Three Essays in Governmental Policies and Inequality

by

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ABSTRACT

In my dissertation, I aim to contribute to the knowledge on how governmental policies shape economic inequality. In two of my chapters, I examine how changes in the housing market influence long-term differences in wealth and educational choices. Chapter I explores the relationship between declining racial residential segregation and wealth inequality, which are connected through changes in house prices and homeownership patterns. Chapter III examines how parental homeownership and changes in house prices affect college major choice, which creates sizable differences in lifetime earnings inequality. Finally, Chapter II studies how earlier return to work from parental leave affects one of the most important sources of the gender wage gap: the employment and earning trajectories of mothers.

In Chapter I, I study how the Fair Housing Act (FHAct), which ended lawful housing discrimination in the US, affected residential segregation and racial wealth inequality. I first present three stylized facts, which show that during the 1970s, (i) racial residential segregation started to decline, (ii) the relocation of Black families from Black neighborhoods intensified, and (iii) average housing wealth increased rapidly among Black families. To investigate whether the FHAct was driving these changes, I estimate a heterogeneous agent life-cycle OLG model with spatial equilibrium and endogenous house prices. With this model, I simulate how the FHAct affected families' relocation across neighborhoods, neighborhood-level house prices, and the Black and White wealth distribution. I find that the FHAct could explain (i) the decreasing residential segregation, (ii) the observed relocation patterns, and (iii) partly the increasing housing wealth of Black families. My results suggest that the FHAct substantially decreased racial wealth inequality at the top, while, somewhat unexpectedly, increased racial wealth inequality between the 2nd and 7th deciles of the wealth distribution.

In Chapter II, with Anikó Bíró, Lili Márk, and Tímea Laura Molnár, we identify the substitution effect in paid parental leave ('PL'), by exploiting a Hungarian reform that affected only the opportunity cost of staying on PL, while keeping the overall benefit amounts unchanged. Hungarian mothers with a child under two years old after 2014 were

provided with strong incentives to return to the labor market earlier than previous cohorts, since they did not have to give up PL benefits while earning their wage income during their children's second year of age. Using an Event Study research design with women without births in the control group and Hungarian linked employer-employee data, we find that mothers in eligible cohorts were by 3 percentage points (30 percent) more likely to work 19–24 months after giving birth – but, no further differences exist in their working propensities or its timing. We find modest effects, on average, on mothers' wages for some groups (for those who gave birth at an older age and live in the capital) and a clear positive effect on their firms' average wage premium 3-5 years after giving birth – these results are due to eligible mothers being less likely to switch 'down' to lower-paying firms than ineligible ones. We also find that eligible mothers sort to occupations that require less analytical thinking, stress tolerance, leadership and willingness to take on responsibilities, and involve less time pressure.

In Chapter III, with Tyler Radler and Nikhil Rao, we examine how parental housing wealth affects children's college major choices by combining surveys of United States undergraduates from the Higher Education Research Institute and spatial variation in housing demand growth during the 2000s housing boom. Using the structural break in three-digit zip-code-level house prices between 2000 and 2006 as an instrument for house price changes, we find that a one standard deviation increase in house prices increases enrollment in high-paying majors by 0.7 p.p, which change is driven by students whose parents are less likely to be homeowners. In contrast, in the case of a similar house price increase, students whose parents are the most likely to own homes decrease their enrollment in high-paying majors by 1.0 p.p. and increase their participation in majors with more volatile future earnings by 1.0 p.p. Overall, we find that house price increases motivated students from low-homeownership areas to switch to higher-paying and less-volatile majors, while students with a high probability of parental homeownership were more likely to enroll in lower-paying and/or more volatile majors.

CHAPTER 1

Did Decreasing Residential Segregation Reduce Racial Wealth Inequality?

1.1 Introduction

Despite numerous ongoing federal programs aiming to decrease racial wealth inequality, today the Black-White wealth gap is nearly as large as it was during the second half of the 1970s. Based on the 2022 wave of the Survey of Consumer Finances, the median non-Hispanic White household has more than six times as much net wealth as the median Black, a ratio that has not decreased since the middle of the 1980s. Racial wealth differences decreased rapidly during the Civil Rights Movement and the passage of the Fair Housing Act (FHAct) in 1968 was expected to aid this process by shrinking racial differences in wealth accumulation. The Fair Housing Act promoted racial equity in access to housing wealth by banning racial discrimination in housing sales, rental, or financing, and through these, ended lawful housing segregation in the United States. The halt of the racial wealth convergence during the 1980s is puzzling, as many scholars, such as Shapiro (2017) and Rothstein (2018), argue that racial segregation was a key element of the build-up of racial wealth differences.

In this paper, I study whether terminating housing discrimination decreased residential segregation and reduced racial wealth inequality following the Fair Housing Act. I model the connection between housing discrimination, segregation, and racial wealth inequality within a structural model, which allows me to rule out all economic changes other than the FHAct. My simulation results reveal that decreasing housing discrimination led to lower levels of racial residential segregation and racial wealth inequality among already wealthy families. However, I find that reducing housing discrimination could increase racial wealth inequality in the middle of the wealth distribution.

I first present three stylized facts that show sizable changes in neighborhood sorting and housing wealth accumulation right after the passage of the Fair Housing Act in 1968. First, the persistent increase of residential segregation between Black and non-Black families

stopped during the 1970s, and until 2000 racial segregation declined as much as it increased during the Great Migration between 1910-1970 (Cutler, Glaeser, and Vigdor, 1999; Boustan, 2011). Second, Black neighborhoods experienced a significant and unprecedented outflow of Black families during the 1970s, which continued in later decades at a slower pace as well (Ellen, 2000). Finally, I show that following the FHAct, the homeownership rate and the average net housing wealth increased at a faster pace among Black than White families.

Then, I study whether the Fair Housing Act was the driving force behind these facts. The decreasing housing discrimination of Black families may have led to the outflow of Black families from Black neighborhoods and decreased segregation. The possibility of buying a home in more integrated neighborhoods could have motivated more Black families to become homeowners and accumulate housing wealth. In this way, the Fair Housing Act could have led to decreasing racial segregation and declining racial differences in housing and total wealth in the long run. However, drawing such a causal relationship is challenging because of the ever-changing economic environment. Without further assumptions, it is impossible to distinguish the effect of the FHAct from other economic developments during the 1970s, like the quickly growing real incomes of Black families.

I quantify the effect of the Fair Housing Act on residential segregation and racial wealth inequality within a quantitative model, which allows me to draw this causal relationship and rule out all economic changes of the 1970s other than the FHAct. I estimate a heterogeneous agent life-cycle overlapping-generation model with spatial equilibrium and endogenous house prices. This quantitative model, among the first in the literature, allows me to jointly model the homeownership decision and location choices. In the model, neighborhood-level house prices are set in spatial equilibrium and households' relocation among neighborhoods creates changes in house prices and, through these, in housing wealth and wealth distribution. I estimate this model using the simulated method of moments to match the spatial and wealth distributions observed in the 1970 decennial census and the Survey of Consumer Finances Plus, assuming that some Black households were not allowed to move into White neighborhoods before the FHAct. As a validation exercise, I show that the estimated model generates realistic life-cycle homeownership and wealth-holding patterns for both Black and White families.

I simulate the effect of the Fair Housing Act by lifting relocation barriers that prevented Black households from residing in White neighborhoods. As Black families relocate among neighborhoods in the model, house prices increase in neighborhoods that gain new residents and decline in those with declining populations, which could motivate White families to move as well. After the relocation of households, a new steady state spatial equilibrium emerges with new house prices and new Black and White wealth distributions. I quantify the effect

of the Fair Housing Act by comparing how the steady state spatial distribution and wealth holding of households changed due to the relocation of Black families in the model.

According to my simulation results, the Fair Housing Act decreased the most common measure of segregation, the dissimilarity index between Blacks and non-Blacks, by 6.8 percentage points, which is slightly higher than the observed decline in segregation during the 1970s, which was 6.1 percentage points. I find that the FHAct reduced racial differences in average housing wealth, and it affected racial wealth inequality differently depending on which part of the initial wealth distribution one considers. In the model, the end of lawful housing discrimination moderately increased wealth inequality among Black families. The wealth holding of the top 25% of the Black wealth distribution increased by 1.5-4%, while the wealth in the 6th Black wealth decile decreased by 1%. As the policy's effect on the White wealth distribution was slightly positive between the 2nd and 8th deciles, racial wealth inequality decreased between the 8th and 10th and increased between the 2nd and 7th deciles of the wealth distribution. My results suggest that the Fair Housing Act did not decrease racial wealth inequality in all parts of the wealth distribution; moreover, I find that racial wealth inequality increased in the majority of the wealth distribution because of the Fair Housing Act.

Among the first in the literature, I model households' homeownership and location choices jointly within a dynamic model, which yields new insights into how homeownership decision affects neighborhood choices over the life-cycle. To the best of my knowledge, besides this paper, only Richard (2024) studies how joint homeownership and location choice affect wealth inequality within a structural model. In this way, I contribute to the growing literature that studies how homeownership or spatial sorting affects wealth inequality.

First, I contribute to the literature on spatial sorting, in which both static (Almagro and Domínguez-Iino, 2024; Weivu, 2024; Davis, Gregory, and Hartley, 2023) and dynamic (Fogli and Guerrieri, 2019; Aliprantis and Carroll, 2018; Chyn and Daruich, 2021) models abstract from modeling homeownership. Because of this modeling simplification, these models cannot capture how families move between neighborhoods to accumulate housing wealth. I contribute to this literature by modeling homeownership decisions, race-dependent mortgage markets, and house market frictions directly and studying how ownership motivation affects Black and White families' location choices differently over their life-cycle. My paper also quantifies how spatial relocation of households affects racial wealth inequality through housing wealth, which so far has not been modeled directly in this literature.

Second, I also contribute to the literature in macroeconomics that studies the determinants of homeownership decisions over the life cycle (Yang, 2009; Brandsaas, 2021; Khorunzhina and Miller, 2022). By adding neighborhood choice into the usual dynamic frame-

work in this literature, I quantify how the possibility of spatial relocation and changing neighborhood-level house prices affect the optimal time of becoming a homeowner over the life-cycle.

Finally, I contribute to the growing literature studying the determinants of Black–White wealth differences. Studies so far have investigated how racial differences in parental transfers (Charles and Hurst, 2002; Aliprantis, Carroll, and Young, 2022), housing and total wealth accumulation (Boehm and Schlottmann, 2004; Derenoncourt et al., 2024), or family composition (Ashman and Neumuller, 2020) affect racial wealth inequality. However, the literature still lacks quantitative evidence on how the spatial sorting of families, and especially desegregation, affects wealth differences between Black and White families through the housing market. My paper contributes to this literature by showing that decreasing racial residential segregation has a sizable and heterogeneous effect on racial wealth inequality. My results suggest that desegregation could lead to increasing racial wealth inequality in the middle of the wealth distribution.

The paper proceeds as follows. In Section 1.2, I present three stylized facts about neighborhood sorting and changing Black housing wealth accumulation trends during the 1970s. In Sections 1.3 and 1.4, I present and estimate a quantitative model that could endogenously create neighborhood sorting, changes in neighborhood-level house prices, and changes in the Black and White wealth distribution. In Section 1.5, I use this model to test whether the Fair Housing Act was the driving force behind all three stylized facts and how the FHAct affected racial wealth inequality through the Black and White wealth distribution. Finally, I conclude my results in Section 1.6.

1.2 Stylized facts on segregation and racial wealth inequality during the 1970s

Data

In this research, I work with two main datasets: (1) I use census tract-level aggregates from decennial censuses to track neighborhood-level changes over time, and (2) I use the Survey of Consumer Finances Plus (SCF+) to measure changes in household asset composition and wealth inequality. My spatial analysis is based on the Longitudinal Tract Data Base (LTDB) developed by Logan, Xu, and Stults (2014). The unit of observations in LTDB is census tracts, and the dataset contains census tract-level aggregates of population and

various socioeconomic variables, starting with with 1970 decennial census. I used the 1970-2000 waves of the study, and I interpolated each census tract in different waves into 2010 census tract boundaries using LTDB's interpolation files. This interpolation allows me to track changes in time-invariant spatial units over time. I extended LTDB with aggregated census tract information from the 1960 census using the National Historical Geographic Information System (NHGIS); I interpolated the 1960 census tract boundaries into the 2010 tract definition with a crosswalk file I created using QGIS.

Until 1990, census tract-level maps and tabulations were only created in core-based statistical areas (CBSA); thus, my sample only contains census tracts from metropolitan areas between 1960 and 2000. To focus on urban areas where Black/non-Black residential segregation was more prevalent, I follow Reardon and Bischoff (2011) by narrowing down the sample for census tracts in the 100 largest cities in 1970 with at least 10,000 Black individuals. The sample contains more than 36,000 census tracts from 87 metropolitan areas. Based on Baum-Snow (2007)'s neighborhood categorization, 23.4% of these tracts resided within the central city of the metropolitan area. As I am interested in how the share of the Black population changed in these neighborhoods, following Ellen (2000), I categorized these census tracts into three categories based on their share of the Black population at a given census year. I define a census tract as "White" if its Black share was less than 10%, as "Mixed" if its Black share was between 10-50%, and as "Black" if it was a majority Black neighborhood at a given census.[1]

I use SCF+ to measure changes in homeownership, housing wealth, and racial wealth inequality. The SCF+ was developed first by Kuhn, Schularick, and Steins (2020), and it is the most commonly used dataset to analyze racial wealth inequality in the second part of the 20th century (Kuhn, Schularick, and Steins, 2020, Derenoncourt et al., 2023; Derenoncourt et al., 2024). The SCF+ combines early waves of the Survey of Consumer Finances run by the Survey Research Center at the University of Michigan with the post-1983 waves of the study, which were run by the Federal Reserve System. The study was run each year between 1949-71, then every three years from 1989, while between 1971-89, it was run in 1977 and 1983. The study was redesigned in 1983, but Kuhn, Schularick, and Steins (2020) harmonized variables and reweighed earlier waves to reflect the 1983 study design. SCF+ is the only dataset that contains both total family wealth and housing asset information starting with

[1]Note that, based on this categorization, majority Asian or Hispanic neighborhoods are also categorized as "White". The 1960 and 1970 censuses did not collect information on Hispanic origin, and the published white population of census tracts contains non-Hispanic and Hispanic whites as well. In these two censuses, the share of "White" neighborhoods with less than 50% of white population is just 0.1%. However, in line with the rising Hispanic population and the distinction between Hispanic and non-Hispanic whites in the 1980-2000 censuses, the share of "White" neighborhoods with less than 50% of the non-Hispanic white population in the 1980-2000 censuses was 5.2%, 9.4%, and 16.2%, respectively.

the early 1960s. However, as SCF+ lacks panel dimension and detailed location information, I cannot analyze how potential relocation affected Black families' wealth accumulation.

To control for the different survey sizes and irregularities in the frequency of SCF survey collection, for this analysis I combine survey waves into 5-year bins. I follow Derenoncourt et al. (2024) by smoothing the time series of wealth components. While they take the unweighted moving average (MA3) of the time series, I smooth by bootstrapping the sample of a wealth component for a given bin from a pool that contains the sample of neighboring bins as well; this smoothing method also allows me to calculate the confidence interval of wealth measures. Every wealth component is measured in 1970 prices, which I deflated using the Census Bureau's C-CPI-U index.

Stylized facts

Stylized fact I: The 1970s was a turning point in the history of segregation in the US

The Fair Housing Act in 1968 and the Equal Credit Opportunity Act in 1974 eased the legal barriers Black families faced moving into majority White neighborhoods, which dramatically changed the landscape of segregation in the United States (Boustan, 2011). Table 1.1 shows that the most often used measure of segregation, the dissimilarity index between Black and non-Black individuals, peaked at the 1970 decennial census and then declined rapidly in the following decades.[2] In just three decades, the average of the CBSA-level dissimilarity indexes dropped by 18.6 percentage points. Based on Cutler, Glaeser, and Vigdor (1999)'s calculation, the magnitude of this decline is larger than the dissimilarity index's increase during the Great Migration (17.7 percentage points increase between 1910-70) when nearly six million Black people moved from rural areas of Southern states to Northern urban areas. As a result, by 2000, the dissimilarity index dropped just under the threshold of 0.6, above which cities are considered very segregated (Boustan, 2011).

The extensive literature on racial segregation suggests that the decline of racial segregation was the result of the widespread relocation of Black families from majority Black neighborhoods into more integrated ones (Ellen, 2000; Fischer et al., 2004). As the spatial distribution of Black agents in Table 1.1 illustrates, the fraction of Black individuals living in Black neighborhoods increased between 1960-70, but then it started to decline continuously.

[2]The dissimilarity index measures how segregated a minority group's spatial distribution is from a majority population within a larger spatial unit (like a metropolitan area). Its value can vary between 0 and 1, and larger values imply a higher level of segregation. Intuitively, the dissimilarity index shows what fraction of the minority group should be relocated to other neighborhoods to reach a random distribution of the minority group. See Massey and Denton (1988) or Reardon and Bischoff (2011) for more details.

Table 1.1: Segregation measures and distribution of families by Black share of neighborhoods

	1960	1970	1980	1990	2000
Residential segregation (Dissimilarity index)					
Black vs. Non-Black	0.780%	0.785%	0.724%	0.657%	0.599%
Share of Black population in White neighborhoods					
Blacks / All population	1.07%	1.16%	1.89%	2.46%	3.03%
Spatial distribution of Black population					
White neigh.	7.5%	7.2%	10.2%	12.6%	13.4%
Mixed neigh.	25.7%	22.1%	23.3%	28.1%	33.3%
Black neigh.	66.8%	70.7%	66.6%	59.3%	53.3%
Spatial distribution of Non-Black population					
White neigh.	87.1%	87.7%	85.3%	81.6%	76.9%
Mixed neigh.	10.7%	10.1%	12.3%	16.0%	20.6%
Black neigh.	2.2%	2.2%	2.4%	2.4%	2.5%
Income segregation (Rank order H), from Reardon et al. (2011)					
Among Black families		0.099	0.133	0.173	0.170
Among White families		0.110	0.117	0.132	0.139
All families		0.124	0.134	0.152	0.157

Sources and notes: 1. Dissimilarity index: own calculation based on LTDB and NHGIS data. The overall dissimilarity index is calculated as the population-weighted mean of the dissimilarity index of the 87 CBSAs in the sample. 2. Spatial distribution of Black population: own calculation based on LTDB and NHGIS data. Averages are weighted with tract population. Neighborhoods are defined based on Black share at each census year. 3. Income segregation information is taken from Reardon and Bischoff (2011).

Within 30 years, the fraction of Black individuals living in White neighborhoods almost doubled, as it increased from 7.2% to 13.4%. Meanwhile, another measure of Black presence in White neighborhoods, the average share of Black individuals in White neighborhoods nearly tripled, from 1.16% to 3.03%. By 2000, 30 years after the Fair Housing Act, almost as many Blacks lived in White and Mixed neighborhoods as in the majority Black ones. Meanwhile, the fraction of non-Black individuals living in White neighborhoods started to decline during the 1970s, primarily because many White tracts became Mixed due to the inflow of Black families.

In line with the decreasing racial residential segregation, the income residential segregation started to increase during the 1970s. As Table 1.1 shows, income segregation increased within both Black and White families, but the rise of income segregation was much faster among Blacks (Reardon and Bischoff, 2011). The rapidly increasing income segregation among Black families suggests that most of the relocating families were from the middle and top of the Black income distribution, many of whom were potentially not allowed to move into majority White neighborhoods before the Fair Housing Act (Hirsch, 1983).

Table 1.2: Change of average Black and non-Black population in Black neighborhoods

	1960	1970	1980	1990	2000
Panel A: Based on 1970 neighborhood categorization					
# of Black	2,870	3,774	3,180	2,731	2,429
# of non-Black	1,992	795	550	658	808
Total	4,862	4,568	3,730	3,389	3,237
Change of Black population		31.5%	-15.7%	-14.1%	-11.1%
Panel B: Based on 1960 neighborhood categorization					
# of Black	4,039	3,868	3,013	2,553	2,274
# of non-Black	1,038	579	547	696	870
Total	5,077	4,446	3,560	3,248	3,145
Change of Black population		-4.2%	-22.1%	-15.3%	-10.9%
Panel C: Based on changing neighborhood categorization					
# of Black	4,039	3,774	3,185	2,859	2,749
# of non-Black	1,038	795	680	674	706
Total	5,077	4,568	3,865	3,533	3,455
Change of Black population		-6.6%	-15.6%	-10.2%	-3.8%

Sources and notes: 1. Spatial distribution of Black population: own calculation based on LTDB and NHGIS data. The sample contains census tracts in the 100 largest CBSAs, which had at least 10,000 Black residents in 1970.

Stylized fact II: Black families started to leave Black neighborhoods during the 1970s

The relocation of Black families from majority Black neighborhoods had a sizable effect on these neighborhoods' population, and it likely sped up the population decline of Black neighborhoods. Table 1.2 summarizes how the average number of Black and non-Black residents changed over time in Black neighborhoods.[3] In Table 1.2, I use three different neighborhood categorizations: I categorized neighborhoods by their Black share in 1970 in Panel A, based on their Black share in 1960 in Panel B, and I defined neighborhoods based on their Black share of each census year in Panel C. Compared to Panel A and B, Panel C contains neighborhoods which became majority Black after 1960 or 1970, and thus the set of neighborhoods in Panel C changes between census years. Panel A shows how neighborhood composition changed in tracts that were majority Black right after the Fair Housing Act, while Panels B and C provide a robustness check by showing that the population change in Panel A is not the consequence of the choice of neighborhood definition.

Panel A of Table 1.2 shows that, on average, neighborhoods that had more than 50% Black share in 1970 experienced a large inflow of Black individuals between 1960-70. This trend reversed during the 1970s when the average number of Blacks in these tracts declined by

[3]In Table 1.2, I present the number of non-Blacks rather than non-Hispanic Whites, as computing the latter is not possible for the 1960 and 1970 censuses because of the lack of information on Hispanic origin.

15.7%, after which the outmigration of Black families continued from these neighborhoods. By lowering the legal obstacles Black families faced during relocation, the Fair Housing Act could have supported this sudden change in Black neighborhoods during the 1970s.

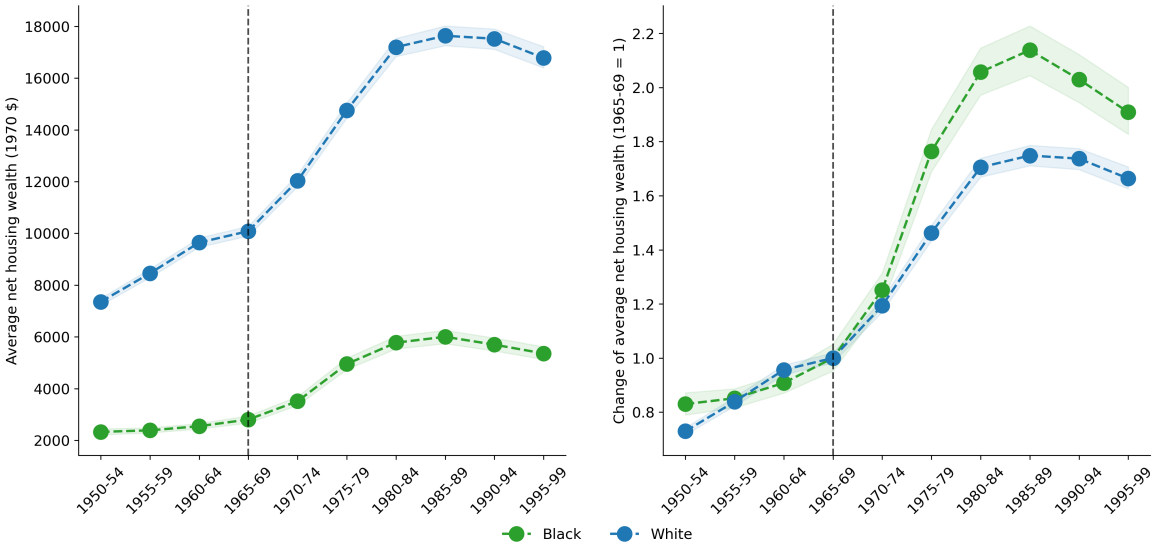
The large number of non-Black individuals who left Black neighborhoods during the 1960s suggests that many of these neighborhoods experienced a quick change in their racial composition, which, in theory, could motivate Black families to leave these tracts. In general, it could be possible that Black neighborhoods experienced a large outflow of the Black population right after they became majority Blacks, which could also explain population changes in Panel A. However, as Panel B of Table 1.2 shows, counter to this explanation, neighborhoods that were already majority Black in 1960 experienced only a modest decline in Black population during the 1960s, but they went through similar depopulation during the 1970s as neighborhoods in Panel A. Panel C of Table 1.2, in which the sample of tracts contains all neighborhoods which were majority Black at the given census year, draws a similar picture by showing that the average number of Black population in Black neighborhoods declined the most in the 1970s. Panel B and C suggest that the large decrease in number of Black population in Black neighborhoods between 1970-80 was a result of a unique change in relocation behavior during the 1970s.

All three neighborhood definitions show that Black neighborhoods experienced a double-digit decline in the average Black population during the 1970s, while the decline continued but slowed down in the next decades. During the same time span, the Black population of the United States increased by 15-20% in each decade between 1960-2000, which makes the depopulation of Black neighborhoods even more dramatic. Interestingly, despite the declining Black population in these tracts, the decline of the average non-Black population reversed between 1980-1990, and Black neighborhoods experienced increasing non-Black presence during the 1990s.

Stylized fact III: Black families' housing wealth increased at a faster pace than Whites'

Differences in Black and White housing wealth have always been at the center of discussions about racial wealth inequality (eg. Shapiro, 2017; Rothstein, 2018). As housing asset dominates the asset portfolio of families in the middle of the wealth distribution (Aladangady and Forde, 2021), racial differences in housing wealth accumulation could create sizable differences in racial wealth inequality over time (Charles and Hurst, 2002). Meanwhile, net housing wealth is the wealth component that could have been the most likely affected directly by the Fair Housing Act. As the right panel of Figure A.1 in the Appendix shows, the total net wealth of Black and White families increased at the same pace during the second part of the 20th century, while total non-house related wealth increased faster among Whites than

Figure 1.1: Evolution of average net housing wealth



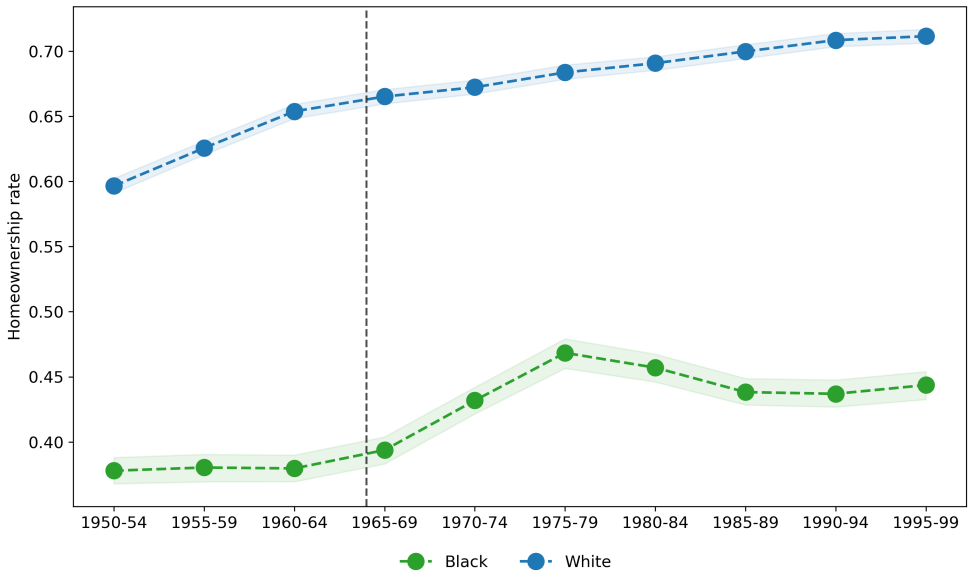
Source: own calculation, based on SCF+ data. Housing wealth is defined as asset value minus the remaining mortgage of the first residence. Transparent areas represent bootstrapped 95% confidence intervals.

Blacks (Figure A.2). However, as the right panel of Figure 1.1 depicts, average net housing wealth increased at a much faster pace among Black families than among Whites right after the Fair Housing Act.

The left panel of Figure 1.1 illustrates that average White housing wealth rised rapidly between 1950-1980 when the nominal difference in average housing wealth between Black and White families increased. Compared to Whites, Black families' housing wealth accumulation was much slower until the 1970s, when it started to accelerate. The right panel of Figure 1.1, which shows how the average housing wealth changed compared to its 1965-69 value, indicates that during the 1970s, the average housing wealth of Black families increased at a faster pace than Whites', which was not the case in previous decades. As the decrease of nominal differences between Black and White average housing wealth requires faster housing wealth accumulation of Black families, Black families' larger housing wealth growth than Whites' in the 1970s could have been a first step to the long-run decline in racial differences in housing wealth. Thus, despite the continuously growing nominal differences between Black and White average housing wealth, the 1970s seemed to be a turning point in Black families housing wealth accumulation.

As the average net housing wealth is the product of the homeownership rate and the average net housing wealth of homeowners, we can decompose whether the fast rise of average Black net housing wealth was created by increasing homeownership (extensive margin),

Figure 1.2: Black and White homeownership rate



Source: own calculation, based on SCF+ data. Transparent areas represent bootstrapped 95% confidence intervals.

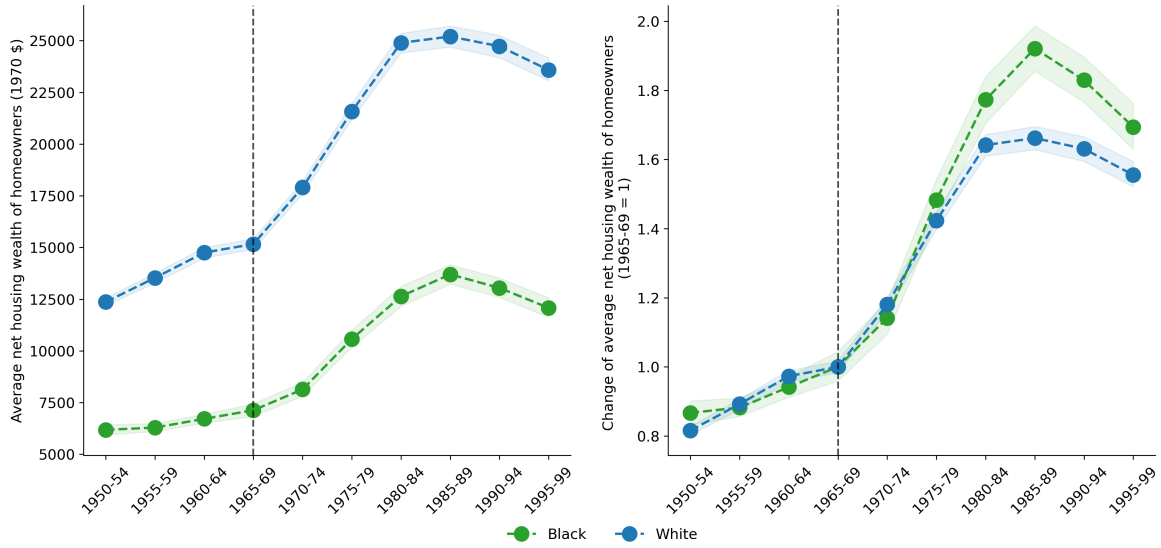
increasing net housing wealth of homeowners (intensive margin), or both. Figure 1.2 shows that Black homeownership rose rapidly during the 1970s but then declined somewhat, which could partly explain the slowdown of average Black housing wealth increase in the 1980s. Nevertheless, the expansion of the Black homeownership rate during the 1970s was the largest among any decades in the second part of the 20th century. White families' homeownership rate increased steadily at the same pace between 1960-2000, without any noticeable change during the 1970s.

Meanwhile, as Figure 1.3 shows, during the 1970s, Black homeowners were able to accumulate housing wealth at a faster pace than in earlier decades. But, during the 1970s, the growth rate of average housing wealth of Black homeowners was nearly the same as Whites (right panel of Figure 1.3), and the nominal difference between the average housing wealth of Black and White homeowners continuously increased (left panel of Figure 1.3).

Thus, during the 1970s, the increasing nominal Black housing wealth in the left panel of Figure 1.1 was the byproduct of the quickly growing Black homeownership rate and the increased growth of the housing wealth of Black homeowners. However, as Black homeowners acquired housing wealth at the same pace as White homeowners, the fact that Black families accumulated housing wealth at a faster pace than Whites during the 1970s (right panel of Figure 1.1) caused by the decreasing racial differences in homeownership rate in the 1970s.

In summary, the 1970s were a favorable environment for housing wealth accumulation

Figure 1.3: Evolution of average net housing wealth of homeowners



Source: own calculation, based on SCF+ data. Housing wealth is defined as asset value minus the remaining mortgage of the first residence. Prices measured in 1970 dollars. Transparent areas represent bootstrapped 95% confidence intervals.

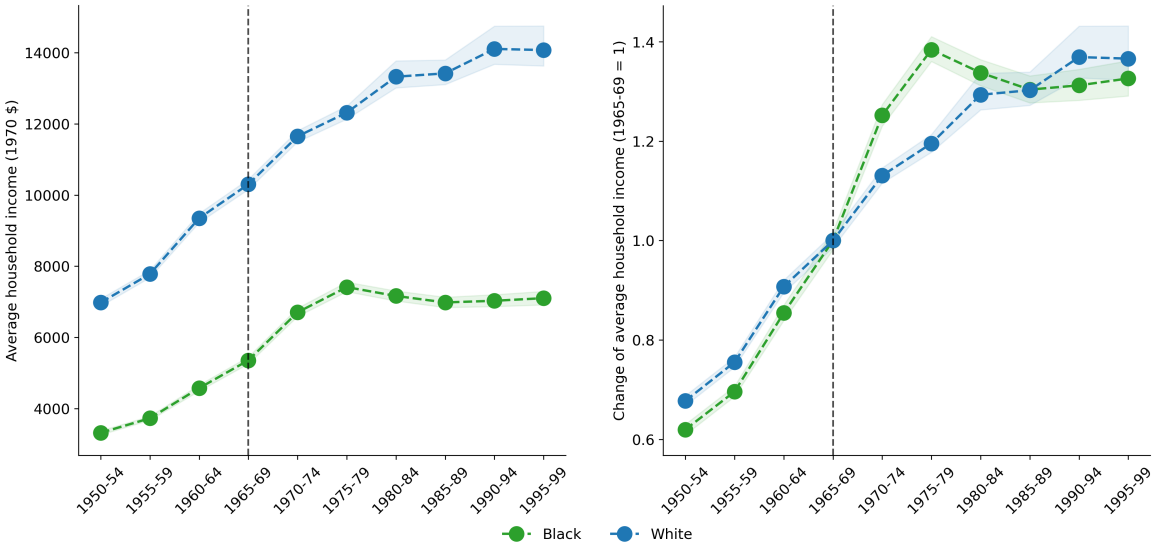
for Black families. Because of the existing racial differences, during this time, the nominal differences between average Black and White housing wealth increased. However, Black families accumulated housing wealth at a faster pace than Whites during the 1970s, which could have been the necessary first step to close the existing racial differences in housing wealth.

Can we draw a causal line between the FHAct and these stylized facts?

The 1970s were a turning point in both the history of racial segregation and racial wealth inequality in the United States. The decades-long increase of racial segregation reversed, and since 1970, Black/non-Black racial segregation has been in constant decline. During the same time, Black families started to move out at an accelerated pace from majority Black neighborhoods, and Black families experienced a quickly increasing homeownership rate and faster housing wealth accumulation than Whites.

In this research I am interested in whether the Fair Housing Act could explain all of these three developments within one causal framework. By easing housing discrimination against Black families, the Fair Housing Act could have caused the sizable relocation of these families to more integrated neighborhoods (Stylized Fact II), which could explain the declining residential segregation (Stylized Fact I). Meanwhile, the new opportunities to accumulate housing wealth in more White neighborhoods could have motivated Black families to move

Figure 1.4: Evolution of average household income



Source: own calculation, based on SCF+ data. Prices measured in 1970 dollars. Transparent areas represent bootstrapped 95% confidence intervals.

there, become homeowners, and start to accumulate housing wealth (Stylized Fact III).

However, drawing a causal relationship between the FHAct, these three stylized facts, and the evolution of racial wealth inequality would be questionable because of the changing economic environment over these decades. The Fair Housing Act was less likely to be able to explain the decline of both Black and White average housing wealth during the 1990s in Figure 1.1. Among other factors, the decreasing real total family income (Figure 1.4) could explain this drop, while the Fair Housing Act likely had a modest effect on family incomes. Similarly, the quickly increasing average Black total household income during the 1970s likely contributed to the increasing homeownership and housing wealth of Black families, and without further assumptions, we cannot distinguish the positive effect of increasing average Black household income on racial wealth inequality from the potential effect of decreasing segregation following the Fair Housing Act. Because of these difficulties, I investigate the potential effect of the Fair Housing Act on these three facts and on racial wealth inequality with a structural model, which enables me to quantify the effect of relocation of Black families following the FHAct on racial wealth inequality by keeping all other relevant economic conditions unchanged.

1.3 Model

In this section, I introduce a quantitative structural model, that I use to quantify how the relocation of Black families affected the spatial sorting of Black and White households and how this relocation altered the Black-White wealth differences at different parts of the wealth distribution.

The model builds on a life-cycle heterogeneous agent OLG framework (similarly to De Nardi, 2004; Ashman and Neumuller, 2020; Brandsaas, 2021; or Chyn and Daruich, 2021), and generates wealth inequality through differences in lifetime income and hand-to-mouth behavior. Agents in the model can endogenously relocate across three neighborhoods, which I estimate to represent White, Mixed, and Black neighborhoods. Unlike most models in the literature with spatial equilibrium (like Aliprantis and Carroll, 2018; Fogli and Guerrieri, 2019; or Chyn and Daruich, 2021), I model homeownership decisions directly, which allows me to model homeownership and location choice jointly and investigate whether the Fair Housing Act affected renters and homeowners differently. The house prices and rents in neighborhoods are endogenous and set in equilibrium, which generates house price change in neighborhoods in case of a changing population. Similarly to Aliprantis and Carroll (2018), I assume that some Black families are initially not allowed to move into the White neighborhood of the model.

1.3.1 Environment

Demographics The model is populated with overlapping generations of agents, who can be either Black or White ($R = \{B, W\}$, where R denotes race). Each period in the model can be interpreted as one year of life. Agents enter the model at age 24 in the neighborhood in which their grandparents resided. New agents inherit their parents' race, their time-invariant and race-dependent income fixed effect ρ_R, and bequest from their grandparents. Agents have one child, who enters the model when agents are 48 years old. Agents retire at age 60, from which point they receive a time-invariant pension income, but otherwise, their decision problem does not change. Agents leave the model after the period of age 72 when they could leave a bequest to their grandchildren, who inherit it at the next period when they enter the model.

Preferences In each period, agents gain utility from consuming consumption goods (c) and housing services ($v(h)$) and from neighborhood amenities by living in the

neighborhood n

$$u(c,h,n)_R = \frac{(c^{1-\alpha}v(h_n)^{\alpha})^{1-\sigma}}{1-\sigma} + \kappa_{R,n}A_n,$$

where α is the expenditure shares of housing consumption, σ measures risk aversion, and A_n is a race-independent amenity level that represents the utility derived from living in neighborhood n. Parameter $\kappa_{R,n}$ is a race-dependent taste factor for living in neighborhood n, which allows Black and White agents to gain different utility by living in the same neighborhood. As a normalization, I set all three of White agents' $\kappa_{W,n}$ to 1. Just like in Brandsaas (2021), function $v(.)$ maps consumption of housing good in the neighborhood n to utility

$$v(h_n) = \begin{cases} \eta_{n,rent} & \text{if renting in neighborhood n} \\ \chi\eta_{n,own} & \text{if owning in neighborhood n } \ (\chi > 1) \end{cases}$$

where χ represents the additional utility derived from owning a house (like potential higher-quality or other benefits that are not explicitly modeled, such as tax benefits). In the model homeownership h is binary ($h = 1$ if the agent is a homeowner, and $h = 0$ if the agent is a renter), but the consumption value of housing good $v(h_n)$ is non-binary, and it differs among neighborhoods. At age 72, agents also derive utility from leaving non-negative bequests to their grandchildren, which following De Nardi (2004), takes the form of

$$beq(b',h',n') = \theta_{1,R}\left(1 + \frac{(1+i)b' + p'_{n'}h'(1-\delta)}{\theta_2}\right)^{1-\sigma} \quad \text{where} \quad b' \geq 0$$

where the race-dependent $\theta_{1,R}$ is capturing the parents' concern about leaving the bequest to their children, and the size of θ_2 controls in which the extent of the bequest is a luxury good. The nominator represents the wealth inherited by offspring: b is bond holding, i is the risk-free interest rate of bonds, p_n is the neighborhood-specific house price, h is an indicator for owning a house, and δ is the per-period depreciation of housing good.

Bequest connects generations in the model and allows agents to hold more wealth than would be possible just by their own income. In the model $\theta_{1,R}$, which controls how much utility agents gain by leaving bequests, is race-dependent for two reasons. First, the SCF+ shows significant racial differences in wealth at age 24 when agents enter the model: the median wealth holding of Black and White households at that age are \$0 and \$1,224, respectively. Second, previous research (Charles and Hurst, 2002; Ashman and Neumuller, 2020) highlighted that racial differences in parental involvement in down payments are an important source of the racial differences in the homeownership rate. As there are no in-vivos transfers in the model, racially differing bequest captures the existing racial differences in parental help in becoming a homeowner.

Neighborhoods There are three neighborhoods n in the model: Black, Mixed, and White. Neighborhoods differ in endogenous house prices p_n and rents q_n, in exogenous amenity levels A_n, and in consumption utility derived from owning $\eta_{n,own}$ or renting $\eta_{n,rent}$ in the neighborhood. Agents moving from neighborhood n to n' need to pay a relocation cost:

$$RC(n, n') = \begin{cases} k & \text{if } n \neq n' \\ 0 & \text{if } n = n' \end{cases}$$

I model the effect of the Fair Housing Act in the model through an exogenous relocation constraint F, which affects only Black agents. Agents with this constraint ($F = 1$) cannot move into the White neighborhood, while Black agents without this constraint ($F = 0$) – just as White agents – can choose from any neighborhoods to live in. I assume that π_F fraction of Black households living in the Black or Mixed neighborhood have this relocation constraint.

Housing market Agents can either rent or own a house. Neighborhood-level house prices are set in equilibrium by a supply function $S_n = \bar{S_n} p_n^{\Delta}$, where p_n is the equilibrium house price in neighborhood n, Δ is the price elasticity of housing supply, and $\bar{S_n}$ is a supply shifter. Housing assets depreciate between periods with a δ rate, and homeowners need to pay selling and buying cost $SBC(n, n')$ if their housing stock changes between periods:

$$SBC(h, h') = \begin{cases} e_s p_n & \text{if } h' < h \\ e_b p_n & \text{if } h' > h \\ 0 & \text{if } h' = h \ \& \ n' = n \end{cases}$$

where e_s and e_b are selling and buying costs, modeled as a fraction of the price. Agents with housing wealth do not need to pay selling or buying costs if they keep their house, but they need to pay both selling and buying costs in case they move into a different neighborhood and buy a house again. Households without housing assets need to pay rent q_n, which is a function of house price p_n.

Income process Active agents between the ages of 24-60 provide one unit of labor supply inelastically in each period. Their income endowment depends on their age, their race-dependent permanent income fixed effect ρ_R, and their race-dependent stochastic income shock ϵ_{ρ_R}. Retired agents between the ages of 60-72 receive a pension, which is a constant fraction of their pre-retirement wage. Agents need to pay progressive income tax

$tax(y)$.

Financial market Each period agents can save in bonds b, which pays $(1 + i)b$ at the beginning of the next period. Agents can have negative bond holding to buy a house if they receive a mortgage, in which case agents owe $(1 + i^m)b$ at the beginning of the next period. Agents cannot hold mortgages and savings in bonds at the same time, and the rate of return on risk-free bonds is lower than the mortgage interest rate ($i < i^m$). The probability of receiving a mortgage in a given period is stochastic, and it depends on the income and race of the agent. At the beginning of each period, agents learn whether they can receive a mortgage ($\mu = 1$) or not ($\mu = 0$). To receive a mortgage, agents also need to satisfy an age-dependent debt-to-income (DTI) and loan-to-value (LTV) ratio, which means agents need to have enough savings first to be able to buy a house. Agents holding a mortgage are guaranteed to have access to a mortgage at the next period, but the actual amount of the next period's mortgage depends on their stochastic income at the next period. Agents need to pay back all of their mortgage loans at age 72, at the latest.

Hand-to-mouth (HtM) households To match the reality that a high fraction of Black households had wealth holding less than 2 weeks of their monthly income in 1970, the model contains hand-to-mouth agents, which is a time-invariant and inheritable trait. The only difference between hand-to-mouth ($HtM = 1$) and non-hand-to-mouth ($HtM = 0$) agents is their discount factor, which is lower for hand-to-mouth households; otherwise, hand-to-mouth agents face exactly the same decision problem as non-hand-to-mouth households.

1.3.2 Homeowners' recursive problem

To make households' decision problem more tractable, each variable without a comma (eg. n or age) represents a state variable, while variables with commas can be either a choice variable in the given period (c' is the optimal consumption in the given state) or next period state variables (eg. age'). I define homeowners as agents who decide to own a house in a given period ($h' = 1$), independently of their initial holding of housing units (h). Depending on agents' initial bond holding (b) and homeownership (h), their residence (n), their stochastic income shock (ϵ), and the possibility of receiving mortgage (μ), in each period agents decide how much to consume (c'), save in bonds (b'), whether to own a house (h') and where to live (n'). I model agents' decision problem in two steps: first, for each neighborhood, agents

make their expected lifetime-utility maximizing consumption, saving, and house purchasing decision, and then they choose the utility-maximizing neighborhoods to live in. We can represent the recursive problem of an agent with race R, permanent income fixed effect ρ, hand-to-mouth condition HtM, and relocation barrier F who decides to own a house in a given period with the following equations:

$$V^{R,\rho,F,HtM}(age, b, h, n, \epsilon, \mu) = \max_{c',b',h',n'} \left\{ u(c', h', n') + \beta_{HtM} \, \mathbb{E} \left[V^{R,\rho,F,HtM}(age', b', h' = 1, n', \epsilon', \mu') \mid \epsilon, \mu \right] \right\}$$

st.

$$y - tax(y) + b(1 + i^*) + p_n h(1 - \delta) = c' + p_{n'} h' + b' + RC(n, n') + SBC(h, h' = 1)$$

$$b' \geq \begin{cases} 0 & \text{if } \mu = 0 \text{ or } age' > 72 \\ max\{-LTV \times p_{n'} h' \, ; \, -DTI \times y \times (1 - (1 + i^m)^{-24})/i^m\} & \text{if } \mu = 1 \; \& \; age' \leq 72 \end{cases}$$

$$i^* = \begin{cases} i & \text{if } b \geq 0 \\ i^m & \text{if } b < 0 \end{cases}$$

$$V^{R,\rho,F}(age' > 72, b', h' = 1, n', \epsilon, \mu) = \theta_{1,R} \left(1 + \frac{(1 + r)b' + p_{n'} h'(1 - \delta)}{\theta_2} \right)^{1-\sigma}$$

1.3.3 Renters' recursive problem

Renters are defined as agents who decide to not hold housing wealth in a given period ($h' = 0$). Renters differ from homeowners in two important dimensions. First, these households need to pay rent q_n instead of living in their own house, which also means they do not transfer wealth across periods in housing wealth ($h' = 0$). Second, as borrowing is allowed in the economy only to finance a house purchase, they cannot choose a negative bond amount ($b' \geq 0$). Then, we can represent the recursive problem of an agent with race R, permanent income fixed effect ρ, hand-to-mouth condition HtM, and relocation barrier F who decides to rent in a given period with the following equations:

$$V^{R,\rho,F,HtM}(age, b, h, n, \epsilon, \mu) = \max_{c',b',h',n'} \left\{ u(c', h', n') + \beta_{HtM} \, \mathbb{E} \left[V^{R,\rho,F,HtM}(age', b', h' = 0, n', \epsilon', \mu') \mid \epsilon, \mu \right] \right\}$$

st.

$$y - tax(y) + b(1 + i^*) + p_n h(1 - \delta) = c' + q_{n'} + b' + RC(n, n') + SBC(h, h' = 0)$$

$$b' \geq 0$$

$$i^* = \begin{cases} i & \text{if } b \geq 0 \\ i^m & \text{if } b < 0 \end{cases}$$

$$V^{R,\rho,F}(age' > 72, b', h' = 0, n', \epsilon, \mu) = \theta_{1,R}\left(1 + \frac{(1+r)b'}{\theta_2}\right)^{1-\sigma}$$

1.3.4 Stationary equilibrium

For a given house price vector P, a stationary equilibrium can be characterized with a collection of value functions $V^{R,\rho,F,HtM}(age, b, h, n, \epsilon, \mu)$, a set of policy functions $c'^{\,R,\rho,F,HtM}(age, b, h, n, \epsilon, \mu)$, $b'^{\,R,\rho,F,HtM}(age, b, h, n, \epsilon, \mu)$, $h'^{\,R,\rho,F,HtM}(age, b, h, n, \epsilon, \mu)$, and $n'^{\,R,\rho,F,HtM}(age, b, h, n, \epsilon, \mu)$, and distribution of households $\Omega_{age}(b, h, n, \epsilon, \mu, R, \rho, F, HtM)$, which satisfies the following conditions:

1. For house price vector P, the policy functions maximize all agents' expected lifetime utility.

2. Policy functions ensure spatial equilibrium, at which the housing market clears in each neighborhood at price vector P.

3. The state distribution of each cohort at any age is invariant over time.

1.4 Estimation

I estimate the model to match the spatial and wealth distribution of Black and White households observed in 1970. I interpret this steady state of the model as the "pre-segregation" era, assuming that most of the relocation of Black families, and thus the decline in residential segregation, took place after 1970, and not between 1968 (when the Fair Housing Act was enacted) and 1970. I make this assumption to be able to compute the spatial distribution of families by using the 1970 decennial census. This assumption seems to be reasonable based on Table 1.2, which shows that large-scale relocation of Black families from Black neighborhoods started in the 1970s and not during the 1960s.

During estimation, I used the census-tract-level aggregated 1970 Census data (LTDB and NHGIS), the 1969-71 waves of SCF+, and the 1968-74 waves of the Panel Study of Income Dynamics (PSID). As my census data contains only urban census tracts, I narrowed down

the SCF+ sample to families that resided in metropolitan areas and in which the head was between the ages of 24 and 72. All calculations are weighted using sample weights (SCF+ and PSID) or the total population of tracts (1970 Census).

1.4.1 Parameters chosen outside of the model

Table 1.3 summarizes all relevant parameters estimated outside of the model.

Preferences Following the literature, I set the risk aversion parameter γ to 2, the discount factors $\beta_{HtM=0}$ to 0.96, and $\beta_{HtM=1}$ to 0.72 (Aguiar, Bils, and Boar, 2020), and the expenditure share of consumption good α to 0.875 (Brandsaas, 2021). To simplify the estimation process, I set θ_2 parameter to 100. Similarly, I set the ratio of White hand-to-mouth agents as 12.14%, which is the fraction of White households holding less wealth than 2 weeks of income in the SCF+.

Income process I model the income process with three components. First, a deterministic age-dependent part ensures that household income follows a life-cycle trajectory observed in the data. Second, a time-invariant income fixed effect generates lifetime income inequality among households, which allows higher-income households to accumulate more wealth over time. Finally, a stochastic income shock term captures individual-level income shocks, which generate income uncertainty during the working age. Estimating the latter two components requires panel dimension, which, unfortunately, is lacking from the SCF+ dataset, which I use to target moments during estimation. To overcome this barrier, I estimate the income process in two steps.

First, using PSID, I estimate the following equation separately for Black and White households

$$log(y_{i,t}) = \beta_0 + \beta_1 age_{i,t} + \beta_{2,R} age_{i,t}^2 + \beta_3 age_{i,t}^3 + FE_i + e_{i,t}$$

where $y_{i,t}$ is total household income inflated into 1970 prices, the cubic polynomial of age is the life-cycle trajectory of the income endowment, FE_i is a fixed effect capturing the permanent income fixed effect, and $e_{i,t}$ is an error term. To calibrate the model's income fixed effect ρ_R, first I divide the sample into three terciles based on the predicted $\widehat{FE_i}$ parameters, and then I set the vector of ρ_R as the weighted mean of the predicted $\widehat{FE_i}$ parameters in these terciles. Then, I calibrate the temporary income shock parameter ϵ_{ρ_R} conditionally on ρ_R: within each $\widehat{FE_i}$ terciles I create three new tertiles based on $\widehat{e_{i,t}}$, and define the vector of ϵ_{ρ_R} as the weighted mean of $\widehat{e_{i,t}}$ in these new terciles. I assume that ϵ_{ρ_R} follows a Markov process with identical transition probabilities (1/3). Calibrating ϵ_{ρ_R}

conditionally on ρ_R allows me to set heterogeneous income shocks by income fixed effect and race, which creates more realistic temporary shocks and income endowments. In the end, this calibration exercise yields three ρ_R and nine ϵ_{ρ_R} values for each race.

In the second step, using the SCF+ data, I regress household income on a cubic polynomial of age. I use the predicted parameters of this regression in the model instead of the ones estimated using PSID because it makes the model's income process fit better to the Black and White income distribution observed in SCF+. After retirement at age 61, agents receive a constant fraction of their age 60 income as pension, which fractions are 60.7% for Black and 70.6% for White households, estimated using the PSID.

I calculated income taxes using the 1970 income tax brackets of married couples filing jointly, which contained 25 marginal tax rates; I set the standard deduction to \$625. To make sure that all households can afford to pay rent, I set the after-tax minimum income in the model as the rent in the White neighborhood. Appendix B contains the estimated income parameters, the overall fit of the income processes, and the income tax brackets.

Mortgage market As Ladd (1998) shows, racial mortgage market discrimination was widespread during the 1960s and 1970s, but based on the 1970 Census of Housing (U.S. Bureau of the Census, 1973), the discrimination seemed to be more prevalent in the extensive margin (whether to get a mortgage) than in the intensive margin (mortgage conditions). The 1970 Census of Housing shows that mortgage conditions of Black applicants were quite similar to population averages in interest rate (6.1% vs. 6.0%), in duration (23.9 vs. 24.5 years), in debt-to-income ratio (11% to 9%), and in loan-to-value ratio at buying (86% vs. 78%, for conventional mortgages). Because of these similarities, I assume that Black and White households face the same mortgage conditions in the model, but the probability of receiving a mortgage differs by race.

I calculated the DTI ratio using the 1968-74 waves of PSID, as SCF+ doesn't contain information on yearly mortgage payments. The weighted mean of DTI of new purchases was 14.46%. I set the mortgage length as 24 years based on the 1970 Census of Housing. These two conditions, with the internally estimated mortgage rate, define a maximum amount of per-period annuity mortgage payment, from which I can calculate the maximum amount of mortgage in the model. LTV ratio also defines an additional restriction on the maximum amount of mortgage, which I calculated using the SCF+. In the model, I assumed that LTV is age-dependent, and I calculated the LTV ratios as the predicted cubic polynomial of age (illustrated in Figure A.4). Because of this assumption, agents cannot hold a high LTV ratio over their lifetime, which makes their life-cycle saving decisions more realistic. I assume that

Table 1.3: Externally estimated parameters

Parameter		Value	Source
Preferences			
Risk aversion	γ	2.0	Standard
Discount factor (non-hand-to-mouth)	$\beta_{HtM=0}$	0.96	Aguiar et. al. (2023)
Discount factor (hand-to-mouth)	$\beta_{HtM=1}$	0.72	Aguiar et. al. (2023)
Expenditure share of consumption	α	0.825	Brandsaas (2021)
Bequest parameter (simplifed)	θ_2	100	
Share of White HtM agents		0.1214	SCF+
Utility from housing good: Owners	$\eta_{W,own}/\eta_{M,own}/\eta_{B,own}$	5.83/5.60/5.60	1970 Census
Utility from housing good: Renters	$\eta_{W,rent}/\eta_{M,rent}/\eta_{B,rent}$	4.33/4.18/4.14	1970 Census
Neighborhoods and housing market			
Housing deprecation	δ	0.025	Standard
House selling/buying cost	e_s/e_b	0.075/0.025	Brandsaas (2021)
Price elasticity of housing supply	Δ	1.37	Saiz (2010)
House price: White neigh.	p_W	\$22,198	1970 Census
House price: Mixed neigh.	p_M	\$16,339	1970 Census
House price: Black neigh.	p_B	\$13,567	1970 Census
Rent parameters: Intercept (W/M/B)		\$659/\$527/\$465	1970 Census
Rent parameters: Slope (W/M/B)		3.35%/2.85%/3.65%	1970 Census
Relocation cost	k	\$150	see text
Share with relocation barrier	π_F	0.5	see text
Mortgage and financial market			
Risk-free rate	i	0.02	Standard
Loan-to-value ratio	LTV	see text	SCF+
Debt-to-income ratio	DTI	0.1446	PSID
Mortgage length		24 years	1970 Census of Housing
Black mortgage discrimination	ψ_B	0.6329	Schafer & Ladd (1981)
75th percentile of household income (1970)	y_{75}	\$13,580	SCF+

the probability of receiving a mortgage is in the form of

$$P_{i,R}(\mu' = 1|\mu = 0) = min\Big(\frac{y_i}{y_{75}}\psi_R; 1\Big); \quad P_{i,R}(\mu' = 1|\mu = 1) = 1$$

where y_i is an agent's income, y_{75} is the 75th percentile of household income from SCF+, and ψ_R is a race-dependent discount factor. This functional form guarantees that agents with mortgages will have access to mortgages in the future, that the probability of getting a mortgage increases with income, and for most income levels Black agents are less likely to receive mortgages. Using mortgage application data from the late 1970s, Schafer and Ladd (1981) found that Black applicants were 1.58–7.82 times as likely to be rejected as Whites with similar observable characteristics. Using this study I set $\psi_B = 1/1.58 = 63.29\%$ for Black agents, while as a normalization I define $\psi_W = 1$ for Whites.

Neighborhoods I use the census tract level 1970 Census to calculate neighborhood characteristics. I map census tracts to my 3 neighborhood categories (White, Mixed, and Black) based on the share of Black individuals in a given tract in 1970. I categorize a

neighborhood as White if the Black share was less than 10 percent, Mixed if its Black share was between 10 and 50 percent, and Black if its Black share was at least 50 percent in 1970.

I defined the consumption value of housing in neighborhoods ($\eta_{n,own}$ and $\eta_{n,rent}$) as the average number of rooms in each neighborhood/tenure cell. I set the relocation cost k to \$150, which was the average renting cost of a moving truck for a day in 1970. To make the model estimation more tractable, as a normalization, I set the race-neutral amenity of households living in the White neighborhood (A_W) as -0.000250. I normalize the three race-dependent neighborhood taste parameters of White agents ($\kappa_{W,W}$, $\kappa_{I,W}$, $\kappa_{B,W}$) to 1, and I assume that Black households gain 10 percent less utility from living in the White neighborhood than White families, which yields $\kappa_{B,W} = 1.1$. This assumption is in line with the finding of the 1976 wave of the Detroit Area Study (Farley and Schuman, 1984) and Aliprantis, Carroll, and Young (2024), which studies found that, all else equal, Black households prefer to reside in Mixed and majority Black neighborhoods over majority White ones. I estimate the remaining two race-independent neighborhood amenities (A_M, A_B) and two race-dependent neighborhood taste parameters ($\kappa_{M,B}$, $\kappa_{B,B}$) within the model. Following Gordon (2023), I set π_F, the fraction of Black households living in the Black or Mixed neighborhood who are not allowed to move into the White neighborhood, to 50%.

Housing market I set neighborhood-level house prices p_n as the average of median house prices in census tracts. Following Brandsaas (2021), I set the selling cost of a home property e_s to 7.5% percent of the price, and the buying cost e_b to 2% of the price. House value deprecates with $\delta = 2.5\%$ between periods. The housing supply is modeled as

$$S_n = \bar{S_n} p_n^{\Delta}$$

where I set Δ as 1.37, which is the population-weighted mean of house supply elasticities of cities in my sample, originally calculated by Saiz (2010). S_n is the fraction of household living in neighborhood n, which is defined by the spatial equilibrium. I calculate the supply shifter parameter $\bar{S_n}$ to make sure that at spatial equilibrium, the equilibrium house prices p_n are equal to house prices computed from the 1970 Census. I calibrate rent in different neighborhoods by estimating the $rent_{i,n} = \beta_0 + \beta_1 p_{i,n} + \varepsilon_i$ equation in the three neighborhood types separately, using tract-level median home prices and rents from the 1970 census. Then, I set neighborhood-level rent q_n using the estimate parameters as $q_n = \widehat{\beta_0} + \widehat{\beta_1} p_n$, where p_n is the equilibrium house price of the model.

1.4.2 Parameters identified within the model

I jointly estimate the 8 remaining parameters of the model by targeting 8 moments with the method of simulated moments. I estimate the remaining two race-independent neighborhood amenities (A_I, A_B) and two race-dependent neighborhood taste parameters ($\kappa_{I,B}$, $\kappa_{B,B}$) within the model, and I identify them with four moments targeting the spatial distribution of Black households in White and Mixed, and White households in Mixed and Black neighborhoods. During estimation, I assumed that agents gain the highest utility by living in the White and lowest utility by living in the Black neighborhood of the model (thus, $A_W > A_I > A_B$), which reflects the existing differences in house prices and rents. Then, I identify the homeownership premium χ and the mortgage rate i^m parameters by targeting the Black and White homeownership rate. Finally, I target the final two moments, the median of the Black and White wealth distribution, with the ratio of Black hand-to-mouth agents and with the White agents' bequest parameter $\theta_{1,W}$. I define Black agents' bequest parameter $\theta_{1,B}$ as one-tenth of $\theta_{1,W}$, as based on SCF+ the median of Black wealth distribution at age 20-24 – when agents enter the model with inherited wealth – was zero.

I estimate the model with simulated annealing, which minimizes the weighted squared distance of the simulated and empirical moments:

$$\mathbf{d}' \, \mathbf{W} \, \mathbf{d}$$

where $\mathbf{d}$ is a vector containing the squared difference of the simulated and empirical moments, and $\mathbf{W}$ is a weighting matrix. The weighting matrix contains the inverse of the empirical moments' bootstrapped variance in the diagonal and zeros in any other places. Table 1.4 presents the estimated parameter values and the difference between the simulated and empirical moments.

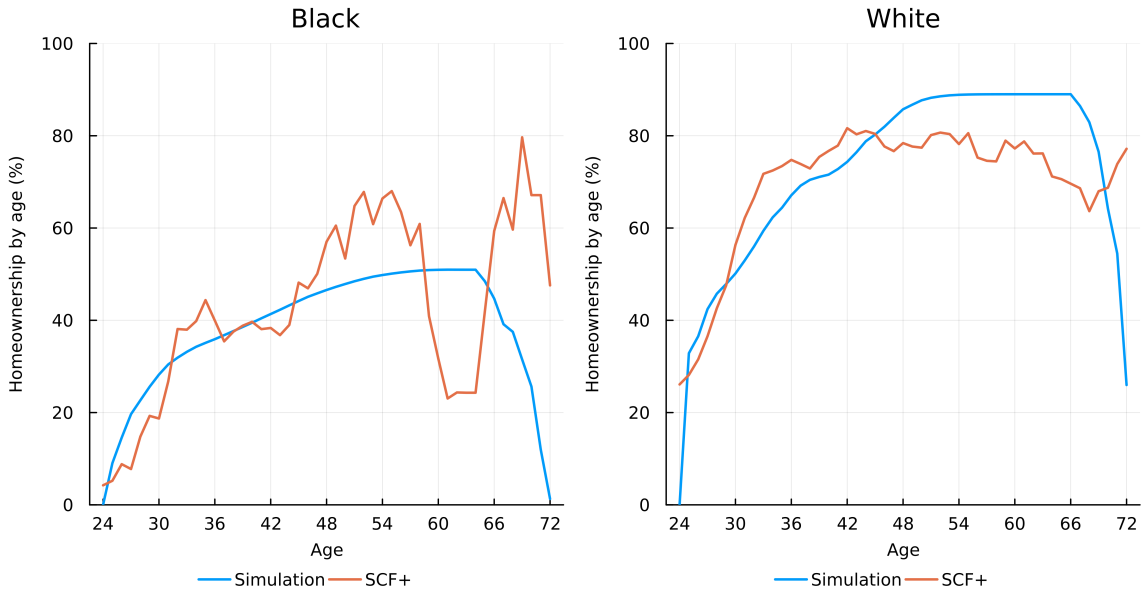
Table 1.4: Internally estimated parameters and model fit

Description	Parameter	Value	Moment	Model	Data
Spatial equilibrium					
Amenity of living in M neigh.	A_M	-32.1e-5	% B living in White neigh.	7.80%	7.29%
Amenity of living in B neigh.	A_B	-36.5e-5	% B living in Mixed neigh.	21.97%	21.53%
Neigh. taste param, B living in M neigh.	$\kappa_{B,M}$	0.8848	% W living in Mixed neigh.	9.19%	9.64%
Neigh. taste param, B living in B neigh.	$\kappa_{B,B}$	0.7940	% W living in Black neigh.	1.70%	2.08%
Mortgage market and homeownership					
Homeownership premium	χ	1.035	B homeownership rate	38.0%	38.3%
Mortgage rate	i^m	4.1%	W homeownership rate	71.9%	69.2%
Bequest & HtM					
Share of hand-to-mouth, B		45%	B median wealth	\$1,257	\$1,237
Bequest parameter, W	$\theta_{1,W}$	-0.00425	W median wealth	\$14,658	\$15,119

The estimated race-dependent neighborhood taste parameters reflect that, compared to Black agents, Whites gain $1/0.8848-1 = 13.0\%$ less utility by living in the Mixed and 25.9% less utility by living in the Black neighborhood. The estimated homeownership premium's magnitude is in line with the literature (eg. 1.015 in Kaplan, Mitman, and Violante (2020), and 1.379 in Brandsaas (2021)), while the mortgage premium ($i^m - i = 2.1\%$) is a little bit higher than in other papers (eg. 0.9% in Brandsaas (2021)). Table 1.4 shows that the model matches most of the targeted moments quite closely. The simulated fractions of Black and White households living in different neighborhoods are within 0.5 percentage points of the targeted moments. The model matches the Black homeownership rate and median wealth moments well, while the median White wealth is nearly 3% lower than the targeted moment, despite that the generated White homeownership is 2.5 percentage points higher than in the data. Despite these differences, the simulated Black–White wealth gap at the median in the model is 11.7, which is close to the 12.2 ratio observed in SCF+.

To be able to run simulations, I set the $\bar{S}_n$ shift parameters in the model. Following Chyn and Daruich (2021), I use the estimated spatial distribution of households in the model to define the demand for housing, which is the fraction of households living in neighborhood n. In spatial equilibrium, the housing market clears, and the housing supply S_n should be equal to the demand for housing, which allows me to calculate the supply shift parameters $\bar{S}_n$ using the spatial distribution of agents in the model and the 1970 house prices observed in the data. The calculated supply shift parameters guarantee that the model economy is in spatial equilibrium with the relocation barrier at the empirically observed house prices.

Figure 1.5: Model fit: Homeownership over the life-cycle



Source: own simulation and own calculation using SCF+. SCF+ data is moving averages (MA5) of homeownership rate at each age.

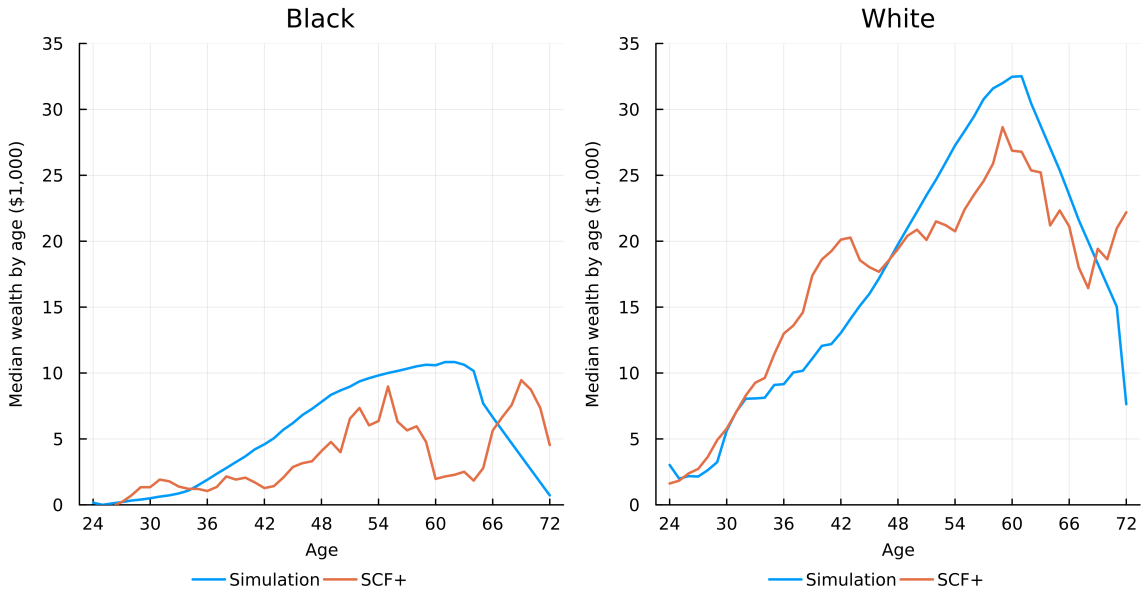
1.4.3 Model fit

The model intends to capture how spatial sorting and homeownership affect households' wealth holding. Table 1.4 showed that the spatial distribution, the homeownership rate, and median wealth holding of both Black and White families of the estimated model fit well with the empirical counterparts. Figures 1.5 and 1.6 illustrate that in the estimated model the evolution of homeownership and median wealth over the life-cycle are also close to the observed life-cycle profiles in the SCF+.

As Figure 1.5 shows, the model generates a realistic homeownership pattern for both Black and White agents. The model captures the fact that Black families' homeownership rate constantly increases over the life-cycle, while the drop in homeownership in the model after age 66 is the result of the certainty that agents leave the model at age 72. Meanwhile, the model captures the quick increase of the White homeownership rate at young age and the relatively flat homeownership rate between age 45-60.

As Figure 1.6 shows, the model generates slightly higher median Black wealth holding over the life-cycle than we can observe in the data. Meanwhile, the model generates a realistic wealth pattern over the life-cycle for White families. Compared to the empirical median wealth holding at the SCF+, the model slightly underpredicts wealth accumulation between

Figure 1.6: Model fit: Median wealth over the life-cycle



Source: Own simulation, own calculation using SCF+. SCF+ data is moving averages (MA5) of median wealth at each age.

ages 30-48 and overpredicts it between ages 48-60. However, the model captures the fact that the wealth holding of White families peaks around age 60, and then it starts to decline quickly.

1.5 Simulations

I use this estimated model to simulate whether the Fair Housing Act could recreate the stylized facts I presented and to quantify how the FHAct affected racial wealth inequality. I simulate the effect of the FHAct by lifting up the relocation barrier in the model economy, which affects π_F fraction of Black families living in Mixed or Black neighborhoods. Black families affected by this relocation friction could prefer to move into the White neighborhood for at least two reasons. First, the amenity level Black agents experience in the White neighborhood ($\kappa_{B,W}A_W$) is higher than in the Mixed ($\kappa_{B,M}A_M$) or in the Black ($\kappa_{B,B}A_B$) neighborhoods; otherwise, no Black agent would prefer to live in the White neighborhood, where the house price and rent are higher. Second, the consumption value of housing goods is also higher in the White neighborhood than in the Mixed and the Black, for both renters and owners (Table 1.3). As house prices and rents are more expensive in the White neighborhood, Black agents need to forgo consuming consumption goods to pay the higher price of living

Table 1.5: Overview of the effect of the FHAct in the model economy

	Before FHAct	After FHAct	Change
Spatial distribution			
Fraction of Black agents living in			
White neighborhood	7.8%	13.8%	+6.0pp
Mixed neighborhood	22.0%	18.4%	–3.6pp
Black neighborhood	70.2%	67.8%	–2.4pp
Fraction of White agents living in			
White neighborhood	89.1%	88.3%	–0.8pp
Mixed neighborhood	9.2%	9.7%	+0.5pp
Black neighborhood	1.7%	2.0%	+0.3pp
Racial residential segregation			
Dissimilarity index (Black/White)	0.813	0.745	–0.068
Equilibrium house price in			
White neighborhood	$22,198	$22,214	+$16
Mixed neighborhood	$16,340	$16,294	–$44
White neighborhood	$13,567	$13,531	–$36
Average housing wealth			
Black	$4,967	$5,278	+$311
White	$12,576	$12,642	+$66
Homeownership			
Black	38.0%	38.1%	+0.1pp
White	71.9%	72.3%	+0.4pp
Average housing wealth of homeowners			
Black	$13,073	$13,864	+$791
White	$17,499	$17,493	–$6
Wealth inequality (Gini index)			
All families	47.44%	47.30%	–0.14pp
Black families	68.22%	68.42%	+0.20pp
White families	44.06%	43.91%	–0.15pp

there. Because of this forgone consumption, some Black agents with lower consumption levels might prefer to stay in the Mixed or the Black neighborhood, as the utility loss of moving and consuming less consumption goods could exceed the utility gain from higher amenity levels and higher housing service consumption. This is unlikely for high-income Black agents with higher consumption levels, for whom the net effect of moving on utility could be positive. Because of this, the end of housing discrimination in the model does not affect all Black agents equally, and only higher-income Black agents are expected to move into the White neighborhood.

By lifting the relocation friction through the Fair Housing Act in the model, previously constrained higher-income Black agents start to move into the White neighborhood, which creates increasing house prices and rents in neighborhoods that gain new residents and declining house prices and rents in neighborhoods that lose population. Then, the changing

house prices could motivate previously unconstrained Black and White families to move. In this way, the relocation of constrained Black families has a spillover effect on other families. At the end of this relocation process, the model reaches a new spatial equilibrium, at which no agent prefers to move at the equilibrium house prices. I quantify the effect of the Fair Housing Act on spatial sorting and racial wealth inequality by comparing the spatial and wealth distribution of this new steady state to the original state with relocation friction.

I compare the quantitative results of the simulations to the empirical changes in spatial distribution and housing wealth observed between 1970-80. I chose this comparison period for two main reasons: first, this is the shortest period following the Fair Housing Act with two decennial censuses, which is the data source of the spatial distribution of Blacks and Whites. Second, the model is estimated to match the economic conditions observed in 1970, and choosing a longer comparison period than 1970-80 would allow more changes in these conditions, which would have created a less realistic counterfactual comparison for simulation results. The goal of the model is to quantify the effect of the Fair Housing Act by keeping all other relevant economic factors unchanged; thus, comparing the model's simulation results to observed empirical changes becomes less and less realistic as we increase the time span.

As important economic factors other than the Fair Housing Act, like the real value of average total family incomes (Figure 1.4), also changed during the 1970s, the simulation results do not need to overlap with the observed changes in spatial distribution and housing wealth. The difference between the simulated and observed aggregates is informative in determining whether the decreasing segregation and the quickly increasing Black housing wealth is the consequence of the Fair Housing Act or the consequence of other economic changes in reality that are not incorporated directly in the model.

1.5.1 FHAct's effect on spatial sorting

As Table 1.5 summarizes, the lift of the relocation barrier, which prevented some Black households from entering the White neighborhood of the model, increases Black presence in the White neighborhood. In the new spatial equilibrium of the model, the fraction of Black households living in the White neighborhood increases from 7.8% to 13.8%, primarily because previously constrained Black households move out from the Mixed and Black to the White neighborhood. As a result, the share of Black families living in the Mixed and Black neighborhoods decreases by 3.6 and 2.4 percentage points, respectively. Meanwhile, because of the relocation of Black families, house prices decrease in Mixed and Black neighborhoods, which motivates White families to move there. As a result, the fraction of White families living in the White neighborhood decreases by 0.8 and increases in the Mixed and Black

Table 1.6: Relocation across neighborhoods: Model vs. Data (1970-80)

	White neigh.	Mixed neigh.	Black neigh.
Panel A: Change in spatial distribution – Simulations			
Black agents	+6.0pp	–3.6pp	–2.4pp
Homeowners	*+4.4pp*	*–2.0pp*	*–2.3pp*
Renters	*+1.6pp*	*–1.6pp*	*–0.1pp*
White agents	–0.8pp	+0.5pp	+0.3pp
Homeowners	*+0.1pp*	*+0.0pp*	*+0.3pp*
Renters	*–0.9pp*	*+0.5pp*	*+0.0pp*
Panel B: Change in spatial distribution – Census, based on population change			
Black population	+3.0pp	+1.1pp	–4.2pp
White population	–2.3pp	+2.1pp	+0.1pp
Panel C: Change in spatial distribution – Census, based on occupied housing units			
Housing units occupied by Blacks	+3.0pp	+0.7pp	–3.7pp
Homeowners	*+0.4pp*	*–0.7pp*	*+2.2pp*
Renters	*+2.6pp*	*+1.5pp*	*–6.0pp*
Housing units occupied by Whites	–2.7pp	+3.1pp	–0.4pp
Homeowners	*–1.6pp*	*+1.9pp*	*–0.1pp*
Renters	*–1.1pp*	*+1.2pp*	*–0.3pp*

Notes: In census data White population contains both Hispanic and non-Hispanic Whites. Neighborhoods are defined based on the Black share observed in 1970 for the 1970 census and based on the Black share in 1980 for the 1980 census.

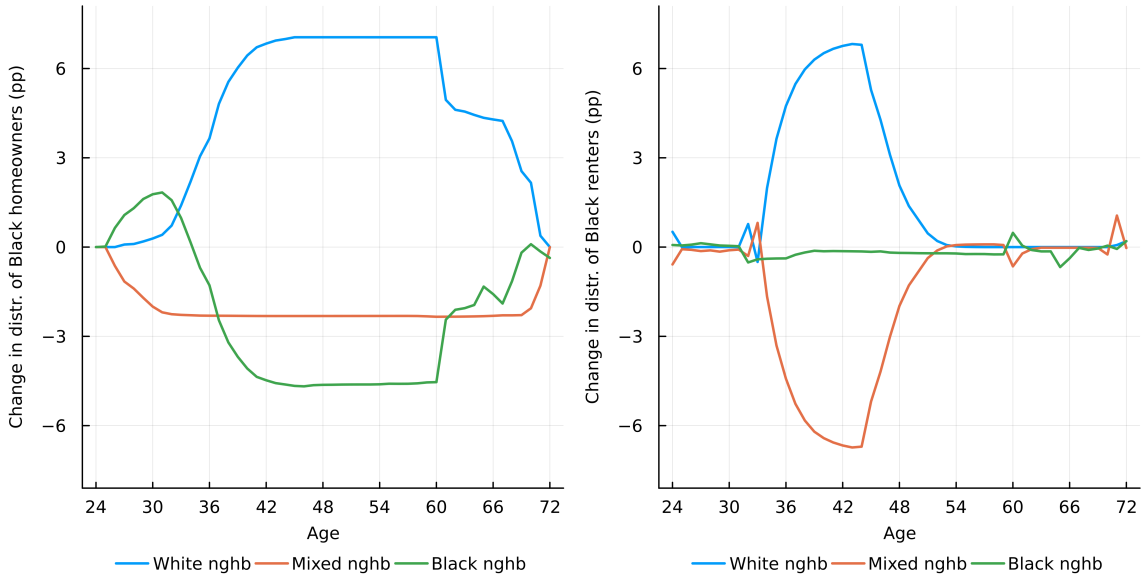
neighborhoods by 0.5 and 0.3 percentage points, respectively. Because of the countermovement of White families into Mixed and Black neighborhoods, the fraction of agents living in each neighborhood remains relatively unchanged, and the change in equilibrium house prices is modest.

Table 1.6 compares the change of spatial distribution of Black and White agents in the model to the observed change of two different distributions from the census. Panel B shows how the distribution of the Black and White population changed between 1970-80 in White, Mixed, and Black neighborhoods,[4] which is a reliable comparison to the simulated changes in Panel A. The spatial distributions of renter and homeowner families are not available in the census, only the distribution of housing units that were occupied by Black or White families. Panel C illustrates how the spatial distribution of housing units changed by race and tenure, which could be informative in determining whether homeowners or renters were more likely to move. In Panel A and C, agents and housing units are divided into renters and homeowners, and the sum of the percentage point changes in homeowners and renters should add up to the total changes.[5] Similarly, the total changes in the spatial distribution

[4]Neighborhoods are defined based on Black share observed in 1970 for the 1970 census, and based on the Black share in 1980 for the 1980 census.

[5]For example, Panel A shows that the share of Black families living in the White neighborhood increased by 6 percentage points. Out of these 6 percentage points, the increasing share of Black homeowners living

Figure 1.7: Change of spatial distribution of Black agents over the life-cycle, by homeownership



Source: Own simulation.

of Blacks and Whites should add up to zero among neighborhoods.[6]

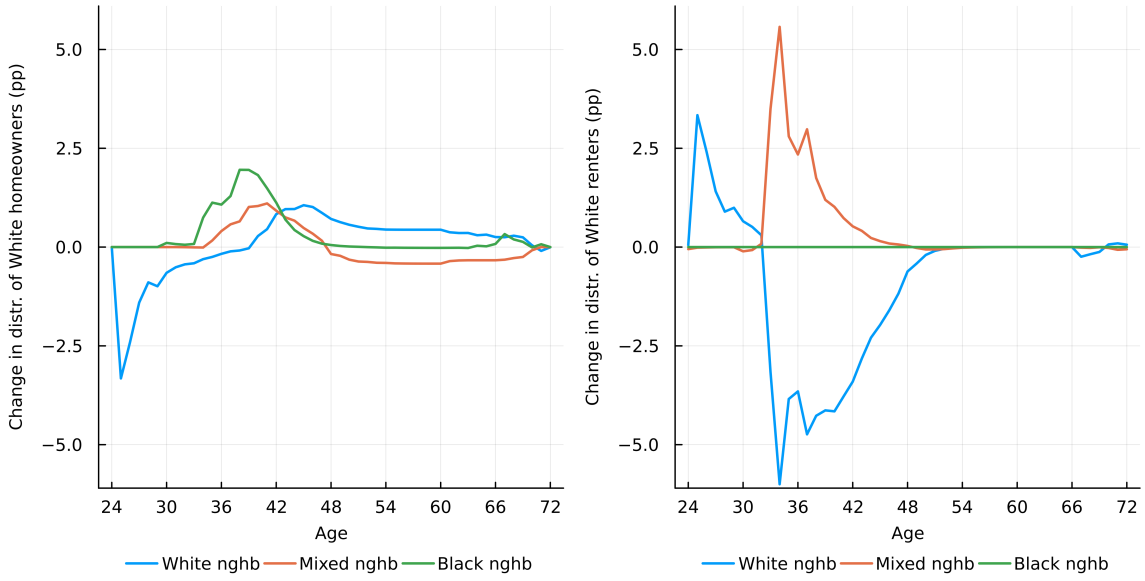
As more than one family could have stayed in one housing unit, the two different measures of spatial distribution change in Panel B and C in Table 1.6 are different, but they depict the same main patterns. Both measures suggest that, between 1970 and 1980, the share of Black families living in White and Mixed neighborhoods increased, while the share of Whites living in Mixed neighborhoods increased by almost the same magnitude as the fraction of Whites decreased in White neighborhoods. The only bigger difference between Panel B and C is the change in the share of the White population and housing units residing in Black neighborhoods. Even if the share of housing units occupied by White households decreased by 0.4 percentage points in Black neighborhoods (in Panel C), the share of Whites living in these neighborhoods increased by 0.1 percentage points (in Panel B).

Comparing simulation results in Panel A with the observed changes in the spatial distribution of Blacks and Whites in Panel B reveals that the Fair Housing Act could explain the

in White neighborhoods explains 4.4 percentage points, and the increasing presence of Black renters in the White neighborhood contributed by 1.6 percentage points.

[6]For example, in Panel A, the share of Black agents living in the White neighborhood increased by 6 percentage points and decreased by 3.6 percentage points in the Mixed and 2.4 percentage points in the Black neighborhood. These changes should add up to zero (+6pp–3.6pp–2.4pp=0pp), or almost zero in case of rounding errors.

Figure 1.8: Change of spatial distribution of White agents over the life-cycle, by homeownership



Source: Own simulation.

direction of changes in the spatial sorting of Blacks and Whites. Simulation results suggest that, because of the Fair Housing Act, Black families relocated from Black to White neighborhoods, while the FHAct also explains why the share of Whites living in White neighborhoods decreased and their share in Mixed and Black neighborhoods increased. Compared to the observed change in spatial distribution, the model predicts a lower level of White relocation from White to Mixed neighborhoods and higher Black relocation to White neighborhoods. Also, in the model, Black presence decreased in Mixed neighborhoods because of the Fair Housing Act, while in reality, the share of Blacks living in Mixed neighborhoods increased between 1970-1980. The source of these differences could be the unmodeled economic changes during the 1970s, or the possibility that neighborhoods could have become Mixed from White as Black families moved there, which the model cannot capture because of the limited number of neighborhoods.

Another possible explanation could be the ongoing difficulties Black families experienced during buying homes in White neighborhoods. In the simulations, after the FHAct, Black families faced no barriers to purchasing homes in the White neighborhood, and most of the relocation of Black families was driven by homeowners who decided to own a home in a White neighborhood instead of a Mixed or Black neighborhood of the model. As Panel C in

1.6 suggests, in reality, most Black relocation was driven by renters instead of homeowners, and most of the increase in Black homeownership took place in Black neighborhoods. The decrease in the share of units rented by Blacks (–6.0pp) is almost as large as the increase in the share of units rented in White and Mixed neighborhoods (+2.6pp and +1.5pp) and owned in Black neighborhoods (+2.2pp) by Black families. The observed change in the distribution of Black-occupied housing units suggests that Black families faced fewer difficulties moving into more integrated neighborhoods as renters, while in the model, Black renters and homeowners face the same constraint moving into the White neighborhood.

The Fair Housing Act had a sizable effect on housing wealth through Black and White agents' location and homeownership decisions in the model. Figures 1.7 and 1.8 illustrate how FHAct changed the spatial distribution and homeownership decision of agents over the life-cycle. The right panel of Figure 1.7 shows that the FHAct mainly affected Black renters' spatial distribution in the middle of agents' lives, as previously constrained Black agents moved into the White neighborhood from the Mixed neighborhood of the model. Because of the unconstrained relocation following the FHAct, many previously constrained Black homeowners moved from the Mixed and Black neighborhood into the White, but following retirement at age 60, many of these Black homeowners moved back to the Black neighborhood and consumed some part of the previously accumulated wealth.

Meanwhile, as Figure 1.8 shows, the spillover effect of Black agents' relocation had a sizable effect on White agents' location and homeownership decisions in the first part of their lives. Following the FHAct, house prices and rents increased in the White and decreased in the Mixed and Black neighborhoods of the model. This motivated some White families to postpone homeownership in the White neighborhood at the beginning of their lives and become homeowners in the Mixed and Black neighborhoods. In the end, as the decreased share of White homeowners in the Mixed and their increased share in the White neighborhood suggest, some White homeowners were able to replace their home in the Mixed with a home in the White neighborhood in the second part of their lives. These agents were able to accumulate more housing wealth than before the FHAct, which increased both White agents' average housing wealth and homeownership rate (Table 1.5).

1.5.2 Could FHAct have caused the three stylized facts?

To determine whether the Fair Housing Act caused the three stylized facts I presented in Section 1.2, I compare the simulated changes in spatial distribution and housing wealth in the model to the observed empirical changes between 1970-80. Choosing this short time window right after the FHAct gives realistic but not perfect comparisons, as counter to the

Table 1.7: Stylized facts: Model vs. Data

	Data (1970-80)	Simulation
Stylized fact I: Decline of racial segregation		
Dissimilarity index (Black/non-Black)	–6.1pp	–6.8pp
Stylized fact II: Black families started to leave Black neighborhoods		
Change in the share of Black families living in Black neighborhoods	–4.1pp	–2.4pp
Stylized fact III: Black housing wealth increased at a faster pace than White		
Racial difference in avg. housing wealth growth (Black – White)	+30.2pp	+5.7pp
Change of racial gap in homeownership (Black – White)	+5.6pp	–0.3pp
Racial difference in avg. housing wealth growth of homeowners (Black – White)	+5.1pp	+6.3pp

model economy, economic conditions changed in the real world.

Stylized Fact I: Decline of residential segregation

In line with the sizable changes in the spatial distribution of Black agents and the countermovement of Whites, the dissimilarity index between Black and White families decreased from 81.3% to 74.5% in the model (Table 1.7). The 6.8 percentage point decline of the dissimilarity index is just slightly higher than the 6.1 percentage point decline observed in the data between 1970-80. This suggests that, by easing the relocation barriers Black families faced while moving to White neighborhoods, the Fair Housing Act could explain the start of residential segregation's decline during the 1970s.

Stylized Fact II: Black families started to leave Black neighborhoods

As Table 1.7 summarizes, the Fair Housing Act decreased the share of Black agents residing in the Black neighborhood of the model. The observed decline in the share of Blacks residing in Black neighborhoods (4.1 percentage points) was larger than the model prediction (2.4 percentage points), which suggests that in reality factors not directly incorporated in the simulations prevented Black families from moving to White neighborhoods. However, the simulation results imply that the Fair Housing Act increased the outmigration of Black families from Black neighborhoods.

Stylized Fact III: Black families' housing wealth increased at a faster pace than Whites'

Figure 1.1 in Section 1.2 shows that even if the nominal differences in Black and White average housing wealth increased during the 1970s (left panel), the growth rate of Black

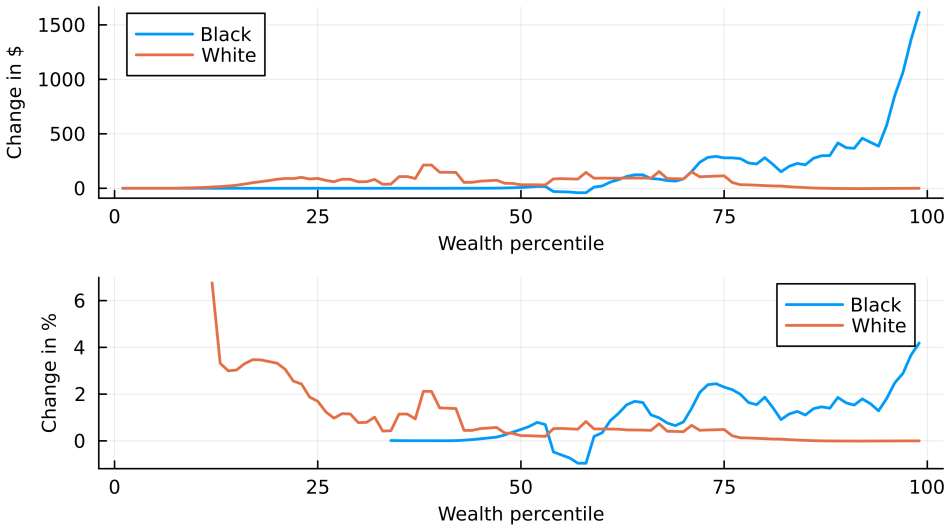
housing wealth was higher than White (right panel), which is the first necessary condition to close the racial gap in nominal housing wealth differences in the long run. As Table 1.7 summarizes, the difference between the growth rate of Black and White average housing wealth between 1965-69 and 1975-79 (right panel of Figure 1.1) was 30.2 percentage points. In the model, the FHAct increased the average Black and White housing wealth by 6.26 and 0.52 percentage points, respectively, which yields a 5.7 percentage point difference in racial housing wealth growth. Thus, the simulation results suggest that the Fair Housing Act increased Black housing wealth at a faster pace than that of Whites', and the magnitude of the FHAct's direct effect is almost a fifth of the observed racial difference in housing wealth growth rate.

We can divide the change in average housing wealth into the change in homeownership rate and the change in average housing wealth of homeowners. This distinction reveals that in the model, Black housing wealth increased faster than White because Black homeowners increased their housing wealth more than White homeowners. Following the FHAct in the model, many Black families decided to own homes in White instead of Mixed and Black neighborhoods (see Figure 1.7), which increased their housing wealth. Meanwhile, the average housing value of White homeowners slightly decreased in the model, primarily because 0.3% of White agents switched from being renters in the White neighborhood to becoming homeowners in the cheaper Black neighborhood (Table 1.6). As a result, in the model the growth rate of Black homeowners's housing wealth was 6.3 percentage points higher than White homeowners', which is quite similar to the observed difference in the data (+5.1 percentage points).

In the model, the Fair Housing Act increased the White homeownership rate (+0.4pp) at a faster pace than the Black homeownership rate (+0.1pp); as a result, racial differences in homeownership rate increased by 0.3 percentage points due to the FHAct in the model. In reality, the Black homeownership rate increased at a faster pace than the White (see Figure 1.2), and racial differences in homeownership rate decreased by 5.6 percentage points. The simulation results suggest that the observed large decline in racial homeownership difference was not the direct effect of the Fair Housing Act but other favorable changes in economic conditions (like increasing real incomes).

All in all, my simulation results suggest that the Fair Housing Act decreased racial differences in average housing wealth, but not at the magnitude observed in the SCF+. However, as the closing gap was the result of relatively wealthy Black homeowners' relocation from the Mixed and Black neighborhood to the White one, the decreasing racial differences in housing wealth might not imply decreasing racial wealth inequality in most parts of the wealth distribution.

Figure 1.9: Effect of free movement on wealth percentiles



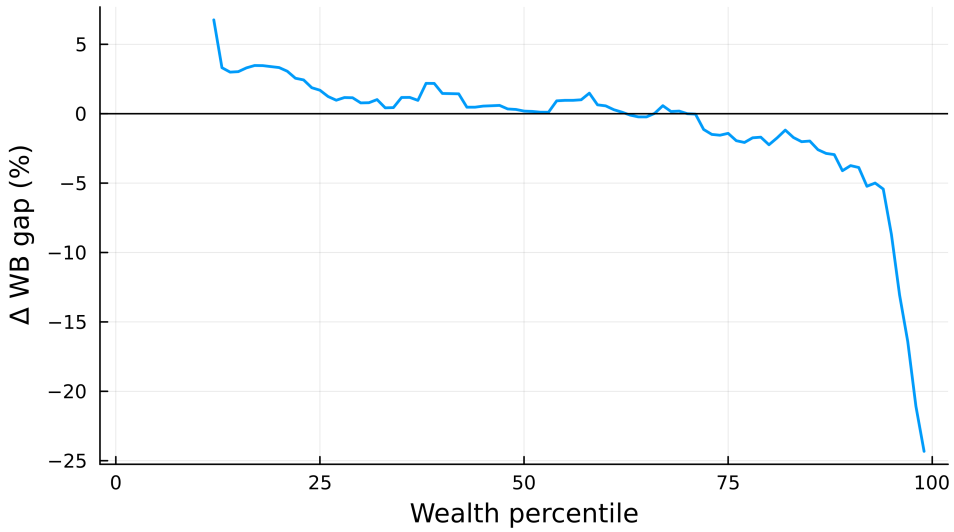
Source: Own simulation. Lines are moving averages (MA5) of the change of simulated wealth percentiles, defined as $p' - p$, where p' is a given wealth percentile without the relocation barrier, and p is the same wealth percentile with the barrier. Missing data points at the bottom were caused by division by zero wealth.

1.5.3 FHAct's effect on racial wealth inequality

Finally, I quantify the effect of the Fair Housing Act on racial wealth inequality by comparing the change in the steady state of Black and White wealth distributions. Figure 1.9 illustrates how the wealth holding changes at a given percentile of the Black and White wealth distribution, both in dollar and real terms. Based on my simulation results, the Fair Housing Act had a sizable and heterogeneous effect on the Black wealth distribution. Wealth holding in the top 25% of the Black wealth distribution increased by 1.5–4%, while in the 6th decile, wealth holding decreased by almost 1%. These results suggest that the positive effect of the FHAct concentrated on the upper end of the Black wealth distribution, while the FHAct could have had a negative effect on the wealth holding of families in the middle of the Black wealth distribution. As a result, the GINI index among Black agents increases from 68.2% to 68.4% in the model, and thus the FHAct increased wealth inequality among Black families.

Meanwhile, the Fair Housing Act increases wealth holding in the bottom and middle part of the White wealth distribution. The top panel of Figure 1.9 shows that even if the change in White wealth holding does not exceed $200 in any wealth percentiles, wealth holding increases by 2–6% around the 15-40th percentiles of the White wealth distribution. One source of this change is the increasing White homeownership in Black neighborhoods at relatively early ages (Figure 1.8), as some White agents switched from being renters in the

Figure 1.10: Effect of free movement on White/Black wealth gap at different percentiles



Source: Own simulation. Lines are moving averages (MA5) of the percent change of simulated White/Black wealth gap at different percentiles; see text for further clarification of calculation. Missing data points at the bottom were caused by division by zero wealth.

White to being homeowners in the Black neighborhood. Also, as Figure 1.8 shows, this switch allowed a small fraction of White agents to own a home in the White instead of the Mixed neighborhood at later ages. So, even if many White agents were not affected by the FHAct in the model, the implied house price changes of the policy alter the optimal homeownership and location choice of White agents on the margin of becoming homeowners. As these agents are concentrated in the middle part of the White wealth distribution, the Fair Housing Act's positive effects concentrated between the 20-60th percentiles of the White wealth distribution. As the top of the White wealth distribution remained unaffected, wealth inequality declined slightly among White families (Table 1.5).

We can quantify the Fair Housing Act's effect on racial wealth inequality by comparing how the nominal differences between the Black and White wealth distribution changed at each wealth percentile:

$$\Delta\, WB\ gap_p = \frac{WB\ gap_{p,post}}{WB\ gap_{p,pre}} = \frac{W_{p,post}\ -\ B_{p,post}}{W_{p,pre}\ -\ B_{p,pre}}$$

where W_p and B_p are the wealth holding of the White and Black wealth distribution at percentile p, WB_p is the nominal difference between the White and Black wealth distribution at percentile p, while pre and $post$ reflect the equilibrium before and after the introduction of the FHAct in the model. Increasing $\Delta\, WB\ gap_p$ means that the Fair Housing Act increased

nominal wealth differences between White and Black households at percentiles p. As Figure 1.10 depicts, the Fair Housing Act decreased racial wealth inequality at the top quarter of the wealth distribution, especially in the very top percentiles, where the decrease reached double digits. As the FHAct did not affect the top of the White wealth distribution in the simulation, the large reduction of racial wealth inequality at the top is the result of relocating Black families accumulating more net housing wealth than before. Meanwhile, the Fair Housing Act slightly increased racial wealth inequality between the 2nd and 7th wealth deciles, where $\Delta\, WB\, gap_p$ increased by 1–2%, on average.

As Figure 1.9 shows, the FHAct decreased wealth holding in only a small part of the Black wealth distribution; for the majority of the households, the policy change was wealth neutral or increased their wealth. In this sense, the FHAct increased the wealth and the utility of almost all agents in the model. However, my simulations reveal that the gains of the FHAct were quite unequal, and most of them concentrated at the top of the Black and at the middle of the White wealth distribution. As a result, the FHAct increased wealth inequality among Black and decreased it among White families. Due to the policy change, wealthy Black families were able to get closer to wealthy White families' wealth holding, but the FHAct also increased the wealth difference between Black and White households in the majority of the wealth distribution. Put another way, even if the vast majority of Black agents experienced increasing or unchanged wealth due to the FHAct in the model, more than half of Black agents experienced that White agents at the same percentile of their wealth distribution benefited more from the policy change than them.

1.5.4 Discussion: How are simulation results affected by the amenity structure of the model?

As a modeling simplification, both race-independent amenities A_n and race-dependent taste factors $\kappa_{R,n}$ are exogenous and do not change as families relocate in the model.[7] Agents' utility from living in neighborhood n also does not depend on any individual-specific taste parameter; conditionally on race, all agents gain the same utility by living in a given neighborhood. This implies that conditionally on race, age, and current income, agents with higher expected lifetime income sort into neighborhoods with higher amenities in the model.

The exogenous amenity structure of the model could have two important implications for families' relocation decisions in the simulations. First, the outflow of high-income Black families does not change amenity levels in the Mixed and Black neighborhoods. Compared

[7] I am currently working on an extension in which I endogenize A_n; as this section shows, endogenizing $\kappa_{R,n}$ does not seem to be necessary.

to endogenous amenity structure, in which the race-dependent amenity A_n is a positive function of neighborhood-level average family income, non-changing amenities likely create less outflow of families from the Mixed and Black neighborhoods. This way, non-changing amenities could partly explain why the change in the share of Black families living in the Black neighborhood (Stylized fact II in Table 1.7) was less in the simulations (-2.4pp) than in reality (-4.1pp). Because of less relocation, the house price change is likely lower with exogenous amenities as well, and changing amenities would imply a higher house price increase in the White and a higher house price drop in the Mixed and Black neighborhoods.

Theoretically, the effect of exogenous race-independent amenities A_n on the simulation results in housing wealth and racial wealth inequality is not clear, but is likely modest. On the one hand, in the case of endogenous amenities, decreasing amenities in the Black and Mixed neighborhoods could reduce the homeownership-motivated movement of White and Black families to these neighborhoods. On the other hand, in the case of endogenous amenities, the even greater decrease in house prices in the Mixed and Black neighborhoods could increase the homeownership-motivated movement of families from the White neighborhood. Moreover, in the case of endogenous amenities, the homeownership-motivated inflow of higher-income families into the Mixed and Black neighborhoods could also increase average family income and amenities there, which, by itself, could lead to higher house prices through more inflow of families. Because of this circularity between the change in amenities and house prices through the homeownership-motivated relocation of families, the new steady-state distribution of housing wealth in the model could be quite similar using either exogenous or average income-dependent endogenous amenities. As a result, using endogenous A_n amenities would likely not change my simulation results on racial wealth inequality substantially.

Second, the other way exogenous amenities could affect simulation results is through race-dependent amenity factor $\kappa_{R,n}$. In the model, the inflow of Black families does not change White families' taste for living in the White neighborhood, which could potentially motivate White families to move. In this case, the model cannot capture the potential tipping of the White neighborhood. However, under reasonable preferences on the racial composition of neighborhoods, tipping is not realistic in the model's White neighborhood. Card, Mas, and Rothstein (2008) found that, during the 1970s, neighborhoods tipped as White families started to leave if the minority share of neighborhoods, on average, exceeded 9-12% in 1970. Meanwhile, in the initial steady state of the model, only 1.1% of families were Black in the White neighborhood, which increased to 1.9% in the new steady state; based on Card, Mas, and Rothstein (2008)'s calculations, neither Black share should indicate tipping in the model's White neighborhood.

1.6 Conclusion

In 1968 the Fair Housing Act (FHAct) terminated lawful housing discrimination, which was assumed to be one of the major sources of racial wealth inequality. In this paper, I presented that, in the following decades, more and more Black families moved to integrated neighborhoods and racial residential segregation declined quickly. Meanwhile, despite Black families being able to accumulate housing wealth at an increased pace during the 1970s, the wealth inequality between Black and White families did not change drastically in the following decades (Derenoncourt et al., 2024). In this paper, I investigate whether, without other changes in the economy, the Fair Housing Act reduced racial residential segregation and racial wealth inequality at the same time.

I quantify the direct effect of the FHAct on segregation and racial wealth inequality by estimating a quantitative heterogeneous agent life-cycle OLG model. Among the first in the literature, I model households' homeownership and location choices jointly within a dynamic model, which yields new insights into whether the FHAct motivated households to change neighborhoods to become homeowners at different points in their lifetime. In the model, households' relocation among neighborhoods endogenously changes house prices and, through them, housing wealth, while changing house prices creates spillover in the model by motivating other households to move. I model housing discrimination by preventing a fraction of Black families from moving into White neighborhoods, while White households are free to move into any neighborhood of the model. I estimate this model to match the observed spatial distribution, homeownership, and median wealth holding of Black and White families in 1970.

I simulate the effect of the FHAct by allowing all Black families to move into the White neighborhood of the model, which motivated 6% of Black households to move there. The relocation of predominantly high-income Black households increased house prices in the White and decreased house prices in the Mixed and Black neighborhoods, which motivated 0.8% of White agents to leave the White neighborhood of the model. As a result, I find that the FHAct reduced the most common measure of segregation, the dissimilarity index between Blacks and non-Blacks, by 6.8 percentage points, which is slightly higher than the observed decline in segregation during the 1970s. Meanwhile, the FHAct also decreased racial differences in average housing wealth, but it had a heterogeneous effect on racial wealth inequality. I find that most wealth gains of the Fair Housing Act concentrated in the upper part of the Black and middle part of the White wealth distribution. Therefore, racial wealth inequality substantially decreased in the top 25% but slightly increased in the middle of the wealth distribution.

My results suggest that, in general, the Fair Housing Act did not lead to a decline in racial wealth inequality; moreover, I find that racial wealth inequality increased in the majority of the wealth distribution due to the Fair Housing Act. As an important policy implication, my work illustrates the importance of taking heterogeneous general-equilibrium effects into account during the design of new policies, which intend to create a more equal society by changing the spatial distribution of families. As my findings suggest, as an unintended consequence, most of the positive effects of these policies could easily be concentrated among already wealthy and non-targeted families.

CHAPTER 2

Does Earlier Return to Work Help Mothers' Career? Evidence via the Substitution Effect of Parental Leave

with Anikó Bíró, Lili Márk, and Tímea Laura Molnár

2.1 Introduction

Career interruptions due to giving birth to children are known to contribute to the gender pay gap (Bertrand, Goldin, and Katz, 2010; Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019), and parental leave policies have been found to strongly affect the length of career interruptions around childbirth (Kleven et al., 2024). At the same time, the literature is inconclusive about whether the length of career interruptions itself affect women's labor market trajectories in wages/earnings in the long run. Earlier cross-country studies on parental leave policies tend to find positive employment effects of short parental leave duration, but negative effects of longer parental leave duration (Olivetti and Petrongolo, 2017), while more recent micro-level studies, exploiting parental leave reforms, suggest that the duration of maternity leave does not negatively affect the employment or earnings of mothers in the long run. For instance, Rossin-Slater, Ruhm, and Waldfogel (2013) analyze the effects of the introduction of paid leave program in California and find that it did not significantly affect employment but had positive effect on hours worked and earnings 1-3 years after childbirth, while Bailey et al. (2025) do not find evidence that the same policy increased employment or boosted earnings in the long run. Schönberg and Ludsteck (2014) analyze a series of expansions of maternity leave duration in Germany and find that each expansion led to a delay in the return to work of mothers, however, the expansions did not have an effect on employment rates and earnings 3–6 years after childbirth. Dahl et al. (2016) show that the expansion of paid maternity leave in Norway increased the time spent

out of the workforce, but it did not affect employment or earnings in the long run. Lalive et al. (2014) and Kleven et al. (2024) arrive at similar conclusions based on a series of reforms affecting the parental leave duration in Austria. Yet, Kuka and Shenhav (2024) show evidence from the US that increasing employment beyond the first year after a first childbirth has positive long-run effect on wages.[1,2]

One potential reason for the inconclusive findings on whether the length of career interruptions affect women's long-run labor trajectories is that parental leave reforms may also affect workforce composition, thereby affecting long-run wage estimates through compositional changes, differently across prior studies. In this paper, we leverage a Hungarian reform that affected only the possibility of working—essentially abolished the return-to-work restriction—during the receipt of parental leave benefits in the child's 2nd year of age, while keeping the overall benefit amounts unchanged. Using an Event Study research design with women without births in the control group and linked employer-employee data, we find that mothers in eligible cohorts were by 3 percentage points (30 percent) more likely to work, but only in the narrow window of 19–24 months after childbirth. We not only find a large effect on mothers' propensity to work exactly in the period when they have the option to work and keep the parental leave benefit at the same time, but also verify that this policy did not have an effect on subsequent fertility. Thus, this policy provides a unique setting to study mothers' long-run labor market trajectories, when a subset of them was incentivized to bring forward their return-to-work, but otherwise no differences across cohorts exists.

With the unique Hungarian policy setting, to the best of our knowledge, we provide the first clean estimates on the *timing* of mothers' return-to-work on mothers' long-run labor market trajectories 3-5 years after childbirth when there are no employment effects anymore—in terms of wages, the firm-specific wage premium and occupation characteristics—without the contaminating effect of changing workforce composition. And while there is extensive literature on mothers' occupational sorting after return-to-work post-childbirth (*e.g.*, Bertrand, Goldin, and Katz, 2010; Cortés and Pan, 2023; Boinet, Norris,

[1]Looking at the effect on health outcomes, while Bütikofer, Riise, and M. Skira (2021) show that the introduction of paid maternity leave in Norway had positive effects on maternal health, Chuard (2023) and Ahammer et al. (2023) show that lengthening the duration of parental leave in Austria led to worse mental health.

[2]There is a smaller literature on the labor market effects of parental leave benefit levels. Bana, Bedard, and Rossin-Slater (2020) estimate the effect of maternity benefit amount on labor market outcomes, applying a regression kink design on maternity benefits in California; they find that higher maternity benefits do not affect the duration of maternity leave and increase the probability of returning to the pre-leave employer instead of switching to a new firm. Ginja, Jans, and Karimi (2020) use a similar empirical strategy to estimate the effect of parental leave benefit levels in Sweden, and find that higher parental leave levels imply a reduction in the labor supply of mothers both in the extensive and intensive margin, but only in the short-run.

and Romiti, 2024), we present first evidence on how *earlier* return-to-work affects such occupational sorting years after childbirth. We find modest effects, on average, on mothers' wages for some groups (for those who gave birth at an older age and live in the capital), and a clear positive effect on their firms' average wage premium 3-5 years after giving birth – these results are due to eligible mothers being less likely to switch 'down' to lower-paying firms. We also find that eligible mothers sort to occupations that require less analytical thinking, stress tolerance, leadership and willingness to take on responsibilities, and involve less time pressure.

Our second contribution is to identify the substitution effect of paid parental leave on mothers' long-term employment propensities. Following Autor and Duggan (2007), (i) the substitution effect arises due to the fact that paid parental leave recipients face an incentive not to work since working would mean sacrificing the benefits, while (ii) the income effect captures that given the parental leave benefits, beneficiaries may prefer leisure to labor, even if working is possible while receiving the benefits. The substitution effect is distortionary and causes deadweight loss, because it distorts the relative price of non-work (Autor and Duggan, 2007; Chetty, 2008). While the distinction of the two effects is a central topic in the literature on the labor market effects of disability insurance and unemployment insurance (Autor and Duggan, 2007; Chetty, 2008; Deuchert and Eugster, 2019; Krekó, Prinz, and Weber, 2024, among others), we are not familiar with studies that distinguish either of the two channels related to paid parental leave. At the same time, identification of the substitution effect is key and policy-relevant, because it shows to what extent paid parental leave distorts the incentives to work and for whom.

Our unique policy setting allows us to identify the substitution effect of paid parental leave. In our setting, before the reform, early return-to-work had a cost in the form of sacrificing the benefits; then, the reform removed this "cost", or, in other words, this "implicit tax" of working. We find that mothers in eligible cohorts were more likely to work, but only in the narrow window of 19–24 months after childbirth. We find that our point estimate on the propensity to work is driven by mothers who give birth to their first child at a relatively older age (after the age of 30), and by those who are high-earners pre-birth.

In what follows, Section 2.2 introduces the parental leave system in Hungary, and illustrates how the reform abolished the return-to-work restriction during the receipt of parental leave benefits. Section 2.3 gives a conceptual framework for the identification of substitution effects in paid parental leave. Section 2.4 describes the data and measurement of key variables, and Section 2.5 describes our empirical strategy. Section 2.6 presents and discusses our preliminary results. Finally, Section 2.7 concludes.

2.2 Institutional Background

2.2.1 Paid Parental Leave in Hungary

Act LXXXIII of 1997 of the Hungarian Labor Code establishes three main types of parental benefits: (1) baby-care allowance, (2) childcare benefit, and (3) childcare allowance. Since most of the beneficiaries of the Hungarian parental benefits are women, we refer to the three types of parental benefits jointly as "maternity benefit", and we refer to recipients as mothers. The baby-care allowance is payable for twenty-four weeks, the childcare benefit is payable after baby-care allowance for up to two years, and the childcare allowance is payable after baby-care allowance and childcare benefit (or immediately after giving birth if the mother is not eligible for baby-care allowance and childcare benefit) for up to three years.

The baby-care allowance and the childcare benefit are social insurance benefits, based on prior employment and earnings. Parents could gain access to these benefits by accumulating at least 365 days of social insurance coverage during two years preceding childbirth. Both the baby-care allowance and childcare benefit provides 70% of the average pre-birth wage, but the maximum amount of childcare benefit is capped at 140% of the minimum wage. To reach this cap mothers needs to earn at least 200% of the minimum wage before giving birth. Because of the capped benefit, the replacement rate for higher-earning mothers is lower than 70%. Meanwhile, childcare allowance is a fixed allowance that is available regardless of labor market history. The amount of childcare allowance equals the minimum old-age pension, irrespective of the pre-childbirth wage of the mother.

Table 2.1: Restrictions on Paid Work During Parental Leave

	Age of child			
	0-6 months	7-12 months	13-24 months	25-36 months
2007-2013	No paid work allowed	No paid work allowed	No paid work allowed	At most 30 hours per week, no restrictions on paid work at home
2014-2015	No paid work allowed	No paid work allowed	No restrictions	No restrictions
2016-2017	No paid work allowed	No restrictions	No restrictions	No restrictions

Notes: We present the restrictions on paid work for mothers eligible for parental leave benefit and childcare benefit. The new regulations from 2014 and 2016 applied also to children born before 2014 and 2016, respectively.

Act LXXXIV of 1998 established restrictions on doing paid work while receiving maternity benefit; we provide an overview of the regulations on paid work during parental leave in Table 2.1. Until the CB+W ("childcare benefit plus wage") reform in 2014, mothers had to forgo their childcare benefit if they decided to return to work before the 2nd birthday of their

youngest children. Similarly, mothers could only keep their childcare allowance if they did not work more than 30 hours per week. These work restrictions acted as a disincentive for mothers to return to work, which caused the sudden jumps in maternal employment right at the running out of childcare benefit (at 24 months) and childcare allowance (at 36 months).

Regarding job protection, mothers enjoy strong job and wage protection by the Hungarian Labor Code (Act I of 2012); the regulation of mothers' job protection did not change during our study period. It is a common belief among both employers and employees that courts greatly favor returning mothers in case of job protection or wage disputes. Starting from the 12th week of pregnancy, women can announce officially their pregnancy to their employers, from which point employers cannot lay them off. Mothers are entitled to unpaid and fully protected leave between the birth and 3rd birthday of their youngest child, during which time they accumulate vacation days. During maternity and parental leave, mothers can return to work at any time they would like to. Within 30 days, employers have to offer them a similar position they held before giving birth, even if they previously hired a replacement for mothers' pre-birth occupation. Compared to their pre-birth wage, returning mothers are entitled to a similar wage raise that other employees in similar roles received during the parental leave. Upon request, employers also need to accommodate mothers who would like to work part-time until the 3rd birthday of their youngest child.

Returning mothers' contracts can only be terminated for cause until the 3rd birthday of their youngest child, which provides another level of job protection. The only uncertainty returning mothers could face arises if their pre-birth role is ceased because of a reorganization, in which case no employee works at their pre-birth occupation. Only in this specific situation can employers terminate returning mothers' contracts. However, in this situation, mothers could decide to stay on paid parental leave until the 3rd birthday of their child instead of returning to an employer that ceased their previous role. Meanwhile, because of potential disputes in courts, employers are also motivated to avoid this situation by communicating clearly the lack of positions for returning mothers, or by offering similar positions mothers hold pre-birth.

Regarding mothers' opportunities to leave their children in facilities upon return to work, their children are eligible for attending early child care centers ('nurseries'), which are state-funded and run by local governments. Nearly 14% of children under age 3 attended one of these centers in 2013, which is lower than the European average of 29% (Makay, 2015). The tuition is usually progressive and heavily subsidized in these centers: most families only need to cover the dining cost of their children (around 10% of monthly minimum wage), while even for the highest earning families the tuition is no more than 15-20% of the monthly minimum wage (Makay, 2012). In theory, children can start early child care education at

any time of the year, but because of the excess demand for early child care, most spots open up in September, when elder children start kindergarten. Families need to apply for these spots between March and May, decisions are made during May and June, usually just before the 3-6 week-long summer break. Due to the cyclical nature of new openings, many children start their early child care education in September or October, making it more likely that mothers will return to work in the second half of the calendar year.

2.2.2 The Childcare Benefit Plus Wage ($CB + W$) Reform

Starting with January 1, 2014, the Hungarian government implemented a childcare benefit reform ("GYED-extra" reform, in Hungarian), which is governed by the Act CCXXIV of 2013. The goal of the reform was to increase the number of births and help mothers returning to work from maternity leave by easing some restrictions of the previous parental leave system. The reform had the following four main pillars:

I. Decreasing work restrictions during maternity leave: The reform abolished all work restrictions after the first birthday of the child, independently from the year the child was born. This change made the biggest impact on mothers receiving childcare benefits, who could return to paid work one year after giving birth without forgoing their benefits. Similarly, this pillar allowed mothers to receive childcare allowance and work at full-time jobs at the same time. In 2016 the government lowered the age threshold from 12 to 6 months.

II. Sibling benefits/allowance: For children born on/after January 1, 2014, families receive childcare benefit/allowance for all their children below the age of 3. Before the reform, the birth of a younger child crowded out benefits of older siblings,[3] and this change intended to motivate families to decrease the gap between childbirths by terminating the previously prevailing financial disincentives.

III. Twin childcare benefits: Eligible mothers with twins born on or after January 1, 2014, could have received childcare benefits for 3 instead of 2 years.

IV. Childcare benefits for college and graduate students: Active students with at least one year of college education became eligible for receiving childcare benefits if their children

[3]We can illustrate the policy change through an example of a mother who receives childcare benefits after the birth of her first child. If this mother gives birth to her second child when her firstborn is 18 months old, then both before and after the reform she would have received parental leave benefit. But, before the reform, she would have lost the childcare benefit of his first children, because the higher parental leave benefit of the first child crowded out the childcare benefit of the second child. As the reform abolished this crowd-out effect, after the reform, this mother would have received both the parental leave benefit and the childcare benefit as well.

are born on or after January 1, 2014. Before the reform, they received only the lower childcare allowance.

The reform was unexpected and sudden. The raw details of the reform were first communicated following a cabinet meeting of the Hungarian Government on November 7, 2013, and the draft bill was published on November 18. The Hungarian National Assembly voted on the draft on December 9, and the final version of Act CCXXIV of 2013 was published on December 18, 2013. Finally, the changes the act introduced started with January 1, 2014, less than a month after it was voted on.

Because of the short time window between the introduction and implementation of the reform, the policy change was truly unexpected for families. This is especially true for mothers giving birth just before the January 1, 2014 cutoff, which made them ineligible for Pillars II-IV of the reform. The quick creation and implementation of the reform meant that women with small children at early 2014 had no knowledge of the reform during their pregnancy, and the reform could not affect their timing of having children.

However, Pillar II of the reform could have affected the future fertility decisions of these mothers. By eliminating the crowding out of current maternity leave benefits, Pillar II could have motivated mothers to decrease the time between subsequent births. In this way Pillar II could have acted as a disincentive for mothers to return to earlierwork, which was one of the main aims of the reform's Pillar I. As later we show, overall the reform increased maternal employment during parental leave, which suggests that Pillar I's financial incentives for returning to work exceeded the disincentives Pillar II created. Meanwhile, we did not find evidence that the reform affected mothers' future fertility, which suggests that Pillar II's effect on timing of future births was moderate.

In this paper, we focus on Pillar I of the Act CCXXIV of 2013, which we refer to as CB+W ("childcare benefit plus wage") – the abolishment of work restrictions after the first birthday of the child. Hungary is one of the few OECD countries (among Czechia, Latvia, and Slovakia) which allows parents to keep all parental leave benefits in case of returning to work, while Estonia cap benefits if wage income exceeds 50% of the parental leave benefits (Bičáková and Kalíšková, 2022; OECD, 2024). Some other OECD countries (France and Slovenia) allow parents to work part-time during parental leave and receive time-adjusted parental leave benefits, while others (Belgium, Germany, and Sweden) give more flexibility to parents to allocate their total available parental benefits in case of part-time employment. However, in these parental leave systems, the total income of parents during parental leave cannot exceed the pre-birth wage in case of returning to work, while in Hungary, the CB+W reform creates a temporary income jump, which could increase the pre-birth income by as much as 70%.

2.2.3 Financial Incentives of the CB+W Reform: A Numerical Example

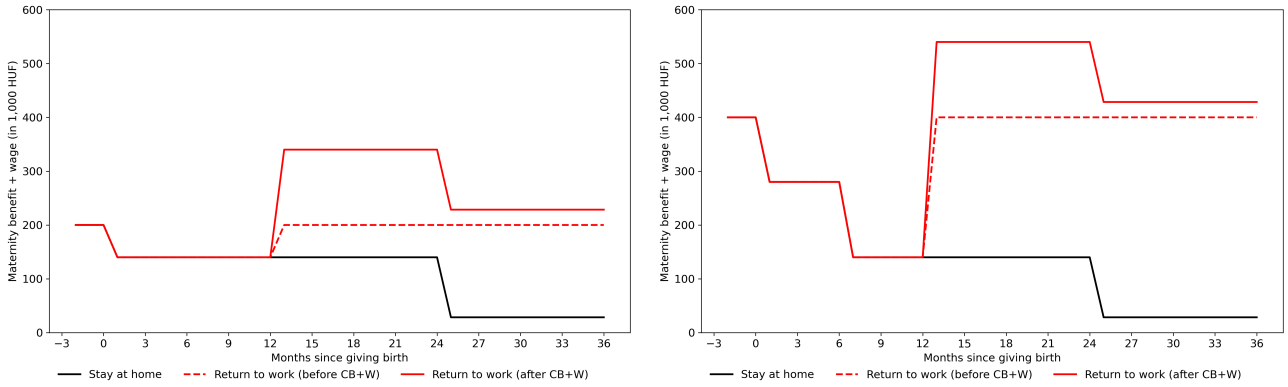
We illustrate the effect of the CB+W reform through a numerical example, which shows that the reform created stronger incentives for lower-income women to return to work before the child's 2nd birthday.

In both panels of Figure 2.1, we depict how the reform affected the income of a hypothetical childcare benefit-eligible mother's decision on whether to stay at home or return to work on the first birthday of her youngest child. In panel (a) the women's pre-birth salary is assumed to be twice the minimum wage, which is exactly the cutoff of the capped childcare benefit; her childcare benefit is 70% of her pre-birth wage. Meanwhile, in the right panel of Figure 2.1, our hypothetical mother's pre-birth wage income is assumed to be four times the minimum wage. This higher-income mother receives the maximum amount of childcare benefit (140% of the minimum wage), and her replacement rate is 35%.

Figure 2.1: The Effect of CB+W Reform on Mothers' Income

(a) Full-Time Salary: Below the Cap

(b) Full-Time Salary: Above the Cap



Notes: In this figure we illustrate how the reform affected a childcare benefit eligible women who needs to decide whether stay at home or return to work at the 1st birthday of her youngest child. In panel (a) this women's pre-birth income is twice the minimum wage, meaning that her replacement rate is 70%. In panel (b) the pre-birth income of the mother is four times the minimum wage, which is over the childcare benefit cutoff. She receives the maximum amount of childcare benefit (140% of minimum wage), and thus her replacement rate is 35%.

Compared to lower-income mothers, before the CB+W reform, higher-income mothers had stronger financial incentives to return earlier. Because of their higher replacement rate, lower-income mothers gained less additional income by an earlier return. In our numerical example, the lower-earning mother in panel (a) would gain 30% of her pre-birth income in case of returning work, which might be only slightly higher than the extra costs of working in a full-time position (such as childcare, transportation, etc.). In contrast, because of her

lower replacement rate, for the higher-income mother returning to work would increase her income by 65% of her pre-birth wage. Hence, relative to their pre-birth income, before the policy change higher-income mothers could gain more by early return to work.

The CB+W reform allowed mothers to keep both their benefits and wage income in case of returning to work before the child's 2nd birthday, which increased the opportunity cost of staying at home. Counter to the previous parental leave system, the CB+W reform created higher financial incentives to return to work for lower-income mothers. In our numerical example, following the reform, the lower-income mother could receive 170% of her pre-birth income until the 2nd birthday of her child by returning to full-time work. Thus, compared to pre-reform, the income gain of returning to work for this women increased from 30% to 100% measured in pre-birth income. In contrast, the higher-income mother in our example receives 135% of her pre-birth wage in case of returning to work after the CB+W reform, and for her the reform increased the income gain of returning to work from 65% to 100%. Thus, compared to their pre-birth income, the policy change created an opportunity for a temporary 70% income boost for the lower-income woman, which was only 35% for the higher-income mother in our example.

In sum, the CB+W reform created a strong financial incentive to return to work before the child's 2nd birthday, given mothers could keep the childcare benefit while working. Considering the income gain of returning to work after the reform relative to the gain before the reform, the reform provided incentives to return to work earlier, particularly for lower-earning mothers because of their high replacement rate.

2.3 Identifying the Substitution Effect of Paid Parental Leave

Paid parental leave decreases mothers' labor supply through two channels, that are rarely discussed in the context of parental leave:through the substitution effect and the income effect. Below we discuss both conceptually, and explain how our unique policy setting helps us to identify the substitution effect.

The substitution effect represents the distortionary effect on work incentives in that mothers lose the parental leave benefit if they start to work, and the loss of the benefit acts as an "implicit tax" on working. As a result, mothers decide not to work even if they would otherwise have positive labor supply at the income level provided by the parental leave benefit. The income effect, on the other hand, refers to the impact of an increase in non-wage income and decreases labor supply simply because at a higher level of income, mothers might prefer

staying at home with their child.

We illustrate both effects in Figure 2.2, using comparative statics of the labor supply decision under three scenarios in a standard neoclassical model of labor supply: (a) before giving birth (or after giving birth but without any parental leave benefit); (b) after giving birth under the pre-reform paid parental leave scheme; (c) the post-reform paid parental leave scheme that abolished return-to-work restrictions.

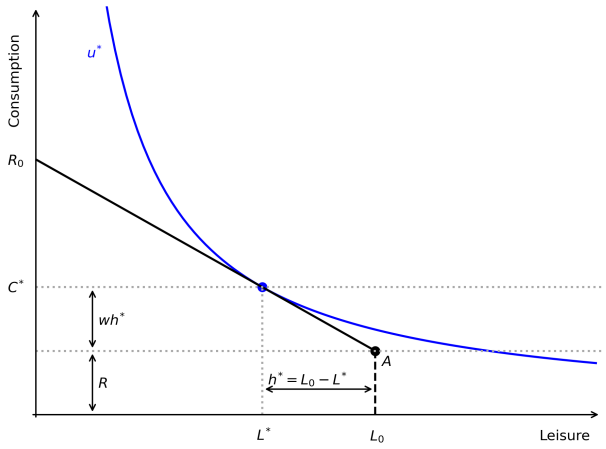
In panel (a), we show the labor supply decision of a woman before giving birth. She chooses the level of leisure (L) and consumption (C) that maximizes her utility, subject to the budget constraint. R_0 denotes her potential income, L_0 is the maximum amount of time to allocate between work and leisure, R is her non-wage income, and w is the hourly wage. At her optimum, she spends L^* hours on leisure, and thus works $h^* = L^0 - L^*$ hours, and consumes C^*. In what follows, we focus on mothers who have non-zero labor supply before giving birth, so that they are all eligible for paid parental leave.

In panel (b) of Figure 2.2, we show the budget constraint after giving birth in the pre-reform parental leave scheme: a mother can earn 70% of her previous wage—on top of her non-wage income, R—but only if she does not work at all. In the case we represent here, the mother stops working after giving birth so that she can keep the parental leave benefit. The first potential reason behind her decision to stop working is due to the income effect: at income level $R + 0.7wh^*$ without any work, her marginal rate of substitution at point B (MRS_B), or, equivalently, her reservation wage, may be higher than w. The second potential reason behind her decision to stop working is due to the substitution effect: even if she wanted to work for wage w on the labor market (i.e., her $w > MRS_B$) she does not do so, because moving from L_0 amount of leisure (0 hours of work) to any positive hours of work she would have to give up parental leave benefit in the pre-reform parental leave scheme.

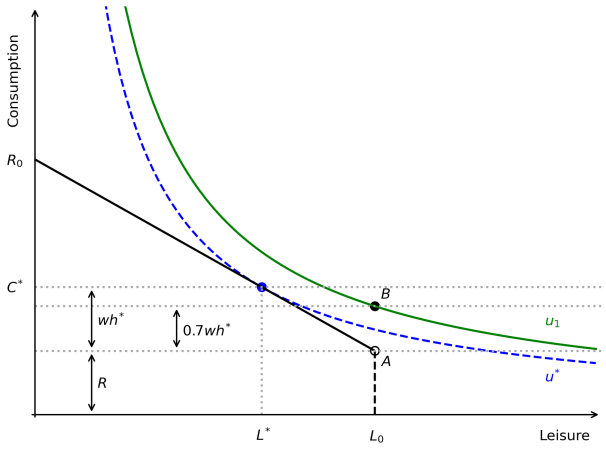
The relative importance of the two channels is policy-relevant: if the substitution effect drives the decrease in mothers' labor supply between scenarios a) and b), there is a deadweight loss (Autor and Duggan, 2007) in the paid parental leave scheme, distorting the incentives to work: mothers, who would otherwise prefer to work, will stay at home because they get paid for staying at home. On the other hand, the income effect captures what policy makers aim for with paid parental leave, that is to provide mothers with sufficient income to allow them to stay at home with their child while keeping their job.

Figure 2.2: The Labor Supply Decision of Mothers in Three Policy Scenarios

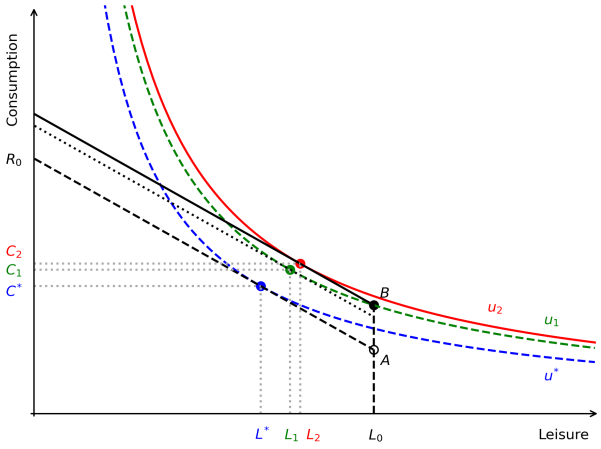
(a) Before Giving Birth / After Giving Birth Without Paid Leave



(b) On Parental Leave, Pre-reform



(c) On Parental Leave, Post-reform



Notes: This figure illustrates the labor supply decision of mothers in different states of the world in the standard neoclassical static model of labor supply. The figures on panel (b) and (c) show the labor market decision of mothers at a given month after giving birth.

In panel (c) of Figure 2.2, we illustrate what happens after the CB+W policy is introduced. In this case, those mothers whose non-participation in the labor market was driven by the second reason in the pre-reform phase, will decide to work due to a 'negative substitution effect', since the implicit tax on labor market participation is gone (for them point B in panel (c) is no longer optimal, the optimal choice is L_2 & C_2). At the same time, mothers who stayed out of the labor force due to the first reason, due to the income effect, will stay out even after the restriction on work while receiving the parental leave benefit is abolished.

(The income effect also affects the hours of work of mothers who switch to working after the reform, and it is represented by the difference between L_1 to L_2 on the figure).

Importantly, in Figure 2.2 we illustrate the labor supply decision of *complier* mothers, i.e., mothers who do not work after childbirth in the pre-reform setting but would work in the post-reform setting. In Table 2.2, we summarize the different types of mothers defined by their labor market participation decision in the different policy environments, and the income and substitution effect that is present for them, as a result of the CB+W reform. Never-workers decide not to work when offered childcare benefit irrespective of the restriction on working, thus for them there is no income and substitution effect. Compliers switch to work as a response to the CB+W reform that is due to the substitution effect, but they work somewhat fewer hours due to the income effect from the childcare benefit. Always-workers might reduce their working hours as a response to the reform, but they would never stop working, since that option with the same income was available for them even before the reform, it did not maximize their utility, and now the budget constraint just loosened compared to that. In sum, the participation decision and thus any change in the observed employment rate of mothers, is only due to the 'negative substitution effect' of the reform.

Table 2.2: Types of Mothers by Their Participation Decision in Different Policy Schemes

	Participation decision		
Policy	Never-worker	Complier	Always-worker
No CB	Yes	Yes	Yes
CB	No	No	Yes
CB+W	No	Yes	Yes
Income effect of CB+W	-	Decrease hours	Decrease hours
Substitution effect of CB+W	-	Switches to work	-

In the analysis of the concept of the two effects we assumed away some important features that determine the participation decision of mothers in reality, however these simplifications do not alter the main message from our illustration. First, we abstract from the fact that having children might lead to different preferences regarding work, leisure and consumption, thus the labor market decision before giving birth and after giving birth without childcare benefit is not necessarily the same. Second, we do not incorporate the cost of childcare (institutional childcare or private nanny) if a mother decides to work that might have an effect on the budget constraint. Third, paid parental leave comes with job protection. Job protection, just like any other dynamic feature of the labor market decision of mothers, e.g.,

the role of experience and or gaps in employment, cannot be incorporated in the static labor supply model.

2.4 Data

2.4.1 Data Source

Our main dataset is an employer-employee linked panel dataset, covering 50 percent of the Hungarian population with a social security number in 2003, which, besides information on labor outcomes, also contains administrative data on medical records and drug expenditures, for years 2009–2017.[4]

The employment-related data, containing the identity of the employer (firm), the type of employment, wage, occupation, and working hours, is provided by the *Hungarian Central Administration of National Pension Insurance*. It contains all sources of income liable for paying social security contribution and all employment counted in the length of service as a base for pension. Information on transfers is from the *Hungarian Central Administration of National Pension Insurance* and the *Hungarian National Health Insurance Fund Administration* for maternal benefit, disability benefit and pension, and from the *Hungarian Ministry of Finance* for unemployment benefit.

Data on inpatient and outpatient episodes, and spending on prescribed medication is available from 2009, from the *Hungarian National Health Insurance Fund Administration*. Births and miscarriages can be identified using International Classification of Diseases (ICD) codes O80-O84 and O00-O08, respectively.

For each individual we observe gender, age, amount of unemployment and child-related and pension benefits, number of days in hospital by month broken down by the cause of hospitalization (births, miscarriages, *etc.*), number of prescriptions by month, and monthly amount spent by drug category (antibiotics, antidepressants, *etc.*). To the extent the individual works in that month, we also observe monthly wage income, monthly hours worked, occupation, industry, and firm identifier.

On the firms' side, the data includes financial data, employment, industry, and foreign ownership data from the *Hungarian National Tax and Customs Administration*, for all double-entry bookkeeping firms in Hungary, reported by the firms annually (in balance

[4]The administrative database used in this paper is a property of the National Health Insurance Fund Administration, the Central Administration of National Pension Insurance, the National Tax and Customs Administration, the National Employment Service, and the Educational Authority of Hungary. The data was processed and provided by the Databank of the HUN-REN Centre for Economic and Regional Studies in Hungary.

sheets, and profit and loss statements).

Finally, we link the Occupation Information Network Database (O*NET) to our data, which describes the task content of occupations (*see* O*NET Resource Center).

2.4.2 Samples: Birth Sample and Control Sample (Women Never Giving Birth)

To construct our *birth sample*, we follow the steps below:

1. We identify women with birth ($N = 262,168$), and their month of *first birth* in $2009 - 2017$.

2. To ensure that we observe women who give birth for the 1st time, we keep mothers in the sample who did not receive any parental benefits preceding the observed *first birth* ($N = 166,873$).

3. We keep mothers who were $25 - 40$-years-old at the time of birth ($N = 124,737$).

4. To see the effect of the CB+W policy change introduced in 2014 January, we select two cohorts, based on when their child was between 13 and 24 months old:

   (a) CB+W-eligible for $7 - 12$ months (*i.e.*, for most of the child's 2nd year of age): mothers whose child turns 13 months old in the time frame 07/2013–12/2013;

   (b) CB+W-ineligible: mothers whose child turns 13 months old in 07/2012–12/2012.

   We select these two cohorts to ensure that the CB+W-eligible and CB+W-ineligible cohorts are affected by similar aggregate shocks (*i.e.*, the time of their childbirth is close to each other), and give birth in the same season (therefore we have the six-month gap between the cohorts). ($N = 14,626$)

5. We keep mothers who were eligible for parental leave benefit, *i.e.*, being employed for at least 12 months in the two years preceding the month of birth. Since there are measurement errors in the employment indicator during pregnancy, we restrict the sample to women whom we observe to receive childcare benefit at least for a month before the child turns two years old ($N = 11,667$).

For the control sample, unaffected by the reform, we select women who never had a child or a diagnosis related to pregnancy, for whom the event date is chosen randomly in a way that the age distribution of the control group at the event date is the same as that of women in the birth sample ($N = 10,138$).

2.4.3 Variable Definitions

Employment. Employment (or working) is defined as, for a given month, having non-zero wage income. We exclude employment under the public work scheme (which is essentially a substitute for unemployment in Hungary), and those months when a women is on maternity leave at least for a day.

Hourly wage. We measure (the logarithm of) deflated hourly wage, for a given month, as:
$\ln(w_{hour}) = \ln\left(\frac{\text{wage income}_m}{\frac{\text{days in month}}{7}\text{working hours}_w}/\text{defl}_y\right)$, where wage income_m contains all the monthly income which are used to calculate social security contributions. For a given month m, working hours_w contain the weekly working hours, and defl_y is the yearly deflator (year-specific average wage of women in the control group divided by the 2014 average wage of women in the control group). We set the value of the hourly wage to missing if the individual is not employed (as per our definition) in a given month.

One data issue with the wage variables needs to be accounted for: after 2014 at workplaces that are eligible to pay out the parental leave benefit (firms with at least 100 employees and smaller firms under special conditions) and childcare benefit (benefits payable until the child is two years old), the amount of such benefits may be recorded as part of the wage income. To account for this issue, we do not use the wage income variable for mothers who are less than two years after giving birth.

Estimated AKM Firm Effect as a Measure of Firm Quality. To capture firm quality, which we approximate with the extent to which a given firm is able to pay higher wages, we estimate firm-specific wage premiums, following the tradition of Abowd, Kramarz and Margolis (AKM, Abowd, Kramarz, and Margolis, 1999).

Consider the equation for worker i at firm j at time t:

$$\ln w_{ijt} = X'_{ijt}\beta + \vartheta_i + \phi_j + \varepsilon_{ijt}, \tag{2.1}$$

where $\ln w_{ijt}$ is the logarithmic wage of worker i at firm j at time t, X_{ijt} is a vector of time-varying observable characteristics with a $k \times 1$ vector of returns β, ϑ_i is time-invariant worker ability (or worker type), ϕ_j is the time-invariant firm-specific wage premium, and ε_{ijt} is the time-varying error term.

To get an estimate for a time-invariant firm quality measure, capturing the wage premium, ϕ_j, we estimate the model in (2.1) using the entire sample of the linked employer-employee data, for years 2003-2017. Following Card, Heining, and Kline (2013); Card, Cardoso, and

Kline (2016); Card et al. (2018), we include the quadratic and cubic age, and year dummies, in X (but no firm-level variables),[5] and the vector of $\hat{\phi}$ is the "estimated AKM firm (fixed) effect". Then, for each year, on the entire sample, we flag firms that have above-median $\hat{\phi}$, and form the deciles, too.

Occupation Characteristics. For this,, we use the O*NET data. We build on Hardy, Keister, and Lewandowski (2018) in assigning O*NET (US-specific) SOC-based occupation categories to European ISCO (International Standard Classification of Occupations) classification and to Hungarian occupation codes ("FEOR" codes). Based on the 2011 February O*NET edition, we create six variables, based on to what extent: (1) the job is achievement-oriented (as in: it requires establishing and maintaining personally challenging achievement goals and exerting effort toward mastering tasks, it requires persistence in the face of obstacles, and it requires a willingness to take on responsibilities and challenges); (2) the job requires leadership and stress tolerance; (3) the job involves leadership and taking responsibility for outcomes; (4) the job involves time pressure; (5) the job requires analytical thinking; and (6) the job's schedule is regular.

2.5 Empirical Strategy

2.5.1 Effect of the Reform on Employment and Fertility

To build intuition for our preferred specification, we begin with a difference-in-differences model that compares the outcome trajectories of CB+W-eligible and CB+W-ineligible women over time. We estimate model (2.2) separately on the sample of mothers and of women never giving birth.

$$\Omega_{it} = \tilde{\alpha} + \sum_k \tilde{\alpha_k} \mathbb{1}[e_{it} = k] + \sum_k \tilde{\beta_k} \mathbb{1}[e_{it} = k] \times D_i + \tilde{\nu_{it}}^{age} + \tilde{\mu_i} + \tilde{\varepsilon_{it}}, \tag{2.2}$$

where Ω_{it} is the outcome variable for individual i at monthly date t, e_{it} denotes the months relative to the event, and $\mathbb{1}[e_{it} = k]$ is a set of indicators for i being observed k periods after the event (where negative k's refer to pre-event periods). D_i is a binary indicator which equals one if a women is CB+W-eligible and zero if she is CB+W-ineligible. $\tilde{\nu_{it}}^{age}$

[5] Card, Heining, and Kline (2013); Card, Cardoso, and Kline (2016); Card et al. (2018) all also include the interaction of these variables with education dummies, but due to data limitations, we are unable to do that.

denotes age effects, $\tilde{\mu_i}$ denotes individual fixed effects. The coefficients of interest are the $\tilde{\beta}_k$ parameters. We make the normalization $\sum_{k=-36}^{-13} \tilde{\beta}_k = 0$.

In model (2.2), the estimated effect of CB+W-eligibility may be confounded with time trends. Because of this concern, our preferred baseline specification is the Event Study regression model (2.3), comparing the outcome trajectories of CB+W-eligible and CB+W-ineligible mothers over time, and including women never giving birth as control group. The Event Study regression model is the following:

$$\begin{aligned}\Omega_{it} = \alpha + \sum_k \alpha_k \mathbb{1}[e_{it} = k] + \sum_k \beta_k \mathbb{1}[e_{it} = k] \times D_i + \\ + \sum_k \gamma_k \mathbb{1}[e_{it} = k] \times B_i + \sum_k \delta_k \mathbb{1}[e_{it} = k] \times B_i \times D_i + \nu_{it}^{age} \times B_i + \mu_i + \varepsilon_{it}, \end{aligned} \tag{2.3}$$

where we use the same notation as in equation (2.2). B_i is a binary indicator of ever giving birth. $\nu_{it}^{age} \times B_i$ denotes age effects interacted with the binary indicator of ever giving birth, μ_i denotes individual fixed effects. The coefficients of interest are the δ_k parameters. We make the normalization $\sum_{k=-36}^{-13} \delta_k = 0$.

We also estimate the average effect of the reform on employment over 19-24 months after childbirth:

$$\begin{aligned}\Omega_{it} = \alpha + \alpha_1 \,\text{afterM19-24}_{it} + \beta \,\text{afterM19-24}_{it} \times D_i + \gamma \,\text{afterM19-24}_{it} \times B_i + \\ \delta \,\text{afterM19-24}_{it} \times B_i \times D_i + \nu_{it}^{age} \times B_i + \mu_i + \varepsilon_{it}, \end{aligned} \tag{2.4}$$

where the afterM19-24_{it} dummy variable equals zero in the 3rd and 2nd years before childbirth, and equals one 19-24 months after childbirth. The coefficient of interest is the δ parameter.

In the model for fertility (equation (2.5)), we do not include the control group (for whom by definition the outcome is zero) and replace the individual fixed effects with a vector of control variables of age, age squared, log of average wage 7-24 months before childbirth, deciles of average AKM firm effect 7-24 months before childbirth (missing values as a separate category), mode of one-digit occupation 7-24 months before childbirth (missing values as a separate category), county of living 12 months before childbirth. Here we do not include individual fixed effects because the outcome variable has little time variation, without any time variation for those mothers who did not have a second birth in our observation period.

$$\Omega_{it} = \lambda + \sum_k \lambda_k \mathbb{1}[e_{it} = k] + \sum_k \theta_k \mathbb{1}[e_{it} = k] \times D_i + X_{it}'\eta + \epsilon_{it}. \tag{2.5}$$

In this specification, we restrict the sample to months 6 to 60 after the birth of the first child. The outcome variable is the binary indicator of giving birth in the given month. The coefficients of interest are the θ_k parameters. We make the normalization $\theta_6 = 0$.

2.5.2 Effect of the Reform on Job Quality

To investigate the effect of CB+W-eligibility on job quality, we focus only on the 3rd, 4th and 5th years after giving birth, in years in which we estimate no employment effects anymore. By definition, job quality is observed only for the employed, and to avoid selection bias, we do not estimate the effect of CB+W-eligibility on job quality in the first two years after childbirth, when we show that CB+W-eligibility affects employment. As job quality indicators, we use the logarithm of hourly wage, estimated AKM firm effect, firm size deciles, employment in a white-collar job, and O*NET occupation scores. We also estimate the effect of CB+W-eligibility on working hours.

We estimate equation (2.3) with the job quality indicators as outcome variable. We restrict the sample to event times -36 to -12 and 25 to 60. We also estimate the average effect of the reform on job quality indicators and working hours in the 3rd, 4th, and 5th year after childbirth, by using an analogous version of equation (2.4), but replacing afterM19-24$_{it}$ with a vector of (afterY3$_{it}$, afterY4$_{it}$, afterY5$_{it}$), denoting the 3rd, 4th, and 5th year after childbirth, respectively.

2.5.3 Robustness: Event Study Research Design on Matched Sample

There exists an imbalance in the pre-birth (or pre-pseudo birth) employment rate of women ever giving birth ($B_i = 1$) and not giving birth ($B_i = 0$) (Panel (a) of Figure 2.3). To verify that our estimates are not affected by the baseline imbalance—a level difference—we apply a matching strategy described below.

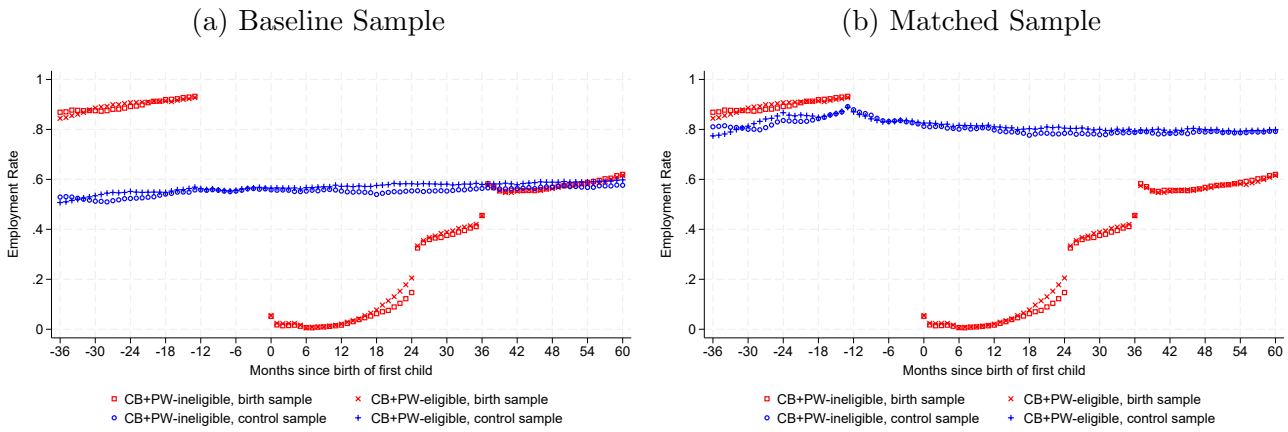
We create a matched sample that includes all women of the birth sample ($B_i = 1$). We create cells based on the age at birth; employment at 13, 24 and 36 months before birth; a binary indicator of the mode of employment 13-36 months before birth is in a white collar job; and the binary indicator of $CB + W$-eligibility. In each cell, we ensure that we have at most as many women never giving birth as women in the birth sample, selecting women in the $B_i = 0$ group randomly if their number exceeds the number of $B_i = 1$ women. This is our matched sample that avoids the over-representation of those $B_i = 0$ women who have very different labor market history than the $B_i = 1$ women.

2.6 Results

2.6.1 Effects on Employment

Panels (a)–(b) of Figure 2.3 show the evolution of the employment rate for the 4 relevant groups in 2 dimensions (eligible / ineligible women; giving birth / not giving birth) in the baseline and matched samples, respectively. The group of women who do not give birth we define as the control group, for whom the time of birth – that also determines event time – has been randomly drawn so that they can be also classified to be in the eligible *vs.* ineligible group. As a reminder, mothers in the eligible cohort gave birth in the same part of the year as mothers in the ineligible cohort, only a year later, thus, these two cohorts of mothers very likely were affected by the same aggregate-level shocks.

Figure 2.3: Employment Rate Over Event Time

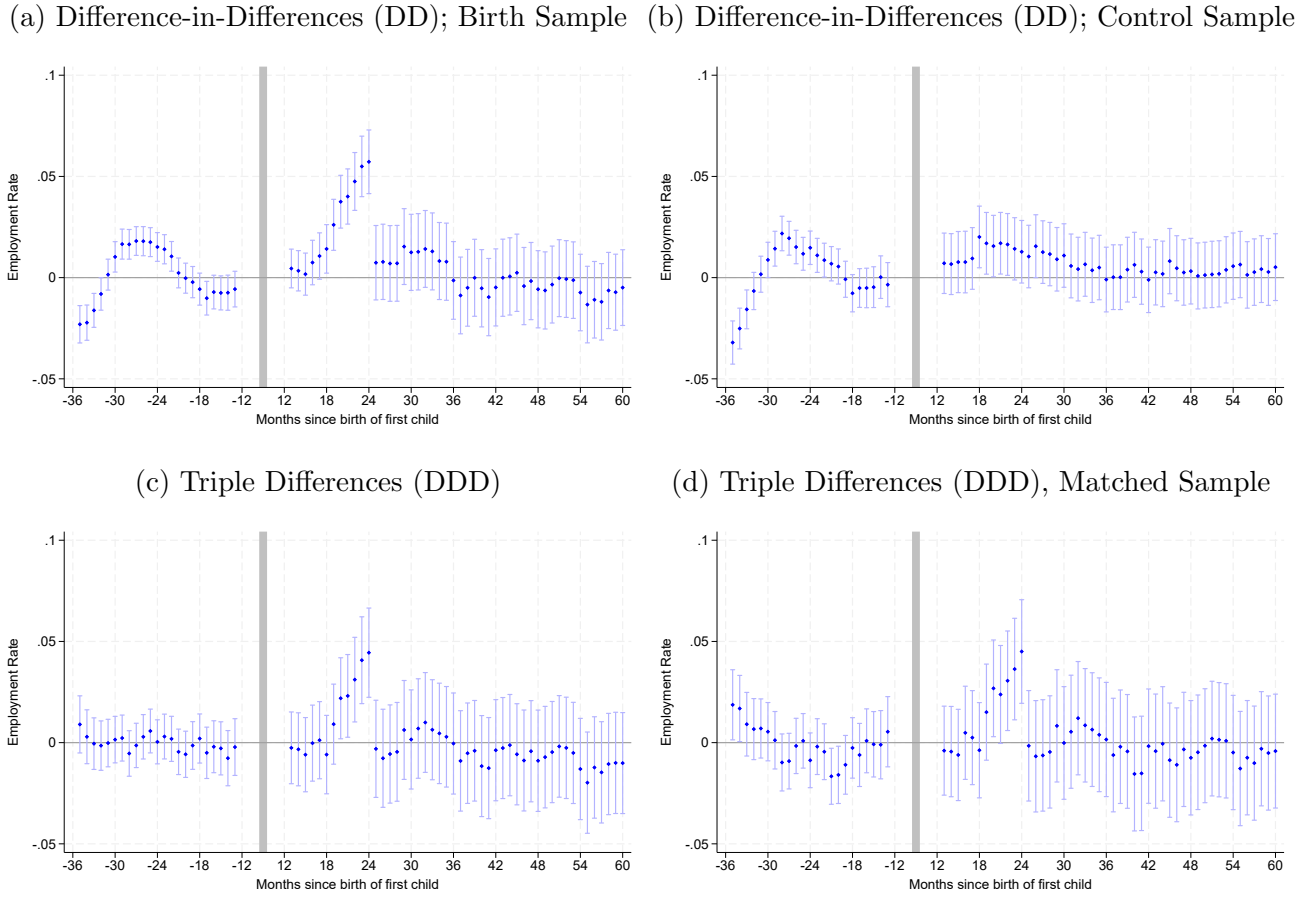


Notes: Sample is as described in Section 2.4.2. The "childbirth" sample consists of women whom we observe giving birth. The "control group" consists of women whom we do not observe giving birth.

Figure 2.3 reveals three interesting facts. First, the employment rate slowly and steadily increases over time in the control group. Second, mothers have higher employment rate before birth than women who do not give birth, which difference, reassuringly, almost disappears in the matched sample. Third, Figure 2.3 also shows that for women giving birth the employment rate starts to increase again only 12 months after giving birth, with discontinuous jumps at the 24 and 36 months mark in their children's age. The gap between the eligible–ineligible mothers widens exactly between 18 and 24 months of their children's age: eligible mothers' employment rate is higher exactly in the 2nd half of the child's 2nd year of age, when return to work was strongly incentivized by the analyzed policy.

Figure 2.4 shows the Event Study coefficient estimates for the birth sample and the control

Figure 2.4: Effect of CB+W-Eligibility on Employment



Notes: Sample is as described in Section 2.4.2. This figure shows the Event Study coefficient estimates for the birth sample and the control sample separately (in panels (a) and (b), respectively), so that their difference is in panel (c). In (a) and (b) we show the estimated $\tilde{\beta}_k$'s from model (2.2). In (a), the sample is women giving birth; in (b), the sample is the control group (women never giving birth). Panel (c) shows the estimated δ_k coefficients from equation (2.3) on the baseline sample, and panel (d) shows the estimated δ_k coefficients from equation (2.3) on the matched sample. Bars are 95 percent confidence intervals based on robust standard errors.

sample separately (in panels (a) and (b), respectively), so that the differences between the estimated coefficients at each event time can be seen in panel (c). Specifically, in panels (a) and (b) we show the estimated $\tilde{\beta}_k$'s from model (2.2). In panel (a), the sample is restricted to women giving birth; in panel (b), the sample is restricted to the control group (women never giving birth).[6] Panel (c) shows the estimated δ_k coefficients from equation (2.3) on the baseline sample, and panel (d) shows the same on the matched sample.

[6]The pre-trend in the second and third year before birth is due to aggregate labor market trends. For example, the CB+W-ineligible group is observed in 2008 at event time -36 and in 2009 at event time -24, between which periods the employment rate decreased in Hungary. On the other hand, the CB+W-eligible group is observed in 2009 at event time -36 and in 2010 at event time -24, between which periods the employment rate levelled off in Hungary.

We find that eligibility to the $\underline{CB + W}$ policy increased the propensity of being employed by 0.9 percentage point in the 19th month after giving birth, and this increase is 2.2, 2.3, 3.1, 4.1, and 4.4 percentage points in months 20–24, respectively. As panel (d) reveals, these results are robust to the matching which takes care of the baseline imbalance. Reassuringly, there are no pre-trends in any of the months prior to the event in either panels (c) or (d), thus the use of a control group – women without giving birth – is helpful to difference out the pre-trends that can be seen in panel (a).

Table 2.3: Heterogeneity in the Effect of the CB+W Policy on Employment

		Full Sample	Matched Sample	*Baseline Mean*
All	afterM19-24 × B × D	0.028***	0.030***	*0.101*
		[0.010]	[0.012]	
By Age at Childbirth				
Age 25-30	afterM19-24 × B × D	0.013	0.005	*0.093*
		[0.014]	[0.017]	
Age 31+	afterM19-24 × B × D	0.044***	0.056***	*0.109*
		[0.013]	[0.016]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.110*	*0.028*	
By Location				
Rural	afterM19-24 × B × D	0.027**	0.030**	*0.102*
		[0.011]	[0.013]	
Capital	afterM19-24 × B × D	0.033	0.035	*0.098*
		[0.022]	[0.026]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.830*	*0.842*	
By Baseline Occupation				
Blue-Collar	afterM19-24 × B × D	0.018	0.027	*0.057*
		[0.015]	[0.021]	
White-Collar	afterM19-24 × B × D	0.025*	0.030**	*0.121*
		[0.014]	[0.014]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.750*	*0.898*	
By Baseline Wage				
Below Median	afterM19-24 × B × D	0.011	0.018	*0.058*
		[0.018]	[0.019]	
Above Median	afterM19-24 × B × D	0.034**	0.037**	*0.141*
		[0.015]	[0.015]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.326*	*0.419*	
By Baseline AKM Firm FE				
Below Median	afterM19-24 × B × D	0.030*	0.045**	*0.079*
		[0.017]	[0.018]	
Above Median	afterM19-24 × B × D	0.020	0.019	*0.125*
		[0.016]	[0.016]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.679*	*0.286*	

Notes: Table shows average effect of the CB+W policy on employment probability 19-24 months after childbirth, using the 3rd and 2nd years preceding childbirth as reference period (equation (2.4)). In the models with heterogeneity terms, all regressors are interacted with the heterogeneity term. Occupation category refers to the mode of occupation 13-36 months prior childbirth. Baseline wage and AKM firm FE refer to 13-24 months prior childbirth. Robust standard errors in brackets. Number of individuals: 21,922. Number of observations: 657,619. *** denotes significant at the 1 percent level, and ** denotes significant at the 5 percent level.

In Table 2.3 we investigate the heterogeneity of the CB+W policy's effect on employment by pre-birth characteristics. The top row indicates that on average, the CB+W policy increased the employment rate by 2.8 percentage points (3.0 percentage points in the matched sample, or about 30 percent) 19-24 months after childbirth. We find that the point estimates for the employment effects are higher for women who have their first child at age 31-40, and, in terms of the corresponding *p-values*, this is the strongest heterogeneity we find in the employment effects. We also see bigger employment effects for mothers whose baseline wage was above the median before childbirth, but also for mothers whose employer's AKM firm effect was below the median (the baseline wage and AKM firm effect refer to 13-24 months prior childbirth). Yet, the differences along these heterogeneity dimensions are not statistically significant. Therefore, our results do not confirm the expectations based on the changing relative financial incentives, that the reform would have had higher effect on lower-income mothers to return to work, than on higher-income mothers.

In sum, the effect of the policy on the propensity to work is driven – at least in terms of the point estimates – by women who are above 30 years old when giving birth, and with above-median pre-wage.

Figure 2.5 shows the composition of the mothers' workforce at different points in time, relative to the birth of their first child. Reassuringly, there are no major differences between the average wage 1 year before the birth of their first child of those working mothers who are in the labor force 36-to-12 months before the birth in the eligible and ineligible cohorts (panel (a)), and the same holds for their average age (panel (b)). (The gray crosses in months 12-to-24 after birth represent the averages for mothers who work in the child's 2nd year of age even if the financial incentives by the policy was not provided for them.)

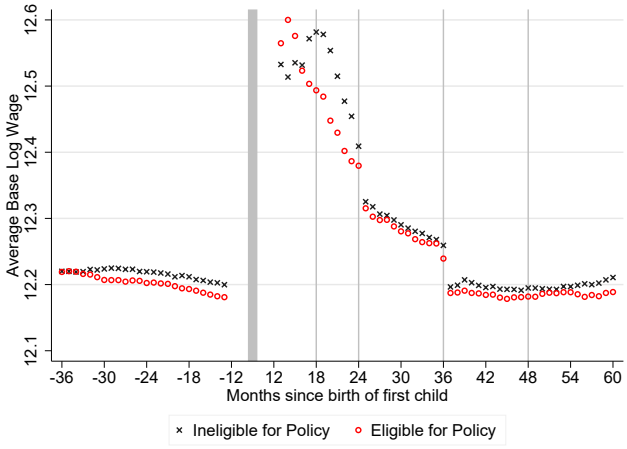
Consistent with previous results is that the policy did not affect mothers' propensity to work differently in the eligible *vs.* ineligible cohorts 3-5 years after the birth of the first child, we do not see any compositional differences in the workforce of the two cohorts 3-5 years after the birth of the first child either. Thus, the estimates for the policy effect on mothers' longer-term job quality outcomes 3-5 years after birth are not tainted by compositional differences in the workforce of the two—eligible *vs.* ineligible—cohorts. Before analyzing these, we assess the average characteristics of the compliers.

2.6.2 Characteristics of Complier Mothers Responding to the Policy

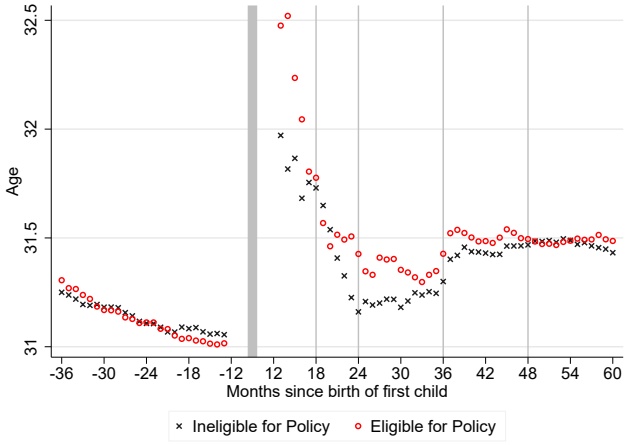
We follow the method proposed by Almond and Doyle (2011) to analyze the characteristics of compliers.

Figure 2.5: Composition of Mothers' Workforce Before and After Birth

(a) Average Pre-Birth Wage of Those Working



(b) Average Age at Birth of Those Working



Notes: Sample only contains women who gave birth, in the eligible and ineligible cohorts, as described in Section 2.4.2.

The endogenous variable we consider is returning to work in months 19–24 after giving birth (W). Thus, compliers in such setting are, by definition, mothers who work in months 19–24 after giving birth if they are eligible for the policy, but who would not work had they been ineligible for the policy. The average characteristics of the compliers can be estimated using the following formula:

$$E\left(X|W_1=1,W_0=0\right)=\frac{\pi_C+\pi_A}{\pi_C}\left[E\left(X|W=1,D=1\right)-\frac{\pi_A}{\pi_C+\pi_A}E\left(X|W=1,D=0\right)\right], \quad (2.6)$$

where $W_1=1$ denotes working 19–24 months after giving birth if being in the eligible cohort (eligible for the policy, $D=1$), $W_0=0$ denotes not working 19–24 months after giving birth if being in the ineligible cohort (ineligible for the policy, $D=0$), π_A is the share of always-takers and π_C is the share of compliers.

Using the independence assumption between D and W, and assuming away defiers by the monotonicity condition, the shares π_A (π_N) can be estimated by the empirical counterpart of $Prob(W_0=1)$ ($Prob(W_1=0)$); in our sample: $\hat{\pi}_A=0.154$ and $\hat{\pi}_N=0.781$. Thus, the estimated share of the compliers is $\hat{\pi}_C=0.065$.

Table 2.4 shows the estimated shares of compliers for various X's, and whether that share significantly differs for the sample. Consistent with results in Table 2.3, complier mothers are significantly older and have a higher pre-birth wage than mothers in the overall sample and, albeit the differences are not all significantly different from zero at the 5 percent level, are more likely to live in the capital of Budapest, are more likely to work in white-collar

occupations before giving birth, and their pre-birth firm has, on average, lower estimated AKM firm effect reflecting lower ability to pay higher wages.

Table 2.4: Average Characteristics of Compliers and of Overall Sample

average among/in:	compliers	sample	p-value of diff
in Budapest	0.270	0.193	0.094
white collar	0.777	0.666	0.023
age-at-birth (years)	31.160	30.295	0.076
base wage (logs)	12.247	12.161	0.260
base AKM firm FE	-0.025	0.049	0.052

2.6.3 Effects on Fertility

Next, we test whether the reform changed families' birth planning behavior: as we discussed in Section 2.2.2, the CB+W and Pillar II of the reform have opposite effect of the timing of subsequent births. By itself, the CB+W reform could have motivated mothers to delay future pregnancy to be able to return to work during parental leave. Meanwhile, by eliminating the crowd-out effect of maternity benefits, Pillar II of the reform incentivized families to reduce the time between births. In our sample, CB+W-eligible mothers (whose child turns 13 months old in the time frame 07/2013–12/2013) are not only eligible to keep their childcare benefits for additional 7-12 months in case of returning to work, but they could also receive maternity benefits for their first and second child for potentially a year longer than CB+W-ineligible mothers (whose child turns 13 months old in 07/2012–12/2012).

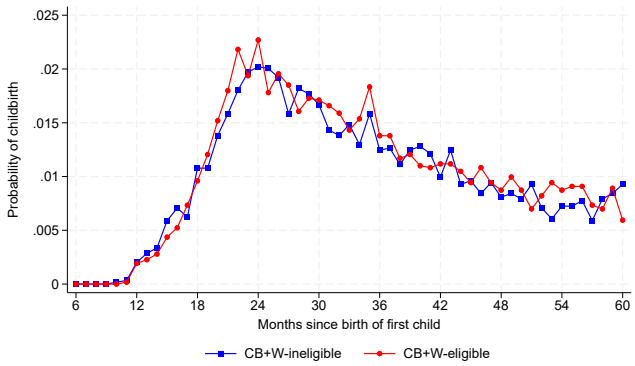
Despite these differences between the eligible and ineligible cohorts' incentives to have a 2nd child, panel (a) of Figure 2.6 suggests that there is no systematic difference in the timing of second birth between our chosen two cohorts of mothers. We test the statistical significance of these raw differences in fertility by estimating equation (2.5) on childbirth rate. Panel (b) of Figure 2.6 confirms that there are no statistically significant difference in future fertility between eligible and ineligible mothers.

Our findings on fertility suggest that the effect of Pillar II of the reform was moderate, as the financial incentives created by Pillar II was not able to offset the CB+W to decrease the time between subsequent births. Overall, the reform did not change families' fertility decisions, which also means that the positive effect on maternal employment we document does not have negative spillovers on fertility.

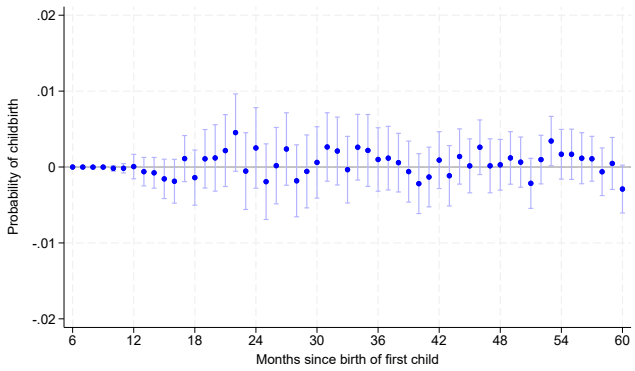
In sum, the policy not only did not affect mothers' propensity to work in the eligible *vs.* ineligible cohorts 3-5 years after the birth of the first child, it also did not affect mothers' subsequent fertility behavior. Thus, the estimates for the policy effect on mothers' labor

Figure 2.6: Effect of the CB+W Policy on Childbirth Rate (Further Child)

(a) Descriptive



(b) Difference-in-differences (DD); birth sample



Notes: Sample is as described in Section 2.4.2, excluding the control group (women never giving birth). Panel (b) shows estimated θ_k coefficients from equation (2.5). Bars are 95 percent confidence intervals based on robust standard errors.

market outcomes 3-5 years after birth are not tainted by compositional differences in the workforce or in motherhood of the two cohorts.

2.6.4 Job Characteristics

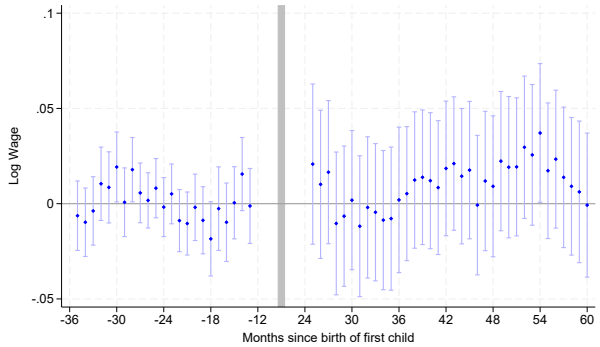
Now we turn to the analysis of job characteristics 3–5 years after giving birth, when there are no differences between the eligible and ineligible mothers in their employment and fertility behaviors, except one notable difference: mothers in eligible cohorts were by 3 percentage points more likely to work 19–24 months after giving birth, corresponding to a large 30 percent effect (and there are no further differences). We show the $\hat{\delta}$ estimates from equation (2.3) in Figure 2.7. Appendix Figure B.1 shows that these results are robust to restricting the estimation sample to the matched sample. We present descriptive time patterns and DiD estimates from equation (2.2) in Figures B.2–B.6. All presented estimates are conditional on working.

Panels (a), (c) and (d) of Figure 2.7 suggests small positive but statistically insignificant effect of CB+W-eligibility on hourly wage, firm size decile, and the probability of employment in a white-collar job. Panel (b) indicates that CB+W-eligibility increases the AKM firm FE of the employer by up to 0.02, corresponding to a 2 percent increase of the firm wage premium. Looking at working hours, panel (e) of Figure 2.7 shows that in the third year after childbirth, the weekly hours worked increase by up to 0.9 hour, and this effect is statistically significant. This increase is due to the direct impact of the CB+W reform, which eliminated

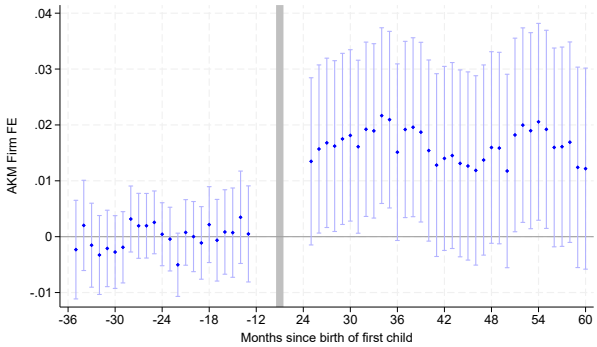
the 30-hour weekly working time restricting for the 3rd year of age of the child.[7]

Figure 2.7: Effect of CB+W-Eligibility on Job Characteristics

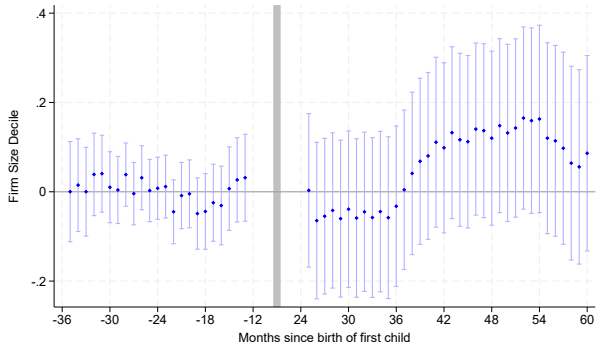
(a) Log Hourly Wage



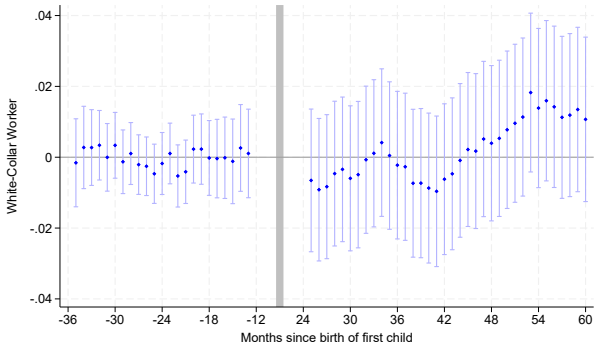
(b) AKM Firm FE



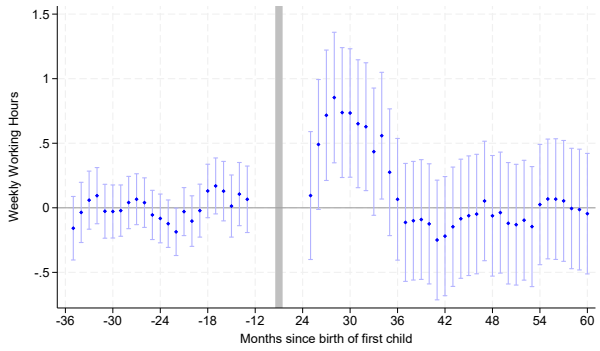
(c) Firm Size Decile



(d) Employment in White-Collar Job



(e) Weekly Hours of Work



Notes: Sample is the baseline sample as described in Section 2.4.2. This figure shows the Event Study coefficient estimates (δ_k coefficients from equation (2.3)). Bars are 95 percent confidence intervals based on robust standard errors.

[7]The reform component abolishing the 30-hour working restriction for PL benefit recipients explains this: some mothers of the ineligible group (whose child turns 13-months-old in 07/2012–12/2012) are eligible only for fewer month, while in the eligible group (whose child turns 13-months-old in 07/2013–12/2013) everyone is eligible for the full length in year 3.

Then, in the first panel of Table 2.5, we show the average effects of CB+W-eligibility in the 3rd, 4th and 5th year after giving birth, for all. The first column shows that wages are, on average, by 0.8 and 1.7 percent higher in the 4th and 5th year after giving birth, respectively, for the CB+W-eligible *vs.* non-eligible mothers *vs.* non-mothers, but these differences are not significant at the usual levels.

Subsequent columns of the first panel reveal that the estimated AKM firm effect – capturing the firm-specific wage premium – is significantly higher by 1.4, 1.3 and 1.6 percent in the 3rd, 4th and 5th year, respectively. Hours worked is significantly higher by 0.41 hours only in the 3rd year after giving birth, but other differences in firm size and being in a white-collar occupation are not significant at the usual levels.

Table 2.5: Heterogeneity in the Effect of the CB+W Policy on Job Characteristics

		log. of hourly wage	AKM firm FE	hours	decile of firm size	1: white collar
All	afterY3 × $B \times D$	-0.004	0.014**	0.407*	-0.03	-0.006
		[0.015]	[0.007]	[0.211]	[0.081]	[0.009]
	afterY4 × $B \times D$	0.008	0.013*	-0.100	0.076	-0.006
		[0.015]	[0.008]	[0.208]	[0.087]	[0.010]
	afterY5 × $B \times D$	0.017	0.016**	-0.058	0.098	0.008
		[0.016]	[0.008]	[0.212]	[0.094]	[0.011]
By Age at Childbirth						
Age 31+	afterY3 × $B \times D$	0.028	0.023**	0.237	0.092	-0.013
		[0.021]	[0.009]	[0.291]	[0.114]	[0.013]
	afterY4 × $B \times D$	0.03	0.024**	-0.134	0.252**	-0.007
		[0.021]	[0.010]	[0.285]	[0.121]	[0.014]
	afterY5 × $B \times D$	0.041*	0.028**	-0.076	0.315**	0.004
		[0.022]	[0.011]	[0.290]	[0.133]	[0.014]
By Location						
Budapest	afterY3 × $B \times D$	0.090***	0.045**	0.334	0.111	-0.018
		[0.035]	[0.018]	[0.511]	[0.175]	[0.018]
	afterY4 × $B \times D$	0.084**	0.043**	-0.551	0.383**	-0.025
		[0.036]	[0.020]	[0.517]	[0.188]	[0.020]
	afterY5 × $B \times D$	0.058	0.041*	-0.265	0.28	-0.011
		[0.039]	[0.022]	[0.524]	[0.204]	[0.021]
By Baseline Occupation						
White-Collar	afterY3 × $B \times D$	0.016	0.023***	0.471**	0.034	
		[0.016]	[0.009]	[0.224]	[0.107]	
	afterY4 × $B \times D$	0.021	0.019*	-0.103	0.178	
		[0.016]	[0.010]	[0.222]	[0.115]	
	afterY5 × $B \times D$	0.026	0.014	-0.093	0.166	
		[0.017]	[0.011]	[0.226]	[0.125]	
By Baseline Wage						
Above Median	afterY3 × $B \times D$		0.017*	0.450**	-0.019	0.009
			[0.009]	[0.221]	[0.099]	[0.010]
	afterY4 × $B \times D$		0.016*	0.111	0.052	0.013
			[0.009]	[0.217]	[0.106]	[0.011]
	afterY5 × $B \times D$		0.013	0.132	0.038	0.027**
			[0.010]	[0.223]	[0.116]	[0.012]
By Baseline AKM Firm FE						
Above Median	afterY3 × $B \times D$	0.006		0.691***	0.025	0.01
		[0.020]		[0.236]	[0.095]	[0.012]
	afterY4 × $B \times D$	0.03		0.158	0.066	0.005
		[0.021]		[0.234]	[0.102]	[0.013]
	afterY5 × $B \times D$	0.024		0.316	0.084	0.018
		[0.022]		[0.238]	[0.110]	[0.014]

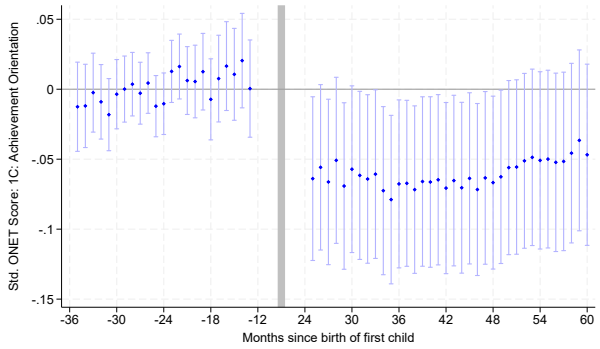
Notes: Table shows average effect of the CB+W policy on job characteristics in the 3rd–5th year after giving birth, using the 3rd and 2nd years preceding childbirth as reference period (equation (2.4)). In the models with heterogeneity terms, all regressors are interacted with the heterogeneity term. Occupation category refers to the mode of occupation 13-36 months prior childbirth. Baseline wage and AKM firm FE refer to 13-24 months prior childbirth. Robust standard errors in brackets. *** denotes significant at the 1 percent level, ** denotes significant at the 5 percent level, and * denotes significant at the 10 percent level. Outcomes are in the column titles.

Further panels reveal that the wage and AKM firm wage premium effects are driven by women who were relatively older (31 years old or older) at the time of the event, as well as those who live in Budapest. For instance, wages of mothers in Budapest are by 8.5–9 percent higher in the 3rd–4th year after giving birth, for the CB+W-eligible *vs.* non-eligible mothers *vs.* non-mothers, and these differences are significant. Similarly, the estimated AKM firm effects for those living in Budapest are by 4.1–4.5 percent higher, and they are also rather driven by white-collars and with above-median baseline wages. Wages of mothers who were at least 31 years old when giving birth are by 3–4 percent higher in the long-run.[8]

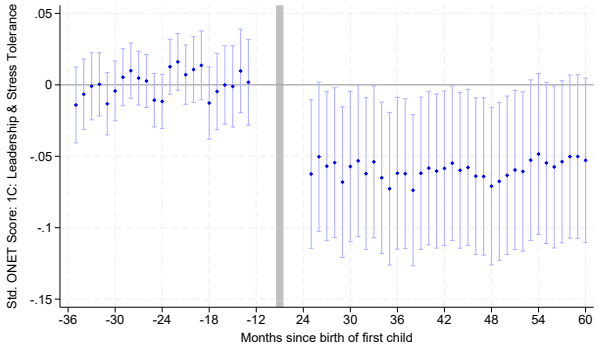
[8]Note, that in the bottom three panels we do not report estimates when the heterogeneity indicator is based on the analyzed outcome variable.

Figure 2.8: Effect of CB+W-Eligibility on Occupation Characteristics

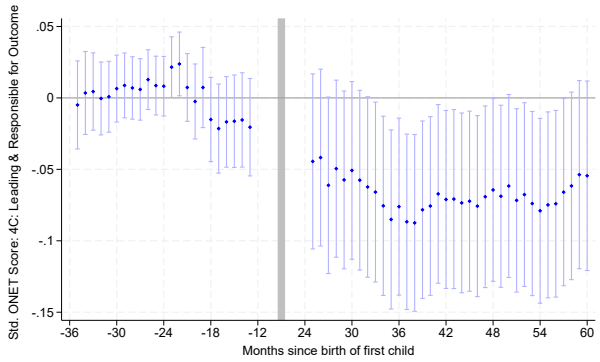
(a) Job is Achievement-Oriented



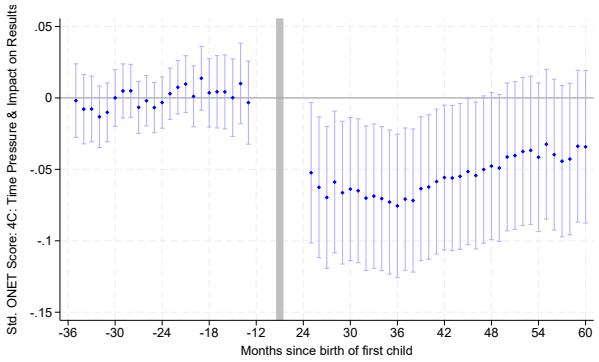
(b) Job Requires Leadership and Stress Tolerance



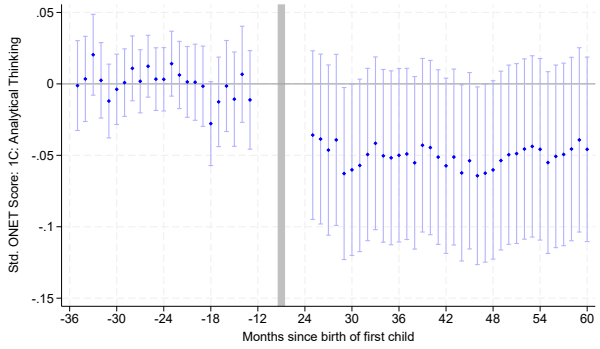
(c) Job Involves Leadership and Respons. for Outcomes



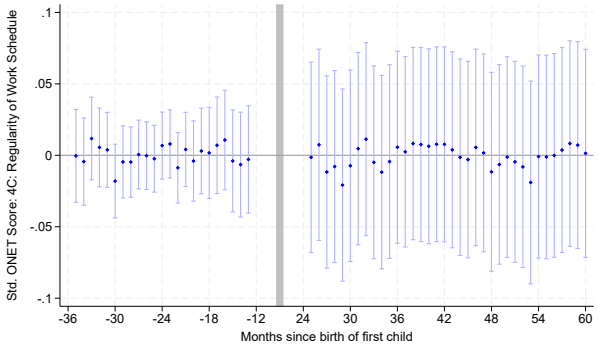
(d) Job Involves Time Pressure and Impact



(e) Job Requires Analytical Thinking



(f) Job's Schedule is Regular



Notes: Sample is the baseline sample as described in Section 2.4.2. This figure shows the Event Study coefficient estimates (δ_k coefficients from equation (2.3)), where the outcomes are standardized ONET scores that describe the the extent of which a job has the characteristic described in the panel titles. Bars are 95 percent confidence intervals based on robust standard errors.

Column (3) reveals that the overall effect on hours in the 3rd year after giving birth is driven by those in white-collar occupations and with above-median baseline wages and

above-median baseline AKM firm effect. Column (4) reveals that the overall effect on firm size decile is driven by those who were relatively older (31 years old or older) at the time of the event, as well as those who live in the capital city, Budapest. There are no groups with a significant effect on the propensity to work in white-collar occupations.

Finally, Figure 2.8 suggests that eligible mothers sorted into occupations that are less achievement-oriented and require less stress tolerance, analytical thinking, leadership and responsibility, and involve less time pressure (we also checked and did not find any substantial heterogeneities for these outcomes).

2.6.5 Mechanisms: Transitions Between Employers and Across Occupations

In this section, we investigate what patterns on transitioning between employers and across occupations can explain the above mentioned results of higher AKM firm effect and higher wage for many subgroups.

Panel (a) of Figure 2.9 depicts the ratio of workers who are employed at the same firm as in event time -13. We do not see clear evidence that the difference between the CB+W-eligible and ineligible mothers would differ from the same difference in the control group (women never giving birth). Panels (b)–(c) show the ratio of workers who are employed at higher (panel (b)) or lower (panel (c)) AKM firm FE firm than at event time -13. Here, panel (c) suggests that 3-5 years after childbirth, employment probability at lower AKM firm FE firm is relatively lower among those mothers who were CB+W-eligible.

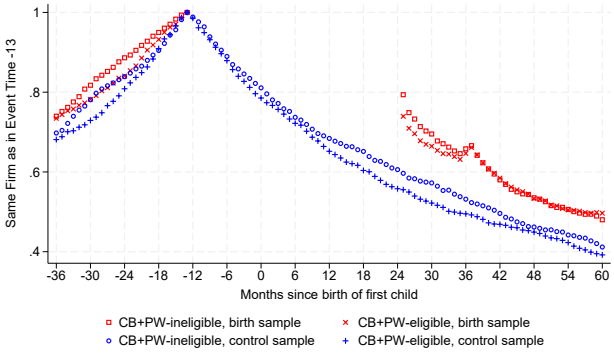
Table 2.6: Twelve-Monthly Effect of the CB+W Policy on Transition Between Employers

	Any transition	Transition to higher AKM FE firm	Transition to lower AKM FE firm
beforeY3 $\times B \times D$	0.0019	0.0015	-0.0000
	[0.0030]	[0.0021]	[0.0020]
afterY3 $\times B \times D$	-0.0100***	-0.0046**	-0.0052**
	[0.0034]	[0.0022]	[0.0022]
afterY4 $\times B \times D$	-0.0015	0.0001	-0.0012
	[0.0032]	[0.0021]	[0.0020]
afterY5 $\times B \times D$	-0.0054*	-0.0008	-0.0040**
	[0.0031]	[0.0020]	[0.0020]

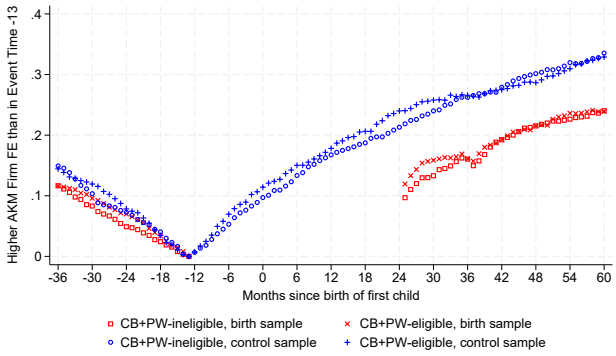
Notes: Table shows average effect of the CB+W policy on the probability of transition between employees (equation (2.4), but allowing the treatment effect to vary across 12-month periods). Baseline is the second year before childbirth. Robust standard errors in brackets. Number of individuals: 10,969. Number of observations: 475,715. Outcomes are in the column titles.

Figure 2.9: Employment at Same Firm, and Higher or Lower Quality Firm as in Event Time -13

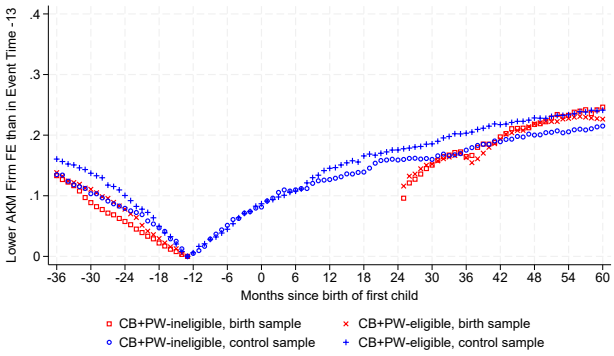
(a) Employment at Same Firm



(b) Employment at Higher AKM Firm FE Firm



(c) Employment at Lower AKM Firm FE Firm



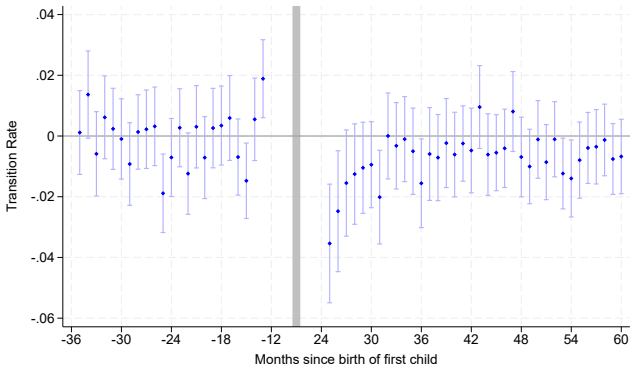
Notes: Sample is the baseline sample as described in Section 2.4.2, restricted to employed individuals.

In Figure 2.10 we show our event study estimates for the transition rates between employers. We estimate equation (2.3). In panel (a), the binary outcome is one if the current employer is different from the last observed employer (conditional on the last employment being observed at most 48 months before), zero otherwise. In panels (b) and (c) the binary outcome is one if the AKM firm FE of the current firm is higher (lower) than the AKM firm FE of the last employer.[9] The estimation results indicate that CB+W-eligibility decreases the transition rate between employers in the 3rd and 5th years after childbirth. These patterns are reinforced by the twelve-monthly average effects reported in Table 2.6, moreover, the decreasing transition rate to lower quality firms is slightly more pronounced than the decreasing transition rates to higher quality firms in the 3rd year after childbirth, and especially in the 5th year after childbirth.

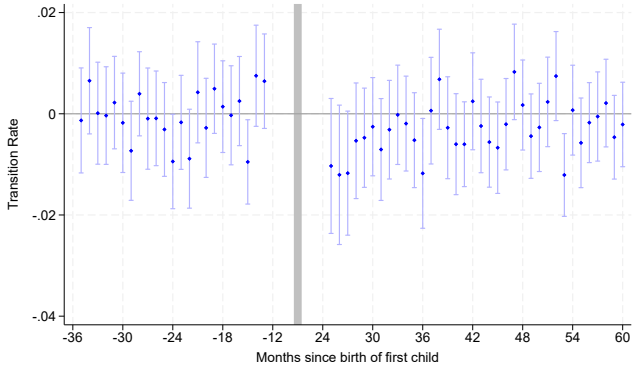
[9]We present descriptive plots for the time patterns of these outcomes in Figure B.7.

Figure 2.10: Effect of CB+W-Eligibility on Transitions Between Employers

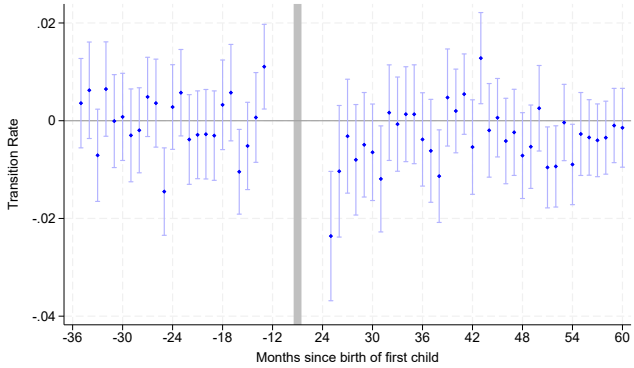
(a) Any Transition



(b) Transition to Higher AKM FE Firm



(c) Transition to Lower AKM FE Firm



Sample is the baseline sample as described in Section 2.4.2. This figure shows the Event Study coefficient estimates (δ_k coefficients from equation (2.3)). Bars are 95 percent confidence intervals based on robust standard errors.

Table 2.7: Effect of the CB+W Policy on the Likelihood of (i) Being at the Same Firm as 1 Year Before the Event, (ii) Being at a Firm with Lower AKM Firm FE, and (iii) Being at a Firm with Higher AKM Firm FE, Average Effects in Years 3–5 After Childbirth

	Full Sample			Matched Sample		
	Same firm	Lower FE	Higher FE	Same firm	Lower FE	Higher FE
afterY3 × B × D	0.0333	-0.0350**	0.0068	0.0331	-0.0386**	0.0101
	[0.0220]	[0.0173]	[0.0173]	[0.0223]	[0.0175]	[0.0175]
afterY4 × B × D	0.0322	-0.0361**	0.0104	0.0317	-0.0374**	0.0114
	[0.0224]	[0.0181]	[0.0181]	[0.0227]	[0.0183]	[0.0183]
afterY5 × B × D	0.0233	-0.0322*	0.0163	0.024	-0.0344*	0.0176
	[0.0228]	[0.0191]	[0.0191]	[0.0231]	[0.0193]	[0.0194]

Notes: Table shows average effects of eligibility to the policy allowing work while being on PPL benefits on the probability of working in years 3–5 after childbirth, using 2nd and 3rd years preceding childbirth as reference period. Robust standard errors in brackets. *** p¡0.01, ** p¡0.05, * p¡0.1. Outcomes are in the column titles.

Table 2.8: Effect of the CB+W Policy on the Difference in the Standardized Occupation Score, Relative to the Baseline Score in the 2nd Year Before Childbirth, Average Effects in Year 2–5 After Childbirth

Difference in: (*Relative to Baseline beforeY2*)	Std. Analytical ONET Score	Std. Achievement ONET Score	Std.Lead-Stress ONET Score	Std.Lead-Resp. ONET Score	Std. Time Pressure Score
afterY2 × $B \times D$	-0.0298	-0.0527	-0.0422	-0.0491	-0.0431
	[0.0333]	[0.0337]	[0.0306]	[0.0382]	[0.0283]
afterY3 × $B \times D$	-0.0424	-0.0599**	-0.0560**	-0.0608**	-0.0666***
	[0.0294]	[0.0291]	[0.0258]	[0.0306]	[0.0245]
afterY4 × $B \times D$	-0.0549*	-0.0683**	-0.0629**	-0.0757**	-0.0601**
	[0.0305]	[0.0302]	[0.0268]	[0.0311]	[0.0251]
afterY5 × $B \times D$	-0.0485	-0.0529*	-0.0546*	-0.0659**	-0.0371
	[0.0319]	[0.0317]	[0.0282]	[0.0325]	[0.0261]

Notes: Table shows average effects of eligibility to the policy allowing work while being on PPL benefits on the probability of working in years 3–5 after childbirth, using 2nd and 3rd years preceding childbirth as reference period. Robust standard errors in brackets. *** p¡0.01, ** p¡0.05, * p¡0.1. Outcomes are in the column titles.

In Table 2.7, we show the policy effects on the propensity to (i) stay at the same firm as 1 year before the event, and, if switching, on the propensity to (ii) switch 'down' to a firm with a lower AKM firm effect (and lower firm-specific wage premium), and to (iii) switch 'up' to a firm with a higher AKM firm effect (and higher firm-specific wage premium). Column (2) reveals that, conditional on switching, eligibility to the policy significantly decreased mothers' propensity to switch 'down' to a firm with a lower AKM firm effect by more than 3 percentage points in all years in the 3rd, 4th and 5th year after childbirth. Notably, eligibility to the policy increased mothers' propensity to stay at the same firm as they were one year before childbirth, although the point estimates are not significant at the usual significance levels. Reassuringly, these results are robust to using the matched sample as the estimation sample.

In Table 2.8, we show the policy effects on the difference in the standardized occupation score, relative to the baseline score in the 2nd year before childbirth: already in the 2nd, and even so in the 3rd year after childbirth eligible mothers have sorted into occupations that require less analytical thinking, stress tolerance, leadership and willingness to take on responsibilities, and involve less time pressure, and the difference stays relatively stable up to years 5 after childbirth (except for the time pressure score). This result suggests important persistence in occupational sorting of mothers with young children.

2.7 Conclusions

In this paper, we exploit a unique paid parental leave (PL) reform in Hungary that affected only the possibility of employment during the receipt of PL benefits, while keeping the overall benefit amounts unchanged, to identify the substitution effect in paid PL on the timing of mothers' return to work, their long-run employment propensity, and subsequent labor market trajectories. Specifically, Hungarian mothers whose child turned into their 2nd year of age after 2014 were provided strong incentives to return to the labor market earlier than previous cohorts, since they did not have to give up PL benefits while earning their wage income during their children's 2nd year of age.

Using an Event Study research design with women without births in the control group and Hungarian linked employer-employee data, we first establish that the policy increased mothers' propensity to work exactly when it provided strong financial incentives, thereby inducing eligible mothers to return earlier to the labor market, in months 19–24 after giving birth. We find that mothers in eligible cohorts were by 30 percent more likely to work 19-24 months after giving birth – but, no further differences exist in their working propensities or its timing. Thus, this policy provides a unique setting to study mothers' long-run labor market trajectories when a subset of them was incentivized to bring forward their return-to-work, but otherwise no differences exist in terms of subsequent employment and fertility behavior.

The estimated impact on employment is due to the response of mothers who previously would not have worked due to the substitution effect, that is the distortional effect, of paid parental leave. This finding suggests that the majority of mothers who decide not to work until their child's 2nd birthday (90% of all mothers before the reform) do so due to the income effect of paid parental leave or changed preferences.

Second, we assess whether earlier return-to-work affects mothers' job quality outcomes. We find modest effects, on average, on mothers' wages for some groups (for those who gave birth at an older age and live in the capital), and a clear positive effect on their firms' average wage premium 3-5 years after giving birth – these results are due to eligible mothers being less likely to switch 'down' to lower-paying firms than ineligible ones. We also find that eligible mothers sort to occupations that require less analytical thinking, stress tolerance, leadership and willingness to take on responsibilities, and involve less time pressure. Our results point to the importance of analyzing also other outcomes than wages, such as the quality of the firms and the characteristics of the occupations mothers sort into, after giving birth to their first child.

CHAPTER 3

Parental Resources and College Major Choice

with Tyler Radler and Nikhil Rao

3.1 Introduction

Choices about college attendance and major are among the few life-altering decisions young adults must make. As income differences between college majors have become larger than the earnings gap between high-school and college graduates (Altonji, Blom, and Meghir, 2012), choosing the right major is more important than ever. While parents have always had a strong direct influence over these decisions, parental resources could also significantly alter college major choice. Parental financial help could allow students to enter majors requiring more years of education, while parental wealth could provide insurance to students entering majors with high earning volatility. Students who expect substantial parental help to finance future expenditures on housing or other expensive assets might be more willing to choose a lower-paying but more rewarding major. Despite its importance, the literature lacks causal evidence on the connection between parental wealth and college major choice.

In this paper, we fill this gap by investigating the role of changing parental wealth in college major choice. Using variation in housing prices across time and geography, we provide evidence that students respond to changes in parental wealth. We study how the major choice of freshmen students changed during the housing boom era between 2000 and 2006, when speculative forces created sizable differences in house price increases across the US. Using these bubbles to instrument for three-digit zip code-level (ZIP3) house price changes, among the first in the literature, we draw a causal link between home price changes and college major choice. We find that, among students whose parents were likely homeowners, increasing house prices led students to switch to majors that provided a lower-earning profile but higher non-pecuniary amenities.

We study the connection between house price increases, parental homeownership, and

college major choice empirically using a difference-in-difference model on the CIRP Freshmen Survey (TFS). The TFS contains the intended major for more than 250,000 freshmen students in the US each year in the early 2000s, which we connect with average earnings outcomes of majors, collected by Hampole (2022). We divide majors into four categories based on their earning potential (low and high-earning majors) and volatility (low and high-volatility majors); the change in enrollment into these four categories is the primary outcome we are interested in. Since our data do not contain information on parental homeownership, we proxy it with the homeownership rates of students' five-digit zip codes. We show that the distribution of students across our four college major categories is similar in low and high-homeownership areas, even though they differ in many other economic characteristics.

We are interested in how enrollment into our four college major categories changes due to a ZIP3-level house price increase. We test this connection within a difference-in-difference model: we examine how the major choice of students living in ZIP3s experiencing a high house price increase changed between 1999-2001 and 2005-2007 compared to students from ZIP3s with a modest price increase. To establish a causal relationship between change in major choice and house prices, we follow Charles, Hurst, and Notowidigdo (2018) and instrument house price changes with the magnitude of the ZIP3-level structural break in house prices. The housing literature established that speculative forces caused the emergence of housing bubbles, which created exogenous variation in house price increases across metropolitan areas. Consistent with Charles, Hurst, and Notowidigdo (2018), we show that the emergence of housing bubbles is unrelated to the change in ZIP3-level college major enrollment during the 1990s.

The findings of our empirical analysis highlight the importance of parental homeownership in students' college major choice. We find that a one-standard-deviation increase in ZIP3-level home prices increases enrollment in high-paying majors by 0.7 percentage points. We show that house price increases affect students differently depending on the likelihood of parental homeownership. We find that students living in low-homeownership areas, where their parents are less likely to be homeowners, increased their participation in higher-paying / less-volatile majors by 2.1 percentage points, and decreased their enrollment in lower-paying / low-volatility majors by 1.9 percentage points due to a one-standard-deviation increase in house prices. Meanwhile, a similar rise in house prices decreased the enrollment of students from high-homeownership areas in high-paying majors by 1 percentage point, mainly through declining participation in high-income / low-volatility majors. Overall, we find that house price increases motivated students from low-homeownership areas to switch to higher-paying and less-volatile majors. In contrast, students with a high probability of parental homeownership were more likely to enroll in lower-paying and/or more volatile

majors.

Then, we study whether house price increases have a different effect by race or family income. We show that the magnitude of the impact of house price increases slightly rises with parental income in both low- and high-homeownership areas, except for the highest-income students, whose major choice seems to depend less on house price increases. We find that house price increases have the opposite effect on the major choice of White and non-White students. White students tend to increase their enrollment in lower-paying and/or highly volatile majors in the case of increasing house prices, while non-White students switch to higher-paying and/or less volatile majors. We demonstrate that one of the primary sources of this difference is compositional, as White students are more likely to reside in areas with high homeownership rates. Our results suggest that White and non-White students living in low-homeownership areas behave similarly when experiencing a house price increase. However, we do not find evidence of increased participation in lower-earning or more volatile majors among non-White students residing in high-homeownership areas, a behavior that is quite prevalent among White students.

3.1.1 Related Literature

There has been a considerable amount of research studying how changes in parental income and wealth affect children's outcomes. This line of work has primarily focused on understanding how shocks to parental resources, for example through job loss (Rege, Telle, and Votruba, 2011; Hilger, 2016; Riukula and Huttunen, 2019), or changes in labor market conditions (Butikofer, Dalla Zuanna, and Salvanes, 2018; Stuart, 2022), affect the education and labor market outcomes of children.[1]

A complementary literature has started to unpack the causal impacts of parental resources to understand how these resources influence children's human capital and labor market decisions. One channel that this line of work has considered is the idea of parental resources as an informal safety net for children. Boar (2021) documents a strong parental motive to engage in precautionary savings to mitigate their children's future income risk. Recent evidence from the United States and Nordic countries shows how parents use their social networks to help their children get desirable jobs (Staiger, 2023) or transfer wealth to finance large asset purchases or help weather adverse shocks (Fagereng, Mogstad, and Rønning, 2021; Kaila, Nix, and Riukula, 2021).

If children are aware that their parents can provide a safety net, they might place greater weight on non-monetary factors when making human capital and labor market decisions and

[1] See Page, Stevens, and Lindo (2007) and Page (2024) for recent reviews.

take on greater risk.[2] Luo and Mongey (2019) presents a quantitative model highlighting the stark trade-off between wages and amenities when individuals have relatively low assets or high debt. Descriptive evidence from the United States suggests that children from richer backgrounds are more likely to work in occupations with high intrinsic non-pecuniary quality (Boar and Lashkari, 2021; Haeck and Laliberté, 2025). Berg and Hebous (2021) use variation in Norwegian wealth tax rules to show that greater parental wealth increases the dispersion of their children's labor market earnings, interpreting this evidence as a link between parental resources and risk-taking. Additionally, there is experimental evidence on how liquidity and credit constraints prevent people in precarious financial situations from wanting to work in high amenity value public service jobs (Coffman et al., 2019).

In this paper, we focus on how students' family backgrounds influence the *types* of human capital that they choose to invest in. A large body of related work has studied students' decisions about whether to attend college and what fields to pursue. Research has documented how students think about the employment probabilities, future wages, and labor market risk when making decisions about what to study (Altonji, Arcidiacono, and Maurel, 2016; Blom, Cadena, and Keys, 2021; Hampole, 2022). While Patnaik, Wiswall, and Zafar (2020) discuss how family can influence college major choice, they focus on information and family preferences. Descriptive evidence from the National Education Longitudinal Study 1988–1994 suggests that students from low socio-economic backgrounds are more likely to choose majors leading to more lucrative economic opportunities, which is in line with our hypotheses (Ma, 2009). However, there is limited causal evidence on how the safety net provided by parental resources influences college major choice.

This paper is also related to the literature studying the importance of housing wealth for whether students attend college, and the selectivity of the institutions that they attend, using the Panel Study of Income Dynamics and National Longitudinal Survey of Youth 1997. Using short-run variation in housing prices, this literature finds that an increase in home equity raises the probability that students enroll in college and attend a flagship public university, and reduces the probability of attending a community college (Lovenheim, 2011; Lovenheim and Reynolds, 2013). These effects are especially pronounced for students from relatively low-income backgrounds (approximately less than $70,000 annual income). While we also use shocks to housing wealth for identification, our focus is on college major choice. We also use data from the Higher Education Research Institute's "The Freshman Survey", which has had limited use in economics to the best of our knowledge (Bradley, 2012).

The remainder of this paper is organized as follows. In section 3.2, we illustrate how

[2]These motives are likely to be especially large in the United States, given that the state provides relatively less insurance than other similar countries (Pfeffer and Hällsten, 2012).

house price increases affect college major choice through two channels, increasing parental wealth and rising housing cost expectations. In section 3.3, we summarize the data we use and how we group majors based on their earning characteristics. In section 3.4, we discuss our empirical methodology and the calculation and validity of our structural break instrument. In section 3.5, we present our empirical findings and summarize how house price changes affect students differently by race, parental income, and the likelihood of parental homeownership. Finally, section 3.6 summarizes our results.

3.2 Connection between home prices and major choice

In this paper, we study how changing house prices affect the college major choice of freshmen students, which effect likely depends on parental resources. In this section, we summarize how changing house prices could affect college major choice in the case of different parental homeownership and income. We present the connection between changing house prices and major choices from students' perspective, even though major choice is likely an iterative process between them and their parents, who may have more information about the earning outcomes of majors and the general economic conditions. In this way, increasing house prices can affect students' final major choice decisions by altering parental expectations regarding their children's future career paths.[3]

Increasing home prices could directly influence college major choices through two channels: altering parental housing wealth and raising expectations for future housing costs. First, increasing parental wealth could allow students to choose (1) majors with higher income volatility, as parental resources could mitigate the consequences on potential future negative wage outcomes, or (2) majors with lower expected earnings, due to potentially increasing future parental help financing larger expenses. Thus, we expect that students whose parents experienced a larger increase in their wealth are more likely to choose college majors with a lower and/or more volatile earning profile than students with a modest parental wealth increase. Second, experiencing a larger house price increase likely increases expectations for future housing expenses and living costs, which could motivate students—or their parents, who may influence their choices— to choose majors with higher earning potential and/or lower income volatility. In this case, increasing house prices prompt students or their parents to consider higher-paying or less volatile career paths, which prepare them for future affordability challenges. The effect of house price changes on college major choice

[3]For example, if a student's preferred major is not aligned with the major that the student's parents would like her to choose, increasing parental wealth due to house price increases could mitigate the parents' will to influence their child's major choice.

through these two channels is not homogeneous and depends on parental homeownership, wealth, and income. We discuss the direction and likely magnitude of the potential effect on students based on whether the parents are homeowners and how well-off they are.

Students from families who do not own a home only experience the negative consequences of house price increases. Due to the lack of parental housing wealth, increasing house prices affect these students' major choices mainly by increasing their expectations for future expenses, but they could also lead to increased family rent payments. Because of these changes, we expect that students without parental homeownership will switch to higher-paying and/or less-volatile majors in the event of a house price increase, which will better prepare them to cover increased current and future housing costs. In theory, the magnitude of shifting to these majors should decrease with parental income and wealth. Students from less well-off families cannot expect as much parental financial support, which highlights the importance of choosing a major that offers a safer, albeit less lucrative, career-earning path.

In contrast, when house prices increase, students from homeowner families experience both increased parental wealth and rising expectations for future housing expenditures. As these two channels have an opposite effect on the preferred earning power and volatility of their college major, the net effect of house price increase on major choice depends on the income and wealth of parents. The channel of incresing future expenses, which motivates students to choose less volatile and higher-paying majors, likely dominates in the case of moderate parental resources, in which case the increasing financial support from parental income or wealth cannot offset increasing future expenses. But, in general, we expect that increasing parental housing wealth allows the majority of students from homeowner families to choose a major with lower earning potential and/or more volatile income distribution.

3.3 Data on college major choice

3.3.1 CIRP Freshmen Survey

Our primary source of data measuring college outcomes is the CIRP Freshmen Survey (TFS) administered by the Higher Education Research Institute (HERI) at UCLA. The survey is administered to first-year students at the beginning of the academic year. During the early 2000s, TFS collected between 265,000 and 290,000 unique responses each year from college freshmen from more than 1,000 institutions in the United States. The survey collects detailed information on family background, academic preparedness, college expectations, and demographics. TFS is the largest nationally representative survey that contains information on the key outcomes of our interest, the intended college majors of freshmen students. The

survey lists a broad range of majors (up to 84) and asks students to select only one option that indicates their probable field of study.[4]

The primary data in this paper comprises the responses of freshmen from the 1999-2001 and 2005-2007 waves of the study, which intervals center around the start and end years (2000 and 2006, respectively) of the housing boom before the 2008 financial crisis. We decided to drop the 2002-2004 waves of TFS, because during these years, housing booms formed at different times across ZIP3 areas. We narrow down our sample to students entering institutions that participated in all of our waves, ensuring that our results were not affected by the selection of colleges for the study. As Charles, Hurst, and Notowidigdo (2018) finds that the MSA-level housing boom decreased college enrollment at two-year colleges, but had no significant effect on enrollment in four-year institutions, we restrict our sample to freshmen entering four-year colleges. We make this restriction to ensure that the characteristics of entering students are similar in both time periods, and that changes in selection for four-year college education due to the housing boom do not affect our results.

Table 3.1: Student characteristics in the 1999-2001 and 2005-2007 waves of TFS

Variable	Pre waves	Post waves	Difference
Real HH Income (2000 USD)	72,948.273	75,112.844	2,164.569***
	(55,399.711)	(60,739.992)	(672.329)
Female	0.545	0.559	0.014***
	(0.498)	(0.496)	(0.003)
Asian (pct of freshmen)	0.079	0.085	0.007*
	(0.269)	(0.279)	(0.004)
Black (pct of freshmen)	0.077	0.083	0.006
	(0.267)	(0.276)	(0.004)
Hispanic (pct of freshmen)	0.066	0.063	-0.003
	(0.247)	(0.243)	(0.007)
White (pct of freshmen)	0.710	0.684	-0.026***
	(0.454)	(0.465)	(0.010)
College graduate: Father	0.533	0.543	0.010*
	(0.499)	(0.498)	(0.006)
College graduate: Mother	0.497	0.542	0.045***
	(0.500)	(0.498)	(0.005)
First-generation student	0.201	0.189	-0.012***
	(0.401)	(0.392)	(0.004)
Observations	320,364	336,315	656,679

Note: Robust standard errors clustered at ZIP3-level.

Table 3.1 presents the main characteristics of the student body in the 1999-2001 and 2005-

[4]Even if undecided is included as an option, in our sample, only 7.5% of students are undecided on their intended major.

2007 waves, which we refer to as the pre- and post-boom waves, respectively. Compared to students in the pre-boom waves, post-boom students have higher real family income, are more likely to have college-educated parents, and are more likely to be female and non-White.

3.3.2 Major Characteristics

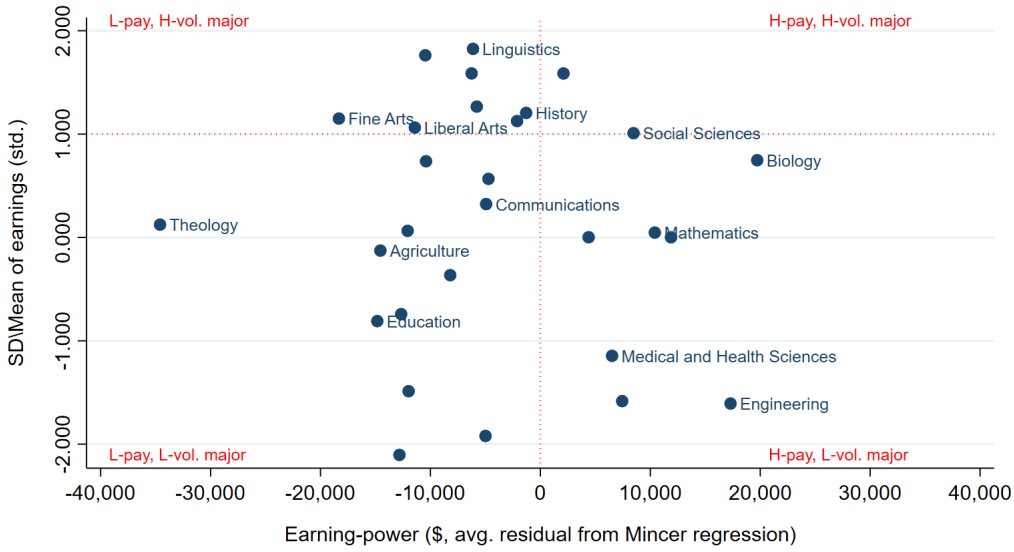
We connect intended majors with their future earning potential using Hampole (2022)'s calculations, summarized in Table C.2. Using the ACS between 2009 and 2019, she quantified different distributional characteristics of the earnings of college graduates aged 21 to 60 for 30 different college majors. To measure which college major provides above-average earnings, Hampole (2022) calculated the mean residual of a Mincer wage regression by college major (column 3 in Table C.2).[5] These average residuals represent how much a college graduate with a given major earns compared to the average college graduate with the same observable characteristics. Based on this measure, the worst-paying college majors are "Theology and Religious Vocations", "Fine Arts", and "Education Administration and Teaching", while "Physical Sciences", "Engineering", and "Biology and Life Sciences" provide the highest earnings among majors. We merge information on earning characteristics in Table C.2 to our main data by aggregating up intended college majors in the TFS into the categorization of the Classification of Instructional Programs (CIP) system, which Hampole (2022) uses.

We use Hampole (2022)'s calculation on the earning characteristics of majors to define our main outcome variables. We use the average residual variable to define our high-earning major variable, which takes the value of one if the average residual is positive. The other main dimension we are interested in is riskiness, which we define with the volatility of earnings. We measure the volatility of college majors in two ways: (1) using only earnings' standard deviation, and (2) dividing earnings' standard deviation by the mean earnings of the major. Using the standard deviation alone has the advantage of easy interpretation, but with the cost of categorizing high-paying majors as highly volatile just because of their earning distributions are more widespread. Table C.2 shows that higher-paying majors also tend to have higher standard deviations. Based on the standard deviation, for example, the English major (standard deviation of $75,013) seems less highly-volatile, and thus less risky than Engineering ($83,227), even though Engineering has higher mean earnings and wages at any observed earning percentiles than English.

Because of the possible misleading risk categorization of the standard deviation, we prefer to use our second option, the SD/Mean variable, to measure volatility. This metric adjusts the spread of the income distribution with its average earnings, and thus it is more suitable for

[5]First, Hampole (2022) regressed the annual wage on age, age-squared, race, ethnicity, sex, and survey-year fixed effects, and then she averaged the calculated residuals from this regression.

Figure 3.1: Distribution of college majors by earning power and volatility



Source: Own calculation, based on data from Table D2 and D3 in Hampole (2022).

measuring volatility conditionally on earning power. Following up on our previous example, the SD/Mean variable indicates that English is a more volatile major than Engineering (1.422 vs. 0.964), which aligns with the general view of the riskiness of these two majors. For better interpretation, we standardize the SD/Mean variable with the weighted mean standard deviation and mean of majors, and characterize a major as highly volatile if the standardized SD/Mean is more than 1.

Then, we divide majors into four categories based on whether they are high-paying and highly volatile; Figure 3.1 illustrates how college majors are distributed over these two dimensions. First, based on our categorizations, the high-paying and low-volatility majors are the STEM fields, Business, and healthcare-related majors. Second, the high-paying and highly volatile category contains only two majors, "Social Sciences" and "Area Ethnic and Civilization Studies"; as more than ten times as many college graduates studied the former, we will later refer to the high-paying and highly volatile category as Social Sciences. Third, almost all of the low-earning but highly volatile majors come from the fields of Humanities; this group includes majors such as English, History, Fine Arts, and Linguistics. Finally, our fourth group, the low-paying and less volatile group, contains many smaller majors; the most populous are Education, Communication, and Social Work.

3.4 Empirical methodology and the housing boom instrument

In this section, we explain the empirical methodology we use to draw a causal relationship between house price increases and college major choices. We study how the housing price boom preceding the 2008 financial crisis influenced the college major choices of freshmen students using an instrumental variable framework. Our empirical methodology builds on the consensus of the housing literature that differences in house price changes among metropolitan areas (MSAs) between 2000 and 2006 were driven by speculative forces rather than fundamental economic changes (see Charles, Hurst, and Notowidigdo (2018) for further discussion). The existence of structural breaks in home price increases creates variation unrelated to economic forces influencing the college major choice of freshmen students. As a result, in our framework, the structural break in home prices becomes a valid instrument, which affects college major choices only through house price increases. In this way, we can measure whether students from ZIP3s experiencing a sizable structural break in house prices due to speculation chose different majors than those who lived in three-digit zip areas with moderate house price increases.

We define our instrument at the ZIP3 level, rather than at the MSA level (as in Charles, Hurst, and Notowidigdo (2018)), which has at least two advantages. First, there are more than three times as many ZIP3 areas (878) as MSAs (256) in our sample, which gives us more variation in house price increases. Second, by using a ZIP3-level instrument, our data also includes rural areas located outside of MSAs. This allows us to draw more generalizable conclusions on the connection between house price increases and college major choice, as we do not need to narrow down our findings to students residing in urban areas.

3.4.1 Methodology

We measure the house price change in a three-digit zip code (ZIP3) area c with ΔP_c, which is the difference between the 2006 and 2000 log annual FHFA house price index. To get more easily interpretable results, we standardize ΔP_c using the population-weighted mean and standard deviation of ZIP3-level home price changes, which we call $\widehat{\Delta P_c}$. We test the connection between $Y_{i,c,t}$, the characteristic of the chosen major of student i from ZIP3 area c at period t, and the standardized house price increase $\widehat{\Delta P_c}$ in the following difference-in-difference framework:

$$Y_{i,c,t} = \alpha + \beta_1 \, \mathbf{1}(t = post) + \beta_2 \, \widehat{\Delta P_c} + \eta \, \mathbf{1}(t = post) \times \widehat{\Delta P_c} + \Gamma_1 \, \mathbf{X}_{i,t} + \Gamma_2 \, \mathbf{X}_{c,2000} + \varepsilon_{i,c,t}$$

in which the vectors of $\mathbf{X}_i$ and $\mathbf{X}_{c,2000}$ represents controls for individual and ZIP3-level characteristics. Individual controls include indicators for gender, high-school GPA, college selectivity, and parental educational attainment. The vector of ZIP3-level controls includes the log of population, the share of college-educated adults between the ages of 25 and 64, the share of foreign-borns, and the share of women in the labor force, all measured in 2000. Our regressions are weighted using TFS's student weights, and we cluster robust standard errors at the ZIP3 level. The main variable of our interest is η, which we can interpret as how a one-standard-deviation increase in ZIP3-level house prices affects different characteristics of the chosen college major.

In section 3.2, we argued that parental homeownership could influence students' college major choices during rapid home price increases. Counter to Lovenheim (2011) and Lovenheim and Reynolds (2013), our main dataset provides limited information on the economic conditions of students' families and lacks data on parental homeownership. We overcome this data limitation by proxying parental homeownership with the homeownership rate of students' five-digit ZIP codes (ZIP5). This approximation builds on the idea that students' parents are more likely to be homeowners in areas with high homeownership rates than in those with low homeownership rates. Then, we quantify whether our baseline diff-in-diff results are statistically different in zip codes with low and high homeownership areas, which proxy how parental homeownership affects major choices in the case of increasing house prices. We test the statistical significance of differences in the estimated effects between low and high homeownership areas in the following triple-difference specification:

$$\begin{aligned} Y_{i,c,t,z} = \alpha &+ \beta_1 \, \mathbf{1}(t = post) + \beta_2 \, \widehat{\Delta P_c} + \beta_3 \, HH_z \\ &+ \kappa_1 \, \mathbf{1}(t = post) \times \widehat{\Delta P_c} + \kappa_2 \, \mathbf{1}(t = post) \times HH_z + \kappa_3 \, \widehat{\Delta P_c} \times HH_z \\ &+ \mu \, \mathbf{1}(t = post) \times \widehat{\Delta P_c} \times HH_z \\ &+ \Gamma_1 \, \mathbf{X}_i + \Gamma_2 \, \mathbf{X}_{c,2000} + \varepsilon_{i,c,t,z} \end{aligned}$$

in which z represents the zip code of student i's permanent address, $HH_{i,z}$ is a dummy variable indicating high-homeownership zip codes, while the vectors of $\mathbf{X}_i$ and $\mathbf{X}_{c,2000}$ contain the same set of controls as our diff-in-diff model. In our analysis, HH_z takes a value of 1 if a student's zip code is among the top 25% of zip codes by homeownership rate, while it is 0 if the zip code is in the bottom 25% of all zip codes.[6] In this way, we compare the effect of house price increases on students with the lowest and highest likelihood of parental homeownership. The primary variable of interest in the triple-diff specification is μ, which

[6]The housing unit-weighted cutoffs of the bottom and top 25% of zip codes by homeownership rate are 57.4% and 78.9%; Table 3.2 shows that the average homeownership rates in these areas are 41.7% and 86.1%, respectively.

Table 3.2: Student characteristics in low and high homeownership ZIP5 areas

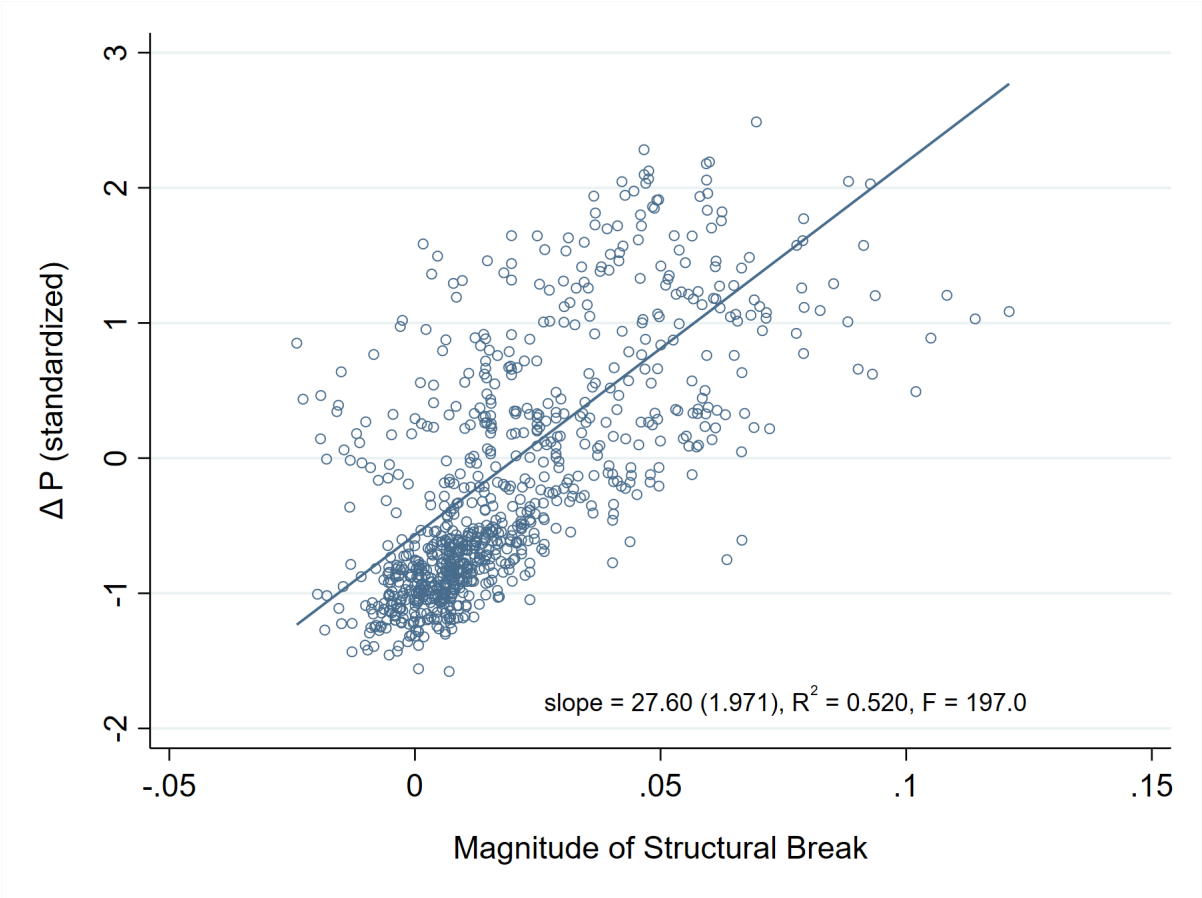
Variable	Low homeownership ZIP5	High homeownership ZIP5	Difference (High-Low)
Homeownership rate (zip code)	0.426	0.858	0.431***
	(0.132)	(0.047)	(0.012)
HH Income: bottom 35pct	0.442	0.242	-0.199***
	(0.497)	(0.428)	(0.028)
HH Income: 36-89th pct	0.380	0.526	0.146***
	(0.485)	(0.499)	(0.020)
HH Income: top 10pct	0.091	0.143	0.052***
	(0.288)	(0.350)	(0.010)
Female	0.565	0.532	-0.033***
	(0.496)	(0.499)	(0.007)
Asian (pct of freshmen)	0.138	0.061	-0.077***
	(0.345)	(0.240)	(0.015)
Black (pct of freshmen)	0.143	0.045	-0.098***
	(0.350)	(0.206)	(0.018)
Hispanic (pct of freshmen)	0.159	0.026	-0.133***
	(0.365)	(0.159)	(0.036)
White (pct of freshmen)	0.465	0.815	0.351***
	(0.499)	(0.388)	(0.040)
High-paying	0.616	0.623	0.007
	(0.486)	(0.485)	(0.007)
High-vol.	0.249	0.225	-0.024***
	(0.432)	(0.417)	(0.007)
L pay, L vol	0.193	0.205	0.013
	(0.395)	(0.404)	(0.008)
L pay, H vol	0.191	0.172	-0.019***
	(0.393)	(0.377)	(0.006)
H pay, L vol	0.558	0.570	0.011
	(0.497)	(0.495)	(0.008)
H pay, H vol	0.058	0.053	-0.005**
	(0.233)	(0.224)	(0.002)
Observations	55,728	115,419	171,147

Note: Low and high homeownership areas include the bottom and top 25% of all ZIP5 areas, respectively.

indicates how a one-standard-deviation increase in ZIP3-level house prices affects the college major choice among students residing in high-homeownership zip codes compared to those living in low-homeownership zip codes.

Our estimation strategy does not require having a balanced student characteristic between low and high homeownership areas, as our house price change variable and structural break instrument are defined at the ZIP3 and not at the five-digit zip code level. However, Table 3.2 shows that students' major choice patterns are quite similar, even if there are significant differences among students living in high and low homeownership areas. Students living in high-homeownership areas are more likely to be White and come from families from the upper part of the household income distribution. Despite these differences, the distribution of students based on their chosen majors' earning potential and volatility is fairly similar. Table 3.2 shows no statistically significant differences in the share of students choosing high-earning, specifically high-earning and low-volatility majors. Although other categories exhibit statistically significant differences between low and high homeownership

Figure 3.2: Validity of housing break instrument: Correlation with house price change



Note: Each dot depicts a ZIP3 area; the vertical axis represents the increase in log house price index between 2000 and 2006. Regression is weighted with the 1990 population of ZIP3 areas.

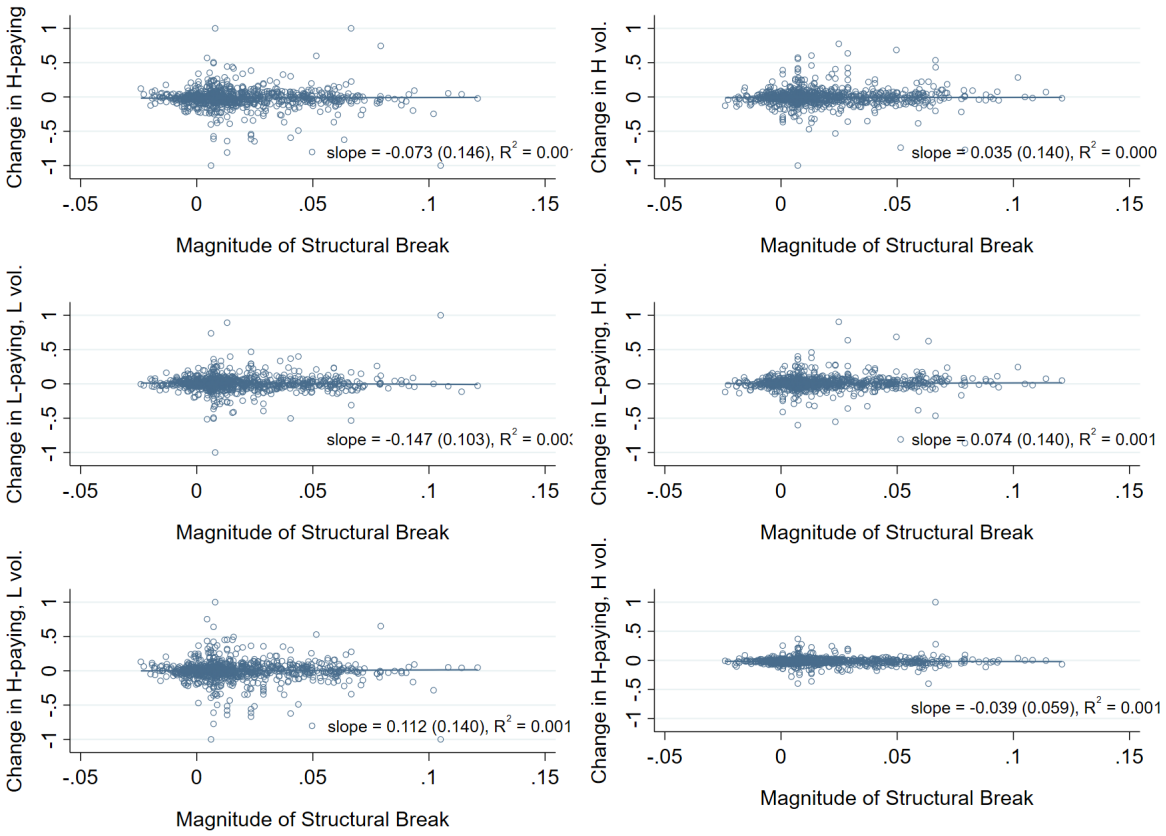
areas, the magnitude of these differences remains relatively small.

3.4.2 Housing bubble instrument

To gain exogenous variation in house price increases between 2000 and 2006 across three-digit zip areas, we follow Charles, Hurst, and Notowidigdo (2018) and instrument our standardized ZIP3-level house price increase variable $\widehat{\Delta P_c}$ with the magnitude of the structural break in ZIP3-level house prices. We interpret these structural breaks as housing bubbles, and our identification strategy builds on the assumption that the magnitude of these bubbles across ZIP3s was unrelated to changes in fundamentals that determine house prices. Our assumption is supported by the emerging consensus of the housing literature, which finds that speculative motivation was the primary driver of house price increases (Shiller, 2007; Burnside, Eichenbaum, and Rebelo, 2016; Charles, Hurst, and Notowidigdo, 2018).

Following Charles, Hurst, and Notowidigdo (2018), we quantify the structural break pa-

Figure 3.3: Independence of housing break instrument: Correlation with lagged outcomes, change between 1989-91 and 1995-97



Note: Each graph shows the change between 1989-91 and 1995-97 in the fraction of freshmen students entering majors by income and volatility categorization. Regressions are weighted with the 1990 population of ZIP3 areas.

rameter λ_c of each ZIP3 area by estimating the following regression using quarterly house price index data between 2000:I and 2005:IV:

$$P_c(t) = \omega_c + \tau_c\, t + \lambda_c\, (t - t_c^*)\, 1\{t > t_c^*\}$$

where $P_c(t)$ is the log of the quarterly FHFA house price index at ZIP3 area c. Variable t_c^* is the assumed quarter in which the structural break took place at ZIP3 area c, while t and $(t - t_c^*)$ are ZIP3-specific linear time trends before and after the structural break. To have sufficient observations before and after the structural break, we assume that the structural break occurred between 2001:I and 2005:I. We estimate the above regression with OLS for each ZIP3 separately for different structural break times t^*, and choose the specification that maximizes the model's fit to the data, measured byR^2. Finally, we interpret the estimated structural break parameter $\hat{\lambda}_c$ as the magnitude of the housing bubble before the 2008

financial crisis in ZIP3 area c, and we use it as an instrument for our standardized ZIP3-level house price increase variable $\widehat{\Delta P_c}$.

Our identification strategy requires that (1) our housing break instrument has a strong correlation with house price increases (relevance), and (2) our instrument affects college major choice only through house price change (exclusion restriction). Figure 3.2 depicts that there is a strong positive correlation between our structural break instrument λ_c and our standardized house price increase variable $\widehat{\Delta P_c}$. Regressing our structural break instrument on house price increase yields F-statistics over 190, which suggests that the relevance of our instrument holds. Our second assumption requires that our structural break parameter does not directly influence our outcome variables, and housing bubbles did not emerge in ZIP3s in which students tend to choose riskier or higher-paying college majors. We validate this assumption by analyzing the connection between our structural break parameter and the change in freshmen college major choice in the early 1990s. Finding a significant connection between these variables would undermine the validity of our instrument, as theoretically, a persistent trend in college major choice changes across different ZIP3 codes could have created different levels of housing bubbles by ZIP3 codes.

To analyze the existence of this channel, we restrict our TFS dataset to students entering college in waves 1989-91 or 1995-97 in institutions where we have observations in each year in this period. Each of these periods corresponds to a 10-year lag of what we previously referred to as the pre-boom (1999-2001) and the post-boom (2005-2007) periods in our main analysis. Then, using the survey weights, we calculate how the share of students enrolling in high-paying and high-volatility majors changed in each ZIP3 between our lagged pre- and post-period. Finally, we test whether any trends in ZIP3-level college major choices could predict the magnitude of housing bubbles in the 2000s. In Figure 3.3 we plot the relationship between our instrument and the ZIP3-level change in the share of students in high-paying and highly volatile majors between the lagged pre- and post-period. We find no evidence of any connection between changes in college major choice and our instrument, which suggests that the emergence of housing bubbles is unrelated to any trends that would motivate students to choose higher-paying or riskier majors.

3.4.3 College attendance and house price increase

Finally, before presenting our findings on the connection between house price increases and college major choices, we provide evidence that attendance in four-year colleges likely did not change with increasing house prices during the housing boom in the 2000s. Non-changing attendance in four-year institutions at this time ensures that our findings on college major

Figure 3.4: Change in share of young adults in college at MSA-level, 2000-2006



Note: Each graph shows the MSA-level change in the share of young high-school graduates in college between 2000 and 2006. College attendance in 2000 was based on the 5% census sample, while we use the combined 2005-2007 ACS samples to compute the share in college in 2006. Regressions are weighted with the MSA-level 2000 populations of given age categories.

choice were caused by changing preference toward majors, which we are interested in, and not by changing selection into college education with unchanged preference toward majors.

As the tabulation of college attendance by age is unavailable at the ZIP3 level, we analyze the connection between house price change and college attendance at the MSA level. We use the publicly available 5% sample of the 2000 decennial census to calculate the share of high school graduates in college education in 2000, while we use the 2005-2007 waves of the American Community Survey to compute college attendance among high school graduates in 2006. Due to data limitations, we cannot distinguish between two-year and four-year colleges in our dataset, and college attendance encompasses enrollment in both types of colleges.

We measure college attendance in two age groups. Our first group consists of students between 18 and 20, the most common age range for high school graduates entering college. With this group, we can investigate whether changes in house prices affect freshmen enrollment in colleges. Then, we extend this age window up to age 25, which measures how house

price changes affect the continuation of college education.

Figure 3.4 presents the connection between the change of college attendance, house price changes, and our housing bubble instrument between 2000 and 2006 at the MSA-level. We computed the house price change and structural break for each MSA in the same way we did for ZIP3 areas. The first row in Figure 3.4 shows that changes in college attendance seem to be unrelated to changes in house prices or our structural break instrument in the 18-20 age group. We do not find evidence of a significant connection between these changes, as the point estimates of the OLS regressions are highly insignificant (with p-values exceeding 0.7). These results suggest that changes in house prices did not affect enrollment in college education immediately after finishing high school.

The second row shows a stronger connection in the 18-25 age group, but the point estimates of the two-variable OLS regressions are not significant at the typically used significance levels. However, using a wider set of controls in a similar empirical setting, Charles, Hurst, and Notowidigdo (2018) finds that increasing housing demand negatively affects college enrollment among the 18-25 age group, suggesting that the housing boom may have influenced students to finish their college education earlier.

These results suggest that house price changes did not influence college enrollment upon finishing high school, but they may have decreased college attendance in later years. This potential connection between house price change and college attendance at a later age does not bias our results on college major choice for two reasons. First, we are interested in how the intended college majors of freshmen students change due to experiencing a house price increase, for whom the more relevant comparison group is high school graduates between the ages of 18 and 20. Among them, we do not find a connection between increasing house prices and college attendance, which suggests that the housing boom did not affect the selection into college education. Second, Charles, Hurst, and Notowidigdo (2018) finds that the effect of house price changes on college attendance is concentrated among students attending two-year institutions. Based on their results, attendance in four-year colleges was not affected by the housing boom, which motivated us to narrow down our sample to students entering four-year colleges.

In summary, our empirical findings on the connection between house price changes and college major choice should not be biased due to changes in the selection into college education. We do not find a systematic connection between college enrollment and house price change at the age when most high school graduates enter college. Furthermore, to ensure our results are not biased because of selection, we also narrow down our sample to freshmen students entering four-year colleges, whose enrollment was unaffected by house price increase based on Charles, Hurst, and Notowidigdo (2018).

Table 3.3: Effect of the 2000s housing boom on major choice, Full sample

	High-paying	High-vol.	L pay, L vol	L pay, H vol	H pay, L vol	H pay, H vol
Panel A: Diff-in-Diff						
Housing Shock x Post	0.007*	0.002	-0.004	-0.003	0.002	0.005***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.001)
Observations	651699	651699	651699	651699	651699	651699
CM	0.613	0.236	0.204	0.183	0.559	0.054
Panel B: Diff-in-Diff, Low homeownership areas						
Housing Shock x Post	0.021***	-0.002	-0.019*	-0.002	0.021***	-0.000
	(0.008)	(0.006)	(0.010)	(0.006)	(0.007)	(0.003)
Observations	105407	105407	105407	105407	105407	105407
CM	0.608	0.256	0.195	0.197	0.550	0.058
Panel C: Diff-in-Diff, High homeownership areas						
Housing Shock x Post	-0.010**	0.010**	0.006	0.004	-0.016***	0.006***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)
Observations	248818	248818	248818	248818	248818	248818
CM	0.619	0.232	0.202	0.179	0.566	0.052
Panel D: Diff-in-Diff-in-Diff						
Housing Shock x Post x High h.ownership	-0.026***	0.006	0.025**	0.002	-0.031***	0.004
	(0.009)	(0.007)	(0.011)	(0.007)	(0.009)	(0.003)
Observations	354225	354225	354225	354225	354225	354225
CM	0.613	0.236	0.204	0.183	0.559	0.054

Note: ZIP3-level controls include the log of population, share of college-educated adults between the ages of 25 and 64, share of foreign-borns, share of women in the labor force, all measured in 2000. Individual controls include indicators for gender, race, high-school GPA, college selectivity, and parental educational attainment. Observations are weighted with the student's weight from TFS. Robust standard errors are in parentheses, clustered at the ZIP3 level. CM displays the average of the outcome variables in the 2000 wave. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.5 Results on college major choice

3.5.1 Main results

Table 3.3 summarizes the overall results of our empirical analysis on the entire student body. In these regressions, our outcome variables are dummies representing whether a college student chooses a high-earning or highly volatile major, as well as the intersection of these two characteristics. Panel A shows that a one-standard-deviation increase in ZIP3-level house prices increases the likelihood of enrolling in a high-paying major by 0.7 percentage points, primarily due to increased participation in high-earning / highly volatile majors. Otherwise, the close-to-zero and insignificant point estimates suggest that differing house price increases did not have a significant impact on the choice of any other major categories or on selecting majors with higher income volatility.

Despite the modest effect of house price increases on major choice in the entire student body, we document significant differences in major selection based on the likelihood of parental homeownership. In Table 3.3's Panels B and C show how our baseline results differ among students living in low and high-homeownership areas, which we use as a proxy for parental homeownership. In Panel B, we restricted our sample to students from the bottom quarter of ZIP5 codes based on homeownership rates; these students should be the least af-

Table 3.4: Effect of the 2000s housing boom on major choice, Students from low-income families

	High-paying	High-vol.	L pay, L vol	L pay, H vol	H pay, L vol	H pay, H vol
Panel A: Diff-in-Diff						
Housing Shock x Post	0.009**	0.002	-0.008*	-0.001	0.006	0.003
	(0.004)	(0.004)	(0.005)	(0.003)	(0.004)	(0.002)
Observations	179427	179427	179427	179427	179427	179427
CM	0.591	0.232	0.225	0.185	0.543	0.047
Panel B: Diff-in-Diff, Low homeownership areas						
Housing Shock x Post	0.020**	-0.003	-0.019*	-0.001	0.022***	-0.002
	(0.008)	(0.009)	(0.011)	(0.008)	(0.008)	(0.004)
Observations	41129	41129	41129	41129	41129	41129
CM	0.617	0.233	0.200	0.182	0.567	0.051
Panel C: Diff-in-Diff, High homeownership areas						
Housing Shock x Post	-0.009	0.014**	0.001	0.009	-0.015*	0.006
	(0.008)	(0.007)	(0.007)	(0.007)	(0.008)	(0.004)
Observations	54671	54671	54671	54671	54671	54671
CM	0.581	0.231	0.234	0.185	0.535	0.046
Panel D: Diff-in-Diff-in-Diff						
Housing Shock x Post x High h.ownership	-0.025**	0.011	0.019	0.006	-0.030***	0.005
	(0.012)	(0.011)	(0.012)	(0.010)	(0.011)	(0.005)
Observations	95800	95800	95800	95800	95800	95800
CM	0.591	0.232	0.225	0.185	0.543	0.047

Note: Families are considered low-income if their total income was in the bottom 32.1 (wave 2000) or 35.5 (wave 2006) percent of the household income distribution. ZIP3-level controls include the log of population, share of college-educated adults between the ages of 25 and 64, share of foreign-borns, and share of women in the labor force, all measured in 2000. Individual controls include indicators for gender, race, high-school GPA, college selectivity, and parental educational attainment. Observations are weighted with the student's weight from TFS. Robust standard errors are in parentheses, clustered at the ZIP3 level. CM displays the average of the outcome variables in the 2000 wave. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

fected by house price changes through increased parental housing wealth. Our results show that, on average, house price increases motivated students from low-homeownership areas to choose higher-paying majors; meanwhile, the average volatility of their chosen majors remained unchanged. We find that a standard deviation increase in ZIP3-level house prices rises participation in high-paying / low-volatility majors by 2.1, and decreases enrollment in low-paying / low-volatility majors by 1.9 percentage points. These results suggest that experiencing higher house price increases motivates students to choose majors with higher earning potential in the case of lower parental housing wealth, which is in line with the expectations we presented in section 3.2.

Meanwhile, we find that the college major selection of students with a high probability of parental homeownership differed substantially from the major choice of students from low-homeownership areas. Panel C shows the effect on students from ZIP5 codes with the highest 25% of homeownership rates; these students' parents were the most likely to be homeowners. Counter to students in Panel B, a one standard deviation increase in ZIP3-level house prices decreased enrollment in high-paying majors by 1.0 percentage points, and increased enrollment in highly volatile majors by 1.0 percentage points. We find that the main source of these changes is the decrease in enrollment in high-earning / low-volatility

Table 3.5: Effect of the 2000s housing boom on major choice, Students from middle-income families

	High-paying	High-vol.	L pay, L vol	L pay, H vol	H pay, L vol	H pay, H vol
Panel A: Diff-in-Diff						
Housing Shock x Post	0.006	0.001	-0.002	-0.004	0.001	0.005***
	(0.005)	(0.003)	(0.004)	(0.003)	(0.005)	(0.001)
Observations	380648	380648	380648	380648	380648	380648
CM	0.615	0.232	0.206	0.180	0.562	0.053
Panel B: Diff-in-Diff, Low homeownership areas						
Housing Shock x Post	0.025**	-0.006	-0.019	-0.006	0.025**	-0.000
	(0.012)	(0.006)	(0.012)	(0.005)	(0.012)	(0.003)
Observations	51562	51562	51562	51562	51562	51562
CM	0.597	0.258	0.205	0.198	0.537	0.060
Panel C: Diff-in-Diff, High homeownership areas						
Housing Shock x Post	-0.011**	0.008*	0.009**	0.003	-0.017***	0.006***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.002)
Observations	152266	152266	152266	152266	152266	152266
CM	0.620	0.227	0.203	0.177	0.570	0.051
Panel D: Diff-in-Diff-in-Diff						
Housing Shock x Post x High h.ownership	-0.033**	0.008	0.030**	0.003	-0.037***	0.005
	(0.014)	(0.008)	(0.014)	(0.007)	(0.014)	(0.004)
Observations	203828	203828	203828	203828	203828	203828
CM	0.615	0.232	0.206	0.180	0.562	0.053

Note: Families are considered middle-income if their total income was between the 32.2 and 86.9 (wave 2000) or 35.6 and 88.5 (wave 2006) percentiles of the household income distribution. ZIP3-level controls include the log of population, share of college-educated adults between the ages of 25 and 64, share of foreign-borns, and share of women in the labor force, all measured in 2000. Individual controls include indicators for gender, race, high-school GPA, college selectivity, and parental educational attainment. Observations are weighted with the student's weight from TFS. Robust standard errors are in parentheses, clustered at the ZIP3 level. CM displays the average of the outcome variables in the 2000 wave. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

majors, which decreases by 1.6 percentage points in response to a one-standard-deviation increase in house prices. At the same time, rising house prices increase participation in all three other major categories, but this increase is statistically significantly different from zero only in the case of high-paying / highly volatile majors.

These results suggest that house price increases likely have a different effect on major choices based on parental homeownership, just as we suggested in section 3.2. Experiencing growing house prices motivated students with low likelihood of parental housing wealth to choose a major that provides higher earning potential, while students with likely higher parental housing wealth pursue majors with lower earning potential and higher volatility. In Panel D, we investigate this finding in our triple-difference setting, which tests whether the major choice of students living in low- and high-homeownership areas is statistically different from each other. Our results show that, in the case of a standard deviation increase in ZIP3-level house prices, enrollment in high-earning majors is 2.6 percentage points lower among students living in high-homeownership areas compared to students living in low-homeownership ZIP5 codes, which difference is statistically significant. Similarly, we find that, compared to students living in low-homeownership areas, students from high-homeownership ZIP5 codes are statistically less likely to enroll in high-paying / low-volatility

Table 3.6: Effect of the 2000s housing boom on major choice, Students from high-income families

	High-paying	High-vol.	L pay, L vol	L pay, H vol	H pay, L vol	H pay, H vol
Panel A: Diff-in-Diff						
Housing Shock x Post	-0.004	0.003	0.006	-0.002	-0.009	0.005*
	(0.006)	(0.005)	(0.005)	(0.004)	(0.006)	(0.003)
Observations	91624	91624	91624	91624	91624	91624
CM	0.665	0.269	0.143	0.192	0.588	0.076
Panel B: Diff-in-Diff, Low homeownership areas						
Housing Shock x Post	0.009	0.009	-0.011	0.002	0.001	0.008
	(0.012)	(0.012)	(0.010)	(0.011)	(0.013)	(0.007)
Observations	12716	12716	12716	12716	12716	12716
CM	0.618	0.359	0.115	0.267	0.525	0.093
Panel C: Diff-in-Diff, High homeownership areas						
Housing Shock x Post	-0.012	0.013*	0.006	0.006	-0.020**	0.007**
	(0.008)	(0.007)	(0.006)	(0.006)	(0.008)	(0.004)
Observations	41881	41881	41881	41881	41881	41881
CM	0.673	0.252	0.146	0.182	0.603	0.070
Panel D: Diff-in-Diff-in-Diff						
Housing Shock x Post x High h.ownership	-0.018	0.001	0.016	0.002	-0.017	-0.001
	(0.015)	(0.014)	(0.013)	(0.012)	(0.016)	(0.007)
Observations	54597	54597	54597	54597	54597	54597
CM	0.665	0.269	0.143	0.192	0.588	0.076

Note: Families are considered high-income if their total income was in the top 13.0 (wave 2000) or 11.4 (wave 2006) percent of the household income distribution. ZIP3-level controls include the log of population, share of college-educated adults between the ages of 25 and 64, share of foreign-borns, and share of women in the labor force, all measured in 2000. Individual controls include indicators for gender, race, high-school GPA, college selectivity, and parental educational attainment. Observations are weighted with the student's weight from TFS. Robust standard errors are in parentheses, clustered at the ZIP3 level. CM displays the average of the outcome variables in the 2000 wave. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

and more likely to select low-paying / low-volatility majors. Our triple difference regression results reinforce our findings in Panels B and C, suggesting that parental homeownership and housing wealth influence students' major choices in the case of increasing house prices.

3.5.2 Heterogeneity

Now, we focus our attention on how the effect of a home price increase on major choice differs by family income and race. We quantify these effects by narrowing down our samples to the given subpopulation of interest.

Family income

To investigate whether house price increase has heterogeneous effects on different parts of the family income distribution, we divide our sample into three distinct subgroups. In these regressions, we narrow our sample down to students who are roughly in the bottom 35%, the top 10%, or in the middle 55% of the family income distribution in their respective survey years. Based on our discussion in section 3.2, we expect the magnitude of our results to be a non-monotone increasing function of family income. Students from higher-income

Table 3.7: Effect of the 2000s housing boom on major choice, White students

	High-paying	High-vol.	L pay, L vol	L pay, H vol	H pay, L vol	H pay, H vol
Panel A: Diff-in-Diff						
Housing Shock x Post	0.004	0.007**	-0.003	-0.001	-0.004	0.007***
	(0.005)	(0.003)	(0.005)	(0.003)	(0.006)	(0.001)
Observations	484283	484283	484283	484283	484283	484283
CM	0.589	0.242	0.222	0.189	0.536	0.052
Panel B: Diff-in-Diff, Low homeownership areas						
Housing Shock x Post	0.026*	-0.009	-0.016	-0.010	0.024	0.001
	(0.015)	(0.010)	(0.018)	(0.010)	(0.015)	(0.004)
Observations	39841	39841	39841	39841	39841	39841
CM	0.548	0.296	0.219	0.233	0.485	0.063
Panel C: Diff-in-Diff, High homeownership areas						
Housing Shock x Post	-0.014***	0.018***	0.005	0.009*	-0.023***	0.009***
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.002)
Observations	227181	227181	227181	227181	227181	227181
CM	0.601	0.232	0.217	0.182	0.551	0.050
Panel D: Diff-in-Diff-in-Diff						
Housing Shock x Post x High h.ownership	-0.038*	0.015	0.029	0.009	-0.043**	0.006
	(0.021)	(0.012)	(0.025)	(0.011)	(0.021)	(0.005)
Observations	267022	267022	267022	267022	267022	267022
CM	0.589	0.242	0.222	0.189	0.536	0.052

Note: ZIP3-level controls include the log of population, share of college-educated adults between the ages of 25 and 64, share of foreign-borns, share of women in the labor force, all measured in 2000. Individual controls include indicators for gender, high-school GPA, college selectivity, and parental educational attainment. Observations are weighted with the student's weight from TFS. Robust standard errors are in parentheses, clustered at the ZIP3 level. CM displays the average of the outcome variables in the 2000 wave. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

homeowner families are expected to be more likely to switch to majors with lower earnings and/or a more volatile income profile. In contrast, students without parental homeownership are more likely to switch to higher-paying and less volatile majors in the case of lower parental income.

Table 3.4 shows that the effect of increasing house prices on the major choice of students from the bottom 35% of the family income distribution is quite similar to what we find in the whole sample. The main difference is that, compared to our results for the entire student body, students from lower-income families decreased their enrollment by 0.8 percentage points in lower-paying / low-volatility majors when experiencing a one-standard-deviation increase in house prices (Panel A). Similar to our findings on the entire sample, in areas with low homeownership rates, increasing house prices motivated students from low-income families to choose majors with higher earning potential and lower income volatility (Panel B). Just as in our overall sample, low-income students from high homeownership ZIP5 areas chose more volatile and lower-paying majors in the case of higher house price increases (Panel C), while the observed differences in major choice across areas with different homeownership rates are statistically significant (Panel D). Overall, the direction and magnitude of these point estimates are quite similar to those found in our overall sample.

Table 3.5 shows that our results on students in the middle of the family income dis-

Table 3.8: Effect of the 2000s housing boom on major choice, non-White students

	High-paying	High-vol.	L pay, L vol	L pay, H vol	H pay, L vol	H pay, H vol
Panel A: Diff-in-Diff						
Housing Shock x Post	0.015***	-0.004	-0.010***	-0.005*	0.014***	0.001
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)
Observations	167416	167416	167416	167416	167416	167416
CM	0.672	0.223	0.161	0.167	0.616	0.056
Panel B: Diff-in-Diff, Low homeownership areas						
Housing Shock x Post	0.022***	-0.005	-0.016***	-0.006	0.021***	0.001
	(0.006)	(0.004)	(0.005)	(0.005)	(0.005)	(0.003)
Observations	90112	90112	90112	90112	90112	90112
CM	0.661	0.226	0.170	0.169	0.604	0.057
Panel C: Diff-in-Diff, High homeownership areas						
Housing Shock x Post	-0.001	0.005	0.002	-0.001	-0.007	0.007
	(0.008)	(0.007)	(0.006)	(0.006)	(0.009)	(0.004)
Observations	28555	28555	28555	28555	28555	28555
CM	0.726	0.217	0.121	0.152	0.662	0.065
Panel D: Diff-in-Diff-in-Diff						
Housing Shock x Post x High h.ownership	-0.020**	0.008	0.017**	0.003	-0.025**	0.005
	(0.009)	(0.008)	(0.007)	(0.007)	(0.010)	(0.005)
Observations	118667	118667	118667	118667	118667	118667
CM	0.672	0.223	0.161	0.167	0.616	0.056

Note: ZIP3-level controls include the log of population, share of college-educated adults between the ages of 25 and 64, share of foreign-borns, share of women in the labor force, all measured in 2000. Individual controls include indicators for gender, high-school GPA, college selectivity, and parental educational attainment. Observations are weighted with the student's weight from TFS. Robust standard errors are in parentheses, clustered at the ZIP3 level. CM displays the average of the outcome variables in the 2000 wave. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

tribution are also similar to what we find among lower-income students and on the whole student body. In line with our expectations, the point estimates for middle-income students are somewhat larger in magnitude compared to low-income students, among both students living in low- and high-homeownership areas (Panels B and C, respectively). Because of these differences, the estimated effects in our triple difference specifications (Panel D) are also greater and statistically stronger than among lower-income students. These results suggest that, in general, house price changes had a slightly greater influence on students in the middle of the family income distribution than on those at the bottom. Meanwhile, Table 3.6 shows that the major choice of students from the highest-income families is less affected by ZIP3-level house price changes. Our point estimates for high-income students from high homeownership areas are similar to those found among low- and middle-income students residing in high homeownership areas (Panel C). However, it is worth noting that the magnitude of the decrease in enrollment in high-paying / low-volatility majors increases with income (-1.7 percentage points for low-income, -2.0 percentage points for middle-income, and -2.3 percentage points for high-income students). Meanwhile, the behavior of high-income students living in areas with low homeownership rates differs significantly from what we find for low- and middle-income families (Panel B). We do not find evidence on increasing enrollment in higher-paying, and especially in higher-paying / low-volatility majors, among

high-income students, which was a consistent finding for students in the lower 90% of the family income distribution. While our point estimates among high-income students in Panel B are not precise, these results suggest that high-income students are less motivated to enroll in higher-paying majors due to experiencing a larger increase in house prices.

Overall, we find that house price increases affect students similarly in areas with high homeownership rates. In the case of experiencing a house price increase, students in these areas tend to choose higher volatility majors, and decrease their enrollment in high-paying / lower-volatility majors. We also document that the magnitude of the decrease in enrollment in high-paying majors, especially in high-paying / low-volatility majors, slightly increases with income. This suggests that students from higher-income families are more likely to switch to lower-paying majors, highlighting the role of parental income in providing insurance against unfavorable earnings outcomes. We also find that the increasing enrollment in higher-paying majors in areas with low homeownerhip rates is less prominent among the highest-income students.

Race

Next, we divide our sample by race and examine whether the increase in house prices has a different effect on the college major choice of White and non-White students. Instead of using the overall homeownership rate, in these regressions we define low and high homeownership areas based on the homeownership rates of White and non-White families in each ZIP5 code. The 2001 wave of the Survey of Consumer Finances reveals that, compared to White households, non-White households have a lower homeownership rate (75.1% vs. 47.0%), average income ($97,518 vs. $54,350), and median wealth ($163,801 vs. $23,303). These differences suggest that non-White students are less likely to live in households with higher incomes and parental homeownership. Based on our discussion in section 3.2, in the case of increasing house prices, we expect to find a shift towards higher-earning and less-volatile majors among non-White students, and an increase in enrollment in lower-earning and more volatile majors among White students.

Our results align with these predictions and demonstrate that White and non-White students make different major choice decisions in response to a house price increase. Panel A of Table 3.7 shows that, counter to the whole student body, White students' enrollment statistically significantly increases in highly volatile, but not in high-earning majors in the case of house price increases. These differences are mainly explained by the behaviour of White students living in high-homeownership areas. As Panel B shows, our point estimates for White students residing in low-homeownership ZIP codes are similar to our results on the entire student body; the small sample size may explain the imprecision of these results.

Meanwhile, the magnitude of our results for White students from high-homeownership areas (Panel C) is larger than that we find in the overall student body (Table 3.3). Compared to the results on all students living in high-homeownership areas (Panel C in Table 3.3), in the case of higher house price increase, White students are less likely to enroll in high-paying (-1.4 percentage points vs. -1.0 percentage points in Table 3.3), and especially in high-paying / low-volatility majors (-2.3 percentage points vs. -1.6 percentage points), while they are more likely to enroll in majors with higher volatility (1.8 percentage points vs 1.0 percentage points). As a result, the magnitude of the White students' triple difference point estimates becomes larger, albeit less precise, than our point estimates on the entire student body.

As Table 3.8 shows, the overall effect of non-White students is quite different from what we find among White students. As with the entire student body, house price increases led to an increase in enrollment in high-paying majors among non-White students. Panel A shows that a one-standard-deviation increase in ZIP3-level home prices resulted in a 1.5 percentage point increase in the enrollment of non-White students in high-paying majors, which is more than double the increase observed in the full sample. Meanwhile, in the case of house price increases, enrollment of non-White students decreased in low-paying / low-volatility majors (point estimate of -1 percentage points), and increased in high-paying / low-volatility majors (1.4 percentage points); these results are smaller and not statistically significant on the whole sample. Panels B and C indicate that these overall effects can be attributed to two distinct factors. First, more than three times as many non-White students lived in low-homeownership areas, so their major choice patterns significantly influence the overall effect on non-White students. The point estimates of non-White students in low-homeownership areas (Panel B) are quite similar to what we find for White students and for the entire student body. Meanwhile, the magnitude of the estimated effects on non-White students in high-homeownership areas (Panel C) are much less than we find for White students, and they are not statistically different from zero. This suggests that switching to lower-earning and more volatile majors in the case of experiencing house price increase is less prevalent among non-White students.

In summary, we find a significant overall difference between White and non-White students. In the case of experiencing increasing ZIP3-level house prices, White students increased their enrollment in higher-volatility majors, while non-White students switched to higher-paying majors. The main source of these differences is compositional: a much larger share of non-White students lived in low-homeownership areas than White students, thus the overall effect on non-White students should be closer to the behaviour of students living in low-homeownership areas. We find that the effect of house price increase on White and non-White students from low-homeownership areas is very similar: both of them increase

participation in majors with higher earning potential in nearly the same magnitude. However, while in high-homeownership areas house price increases motivated White students to switch to lower-earning and higher volatility majors, we do not find a similar effect on non-White students.

3.6 Conclusion

In this paper, we study the connection between changing house prices and college major choice. We analyse this connection empirically using a difference-in-difference model on the CIRP Freshmen Survey microdata and aggregated earnings characteristics of majors. In our empirical specification, we compare how first-year students experiencing a substantial increase in three-digit ZIP code-level house prices between 2000 and 2006 changed their college major compared to students living in ZIP3 areas with a lower price increase. We provide the first evidence of a causal relationship between home price change and major choice by instrumenting the changing house prices with the structural break in ZIP3-level house price indexes. Our identification strategy builds on the consensus of the housing literature, which argues that the structural breaks were created by speculative forces rather than changes in economic fundamentals. In this case, our structural break instrument is uncorrelated with potential patterns in major choice between different ZIP3 areas.

Overall, we find that a one-standard-deviation increase in ZIP3-level home prices increased freshmen enrollment in high-paying majors by 0.7 percentage points. These changes occurred mainly due to the increasing enrollment of students from lower-homeownership areas in high-paying / low-volatility majors, which include most of the STEM fields, Business, and healthcare-related majors. We show that house price increases affect students differently depending on the likelihood of parental homeownership. We find that students living in low-homeownership areas, where their parents are less likely to be homeowners, increased their participation in higher-paying / less-volatile majors by 2.1 percentage points, and decreased their enrollment in lower-paying / low-volatility majors by 1.9 percentage points due to a one-standard-deviation increase in house prices. Meanwhile, a similar rise in house prices decreased the enrollment of students from high-homeownership in high-paying majors by 1.0 percentage point, mainly through the declining participation in high-income / low-volatility majors. Overall, we find that house price increases motivated students from low-homeownership areas to switch to higher-paying and less-volatile majors. In contrast, students with a high probability of parental homeownership were more likely to enroll in lower-paying and/or more volatile majors.

Then, we study how these effects change with increasing parental income and race. We

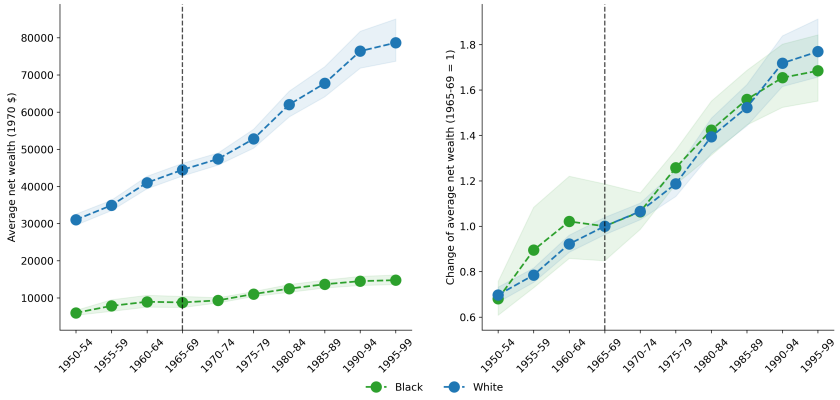
show that the magnitude of the impact of house price growth slightly increases with parental income in both low- and high-homeownership areas, except for the highest-income students, whose major choice seems to depend less on house price increases. We find that house price increases have the opposite effect on the major choice of White and non-White students. White students tend to increase their enrollment in lower-paying and/or highly-volatile majors in the case of incresing house prices, while non-White students switch to higher-paying and/or less-volatile majors. We argue that one of the primary sources of this difference is compositional, as White students are more likely to live in areas with high homeownership rates. Our results suggest that White and non-White students living in low-homeownership areas behave similarly when experiencing a house price increase. Meanwhile, we do not find evidence of increased participation in lower-earning or more volatile majors for non-White students residing in high-homeownership areas, which is quite prevalent among White students.

In summary, our empirical results underscore the importance of parental homeownership in college major choice. When house prices increase, students from families with a higher probability of parental homeownership are likelier to choose college majors with lower-earning and more volatile earning profiles. Meanwhile, we find that students whose parents are less likely to be homeowners behave in the opposite way in response to the same house price increase, as they increase their enrollment in higher-earning and less volatile majors. In future works, we intend to connect this finding with intergenerational income mobility and wealth inequality.

APPENDIX A

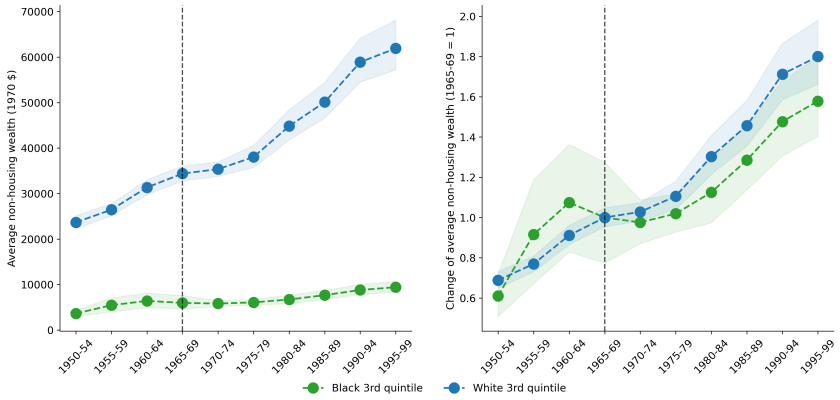
Appendix: Chapter 1

Figure A.1: Evolution of average net wealth of the Black and White wealth distribution



Source: own calculation, based on SCF+ data. Net wealth is defined as the difference between total asset value minus existing debt. Prices measured in 1970 dollars. Transparent areas represent bootstrapped 95% confidence intervals.

Figure A.2: Evolution of average non-housing wealth of the Black and White wealth distribution



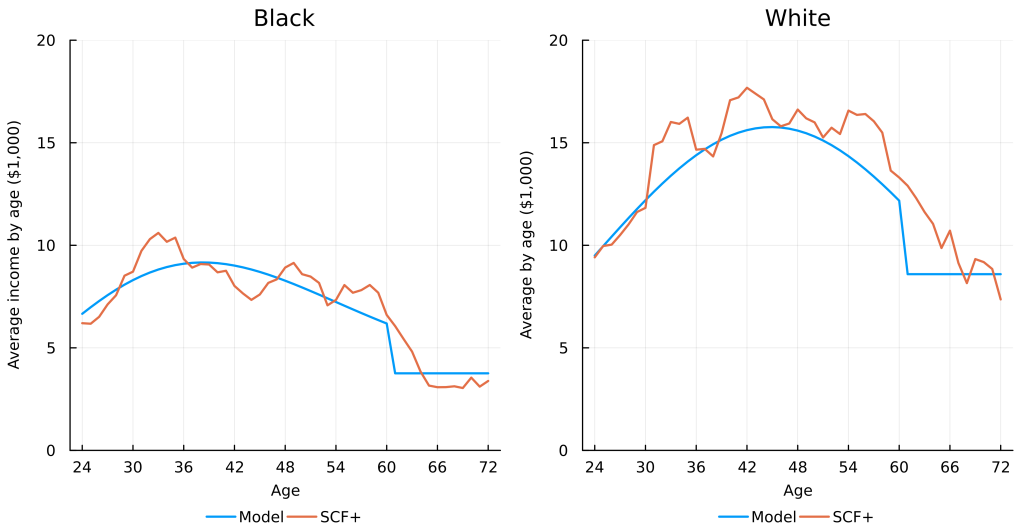
Source: own calculation, based on SCF+ data. Non-housing wealth is the difference between total net wealth and housing wealth, which is defined as asset value minus the remaining mortgage of the first residence. Prices measured in 1970 dollars. Transparent areas represent bootstrapped 95% confidence intervals.

Table A.1: Calibrated parameters of the income process

	Black income distribution	**White income distribution**
Life-cycle elements		
Age^0	5.879769	7.155332
Age^1	.1877262	.1076763
Age^2	-.0036298	-.0012546
Age^3	.0000205	8.21e-07
Income fixed effect ρ_R		
Low	-.8481999	-.6473573
Medium	.0899008	.0929006
High	.7667469	.5564716
Temporary income shock parameter with Low ρ_R: $\epsilon_{\rho=L,R}$		
Low	-.3672556	-.2847186
Medium	.0019360	.00606490
High	.3653196	.2787603
Temporary income shock parameter with Medium ρ_R: $\epsilon_{\rho=M,R}$		
Low	-.2831905	-.2100579
Medium	.0118372	-.0003271
High	.2717945	.2105470
Temporary income shock parameter with High ρ_R: $\epsilon_{\rho=H,R}$		
Low	-.2307297	-.2016939
Medium	.0115538	.00829980
High	.2215885	.1935596

Notes: Own calculation based on PSID and SCF+ data. See text for more details.

Figure A.3: Fit of calibrated income distribution



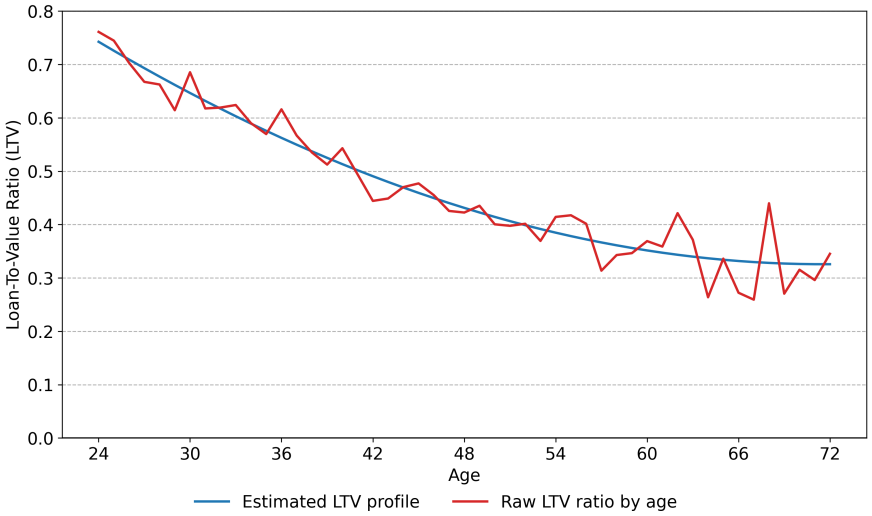
Source: own calculation, based on SCF+ and PSID data. Income is defined as total familiy income. Prices measured in 1970 dollars.

Table A.2: Marginal tax rates in 1970 for married couples filling jointly

Over	But not over	Marginal tax rate	Over	But not over	Marginal tax rate
0	1000	14%	40,000	44,000	48%
1,000	2,000	15%	44,000	52,000	50%
2,000	3,000	16%	52,000	64,000	53%
3,000	4,000	17%	64,000	76,000	55%
4,000	8,000	19%	76,000	88,000	58%
8,000	12,000	22%	88,000	100,000	60%
12,000	16,000	25%	100,000	120,000	62%
16,000	20,000	28%	120,000	140,000	64%
20,000	24,000	32%	140,000	160,000	66%
24,000	28,000	36%	160,000	180,000	68%
28,000	32,000	39%	180,000	200,000	69%
32,000	36,000	42%	200,000		70%
36,000	40,000	45%			

Source: 1970 Instructions for Form 1040, Schedule Y, https://www.irs.gov/pub/irs-prior/i1040--1970.pdf, accessed at 10/24/2024.

Figure A.4: Estimated LTV ratio



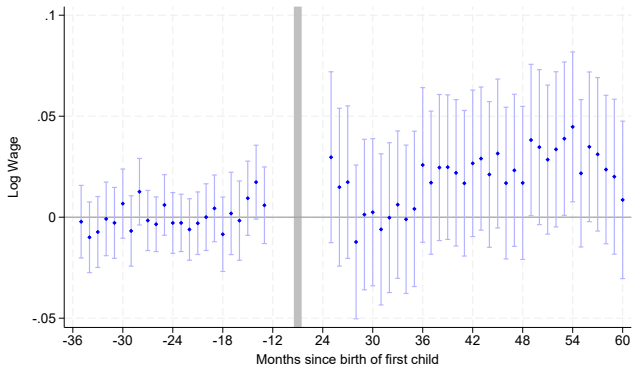
Source: own calculation, based on SCF+. Estimated cubic function: $LTV = (123.8819 - 2.425384 \times age + 0.0142931 \times age^2 + 0.0000247 \times age^3)/100$.

APPENDIX B

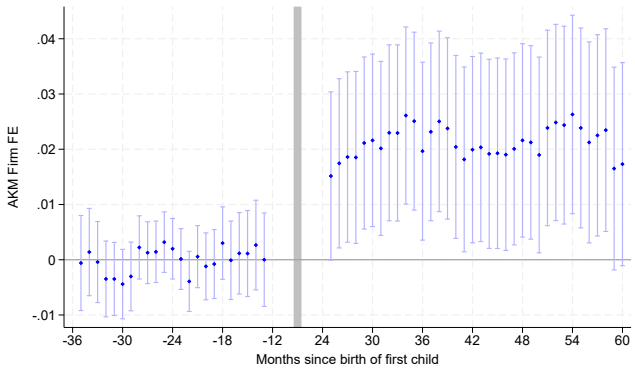
Appendix: Chapter 2

Figure B.1: Effect of CB+W-Eligibility on Job Characteristics, Matched Sample

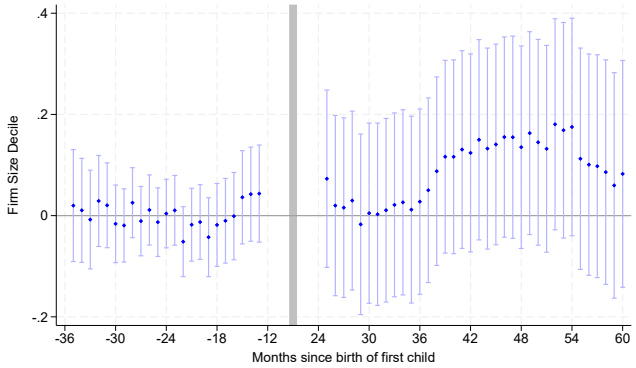
(a) Log Hourly Wage



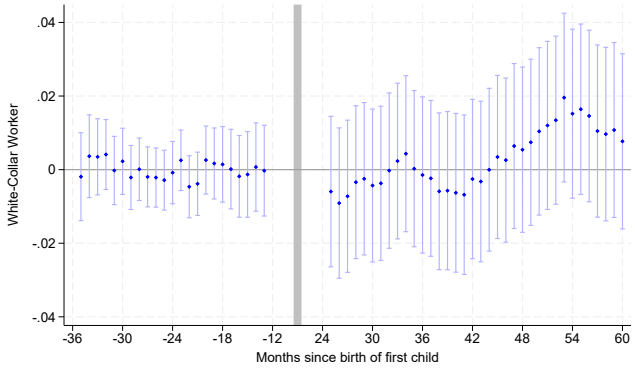
(b) AKM Firm FE



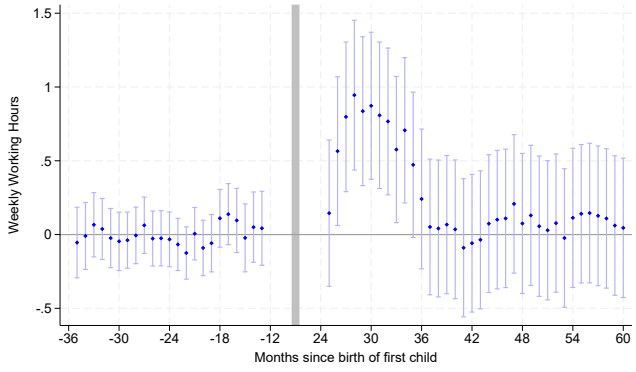
(c) Firm Size Decile



(d) Employment in White-Collar Job



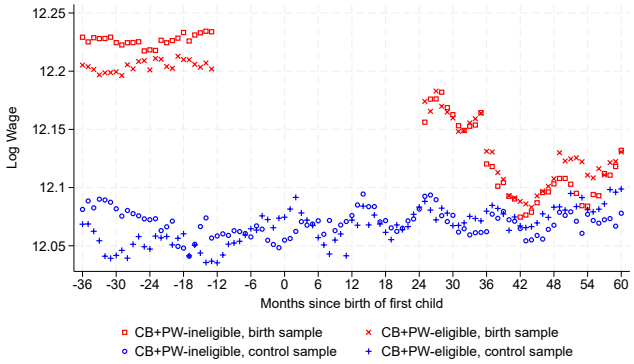
(e) Weekly Hours of Work



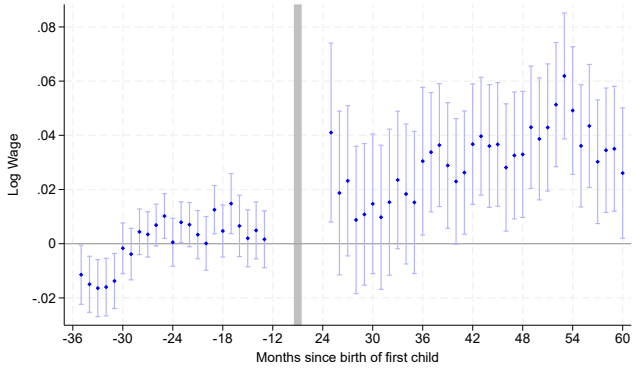
Notes: Sample is the matched sample, as described in Section 2.4.2. This figure shows the Event Study coefficient estimates (δ_k coefficients from equation (2.3)). Bars are 95 percent confidence intervals based on robust standard errors.

Figure B.2: Logarithm of Hourly Wage: Descriptive Patterns and DD Estimates

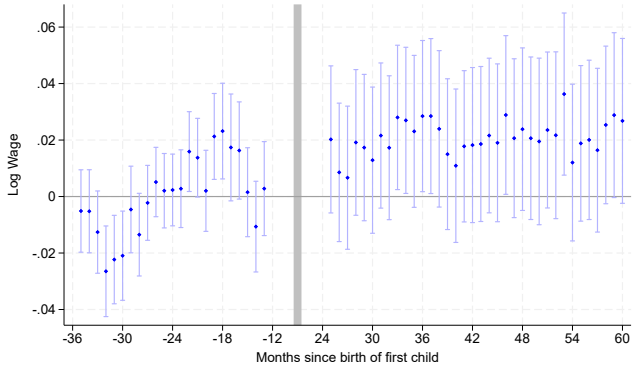
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



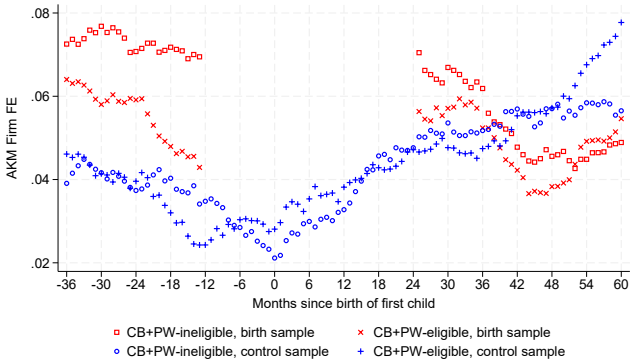
(c) Difference-in-differences (DD); control sample



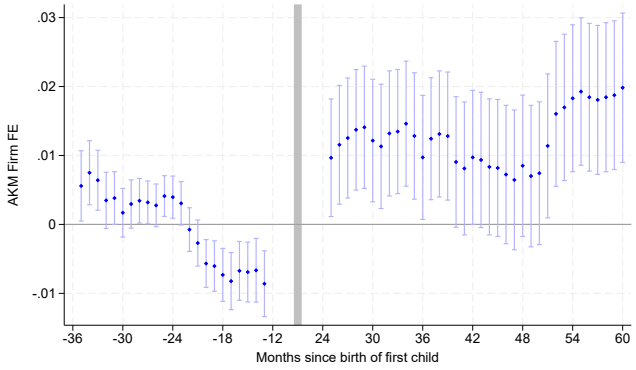
Notes: Sample is the baseline sample as described in Section 2.4.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\hat{\beta}_k$'s from model (2.2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Figure B.3: AKM Firm FE: Descriptive Patterns and DD Estimates

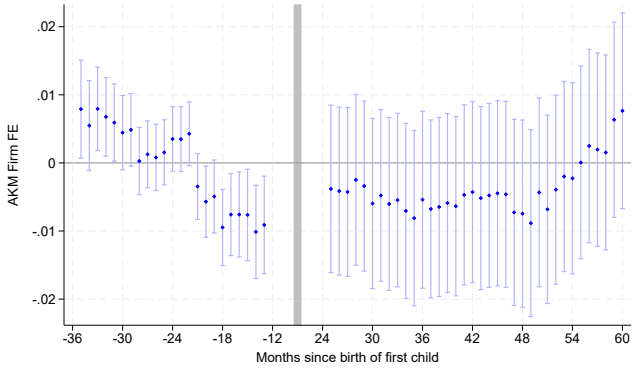
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



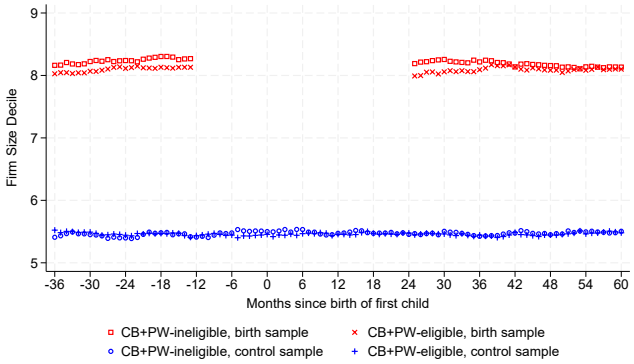
(c) Difference-in-differences (DD); control sample



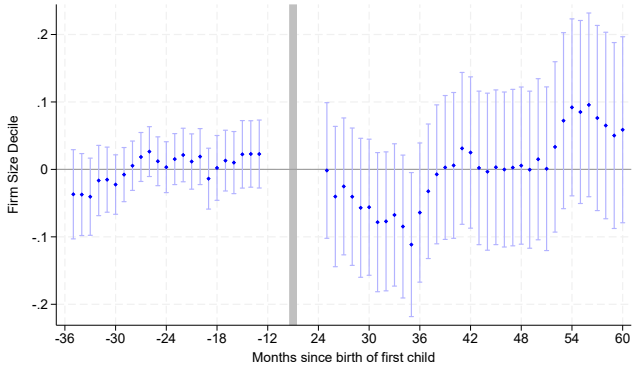
Notes: Sample is the baseline sample as described in Section 2.4.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated β_k's from model (2.2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Figure B.4: Firm Size Decile: Descriptive Patterns and DD Estimates

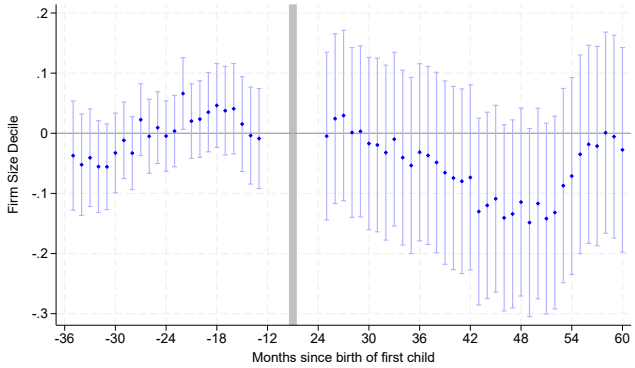
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



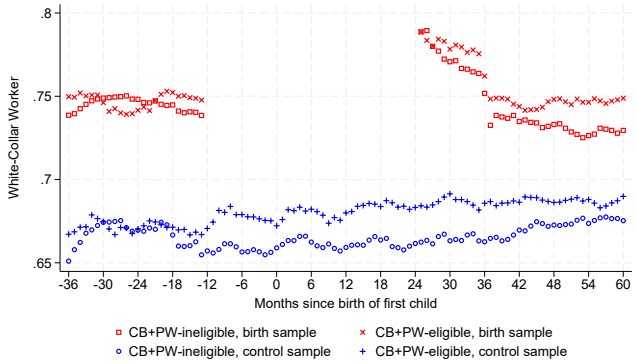
(c) Difference-in-differences (DD); control sample



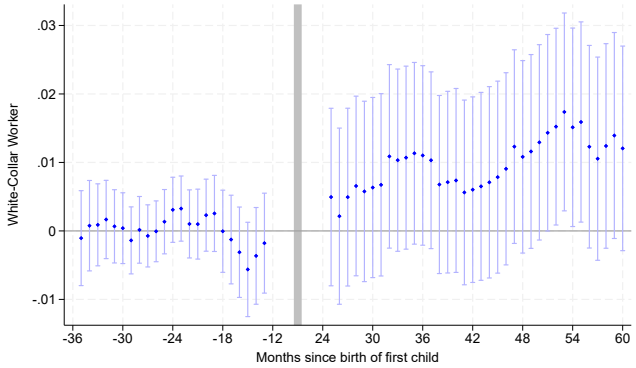
Notes: Sample is the baseline sample as described in Section 2.4.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\hat{\beta}_k$'s from model (2.2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Figure B.5: Employment in White Collar Job: Descriptive Patterns and DD Estimates

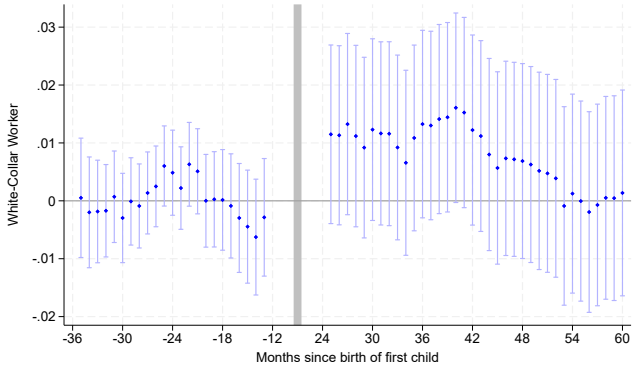
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



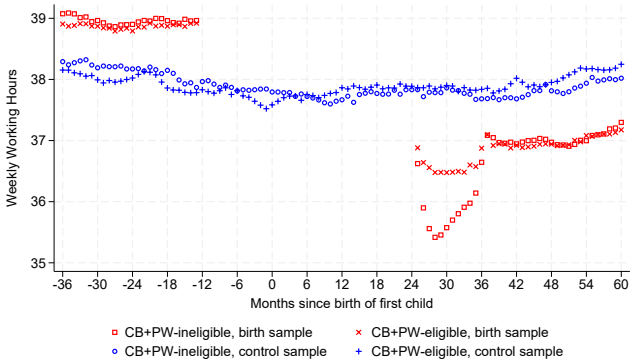
(c) Difference-in-differences (DD); control sample



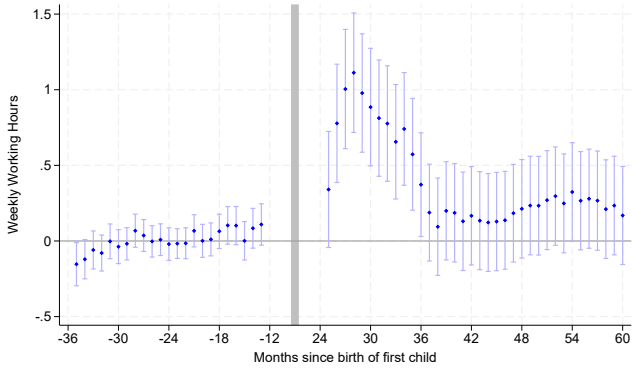
Notes: Sample is the baseline sample as described in Section 2.4.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated β_k's from model (2.2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Figure B.6: Weekly Working Hours: Descriptive Patterns and DD Estimates

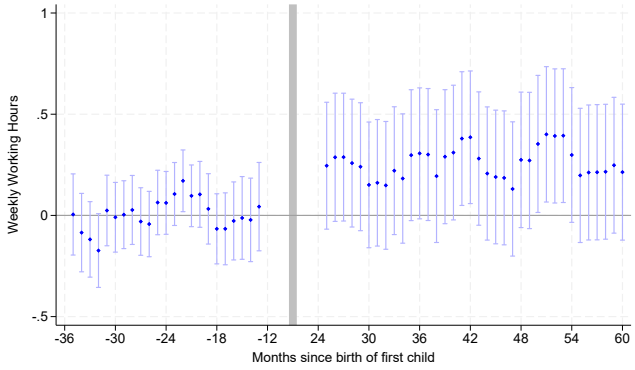
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



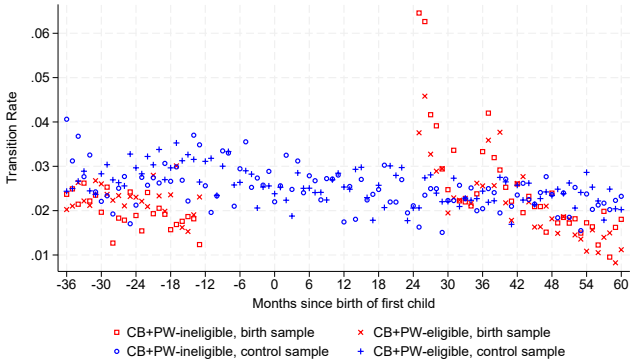
(c) Difference-in-differences (DD); control sample



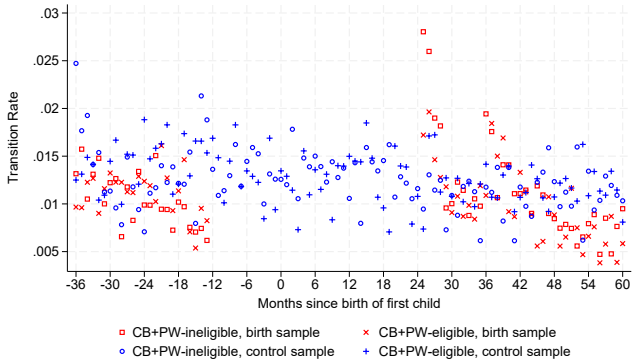
Notes: Sample is the baseline sample as described in Section 2.4.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\hat{\beta}_k$'s from model (2.2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Figure B.7: Transition Rate Between Employers

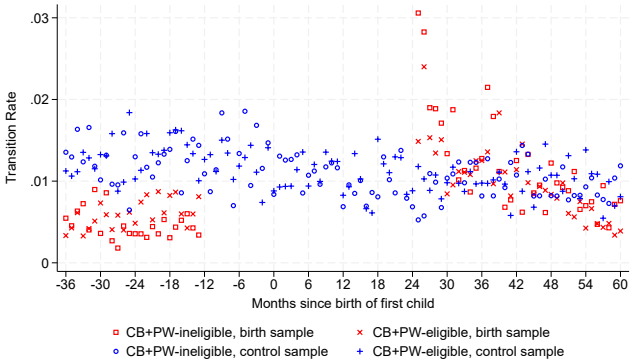
(a) Any Transition



(b) Transition to Higher AKM Firm FE Firm



(c) Transition to Lower AKM Firm FE Firm



Sample is the baseline sample as described in Section 2.4.2.

APPENDIX C

Appendix: Chapter 3

Table C.1: Earning potential and income volatility of majors

	Avg. residual	P25	P50	P90	Mean	SD	SD/Mean	SD/Mean (std)
Low-paying, Low volatility majors	-11,734	11,928	38,485	91,879	45,866	51,890	1.131	-0.443
Low-paying, High volatility majors	-9,109	6,154	33,230	107,561	49,134	67,929	1.381	1.303
High-paying, Low volatility majors	8,961	22,011	54,933	145,476	71,822	81,316	1.133	-0.427
High-paying, High volatility majors	8,201	11,794	44,590	148,927	69,537	93,290	1.342	1.034

Source: Table D2 and D3 in Hampole (2022), own calculation.

Table C.2: Earning potential and income volatility of majors

	N	Avg. residual	P25	P50	P90	Mean	SD	SD/mean	SD/Mean (std)	High-paying	High vol.
Agriculture	54,203	-14,551	12,000	40,000	110,000	52,753	62,031	1.176	-0.127	0	0
Architecture	28,823	-8,198	11,500	48,000	120,000	58,543	66,842	1.142	-0.365	0	0
Area Ethnic and Civilization Studies	13,566	2,102	7,200	35,450	125,000	55,798	79,317	1.422	1.587	1	1
Biology and Life Sciences	224,085	19,728	14,000	50,000	201,000	84,785	110,323	1.301	0.747	1	0
Business	927,466	4,390	20,000	50,000	135,000	66,628	79,579	1.194	0.002	1	0
Communications	200,275	-4,934	11,200	40,000	110,000	52,979	65,712	1.240	0.323	0	0
Computer and Information Sciences	146,201	7,443	28,800	65,000	144,000	74,841	72,377	0.967	-1.584	1	0
Criminal Justice and Fire Protection	87,316	-11,986	20,000	42,000	100,000	50,798	49,824	0.981	-1.488	0	0
Education Administration and Teaching	340,408	-14,834	9,000	35,000	72,000	36,930	39,812	1.078	-0.810	0	0
Engineering	288,313	17,298	34,500	75,000	160,000	86,362	83,227	0.964	-1.607	1	0
Engineering Technologies	33,260	-4,988	30,000	61,000	130,000	69,351	63,715	0.919	-1.921	0	0
English Lang Lit and Comp	117,561	-6,255	6,000	35,000	116,000	52,766	75,013	1.422	1.587	0	1
Environment and Natural Resources	31,210	-12,662	17,000	43,000	100,000	52,310	56,901	1.088	-0.742	0	0
Family and Consumer Sciences	65	-12,820	3,000	35,000	90,000	39,728	35,453	0.892	-2.104	0	0
Fine Arts	191,799	-18,325	3,100	28,000	90,000	39,411	53,559	1.359	1.150	0	1
History	78,114	-1,294	12,000	42,000	150,000	66,735	91,211	1.367	1.205	0	1
Interdisc and Multi-Disc Studies	36,964	-2,118	9,000	36,000	109,000	51,037	69,181	1.355	1.126	0	1
Law	8,507	-4,720	10,000	40,000	110,000	52,341	66,751	1.275	0.567	0	0
Liberal Arts and Humanities	59,898	-11,420	6,000	34,300	100,000	47,770	64,320	1.346	1.063	0	1
Linguistics and Foreign Languages	37,430	-6,127	3,700	32,000	107,000	49,058	71,410	1.456	1.825	0	1
Mathematics and Statistics	47,773	10,414	17,200	54,000	154,000	75,138	90,229	1.201	0.047	1	0
Medical and Health Sciences and Services	312,050	6,523	20,500	50,000	110,000	58,921	60,680	1.030	-1.146	1	0
Philosophy and Religious Studies	26,146	-10,464	7,700	37,000	135,000	60,164	87,041	1.447	1.763	0	1
Physical Fitness and Leisure	48,301	-12,074	11,000	35,000	90,000	44,070	53,027	1.203	0.064	0	0
Physical Sciences	114,863	11,886	20,000	55,000	180,000	81,802	97,693	1.194	0.001	1	0
Psychology	188,336	-5,787	6,700	32,650	101,000	48,013	66,044	1.376	1.266	0	1
Public Affairs Policy and Social Work	46,623	-10,402	9,000	32,000	80,000	39,904	51,864	1.300	0.737	0	0
Social Sciences	302,590	8,474	12,000	45,000	150,000	70,153	93,917	1.339	1.009	1	1
Theology and Religious Vocations	23,393	-34,594	7,000	29,400	75,000	36,285	43,973	1.212	0.124	0	0

Source: Table D2 and D3 in Hampole (2022), own calculation.

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