Arkhangelsky et al. (2021), while the last two columns show the estimates resulting from an equal weighting of each birth cohort. The first column of each of these two sets includes all birth cohorts in the pre-period, while the second one is estimated without the 1978 birth cohort, i.e., the last one before the reform, which is potentially subject to anticipation effects. Bootstrapped standard errors are reported in parentheses, and statistical significance is reported according to the following symbology. *p<0.1; **p<0.05; ***p<0.01. Source: *French Population Census*, wave 2014, INSEE.

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