

Essays in Labor Economics and Intergenerational Mobility

by

Pietro Campa

Submitted to the *Geneva School of Economics and Management*

in partial fulfillment of the requirements for the degree of

Ph.D. in Economics

at the

University of Geneva

September 2025

Author: Pietro Campa, University of Geneva

Main Advisor: Prof. Giacomo De Giorgi, University of Geneva

Co-Advisor: Prof. Frédéric Robert-Nicoud, University of Geneva

Committee Members: Prof. Christophe Gaillac, University of Geneva

Prof. Enrico Moretti, University of California, Berkeley

THESIS SUPERVISOR:

Giacomo De Giorgi

Professor of Economics

University of Geneva

THESIS CO-SUPERVISOR:

Frédéric Robert-Nicoud

Professor of Economics

University of Geneva

COMMITTEE CHAIR:

Christophe Gaillac

Professor of Economics

University of Geneva

EXTERNAL ADVISOR:

Enrico Moretti

Professor of Economics

University of California, Berkeley

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Preface: This dissertation comprises three chapters exploring the impacts of initial conditions, such as parental background or environmental factors in childhood, on lifetime economic outcomes. Such initial conditions are often unrelated to individual effort or ability. Yet, they profoundly shape access to opportunities, raising fundamental questions about fairness and efficiency.

Why do differences in parental background translate so persistently into disparities in lifetime outcomes? How do these early circumstances position one child for success while creating major obstacles for another? Addressing these questions is key to critical discussion and effective design of policies aimed at promoting individual well-being and social progress. Ultimately, this thesis is driven by curiosity about the roots of economic inequality and enthusiasm for the role scientific evidence plays in improving our societies.

The first chapter explores how segregation in school-based social networks mirrors parental income disparities, reinforcing the intergenerational transmission of economic inequality. The second chapter investigates the long-term effects of early-life credit access, demonstrating that disparities in borrowing capacity persist over time and are strongly associated with parents' access to credit. The third chapter examines how early-life credit constraints substantially limit access to financial resources later in life.

Essais en Économie du Travail et Mobilité Intergénérationnelle

par

Pietro Campa

Soumis à l'Université de Genève en septembre 2025

en vue de l'obtention du grade de

Ph.D. in ECONOMICS.

Préface: Cette dissertation comprend trois chapitres qui explorent les impacts des conditions initiales, telles que le milieu parental ou les facteurs environnementaux durant l'enfance, sur les résultats économiques tout au long de la vie. Ces conditions initiales sont souvent indépendantes de l'effort ou des capacités individuelles. Pourtant, elles influencent profondément l'accès aux opportunités, soulevant des questions fondamentales de justice et d'efficacité.

Pourquoi les différences de milieu parental se traduisent-elles de manière aussi persistante par des disparités dans les résultats de vie? Comment ces circonstances précoce placent-elles certains enfants sur la voie du succès tout en créant d'importants obstacles pour d'autres? Répondre à ces questions est essentiel pour une réflexion critique et la conception efficace de politiques visant à promouvoir le bien-être individuel et le progrès social. Cette thèse est animée par la curiosité sur les racines des inégalités économiques et par l'enthousiasme pour le rôle que joue la preuve scientifique dans l'amélioration de nos sociétés.

Le premier chapitre analyse comment la ségrégation au sein des réseaux sociaux scolaires reflète les disparités de revenus parentaux, renforçant ainsi la transmission intergénérationnelle des inégalités économiques. Le deuxième chapitre étudie les effets à long terme de l'accès au crédit durant la petite enfance, démontrant que les disparités dans la capacité d'emprunt persistent dans le temps et sont fortement associées à l'accès au crédit des parents. Le troisième chapitre examine comment les contraintes de crédit en bas âge limitent substantiellement l'accès aux ressources financières à l'âge adulte.

“The word *timshel* -‘Thou mayest’- that gives a choice.
It might be the most important word in the world.
That says the way is open.”

—*East of Eden*, John Steinbeck

Acknowledgements

I am deeply grateful to the Geneva School of Economics and Management and to the faculty members at the Institute for Economics and Econometrics for the extensive support I received—financially, intellectually, and personally—throughout my doctoral studies.

I feel most grateful to my thesis advisor, Prof. Giacomo De Giorgi, whose commitment to critical thinking and research excellence gave me the opportunity to grow through conversations with a truly inspiring mentor. His influence is reflected in much of what I have learned about research and in the way I engage in intellectual inquiry.

I am also sincerely grateful to my thesis co-advisor, Prof. Frédéric R. Nicoud, whose intellectual rigor and generosity encouraged me to explore dimensions of economic theory I might not have otherwise encountered.

I am especially grateful to Prof. Enrico Moretti for welcoming me to UC Berkeley during the final stages of my Ph.D. His hospitality gave me the rare opportunity to engage directly at the frontier of economic research and to broaden the scope of my work through generous, critical, and inspiring discussions.

I also wish to thank Prof. Joan Llull and the Universidad Autónoma de Barcelona and Prof. Mauricio Prado and Prof. Battista Severgnini at Copenhagen Business School for their hospitality and critical support during the early stages of developing my research agenda.

Finally, I want to aknowledge the support of Prof. Julien X. Daubanes, who turned our teacher-teaching assistant collaboration into countless opportunities for mentorship.

I would like to thank Federica for the privilege of exploring this chapter with her by my side: the wise, joyful, and brave human being she is.

I am also fortunate to have shared this journey with many colleagues and friends who supported and enriched my experience throughout these years. Among them: Davide, Andrea, and all the postdocs in my home department in Geneva; Flavia, Pritam, Utsoree, Helena, Aneta, Lorena, Pablo, and Morgane; and all my fellow Ph.D. colleagues. I also warmly thank Federica, Maddalena, Gustave, and the irreplaceable friends and colleagues I met during research exchanges, summer schools, and conferences.

Finally, I would not fully appreciate this journey without acknowledging where it began. I am deeply thankful to my family —Maria Pia, Roberto, and Giovanni— and to my friends Daniele, Francesca, Jacopo, Elisa, Nadia, and especially Saverio. They let me go, supported me as I explored my own path, and welcomed me back each time with the same love and generosity. A final thank you goes to Dr. Soldati, whose support helped me navigate the personal choices and transitions that have shaped these last exciting five years.

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No Kid is an Island: Peer Effects and Intergenerational Mobility

Pietro Campa
University of Geneva
Job Market Paper

Abstract: Economic inequalities persist from one generation to the next. To what extent is this due to children's social interactions replicating parental disparities? This paper studies the role of peers' parental background for social mobility. Exploiting within school across cohort exogenous variation in schoolmates' parental background among Danish high school students, I show that a \$1 increase in average schoolmates' parental earnings results in a \$0.08 increase in adult earnings. This effect is as large as 42% of the parent-child correlation in earnings. I find that former schoolmates are connected on labor market networks: as their career advances, they open doors to higher paying firms and provide more attractive outside options to their peers. While existing research documents own parental characteristics and local economic conditions as crucial, this paper highlights the role of peer exposure in shaping intergenerational mobility, suggesting that interactions among children from diverse backgrounds are key for access to opportunities.

I thank Giacomo De Giorgi, Enrico Moretti and Frederic Robert-Nicoud for precious guidance and support. Gratitude is also extended to Ainoa Aparicio-Fenoll, Pietro Biroli, Sydnee Caldwell, Hilary Hoynes, Patrick Kline, Joan Llull, Michele Pellizzari, Ricardo Perez-Truglia, Jörn-Steffen Pischke, Luigi Pistaferri, Pauline Rossi, Jan Stuhler, Aleksey Tetenov, Michela Tincani, Harrison Wheeler and Danny Yagan for insightful conversations. This project also benefited from constructive comments from participants at numerous conferences and seminars such as AlpPop Conference, IRLE Seminar at UC Berkeley, CEA Annual Meeting, SOLE Annual Meeting, WEAI Annual Meeting, EEA Annual Meeting, EALE Annual Conference, AIEL Conference, the Barcelona School of Economics PhD Jamboree, UniTo-CCA PhD Workshop in Economics, Brucchi-Luchino Labor Economics Workshop and Winter Meeting of the Econometric Society. Special thanks are due to Mauricio Prado, Battista Severgnini, and Copenhagen Business School for their generous support in facilitating access to the data. All errors are my own. I gratefully acknowledge financial support from the GSEM - IEE and the Fonds Général at the University of Geneva.

1.1 Introduction

Parental investments are key to children’s development (Becker and Tomes, 1979; Carneiro et al., 2021). Yet, kids often interact with peers from similar socioeconomic backgrounds, as parents sort across schools and neighborhoods. How does exposure to peers from families of different income levels influence future earnings? While research has explored the impact of local economic conditions on intergenerational mobility (Chetty and Hendren, 2018) and the role of peers in educational achievement (Sacerdote, 2011), much less is known about how social interactions transmit income inequality through generations.

I estimate the impact of exposure to peers from different parental backgrounds exploiting exogenous variation in high school composition across different cohorts as in Hoxby (2000). A \$1 increase in schoolmates’ average parental earnings results in a \$0.08 increase in annual adult earnings. This effect is as large as 42% of the observed parent-child earnings correlation.

Children may benefit in multiple ways from peers with higher-earning parents, who often possess more human capital (Adermon et al., 2021) and better-paying jobs (Dobbin and Zohar, 2023; Forsberg et al., 2024). Exposure to such peers can promote skill spillovers and open doors to higher-paying jobs. I find limited evidence that this exposure boosts educational attainment. Instead, by examining wage and career paths, I show that former schoolmates leverage these connections to enter better-paying firms and benefit from peers’ career advancements.

These findings indicate that social interactions contribute to the persistence of inequalities across generations. Identifying this mechanism is crucial for understanding intergenerational mobility. For instance, disparities in opportunities across social settings, such as neighborhoods (Chetty and Hendren, 2018), may be influenced by peer exposure. These insights highlight the policy relevance of fostering interactions among children from diverse backgrounds to promote access to opportunities.

In the first part of the paper, I estimate the causal effect of peer exposure exploiting variation in school composition as suggested by Hoxby (2000).¹ This approach addresses the key concern of *endogenous sorting* into schools: if students choose schools based on traits correlated with individual outcomes, such as ability, the observed relationship between peers’ parental income and individual earnings could be spurious. To address this concern, I focus on variation among students who attended the same school but in different cohorts. Additionally, to isolate unexpected variation in school composition, I rely on idiosyncratic, cohort-specific deviations from school time trends, which I argue are unanticipated and exogenous from the students’ perspective.²

¹This approach has been extensively applied in the literature. Among others: Black et al. (2013); Carrell et al. (2018); Brenøe and Zölitz (2020); Cattan et al. (2022); Mertz et al. (2024).

²I provide several robustness checks to support this assumption, among others, showing that deviations in cohort composition are uncorrelated with students’ predetermined characteristics.

A second concern relates to the possibility of *correlated shocks* that could influence individual outcomes similarly among group members, independent of social interactions. For instance, an increase in local labor demand could raise both peers' parental earnings and one's own earnings, even without peer effects. I demonstrate that the main results remain robust when controlling for cohort-by-municipality fixed effects, indicating that local economic fluctuations are not driving the findings. In the paper, I address similar concerns and present evidence supporting that *correlated shocks* are not affecting the results.

I find that a \$1 increase in schoolmates' average parental earnings results in a \$0.08 increase in adult yearly earnings. This effect is as large as 42% of the parent-child earnings correlation.³ Children from different parental backgrounds experience a similar exposure effect. However, the effect is nonlinear. The effect of each extra unit of earnings on schoolmates' parental earnings' is decreasing in the level of schoolmates' parental earnings. Thus, a marginal change in peer exposure has a larger effect on adult earnings for children exposed to lower-SES peers.

What is the mechanism driving this effect? To explore whether it is driven by increased educational opportunities or improved access to higher-paying jobs, I analyze the impact of exposure to higher-SES peers on both outcomes. A one-standard-deviation increase in average schoolmates' parental earnings has a small positive effect on the probability of obtaining a college degree (+0.9 p.p., +1.3%) and a more significant effect on the probability of working at higher-paying plants, with a +0.2 p.p. increase (+10%) in the plant-specific wage premium at the age of 30.⁴

Motivated by these findings, I focus on the role of labor market interactions in determining exposure effects in the second part of the paper. I first document stark differences in access to higher-paying firms and occupations by parental background. Next, I show that *social ties* formed among schoolmates persist in the labor market, leading to spillovers of the advantages inherited from their parents.

First, school connections determine access to jobs. Comparing the probability of joining the plant of an *actual* schoolmate versus *almost* schoolmates (i.e. those who attended the same school in the cohort immediately before or after), I find that 1.4% of students join a firm due to a high school social tie.⁵ Moreover, while I find that *connected hires* are more frequent among students from similar parental backgrounds, I document how low-SES students also join high-SES peers' plants because of exposure in high school.

Second, workers benefit from the career advancements of their former schoolmates. As highlighted by Manski (1993), identifying the impact of a peer's achievement (such as a promotion

³The results are robust to alternative measures of individual earnings such as percentile ranks.

⁴Plant-specific wage premia are computed as in Abowd et al. (1999) and Card et al. (2013). They can be interpreted as the percentage increase in wage paid to a worker upon employment at a given plant.

⁵This approach applies to the school setting identification designs that use variation in the timing of employment within a firm to test for the role of coworkers as social ties on the labor market (Hensvik and Skans, 2016; Caldwell and Harmon, 2019; Glitz and Vejlin, 2021).

to a managerial position) is challenging because shared characteristics within the group may influence such an event. To address this challenge, I develop a novel research design that exploits variation in the timing of peers' promotions. While common group characteristics may affect the probability of a promotion, some variation in promotion timing is likely idiosyncratic. Leveraging this exogenous variation in a difference-in-differences framework, I find that a schoolmate's promotion increases peers' hourly wages by \$1.53 in the subsequent years. I present evidence suggesting that this effect is consistent with peers offering outside options triggering wage negotiations, as suggested by job search models featuring on-the-job search (Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Bagger et al., 2014).

This paper contributes to the intergenerational mobility literature by highlighting how social interactions affect access to opportunities. While seminal theoretical works (Benabou, 1993; Durlauf, 1996) underscore the incentives for parents to form homogeneous peer groups due to the impact of their investments on neighboring children,⁶ evidence on the relationship between peer exposure and social mobility is scarce due to extensive data requirements and significant identification challenges.

In a major contribution to address data limitations, Chetty et al. (2022a) collect Facebook friendship data for the U.S. to construct large-scale measures of network segregation, revealing a negative correlation between social network segregation and intergenerational mobility rates across space. My paper contributes to this literature by exploiting a different data source and focusing on high school exposure, showing that exposure to higher-SES peers has a causal effect on adult earnings.

Moreover, an important strand of research examines how the economic opportunities of children are shaped by the neighborhoods they grow up in (Chetty and Hendren, 2018).⁷ I add to this body of work by highlighting social interactions as a key factor in social mobility, distinct from the influence of local economic conditions. Specifically, the findings of this paper suggest that the characteristics of individuals residing in different communities are likely to play a role, as *potential peers*, in determining the disparities in access to opportunities observed across neighborhoods.⁸

Finally, I consider my findings closely related to Cattan et al. (2022), who show that Norwegian high school classmates enhance access to elite colleges if their parents are alumni of those institutions. My paper differs in that it focuses on the transmission of earnings and access to

⁶Consistently, Abdulkadiroglu et al. (2020) and Eshaghnia et al. (2023) show that parents value peer exposure in selecting neighborhoods and schools.

⁷*Neighborhood effects* are identified using plausibly random variation in children's ages at the time of moving, following the approach by Chetty and Hendren (2018), and replicated with similar findings in Africa (Alesina et al., 2021), Australia (Deutscher, 2020), and Israel (Aloni and Avivi, 2024).

⁸Several structural models aim at identifying the role of peer effects in determining social mobility (Fogli and Guerrieri, 2019; Agostinelli et al., 2020; Eckert and Kleineberg, 2021; Chyn and Daruich, 2023). This paper complements this strand of work by developing a research design aimed at identifying the causal effect of interest exploiting a natural experiment.

jobs rather than on education and access to colleges.

The second strand of literature this paper contributes to is the extensive research on the role of social ties in labor markets. Since Granovetter (1983), there has been considerable focus on how social ties among workers facilitate access to jobs, for example, providing potential employers with information on candidates' productivity (Hensvik and Skans, 2016; Glitz and Vejlin, 2021). More recently, a growing body of work is exploring how parents influence access to firms (Kramarz and Skans, 2014; Staiger, 2023), occupations (Ventura, 2024), and higher-paying jobs (Dobbin and Zohar, 2023; Forsberg et al., 2024). This paper adds to this literature by demonstrating how peer exposure affects job access, potentially passing on advantages inherited from parents to schoolmates. This finding is especially relevant in the context of recent work (Calvó-Armengol and Jackson, 2007; Bolte et al., 2024) highlighting how concentrated job referrals arising from segregated social networks reinforce inequalities and constrain productivity.

A third contribution of this paper is the development of a novel identification strategy that leverages the timing of managerial promotions to identify their effects on former schoolmates. This approach addresses an identification issue related to the *reflection problem* formalized by Manski (1993).⁹ Group characteristics could confound the relationship between promotions and peers' wages. Assuming groups with and without promotions would have the same wage growth absent the promotion, the timing differences allow identification of effects independent of group composition in a difference-in-differences framework.¹⁰

This method exploits variation in the timing of the event of interest, while existing approaches typically rely on variation in the probability of such events through instruments (Moffitt, 2001; Lalivé and Cattaneo, 2009; Rossi and Xiao, 2023) or network structure (Bramoullé et al., 2009; De Giorgi et al., 2010). To my knowledge, the closest related exercise is Caldwell and Harmon (2019), who use time variation of a continuous measure of workers' outside options in a model with worker fixed effects. While the event I examine (a peer's promotion) is more narrowly defined than theirs (any job transition among peers), my framework allows testing for potential pre-trends.

This paper's final contribution is identifying long-lasting peer effects stemming from exposure to High School peers. The results of this paper add to a large body of evidence on peer effects. In this direction, the closest results to this paper are in Carrell et al. (2018), who finds evidence of reduced earnings as a consequence of exposure to *disruptive peers* in elementary school and Fruehwirth and Gagete-Miranda (2019), who finds that kindergarteners whose classmates' parents have higher education, have higher educational outcomes.

These results enhance our understanding of social interactions as a key factor in intergen-

⁹Manski (1993) examines the impact of peers' outcomes on the same outcome for an individual, whereas I focus on how a distinct peer outcome (promotion) affects a specific individual outcome (wage).

¹⁰I apply to this setting the empirical specifications and inference techniques from recent advances in difference-in-differences designs with staggered treatments, as in Callaway and Sant'Anna (2021).

erational mobility. On one side, the causal effect of peer exposure identified in this paper and the observed school segregation by parental earnings suggests that social interactions reinforce inequalities. At the same time, this finding can inform policy discussions aimed at improving access to opportunities, such as school desegregation.

The rest of the paper is organized as follows: Section 2 describes the sample and the institutional framework. Section 3 presents the empirical strategy and the results concerning the effect of exposure to schoolmates. Section 4 presents descriptive evidence on the differential access to higher-paying firms by parental background. Section 5 presents the results on labor market networks and the effect of peers' promotions on wages. Finally, section 6 discusses the results, and section 7 concludes.

1.2 Institutional Framework and Sample Description

1.2.1 Institutional Setting: Danish High-Schools

Danish students complete compulsory education by the 9th grade, typically at age 16. Attendance of an extra 10th grade is optional. After finishing lower secondary education, students can choose to either enroll in a high school that grants access to tertiary education, attend a vocational school, or discontinue their education¹¹.

High school programs range from 2 to 3 years, depending on the track chosen, and aim to prepare students for tertiary education or entry into the labor market. They are organized into four principal tracks, each featuring distinct curricula. These tracks are tailored to prepare students for university-level studies or provide more technical and business-oriented training. Each track includes compulsory courses and elective subjects, allowing for some degree of curricular flexibility.

Admission to high school is conditional upon the successful completion of lower secondary education. Students submit ranked preferences for high schools and tracks. School placement is determined at the national level based on students' residential addresses: when preferred schools are oversubscribed, students are assigned to a similar school within their district. Although high schools are self-governing institutions, their funding primarily comes from state transfers, and tuition fees are either absent or minimal¹².

¹¹In the years considered in this paper, roughly 45.6% of the students enrolled in a high school by the age of 19.

¹²According to OECD statistics, 3% of the expenditure on upper and lower secondary education is directly financed by households (Nusche et al., 2016).

1.2.2 Sample Selection

Administrative registers covering the universe of the Danish population from 1980 to 2019 are the primary data sources of this paper. The sample includes the students who enrolled in a Danish high school from 1997 to 2007¹³. Parents and schoolmates are identified through family and school records, respectively. Additionally, earnings are tracked annually using tax records, while labor market outcomes are observed each year through employer-employee matched data for both the children and their parents.

Of the 387,061 students who enrolled in Danish high schools from 1997 to 2007, I exclude 22,848 (5.9%) who enrolled after the age of 19, and 9,496 (1.3%) with missing information on adult or parental earnings. Additionally, I drop 1,775 (0.4%) students from cohorts where more than 50% of their peers enrolled after age 19, and 2,078 (0.5%) students who attended schools with fewer than 4 consecutive years of observation. The final sample consists of 350,864 students from 339 schools across 11 cohorts.

1.2.3 Measures of Earnings and Peer Exposure

Individual Earnings Earnings are measured as the average annual earnings from the main occupation and self-employment before tax from age 28 to 32, as reported in tax registers. For ease of interpretation, earnings are reported as of 2015 USD dollars.

Individual Earnings - Ranks As high-SES individuals engage in longer education, measuring earnings in levels at the age of 30 might not capture differences in lifetime earnings. To address concerns due to differences in lifecycle wage profiles, I construct percentile ranks of individual earnings (as defined above) relative to the in-sample distribution of children born in the same cohort.¹⁴

Parental Earnings For each parent, I measure earnings as the average yearly earnings from the main occupation and self-employment over their child's first 18 years of life. I then construct parental earnings as the average among the parents of each child.

Parental Earnings - Ranks I construct percentile ranks of parental earnings (as defined above) relative to the in-sample distribution of children born in the same cohort.

School-Cohort Schoolmates are identified as the children who enrolled at the same school and track in the same cohort as the child of interest, excluding the child itself.

¹³These cohorts are chosen to include the children born between 1980 and 1987. Those are the individuals observed until the age of 32 in 2019, the last year of observation.

¹⁴Evidence from Sweden suggests that the rank-rank correlation is less sensitive to measurement issues due to different lifecycle wage profiles (Nyblom and Stuhler, 2016).

1.2.4 Descriptive Statistics

The main characteristics of the sample are reported in Table 1.1. The sample includes 350,864 students enrolled in 11 cohorts across 339 schools. The average student has 152 schoolmates; her parents earn $\sim \$47,500$ per year, and she earns $\sim \$44,000$ per year by the age of 28 – 32.

Table 1.1: Descriptive Statistics

	Mean	Standard Dev.	N
Female	0.57	(0.50)	350,864
N of Schoolmates	152.55	(76.82)	350,864
Earnings 28-32	43,924.34	(26,113.03)	350,864
Earnings 28-32 (Rank)	50.47	(28.91)	350,864
Father - Earnings When Kid 0-18	60,690.43	(44,381.77)	345,848
Mother - Earnings When Kid 0-18	34,713.53	(19,286.28)	350,503
Parental Earnings	47,433.09	(25,891.19)	350,864
Parental earnings (Rank)	50.50	(28.87)	350,864

Note: The table presents summary statistics for the main sample of students enrolled in a Danish high school from 1997 to 2007 and their parents, including the sample mean and standard deviation, along with the sample size (N) for each variable analyzed.

Table 1.2 reports conditional means of adult, parental, and schoolmates' earnings by quartile of parental earnings. Two main facts emerge from the table. First, children from higher-income parents tend to have higher earnings themselves. The difference in earnings between children from the top and the bottom quartile is substantial: \$10,872.59 or 9.26 percentile ranks. Second, differences in parental earnings are mirrored in schoolmates' parental earnings. The difference between the schoolmates' parental earnings of children from the top and the bottom quartile is \$5,656.8 and 7.12 percentile ranks, amounting to $\sim 10\%$ of the difference in their own parental background.

Table 1.2: Adult Earnings by Parental Earnings Quartile

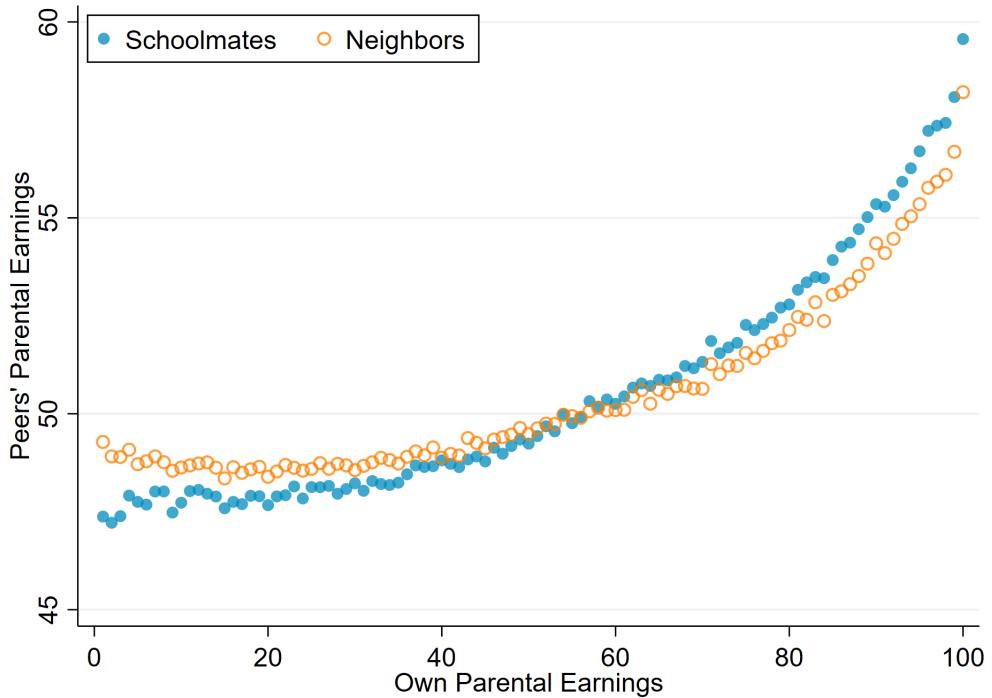
	Quartile of Parental Earnings			
	Q1	Q2	Q3	Q4
Parental earnings	23,284.68	40,763.86	50,543.88	75,141.88
Parental earnings (Rank)	12.56	38.48	63.64	88.12
Earnings 28-32	38,218.18	42,840.12	45,548.68	49,090.77
Earnings 28-32 (Rank)	43.56	49.33	52.82	56.18
SM Par. earnings	45,362.70	45,960.48	47,393.41	51,019.50
SM Par. earnings (Rank)	47.80	48.58	50.70	54.92
N	87,719	87,715	87,718	87,712

Note: The table presents average earnings outcomes for students in the sample, their parents and their schoolmates' parents, conditional on quartile of parental earnings. Earnings are measured in nominal terms (adjusted as 2015 USD) or in percentile ranks computed w.r.t. the distribution of students born in the same cohort belonging to the sample.

Finally, Figure 1.1 plots the conditional means of schoolmates' parental earnings (blue dots) and neighbors' parental earnings (orange circles), by percentile of own parental earnings¹⁵. Most of the segregation in schoolmates' parental earnings emerges at the right end of the earnings distribution, and the same pattern is observed for neighbors. These facts are informative on the institutional context of Danish high schools: free access to education is likely to mitigate segregation, especially at the bottom of the earnings distribution, while residential segregation potentially drives sorting into more homogeneous peer groups at the top of the distribution.

¹⁵For each student, I define as her neighbors the kids who live in the same municipality and enroll in a high school in the same year as herself.

Figure 1.1: Sorting across Schools and Neighborhoods



Note: The graph plots the sample average of schoolmates' and neighbors' parental earnings, conditional on the percentile of the in-sample parental earnings distribution. Peers' parental earnings are also measured in percentile ranks of the in-sample parental earnings distribution. Blue dots represent schoolmates' average parental earnings, orange circles represent neighbors' average parental earnings. Neighbors are defined as individuals born in the same calendar year and registered as living in the same municipality in the year of enrollment in high school.

On the one hand, the correlation of parental backgrounds among schoolmates challenges the identification of exposure effects, as it suggests that unobservable characteristics of students' families might be correlated among schoolmates. On the other hand, if exposure to peers matters, school segregation results in children from higher-income families enjoying a double advantage: the first from their own family background and the second from the peers they are exposed to. The research design presented in the next section addresses the main threats to identification by comparing students who sorted in the same High School in different cohorts.

1.3 Peer Exposure

1.3.1 Research Design

Random assignment of children to schools would serve as the ideal experiment to estimate exposure effects. In the absence of such an experiment, unobserved determinants of outcomes might be correlated within groups because of endogenous sorting or group-level correlated shocks. Individuals sharing similar unobserved characteristics (such as ability) might sort in the same group or common shocks (such as changes in local economic conditions) might simultaneously affect group members and generate correlation in outcomes even in the absence of peer effects.

To address this issue, I follow a within-school across-cohort design, as introduced by Hoxby (2000).¹⁶ Namely, I compare cohorts within the same school and leverage as identifying variation only the deviation from the school-specific time trend of school composition. To do so, I estimate the following model:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 \bar{X}_{-i} + Z'_i \delta + \gamma_{s(i)} + \tau_{s(i)} c(i) + \varepsilon_i. \quad (1.1)$$

Y_i and X_i represent earnings for each child i and her parents, respectively; \bar{X}_{-i} is the leave-one-out mean of i 's schoolmates' parental earnings; $c(i)$, $s(i)$ denote the cohort and the school of individual i , respectively. Hence, $\gamma_{s(i)}$ is a set of school fixed effects, and $\tau_{s(i)} c(i)$ represents school-specific time trends. Finally, Z_i is a vector of predetermined individual characteristics such as gender, year of birth, and year of birth of each of the parents. The main coefficient of interest (β_2) captures the marginal effect of peer's parental earnings on individual earnings. As such, this parameter is a combination of the direct effect of exposure to peers from different SES and the indirect effect emerging from peers' achievements.¹⁷

The main identifying assumption is that deviations from school-specific time trends in school composition are as good as random from the individual's perspective. I argue that the identifying variation captures deviations from expected school composition partially arising from idiosyncratic shocks to birth timing, which affect the composition of schoolmates without triggering parental responses like moving away from districts experiencing adverse shocks. In the following section, I provide evidence supporting this claim and further present findings that rule out *correlated shocks* experienced simultaneously by group members as a potential driver of the results.

¹⁶Among several subsequent applications of the same identification, the closest to mine are Black et al. (2013), Carrell et al. (2018), Brenøe and Zöllitz (2020), Cattan et al. (2022) and Mertz et al. (2024).

¹⁷In subsection 1.5.2, I exploit time variation schoolmates' promotions to identify their effect on peers' wages, separately from exposure to different parental backgrounds.

1.3.2 Support for the Validity of the Identifying Assumption

In this section, I provide several pieces of evidence supporting the identifying assumption that deviations from school-specific time trends in school composition are uncorrelated with unobserved determinants of students' earnings.

First, Table Table 1.3 presents a comparison of the standard deviation of \bar{X}_{-i} and of its residuals obtained from regressing the same variable on school-specific time trends. While most of the variation in peers' parental background is captured by time trends, residuals account for one-fourth of the variation in school composition. This is the identifying variation leveraged in the main specification.

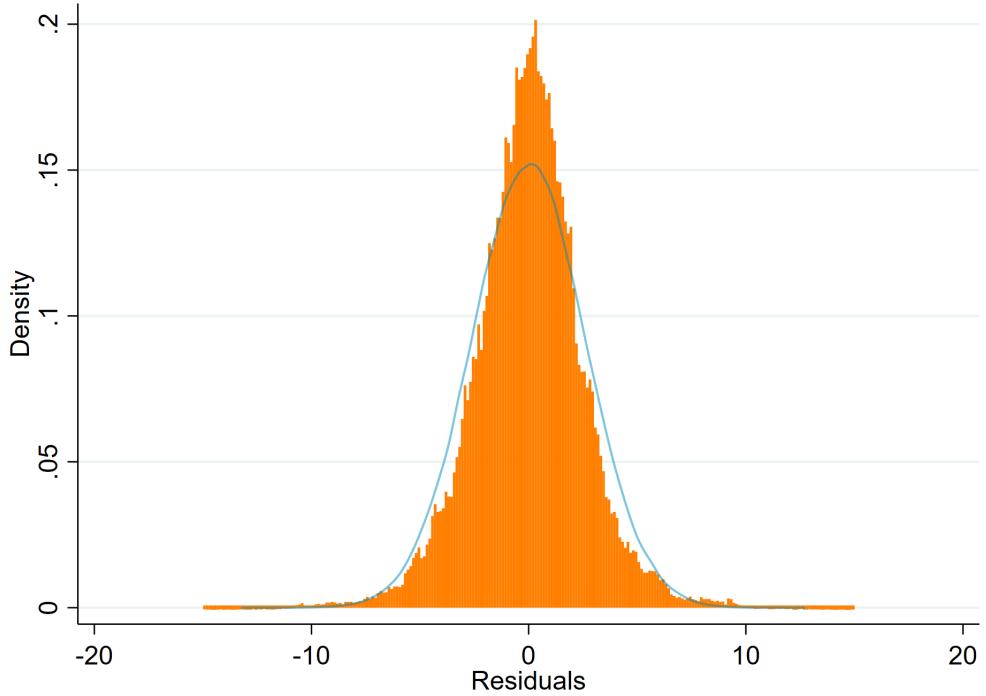
Table 1.3: Residual Variation in Schoolmates' Parental Earnings

	mean	sd	count
Schoolmates' Parental Earnings	50.50	9.29	350,821
Schoolmates' Parental Earnings - residual (linear trend)	-0.00	2.62	350,821
Schoolmates' Parental Earnings - residual (nonlinear trend, 2nd order)	0.00	2.41	350,821
Schoolmates' Parental Earnings - residual (linear trend, 3rd order)	0.00	2.23	350,821
Schoolmates' Parental Earnings - residual (moving avg.)	0.00	2.61	216,270

Note: The table presents descriptive statistics on schoolmates' parental earnings. Schoolmates' parental earnings are measured as the leave-one-out average earnings of the parents of each schoolmate, excluding own parents. Parental earnings are measured as percentile ranks of earnings with respect to the sample distribution of parental earnings of students born in the same year, and the average from age 0 to 18 of the child is computed. The table reports mean and standard deviations of the residuals of the same measure as resulting from a regression on school-specific, linear and nonlinear time trends.

Second, as shown in Figure 1.2, the distribution of the residuals of school composition is well approximated by a normal distribution, supporting the intuition that deviations from expected school compositions are as good as random.

Figure 1.2: Residuals are Normally Distributed

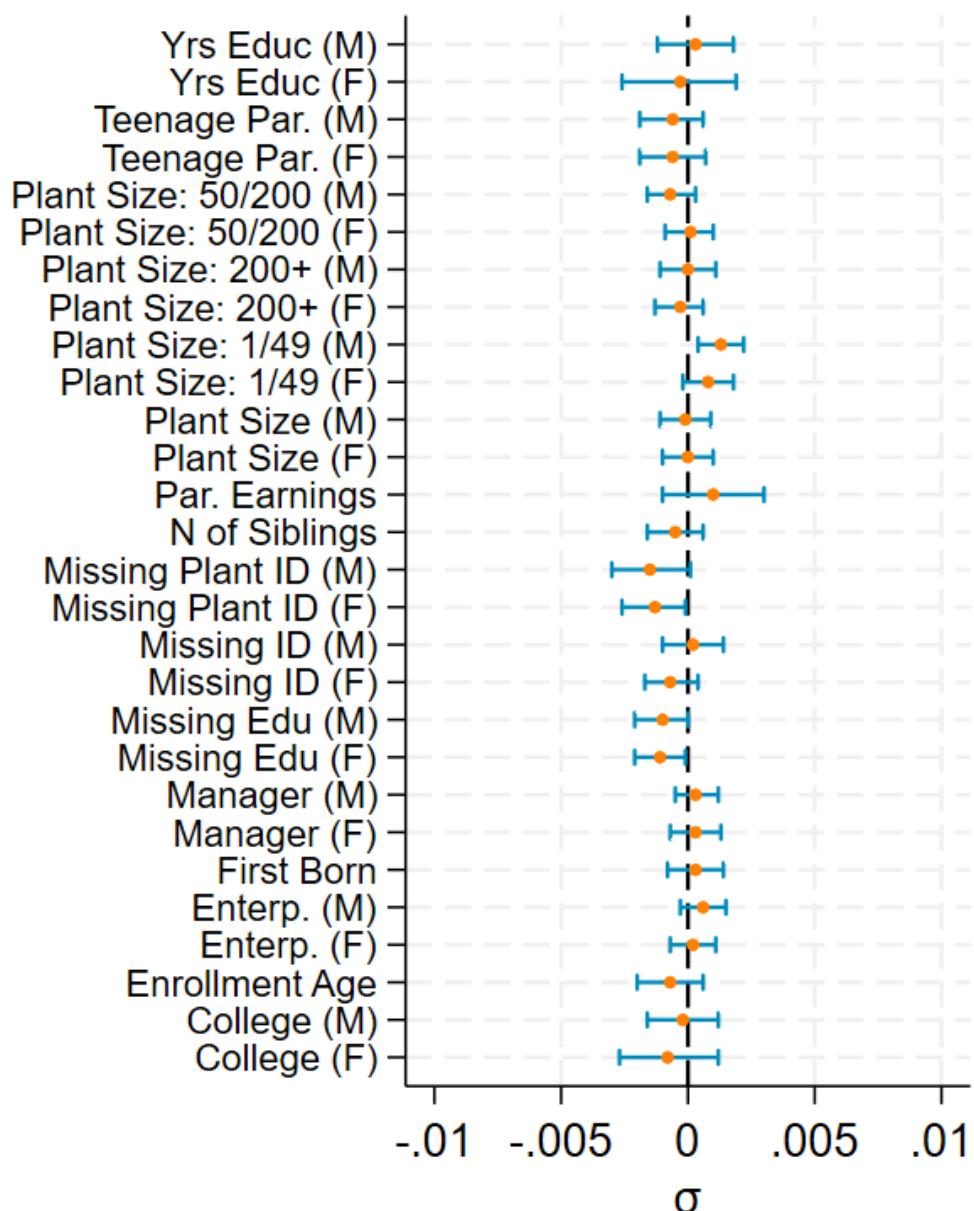


Note: The graph plots the empirical PDF of the residual of average schoolmates' parental background from a regression on school fixed effects and school specific linear time trends. Average schoolmates' parental background is defined as the leave-one-out average of schoolmates parental earnings in percentiles of the national distribution. The PDF of a normal distribution with the same mean and standard deviation as the residuals is represented by the continuous blue line.

Third, individual predetermined characteristics, including own parental earnings, are uncorrelated with the residuals in school composition. One could consider this as a balance test: if students were to anticipate deviations from school composition, their characteristics would be correlated with such deviations. For example, children from higher earnings or better-educated parents would enroll in school cohorts experiencing positive deviations from their expected composition. Figure 4.2 reports the coefficients from a balance test regressing school composition on several predetermined characteristics, including school-specific time trends.¹⁸ Table 4.1 reports the same coefficients in table format. As summarized in Table 1.4, only 1 of the 28 variables considered report a correlation with the residuals of school composition statistically different from zero at 99% confidence level.

¹⁸I standardize dependent variables and include as a regressor the school level average parental earnings to control for mechanical negative correlation due to the leave-one-out nature of the measure considered, following a standard practice introduced by Guryan et al., 2009 and applied to a similar context by Brenøe and Zöllitz, 2020.

Figure 1.3: No Correlation Between Traits and (Residuals of) Peers' Parental Earnings



Note: The graph reports coefficients from separate regressions regressing (standardized) schoolmates parental earnings on different (standardized) measures of predetermined characteristics, including controls for own parental earnings, school specific time trend and school-level average realizations of parental earnings to control for mechanical negative correlation due to the leave-one-out nature of the measure considered, following a standard practice introduced by Guryan et al. (2009) and applied to a similar context by Brenøe and Zölitz (2020).

Table 1.4: No Correlation Between Traits and (Residuals of) Peers' Parental Earnings

	N of test with $H_0 : \beta = 0$ is rejected			N of tests
	P-value<.1	P-value<.05	P-value<.01	
School FE	13 (46.42%)	12 (42.85%)	9 (32.14%)	28 (100.00%)
School time trend	4 (14.28%)	3 (10.71%)	1 (3.57%)	28 (100.00%)

Note: This Table shows aggregate results from separate OLS regressions reported in Table 4.1. All regressions include cohort fixed effects and school fixed effects. The first row refers to regressions which do not include school-specific time trends. The second row refers to regressions which include school-specific time trends. The table reports the number (and the share in parentheses) of variables which report correlation with the leave one out average of peers' parental earning different from zero at 90%, 95% and 99% confidence level.

Table 1.5: Residuals in School Composition are Uncorrelated Over Time

	N of test with $H_0 : \beta = 0$ is rejected			N of tests
	P-value<.01	P-value<.05	P-value<.1	
None	3 (0.9%)	17 (5.1%)	14 (4.2%)	332 (100%)
Linear	3 (0.9%)	10 (3%)	15 (4.5%)	332 (100%)
Quadratic	3 (0.9%)	12 (3.6%)	12 (3.6%)	332 (100%)
Cubic	4 (1.2%)	5 (1.5%)	9 (2.7%)	332 (100%)

Note: This Table shows aggregate results from separate school-specific time series regressions. All regressions test for the school specific AR(1) coefficient of the correlation over time in school composition. School composition is measured as the average parental earning of students enrolled in each school and cohort. The first row of the table refers to regressions which do not include school-specific time trends. The latter rows of the table refers to regressions which include school-specific time trends. The table reports the number (and the share in parenthesis) of variables which report correlation with the leave one out average of peers' parental earnings different from zero at 99%, 95% and 90% confidence level.

Fourth, cohort-specific deviations from school time trends are uncorrelated over time. This result confirms that residual variation in school composition does not follow a predictable

pattern, reinforcing the idea that students are unlikely to anticipate these changes. In Table 1.5, I present evidence from school-specific time series regression testing for autocorrelation in cohort composition. Upon inclusion of linear time trends, only 4.5% of the schools in the sample exhibit a correlation over time in school composition statistically different from zero at 90% confidence level.

Overall, I interpret the evidence collected so far as supporting the identifying assumption that deviations from school-specific time trends in school composition are as good as random from the individual's perspective. However, a further potential concern involves *correlated shocks* at the group level, potentially driving deviations in school composition and in individual earnings simultaneously.

Such group-level shocks might involve changes in school policies following the intake of a higher earnings cohort. However, the centralized funding of Danish High Schools is designed to equalize access to resources between schools and is financed through national-level taxation, thus making this event unlikely. To further address this concern, I estimate the effect of shocks from adjacent cohorts regressing adult earnings on school composition in adjacent years. As shown in Table 4.4, shocks to previous (and future) cohorts do not affect earnings. To drive the results, correlated shocks at the school level should affect the cohort of interest and leave no trace on subsequent cohorts. I interpret this as evidence that group-level shocks do not drive the results at the school level.

Alternatively, cohort-specific fluctuations of local economic conditions might affect both parental earnings and children's outcomes. However, the long time span over which parental earnings are measured is likely to capture permanent earnings rather than transitory fluctuations. Consistently, as shown in Table 4.3, the coefficient of interest is robust to inclusion of cohort-by-municipality fixed effects in the regression. Overall, I interpret these results as supporting the identifying assumption that correlated shocks are unlikely to drive the results.

1.3.3 Results

Table 1.6: Main Results

	Ranks				2015 USD			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Par. Earnings	0.161*** (0.003)	0.157*** (0.002)	0.145*** (0.002)	0.145*** (0.002)	0.176*** (0.024)	0.173*** (0.026)	0.163*** (0.028)	0.163*** (0.028)
Schoolmates' Par. Earnings		0.046* (0.024)	0.068*** (0.018)	0.067*** (0.021)		0.040 (0.032)	0.094*** (0.020)	0.075*** (0.024)
Observations	345834	345791	345791	345791	345834	345791	345791	345791
Cohort FE	No	No	Yes	Yes	No	No	Yes	Yes
School FE	No	No	Yes	Yes	No	No	Yes	Yes
School Time Trend	No	No	No	Yes	No	No	No	Yes
R^2	0.07	0.07	0.10	0.10	0.07	0.07	0.10	0.10

Note: Estimates from separate OLS regressions. Dependent variable is children earnings by the age of 28-32, measured in percentile ranks of the distribution of students born in the same year in columns (1) – (4) and in 2015 USD in columns (5) – (8). All specifications include controls: fixed effects for year of birth, mother age at birth, father age at birth, and gender. SEs in parentheses are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01

In column (1), the coefficient on parental earnings is the rank-rank coefficient measuring parent-child correlation in earnings.¹⁹ Each extra percentile in parental earnings is associated with 0.16 increase in children earnings. Column (2) reports the correlation of adult earnings with parental earnings and peers' parental earnings. The decrease in the magnitude of the coefficient of parental earnings from column (1) to column (2) highlights the positive correlation between own and peers' parents. Finally, column (3) includes school and cohort fixed effects, and column (4) includes school-specific linear time trends. My preferred specification is in column (4), as including school-specific time trends is more likely to prevent the results from endogenous sorting into schools as described in section 1.3.2. The coefficient of interest is positive and statistically different from zero at the 99% confidence level in all specifications.

Two main results derive from the estimates in Table 1.6. First, the coefficient of interest is positive and statistically different from zero at 99% confidence level suggesting that peer exposure affects adult earnings. A 1 percentile increase in schoolmates' average parental earnings results in a 0.067 percentile increase in adult yearly earnings. When earnings are measured in nominal terms, a \$1 increase in schoolmates' average parental earnings results in a \$0.08 increase in adult yearly earnings. Second, the magnitude of the effect is 41.6% of the parent-child correlation in earnings when earnings are measured in percentile ranks and 42.8% when

¹⁹While this number is slightly lower than in similar studies (Landersø and Heckman, 2017), one has to consider that the sample of this paper is not representative of the entire population of Denmark, but of the set of students who enrolled in high school.

earnings are measured in nominal terms. Overall, the results suggest that exposure to schoolmates' parental earnings is a statistically significant and quantitatively important determinant of adult earnings.

1.3.4 Effect Heterogeneity and Nonlinearity

Figure 1.4 documents the heterogeneity of the exposure effect on students from different levels of parental earnings. The graph reports the point estimate and the 90% confidence intervals of the marginal impact of exposure to schoolmates' parental earnings implied by including in the model from eq. 1.1 a complete set of interaction dummies for each tercile of the distribution of parental earnings. The effect is homogeneous with respect to parental background: an average extra unit in peers' parental earnings benefits children from different parental backgrounds similarly.

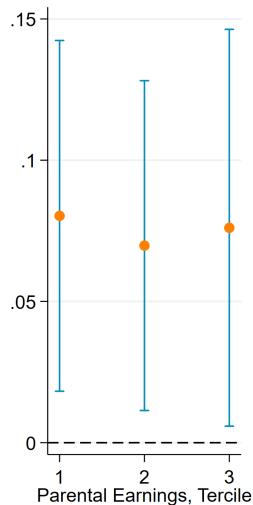
However, the exposure effect is nonlinear. Figure 1.5 plots the marginal effects and the relative 90% confidence intervals from estimating a version of the model in eq. 1.1 where a quadratic polynomial for average schoolmates' parental earnings is included. The effect is evaluated at different levels of exposure to peers' parental earnings²⁰. The effect is decreasing in the level of peers' parental earnings.

Overall, the results suggest that exposure to schoolmates' parental earnings is a significant determinant of adult earnings. The magnitude of the effect is such that for a given difference in parental earnings, a change in exposure of the same magnitude would close 42% in the earnings gap. Moreover, the decreasing marginal effect of average school composition suggests that interventions aimed at desegregating schools might achieve higher levels of aggregate earnings by reallocating low-SES students between the most segregated schools, from schools with the worst average composition to those with the best average composition. This would improve peer exposure, where it has the higher marginal effect, and worsen peer exposure in schools where effects are more attenuated.

Given the magnitude of the estimated effect, understanding how exposure to schoolmates' parental earnings influences adult earnings is crucial. In the next section, I will provide evidence on the impact of exposure to schoolmates on education and labor market outcomes.

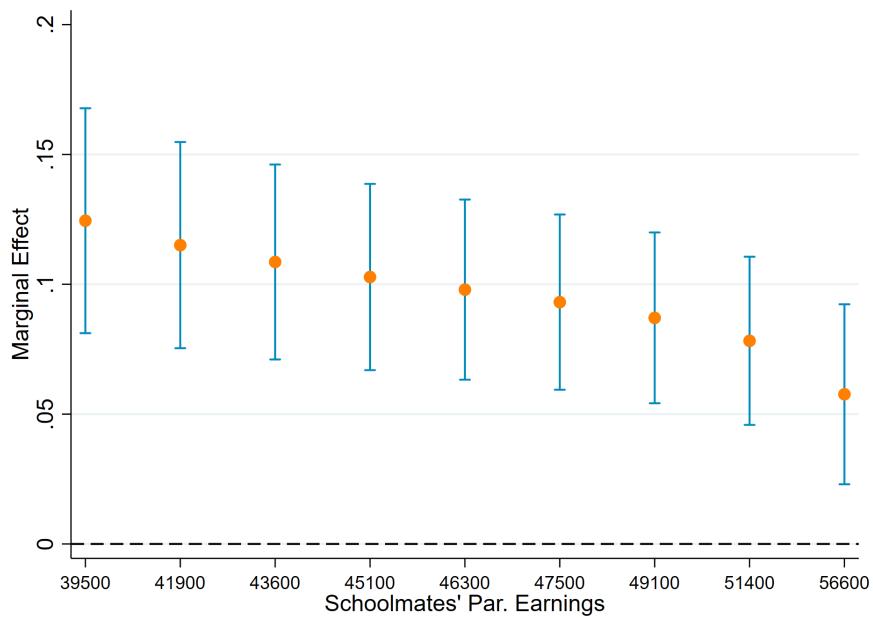
²⁰The levels are chosen as the deciles of the average school composition and are reported as labels in the graphs after rounding them to the closest hundreds.

Figure 1.4: Heterogeneity by Parental Earnings



Note: The graph plots the marginal effect (along with 90% confidence intervals) of the coefficient measuring the impact of peers' parental earnings on own earnings from a version of eq. 1.1 which includes a full set of interactions between all the independent variables and a set of dummies for each tercile of parental earnings. Standard errors are clustered at the school level.

Figure 1.5: Decreasing Marginal Effects



Note: The graph plots the marginal effects and the relative 90% confidence intervals from estimation of the model in eq. 1 where a quadratic polynomial for average schoolmates' parental earnings is included, evaluated at different levels of exposures to peers parental earnings. The horizontal axis report the deciles of the distribution of schoolmates parental earnings (rounded to the closest hundreds), at which the marginal effect is computed.

1.4 Education and Labor Market Outcomes

1.4.1 Parent-Child Correlation

In this section, I provide evidence on the vertical correlation of education and labor market outcomes with parental earnings: children from higher-earnings parents are more likely to obtain a College degree and have higher-paying jobs.

Table 1.7 documents education levels and labor market outcomes at the age of 30 for the whole sample and conditioning on terciles of parental earnings. As shown in the table, 73.2% of the students eventually obtain a university degree. This probability increases with parental earnings: children from the highest tercile are 15.7 *p.p.* (24%) more likely to get a university degree than children from the bottom tercile. When labor market outcomes are considered, a similar pattern emerges. Higher-SES children are 4.3 *p.p.* (5.8%) more likely to be employed and, conditional on being employed, to have managerial position 0.4 *p.p.* (18.1%). Finally, they work at higher-paying plants. By the age of 30, the difference in plant-specific pay premia between low and high-SES is 2.8 *p.p.* (350%).²¹ These differences are reflected by differences in hourly wages, which are \$3.26 (11.2%) higher for children from families at the top tercile of the earnings distribution than those from the bottom tercile.

Do any of these differences spill over to schoolmates upon exposure? As children from higher-earning parents have higher levels of human capital and higher-paying jobs, does exposure to higher-SES schoolmates result in higher levels of education or better labor market outcomes? In this section, I estimate the effect of peer exposure on education and labor market outcomes by the age of 28-32.

²¹Plant-specific pay premia are estimated by decomposing wages for the entire population of Danish workers from 2000 to 2019 into the plant and worker-specific components as proposed by Abowd et al. (1999) and implemented among others by Card et al. (2013). They can be interpreted as the percentage increase in wage paid to a worker upon employment at a given plant.

Table 1.7: Education and Labor Market Outcomes: SES Gradient

	Par. Earnings: Tercile			
	1	2	3	All
College	0.653 [115,792]	0.731 [115,783]	0.810 [119,289]	0.732 [350,864]
College, STEM	0.098 [115,792]	0.112 [115,783]	0.141 [119,289]	0.117 [350,864]
Employed	0.729 [115,792]	0.789 [115,783]	0.772 [119,289]	0.763 [350,864]
Manager	0.018 [84,375]	0.020 [91,340]	0.022 [92,127]	0.020 [267,842]
Hourly Wage	28.895 [84,375]	29.800 [91,340]	32.156 [92,127]	30.325 [267,842]
Plant Wage Premium (AKM)	0.008 [81,899]	0.014 [89,100]	0.036 [90,478]	0.020 [261,477]
Top Tercile Plant Wage Premium (AKM)	0.462 [81,899]	0.480 [89,100]	0.568 [90,478]	0.505 [261,477]

Note: The table presents average outcomes for students in the sample, measured at the age of 30 years old. Sample averages conditional on tercile of parental earnings are reported, with the number of observations in each cell reported in square brackets. College is a dummy variable equal to one if the individual has completed a College degree. College, STEM is a dummy variable equal to one if the individual has completed a College degree in the fields of science, technology, engineering or mathematics. Employed is a dummy variable equal to one if the individual is employed at the age of 30. Manager is a dummy variable equal to one if the individual is employed as a manager at the age of 30, defined only for employed individuals. Hourly Wage is the hourly wage at the main occupation at the age of 30, defined only for employed individuals. Top quartile and Top decile are dummy variables equal to one if the individual is employed at a plant whose AKM fixed effect (as in Abowd et al. (1999)) is in the top quartile or decile of the national distribution of plant fixed effects, respectively, defined only for employed individuals.

1.4.2 Exposure Effects on Education and Labor Market Outcomes

Table 1.8 reports OLS estimates of the coefficients in eq. 1.1 where the dependent variable is replaced with the outcome of interest and parental earnings are standardized to have mean zero and standard deviation one. The lower panel of the table reports a different set of regression coefficients where all the independent variables are interacted with dummies for each tercile of parental earnings. For ease of comparison, the first column of the table reports the effect of a one standard deviation increase in peer exposure on lifetime earnings in nominal terms. A one-standard-deviation increase in average schoolmates' parental earnings increases lifetime earnings by \$596.32 (1.3%). When other outcomes are considered, the same difference in exposure results in a positive effect on the probability of obtaining a college degree (+1.2 p.p., +1.6%), an increase in hourly wages of \$0.30(1.1%), an increase in the probability of having a managerial position 0.4 p.p. (18.1%) and an increase in plant-specific pay premium of 0.2 p.p. (10.0%). When the effect is considered by terciles of parental earnings, while the point estimates suggest a larger effect of exposure on education for high-SES and on labor market outcomes for low-SES children, the confidence intervals are large, and one cannot reject the null hypothesis of homogeneity of the effect across terciles of parental earnings.

While showing that exposure to higher-SES peers has a positive effect on education, potentially via spillovers in human capital formation as in Fruehwirth and Gagete-Miranda (2019) or transmission of information as suggested by Cattan et al. (2022), the results in Table 1.8 stress the importance of labor market outcomes as a key mechanism in the transmission of inequalities across generations. Given a \$8,178.66 earnings premium for college-educated individuals in the sample, a back-of-the-envelope calculation suggests that a 1.2 p.p. increase in the probability of obtaining a college degree due to exposure to higher-SES peers would result in a \$98.15 increase in earnings, amounting to 16.5% of the effect measured on hourly wages. I interpret this as evidence of alternative mechanisms, along with spillovers on educational achievement, driving the effect of peer exposure on adult earnings. The next section of the paper investigates the role of labor market interactions among former schoolmates in this process.

Table 1.8: Education and Labor Market Outcomes: Exposure Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Earnings	College	STEM	Employed	Manager	Hourly Wage	Plant FE	Plant FE > p(66)
<i>Panel A</i>								
SMs Par. Earn.	596.328** (188.773)	0.012*** (0.003)	-0.000 (0.002)	0.003 (0.003)	0.003** (0.001)	0.301*** (0.098)	0.002** (0.001)	0.010*** (0.004)
Observations	345791	345791	345791	345791	264504	264504	258232	258232
R ²	0.10	0.09	0.08	0.18	0.01	0.05	0.07	0.03
Mean D.V.	43934.34	0.73	0.12	0.76	0.02	30.33	0.02	0.50
<i>Panel B</i>								
SMs Par. Earn. × Ter. = 1	638.92** (297.544)	0.009* (0.006)	-0.000 (0.004)	0.005 (0.005)	0.003* (0.002)	0.396** (0.195)	0.001 (0.002)	0.013* (0.007)
SMs Par. Earn. × Ter. = 2	522.56* (281.563)	0.009* (0.005)	-0.001 (0.004)	0.001 (0.005)	0.002 (0.002)	0.312** (0.152)	0.005*** (0.002)	0.018*** (0.007)
SMs Par. Earn. × Ter. = 3	571.67* (342.054)	0.016*** (0.004)	-0.001 (0.003)	0.001 (0.004)	0.003* (0.002)	0.256 (0.184)	0.001 (0.002)	0.005 (0.006)
Observations	345782	345782	345782	345782	264497	264497	258223	258223
R ²	0.11	0.10	0.08	0.19	0.02	0.06	0.08	0.03
Mean D.V., Ter. 1	39071.45	.65	.1	.73	.02	28.89	.01	.46
Mean D.V., Ter. 2	44231.28	.73	.11	.79	.02	29.8	.01	.48
Mean D.V., Ter. 3	48337.05	.81	.14	.77	.02	32.16	.04	.56

Note: The table reports OLS estimates from the same model as in eq. 1.1, where earnings are measured in 2015 USD and the dependent variable is reported at the top of each column. Panel A reports the coefficients for the model as specified in 1.1, while Panel B reports the coefficients for the same model upon inclusion of interaction terms with dummies for parental earnings' terciles. All specifications include controls: fixed effects for year of birth, mother age at birth, father age at birth, and gender. SEs in parentheses are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01

1.5 Former Schoolmates as Weak Ties on the Labor Market

Do friendships formed in school impact career paths and job prospects? Can classmates help gain access to higher-paying companies and higher wages? In this section of the paper, I show that former schoolmates' career paths are interconnected: 1.4% of the students in the sample secure a job at a company due to school connections, and the average worker experiences a \$3.32 (+10.0%) increase in hourly wage as a peer is promoted to a managerial position.

1.5.1 Connected Hires

I begin by examining the effect of school connections on access to jobs. Do schoolmates facilitate access to the firms where they are employed? If so, do low-SES students leverage their high-SES former schoolmates' networks to help bridge the labor market outcome gaps identified in the previous section?

Identifying workplace changes driven by social network effects is challenging due to the endogenous nature of social interactions. Social relationships are often characterized by homophily, whereby individuals within a group tend to have correlated observable and unobservable characteristics. As a result, individuals might join the same firm because they share characteristics that make the firm appealing to them (e.g. skills or location) rather than because of social connections.

High Schools provide a natural experiment to address this identification challenge. *Almost* schoolmates (i.e., individuals who enrolled in the same high school in adjacent cohorts) are likely comparable to *actual* schoolmates, as they sorted into the same High School at a similar time. However, they are less likely be social ties.²² Therefore, comparing the share of individuals who join the plant of an actual schoolmate to the share of individuals who join the plant of an almost schoolmate is informative on how many job switches are due to school connections. Intuitively, if the share of connected switches is higher for *actual* schoolmates than for *almost* schoolmates, this suggests that social connections formed in high school are a determinant of workplace changes.

I apply the following procedure. First, I compute the share of individuals who join a plant where any of her *actual* schoolmates is employed. Then, for each cohort within each school, I randomly draw without replacement a set of *almost* schoolmates as large as the number of *actual schoolmates* and compute the share of individuals who joined a plant where any of their *almost* schoolmates are employed. To avoid simultaneous moves confounding the measurement,

²²This approach applies to the school setting identification designs that use variation in the timing of employment within a firm to test for the role of coworkers as social ties on the labor market (Hensvik and Skans, 2016; Caldwell and Harmon, 2019; Glitz and Vejlin, 2021).

I condition on the joined peer being employed at the receiving plant for at least one year before the switch. I consider as a workplace the plant²³ of main employment in November of each year and focus on changes in workplaces happening from the 4th to the 14th year since enrollment in high school.

By conducting independent draws of *almost* schoolmates and computing the share of students who join the plant of an *almost* schoolmate at each draw, I construct a counterfactual distribution representing the share of switches that would have been directed at schoolmates under the null hypothesis of no network effects. I then use this counterfactual distribution to test whether the share of connected switches is higher than in the absence of network effects, computing p-values as the share of draws that resulted in a lower probability than the realized one.

Figure 2.6 reports the main results of the analysis. Orange bars show the probability of joining a the plant of an *actual* schoolmate. Blue bars display the probability of joining the plant of an *almost* schoolmate as the average across 1,000 independent draws of almost schoolmates from adjacent cohorts. Probabilities are computed separately for each tercile of parental earnings. Network effects are positive and statistically significant for the subpopulations considered.

Former schoolmates facilitate access to their plants. As shown in panel (*a*) of the table, 12.9% (15.5%) of low-SES (high-SES) joined the plant of a former schoolmate from 4 to 14 years after enrollment in high school. Out of these changes in workplace, 1.18*p.p.* (7.6%) and 1.56*p.p.* (10.1%) are driven by social connections developed in high school by low and high SES respectively.

Switches to peers' workplaces are clustered by parental background. Panel (*b*) displays the probability of joining a peer's plant, conditional on such peer having parents from the top tercile of the distribution of parental earnings. High-SES children are 70% more likely to join a peer from the same parental background than low-SES children. Despite this fact, students from all parental backgrounds do join high-SES plants because of school connections.

Differences in access to workplaces where high-SES peers work reflect differences in access to high-paying plants.²⁴ Panel (*c*) shows the probability of joining a peer at a high-wage plant, confirming that high-SES children are 55% more likely to join such establishments than low-SES children. However, school connections drive 15.4% (0.46*p.p.*) of such switches for low-SES and 17.3% (0.66*p.p.*) for high-SES workers. This suggests that while high-SES children are more likely to join high-wage plants, low-SES children are still able to leverage school connections to access high-wage workplaces.

²³Plants are assigned a unique identifier by DST. While changes of identifiers over time for the same establishment are not infrequent, they affect firms of *actual* and *almost* schoolmates at the same rate and thus are not a concern.

²⁴High wage plants are defined as the establishment whose plant fixed effect is at the top quartile of the national distribution of plant-specific AKM fixed effects as in Abowd et al. (1999).

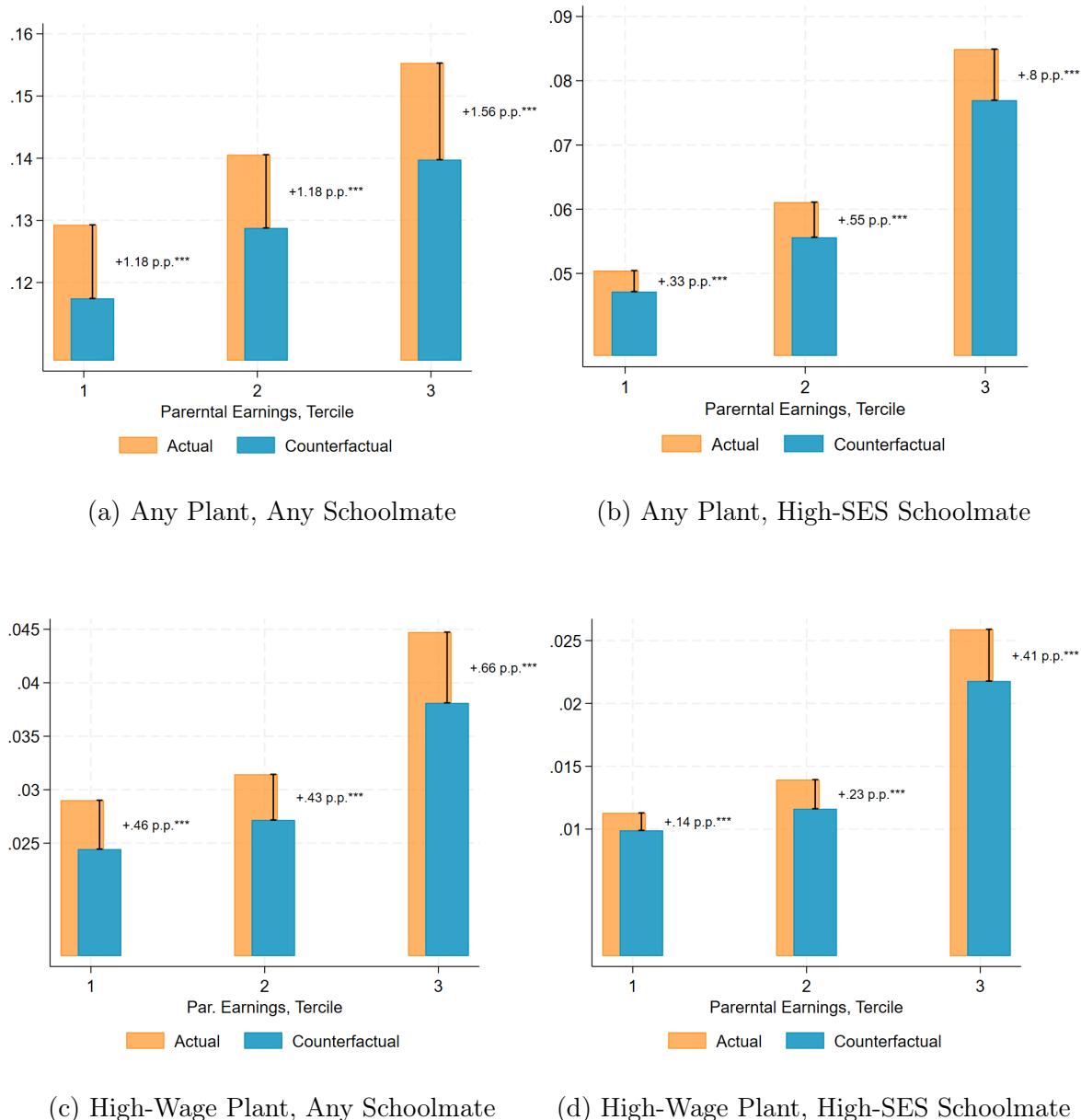


Figure 1.6: Probability of Joining a Plant Where a Schoolmate is Employed

Note: The bar graphs display the probability of joining a plant where a schoolmate is employed, measured 4 to 14 years after high school enrollment. The probability is calculated as the share of individuals who join actual schoolmates' plants (orange bars, left) versus counterfactual schoolmates' plants (blue bars, right), averaged across 1,000 independent draws of almost schoolmates from adjacent cohorts. The plots show the difference in percentage points between the actual and counterfactual probabilities. P-values are computed by determining the share of counterfactual draws that result in a higher probability of joining an almost schoolmate compared to an actual schoolmate. Each panel computes the probability of joining any peer vs high-SES peers, at any firm or at a high-wage plant. High-SES peers are defined as children from parents at the top tercile of parental earnings, high wage plants are plants whose AKM fixed effect is within the top quartile of the national distribution. Significance levels are indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This result is confirmed even when we restrict our focus to switches aimed at high-wage plants connected by a high-SES peer. Panel (*d*) shows the probability of joining a high-SES peer at a high wage plant. Also, for this restricted set of switches, school connections are a statistically significant determinant of workplace changes for both high and low-SES children.

Overall, school exposure is a significant determinant of workplace changes. High school connections determine a change in workplace directed at a plant where a former schoolmate is employed for 1.4% of the individuals in the sample. Moreover, while high-SES students are more likely to leverage the connections developed in school and join high-SES students at high-wage firms, school connections open door to higher paying jobs for low-SES students too.

An unresolved question from this analysis concerns the magnitude (and direction) of wage gains resulting from workplace changes facilitated by school connections. Developing an identification strategy to address this issue is a potential avenue for future research. However, the evidence presented so far indicates that school connections influence access to employment opportunities. In the next section, I focus on a direct implication of this finding: if peers offer job opportunities, do they also improve peers' bargaining power in wage negotiations when their career prospects improve, such as after a promotion to a managerial role?

1.5.2 Wage Spillovers

Social ties might facilitate career advancements even without attracting peers to their firm. Since peers facilitate access to new job opportunities, as shown in the previous section, workers may leverage their peers' outside options to negotiate higher wages with their current employers, even without changing workplaces. Workers receiving an outside offer from a former schoolmate employed at a different firm may use it as a bargaining tool, as their current employer may find profitable to match the offer to retain the worker by raising their wage. This mechanism is standard in job-search models that feature on-the-job search (Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Bagger et al., 2014).

The ideal experiment to test for spillovers from peers' outside options would involve exogenous variation in peers' career trajectories. However, in natural settings, such variation is likely spurious due to self-selection into peer groups or endogenous peer effects affecting peers' careers. For example, the promotion of a worker to a managerial position is likely to be associated with individual earnings determinants, such as human capital or skills. These characteristics may be correlated among peers due to the sorting of similarly skilled individuals into the same group. Or, even absent such sorting, prior exposure to the same peers may have influenced individual productivity, confounding the observed relationship between peers' labor market outcomes and individual wages.

This identification problem has been formalized by Manski (1993) as the *reflection problem*. Possible solutions have been proposed exploiting exogenous variation in individual outcomes

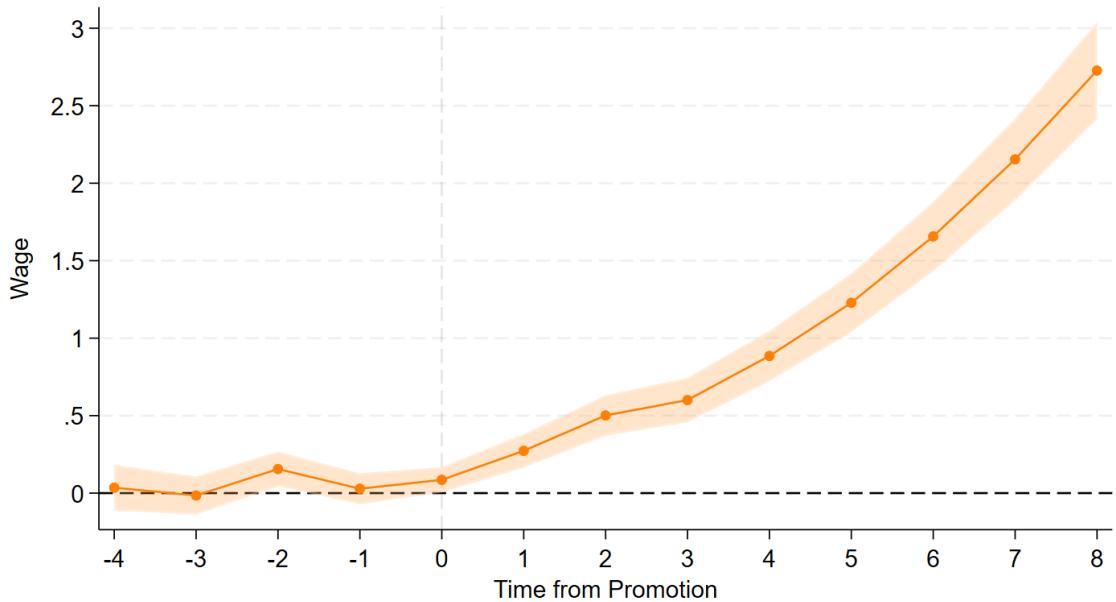
due to exposure to group-level shocks (Moffitt, 2001; Lalivé and Cattaneo, 2009; Rossi and Xiao, 2023) or non-overlapping peers (Bramoullé et al., 2009; De Giorgi et al., 2010). In this section, I propose a different approach by exploiting exogenous variation in the timing of peers' outcome realizations. Intuitively, while previous exposure to peers might influence the probability of being promoted to a managerial position, some variation in the timing of promotions is likely to be exogenous to individual and group characteristics. Exploiting this exogenous variation, the comparison of wages of individuals who already experienced a peer's promotion and those who have not yet identifies the effect of peers' labor market outcomes on individual wages.

Namely, I estimate the following model of difference-in-differences:

$$W_{sc,t} = \alpha_{sc}^{\tau} + \alpha_t^{\tau} + \sum_l \delta_l^{\tau} (M_{sc}^{\tau} \cdot \mathbb{1}\{t = \tau + l\}) + \epsilon_{sc,t}. \quad (1.2)$$

Where $W_{sc,t}$ represents the average wage of the members of group sc (those who attended high school s in cohort c) at year t , M_{sc}^{τ} is a dummy variable equal to one if the group sc experienced a peer's promotion at year τ , and 0 if it did not yet. I consider only the first promotion to manager for each group, and I exclude the individuals who became managers from the sample. The coefficient δ_l^{τ} measure the effect of a peer's promotion on individual wages l years after the treatment, for those who experienced a promotion in year τ . I follow the procedure in Callaway and Sant'Anna (2021) to estimate the coefficients of interest for each year of treatment τ and aggregate them to compute dynamic treatment effects. The identifying assumption is that in the counterfactual scenario where the peer's promotion did not occur, the average wage of the group would have changed by the same amount as the average wage of the groups that had not yet experienced the peer's promotion.

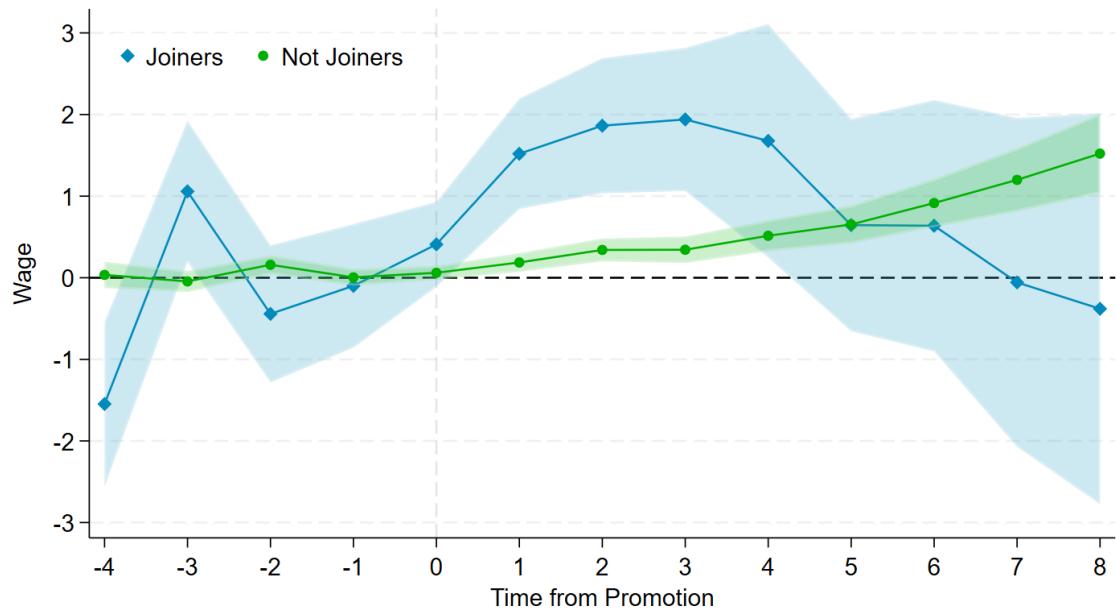
Figure 1.8: Effect of a Peers' Promotion on Wages (a)



Note: The graph reports estimates of the effect of a schoolmate's promotion on own hourly wages for each period before and after promotion, obtained by estimating the model in equation (1.2) as in Callaway and Sant'Anna (2021). Each group is composed by schoolmates who enrolled at the same high school in the same cohort, except the first person becoming a manager. Each group is considered treated from the first year in which a member becomes a manager onwards. Confidence intervals at the 95% level are reported as a shaded area.

Figure 1.8 plots the estimated dynamic treatment effects of a peer's promotion to manager on individual wages. The graph reports the point estimates and the 95% confidence intervals of the coefficients δ_l^τ in eq. 1.2 for each year after the treatment l , aggregated across different years of treatment τ as in Callaway and Sant'Anna (2021). The results suggest that a peer's promotion to manager has a positive effect on individual wages, resulting in a \$3.32 increase in hourly wages in the years following the promotion of a peer. Moreover, the absence of difference in trends between treated and control groups in the years before the treatment is reassuring about the validity of the identifying assumption.

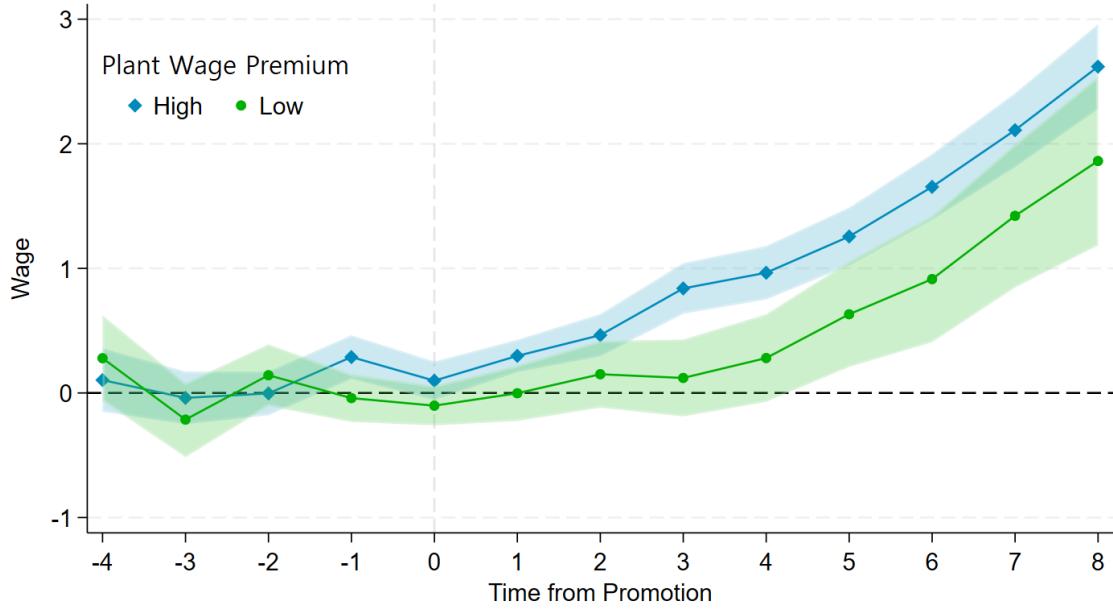
Figure 1.9: Effect of a Peers' Promotion on Wages (b)



Note: The graph reports estimates of the effect of a schoolmate's promotion on own hourly wages for each period before and after promotion, obtained by estimating the model in equation (1.2) as in Callaway and Sant'Anna (2021), estimated separately for workers who join the promoted manager's firm (blue diamonds) and those who do not (green circles). Each group is composed by schoolmates who enrolled at the same high school in the same cohort, except the first person becoming a manager. Each group is considered treated from the first year in which a member becomes a manager onwards. Confidence intervals at the 95% level are reported as a shaded area. Joiners are the individuals who work at the same plant as the promoted manager at any point in time from the time of the promotion onwards, not joiners are the others.

To gain further insight into how a peer's promotion affects own wages, Figure 1.9 shows the effect of a peer's promotion to manager on individual wages, estimated separately for individuals who worked at the same establishment as the promoted peer in any period after the promotion (*joiners*) and for those who did not (*not joiners*). While the most significant wage gains are realized by the individuals who join the promoted peer, also those who do not join the new manager realize a wage gain of \$1.62 in the years following the promotion. This indicates that the impact of a peer's promotion on an individual's wages may not only result from directly joining the promoted peer but also from the promotion's influence on the individual's ability to negotiate wages.

Figure 1.10: Effect of a Peers' Promotion on Wages (c)



Note: The graph reports estimates of the effect of a schoolmate's promotion on own hourly wages for each period before and after promotion, obtained by estimating the model in equation (1.2) as in Callaway and Sant'Anna (2021), estimated separately for groups with high (blue diamonds) and low (green circles) outside options. Groups are classified as having low outside options if the AKM plant fixed effect of the promoted manager's workplace falls within the bottom tercile of the distribution, and high outside options if it falls within the top tercile. Each group is composed by schoolmates who enrolled at the same high school in the same cohort, except the first person becoming a manager. Each group is considered treated from the first year in which a member becomes a manager onwards. Confidence intervals at the 95% level are reported as a shaded area. Low and high outside options are distinguished based on the AKM plant fixed effect of the plant where the promoted manager worked.

Finally, outside options improve bargaining positions the more appealing they are: everything else equal, a worker who is poached from a higher-wage firm can obtain a higher wage increase if her employer wants to retain the match. To test this implication, Figure 1.10 reports the effect of a peer's promotion, estimated separately for peer groups whose manager was promoted at a plant from the top and the bottom tercile of the distribution of plants' pay-premia.²⁵ The effect of a peer's promotion is larger when the promoted peer is employed at a high-wage plant. This result is in line with the interpretation of peers' promotion as an improvement in the outside option of the individual: the higher the productivity of the firm where the peer is promoted, the more attractive the outside option from a potential job offer and the larger the realized wage gains.

Overall, the results of this section suggest that social connections developed in high school provide outside options that individuals can leverage to negotiate higher wages. When a school

²⁵Plant-specific wage premia are computed as in Abowd et al. (1999) and Card et al. (2013). They can be interpreted as the percentage increase in wage paid to a worker upon employment at a given plant.

connection gets promoted to a managerial position, her peers see a persistent increase in hourly wages, also conditional on not joining the promoted peer. Moreover, the wage gains are larger when the promoted peer is employed at a high-wage firm.

1.6 Discussion

This paper investigates peer exposure as a determinant for social mobility: parental inequalities are transmitted to children’s peers via social spillovers. A \$1 increase in schoolmates’ parental earnings results in a \$0.08 increase in adult yearly earnings. Moreover, stark differences in access to higher-pay jobs between children from different parental backgrounds are coupled with schoolmates facilitating access to jobs and higher wages. In this section, I will relate the results of this paper to existing literature and the institutional context in which they are found, illustrating how they advance our understanding of the determinants of international mobility and which questions are left open for future research.

Where children are raised significantly impacts their success: this has led economists to study *neighborhood effects* (Chetty and Hendren, 2018; Chyn, 2018; Alesina et al., 2021; Deutscher, 2020; Mogstad and Torsvik, 2023; Aloni and Avivi, 2024). At the same time, parents also value *with whom* their children interact when selecting neighborhoods and schools (Heckman and Landersø, 2022; Abdulkadiroglu et al., 2020; Eshaghnia et al., 2023). This paper contributes to this literature by providing causal evidence of the importance of social interactions in transmitting earnings across generations. When considered in the context of this literature, the results suggest that that *neighborhood effects* are partly driven by the social networks they expose children to.

High schools are a compelling setting to study peer exposure on adult outcomes. Considerable evidence has been collected on the crucial experience of high school years for individual development. For example, Cattan et al. (2022) documents the role of high schools in shaping children’s educational choices, Carrell et al. (2018) shows how disruptive schoolmates affect lifetime earnings, and Black et al. (2013) and Brenøe and Zöllitz (2020) show how high school gender composition affects long-run economic outcomes. At the same time, high schools are one of the institutional contexts where social networks are formed. In particular, Chetty et al. (2022b) document how cross-SES friendships are formed in U.S. high schools, stressing the role of *exposure* in offsetting the natural tendency of creating links within SES clusters. The results of this paper confirm this hypothesis, showing that social ties developed in high school are long-lasting and determine access to jobs and higher wages. This makes the question of how to design schools to foster cross-SES interactions a key policy target.

One limitation of the approach of this paper is that the endogenous behavioral responses of agents exposed to different peers remain unobserved. For instance, endogenous changes in parenting styles may counteract the effects of desegregation policies, as high-SES parents may prevent their children from interacting with schoolmates from lower-SES backgrounds (Agostinelli et al., 2020; Doepke and Zilibotti, 2017).²⁶ Nonetheless, the nonlinearities in

²⁶A related but broader question lies in which interventions might be developed to limit such behavioral responses. For example, suppose parents restrict cross-SES interactions due to concerns about potential spillover

the exposure effect identified in this paper suggest potential gains from a policy aimed at reallocating low-SES students between the most segregated schools, from those with the worst average composition to those with the best average composition. This would improve peer exposure where it has the higher marginal effect, and worsen peer exposure in schools where effects are more attenuated. However, addressing the nonlinearity of the exposure effect within contexts of endogenous network formation remains an open question for future research.

It is also important to consider the specificity of the Danish institutional context to draw conclusions on the validity of the present results in different settings. Providing equal access to education in Denmark is likely limiting students' segregation by parental earnings across schools. Still, schoolmates' parental background do affect Danish adult earnings. This highlights the importance of considering the role of social networks in shaping economic outcomes, which might play an even more critical role in contexts where heterogeneity in education prices increases the correlation of parental backgrounds among schoolmates.

A key contribution of this paper is the analysis of the different roles high school peers play in fostering social mobility. I document how exposure to higher-SES peers leads to a limited increase in the probability of obtaining a college degree, but it significantly affects wages and access to higher-paying firms. It is instructive to consider these findings in relation to the institutional context of Danish upper secondary education. Danish high schools are not compulsory and are designed to prepare students for college. As such, they might attract students already inclined to pursue further studies, making them less influenced by peer exposure. Nevertheless, the findings on the significance of school connections for labor market outcomes indicate that the influence of peers extends beyond educational choices and has a broader impact on labor market success.

In particular, children exposed to higher-SES peers gain access to higher-paying firms and earn higher wages. These findings align with emerging literature showing significant differences in labor market outcomes based on parental background. For example, children often inherit occupations and employers from their parents (Kramarz and Skans, 2014; Staiger, 2023; Ventura, 2024), and those from higher-SES families tend to access higher-paying firms (Dobbin and Zohar, 2023; Forsberg et al., 2024). This paper highlights similar patterns and emphasizes the potential role of social exposure in reinforcing these differences. Children from high-SES families are not only more likely to be employed at high-paying firms, but they also benefit from being exposed to peers from similar high-SES backgrounds, as they facilitate access to higher-wage firms through their social networks.

In the last part of the paper, I show how school connections open doors to jobs and generate wage spillovers. These results are consistent with job search models that feature on-the-job effects related to risky behaviors. In that case, it may be worthwhile to couple desegregation policies with initiatives to curb these behaviors among students.

search (Postel-Vinay and Robin, 2002; Cahuc et al., 2006). Workers can achieve wage gains by receiving attractive outside offers and using them to negotiate higher wages with their current employer. My findings are consistent with those of Bagger et al. (2014), who show that outside options are key for young Danish workers in advancing up the job ladder early in their careers. While I cannot rule out alternative mechanisms, such as peers providing information to update biased beliefs on the wage distribution, the causal evidence of spillovers from schoolmates collected in this paper highlights the lasting importance of social connections in accessing opportunities and achieving higher wages.

Finally, the results of this paper are to be considered as a novel addition to an existing literature on different types of peer effects affecting long term economic outcomes. Important results on the role of peers in shaping aspirations (Genicot and Ray, 2020), social norms (Bursztyn et al., 2018), expectations (Bellue, 2023), human capital (Fruehwirth and Gagete-Miranda, 2019) and social capital (Cattan et al., 2022) are complementary to those of this paper and stress different, but similarly important channels linking exposure to social mobility.

This paper’s findings show how segregation in social interactions reinforces inequalities across generations. These insights deepen our understanding of the forces shaping social mobility and provide guidance for policies aimed at reducing inequality. Prominent policy options include school desegregation and the strategic design of shared spaces and leisure activities that promote interactions among individuals from diverse socioeconomic backgrounds.

However, two caveats must be considered when considering policy implications. First, the causal effects identified in this paper may not be policy-invariant. For instance, the impact of a large-scale peer redistribution policy could be less significant than suggested, as families may adopt more authoritarian parenting styles to mitigate perceived adverse effects from interactions with lower-SES peers. Second, any consideration of optimal policies should be framed within a clear normative statement of the policy objectives. While various arguments can be made in favor of reducing inequalities, it is beyond the scope of this paper to take a stand in this debate. Ultimately, the contribution of this paper is positive in its nature, emphasizing how social interactions facilitate the transmission of inequalities across generations.

1.7 Conclusions

This paper identifies the impact of interactions across socioeconomic groups on children's future earning potential. Exposure to peers from higher-SES families is shown to positively influence adult earnings, with a \$1 increase in the average parental earnings of schoolmates resulting in an \$0.08 increase in yearly adult earnings. While peer exposure has a limited effect on educational attainment, the connections formed in school persist into the labor market, facilitating access to higher-paying jobs and generating spillovers from peers' promotions. These findings highlight the value of policies aimed at fostering interactions among students from diverse backgrounds, such as school desegregation and the design of safe shared spaces. Overall, the insights from this paper identify a critical determinant of social mobility that can inform policy discussions and serve as a foundation for future research on the role of social interactions in shaping economic outcomes.

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Born to be (sub)Prime: an Exploratory Analysis

Helena Bach

University of Geneva

Pietro Campa

University of Geneva

Giacomo De Giorgi

University of Geneva

Jaromir Nosal Davide Pietrobon

Boston College Lund University

As published in May 2023 on the
American Economic Association Papers and Proceedings.

Abstract: We study how inheriting parents' credit histories affects credit scores, access to credit, and subsequent experiences of young individuals entering the credit market. First, having an inherited credit history significantly positively affects credit scores at entry. Second, initial credit scores are very persistent. Third, inherited credit histories only affect outcomes through the initial credit score distribution. Finally, initial credit scores have significant persistent effects on credit use and access, such as having a mortgage or credit card penetration and utilization rate. Our results point to the importance of initial conditions in credit markets and are consistent with mechanisms based on multiplicity of equilibria and self-fulfilling liquidity traps in which lack of access to credit due to low credit scores re-affirms the low credit score ranking of an individual.

2.1 Introduction

Economic mobility is an important and long-studied topic in the economic literature (Solon (1992), Chetty et al. (2014), Jantti and Jenkins (2015)). However, mobility in the credit market has so far received far less attention, partially due to lack of available data, with the notable exception of Ghent and Kudlyak (2016). Credit score and consumer credit play a crucial role in consumption, investment and housing decisions (Guiso and Sodini (2013), Laufer and Paciorek (2022), Topel and Rosen (1988)), potentially affecting the ability to smooth out shocks (Keys et al. (2017) and Hundtofte et al. (2019)) and finance investments in human capital (Wiederspan (2016), Solis (2017)). Additionally, credit scores are widely used in evaluating job applications (Bos et al. (2018), Ballance et al. (2020)), or rental and utility contract applications. In this paper, we use a large panel dataset of credit records that allows us to study how inheriting parents' credit histories affects credit scores and access to credit of young individuals entering the credit market, and their subsequent experiences in the credit market. First, having an inherited credit history significantly positively affects credit scores at entry. Second, initial credit scores are very persistent, with differences persisting for more than 10 years onwards. Third, inherited credit histories only affect outcomes through the initial credit score distribution, which then pins down the subsequent evolution of the credit scores. Finally, initial credit scores have significant persistent effects on credit use and access, such as having a mortgage or credit card penetration and utilization rate. Our results point to the importance of initial conditions in credit markets and are consistent with mechanisms based on multiplicity of equilibria and self-fulfilling liquidity traps in which lack of access to credit due to low credit scores re-affirms the low credit score ranking of an individual.

2.2 Data

This paper is based on Experian proprietary data on credit reports for about a 1% of the US population with a valid credit score in 2010. For those individuals, the data spans the years 2004 to 2016 at yearly frequency. We pull the credit data around the 30th of June of each year. The data contain basic socio-demographic such as date of birth, zip code of residence, and a rich array of about 400 credit variables. These credit variables cover a range of outcomes, from the total number of accounts, to number of credit cards, their balances and limits, total number of mortgages, balances, plus delinquencies, default, and bankruptcy.

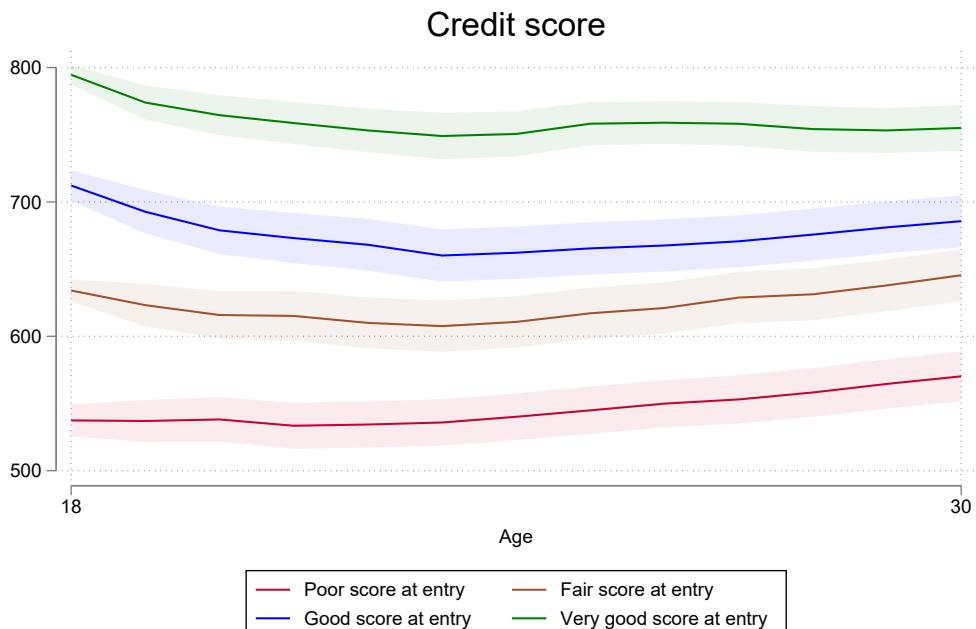
In what follows, we study the credit outcomes of individuals who are 18 years old in 2004 (born in 1986) across years 2004–2016. As **initial credit score** of these individuals, we take the first reported credit score for them, if observed before the age of 23. As **final credit score**, we take their credit score in 2016 (i.e. at age 30). We consider 4 bins of credit scores, with cutoffs taken from Experian's risk categorization guidelines: a *Poor* credit score falls at

or below 600; *Fair* credit score falls between 601 and 660; *Good* credit score is in the range 661-780; and a *Very good* credit score is above 780. Importantly, we are also able to capture the length of inherited credit histories by measuring the age of oldest account on the individual's credit record at entry. On average, 56% of individuals have histories longer than 6 months at entry, which we interpret as coming from being added to parents' account while being a minor.¹ These inherited histories are on average 28 months long in our data and range from 0 to 32 years.

2.3 Life Cycle Profiles

We first study the evolution and persistence of individual credit scores as a function of initial credit score by bin. Initial credit scores exhibit slight growth over the lifecycle for individuals starting with a *Poor* initial credit score and essentially no growth for the *Fair* and *Good* initial credit scores (Figure 2.1).

Figure 2.1: Credit Score by Age and Initial Bin

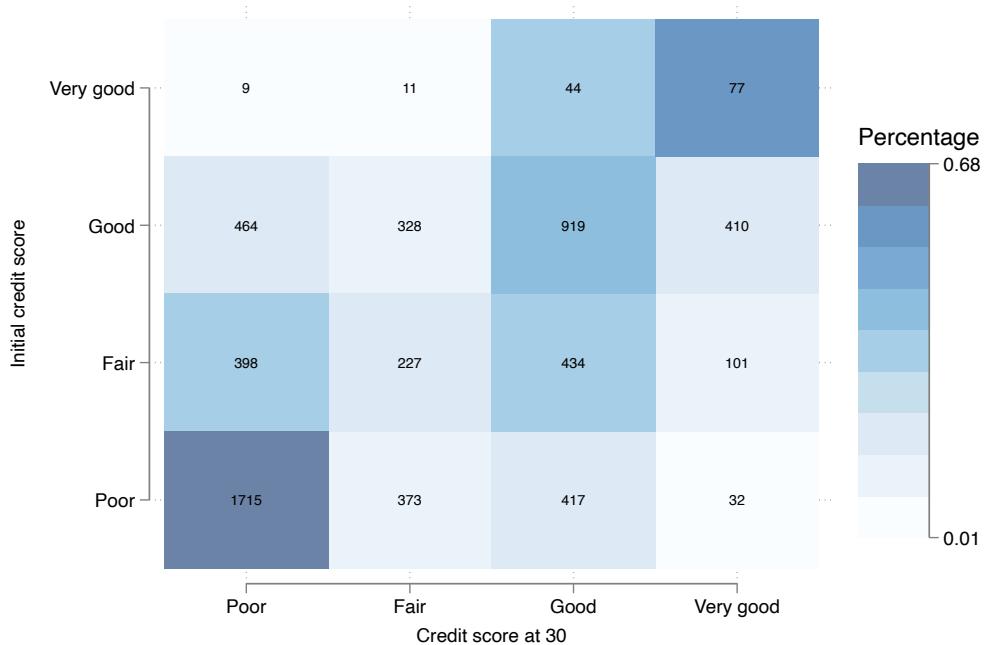


Note: The plot shows the evolution of credit scores by initial credit score bin. – 95% Confidence intervals shaded.

¹The so called *piggybacking* on non-relatives' credit accounts, the for-profit version of adding authorized users, started to gain popularity in later years, as per Martin (2022): "...These piggybacking companies, which started to emerge in 2007...".

This results in very high persistence of the initial credit score bins, especially for *Poor* and *Very good* initial credit scores. As Figure 2.2 documents, 68% of individuals entering with a *Poor* credit score remain in that bin by age 30, while only 18% end up in the *Good* or better bins. This points to the importance of initial credit scores, and begs the question of what determines the credit score of individuals with extremely short personal credit histories. Below, in Section 2.4, we document the role of inherited histories in pinning down the initial credit score distribution.

Figure 2.2: Persistence of initial credit score bins.



Note: The plot shows the transition matrix of initial credit score bins to final (age 30) credit score bins. The rows represent the initial credit score bins and the columns represent the final credit score bins. The color intensity is proportional to the fraction of individuals in each bin. The number reported in each cell is the number of individuals in that cell.

2.4 Inter-generational Linkages

We measure the importance of inherited histories by regressing the initial credit score on the length of the inherited credit history (intensive margin) or on a dummy variable that takes the value of one if an individual has an inherited credit history and zero otherwise. We report the estimates in Table 2.1. Inherited histories, at the intensive margin (extensive margin), are able to explain about 20% (10%) of the variation of the initial credit scores. On the intensive margin, an individual with an average inherited history (28 months) has an initial credit score at entry that is 36 points higher than an individual with no inherited history (column (1)). Overall, having any inherited history increases credit score at entry by 58 points (column (2)). Both of these imply an economically sizeable impact of inherited histories on initial credit scores, large enough to be able to shift individuals across the credit score bins.

Table 2.1: Initial Credit Score and History

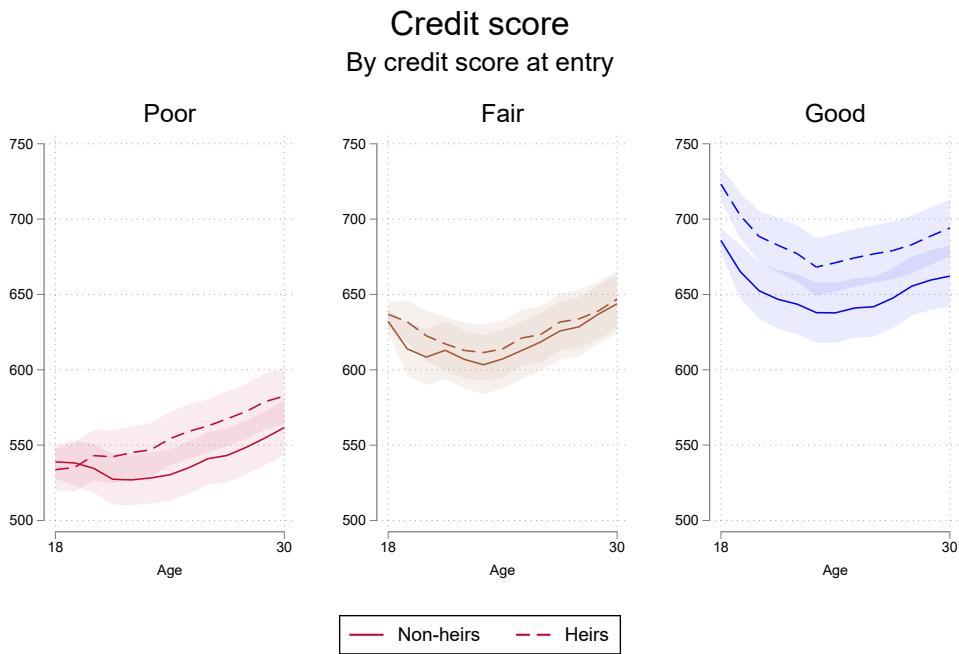
	(1)	(2)
History	1.309*** (0.051)	
History ²	-0.002*** (0.002)	
1 (History)		58.19*** (2.270)
Constant	594.0*** (1.319)	586.8*** (1.699)
Observations	6064	6064
Adjusted R^2	0.175	0.098

Note: Estimates from separate OLS regressions. Dependent variable is initial credit score. SEs in parentheses. History is measured as the number of months since the first credit record linked to any of the individual's accounts was established. The dummy for history takes value 1 if the first credit record was filed before the individual turned 18. * p<0.10, ** p<0.05, *** p<0.01

The next set of results, presented in Figure 2.3, show the life-cycle profiles of credit score conditional on initial credit score bin *and* having some or zero inherited histories. For the *Poor* and *Fair* categories, once we condition on initial credit score bin, having inherited history does not bring in new information, as the profiles with and without histories are not statistically

different from each other. For the *Good* credit score category, initial conditions pin down the difference between the two life-cycle profiles, which then evolve in parallel, exhibiting no convergence. In this case, again, initial conditions contain all information needed to pin down the difference. Summarizing, inherited histories affect life-cycle profile of credit scores through initial credit scores as a sufficient statistic for the three categories (there are no individuals with no history entering the *Very good* bin).

Figure 2.3: Credit Score Evolution, by Initial Condition



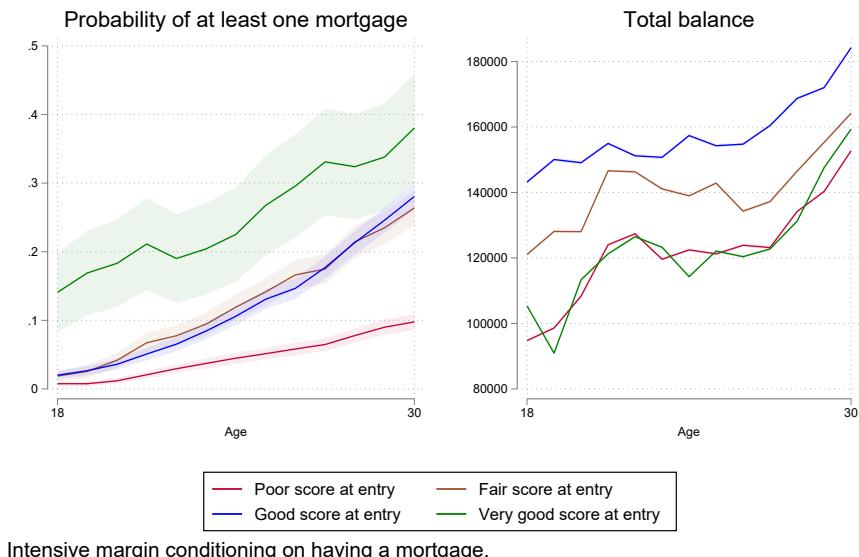
Note: The graph shows the evolution of credit scores by initial credit score bin and inherited history status. The lines represent the average credit score for each group, with 95% confidence intervals shaded. Individuals who have inherited credit records are represented by dashed lines.

2.5 Other Credit Outcomes

In this section, we expand our analysis to other credit market outcomes, focusing on mortgages and credit cards. For mortgage outcomes, presented in Figure 2.4, the differences by initial credit scores are mostly on the extensive margin. Only 10% of *Poor* initial credit score individuals have a mortgage by age 30, while the same fraction is 30% for *Good* and *Fair* initial scores and 40% for *Very good* initial scores. At the intensive margin, what we notice in the right panel of Figure 2.4 is that the amounts borrowed by *Very good* and *Poor* initial credit score individuals are very similar, while the other two categories borrow more. This result is driven by geographical variation, as when we control for zip code effects, the differences between bins disappear. That means that credit scores mostly affect access and use of mortgage credit, with the conditional amounts pinned down by location.

For credit cards, the pattern continues. By age 30, 60% of *Poor* initial credit scores have at least one credit card versus 100% for *Good* and *Very good* initial credit scores and above 90% for *Fair* (Figure 2.5). The dynamics are also interesting, as in essence nothing happens at the extensive margin after age 22, with all the profiles being flat. At the intensive margin, presented in Figure 2.6, higher initial credit score individuals have higher credit limits, which, combined with similar balances, results in higher utilization rates for low initial credit scores: 60% utilization for *Poor* versus 20% for *Very good* initial credit scores. Of course, these large utilization rates negatively affect future scores and therefore lower the chances of additional access to credit.

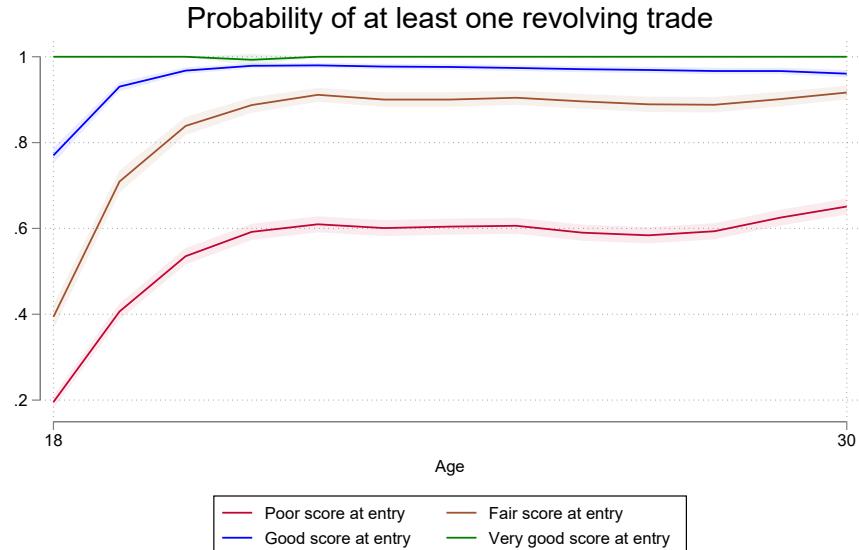
Figure 2.4: Mortgage Balances Evolution, by Initial Condition



Intensive margin conditioning on having a mortgage.

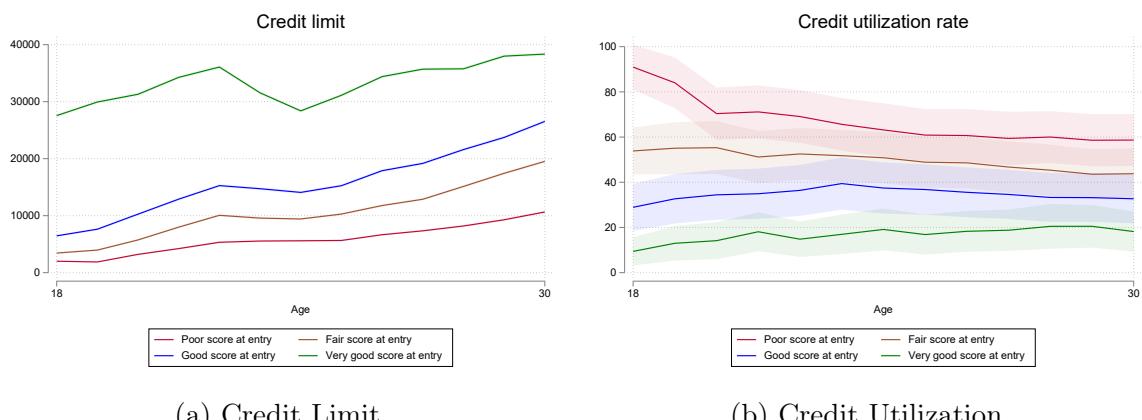
Note: The graph shows the evolution of mortgage uptake by initial credit score. The lines represent the probability of having a mortgage (left) and the average balance (right) for each group, with 95% confidence intervals shaded.

Figure 2.5: Credit Card Penetration, by Initial Condition



Note: The graph shows the evolution of credit card penetration by initial credit score. The lines represent the probability of having a credit card for each group, with 95% confidence intervals shaded.

Figure 2.6: Evolution of Credit Card Usage



Note: The graph shows the evolution of credit card usage by initial credit score. The lines represent the average credit limit (left) and the average utilization rate in percentage points (right) for each group, with 95% confidence intervals shaded.

2.6 How Predictive are History and Initial Scores?

To further assess the relevance of the initial conditions in the credit market, we perform a simple exercise of classification through a machine learning procedure. Specifically, we compare how much predictive power there is in only two variables: i. number of months of the oldest open account; ii. initial credit score, versus an extended model, where we use these two variables plus another 156 variables which record individual behavior for all the years 2004 to 2016.²

For this exercise, we define as subprime those individuals with a score below 620 at age 30, and use a random forest model (Breiman (2001)) to classify whether individuals are subprime borrowers at age 30. In the two-variables model, the use of a random forest is perhaps extreme, but will allow for a meaningful comparison. We train the model on a random subsample of 75% of the individuals in the original sample. We then evaluate the model's performance by means of a confusion matrix; i.e., a matrix whose rows represent the instances in the actual class (how many subprime-at-30 and prime-at-30 people there are in the data) and columns represent the instances in the predicted class (how many subprime-at-30 and prime-at-30 people the model predicts). We find that the two-variable model is able to correctly classify subprime borrowers with a 69% probability and prime borrowers with a 67% probability, for an accuracy rate of .68 (95% Confidence Interval, .66 - .7). The enhanced model correctly classifies subprime and prime borrowers with an 85% and 86% probability respectively, for an accuracy rate of .85 (95% Confidence Interval, .83 - .87). This means that the two-variable model, which uses only the initial conditions, is 80% as accurate as the model where we use a 13-year-long history of credit market behavior, including information that is contemporaneous (2016) to the classification.

²The full list of variable used is: i. number of months of the oldest open account; ii. initial credit score; plus for all the available years 2004 to 2016, iii. total number number of trades ; iv. total number of mortgages trades; v. total number of open trades; vi. total number of open mortgage trades; vii. total balance on open trades reported in the last 6 months; viii. total balance on open revolving trades reported in the last 6 months; ix. total number of revolving trades; x. total balance on open trades reported in the last 6 months; xi. total number of trades with max delinquencies of 30 days; xii. total number of trades with max delinquencies of 60 days; xiii. total number of trades with max delinquencies of 90 days; xiv. total number of trades with max delinquencies of 90-180 days. That is 156 variables.

2.7 Conclusions

Our empirical results point to the importance of initial conditions (credit scores) for the subsequent evolution of credit scores and other credit outcomes such as use of mortgage and revolving credit. A natural question arises: since initial credit scores are not based on past behavior of the specific individual, why do we see persistent effects of initial conditions over the life-cycle? One hypothesis is that initial credit scores already contain a lot of information about an individual's creditworthiness. Alternatively, our findings are also consistent with the hypothesis that since one needs a high credit score in order to obtain credit and credit to have a higher credit score, initial condition serve as a self-fulfilling prophecy by keeping individuals locked into their initial low credit bins by cutting them off from credit. In addition to the constraints imposed on individuals due to low credit scores outside of the credit market, reduced access to mortgage credit can have long term consequences for wealth accumulation via housing equity. Shedding more light on this issue is an important avenue for future research.

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Born Sub-Prime: Long-Term Impact of Early-Life Credit Access

Helena Bach

Pietro Campa

Giacomo De Giorgi

University of Geneva

University of Geneva

University of Geneva

Jaromir Nosal Davide Pietrobon

Boston College Lund University

Abstract: Using a comprehensive panel of credit reports, we exploit merger-induced branch closures to study the long-run effects of plausibly exogenous decreases in credit availability for young, low credit score individuals. We find that these events significantly worsen credit outcomes, and are associated with limited access to unsecured credit and significantly lower credit scores. In addition, we document substantial declines in the probability of relocating and worsened chances of relocating to high-income zip codes. We show that our results hold for young, low credit score and short credit history individuals, consistent with a mechanism by which reduced access to credit prevents young borrowers from building credit histories and thereby trapping them in a low-score and low-access state. Our findings highlight the importance of early access to credit in shaping long term financial and geographic mobility and point to the potential for targeted interventions to improve credit access for young individuals with limited credit histories.

3.1 Introduction

Credit access is a crucial determinant of economic mobility, with implications on consumption smoothing, job search, financing higher education and wealth accumulation through housing decisions.¹ A growing body of evidence highlights the importance of initial credit access, documenting persistence in credit outcomes both over the lifecycle (Bach et al., 2023) and across generations (Ghent and Kudlyak, 2016), as well as disparities across race, geography, and parental background (Hendren et al., 2025). Does the persistence in credit access over the lifecycle reflect early screening of borrower types? Or is there a ‘credit trap’, in which initial low scores limit future access to credit, causing low scores to stick? This mechanism would directly impact economic inequality even between ex-ante identical individuals, further to exacerbate inequalities due to initial conditions.

Evidence of a credit trap would support the case for policy interventions aimed at expanding credit access for individuals with limited borrowing histories. In order to provide some clarity on this we exploit an empirical design where, in some narrowly defined neighbourhoods, young low credit score individuals have harder time accessing credit due to the quasi-random closure of a local bank branch. To this end, we exploit localized negative shocks to credit supply resulting from merger-induced bank branch closures, following Nguyen (2019). The intuition behind the identification strategy is that when large banks merge, they often have overlapping branches in close proximity. Some of these branches are closed during post-merger restructuring, not due to local economic conditions, but because of geographic redundancy. These branch closures give rise to quasi-experimental reductions in credit supply.²

Using a large panel of U.S. credit reports, we study how reduced credit access at the start of sub-prime borrowers’ credit histories affects their credit outcomes over time.³ To implement our research design, we define an individual as treated if, at the time of a merger between two banks with at least \$2 billion in pre-merger assets, he or she resided in a ZIP code where both the acquiring and target banks operated branches prior to the merger—henceforth, a ZIP code exposed to merger-induced branch redundancy. We then compare the outcomes of treated individuals with those of control individuals, defined as people who resided in ZIP codes that: (1) are within the same county as ZIP codes exposed to merger-induced branch redundancy; (2) had the same number of branches from large banks as the exposed ZIP codes; but (3) where the merging institutions did not *both* operate branches. We follow Callaway and Sant’Anna (2021)

¹See Guiso and Sodini (2013), Solis (2017), and Bos et al. (2018).

²Although online credit was already available in the early 2000s, spatial proximity to bank branches remains an important determinant of credit access (Anenberg et al., 2018; Argyle et al., 2022). Because our analysis focuses on young individuals with limited credit histories, access to credit through physical bank branches may play an especially important role.

³In line with industry standards, a sub-prime borrower is defined as an individual with a credit score below 660.

and use a staggered difference-in-differences model to estimate the effect of branch closures on credit outcomes up to 10 years after exposure.⁴

Identification relies on the assumption that, absent the merger, individuals residing in ZIP codes exposed to merger-induced branch closures would have experienced similar trends in credit availability as individuals residing in unexposed ZIP codes. This assumption is credible for two reasons. First, exposure to merger-induced branch closures is unlikely to be driven by ZIP-code-level economic shocks: on average across mergers, only 9% of a buyer bank's branches were located in ZIP codes where the target bank also operated a branch, suggesting that local conditions in those ZIP codes did not influence merger decisions. Second, our difference-in-differences design accounts for time-invariant differences between treated and control ZIP codes, requiring any confounding factors to vary both across space and over time to bias our estimates. Additionally, we find that credit scores and access for older individuals are unaffected by branch closures, suggesting that the observed effects on young borrowers are not driven by local economic conditions, which would have impacted both young and old individuals.

The number of branches evolves similarly in treated and control ZIP codes prior to the mergers. Following the mergers, however, treated ZIP codes experience a decline of about 0.5 branches, a reduction that persists over the subsequent decade. Merger-induced branch closures significantly reduce the credit available to individuals in treated ZIP codes. These effects, interpreted as intent-to-treat or reduced-form estimates, are large and persistent: credit card ownership drops by 2 percentage points (corresponding to a 5.5% decline relative to the 2004 sample average, which serves as the baseline throughout the analysis), and total credit card limit decreases by \$250 on impact and by as much as \$1,500 (20.2%) 10 years after treatment. This reduction in credit access translates in lower credit scores: 10 years after the event, treated individuals have credit scores that are 10–12 points lower and are over 4 percentage points (20.3%) less likely of being identified as prime borrowers. The drop in credit scores is not driven by increased delinquencies. The treatment has no statistically significant effect on the likelihood of late payments, defaults (defined as payments over 90 days past due), or Chapter 7 bankruptcies.⁵

Reduction of bank presence has also negative impact on mortgages and auto loans. Ten years after treatment, the average mortgage size declines by up to \$20,000, a 14.6% reduction relative to baseline. The average auto loan size also decreases by \$500 (5.8%) on impact, though this effect dissipates over time. Notably, treated individuals file more inquiries for these lines of credit. For example, auto loan inquiries increase by 0.3 (50%). This pattern suggests that customers must submit more applications to secure credit, potentially due to lenders' reduced

⁴To be able to estimate the long-term effects of branch closures, our analysis focuses on mergers filed between 2004 and 2007.

⁵While the likelihood of Chapter 13 bankruptcies marginally increases following treatment, the magnitude of this effect is far too small to account for the observed worsening in credit outcomes.

willingness to approve these individuals for loans. We also find that branch closures reduce geographic mobility. In the short run, treated individuals are 2 percentage points (5%) less likely to move outside their ZIP code. In the long run, they are 4 percentage points (13.1%) more likely to relocate to a low-income ZIP code and 6 percentage points (19.2%) less likely to relocate to a high-income ZIP code. These results suggest that constrained credit access not only reduces mobility, but also alters relocation patterns in ways that may limit access to opportunity.

To shed light on the mechanism behind our results, we study how branch closures differentially affect individuals depending on the amount of information already present in their credit reports at the time of treatment. Specifically, we categorize individuals based on whether they have inherited credit histories, as indicated by credit records that predate their entry into the credit bureau files.⁶ We find that the negative effects of branch closures on credit scores and borrowing are driven almost entirely by individuals entering the credit market without pre-existing histories. In the same spirit, we find that the negative effects of branch closures on credit outcomes are smaller and shorter-lived for older individuals aged 23–30 than for younger individuals aged 18–22. These findings are difficult to reconcile with a pure screening model, in which credit histories simply reflect underlying borrower quality. Instead, they point to the existence of a poverty trap in the credit market: for individuals who have not already been screened, reduced credit access leads to worse outcomes over time, which in turn hinders the accumulation of credit history and limits future access. These results suggest that policy interventions aimed at improving credit access for young or disadvantaged individuals may be self-sustaining—helping individuals escape bad equilibria in the credit market and reducing persistent inequalities in credit opportunities over the lifecycle.

This paper is the first to provide evidence on how changes in early credit access shape credit outcomes over the lifecycle. In doing so, it contributes to the large literature on economic mobility (Solon, 1992; Chetty et al., 2014; Jantti and Jenkins, 2015), by highlighting the role of credit availability as a determinant of access to opportunity. It also relates closely to (Ghent and Kudlyak, 2016) and (Bach et al., 2023), who document the persistence of credit outcomes both across generations and over the life-cycle.

Our findings indicate that restricted access to credit early in life not only limits short-term borrowing but also leads to persistent disadvantages in future credit scores, credit outcomes, and geographic mobility. The literature has identified several mechanisms through which credit availability can shape economic trajectories. First, access to credit helps smooth consumption, particularly when savings are limited (Zeldes, 1989; Jappelli, 1990; Deaton, 1991; Keys et al.,

⁶As in Bach et al. (2023), individuals with inherited credit histories are defined as those with credit records predating the year in which they turned 17—most likely because they were listed as co-users on their parents’ accounts while still minors.

2017; Hundtofte et al., 2019). Second, limited borrowing capacity constrain investment in human capital (Wiederspan, 2016; Solis, 2017) and housing (Topel and Rosen, 1988; Laufer and Paciorek, 2022). Third, credit access supports entrepreneurial activity among young individuals with limited capital (Evans and Jovanovic, 1989; Banerjee and Newman, 1993), and facilitate geographic mobility (Bilal and Rossi-Hansberg, 2021; Giannone et al., 2023; Molloy et al., 2022) by enabling moves to locations with stronger economic opportunities (Chetty et al., 2016). Finally, credit scores influence outcomes beyond credit markets, including employment decisions (Bos et al., 2018; Ballance et al., 2020), and access to rental housing or utility services.

Our results on the role of credit access in enabling relocation to high-opportunity areas are particularly related to Bos et al. (2018), who use a natural experiment in Sweden to show that negative credit history information reduces employment and geographic mobility. Our approach differs in two respects First, while Bos et al. (2018) examine the effects of changes in the information available to potential lenders and employers, we study the effects of an exogenous reduction in credit supply. Second, whereas their focus is primarily on labor market outcomes, we analyze a broader set of credit outcomes—including various types of debt and delinquency behavior—and show that the negative effects are concentrated among young individuals with limited credit histories.

The remainder of the paper is organized as follows. Section 3.2 describes the data, with particular focus on the Experian credit reports and the construction of the main sample. Section 3.3 outlines the empirical strategy. Section 3.4 presents the main results. Section 3.5 investigates underlying mechanisms and reports robustness checks. Section 3.6 concludes. Additional results and descriptive statistics are provided in the Appendix.

3.2 Data

3.2.1 Data from Credit Reports

We use proprietary credit report data from Experian, covering approximately 2.2 million individuals—representing a 1% random sample of the U.S. population who had a credit score in 2010. The dataset spans the years 2004 to 2016, with observations collected annually as of June 30. It includes basic demographic information such as date of birth and ZIP code of residence, along with a rich set of about 400 credit-related variables. These capture a wide range of credit outcomes, including the number of credit accounts (referred to as “trades”), credit cards, card balances and limits, mortgage holdings and balances, as well as indicators of delinquency, default, and bankruptcy.⁷

⁷For other papers using Experian’s data on credit reports, see ?, De Giorgi et al. (2021), Bach et al. (2023), De Giorgi et al. (2024), and De Giorgi and Naguib (2024).

Our sample consists of 22,890 young customers with low initial credit scores. To construct this sample, we first select individuals who, in the year 2004, are 22 years old or younger and have a credit score below 600. These criteria are satisfied for 51,284 individuals with complete histories.⁸ To implement the research design presented in Section ??, we further restrict the sample to individuals who, at the time of a bank merger between 2005 and 2007, were living either in ZIP codes with branches from both merging banks or in the same county as such ZIP codes. This restriction excludes 28,394 individuals who were not residing in any county affected by a bank merger between 2005 and 2007.

Table 3.1 presents summary statistics for the sample in 2004, 2010, and 2016. Panel A presents general statistics on demographics, credit scores, and overall credit utilization pooling together all types of credit lines.⁹ Panel B focuses on our main credit line of interest: credit cards. We observe credit outcomes for our panel of 22,890 individuals over 12 years, from their twenties and early thirties. These individuals begin their life with a low credit score by the definition of our sample: in 2004, the average credit score is 526. As already noticed in Bach et al. (2023), this initial condition is remarkably persistent: by 2016, only 21% of the individuals in our sample cross the threshold of 660 (defined by Experian as that of a *good* credit score) and the average score is still below 600. Interestingly, while the number of credit lines and their balance increase by a factor of 3 and 10, respectively, the average number of delinquent credit lines remains stable around 0.1.

Panel B of table 3.1 focuses on credit cards. At the beginning of the panel, 36% of the individuals in our sample have at least one credit card. By 2016, this share is increased to 54%. Notably, credit card holders have multiple credit cards open at any given point in time: 2.72 on average in 2004 and 4.31 in 2016. The average credit card holder has a credit limit of around \$2,380 in 2004, which increases to \$13,600 in 2016. Inspecting utilization, we observe that the average credit card holder in the sample is likely to exhaust their credit limit in 2004, with a utilization rate of 85.3%. This number drops to 52.9% in 2016. This pattern is mirrored by a decrease in the number of credit cards with a delinquency of more than 90 days, which drops from 0.56 in 2004 to 0.29 in 2016.

Overall, our sample represents customers with low credit scores and limited access to credit at the beginning of the panel. For those who gain access to credit cards, we observe a substantial relaxation over time of their borrowing constraints, coupled with a gradual decrease in utilization rates. This is accompanied by a reduction in the number of delinquent cards, suggesting that those individuals are able to manage their credit lines better over time.

⁸This number excludes 3,177 individuals with incomplete histories.

⁹Table 6.5 in the Appendix details the number of credit lines separately for mortgages and auto loans.

Table 3.1: Summary Statistics of Credit Outcomes (2004–2016)

	2004	2010	2016
A: Customer Information			
Age	20.65 (1.22)	26.65 (1.22)	32.65 (1.22)
Credit Score	526.17 (48.06)	553.47 (80.64)	583.53 (92.10)
Good Credit Score (> 660)	0.00 (0.00)	0.12 (0.32)	0.21 (0.41)
N Open Credit Lines	1.51 (2.34)	2.27 (3.13)	4.20 (5.06)
Balance	4,784.71 (17,984.39)	19,597.01 (52,033.17)	43,925.22 (88,766.52)
N Credit Lines 30d+ Delinquent	0.10 (0.37)	0.05 (0.28)	0.07 (0.35)
N Inquiries	3.34 (2.68)	2.84 (2.40)	2.91 (2.59)
B: Credit Cards			
P Credit Card Holder	0.36 (0.48)	0.34 (0.47)	0.54 (0.50)
N Credit Cards	0.98 (1.84)	1.17 (2.14)	2.33 (3.47)
Credit Limit, CC	856.84 (3,046.16)	2,222.70 (6,221.81)	7,357.94 (15,378.57)
Utilization %, CC	85.30 (50.69)	56.76 (42.87)	52.99 (37.89)
P Credit Card 90d+ Delinquent	0.30 (0.46)	0.19 (0.39)	0.15 (0.36)
N Inquiries, CC	1.33 (1.51)	1.05 (1.32)	1.23 (1.58)
Observations	22890	22890	22890

Note: the table reports summary statistics for the sample of individuals from 18 to 22 years old with low credit scores in 2004. The table reports the average and standard deviation of the variables listed in the first column. Panel A reports general statistics on demographics, credit scores, and overall credit utilization pooling together all types of credit lines. Panel B focuses on credit cards. Averages are computed over the whole sample, except for Utilization rate of credit cards, which is defined only for those having a credit card.

3.2.2 Bank Branches and Mergers

We use publicly available data from the Federal Deposit Insurance Corporation (FDIC) to geolocate bank activity in each ZIP code. FDIC data on bank deposits include information on the universe of banking institutions active in the US and their branches. Most importantly for our purpose, for each active bank branch, we observe the ZIP code in which they operate and the identifier of financial institution they belong to as of the 30th of June of each year. The same identifier for the financial institution is included in the register of business combinations, which reports data on merger agreements among banks active in the US. From the register we can identify the banks involved in each merger, their role (acquirer or acquired) and the date of each merger. To study the effect of merger-induced branch closures, we focus on mergers that involve large financial institutions between the 1st of July 2004 and the 30th of June 2007 among banks holding more than \$2 Billion in pre-merger Asset. Table 3.2 reports the list of the banks involved in the mergers included in our analysis.

Table 3.2: Mergers Among Large Banks (2005-2007)

Buyer	Target	Date
Sky Bank	The Second National Bank of Warren	02/07/2004
The Provident Bank	First Savings Bank	14/07/2004
Sovereign Bank	Compass Bank for Savings	23/07/2004
JPMorgan Chase Bank	Bank One, N. A.	13/11/2004
Fifth Third Bank	First National Bank of Florida	01/01/2005
Wachovia Bank, N. A.	SouthTrust Bank	03/01/2005
First Niagara Bank	Hudson River Bank & Trust Company	14/01/2005
North Fork Bank	GreenPoint Bank	22/02/2005
Associated Bank, N. A.	First Federal Capital Bank	19/02/2005
Sovereign Bank	Waypoint Bank	11/02/2005
National City Bank	The Provident Bank	04/03/2005
SunTrust Bank	National Bank of Commerce	22/04/2005
Bank of the West	Commercial Federal Bank	03/12/2005
TD BankNorth, N. A.	Hudson United Bank	31/01/2006
The Huntington National Bank	Unizan Bank, N. A.	01/03/2006
Sovereign Bank	Independence Community Bank	09/09/2006
Washington Mutual Bank	Commercial Capital Bank, FSB	01/10/2006
MB Financial Bank, N. A.	Oak Brook Bank	02/11/2006
Regions Bank	AmSouth Bank	04/11/2006
New York Community Bank	Penn Federal Savings Bank	02/04/2007
Citizens Bank	Republic Bank	28/04/2007

Note: the table reports the list of mergers among large banks between the 1st of July 2004 and the 30th of June 2007 among banks holding more than \$2 Billion in pre-merger Asset. The table reports the name of the acquirer and target banks and the date of the merger as filed from the FDIC.

In table 3.3, we report descriptive statistics for the banks involved in the mergers we consider. Out of a total of 21 mergers, the average acquiring institution has almost 600 branches operating in 460 ZIP codes, while the average target bank has 217 branches operating in 166 ZIP codes. Overall, buyer banks are larger than the institutions they acquire, as measured by average deposits among the two types of institutions. Importantly, the average buyer bank operates only 9% of its branches in a ZIP code where the target bank also operates at least one. The share of deposits in overlapping branches is comparable. This suggests that overlapping branches are a small portion of the buyer's bank branches and are not financially more crucial than others as measured by the value of their deposits. Appendix Map 6.1 illustrates the geographic distribution of counties with overlapping buyer and target branches operating in the same ZIP codes, highlighting that such closures are more concentrated in densely populated areas.

Table 3.3: Merging Institutions (2005-2007)

	Buyer	Target
N Branches	598.75 (658.22)	217.33 (349.30)
N Zip	460.60 (488.07)	166.43 (256.07)
Deposits	52,359.31 (75,159.85)	13,749.68 (28,253.01)
Share Overlapping Branches	0.09 (0.08)	0.25 (0.17)
Share Overlapping Zip	0.08 (0.07)	0.24 (0.17)
Share Overlapping Deposits	0.10 (0.11)	0.22 (0.17)
Observations	21	21

Note: the table reports descriptive statistics for the merging institutions involved in the mergers listed in table 3.2. The table reports the average and standard deviation of the variables listed in the first column, separately for buyer and target banks.

3.3 Empirical Strategy

The goal of our empirical analysis is to study the effect of exogenous credit reduction for young customers with low credit scores. To isolate negative supply shocks from demand factors, we exploit merger-induced local bank branch closures, adapting the approach of Nguyen (2019). In this section, we first discuss the role of local branches for credit access, then we describe our identification strategy and finally we present the empirical model we use to estimate the effects of interest.

Local branches matter for credit access as they reduce the search costs faced by customers looking for financial products. While this statement is still true after the advent of online banking Argyle et al. (2022), geographical proximity to a bank office was even more crucial in the years we consider. As of 2007, two out of five US customers reported that geographical proximity was the most important reason driving the choice of their financial institution.¹⁰ More generally, closing a local branch office reduces the choice set of financial institutions available to customers within reach at a given cost. Our results support the interpretation that this worsens access to credit, especially for young, low credit score individuals.

The geographical distribution of bank branches (and their closures) likely reflects local credit demand. To isolate supply-side effects, we exploit merger-induced branch closures as plausibly exogenous shocks to locally available credit. Consider a merger between two large financial institutions. Prior to the merger, some ZIP codes may have branches from both institutions. During post-merger restructuring, these overlapping branches are often closed to reduce operational costs, as they become geographically redundant under common ownership. As a result, the ZIP codes in which they operate serve as treated, when compared with ZIP codes with similar bank presence before the merger, since the reduction in the number of branches (and banks) is not driven by local demand factors but by geographical redundancy.

To further address potential differences in time-invariant characteristics across ZIP codes, we implement a difference-in-differences design to compare outcomes between treated and control areas. This approach relies on the identifying assumption that in the absence of the merger, ZIP codes with overlapping branches would have experienced similar trends in credit availability as ZIP codes with comparable pre-merger bank presence.

This assumption is credible for two main reasons. First, ZIP codes with overlapping branches account for a small portion (on average less than 10%) of the merging banks' overall presence, making it unlikely that local economic conditions influence the merger decision. Second, the difference-in-differences design controls for time-invariant differences between treated and control ZIP codes, meaning that any confounding factors would need to vary both across locations

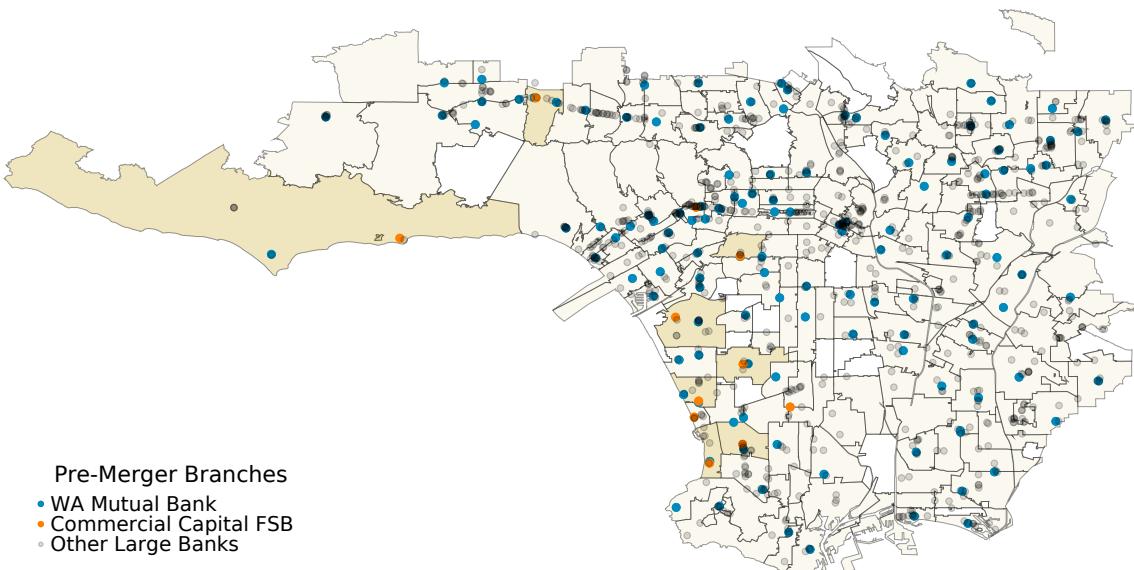
¹⁰As of 2007, 40.7% of US credit customers interviewed by the SCF FED answered "Location of their office" to the question "What is the most important reason you chose the institution that you did for your main checking account?". Source: authors' calculations and Anenberg et al. (2018)

and over time to bias our estimates. In this regard, the absence of differential pre-trends in bank activity provides further support for the validity of our identification strategy.

To implement the research design, we focus on mergers between large financial institutions (defined as those with more than \$2 billion in pre-merger assets) occurring between 2005 and 2007.¹¹ For each merger, we define treated ZIP codes as those where both the acquiring and target banks operated a branch in the year prior to the merger. The control group consists of ZIP codes within the same county as the treated areas. When comparing the evolution of outcomes between these groups in a staggered difference-in-differences design, we control for the number of large banks active in each ZIP code at baseline in 2004.

As an illustration of our empirical strategy, Figure 3.1 reports the geolocation of branches from Washington Mutual Bank, Commercial Capital FSB and other large banks as 2005. Following the 2006 merger between the two mentioned banks, all ZIP codes hosting branches from both the buyer and the target bank are considered as treated (darker shaded areas) while other ZIP codes with large banks are included in the control group (lighter shaded areas).

Figure 3.1: Bank Branches in Los Angeles County, CA 2005



Note: The map shows the locations of bank branches in the ZIP codes of Los Angeles County, CA, in 2005. Blue dots represent branches of the acquiring bank, Washington Mutual Bank, while orange dots denote branches of the target bank, Commercial Capital FSB. Shaded black dots indicate branches of other large banks. Dark-shaded areas represent ZIP codes with overlapping branches from both WA Mutual Bank and Commercial Capital FSB, which constitute the treated group. Light-shaded areas represent other ZIP codes with large banks, which are included in the control group. Source: FDIC, authors' own calculations.

¹¹The choice of merger years is driven by our data structure and identification strategy. Since our dataset begins in 2004, selecting mergers from 2005 onward ensures that we observe an untreated pre-merger period for all cases. Additionally, we exclude mergers occurring after 2007 to avoid confounding effects from the 2008 financial crisis. Results are robust to the inclusion of mergers happened in 2008.

To compare the evolution of outcomes between treated and control ZIP codes, we follow the approach of Callaway and Sant'Anna (2021) and estimate the staggered DiD model:

$$B_{r,t} = \alpha_r^\tau + \gamma_t^\tau + \sum_l \beta_l^\tau (Z_r^\tau \cdot \mathbf{1}\{t = \tau + l\}) + \lambda_t^\tau \mathbf{X}_r + \epsilon_{r,t}^\tau. \quad (3.1)$$

$B_{r,t}$ represents the outcome of interest for ZIP code r in year t , α_r^τ and γ_t^τ are ZIP code and time year effects, respectively, and \mathbf{X}_r is a vector of ZIP code characteristics at baseline year 2004.¹² The variable Z_r^τ equals one for ZIP codes which, in the year $\tau - 1$, host branches belonging to different banks eventually merging among each other in year $\tau \in \{2005; 2006; 2007\}$, and zero for ZIP codes never hosting such overlapping branches. The coefficients β_l^τ capture the average treatment effect for treated ZIP codes at time $\tau + l$. Following Callaway and Sant'Anna (2021), we estimate the model separately for each year of mergers τ , including in the control group only never-treated ZIP codes, and present the results in an event study format by averaging the β_l^τ across merger years τ for each lead or lag l relative to the merger date.

We further modify model (3.1) to compare outcomes among individuals differently exposed to the merger, based on whether they live in a treated ZIP code or not in the year of a merger. To do so, we estimate:

$$Y_{i,t} = \mu_i^\tau + \kappa_t^\tau + \sum_l \delta_l^\tau (Z_{r(i,\tau)} \cdot \mathbf{1}\{t = \tau + l\}) + \phi_t^\tau \mathbf{X}_{r(i,\tau-1)} + e_{i,t}^\tau. \quad (3.2)$$

$Y_{i,t}$ represents the outcome of interest measured for individual i in year t , while μ_i^τ and κ_t^τ are individual and year fixed effects, respectively. The operator $r(i, \tau)$ denotes the ZIP code where each individual i lives in the year τ . Thus, the variable $Z_{r(i,\tau)}$ is equal to one for individuals living in treated ZIP codes in the year of a merger and zero for individuals who never lived in a treated ZIP code when a merger took place. $\mathbf{X}_{r(i,\tau-1)}$ is the same vector of ZIP code characteristics included in (3.1) measured for the ZIP code in which the individual is living in the year prior to the merger. As above, the coefficients δ_l^τ capture the average treatment effect for individuals in treated ZIP codes at time $\tau + l$ and we use the same method from Callaway and Sant'Anna (2021) to aggregate our results across different merger years.

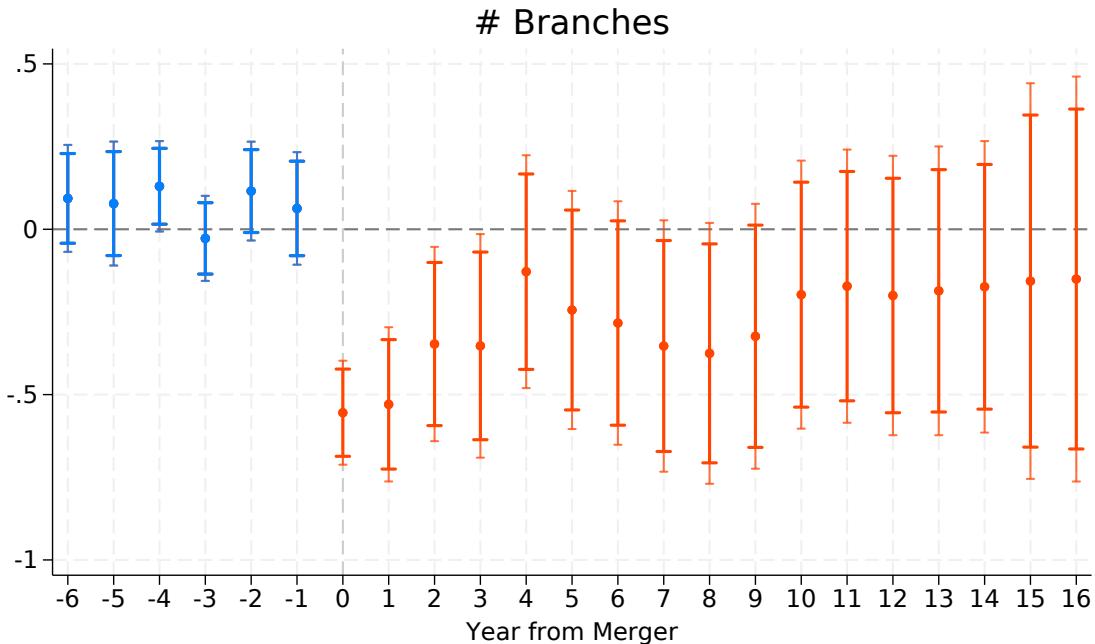
¹²In our preferred specification, this vector includes a set of indicators for the number of large banks operating a branch in ZIP code r as of 2004. Our results are robust to the inclusion of additional controls based on 2000 Census ZIP code demographics, such as population, median income, and the share of households below the poverty line.

3.4 Results

3.4.1 Mergers and Branch Consolidations

The first question we address in this section is whether mergers between large banks lead to branch closures that disproportionately affect ZIP codes where both the acquiring and target banks have a presence. To do so, we estimate model (3.1), using as a dependent variable the number of branches active in a ZIP code in a given year. Figure 3.2 reports the average treatment effect (ATT) in an event-study fashion, comparing differences in trends between treated and control ZIP codes before and after the merger.

Figure 3.2: Number of Open Branches, Event Study



Note: This figure illustrates the relationship between exposure to mergers and the number of branches operating in a ZIP code, based on differences in trends between treated and control zip codes, as specified in model 3.1. The bars represent 95 and 90 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated ZIP codes for each merger between 2005 and 2007, using the method developed by Callaway and Sant'Anna (2021).

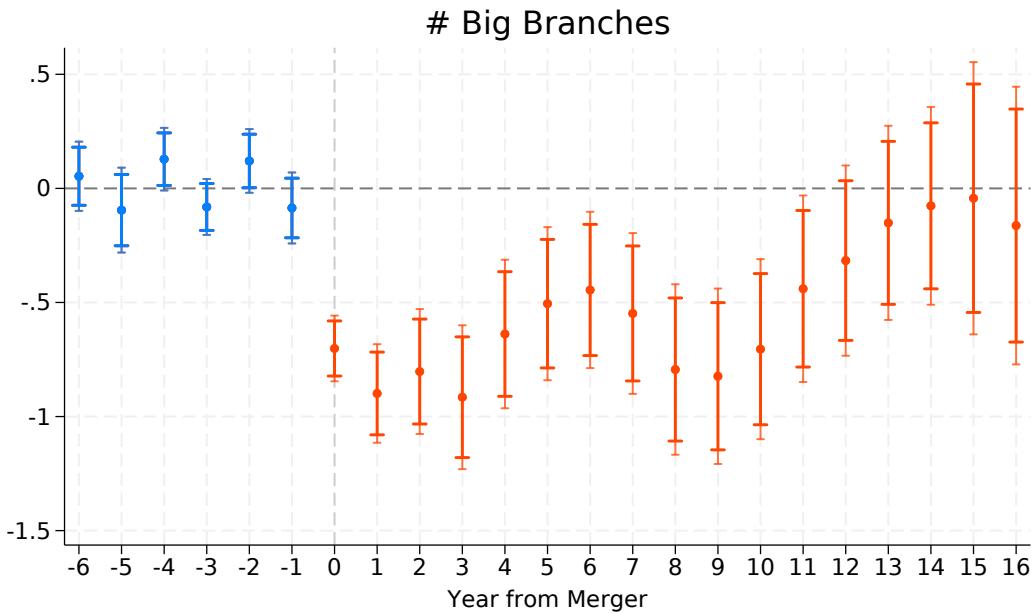
The number of branches in treated ZIP codes significantly decreases after the merger. The ATT is negative and significant in the short-run: areas served by both the buyer and the target bank experience a reduction of around 0.5 branches in the three years following the merger.¹³ This suggests that mergers between large banks lead to branch closures in areas where both

¹³This estimate is consistent with Nguyen (2019), who finds a decrease in average number of branches of around 0.3 in the first 2 years after the merger. Notice that our results involve mergers happening in different years and involving different banks.

banks have a presence, which is consistent with the idea that mergers can lead to reduced credit access because of an increase in the average distance from a branch or decreased competition among credit providers. Reassuringly, the treated and control areas exhibit similar trends before the merger, reinforcing the credibility of the assumption of parallel trends absent the merger.

Figure 3.3 replicates the same design, using as a dependent variable the number of branches from large banks only (\$2bn or larger in terms of assets). The short-run effect of a merger is qualitatively similar: in the aftermath of the merger, areas with overlapping branches are more likely to experience a branch closure, resulting in a decrease of the number of bank branches of around 0.8. However, this effect is larger and persists in the long run up to 10 years after the merger. This suggests that while mergers between large banks result in the closure of one branch, over time they are accompanied by a higher entry rate of smaller institutions compared to the control group, which mitigates the overall impact on the total number of branches.

Figure 3.3: Number of Open Branches, Event Study (Large Banks Only)



Note: This figure illustrates the relationship between exposure to mergers and the number of branches owned by large banks operating a zip code, based on differences in trends between overlapping and non-overlapping ZIP codes, as specified in model 3.1. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated ZIP codes for each merger between 2005 and 2007, using the method developed by Callaway and Sant'Anna (2021).

Overall, the results support the notion that mergers among large banks are unlikely to be driven by concerns over local competition. First, before the merger, bank activity evolved along parallel trends in treated and control ZIP codes. If mergers were driven by different pre-existing trends in local demand, we would expect to see differential changes in branch activity prior to the merger, as banks are free to relocate their branches at any time. Second, while the number of large bank branches is persistently reduced after the merger, smaller financial institutions increase their presence disproportionately more in treated ZIP codes in the following years. This pattern makes it unlikely that the merger and subsequent branch closure was driven by a deterioration in local demand, which would have had the opposite effect on the presence of smaller institutions.

In conclusion, the results of this section are consistent with mergers inducing branch closures motivated by geographical redundancy from the perspective of the buyer bank. This implies a reduction in the number of financial institutions available at a given distance for customers seeking financial services. We interpret this variation as a supply side shock to the offered financial services in the area.

3.4.2 Unsecured Credit and Credit Scores

We now turn to the main question of this paper: how do negative shocks to credit supply affect the credit outcomes of young individuals? To answer this question, we estimate the model in equation (3.2) using as dependent variable various measures of credit access and financial behavior. We interpret our results as reduced form estimates, which leverage the variation in branch closures induced by mergers as an exogenous instrument for credit supply. As such, the effects that we present in this section can be thought of as the effect of an average reduction in the number of branches available to young individuals of around 0.5.

Figure ?? presents the main results on access to credit cards.¹⁴ The top-left panel shows that a merger-induced reduction in bank branch availability decreases the probability of holding a credit card by 2*p.p.* on impact, equivalent to 5.6% of the sample average in 2004. This effect persists for up to 10 years after the merger, although its statistical significance diminishes over time. The top-right panel documents an average reduction of approximately 0.2 in the number of open credit cards, an effect that remains statistically significant throughout the 10-year post-merger period. The bottom-left panel shows a gradual decline in the total amount of credit available to customers: the average credit limit (calculated across all individuals, not just credit card holders) decreases by about \$500 within three years of the merger and continues to decline thereafter. Ultimately, exposed customers experience a reduction of approximately \$1,500 in available credit. Turning to default behavior, the bottom-right panel shows mixed

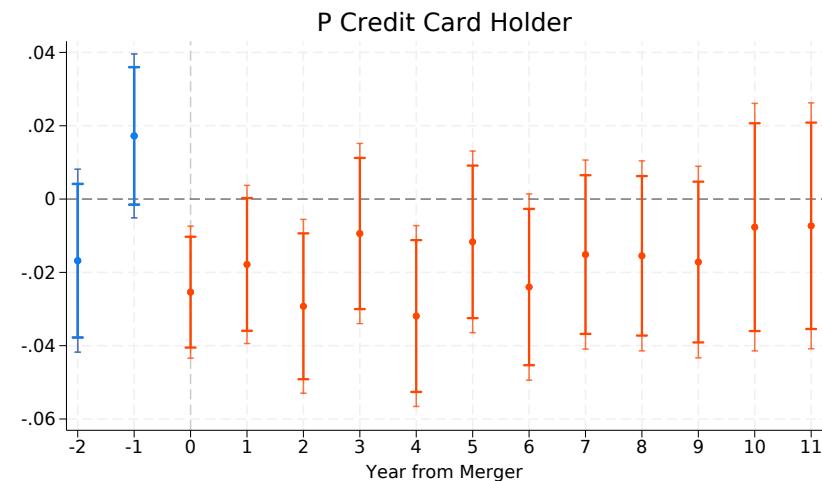
¹⁴In figures based on credit bureau data, we can only show two years of pre-trends due to our data starting in 2004.

evidence on the probability of having a late payment on a credit card (90-day delinquencies), with medium-term drop and no significant impact in the short- or long-run. This finding is confirmed by results for bankruptcy filings in Appendix figure 6.3, where we find insignificant impact on Chapter 7 bankruptcies, and small and non-monotonic effect on Chapter 13.

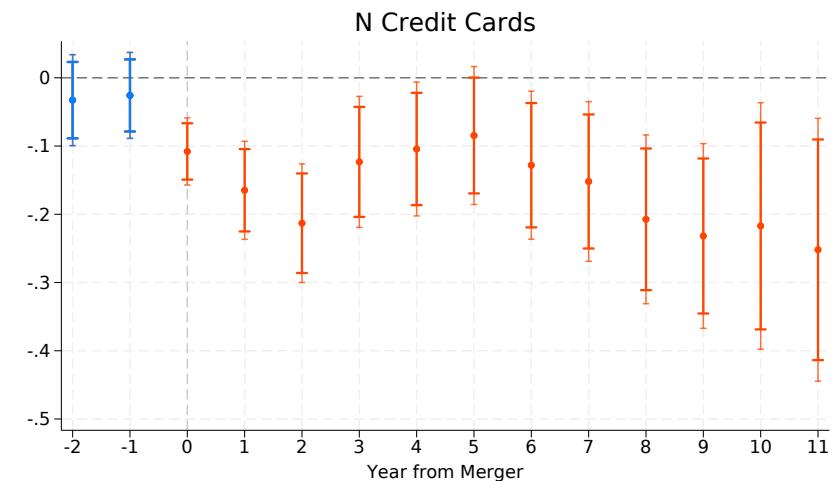
Overall, these results suggest that branch closings significantly affect the amount of unsecured credit young customers can access through credit cards. In addition to providing a tool for smoothing income and expense shocks, credit cards are one of the primary tools for building a credit history through consistent usage and timely payments. Hence, reduced access may hinder credit score development and, in turn, limit access to other forms of credit in the future. We investigate these outcomes next.

The two panels of figure 3.5 describe the dynamic effect of the negative credit supply shock on credit scores. On average, young customers facing reduction in credit availability experience a decrease in their credit score of 3-6 points in the immediate aftermath of the merger, and this difference grows over time up to 12 points 10 years after the merger. This effect, besides being statistically significant at standard levels of confidence, is also large enough to move customers from one *type* of credit profiling to another. In the bottom panel of figure 3.5 we document the change in the probability of having a credit score above the threshold value of 660, which Experian defines as the minimum for Credit Score to be categorized as *Good*. This probability decreases by 1*p.p.* on impact, and continues to decrease up to 4*p.p.* after 10 years. The magnitude of the estimated effect is equivalent to a reduction of around 20% of the sample average of this probability in 2016. This finding is particularly relevant, as it suggests that an early reduction in credit availability has long-lasting effects on the credit standing of young individuals, which may have implications for their future access to credit and financial opportunities.

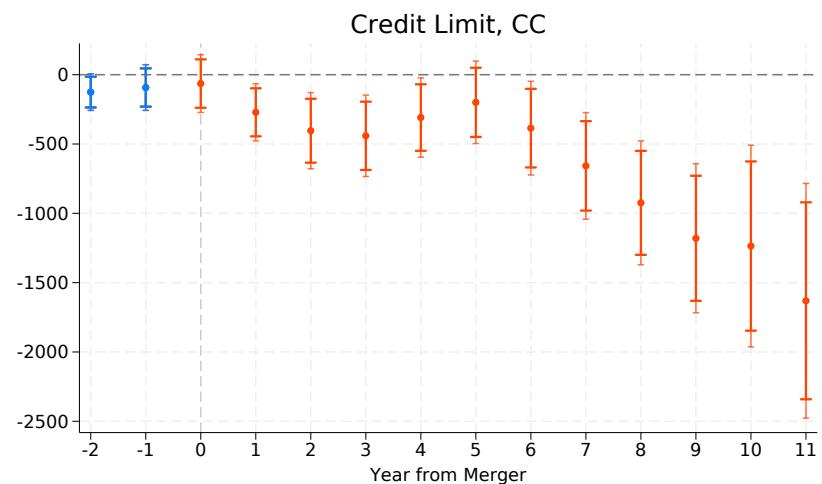
Figure 3.4: The Effect of Credit Shock on Credit Cards



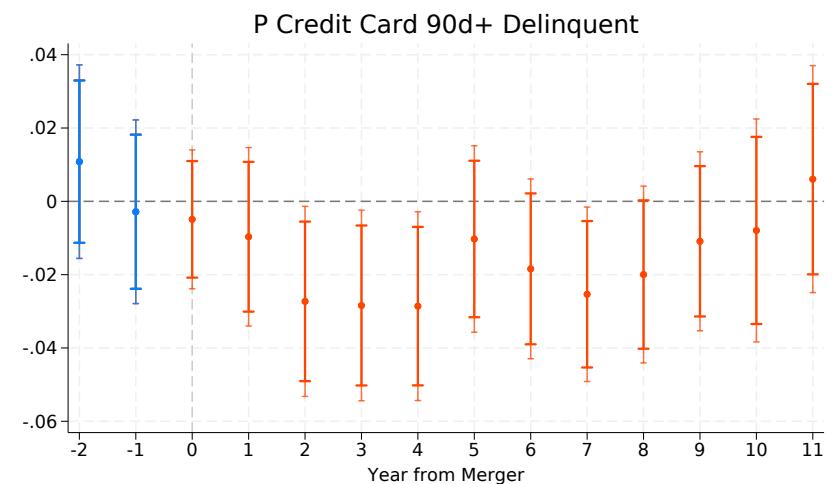
(a) Share of Credit Card Holders



(b) Number of Open Credit Cards

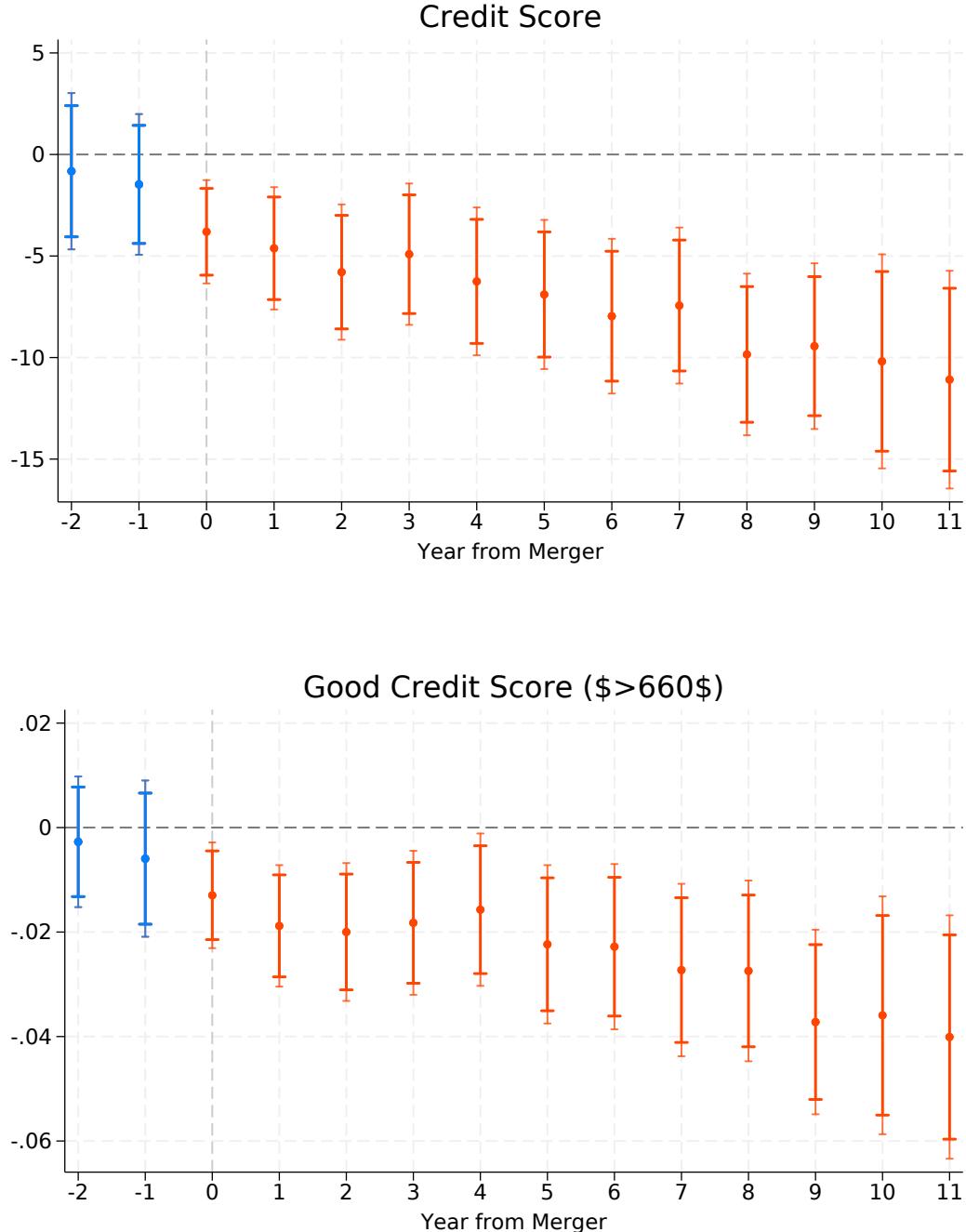


(c) Credit Limit



(d) Share of delinquent (90d+) Credit Card Holders

Figure 3.5: The Effect of Credit Shock on Credit Score



Note: Each plot reports the relationship between exposure to mergers and different measures of credit scores. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated individuals for each merger between 2005 and 2007, using the method developed by Callaway and Sant'Anna (2021). The dependent variable is reported above each plot.

3.4.3 Mortgage and Auto Loans

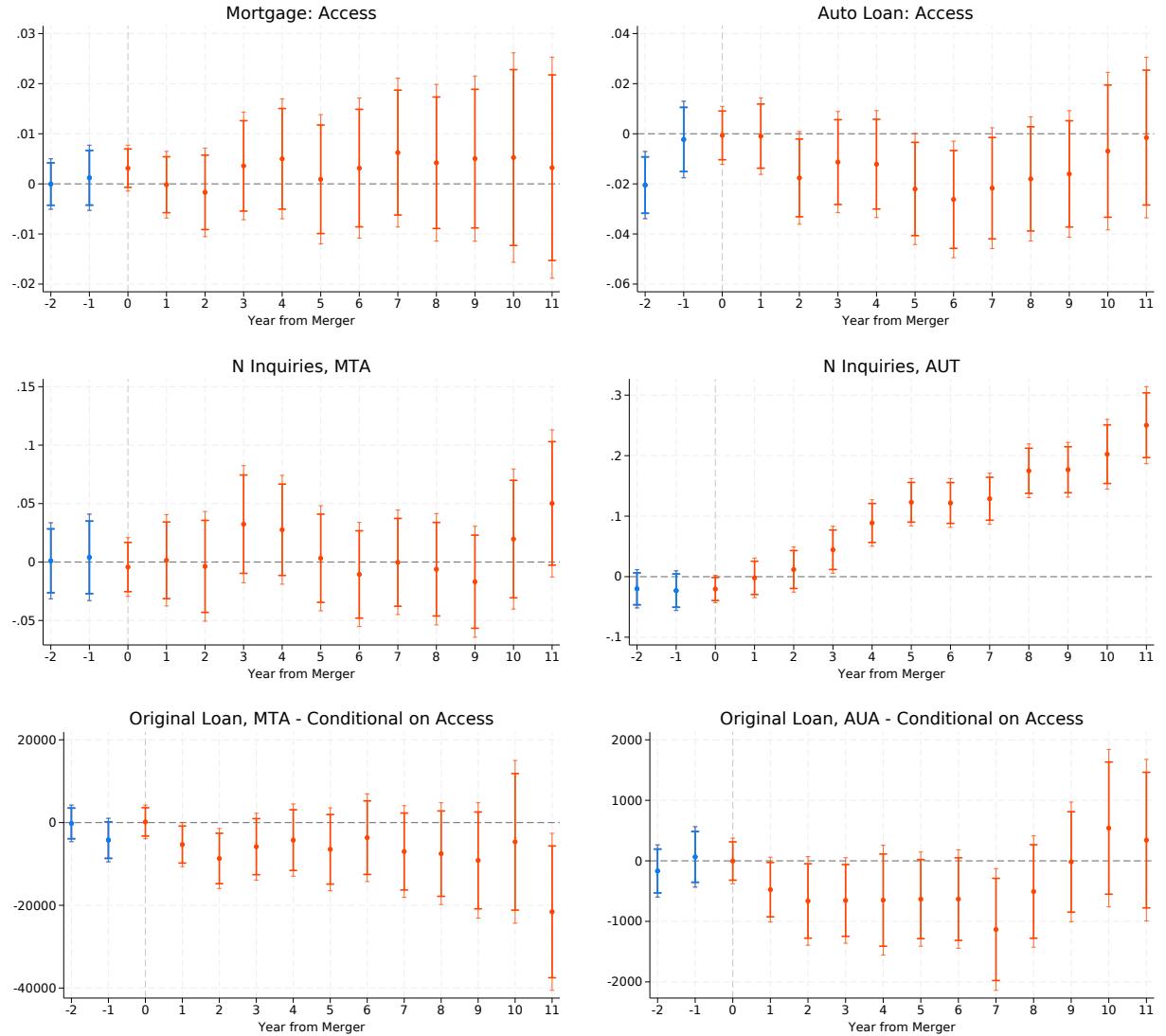
A decline in an individual's credit standing (e.g., a drop in their credit score) can significantly affect their access to credit, as financial institutions use credit profiles to determine the terms and availability of financial products offered to customers. In this section, we explore this dimension by studying the evolution of access to mortgages and car loans. According to the 2007 Survey of Consumer Finances, mortgages accounted for 70% of total household debt in the U.S., followed by auto loans at 9%. Beyond their larger nominal value, these two types of credit are particularly important because they support critical household investments. Mortgages enable investment in home equity and facilitate access to housing in areas with greater economic opportunity, potentially with significant implications for economic mobility. Auto loans, meanwhile, facilitate access to transportation, which is often necessary for commuting and accessing employment opportunities.

The left panels of Figure 3.6 show the effect of the merger-induced reduction in credit availability on mortgages. While we find no significant change in the probability of holding a mortgage or in the likelihood of applying for one (resulting in an hard inquiry registered by Experian), there is a notable decrease in the original loan amount among those who do take out a mortgage. Specifically, among mortgage holders, the original loan amount declines by approximately \$5,000 in the first two years following the merger. This difference becomes noisy and loses significance in the intermediate years, but reemerges in the final year of the sample, when treated customers have mortgage loan amounts that are about \$20,000 lower than those of control customers, equivalent to 14.61% of the average loan amount among mortgage holders in 2016.

The right panels of Figure 3.6 present the corresponding estimates for auto loans. Individuals exposed to the merger-induced credit shock experience a reduction of approximately 2*p.p.* in the probability of holding an auto loan between the second and the eight year after the merger, equivalent to about 10% of the sample average in 2004. However, this effect is not statistically significant in the following years, and its point estimate gradually converges to zero. In contrast, affected customers show an increase in the number of inquiries for auto loans, with the average rising by up to 0.3 inquiries (50% of the unconditional mean in 2016) ten years after the merger. The average original loan amount decreases by about \$500 (9.4% of the average in 2010) in the first eight years post-merger, though this difference dissipates over time.

Overall, these findings suggest that a negative credit supply shock does not significantly alter young individuals' probability of holding a mortgage but does limit their ability to secure larger loan amounts. For auto loans, the shock appears to reduce the effectiveness of credit-seeking behavior, potentially delaying access to this form of credit.

Figure 3.6: The Effect of Credit Shock on Mortgages and Auto Loans



Note: Each plot reports the relationship between exposure to mergers and different outcomes related to mortgages and auto loans. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated individuals for each merger between 2005 and 2007, using the method developed by Callaway and Sant'Anna (2021). The dependent variable is reported above each plot. Mortgage Original Loan amounts are reported conditional on having a mortgage at any time between 2004 and 2016. Auto Original Loan amounts are reported unconditionally on having a loan.

3.4.4 Geographic Mobility

The ability to move to a new location is a crucial aspect of economic mobility, as it allows individuals to access better job opportunities and improve their overall economic conditions. In this section, we examine how the merger-induced credit supply shock affects the likelihood of moving to a different address and the average income level at the new location.

Before turning to the results, we present descriptive statistics on the geographic distribution of individuals in our sample over time. Using publicly available IRS data, we compute the average income level of each ZIP code and rank them based on their position in the national distribution for each year. Table 3.4 shows that our sample is disproportionately concentrated in lower-income areas: 42% of the individuals live in ZIP codes in the bottom income tercile in 2004, and 39% do so in 2016.¹⁵ As shown in panel A of Figure 3.7, individuals in our sample tend to relocate relatively early. By 2005, 40% had already moved out of their 2004 ZIP code, and by 2010, the cumulative share of movers had nearly reached its peak at 80%.

Since access to credit can influence migration decisions through multiple channels, we now turn to examining the effects of credit supply shocks on geographic mobility. The top panel of Figure 3.7 shows a drop of 2 *p.p.* (5% of the sample average in 2005) in the probability of moving to a different ZIP code in the first four years after the merger. Moreover, beyond the average response, there are offsetting effects on mobility to higher-income versus lower-income ZIP codes, as measured by IRS average income. The bottom panels of figure 3.7 show that in the long-run, treated individuals are 4 *p.p.* (13%) more likely to have moved to a low income ZIP code and 6 *p.p.* (19%) less likely to have moved to a high income ZIP code. Overall, treated individuals delay their choice of relocating away from their original ZIP code in the first years after the shock, and, once they move, they relocate to lower income areas. These results are consistent with our prior findings on mortgage amount, since high income ZIP codes are more likely to be characterized by higher house prices. This is consistent with the idea that limited access to credit may constrain individuals' ability to move to locations which offer better economic opportunities, as they may be unable to finance housing or other relocation-related expenses.

¹⁵Moreover, as shown in Appendix Figure 6.6, this initial concentration is persistent: by 2016, two out of three individuals who lived in a low-income ZIP code in 2004 are in a similarly ranked ZIP code in 2016.

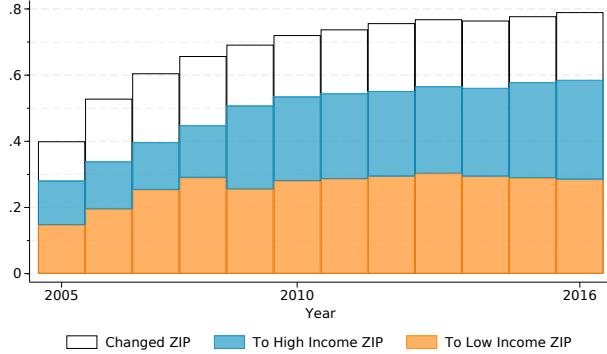
Table 3.4: Summary Statistics of Geographic Outcomes (2004–2016)

	2004	2010	2016
ZIP Income - Percentile	43.53 (31.38)	45.21 (32.37)	47.40 (32.21)
Bottom 33% ZIP - Income	0.42 (0.49)	0.43 (0.49)	0.39 (0.49)
Top 33% ZIP - Income	0.30 (0.46)	0.34 (0.47)	0.36 (0.48)
Changed ZIP	0.00 (0.00)	0.72 (0.45)	0.79 (0.41)
Changed ZIP, to Low Income ZIP	0.00 (0.00)	0.28 (0.45)	0.29 (0.45)
Changed ZIP, to High Income ZIP	0.00 (0.00)	0.25 (0.44)	0.30 (0.46)
Observations	22,890	22,890	22,890

Note: The table reports summary statistics for the main sample ZIP code characteristics. The table reports the average and standard deviation of the variables listed in the first column. For each year, we classify ZIP codes into terciles based on their average income rank in the national distribution of average income as computed from IRS publicly available data.

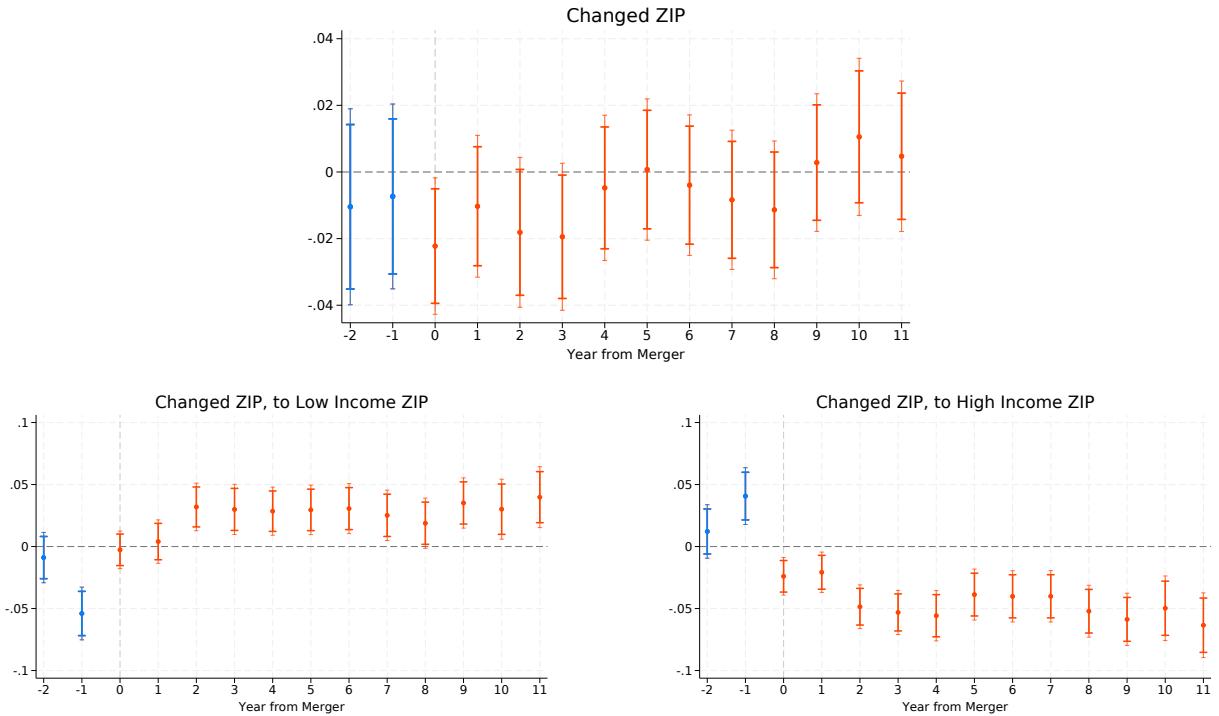
Figure 3.7: The Effect of Credit Shocks on Geographic Mobility

Panel A: Migration Patterns



Note: The graph shows the probability of living in a ZIP code different than in 2004 (empty bars), along with the probability of living in a ZIP code different than in 2004 in the bottom tercile of the income distribution (orange bars) and in the top tercile (blue bars). ZIP codes are classified into terciles each year using IRS income data.

Panel B: Effect of Credit Shock on Mobility



Note: Each plot reports the relationship between exposure to mergers and different measures of geographical mobility. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated individuals for each merger between 2005 and 2007, using the method developed by Callaway and Sant'Anna (2021). The dependent variable is reported above each plot.

3.5 Mechanism: Larger Effects for Short Histories

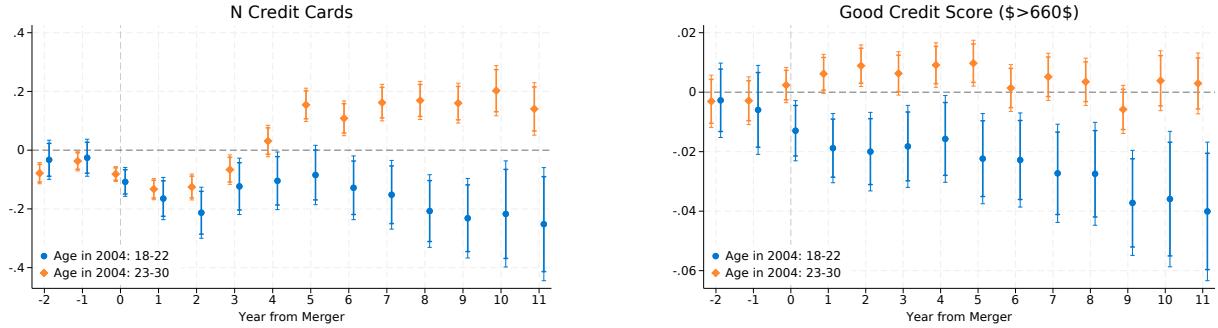
In this section, we explore the heterogeneity of the effect of credit supply shocks by age and length of credit history to validate the assumption behind our identification strategy and document an important mechanism driving our result: credit scores of individuals with shorter credit histories are more disrupted by early shocks to credit supply.

3.5.1 Older Versus Younger Cohorts

A potential concern for the validity of our research design involves the localized nature of our treatment. Specifically, a reduction in credit availability at the ZIP code level, even if uncorrelated with local economic conditions or demand factors, could still lead to a slowdown in the local economy. For instance, reduced access to credit for local businesses may dampen economic activity, which could, in turn, deteriorate the area's economic prospects. If so, our findings on the long-term consequences of credit shocks might reflect a decline in local economic opportunities, rather than the disruption of an individual's ability to build a history of timely payments.

To address this concern, we contrast the effect of the credit shock on our main sample of young individuals (ages 18-22 in 2004) with the effect of the same shock on their older (ages 23-30 in 2004) neighbors. The results are presented in figure 3.8, where we report estimates of δ_l^τ from the estimation of model (3.2) separately for each age group. As shown in the left panel, both groups suffer a reduction in the available credit as measured by the number of credit cards they hold in the short-run. However, in the medium- to long-term the effect on older individuals becomes positive, while younger individuals suffer a loss of 0.2 credit cards on average, as estimated before. More notably, as demonstrated in the right panel of figure 3.8, the negative consequences on credit scores apply only to young individuals. The effect of the shock of older cohorts is negligible and, if anything, positive in terms of point estimates. This finding provides evidence against the possibility of our shock generating a local economic downturn, as it would be unlikely to leave older age groups unaffected or improve their credit standing. Additionally, it provides suggestive evidence that negative consequences of early credit shocks are concentrated among younger individuals, whose credit histories are potentially shorter and less established, which would support a ‘credit trap’ interpretation that lack of credit access locks-in short credit history individuals into low creditworthiness. We address this point directly in the next section, comparing the effect of the credit supply shock on individuals with and without an ‘inherited’ credit history due to being a co-user on family accounts.

Figure 3.8: The Effect of Credit Shock by Age



Note: Each plot reports the relationship between exposure to mergers and different measures of credit and credit score. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated individuals for each merger between 2005 and 2007, using the method developed by Callaway and Sant'Anna (2021). The dependent variable is reported above each plot.

3.5.2 Impact of Having Pre-Existing Credit History

The second set of results of this section sheds light on the main mechanism behind our findings. Specifically, our main hypothesis is that the exogenous reduction in the amount of credit available to young customers has long term implications, because it prevents them from building a history of timely payments. A key implication of this hypothesis is that the effect of the credit supply shock should be larger for individuals with a shorter existing credit history, as for them a marginal additional credit line is more significant. We test this implication by comparing the effect of the credit supply shock on customers with and without an inherited credit history. We define individuals as having an inherited credit history if they have any recorded activity predating their 17th birthday, based on the number of months since the opening of their oldest credit line. These longer credit histories are most likely a result of being assigned as a co-user on family member's accounts while still being a minor.¹⁶

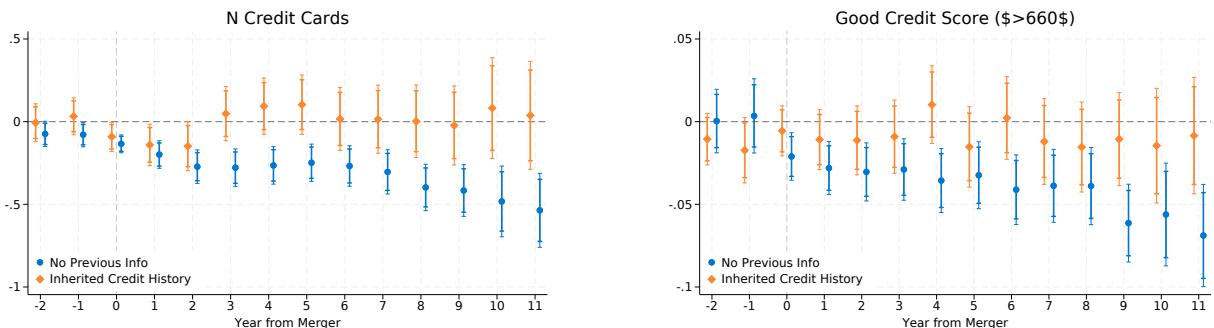
The results are presented in figure 3.9. The left panel shows that the effect of merger-induced branch closures on the number of credit cards is smaller and statistically not significantly different from zero for individuals with inherited credit histories, while being almost twice as large and more significant compared to the average estimates in figure ???. The right panel shows the effect on the probability of having a *Good Credit Score* (i.e., above 660). Similarly, the results indicate that the negative impact of the credit shock is concentrated among individuals without an inherited credit history, who are 6 percentage points less likely to have a good

¹⁶It is common for young individuals to open their first line of credit as an authorized user linked to the account of a customer with a longer credit history (potentially, a parent). As already documented in Bach et al. (2023), individuals that enter the market with an inherited credit history have a significantly higher credit score at entry and throughout their lifecycle.

credit score in the long run, and exhibit a statistically significant negative impact. In contrast, individuals with an inherited credit history appear unaffected by the shock.

These results underline the importance of an established credit history in mitigating the negative consequences of a credit supply shock, potentially suggesting that the opportunity to build a credit history is crucial for young individuals' credit score development.

Figure 3.9: The Effect of Credit Shock by Pre-existing Credit History



Note: Each plot reports the relationship between exposure to mergers and different measures of credit and credit score. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated individuals for each merger between 2005 and 2007, using the method developed by Callaway and Sant'Anna (2021). The dependent variable is reported above each plot.

3.6 Conclusions

This paper investigates the long-term effects of early credit supply shocks on sub-prime borrowers. Using exogenous variation in credit supply caused by merger-induced bank branch closures, we provide empirical evidence that reduced credit availability when individuals first enter the credit market has large and persistent negative effects on future credit scores and borrowing. Our findings are consistent with the existence of a poverty trap in the credit market, in which initial barriers to credit access hinder individuals' ability to build credit scores, thereby reinforcing long-term exclusion from borrowing.

These findings have important implications for policies aimed at promoting inclusion in consumer credit markets. If early credit scarcity initiates a self-reinforcing cycle of credit exclusion, then targeted interventions for young or disadvantaged individuals could deliver substantial long-term returns—potentially even paying for themselves. Mitigating such early constraints may enhance economic mobility and reduce inequality in borrowing opportunities over the life-cycle.

3.7 References

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Chapter 4

Appendix to the First Chapter

Table 4.1: Balance Test - Extended Version

School FEs			School Time Trend			Characteristic
b	SE	P-value	b	se	P-value	
0,006	0,001	0,000	0,001	0,001	0,321	Parental Earnings
-0,004	0,001	0,000	-0,001	0,001	0,323	Age of Enrollment
0,001	0,001	0,318	0,000	0,001	0,551	Fisrt Born
0,000	0,001	0,768	0,000	0,001	0,391	N of Siblings
0,003	0,001	0,024	0,000	0,001	0,784	Yrs. Educ. - F
0,001	0,001	0,467	-0,001	0,001	0,451	Plant Size - F
0,001	0,001	0,467	-0,001	0,001	0,451	Plant Size - F
0,001	0,001	0,467	-0,001	0,001	0,451	Plant Size - F
0,001	0,001	0,467	-0,001	0,001	0,451	Plant Size - F
-0,001	0,001	0,116	-0,001	0,001	0,344	Teenage Par. - F
-0,003	0,001	0,000	-0,001	0,000	0,026	Missing Edu. - F
-0,001	0,001	0,103	-0,001	0,001	0,209	Missing Parent- F
0,001	0,001	0,293	0,001	0,000	0,100	Plant size: 1 to 49 - F
0,001	0,001	0,376	0,000	0,000	0,863	Plant size: 50 to 200 - F
0,000	0,001	0,774	0,000	0,000	0,506	Plant size: 200+ - F
-0,003	0,001	0,000	-0,001	0,001	0,036	Missing plant ID - F
0,004	0,001	0,000	0,000	0,001	0,695	Yrs. Educ. - M
0,002	0,001	0,024	0,000	0,001	0,780	Firm Size - M
0,002	0,001	0,024	0,000	0,001	0,780	Firm Size - M
0,002	0,001	0,024	0,000	0,001	0,780	Firm Size - M
0,002	0,001	0,024	0,000	0,001	0,780	Firm Size - M
-0,001	0,001	0,109	-0,001	0,001	0,334	Teenage Par. - M
-0,003	0,001	0,000	-0,001	0,001	0,062	Missing Edu. - M
0,000	0,001	0,525	0,000	0,001	0,737	Missing Parent- M
0,002	0,001	0,000	0,001	0,000	0,004	Plant size: 1 to 49 - M
-0,001	0,001	0,302	-0,001	0,000	0,161	Plant size: 50 to 200 - M
0,000	0,001	0,521	0,000	0,001	0,995	Plant size: 200+ - M
-0,004	0,001	0,000	-0,001	0,001	0,066	Missing plant ID - M

Note: The table reports coefficients from separate regressions regressing (standardized) schoolmates parental earnings on several (standardized) measures of predetermined characteristics. All regressions include controls for own parental earnings, cohort fixed effects and school-level average realizations of parental earnings to control for mechanical negative correlation due to the leave-one-out nature of the measure considered, following a standard practice introduced by ? and applied to a similar context by ?. The first column reports the coefficient peer's parental earnings from a regression including school fixed effects, the fourth column reports the coefficient peer's parental earnings from a regression including school-specific linear time trends.

Table 4.2: Parental Earnings are Orthogonal to (residuals of) Peers' Parental Earnings

	(1)	(2)	(3)	(4)
	Par. Earnings	Par. Earnings	Par. Earnings	Par. Earnings
Schoolmates' Par. Earnings	0.044*** (0.012)	0.007 (0.011)	0.039*** (0.011)	0.006 (0.011)
Observations	350821	350821	345801	345801
School and time FE	Yes	Yes	Yes	Yes
Individual and school controls	No	No	Yes	Yes
School time trend	None	Linear	None	Linear
P-value of parental background	0	.527	0	.59

SEs in parentheses are clustered at the school level.

Note: The dependent variable in all columns is the percentile of parental earnings. All columns include the leave-one-out average of parental earnings at the school level, to account for negative mechanical bias due to using leave-one-out measure of peer characteristics, as suggested by Guryan, Kroft, and Notowidigdo (2009) correction method. Individual controls included in Columns (3)-(4) include fixed effects for gender, year of birth and mother and father age at birth. The p-value reported in the last line refers to the coefficient on peers' parental earnings. All variables are standardized. SE clustered at the school level are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.3: Higher Order Time Trends and Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
Parental earnings (Rank)	0.146*** (0.002)	0.146*** (0.002)	0.146*** (0.002)	0.161*** (0.003)	0.145*** (0.002)
SM Par. earnings (Rank)	0.068*** (0.021)	0.047** (0.021)	0.034 (0.023)	0.077** (0.030)	0.090*** (0.021)
SM Par. earnings (Rank, moving average)				-0.026 (0.041)	
Observations	345801	345801	345801	213168	345439
School FE	Yes	Yes	Yes	No	Yes
Cohort FE	Yes	Yes	Yes	No	Yes
School t trend (1st order)	Yes	Yes	Yes	No	Yes
School t trend (2nd order)	No	Yes	Yes	No	No
School t trend (3rd order)	No	No	Yes	No	No
School×Municipality	No	No	No	No	Yes
<i>R</i> ²	0.10	0.10	0.11	0.07	0.11

Note: Estimates from separate OLS regressions. dependent variable is children earnings by the age of 28-32. All earnings are expressed in ranks with respect tot the cohort-specific national distribution. All speifications include controls for year of birth, mother age at birth, father age at birth, gender and cohort size. Municipality is defined as the municipality of recidence in the year of enrollment. SEs in parentheses are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01

Table 4.4: Adjacent Cohorts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\tilde{X}_{i,t-3}$	-0.040** (0.018)						
$\tilde{X}_{i,t-2}$		-0.039* (0.022)					
$\tilde{X}_{i,t-1}$			0.018 (0.020)				
$\tilde{X}_{i,t}$				0.069*** (0.022)			
$\tilde{X}_{i,t+1}$					0.010 (0.021)		
$\tilde{X}_{i,t+2}$						0.031 (0.025)	
$\tilde{X}_{i,t+3}$							-0.029 (0.025)
Observations	254007	285381	317009	350821	312911	277114	242501

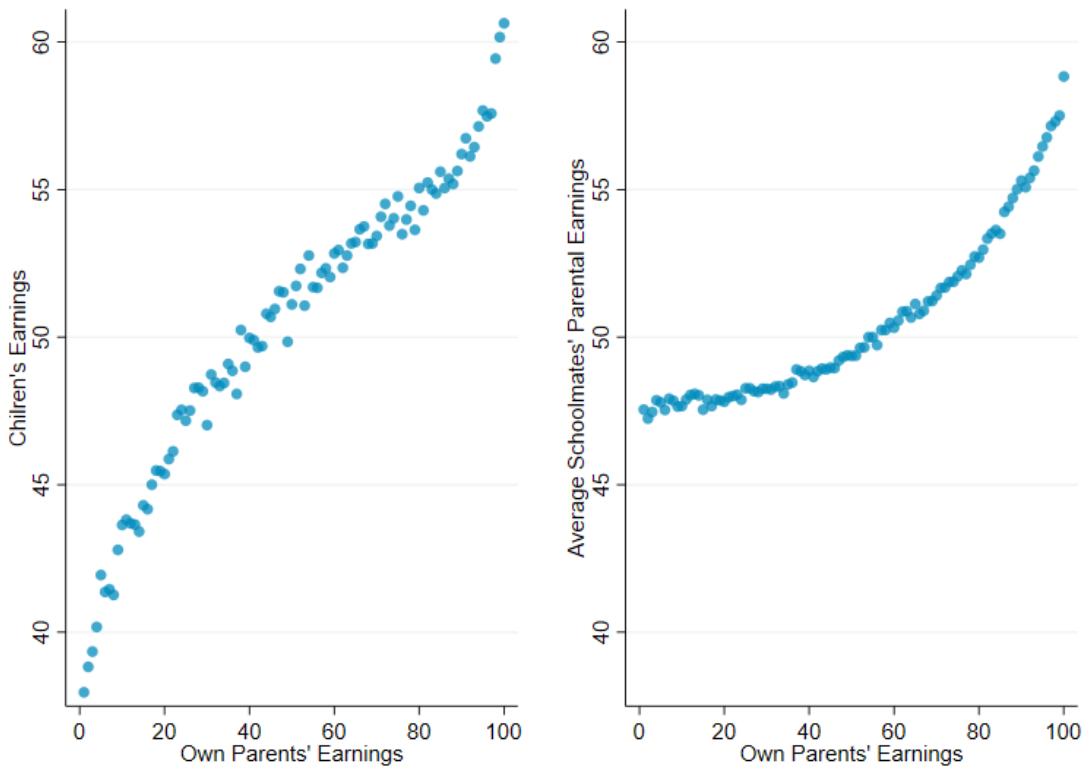
SEs in parentheses are clustered at the school-cohort level.

School FEs and school-specific time trends are included.

* p<0.10, ** p<0.05, *** p<0.01

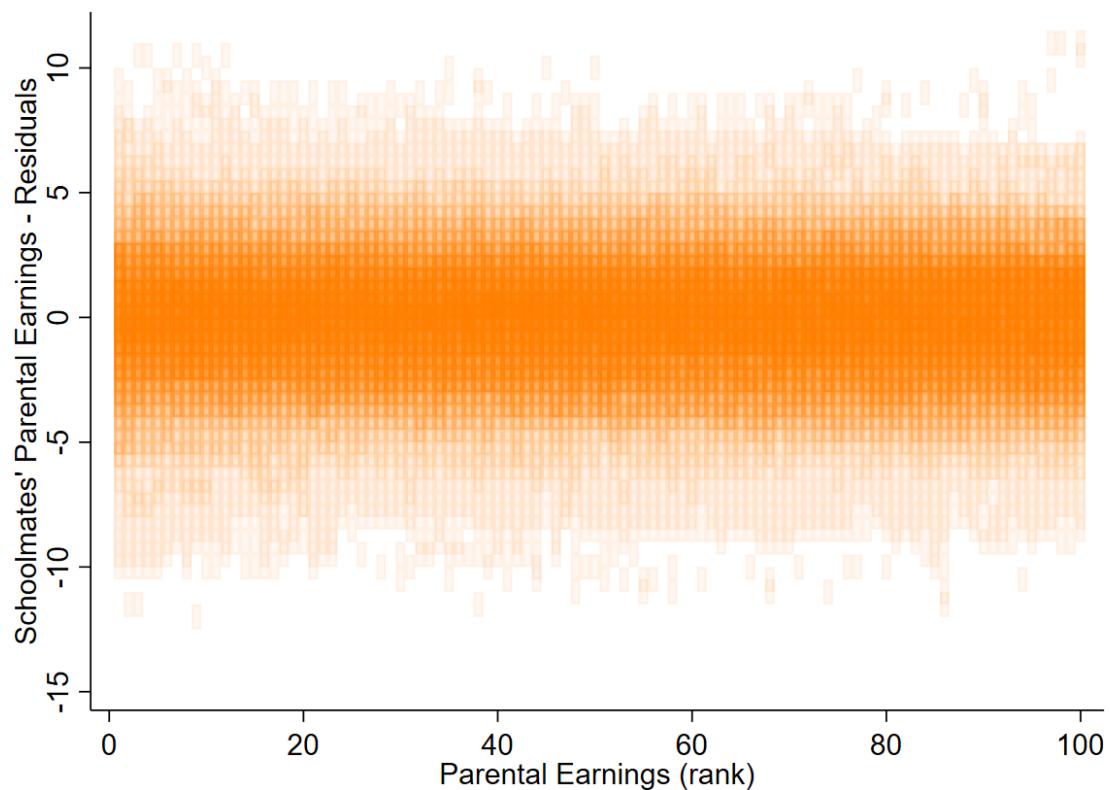
Note: Each column reports estimates from separate OLS regressions. Dependent variable is children earnings by the age of 28-32. All earnings are expressed in ranks with respect to the cohort-specific national distribution. $\tilde{X}_{i,t+c}$ is the average of parental earnings among students who enrolled in the same school as i , c years after the actual cohort of i . SEs in parentheses are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01

Figure 4.1: Intergenerational Mobility and Peer Exposure



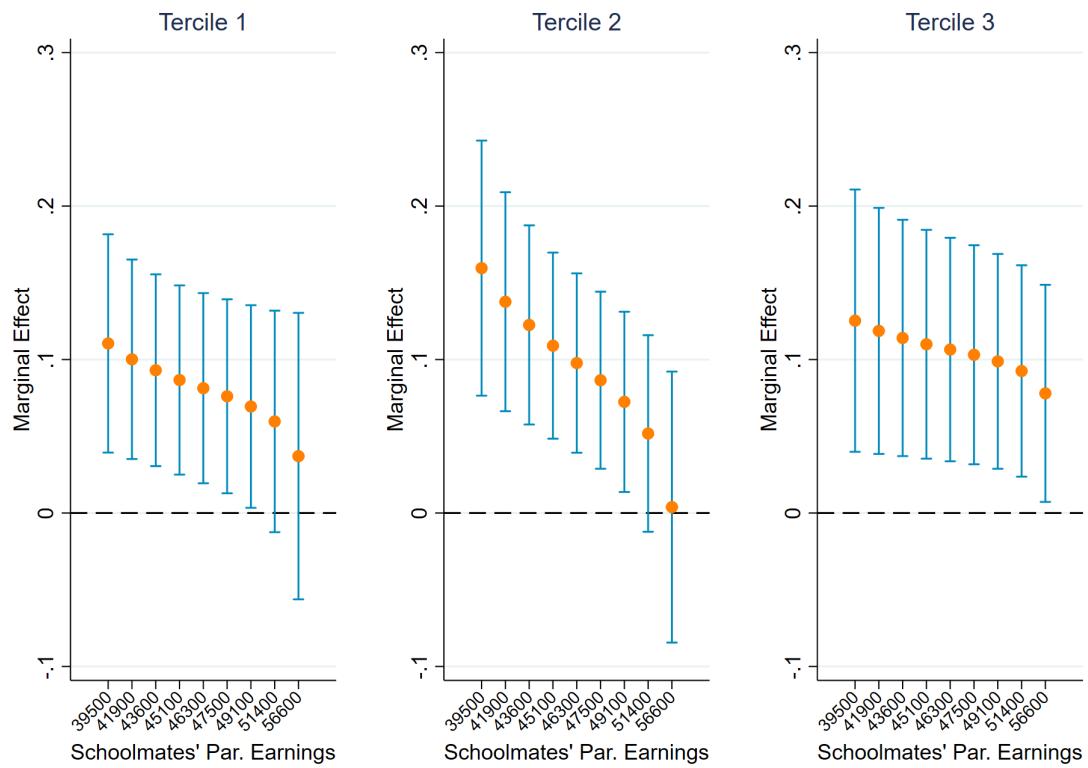
Note: The left graph plots the average earnings by the age 28-32 conditional on own parental earnings. The right graph plots the average peers' parental earnings conditional on own parental earnings. Peers' parental earnings are computed as the leave one out average of parental earnings among schoolmates. Earnings and parental earnings are measured in percentiles of the national earnings distribution.

Figure 4.2: Parental Earnings are Orthogonal to (residuals of) Peers' Parental Earnings



Note: The graph plots the empirical bivariate distribution of the residual of average schoolmates' parental background from a regression on school fixed effects (vertical axis) and school specific linear time trends and own parental background (horizontal axis). Average schoolmates' parental background is defines as the leave-one-out average of schoolmates parental earnings in percentiles of the national distribution.

Figure 4.3: Decreasing Marginal Effects by SES

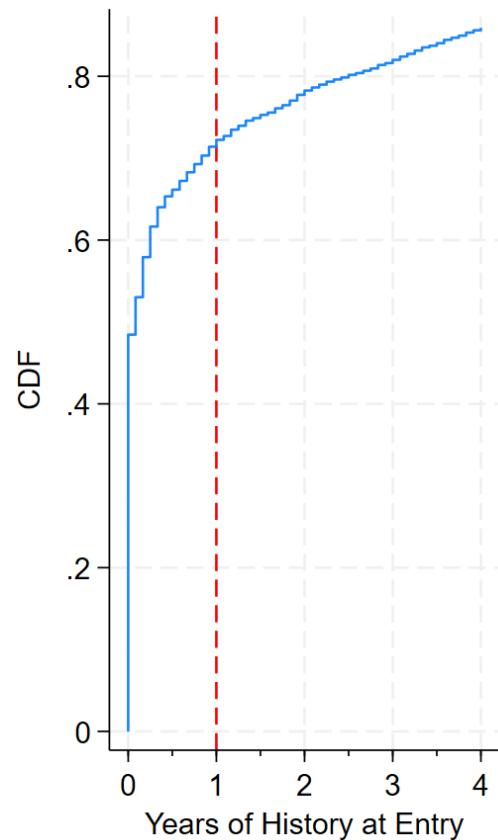


Note: The graph plots the marginal effects and the relative 90% confidence intervals from estimation of the model in eq. 1 where a quadratic polynomial for average schoolmates' parental earnings is included and a full set of interactions with dummies on tercile of parental earnings is included, evaluated at different levels of exposures to peers parental earnings and different tercile of parental earnings. The horizontal axis report the deciles of the distribution of schoolmates parental earnings (rounded to the closest hundreds), at which the marginal effect is computed.

Chapter 5

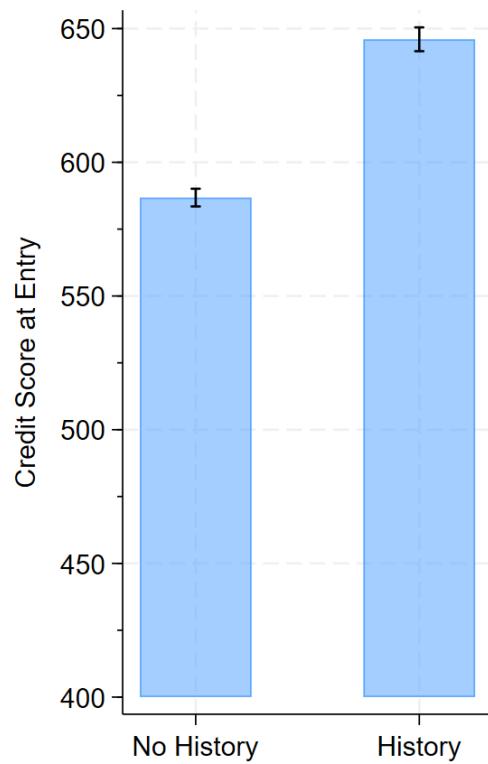
Appendix to the Second Chapter

Figure 5.1: Length of History at Entry



Note: The graph shows the empirical cumulative distribution function of the length of history at entry in the credit market. The length of history is defined as the number of years between the first credit record associated to each customer and the time of entry in the credit market.

Figure 5.2: Initial Credit Score: Heirs and Non-Heirs

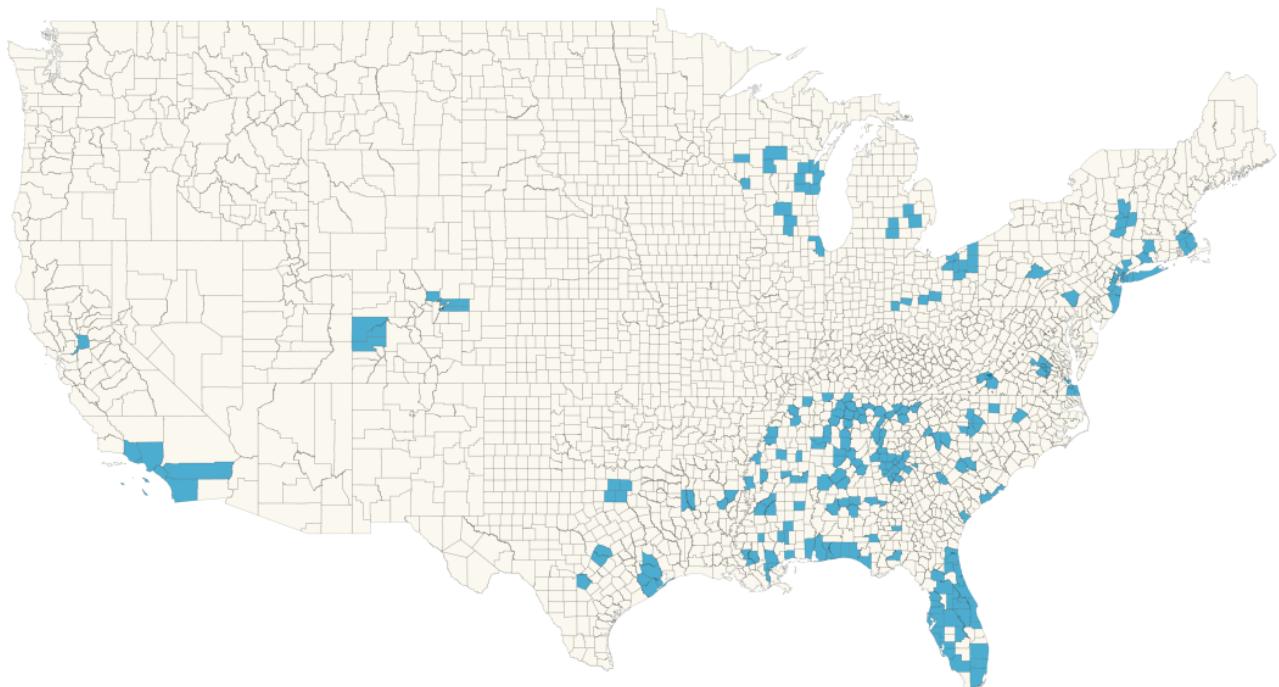


Note: The graph shows the average initial credit score at entry in the credit market for heirs and non-heirs. Heirs are customers who have a length of credit history predating the time of entry in the credit market by at least 6 months. 95% confidence intervals are reported.

Chapter 6

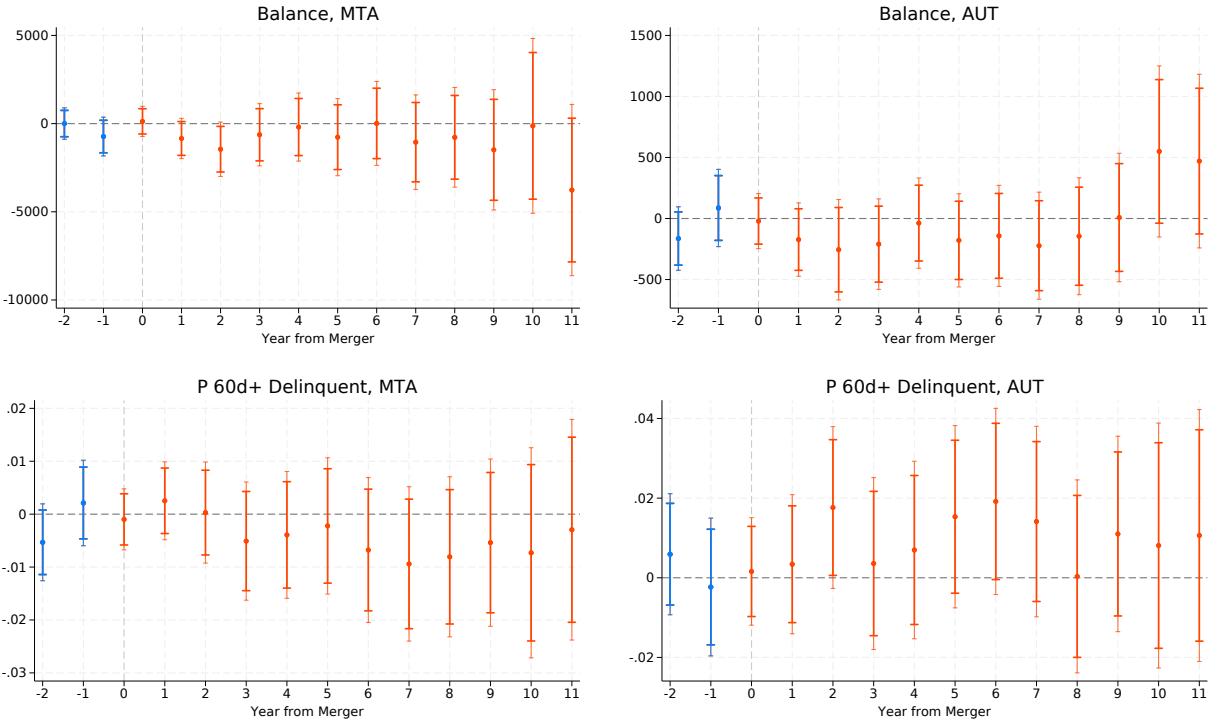
Appendix to the Third Chapter

Figure 6.1: Counties with Overlapping Branches



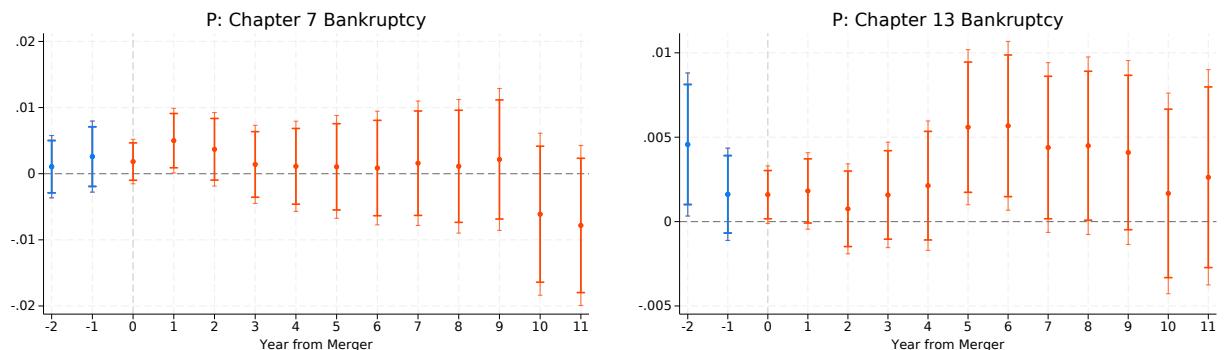
Note: The map shows the counties in the US where we observe overlapping branches from large banks involved in mergers between 2005 and 2007. The shaded areas represent counties with at least one zip code with overlapping branches from both the acquiring and target banks. Source: FDIC, authors' own calculations.

Figure 6.2: The Effect of Credit Shock on Mortgages and Auto Loans



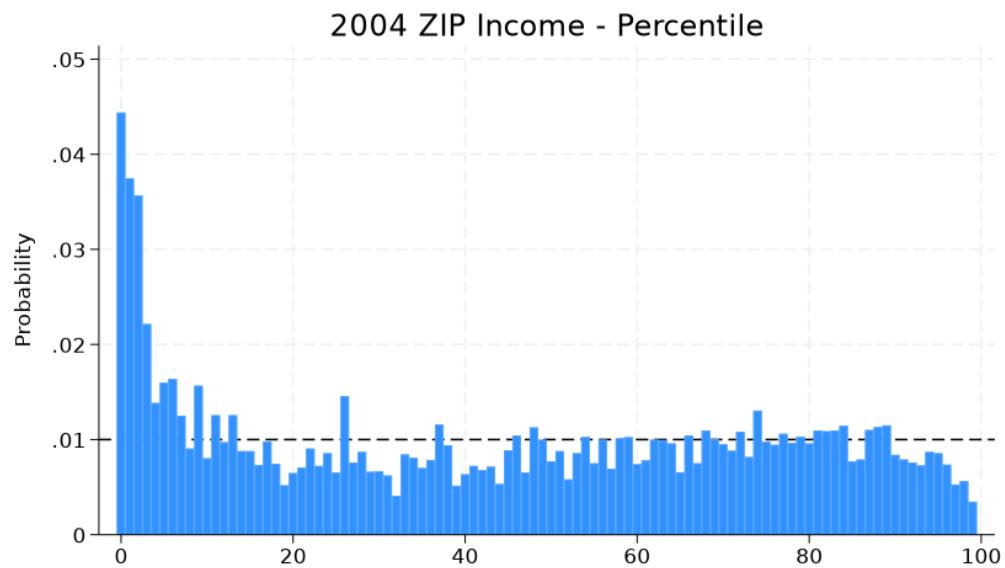
Note: Each plot reports the relationship between exposure to mergers and different measures of mortgages and auto loans. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated individuals for each merger between 2005 and 2007, using the method developed by ?. The dependent variable is reported above each plot.

Figure 6.3: The Effect of Credit Shock on Bankruptcy



Note: Each plot reports the relationship between exposure to mergers and different measures of bankruptcies. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated individuals for each merger between 2005 and 2007, using the method developed by ?. The dependent variable is reported above each plot.

Figure 6.4: Initial Distribution of Individuals by ZIP Code Income



Note: The Graph represents the probability distribution of individuals in the main sample living in a ZIP code in each percentile of the average ZIP code income distribution in 2004. Average ZIP code income is computed from IRS publicly available data.

Figure 6.5: Migration Patterns

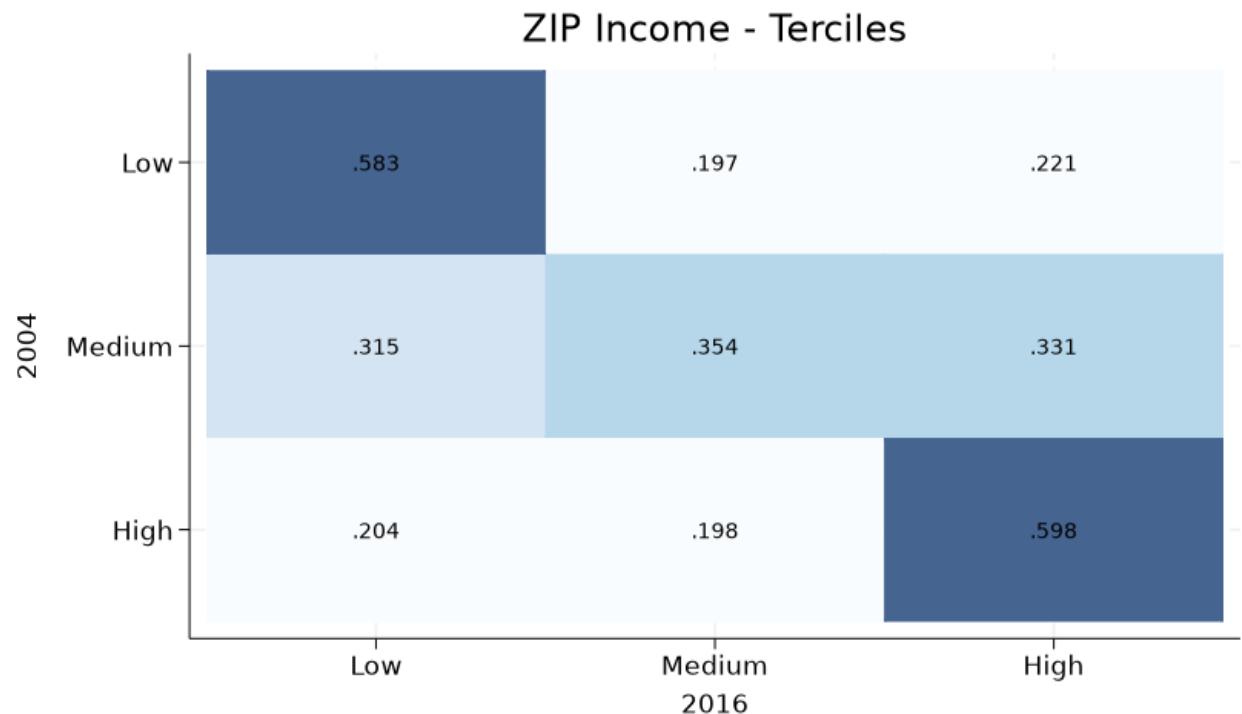
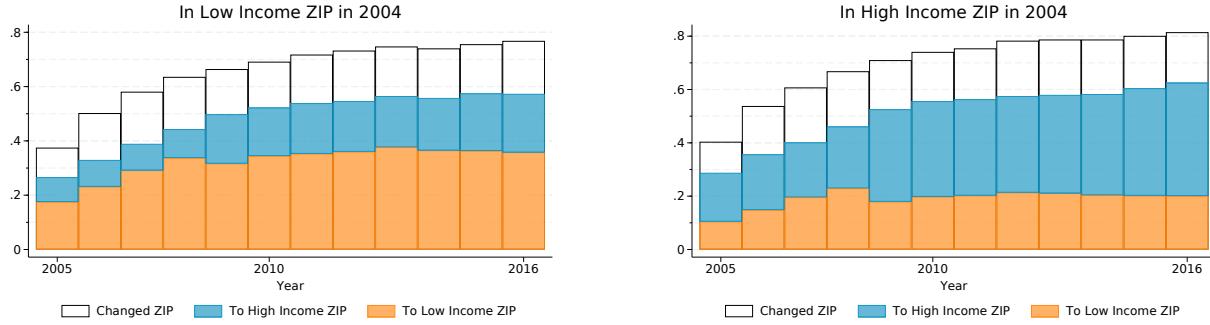


Figure 6.6: Transition of Individuals by ZIP Code Income

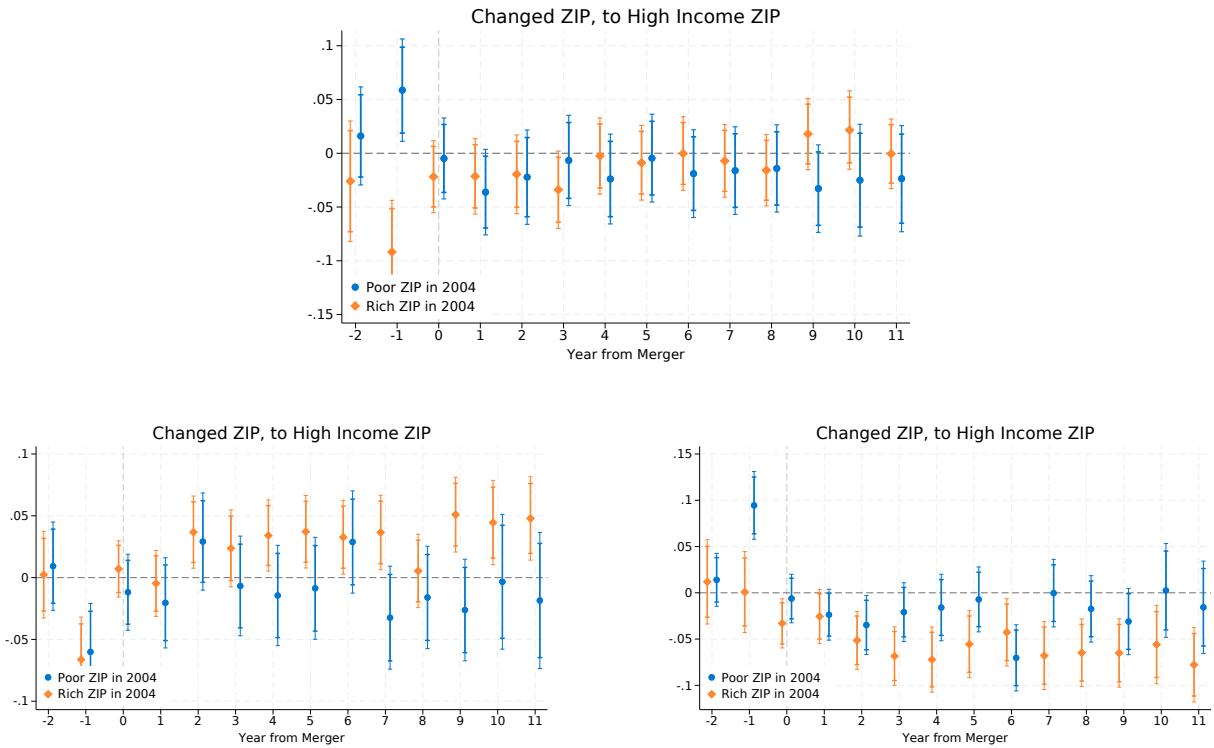
Note: The Graph represents the probability of individuals living in a ZIP code in each tercile of the income distribution in 2004 (rows) living in ZIP codes in each tercile of the income distribution in 2010 and 2016 (columns). Darker shaded areas represent higher transition probabilities. The numbers reported in each cell represent the row-normalized probability of individuals living in a ZIP code in each tercile of the income distribution in 2016 conditioning on living in a ZIP code in each tercile of the income distribution in 2004.

Figure 6.7: Migration Probability over the Life-Cycle



Note: Each graph shows the probability of living in a ZIP code different than in 2004 (empty bars) along with the probability of living in a ZIP code different than in 2004 in the bottom tercile of the income distribution (bottom orange bars) and in the top tercile of the income distribution (middle blue bars). For each year, we classify ZIP codes into terciles based on their average income rank in the national distribution of average income as computed from IRS publicly available data. The left graph refers to the population of individuals living in a ZIP code in the bottom tercile of the income distribution in 2004, while the right graph refers to individuals living in a ZIP code in the top tercile of the income distribution in 2004.

Figure 6.8: The Effect of Credit Shock on Geographic Mobility



Note: Each plot reports the relationship between exposure to mergers and different measures of geographical mobility by type of ZIP code of residence in 2004. The bars represent 90 and 95 percent confidence intervals. Estimates and standard errors are obtained by aggregating average treatment effects on the treated (ATTs), calculated by comparing treated and never-treated individuals for each merger between 2005 and 2007, using the method developed by ?. The dependent variable is reported above each plot.

Table 6.1: Summary Statistics of Credit Outcomes (2004–2016)

	2004	2010	2016
C: Mortgages			
Mortgage: Access	0.01 (0.11)	0.10 (0.30)	0.19 (0.40)
N of Open Mortgages	0.01 (0.14)	0.08 (0.31)	0.15 (0.39)
Balance, MTA	1,443.91 (15,515.79)	10,882.38 (46,546.15)	25,067.00 (76,880.83)
Original Loan, MTA	1,474.23 (15,749.82)	11,167.92 (47,645.04)	26,641.43 (80,885.98)
Original Loan, MTA - Conditional on Access	7,572.98 (35,046.76)	57,368.40 (94,931.93)	136854.21 (136117.41)
Amount 30d+ Delinquent, MTA	1.80 (58.89)	5.66 (117.82)	9.74 (300.80)
P 60d+ Delinquent, MTA	0.99 (0.12)	0.92 (0.26)	0.86 (0.35)
N Inquiries, MTA	0.17 (0.58)	0.40 (0.92)	0.50 (0.98)
D: Auto Loans			
Auto Loan: Access	0.21 (0.41)	0.50 (0.50)	0.61 (0.49)
N of Auto Loans (ever)	0.28 (0.66)	0.90 (1.24)	1.41 (1.76)
Balance, AUT	1,687.84 (4,947.46)	3,533.68 (7,619.13)	7,236.53 (12,747.98)
Original Loan, AUT	2,176.14 (6,514.35)	5,281.55 (10,531.97)	9,746.71 (16,079.44)
Original Loan, AUA - Conditional on Access	2,976.41 (6,988.23)	8,481.34 (12,352.58)	16,038.97 (18,015.13)
P 60d+ Delinquent, AUT	0.85 (0.35)	0.70 (0.46)	0.57 (0.50)
Observations	22890	22890	22890

Table 6.2: Summary Statistics by History at Entry (a)

	2004		2010		2016		
	<i>Inherited History</i>	<i>YES</i>	<i>NO</i>	<i>YES</i>	<i>NO</i>	<i>YES</i>	<i>NO</i>
A: Customer Information							
Age		20.36 (1.25)	20.99 (1.08)	26.36 (1.25)	26.99 (1.08)	32.36 (1.25)	32.99 (1.08)
Credit Score		526.95 (47.02)	525.21 (49.28)	546.90 (81.70)	561.52 (78.58)	576.73 (92.76)	591.85 (90.61)
Good Credit Score (> 660)		0.00 (0.00)	0.00 (0.00)	0.11 (0.32)	0.12 (0.33)	0.19 (0.40)	0.23 (0.42)
N Open Credit Lines		0.53 (0.90)	2.72 (2.93)	1.83 (2.82)	2.80 (3.40)	3.52 (4.59)	5.04 (5.46)
Balance		1,541.32 (8,631.44)	8,756.20 (24,487.52)	15,217.81 (45,007.18)	24,959.30 (59,079.18)	36,218.89 (78,987.23)	53,361.54 (98,627.12)
N Credit Lines 30d+ Delinquent		0.04 (0.20)	0.17 (0.49)	0.04 (0.25)	0.06 (0.31)	0.06 (0.32)	0.08 (0.38)
N Inquiries		2.58 (2.23)	4.28 (2.88)	2.64 (2.28)	3.09 (2.52)	2.72 (2.51)	3.14 (2.68)
B: Credit Cards							
P Credit Card Holder		0.23 (0.42)	0.52 (0.50)	0.29 (0.45)	0.41 (0.49)	0.48 (0.50)	0.63 (0.48)
N Credit Cards		0.34 (0.72)	1.75 (2.42)	0.92 (1.89)	1.48 (2.39)	1.86 (3.03)	2.92 (3.87)
Credit Limit, CC		179.85 (690.04)	1,685.81 (4,337.14)	1,842.59 (5,611.36)	2,688.14 (6,867.78)	5,826.57 (13,083.47)	9,233.09 (17,610.33)
Utilization %, CC		92.87 (46.90)	81.64 (52.03)	53.40 (44.50)	59.67 (41.19)	52.15 (38.21)	53.77 (37.57)
P Credit Card 90d+ Delinquent		0.12 (0.33)	0.52 (0.50)	0.16 (0.37)	0.22 (0.42)	0.13 (0.34)	0.17 (0.38)
N Inquiries, CC		1.02 (1.26)	1.71 (1.68)	0.98 (1.28)	1.13 (1.37)	1.12 (1.51)	1.37 (1.65)
Observations		12600	10290	12600	10290	12600	10290

Table 6.3: Summary Statistics by History at Entry (b)

Inherited History	2004		2010		2016	
	YES	NO	YES	NO	YES	NO
C: Mortgages						
Mortgage: Access	0.00 (0.06)	0.02 (0.15)	0.07 (0.26)	0.13 (0.34)	0.16 (0.37)	0.24 (0.43)
N of Open Mortgages	0.00 (0.07)	0.03 (0.19)	0.06 (0.27)	0.10 (0.34)	0.13 (0.37)	0.17 (0.42)
Balance, MTA	416.48 (7,809.41)	2,701.99 (21,400.87)	8,346.11 (40,429.43)	13,988.02 (52,921.43)	20,791.42 (68,529.70)	30,302.40 (85,722.88)
Original Loan, MTA	419.97 (7,859.73)	2,765.16 (21,752.16)	8,529.86 (41,082.73)	14,398.19 (54,445.34)	22,054.89 (72,078.58)	32,257.60 (90,197.33)
Original Loan, MTA - Conditional on Access	2,648.47 (19,591.67)	11,575.89 (43,351.70)	53,791.93 (90,621.51)	60,275.56 (98,218.91)	139084.90 (128420.54)	135040.98 (142067.12)
Amount 30d+ Delinquent, MTA	0.56 (22.75)	3.32 (84.13)	4.41 (112.50)	7.19 (124.00)	9.90 (377.60)	9.54 (163.39)
P 60d+ Delinquent, MTA	1.00 (0.07)	0.97 (0.16)	0.94 (0.24)	0.91 (0.29)	0.88 (0.33)	0.83 (0.37)
N Inquiries, MTA	0.11 (0.42)	0.24 (0.72)	0.32 (0.80)	0.49 (1.03)	0.45 (0.93)	0.56 (1.04)
D: Auto Loans						
Auto Loan: Access	0.09 (0.28)	0.36 (0.48)	0.41 (0.49)	0.61 (0.49)	0.55 (0.50)	0.68 (0.46)
N of Auto Loans (ever)	0.09 (0.31)	0.51 (0.87)	0.65 (0.99)	1.21 (1.43)	1.18 (1.58)	1.70 (1.91)
Balance, AUT	705.31 (3,033.22)	2,890.95 (6,368.41)	2,830.93 (6,765.29)	4,394.20 (8,470.59)	6,079.57 (11,573.23)	8,653.21 (13,923.59)
Original Loan, AUT	827.37 (3,491.68)	3,827.69 (8,632.53)	4,257.32 (9,405.05)	6,535.71 (11,644.19)	8,144.48 (14,591.89)	11,708.62 (17,534.21)
Original Loan, AUA - Conditional on Access	1,184.05 (4,167.93)	4,726.49 (8,565.25)	7,601.92 (11,550.41)	9,340.02 (13,032.17)	14,933.12 (17,001.07)	17,118.74 (18,891.84)
P 60d+ Delinquent, AUT	0.93 (0.25)	0.76 (0.43)	0.75 (0.43)	0.64 (0.48)	0.61 (0.49)	0.51 (0.50)
Observations	12600	10290	12600	10290	12600	10290

Table 6.4: Summary Statistics for Older Cohorts (23-30 in 2004) (a)

	2004	2010	2016
A: Customer Information			
Age	26.35 (2.59)	32.35 (2.59)	38.35 (2.59)
Credit Score	624.98 (99.97)	635.14 (110.17)	656.43 (108.99)
Good Credit Score (> 660)	0.39 (0.49)	0.43 (0.49)	0.50 (0.50)
N Open Credit Lines	4.95 (4.75)	4.75 (4.45)	5.56 (5.12)
Balance	37,085.25 (79,435.75)	88,717.33 (157236.56)	115507.30 (197591.56)
N Credit Lines 30d+ Delinquent	0.06 (0.29)	0.06 (0.31)	0.05 (0.31)
N Inquiries	3.15 (2.65)	2.43 (2.27)	2.28 (2.29)
B: Credit Cards			
P Credit Card Holder	0.68 (0.47)	0.65 (0.48)	0.73 (0.44)
N Credit Cards	3.50 (3.90)	3.11 (3.52)	3.62 (3.98)
Credit Limit, CC	9,749.79 (15,688.04)	14,076.90 (21,429.41)	21,530.15 (29,955.81)
Utilization %, CC	38.70 (40.10)	36.46 (37.18)	36.87 (34.16)
P Credit Card 90d+ Delinquent	0.20 (0.40)	0.17 (0.37)	0.11 (0.31)
N Inquiries, CC	1.12 (1.38)	0.80 (1.17)	0.95 (1.36)
Observations	144975	144975	144975

Table 6.5: Summary Statistics for Older Cohorts (23-30 in 2004) (b)

	2004	2010	2016
C: Mortgages			
Mortgage: Access	0.16 (0.37)	0.42 (0.49)	0.51 (0.50)
N of Open Mortgages	0.21 (0.53)	0.44 (0.75)	0.46 (0.72)
Balance, MTA	24,883.71 (73,460.14)	71,853.84 (148522.23)	91,967.31 (186993.30)
Original Loan, MTA	26,306.92 (212571.09)	76,386.14 (157036.12)	102173.11 (205408.59)
Original Loan, MTA - Conditional on Access	51,085.60 (294079.29)	148334.77 (192913.90)	198410.67 (250679.07)
Amount 30d+ Delinquent, MTA	5.13 (108.35)	22.30 (410.21)	15.67 (475.49)
P 60d+ Delinquent, MTA	0.82 (0.38)	0.66 (0.47)	0.63 (0.48)
N Inquiries, MTA	0.59 (1.16)	0.63 (1.12)	0.54 (1.00)
D: Auto Loans			
Auto Loan: Access	0.52 (0.50)	0.68 (0.47)	0.70 (0.46)
N of Auto Loans (ever)	1.01 (1.36)	1.68 (1.85)	1.91 (2.12)
Balance, AUT	4,946.77 (9,241.31)	5,577.10 (10,558.57)	8,418.31 (14,922.79)
Original Loan, AUT	7,011.24 (12,117.46)	9,007.14 (15,650.01)	12,313.54 (19,796.90)
Original Loan, AUA - Conditional on Access	9,037.52 (13,270.89)	12,685.69 (17,328.88)	17,518.70 (21,596.31)
P 60d+ Delinquent, AUT	0.58 (0.49)	0.46 (0.50)	0.41 (0.49)
Observations	144975	144975	144975