The Grandparenthood Effect: Labor Force Attachment Responses and Trends Among Older Workers

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Abstract

51% of those aged 51-64 are grandparents, but little is known how their labor force attachment differs from the grandchildless. Grandchildren's impact on age of retirement, hours worked, grandfather's participation in the labor force, and grandmother's non-zero annual hours worked are estimated. Endogeneity between fertility timing and grandparent characteristics is instrumented for by exploiting exogenous variation state-by-year access to reproductive technologies. I find that each marginal grandchild induces grandfathers to be 5.7% more likely to be retired. Grandmothers respond to the marginal grandchild by working 120 fewer hours a year, be 8.5% more likely to be retired, and be 8.4% less likely to report non-zero working hours. Maternal grandmothers respond to the marginal grandchild by being 5.8% more likely to be too disabled to work. A predictive exercise simulating LFP rates from CPS data with interactions between grandchildren measures and Social Security benefits indicates that a 1 point increase in the fraction who are grandfathers decreases the LFP rate by 0.18 points, and by 4.1 points in response to a 1 child rise in the average number of grandchildren. Overall, though, simulated LFP rates from alternative fertility histories do not meaningfully diverge from trends in the observed LFP rate, although the levels of LFP rates would have been between 3-5 points higher between 1962-1994 if the Baby Boom had not occurred.

JEL Codes: J13, J14, J22, J26

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Introduction

Fears of a looming workforce crunch caused by population aging have been partially allayed by rising labor force participation by older workers. The rise in labor force attachment in this group since the early 1990's (from 31% to 40%) has been ascribed to changes in educational attainment, retirement incentives, and improvements in life expectancy (Maestas and Zissimopoulos, 2010). However, labor force attachment gains seen since the early 1990's have stalled out since the Great Recession, as the labor force participation rate for workers 55 and older has remained essentially unchanged at 40% since 2009.[2] While causes for this recent stagnation are numerous, the role of changes in family size and composition that occurred across the industrialized world since the 1960's has been largely overlooked. This omission is curious given the broad reach of grandparenthood: according to a Pew survey, 51% of people aged 50-64 have grandchildren (Taylor et al. (2009)). In spite of these facts, the economics literature has little to offer to policymakers wanting to understand interactions between fertility trends, grandparenthood, older workers' labor force participation, and retirement timing.

In the United States as of 2015, 24% ($888 billion) of the federal budget goes to Social Secuirty alone, with another 17% ($546 billion) going to Medicare, the old age health insurance program for people 65 and older (Center for Budget and Policy Priorities, 2016). Thus, collectively, spending on retirees is now over 40% of the federal budget, so that the future of federal expenditures is sensitive to trends in labor force participation among current and future retirees. Yet, important but currently unanswerable questions include: what are the net costs to Social Security if each additional grandchild prompts the grandparents to work 2 weeks less a year? What is the effect over a 10 or 20 year window 20 years? If policymakers want to restore solvency to Social Security without major changes to revenue-collection or benefits, should they be hoping for a baby boom or a steady, sustained rise in the birth rate? Is the current baby bust as worrisome for entitlements if it prompts greater labor force attachment among current and prospective beneficiaries? Consequently, can policymakers expect older workers' labor force participation to resume rising as the birth rate continues to drop? Are there lags between grandchildren's arrival and grandparents' labor force attachment response?

In this study, I examine how grandparenthood shapes labor force attachment among older workers. First, I show new evidence that grandchildren's presence changes both grandfathers' and grandmothers' labor supply. I use an intergenerational extended survey of US families

[2]U.S. Bureau of Labor Statistics, Civilian Labor Force Participation Rate: 55 years and over [LNS11324230], retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/LNS11324230, April 24, 2017.

from the Panel Study of Income Dynamics (PSID) to empirically test how individuals respond to grandchildren. Since a naive model might yield biased results from endogeneity between grandparents' labor force characteristics and fertility timing by their adult children, I instrument for fertility using changes in legal barriers to women's access to reproductive technologies. I find a labor attachment decrease effect for both grandmothers and grandfathers, the first time that there is a labor force attachment response for grandfathers in the literature. Point estimates for grandfathers range from 12.1% more likely to be retired with each additional grandchild, which is the midpoint estimate from point estimates that are robust across a variety of specifications. Grandmothers have both an extensive and intensive margin respond to the marginal grandchild, increasing their propensity to be retired by 19.1%, and working 302 fewer hours a year. Maternal grandmothers have a stronger intensive margin response relative to paternal grandmothers, but paternal grandmothers are more likely to be retired. Grandfathers do not substantively respond differently to the marginal grandchild by the parent's sex. In spite of evidence from the literature on linkages between health, social isolation, and aging, I find no evidence that grandchildren decrease self-reported disability, and instead find that maternal grandmothers become 5.8% more likely to be too disabled to work with each additional grandchild.

Second, after establishing a labor attachment effect for both men and women, I analyze the changes in older men's labor force participation (LFP) rates are a function of the post-1960's fertility transition. This section tests a novel hypothesis to the question of why older men's labor force participation fell steadily from about 1970 to 1994: whether the grandparenthood surge caused by the Baby Boom contributed to the LFP drop. I use the Current Population Survey (CPS) to generate estimates of national-level labor force participation rates by age. I create historical measures by birth cohort of the fraction who were grandfathers and their average number of grandchildren using Centers for Disease Control and Prevention (CDC), Vital Statistics, Health and Retirement Study (HRS), and Retirement History Longitudinal Survey (RHLS) data to test how LFP rates responded to changes in grandparenthood.

While the individual level results point to a differential grandparent response over non-grandparents, the hypothesis that grandparenthood trends played a major role in the fall and rise in labor force participation of older men between the 1960's to the Great Recession is not strongly supported by the data. I find that whether grandchildren change older men's LFP in the aggregate is sensitive to the specification of birth cohort controls, but that there is robust evidence for economically meaningful interaction effects between grandparenthood and Social Security benefit levels. Each additional average grandchild lowers their national labor force participation rate by between 2.5-4.1 points (assuming average values of other controls). Similarly, a 1% increase in the fraction who are grandparents would decrease the LFP rate by 0.19 points.

The paper proceeds as follows: Section I reviews the existing literature on grandparents, their labor supply, and how trends in both may be interrelated. Section II describes the PSID and its grandparent samples, the CPS, and other data sources used to estimate national trends in LFP and grandparenthood. The research design and empirical approach for the individual-level PSID estimations and the results of those estimations are discussed in Section III. Section IV discusses the empirical approach and results for the national-level trends chiefly from the CPS data. Section V concludes.

1 Trends in Grandparenthood and Labor Force Participation Among Older Workers

Recent trends offer some optimism because population aging's pressure on social insurance systems' solvency is being offset by rises in older worker's LFP (Organisation for Economic Development and Cooperation (2006)). Figure 1 shows the national trends in labor force participation among workers 55 and older. Since World War II, LFP among workers 55 and older steadily declined from 1948 to 1970 from 43.3% to 39.0% (-0.2% per year), before more steeply dropping off between 1970 to 1987 from 39% to 30% (-0.5% per year).[3] However, since the late 1980's and early 1990's, LFP for older workers reversed and rose until the Great Recession (from 30.1% in 1994 to 40.0% in 2009), and has leveled off at about 40% until the present.

While Figure 1 shows participation has recovered from its early 1990's lows, LFP in this age group was even higher as late as 1960 despite a 5 year life expectancy increase over the intervening 50 years (79.3 versus 84.3 years, National Center for Health Statistics (2015)). Further, older workers are as healthy or healthier now than they were 50 years ago. The fraction of adults aged 55-64 and 65 and over who smoke is one-third of what it was in 1965 (National Center for Health Statistics, 2015). The fraction of adults ages 40-59 reporting a work-limiting health condition or a disability has been roughly stable since 1988 (Autor (2015)), and the rate of adults claiming disability insurance for heart disease and cancer declined between 1983 and 2003 (Autor and Duggan, 2006).

Several papers have tried to explain these shifts in LFP. The LFP rise since the 1980's has been ascribed partly to a society-wide shift from defined benefit to defined contribution retirement plans (Hurd and Rohwedder, 2011; Heiland and Li, 2012), changes in Social

[3]Policymakers responded in 1977 and 1983 by decreasing the generosity of Social Security with seemingly little impact on labor force participation according to Kreuger and Pischke (1992).

Security rules (Behaghel and Blau, 2012; Blau and Goodstein, 2010; Gustman and Steinmeier, 2009; Hurd and Rohwedder, 2011), trends in technical skill accumulation among older workers (Burlon and Vilalta-Bufi, 2016), gains in educational attainment in successive birth cohorts (Goldin and Katz, 2007; Burtless, 2013; Maestas and Zissimopoulos, 2010), and rising female labor force participation causing men to coordinate retirement timing with their wives (Schirle, 2008; Gustman and Steinmeier, 2000).[4]

Blau and Goodstein (2010) explored a variety of factors to explain the post-war fall and rise in labor force participation among older workers. While they ascribe the post-1990 rise largely to greater educational attainment and reduced Social Security generosity, reasons why labor force participation fell remained unaccounted for. Ultimately, the problem, as stated by Blau and Goodstein (2010, p. 356), remains:

> "Two key points remain unresolved by the findings reported here: what caused the long decline in LFP among older men and why is Social Security more important in accounting for recent LFP increases than in explaining the previous decline? The first question has been studied for many years without much success, and unfortunately, our results do not suggest any new avenues of research."

1.1 Trends in Grandparenthood

Over the post-WWII period, grandparenthood has risen and fallen with the national birthrate. The Centers for Disease Control and other agencies do not track grandparenthood, but published birth and marriage data give strong clues as to how older workers' families are evolving. Figure 2 shows that births fell through 1920's and into the Great Depression, before rising after World War II and spiking in the late 1950's as part of the Baby Boom. Thereafter, the birthrate flattened in the 1970's and except for minor fluctuations over the intervening years, has largely hovered around 65-75 births per 1,000 women aged 15-44.

Not only are the implied number of grandchildren changing over time, but when people become grandparents is, too. The first evidence is that couples throughout the world are choosing to have fewer children and to have them later (Morgan (2003); Bloom et al. (2009); Caldwell (2004)). For example, Livingstone and Cohn (2010) found that in the United States a greater share of births were to teenagers than to women 35 and older until 2008, when the reverse became true.

Secondly, the age of first marriage declined for men and women between 1890 (26.1 and 22) and 1949 (22.7 and 20.5), flattened out around 23 for men and 20.5-21 for women, before

[4] Wives are often younger than husbands, so a preference for a joint retirement would prompt men to delay retirement until their wives were also eligible.

starting to rise steadily since about 1975. Figure 3 shows these trends, and that median age of first marriage recently overtook 29 for men and 27 for women. These shifts are significant because the married fertility rate has always been higher than the unmarried fertility rate, and that until about 1970, over 90% of all births were to married women (Kendall and Tamura (2010)). Marital shifts in turn impacted both the age of first birth and the fraction of women remaining childless. The median women born in 1910 was first married at 22 (in 1932) and had her first baby at 23 (in 1933), and about 20% reached 45 (in 1955) childless. In contrast, the next generation of women born in 1935 was first married at 21 (in 1956), had her first baby at 22 (in 1957), and only 11.4% reached 45 (in 1980) childless (Kirmeyer and Hamilton (2011)). Since the average man married a woman roughly three years younger than himself between 1920 and 1940, these figures can be extrapolated to imply that the average man born in 1907 had *at least* a 20% childless rate versus a man born in 1932 had roughly a 12% childless rate. By extension, at minimum, the grandchild-less rate for both cohorts is 20% and 12%, respectively. Comparing these statistics to the LFP rate in Figure 1, a man born in 1907 turning 55 in 1962, when the LFP rate for those 55 and older was 40%, but a man born in 1932 turning 55 in 1987, the corresponding LFP rate was 30%.

The next generation of men and women show a different pattern: the median woman born in 1960 was first married at 22-23 (in 1982-1983), and had her first baby at 25 (in 1985), and about 15.6% reached 45 (in 2005) childless (Kirmeyer and Hamilton (2011)). When their likely partners (born in 1958) reached 55 (in 2013), the LFP rate was back up to 40%. However, these rough statistics cannot accurately convey either what fraction of older workers were actually grandchildless and what the joint distribution of grandchildlessness and labor force participation was, but these numbers help motivate the potential connection between grandparenthood and labor force attachment.

The descriptive evidence on time transfers between grandparents and adult children indicates that the above-mentioned trend co-movements are not at odds with the microdata. Descriptive statistics on grandparent-to-adult child time transfers is presented in Table 1 courtesy of the PSID's 2013 Family Rosters and Transfers module. Adult children with their own children received on average about 25 more hours in time transfers a year from both sets of grandparents than childless households. The grandparent's marital status and sex matters, as does the sex of the adult child. Married grandparents are more time-generous than unmarried grandparents, and the mother's parents are more generous than the father's. In almost all cases, except for single grandfathers, potential or actual grandparents indeed give more time transfers to adult children with their own children than those without.

Figures 4a and 4b plot the LFP rates by selected age groups against the fraction that are grandparents in each age group by year and the average number of grandchildren in each age group by year, respectively.[5] The figures here do not show a particularly tight link between grandparenthood trends and the labor force participation for those 50-61, partly because it is not possible to look at LFP trends in these age brackets before and during the Baby Boom. Neither, however, do they contradict the idea that there might be some causal connection. However, for those 62-64, 65-66, and 67-69, grandchildren peak just before LFP rates in those groups reaches its nadir. Although the alignment in trends is not exact, the graphs strongly suggest that at least for workers 62 and older, some relationship might exist between grandchildren trends and their labor force attachment.

1.2 The Literature on Grandparents and Grandchildren

While the past 70 years has witnessed large shifts in grandparenthood and older workers' labor force activity, the labor economics literature on grandparenthood is thin. Existing work offers mixed indications on the kind of labor market response grandparents are most likely. Ho (2015) found using Health and Retirement Survey (HRS) data that grandparent responses seem to vary according to their marital status and financial resources. Grandparents are most likely to help with newborns, and grandparents living in close proximity provide larger time transfers. Married grandparents are both more likely to be employed and to give financial help, although to what extent that is due to married couples having more resources or being able to provide both time and financial assistance to the new parents is unclear. In comparison, single grandparents made no time or financial adjustments in response to new grandchildren. Because the study did not attempt to instrument for the adult children's fertility, it is hard to know which of these results might be significant when endogeneity bias is removed from the estimates.

The sign on the grandchild measures in the disability regressions is, like the other labor market outcomes, also ambiguous *ex ante*. While several health and psychology studies have found that more socially isolated individuals exhibit worse physical health (Cornwell and Waite (2009)), greater cognitive decline (Donovan et al. (2016)), and higher mortality rates (Holt-Lunstad, Smith, and Layton (2010)), grandparents may claim that they are disabled in order to advantage themselves of Social Security Disability Insurance payments if grandparenthood induces a rise in their utility of leisure.

Most other studies have focused just on questions of time transfers. In part, this reflects what grandparents desire themselves. A Pew Survey (Taylor et al (2009)) reported that spending time with grandchildren is what the elderly most value about getting older. Hochman and Lewin-Epstein (2013) found from survey data of elderly Europeans that grand-

[5] The grandparent statistics reported here were generated from the methods described in Appendix C.

parents are more likely to report a desire to retire early. This result was higher in countries that have less generous public childcare policies, suggesting that grandparents do respond to the childcare needs of their children by decreasing labor force attachment. The intuition behind the second finding is confirmed in Compton and Pollak (2014), Posadas and Vidal-Fernandez (2013), and Aparicio-Fenoli and Vidal-Fernandez (2015) who each find that grandmothers providing childcare for new parents increases the mother's labor supply. This help may not make much difference in the grandmother's own labor supply, as Whelan (2012) found that as long as the grandmother's help was for less than 12 hours a week, labor supply was not affected.

However, these studies do not account for the possibility that fertility timing and grandparent labor force characteristics are jointly determined. Namely, adult children's fertility decisions may be based on the likelihood they will receive grandparent assistance. If adult children believe they will need assistance, they could time their childbearing to correspond to the grandparent's ability to help. This possibility could bias estimates of the grandparents' labor response, because it is then unclear if the arrival of grandchildren causes a change in grandparent's behavior or the grandparent's willingness to provide financial or childcare help influences adult children's decision on when to have their own children.

Three other studies attempt to address the endogeneity bias by using instrumental variables to estimate how grandchildren affect grandparent labor force attachment. First, Wang and Marcotte (2007) use PSID survey data to study how grandparents who are raising grandchildren change their labor force behavior when the grandchildren move in. Their interest is chiefly in comparing three-generation versus skipped-generation households, so their instrument includes the existence and number of grandchildren.[6] They find that compared to independent-living grandparents, grandparents co-residing with grandchildren are more likely to increase their labor force participation. However, the narrowness of the research question means that it is of limited use for understanding the relationship between grandchildren and grandparents labor force attachment, as only about 7% of grandchildren live in a grandparent-headed household according to the Population Reference Bureau.[7]

The second and more comparable study is by Rupert and Zanella (2018), which also estimates the impact of grandchildren on grandparents using the PSID. Their study finds that becoming a grandparent reduces the annual number of hours worked for grandmothers

[6]The rest of their excluded instruments are state-level characteristics: teenage pregnancy and incarceration rates plus the generosity of state kinship foster care arrangements.

[7]Paola Scommengna, Population Reference Bureau, "More U.S. Children Raised by Grandparents", `http://www.prb.org/Publications/Articles/2012/US-children-grandparents.aspx`, last accessed January 30, 2016. The PSID likely has a substantial subsample of these families due to the low-income OEO oversample that was originally included.

by at least 150 hours (< 4 work weeks), but no significant effect was found for grandfathers. Rupert and Zanella instrument for arrival of the first grandchild by exploiting variation in the sex of oldest adult child of the grandparents. Their empirical strategy rests on the fact that, on average, women marry and bear children at younger ages than men, meaning that parents of adult daughters will then be more likely to become grandparents at younger ages than parents of adult sons.

Their paper is informative but my proposed strategy overcomes several empirical difficulties that the Rupert and Zanella approach encounters. The first is that the authors eschew the PSID's sampling weights, arguing that conditioning on the covariates that the sampling weights account for is preferable to weighting directly. Chiefly, the authors condition on the families' 1967 income. This approach ignores that the oversample of low-income households was done on additional characteristics, such as race and location. Their results thus risk introducing selection bias on properties not accounted for in the covariates but are otherwise accounted for in the sampling weights. This study accordingly uses the PSID's sampling weights.

The second is that they have to use pooled 2SLS instead of a panel fixed effects model and, as they acknowledge, they cannot non-parametrically account for time-invariant factors. Since my instrument's variation occurs at the state-by-year level, I am able to include individual fixed effects in all results. A third difference is the first stage's difference-in-difference framework ensures that the local average treatment effect (LATE) will exploit more variation in the compliers' characteristics. Their model has to account for the fact that the compliers are restricted to the maternal grandparents, who are likely to bequeath greater time transfers than paternal grandparents. Since I use both the daughter and daughter-in-law's characteristics, I can calculate separate LATE's for maternal and paternal grandparents. The last significant difference is that when looking at the impact of a marginal grandchild (*i.e.*, the impact of each additional grandchild), they hold that the endogenous decision is to become a first-time parent, but subsequent children and siblings' fertility are both exogenous. However, there is no justification given for this assumption, so that their consequent finding that additional grandchildren increase labor force participation possibly has endogeneity bias. This study explicitly accounts for endogenous fertility of grandchildren regardless of birth order.

Lastly, their instrument's validity with respect to the exclusion criterion is undetermined. As they openly acknowledge, the literature is inconclusive on whether it can be assumed that the sex of the first-born child exerts no impacts on the parents' labor supply. They run several empirical tests to support the instrument's validity but due to the sampling issue discussed above, it is not clear that the matter is settled. Thus, using instead state policies which more transparently satisfy the exclusion criterion is a way to address the same question while sidestepping the aforementioned concerns.

The third paper by Frimmel, Halla, Schmidpeter, and Winter-Ebmer (2017) looks just at grandmother's labor supply using the universe of Austrian administrative data. Their paper's empirical method is a timing-of-events approach, under the assumption that grandmothers' do not know the conception timing of their first grandchild. They then estimate the impact of the marginal grandchild using the arrival of twins as an instrument. They find that grandmothers reduce their labor supply by half a year, and that the labor supply reduction is compounded with each additional grandchild. This paper offers three advantages over theirs. The first is that I estimate both grandmothers' and grandfathers' labor supply outcomes. The second is that I make no exogeneity assumptions along the extensive margin of grandparenthood. The third is important contribution and has a much larger pool of potential compliers (all treated age-eligible adults) to estimate my effects, rather than the proportion of the population with twins, a relatively rare event.

2 Data Description

The sample of grandparents and their families is drawn from the PSID, a dataset that follows about 4,800 households initially sampled in 1968 and their lineal descendants. The original sample is composed of two subsamples: a nationally representative sample of 2,930 families (called the SRC Sample) and an oversample of 1,872 low-income families (the SEO Sample).[8] The PSID follows the family members of the original sample households as they move out, marry, and form families of their own, resulting in about 70,000 individuals appearing in at least one survey. This survey design makes the PSID a uniquely rich source of information on intergenerational dynamics, especially because the PSID supplements the main survey with auxiliary datasets on marriage and childbirth histories. Between 1968 and 1997, the survey was conducted annually, and from 1999 to the present has been conducted biennially.

The PSID makes available a series of files that enable identification of all surveyed descendants of a given individual through their Family Identification Mapping System (FIMS). Using the FIMS, I have identified the adult children and grandchildren of each grandparent, and then merge on the survey responses of each respondent. Location and age information in the PSID also allows me to code with a high level of precision the likely abortion and

[8] In 1990, the sample was updated to include 2,000 post-1968 immigrant families (exclusively of Latino origin), but they were dropped in 1995. In 1997, the sample was again refreshed by adding 500 post-1968 immigrant families. Because the instrument is dependent on the individual being observed between 1968-1980, these families are not included in this study.

contraception access status that the female respondents had. For a complete overview of how abortion and contraception access was encoded, see Appendix A.[9] In addition to observing demographic characteristics, such as marital status, age, race, and educational attainment, the dataset also measures respondent's key labor market characteristics: retirement status, annual hours worked, and labor force and employment status.

Individuals who are greater than age 22 and were the current or future parents of at least one child were chosen as the sample of potential grandparents. Being aged 22 as the minimum age cutoff was chosen to limit confounding variation between education and labor force characteristics. My panel has 7,611 grandmothers and 5,205 grandfathers across 39 survey years (1968-2015).

Table 2 shows selected summary statistics on grandfathers and grandmothers. Individuals in the sample were observed to become first-time grandfathers around age 52 and grandmothers around 49-50,[10] which is still within the prime working years, and then retire about 5-7 years thereafter. Differences in mean ages between grandfathers and grandmothers reflect that families were usually sampled as a household, so that the age gap between husbands and wives got "passed through" into the sample.

For national-level labor force participation trends, I use March Current Population Survey (CPS) micro-data to create a synthetic panel dataset, and supplement it with data drawn from Social Security Administration (SSA). As in Blau and Goodstein (2010), I aggregate individual-level records on men aged 55-69 from the CPS into cells defined by year, birth year, and Census Division. I then supplement it with men aged 50-54 to provide more data on the impact of grandparenthood on the labor force attachment in this cohort. The resulting panel covers 74 birth cohorts (1892-1965) between 1962 to 2015.

For each birth cohort, I calculated the fraction who were grandparents and their average number of grandchildren at both the birth cohort-age-education group and birth cohort-age-state level using Health and Retirement Study and Retirement History Longitudinal Survey data. I was not able to use PSID data, primarily because it is not a large enough sample of older individuals to generate credible grandparent statistics at the birth cohort level. Instead, I combined two data sources that oversample older individuals longitudinally to estimate this fraction. The first is the Health and Retirement Study (HRS) data which sampled roughly 20,000 older individuals in successive birth cohorts from 1992 to 2014. The

[9] As detailed below, most states regulated access on the basis of age, but a few did so on the basis of educational attainment (minor HS graduates can buy contraceptives in Alabama and Pennsylvania) or marital status (Alabama, Florida, Maine, Missouri, New Jersey, Texas, West Virginia). Coverage can thus be ascertained with a high degree of accuracy in the PSID that other studies might overlook.

[10] Adult children who were not living with the Head and Wife of household in 1968 are not consistently surveyed by the PSID, so this statistic is biased upwards somewhat.

second is the Retirement History Longitudinal Survey (RHLS), the predecessor of the HRS, which sample 11,000 plus individuals chiefly born between 1906 and 1911 biennially from 1969 to 1979. Unfortunately, only the 1975, 1977, and 1979 questionnaires asked about the number of living grandchildren but the two datasets combined provide important evidence on the evolution of grandparenthood over time. Appendix C has more detail on how this measure was constructed by using the data points to estimate the fraction grandparents and their average number of children for the various crosstabs.

I then use Blau and Goodstein's method to extend simulated work lifetime earnings histories and use these to generate expected Social Security old age and disability benefits payments for either retiring at ages 62, 65, and 70, or dropping out of the labor force and claiming disability payments from ages 50-64.

3 Individual-Level Estimation with the PSID

In this section, I test whether and how grandchildren alter grandparents' behavior. Regressions for grandmothers and grandfathers are estimated separately. The left-hand side variable is the grandparent's labor market outcome: retirement status, annual hours worked (conditional on not being retired), labor force status (grandfathers), non-zero hours reported (grandmothers), and whether the individual has a disability that prevents him or her from working.[11]

I examine here two main effects of grandparenthood. First, I create an indicator to test for the impact of being a grandparent on the labor market outcomes of interest. I then study total fertility effects by estimating the grandparent response to the marginal grandchild each adult child provides.

[11] Labor force status is not reported for wives in every year in the PSID, so an indicator for whether the grandmother reported some working hours is used as a stand-in. Compared to a measure of being in the labor force, it codes to zero grandmothers who were unemployed and looking for work (and are technically in the labor force), and it will code to 1 grandmothers who report some hours worked, but are students, retired, or homemakers. For those years where labor force status is available (1976 onwards), the correlation between a indicator for being in the labor force and an indicator for reporting non-zero annual workings hours is 0.5468.

3.1 Empirical Strategy for Individual-Level Estimates

The first grandchildren impact channel is whether there is a grandparent margin to labor force attachment. The equation to estimate this channel takes the form of

$$\begin{aligned} Outcome_{gst} =& \beta_0 + \beta_1 \mathbb{1}\{Grandparent_{gst}\} + \beta_2 GPDemVars_{gst} \\ &+ \beta_3 ACDemVars_{gst} + \lambda_t + \theta_{gs} + (\theta_{gs} * \lambda_t) + \iota_g + u_{gst}. \end{aligned} \tag{1}$$

The unit of observation is the grandparent and the key variable of interest is the indicator for grandparent status, $\mathbb{1}\{Grandparent_{gst}\}$, which was created by finding the birth year of the oldest grandchild. $Outcome_{gst}$ is either grandparent g's annual number of hours worked, retirement status, disability status, or whether the grandparent is in the labor force or reporting non-zero hours worked in year t in state s. The right hand side is populated with the demographic information of both the grandparent and the eldest adult child, plus state, year, state-by-year, and grandparent fixed effects. I use only the eldest daughter or daughter-in-law's controls, under the assumption that the eldest grandchild will be born to the eldest daughter(-in-law) in the family.[12]

$GPDemVars_{gst}$ is a vector of demographic information about the grandparent, which includes a dummy for whether the grandparent is age-eligible for early or full Social Security benefits;[13] the Social Security age-elibility of the other grandparent, to at least partially account for both joint retirement decision-making and the labor supply of the other partner; age as fourth-order polynomial, reflecting that often labor force attachment first rises and then falls with age; marital status and interactions between marital status and the Social Security age-eligiblity of the other grandparent to capture further variation in potential intra-family labor supply decision-making. Time-invariant grandparent characteristics are not included, because the grandparent fixed effects cause them to drop out.[14]

$ACDemVars_{gst}$ is a vector of the adult child's demographic information, namely age and marital status. θ_{gs} and λ_t are vectors of state and year dummies.[15] State fixed effects

[12] The alternative estimation strategy is to use the actual adult child of the eldest grandchild, but regressions employing this strategy yielded substantially weaker first stages for the instrumental variables regression discussed in Section 3.1.1. The other alternative is to estimate regressions where all adult children's characteristics are used to estimate the first stage, whose results I discuss in Section 3.4

[13] People can become eligible for actuarially-adjusted benefits at 62 as long as they have worked a sufficient number of quarters, but the work requirement is difficult to accurately estimate in the PSID. because for the oldest in-sample workers, their pre-1967 working hours were not observed, and for later-observed individuals, the biannual survey means each person has substantial unobserved work history. Thus, this dummy is measured only as a function of age.

[14] To avoid endogeneity bias between education level and labor supply, educational attainment measures are not included, but by construction, there is very little change in educational attainment in sample, so that the individual fixed effects will effectively be conditioning for the requisite education levels.

[15] As stated in the data description, the state here is the individual's 1968 state. These also fall out of the

control for time-invariant characteristics common to all residents who lived in state s in 1968, year fixed effects control for year-specific shocks, and state-by-year fixed effects thus control for state-specific yearly shocks . These could include state-specific employment or economic shocks common to all individuals in a given year that would influence labor force attachment coincident with fertility timing, also affected by economic conditions (Amialchuk (2011); Black et al. (2013); Schaller (2016)). Each regression is run separately for grandmothers and grandfathers. Grandparent fixed effects, ι_g, are included to control for unobserved, time-invariant characteristics of grandparents and their relationships with their children.

Foreshadowing the IV estimation, the standard errors are clustered at the state level. Because the PSID only sampled 39 states in 1968, this decision introduces a potential source of downward bias into the standard errors, leading to the possibility that the null hypotheses will be consistently overrejected.[16] Thus, in each case, the standard errors are inflated using the Bias Reduced Linearization method of Bell and McCaffrey (2002), referred to hereafter as the CR2VE correction, where the residuals are transformed according:

$$\tilde{u}_g = \left[I_{N_g} - H_{gg}\right]^{1/2} \hat{u}_g. \tag{2}$$

More discussion on the CR2VE correction is in Appendix B, including how Equation 2 is transformed for the 2SLS equation.

Total fertility effects are analyzed with the panel fixed effects model below:

$$\begin{aligned} Outcome_{igst} =& \beta_0 + \beta_1 ChildCount_{igst} + \beta_2 GPDemVars_{igst} \\ &+ \beta_3 ACDemVars_{igst} + \lambda_t + \theta_{gs} + (\theta_{gs} * \lambda_t) + \iota_i + u_{igst} \end{aligned} \tag{3}$$

The unit of observation in these regressions is at the adult child level rather than the grandparent level, to reflect the fact that the fertility decision is made by the grown children. This design also makes instrumenting for fertility more tractable. If instead I attempted to instrument for the total number of grandchildren, each adult child would require an age and birth year-dependent instrument for their fertility, so that the number of covariates would change grandparent to grandparent. This design allows for consistent instrumenting for total fertility and fertility timing while preserving the ability to observe the labor supply change from the marginal grandchild. One alternative is running the estimation strategy on just 1, 2, or 3 adult child families at a time. As discussed in Section 3.4, a robustness check for the largest adult child subgroup of 2 is performed, as other subpopulations create small cell sizes.

model when grandparent fixed effects are added.

[16] See Cameron and Miller (2015) for an overview of the few cluster problem.

The other advantage is that it allows me to include adult children of any birth order, in contrast to the panel in Equation (1). Grandparents may give the most to the first grandchild (usually born to the eldest adult child) and less with each subsequent grandchild as the novelty wears out or the family elasticity of labor shrinks as one moves further down the individual's labor supply function. To account for heterogeneous fertility preferences among adult children and the impacts of birth order, sex, and their interactions non-parametrically, these regressions will use adult child fixed effects (ι_i) instead of grandparent fixed effects. The adult child fixed effects completely subsume the grandparent fixed effects, controlling time-invariant factors from both generations.

The family's PSID-provided 1968 sampling weight is adjusted to reflect the number of times a grandparent appears in this dataset, which is simply equal to the number of adult children they have. All other variables in this regression are otherwise the same as in Equation (1).

3.1.1 Endogeneity of Timing and Number of Grandchildren

If, however, adult children are basing the fertility decisions on anticipated changes in grandparent's labor supply, then Equations (1) and (3) cannot be consistently estimated. As discussed in the introduction, parents might time their fertility with anticipated changes in their parents labor force status, so that a panel fixed effects model would overestimate the impact of grandchildren. Similarly, they might wait to have children for when their parents achieve financial stability, so that the models underestimate the impact of grandchildren.

My identification strategy in light of this likely endogeneity is based on changes in legal barriers to abortion and contraception access that occurred throughout the US mostly in the 1960's and 1970's. The identifying assumption is that there were no other state-by-year variables that also affected fertility coincident with the repeal of the access barriers. The number of children an adult woman has is modeled as being a function of access to oral contraceptives and abortion on-demand, the distance to an abortion early-legalization state.[17]

These policy changes are used to instrument for all three key variables discussed in the previous section. The first-stage regression for $ChildCount_{igst}$ and $\mathbb{1}\{Grandparent_{gst}\}$ is:

$$\begin{aligned} ChildCount_{igst} &= \pi_0 + \pi_1 PillAccess_{igst} + \pi_2 AbortionAccess_{igst} \\ &+ \pi_3 AbortionAccess\_LT250_{igst} + \nu_{igst}, \end{aligned} \tag{4}$$

where $PillAccess_{igst}$ is the fraction of year t that adult daughter i in state s could buy oral

[17] More information on the policy changes can be found in Sections A.1 and A.2.

contraceptives under the age of 21; similarly, $AbortionAccess_{igst}$ codes the fraction of year t that an undesired conception could occur and then later aborted. $AbortionAccess\_LT250_{igst}$ is used to code access by grouping the distance a state is to either California, New York, or Washington D.C.,[18] because non-residents who were age-eligible could get an abortion. This variable measures how far a pregnant woman has to travel for an abortion, under the assumption that legalization's impact would be strongest in neighboring states.

The grandparenthood indicator, $\mathbb{1}\{Grandparent_{gst}\}$, is instrumented for after accounting for the change in the unit of observation. Grandparenthood status is now a function of the eldest daughter or daughter-in-law's exposure to changes in contraception and abortion access barriers. This takes the form of

$$\begin{aligned}\mathbb{1}\{Grandparent_{gst}\} &= \pi_0 + \pi_1 PillAccess_{gst} + \pi_2 AbortionAccess_{gst} \\ &+ \pi_3 AbortionAccessLT250_{gst} + \nu_{gst}, \end{aligned} \tag{5}$$

where the policy variables described in Equation (4) are now the exposure for the eldest daughter or daughter-in-law for grandparent g to the changes in access.

3.2 Individual-Level Results

3.2.1 Panel Fixed Effects Estimates

Estimation results for Equations (1) and (3) are in Table 3, which reports the effect of being a grandparent and the marginal effect of an additional grandchild on five labor market outcomes: being retired, being too disabled to work, annual number of hours worked conditional on not being retired, and being in the labor force (grandfathers) or reporting non-zero working hours (grandmothers).

Being a grandparent is associated with a significant labor force detachment effect. Retirement propensity increases for both grandfathers (by 4.8%) and grandmothers (2.0%), and annual hours worked decreases for grandmothers by 76.4 hours. Becoming a grandmother has zero effect on being too disabled to work, but grandmothers are 3% less likely to report non-zero hours worked. Grandmothers also report 74.4 fewer hours worked a year

[18] The categorizations were done by Levine et al. (1999) and Ananat, Gruber, and Levine (2007) on the basis of how the maximal distance a person would have to drive to get an abortion within half a day (<250 miles) or greater. Those papers do not code DC as a repeal state, as I do, so I made the requisite recategorizations. Joyce, Tam, and Zhang (2013) offers compelling evidence that New York's lack of residency requirement, in particular, acted as an exogenous shock on birth rates in neighboring states. In my study, women are coded by age on the basis of how close they are to the closest early legalization state they are eligible to get an abortion at. For example, Washington State legalized abortion in December 1970, but minors needed parental permission. Washington State's policy had a residency requirement, so I assume that its legalization had no impact on women in neighboring states.

conditional on not being retired. Beyond being more likely to be retired, grandfathers do not seem to otherwise have a significantly different labor market attachment relative to the grandchildless.

The marginal effect of each additional grandchild for grandfathers is that they are 3.6% more likely to be retired (significant at the 1% level), and 1.2% less likely to be in the labor force (significant at the 5% level). As with the grandparenthood regressions, only extensive margin effects (being retired and labor force status) are statistically significant, although the sign on the marginal grandchild is now negative in the hours worked regression. Grandmothers, on the other hand, show a stronger intensive than extensive margin effect: working 47.5 fewer hours annually for each additional grandchild, but becoming only 1.8% more likely to retire, and 1.6% less likely to report non-zero annual hours worked, all significant at the 1% level. Like grandfathers, their propensity to report being too disabled to work changes by an effect essentially indistinguishable from zero.

The "In Labor Force" measure and "Non-Zero Working Hours" measure are not directly comparable, but broadly speaking, both grandmothers and grandfathers decrease their labor force attachment and labor supply with each additional grandchild. Grandfathers exhibit only a statistically significant extensive margin response, although not by claiming to be too disabled to work. Grandmothers have a generally smaller retirement response than grandfathers, but they have a larger foregone hours worked effect. The smaller extensive margin response by grandmothers could well be because they have lower labor force attachment than grandfathers, generally, while having higher childcare expectations on their time than grandfathers.

3.2.2 First-Stage Results

The results in Table 3 cannot be understood as a causal labor supply effect until the endogeneity concern is addressed. Table 4 shows the first-stage estimates for Equations (4) and (5). For the access to contraception and abortion to be a valid instrument for the number and timing grandchildren, the results should show evidence that exposure to the policies changed either fertility timing, total parity, or both.

The coefficient estimates on the policy variables largely affirm the intuition that the pill and abortion decreased fertility. There is a clear pattern that the pill and abortion played relatively little role in changing when people first became grandparents, but plausibly a much larger role in how many grandchildren they had. Across specifications, access to the pill decreased the probability a person would be a grandparent in a given year by between 0.2%-2.3%, with none of these results being statistically significant. Similarly, abortion access changed the probability a person would become a grandparent in a given year by between

1.3% to -3.2%, with no point estimate being statistically significant. The range and lack of significance is fairly consistent evidence that abortion had little effect on grandparenthood timing.

In light of the essentially null effect of abortion, it is surprising that abortion access in a nearby state ($\leq$250 miles away) between 1969 (when California first legalized access) and 1973 (*Roe*) is negative and statistically significant at the 1% level across all specifications, with the decrease in probability ranging from 6.8% to 10.5%. State-by-year variation in abortion access for women older than 21 is somewhat limited, as almost all policy changed occurred over 1969 to 1973 and was only driven by 4 states in the PSID (CA, WA, DC, and NY), which may limit the ability to gauge its effects. On the flip side, the list of nearby states is much longer (AZ, CT, ME, MD, MA, MI, NV, NH, NJ, OH, OR, and PA). It may be that abortion, overall, does not substantially change a person's probability or timing of becoming a grandparent, but it may have initially when legalization was new and more of a true policy "shock".

In contrast to the grandparenthood status regressions, the child count regression shows that contraception and abortion access is a strong predictor of whether a person has an additional child. Pill access point estimates are all significant at the 1% level, and predict that a person has .106 to .160 fewer children in a given year. Abortion access point estimates are only significant (at the 5% level) in the grandfather regressions for retirement status, disability status, and labor force status, but nonetheless, is negative across all specifications, and decreases the number of children in a given year by between 0.009 to 0.109. Abortion access in a nearby state is significant at the 1% level across all specifications except for grandmothers' conditional hours worked, and point estimates indicate that an exposed person had between 0.008 to 0.223 fewer children in a given year. Restricted to just significant results, the range becomes 0.151 to 0.223 fewer children in a given year.

The first stage results here are clear that the access policies shaped the reported fertility of exposed persons, but are less clear on whether they impacted the timing of the first grandchild. In conjunction with the second-stage results presented below in Section 3.2.3, I will also show the complier population characteristics for grandparenthood status (Table 6) to determine whether abortion access in nearby states is sufficient to support a strong first stage. An analysis of the complier population for the marginal grandchild is presented in Table 7.

3.2.3 Second Stage Results

Results with the instrumented values of $\mathbb{1}\{Grandparent_{gst}\}$ and $ChildCount_{igst}$ are reported in Table 5. It is clear from a comparison with the results in Table 3, that a panel fixed effects model understates grandchildren's impact. This is the same directional bias as reported in Rupert and Zanella (2018) [CHECK FRIMMEL]. There is strong evidence that the grandparenthood/grandchildren channel exists most strongly for men at the extensive margin, and in both margins for women.

Reported is the effective F-statistic for the result of a weak instrument test estimated with cluster-robust standard errors using the procedure described in Montiel Olea and Pflueger (2013). Their F-statistic has the advantage over other methods for gauging the first-stage's strength, in that it is robust to clustering the standard errors (unlike the Cragg-Donald Wald F-statistic), and has well-motivated critical values (unlike the Kleibergen-Paap rk Wald F statistic). The Montiel Olea-Pflueger effective F-statistic tests the null hypothesis at the 5% level that the Nagar bias exceeds τ% of a "worst-case" bias. Table 5 reports the worst-case bias critical values for $\tau \in \{5\%, 10\%\}$.

A weak instrument is thus defined here as being unable to reject 10% worst-case Nagar bias at the 5% level, although greater weight is given to results with Montiel-Pflueger F statistics that reject 5% worst-case Nagar bias.

I find large and statistically significant evidence that grandfathers have much lower labor force attachment than non-grandfathers, being 66.8% more likely to be retired and 70.4% more likely to be out of the labor force. For grandmothers, I find that they are 187%(!) more likely to be out of the labor force. However, for all grandparenthood specifications, the weak instrument hypothesis cannot be rejected. While not reported here, the F-statistics cannot even reject the hypothesis of 30% worst-case bias. Thus, these results are probably less reliable indicators of the true grandparenthood margin than the OLS evidence in Table 3.

On the other hand, the weak instrument hypothesis can be safely rejected for all marginal grandchild results, even rejecting the 5% worst-case bias hypothesis. Here, grandfathers and grandmothers have both extensive and intensive margin reactions, with the effect on grandmothers' labor supply strictly dominating the effect on grandfathers'. Grandfathers report being 18.5% more likely to be retired (significant at the 1% level), be 19.9% more likely to be out of the labor force (5% level), and work 316.7 fewer hours a year (10% level). Grandmothers report being 26.5% more likely to be retired (significant at the 1% level), 51.6% less likely to report non-zero hours worked (1% level), and work 483.9 fewer hours per year (10% level).

This is equivalent to 11.6 fewer 40-hour working weeks a year, a substantial labor supply reduction, and roughly equal to the 3 month absence authorized by the Family Medical Leave Act.[19] For grandfathers, the intensive margin reduction is closer to 7.9 fewer 40 hour working

[19]Family and Medical Leave Act of 1993, 29 U.S.C. §§ 2601–2654 (2006)

weeks. Both grandfathers and grandmothers report being more likely to be too disabled to work with each additional grandchild (1.4% and 2.7%, respectively), but this effect is not statistically significant.

The evidence in Table 5 shows that grandparents with bigger extended families have less labor force attachment than those with smaller families. It is also evident that the most economically significant responses are in the retirement and labor supply regressions. A key finding is that each grandchild makes grandfathers 18.2% more likely to be retired, and grandmothers 20.2% more likely to be retired. If, for example, the average number of grandchildren rises by 1, then approximately an additional 20% of grandfathers and grandmothers will be retired.

Since the results here are not average treatment effects, but instead local average treatment effects (LATEs) from the population of compliers, some information is needed on the size of the complier population relative to the sample. Table 6 has complier probabilities for the grandparenthood status regressions and Table 7 has complier probabilities for the marginal grandchild regressions. Unsurprisingly, the complier population in the grandparenthood regressions is tiny. Across the grandfather sample, only 1.5% of the sample were compliers, and across the grandmother sample, just 0.5% were compliers. Similarly, just 6.2% of the sample were abortion access compliers in the grandfather sample, but 0% were for grandmothers, namely because the positive point estimates in Table 4 indicate that the abortion access instrument did not assign anyone to lower fertility in these specifications.

While the coefficients on the abortion in a nearby state dummies are always negative and statistically significant, the complier population on people who were less likely to become grandparents is small: just 0.1% to 0.2%, reflecting both that only a subset of states were affected and only for a limited number of years. The complier population on non-exposed people who were more likely to become grandparents is much larger: 24.6% to 26.5% in the grandfather and grandmother samples, respectively.

Since abortion and contraception access has a direct effect on women's fertility and a more indirect effect on men's, it's possible that the complier population would be driven by women, and thus maternal grandparents. However, when broken out by maternal versus paternal grandparents, the opposite effect is true: the total complier probabilities are about twice as large among paternal versus maternal grandparents. There are two main possibilities. One is that the population of paternal grandparents is for various reasons more likely to include compliers on the sons' side. This could be if male compliers are mostly through their wives, so that in fact compliance is mostly driven by marital fertility. The other possibility is that the fertility spillovers onto men are stronger than previously realized. This could be if women exploited these technologies to be more selective about who fathered their children than previously. A further analysis of this question is beyond the scope of this paper, but this disparity is an important part in interpreting the LATEs reported in Table 5.

In contrast to Table 6, the complier analysis in Table 7 reinforces the strength of using pill and abortion access as instruments for the marginal grandchild. The cumulative compliance rate across all rows ranges from 24.8%-39.4% for the pill; 4.1%-27.5% for abortion; and 1.9%-3.8% for abortion in a nearby state, suggesting that pill compliers are driving the greatest share of the LATE. Overall compliance rates in the grandfather sample is 67.5% and is 46.4% in the grandmother sample, meaning that the LATE reflects a substantial part of the total average treatment effects. Compliance mostly comes along the margin from no children to one child, but a somewhat greater share of abortion compliers are among those going from one to two children. In the nearby abortion state rows, the majority of the compliers comes from the one to two child margin.

These compliance rates compare favorably with results found by others in the fertility literature. Several papers have found that the women used the pill to delay childbearing (Goldin and Katz, 2002; Bailey, 2006; Bailey, Hershbein, and Miller, 2012; Guldi, 2008), and that women used abortion to delay fertility and reduce total fertility (Ananat, Gruber, and Levine, 2007; Guldi, 2008; Myers, 2017; Klerman, 1999; Levine et al., 1999; Joyce, Tan, and Zhang, 2013). Like Guldi (2008), I find that the impact of both technologies is greater on first births than second, but that abortion has a greater relative impact on second births than the pill does. These results do sharply disagree with Myers (2017) that abortion was far more influential than the pill. Appendix A draws mostly her codings for the instrument, so my reversal of her findings is puzzling, except to say we use very different datasets and settings but future work may want to investigate this discrepancy in more detail.

Compliance is lowest in the maternal grandmother regressions, but this may be because the maternal grandmother sample is most influenced by childbearing trends among single mothers. Single mothers are more likely to be poor and non-white, and there is evidence that these populations were the least likely to uptake the pill. Guldi (2008) found that pill access did not explain fertility trends among non-whites. Myers (2017) finds that the pill had stronger effects among whites, and no effects on blacks or college graduates. The paper also finds that abortion lead to greater reductions in the probability of a teen birth among blacks and those without a college education, but if so, then these effects do not appear strong enough in this sample to motivate a larger complier population.

In light of the findings in the compliance analysis tables, I break out the second-stage results by maternal versus paternal grandparents in Table 8. Again, none of the grandparenthood regressions can reject the weak instrument hypothesis, but the F-statistics in the paternal grandparent regressions are always a bit bigger than the F-statistics reported for the maternal grandparents. This is also true of the F-statistics for the paternal grandparents are always bigger than the maternal grandparent ones. The 5% worst-case bias can be rejected in all specifications except for three of the four maternal grandmother regressions: being retired (can only reject 10% worst-case bias), conditional hours worked (cannot reject weak instrument hypothesis at all), reporting non-zero hours worked (can only reject 10% worst-case bias).

One surprising result is that paternal grandfathers, thought to be the least responsive to grandchildren, show a stronger labor force detachment effect than maternal grandfathers. While maternal grandfathers are 21.3% more likely to be retired compared to 19.6% more likely in response to the marginal grandchild, paternal grandfathers are 23.9% more likely to be out of the labor force and work 473 fewer hours a year (significant at the 5% level), compared to 21.7% and 183.3 hours (not significant) for maternal grandfathers. Again, because these results are LATEs and not average treatment effects, it is possible that the adult son compliers are disproportionately more likely to come from families with higher labor supply elasticities than adult daughter compliers. These differences are beyond the scope of the current study, but point towards interesting avenues for future research.

Paternal grandmothers show a stronger extensive margin response than paternal grandfathers, being 21.4% more likely to be retired and 41% less likely to report non-zero hours (versus 19.6% more likely to be retired and 23.9% more likely to be out of the labor force). More surprising, paternal grandmothers have a smaller hours effect than paternal grandfathers, only 184.9 hours, and not statistically significant.

Most of the causal literature has focused on maternal grandmother's labor supply (Frimmel et al., 2017; Rupert and Zanella, 2018),[20] Instead, it is possible that the greatest effects are on same-sex parent-child pairings. Subtracting one standard error from each of the maternal grandmother effects would still yield either larger or similar point estimates on the retirement and labor force/non-zero hours regressions than seen from either maternal grandfathers or paternal grandmothers. Although the hours worked regression for maternal grandmothers has a weak first-stage, subtracting a standard deviation and a half from the point estimate would still yield a larger overall effect (in absolute terms) than seen from either maternal grandfathers or paternal grandmothers. Maternal grandmothers may provide the most childcare, but paternal grandfathers may see their sons as continuing the family line, and favor those grandchildren with foregone labor supply.

[20] Rupert and Zanella's study can only identify a causal effect from maternal grandparents, and only find a effect for grandmothers. Frimmel et al. [EXPLAIN].

3.3 Extending the First Stage

While the weak instrument hypothesis can be rejected for the child count regressions, changes in abortion and contraception legal access do not seem to adequately predict when a person became a grandparent. I therefore extend the first stage by adding 10 lags to each policy, which could more clearly capture both fertility delaying or total fertility effects of these laws. The first-stage regression for $ChildCount_{igst}$ and $\mathbb{1}\{Grandparent_{gst}\}$ now becomes:

$$\begin{aligned}
&ChildCount_{igst} = \pi_0 + \pi_1 PillAccess_{igst} + \pi_2 AbortionAccess_{igst} \\
&+ \pi_3 AbortionAccess_LT250_{igst} + \pi_4 PillAccessLags_{igst} \\
&+ \pi_5 AbortionAccessLags_{igst} + \pi_7 AbortionAccLags_LT250_{igst} + \nu_{igst},
\end{aligned} \tag{6}$$

and

$$\begin{aligned}
&\mathbb{1}\{Grandparent_{gst}\} = \pi_0 + \pi_1 PillAccess_{gst} + \pi_2 AbortionAccess_{gst} \\
&+ \pi_3 AbortionAccessLT250_{gst} + \pi_4 PillAccessLags_{gst} \\
&+ \pi_5 AbortionAccessLags_{gst} + \pi_6 AbortionAccessLagsLT250_{gst} + \nu_{gst},
\end{aligned} \tag{7}$$

respectively. $PillAccessLags_{igst}$, $AbortionAccessLag_{igst}$, and $AbortionAccessLags\_LT250_{igst}$ is a vector of one to ten period lags for each policy variable.

To illustrate the intuition behind the lags, recall that *Roe* was decided in January 22, 1973. Women who conceived in all of November or December 1972 (and part of October) were eligible to end those pregnancies. Thus, for eligible women living in states whose statutes were invalidated by *Roe* are coded as having access for 71/366=19.4% of 1972. Conceptions between October 1972-January 1973 would have resulted in births in July 1973-October 1973, just after the PSID had concluded most of its 1973 interviews. Thus, had those conceptions been carried to term, the children would have first "appeared" in the 1974 survey. Including the coding for the consecutive lags going back ten years accounts for possible timing combinations between conceptions, the ability to abort them, and when the PSID surveys were conducted, while allowing for a fertility delaying effect that abortion and contraception permit.

Further, given the timing of the PSID surveys, most state-by-year variation in access to the pill among adults occurred between the 1960 introduction of the oral contraceptive and 1965's *Griswold* decision.[21] Between 1968-1976, most state-by-year variation in pill access

[21] See Appendix A for more details.

occurs among those younger than 21. Thus, 10 year policy lags could potentially pick up on additional state-by-year variation among a larger pool of adults.

The new first stage results are presented in Table 9, with updated second stage results for the overall grandparent effects in Table 10 and effects for maternal versus paternal grandparents in Table 11.

The revised first stage regressions do not give strong evidence that these policies are adequate instruments for grandparenthood, because none of the contemporaneous or one-period lag coefficients on pill or abortion access are negative and statistically significant, with the exception of the contemporaneous effect on abortion access in a nearby state. Nonetheless, both the child count and the grandparenthood status regressions largely agree on several points. First, the policies' contemporaneous effects on fertility are negative, and in the case of the child count regressions, statistically significant for all policies. Second, the longer-run effects are positive: they revert to positive by the second lag for the grandparenthood regressions and by the third lag in the child count regressions. Overall, these regressions suggest that the access policies did change fertility timing across a 2-3 year window, but did little to change total fertility.

The revised second stage results presented in Table 10 have smaller point estimates across almost all specifications than what was reported in Table 5. F-statistics in the grandparenthood status regressions have also increased, so that the 10% worst-case bias hypothesis can be rejected for all specifications except the conditional hours worked regressions and the too disabled to work regression for grandfathers. While grandfathers report lower labor force attachment across specifications than non-grandfathers, none of these effects are statistically significant. Grandmothers report a significant intensive margin effect, reporting working 457 fewer hours a year, but the weak instrument hypothesis cannot be rejected so the results are not definitive.

There is only one strong result from this panel: grandmothers are 12.6% more likely to be disabled than non-grandmothers, with an F-statistic that can almost reject the 5% worst-case bias. This is a large effect, in part because the countervailing effect would be that grandmothers providing childcare are performing often physically demanding work for their families, which (assuming people are largely self-reporting truthfully) would arguably make a smaller, less-statistically significant result more readily credible. However, considering the evidence presented here holistically, the overwhelming direction for both sexes is to reduce labor supply and attachment, so the disability results are internally consistent. Given the strong evidence for SSDI's effect on inducing labor force detachment (Maestas, Mullen, and Strand, 2013), it is possible that some grandmothers are differentially using SSDI to leave the labor force and provide childcare or other family services.

Again breaking out the results into maternal versus paternal grandparents, the paternal grandparents demonstrate a stronger labor response than maternal ones. Table 11 shows that even with 10 lags, no first stage can reject the 5% worst-case bias hypothesis, although the grandmother regressions can reject the 10% worst-case bias hypothesis in the retirement, disability, and non-zero hours specifications. Given that context, both types of grandmothers show a large decrease in conditional hours worked of between 401 (maternal, significant at the 10% level) to 526 (paternal, 5% level) hours foregone. Paternal grandmothers are 19% more likely to be retired, and maternal grandmothers are 20% more likely to self-report being too disabled to work. This is in contrast to paternal grandmothers, who are only 0.3% more likely to be too disabled to work.

Compared to Table 8, the coefficients in the marginal grandchild panel are about half the size with the extended first stage. The marginal grandchild induces grandparents to be 8.0% (maternal grandmothers, 1% level) to 14.7% (paternal grandmothers, 1% level) more likely to retire; -0.6% (maternal grandfathers, not significant) to 5.8% (maternal grandmothers, 5% level) more likely to be too disabled to work. Non-retired grandmothers work between 193 (paternal, 5% level) to 339 (maternal, 1% level) fewer hours with each additional grandchild.

3.4 Robustness Checks

The above research design raises several concerns. One concern is that it introduces omitted variable bias, especially in the child count regressions, where fertility coordination between adult children is unobserved and may confound the results. Even in the grandparenthood status regression, the decision to use the eldest adult child's characteristics to populate the instrument predicting the arrival of the eldest grandchild, regardless if that adult child actually mothered or fathered that child, could be introducing misspecification via unobserved, time-varying heterogeneity between siblings. To address these concerns, I use the biggest subsample of adult child counts, two adult children, to create a first stage regression that has the total grandchild count as a function of the policy exposure measures for both adult children. Similarly, I also re-instrument for the grandparenthood dummy as a function of both adult children's policy exposure. I present the results in Table 12.[22]

One drawback to this approach is that the Montiel-Pflueger F-statistic cannot be calculated if the number of excluded instruments (66) exceeds the number of clusters (39). If the F-statistics reported for the base instruments regressions are indicative, then one can assume this approach results in weaker first-stages. Unfortunately, the F-statistics are now so low that the grandparenthood status results should not be credibly evaluated as evidence

[22]Not reported here are the first-stage results, which are similar in sign and significance as the results presented in Tables 4 and 9. These results are available upon request.

for or against the hypothesis of a labor force attachment effect.

Nonetheless, the child count results here are not dramatically different than those reported in Table 5. This is not very surprising, in that grandfathers with 2 or fewer adult children are 63% of the sample, and 59% in the grandmother sample, so that results in the original specification are going to be mostly driven by men and women with relatively none-to-limited omitted variable bias in this channel. I now find that grandfathers are 18.4% more likely to be retired with each additional grandchild using (versus 18.5% in the main specification), and work 286 fewer hours (versus 317 hours above), although this is no longer significant. Grandmothers are now 17.8% more likely to be retired, versus 26.5% more likely above, but now work 596 fewer hours (not significant) versus 484 fewer hours above (significant at the 5% level).

The child count results with 10 policy lags apiece in the first stage are also fairly similar to those reported, with perhaps mild shrinkage in the point estimates. The most substantive difference is that the grandmother intensive margin effect shrinks from 299 fewer hours to 229 fewer hours worked. The positive effect on disability remains, and grows, so that each grandchild increases the probability a grandmother self-reports being too disabled to work by 5.1%. While it is beyond the scope of this paper to investigate this unexpected result, I note that if the above regressions are any indication, this is being driven by maternal grandmothers.[23]

The last robustness check is to ensure that the first-stage is not in fact confounded with age effects, because so many of the eligibility requirements were based on age. As age is an important determinant of fertility and labor force attachment, I control for this potential omitted variable bias by introducting 10 year age range fixed effects for the adult children and interact them with year fixed effects and state fixed effects to saturate the model. Table 13 shows that the child count regressions remain largely unchanged by this introduction, particularly that the weak instrument hypothesis can still be rejected. Unfortunately, it remains the case that the grandparenthood status regressions cannot be causally interpreted with such weak F-statistics, although I note that in the 10 policy lag regression for grandmother's disability status, the 10% worst-case bias hypothesis can be rejected and there is again evidence that grandmothers are more likely to self-report being too disabled to work.

The base instrument regressions in the child count panel are not meaningfully different in most cases from those results reported in Table 5. Perhaps the only meaningful difference is that grandmother's intensive margin effect shrinks from 483.9 fewer hours to 133 fewer hours

[23] As noted above, it may be that the maternal grandmother regressions may have a greater share of single mothers, so that the effects are more sensitive to trends among those in poverty, including a reliance on public assistance.

and loses its significance, and the non-zero hours effect from the marginal grandchild is cut roughly in half, from 51.6% to 23.3%. The 10 policy lag regressions, however, show a greater difference from the Table 10 results. Now, the marginal grandchild causes grandfathers to be 5.7% more likely to be retired, instead of 10.6%. Further, now grandfathers will work 131 fewer hours (significant at the 1% level) a year, as opposed to 59 fewer hours and not significant. Grandmothers' response has also changed, being now only 8.5% more likely to be retired (originally 11.6%), 8.4% less likely to report non-zero hours worked (originally 13.8%), and work 120 fewer hours a year (originally 299 fewer hours). Also pertinent is that the disability status regression loses its significance altogether, while being the only regression to still reject the 5% worst-case bias hypothesis.

3.5 Conclusions on PSID Results

The results in the specifications including fixed effects for adult children's age ranges (Table 13) show the most modest effects for grandchildren on grandparents labor force attachment, but nonetheless, still present economically meaningful effects of grandchildren on grandparents' labor supply. I can only reject the weak instrument hypothesis of 10% worst-case Nagar bias in the grandparenthood regressions for grandmothers when I include 10 policy lags on each access instrument. In those cases, the only significant finding is that grandmothers are 12.6%-14.5% more likely to be disabled to work. Table 11 suggests that this is being mostly driven by maternal grandmothers. I hypothesize that since these grandparents are most exposed to trends among single mothers, this reflects that maternal grandmothers may be most likely to use SSDI benefits, but the PSID does not provide enough data to test this.

From the 10 policy lags panel, I find that each additional grandchild increases grandfather's likelihood of being retired by 5.7% decreases the total annual hours worked by 131 hours (5.25 work weeks). Grandmothers respond to the marginal grandchild by working 120 hours fewer a year, become 8.5% more likely to be retired, and become 8.4% less likely to report non-zero hours worked. Between 1970 to 2015, I estimate in Section 4.2 that the average number of grandchildren has fallen by about 2, which should translate (*ceteris paribus*) into roughly 11.5% decrease in the number of retired men and a 17% decrease in the number of retired women. A cursory examination of the LFP trends graph in Figure 1 shows that 1970 LFP was only slightly lower than 2015 LFP, but the large trough in between suggests that the fertility fall may have played some intermediating role.

Compared to what is found in Rupert and Zanella (2018) and Frimmel et al. (2017), I find a generally more modest grandmother intensive margin response. Frimmel et al. finds that each additional grandchild reduces grandmothers' labor supply by 0.4 years, and Rupert and Zanella find a 30% reduction in hours worked. Although the Rupert and Zanella result is along the extensive grandparenthood margin, a 30% reduction in hours would translate into about 260 fewer hours a year using the averages reported in Table 2. Thus, while these results are not greatly different than those found by Rupert and Zanella, these results have some credibility advantages in terms of instrument validity and being able to use individual fixed effects at both the grandparent and adult child level.

In all, using the most conservative estimates, the marginal grandchild reduces grandparents' hours supplied by about a month and substantively increases the probability of being retired. Given that the average person over age 50 now has about 4 grandchildren, we would expect grandfathers to be about 22.8% more likely to be retired than the grandchildless and to work 520 fewer hours, or about 0.26 of a 40 hour, 50 week working year. Likewise, for grandmothers, we expect them to be 34% more likely to be retired and to work 480 fewer hours, or 0.24 of a working year.

A key contribution across Tables 5 to 13 is the presence of a grandfather retirement response to grandchildren. Estimates range from 21.3% to 5.7%, but a statistically significant response is robust to every attempted specification. While the typical finding is that men's labor supply is inelastic to children, men with families may simply backload their labor attachment response until after they are out of their prime working years. Thus, the results here support the hypothesis that fertility trends may have played a significant role in older men's post-WWII labor force participation.

4 National Labor Force Participation Trends Estimation

The results of Section 3 suggest that grandchildren alter grandparent's labor supply at different rates depending on their retirement eligibility. This results informs the empirical strategy for national-level trends because it gives a starting place for the expected lag between the adult children's fertility decision and the grandparent's response.

I now build off of Blau and Goodstein (2010) to estimate how changes in the supply of grandchildren change older workers' labor supply. They use data from the CPS and the Social Security Administration (SSA) to model the employment decision rule for older workers. Their model accounts for how variation in Social Security benefits, disability insurance, educational attainment, and the labor force participation of spouses impact the fall and rise in labor force participation among men aged 55-69. Identification in their model occurs from variation at the year-by-birth year-by-education group level.

Their paper also focuses exclusively on older men, and for this analysis, I too will only analyze the labor force participation of men. Given the sea change in labor force attachment shown by women between 1962 and the present, credibly estimating a model for women is an exercise that will be left for future research. I will also use the CPS instead of the PSID, in part because the PSID sample was unrepresentative of the nation at various points in its cycle, and the CPS is designed specifically to permit credible estimates of national-level descriptive statistics from micro data.

I augment their model by extending the time series out to 2015 and by adding two grandparent measures: the fraction grandparents and the average number of grandchildren. I run all specifications under the assumption that agents have perfect foresight, because it is a more "conservative" assumption from an identification standpoint.[24]

Addressing endogeneity by means of an instrumental variables approach like the one used in Section 3 is not straightforward in the CPS. Expanding the panel to the year-by-birth year-by-age-by-education level-by-state would allow me to identify changes in grandparenthood characteristics using state-level birth rate variation. However, this would come at the cost of generating many small cell counts and thus limits identifying variation in all causal channels with a dataset like the CPS, which can only survey so many households at a time. Further, the ability to generate grandparent measures at such a fine level is currently not feasible with existing data sources. Thus, this exercise is included mostly as an exploration of grandparenthood's effect given observed patterns, and not as a causal exercise. Nevertheless, given how little is understood about these trends, simulating how grandparenthood trends impacted labor force participation trends can still help motivate future research on these questions.

4.1 Labor Force Participation Model: Blau and Goodstein Extension

I begin by modifying the model created by Blau and Goodstein (2010), which looks at labor force participation among older men by creating a simulated panel of older men by

[24]Papers that have assumed myopic expectations have generated results that are counterintuitive and specification-sensitive (Kreuger and Pischke (1992); Blau and Goodstein (2010)).

year by birth year by education grouping and takes the form:

$$\begin{aligned}
&LFP_{eabt} = \delta_0 + \delta_1 GP_Measure_{eabt} + \delta_2 SSB65_{eb} + \delta_3(SSB62_{eb} - SSB65_{eb}) \\
&+ \delta_4(SSB62_{eb} - SSB65_{eb}) + \delta_5 AME_{eb} + \delta_6 DisabilityBenefit_{eabt} \\
&+ \delta_7 ln(PredictedWage_{eat}) + \delta_8 Demographics_{eabt} + \delta_9 EducationGroup_e \\
&+ \delta_{10} Year_t + \delta_{11} BirthYear_b + \delta_{12} Age_a + \epsilon_{eabt},
\end{aligned} \tag{8}$$

where $GP\_Measure_{eabt}$, the key variable of interest, is either the fraction who are grandparents in each age cohort a and birth year b in year t at education attainment level e or the number of grandchildren; $Demographics_{eabt}$ controls for the fractions married, previously married, white, black, U.S. Armed Services veteran, or reported being in bad health; $EducationGroup_e$ is a vector of indicators for either having less than high school education, a high school education, some college, or college-plus; $Year_t$ is a vector for year dummies; Age_a is a vector of age dummies; and $BirthYear_b$ is likewise a vector for birth year dummies.

The Blau and Goodstein empirical model approximates the decision rule for labor force participation at older ages under a life cycle model of employment and retirement where men seek to maximize the expected present discounted value of remaining lifetime utility, subject to various constraints.[25] The decision rule for Social Security participation is estimated by means of the retirement benefits a worker could receive at ages 62 ($SSB6_{eb}2$, early retirement), 65 ($SSB65_{eb}$, or full retirement), and 70 ($SSB70_{eb}$, or delayed retirement). Differencing between $SSB6_{eb}$ and $SSB65_{eb}$ models the tradeoff between early and full retirement, and likewise, the difference between $SSB70_{eb}$ and $SSB65_{eb}$ the tradeoff between earning the Delayed Retirement Credit (DRC) and accepting full retirement, or primary insurance amount (PIA). To separate the Social Security wealth effect from changes in lifetime earnings, the average monthly lifetime earnings, AME_{eb}, from ages 27 to 65 for the average worker in birth cohort b at education level e is included. Higher values of $SSB70_{eb} - SSB65_{eb}$ imply a stronger incentive to delay retirement, and likewise, lower (more negative) values of $SSB62_{eb} - SSB65_{eb}$ also imply a stronger incentive to delay retirement. $ln(PredictedWage_{eat})$ models the log of the predicted prevailing wage that incentivizes increasing the worker's labor supply.

The model includes the average monthly Social Security Disability Insurance amount received by a worker in birth cohort b at education level e if they were to work until year $t-2$, receive no earnings in year $t-1$ and then be on SSDI from year t until age 65. The lack of earnings in year $t-1$ mimics the 5 month waiting period a worker must observe before receiving SSDI.

[25] More information on their model can be found on Blau and Goodstein (2010), p. 332.

The model also includes the fraction of married men whose spouse's are in the labor force, *pace* Schirle (2008) and Gustman and Steinmeier (2000) that men may prolong their labor force attachment out of a desire to jointly retire with their wives.

The birth year dummies are particularly important with respect to identification of effects other than grandparenthood. Changes in Social Security can typically be identified either by exogenous changes in eligibility rules, non-linearities in benefit rules, or variation in lifetime earnings growth across birth cohorts. The first is perfectly collinear with birth year fixed effects, and in Blau and Goodstein (2010), they report that relying on variation other than these exogenous rule changes yields counterintuitive and problematic results that do not seem to capture the variation in labor force participation. Thus, I anticipate that the results will be sensitive to what level of birth year fixed effects I include, so I run several specifications to control for birth year effects: the birth year squared; single year birth year fixed effects; the birth year squared plus 2 year birth cohort fixed effects; and the birth year squared plus 4 year birth cohort fixed effects.[26] In the tables, I refer to the specifications without birth cohort fixed effects as those with "Timetrends".

Lastly, since the results from Table 5 suggest that grandfathers are mostly influenced through the labor extensive margin, it stands to reason that grandfathers may have higher reservation wages than the grandchildless. Thus, I interact the employment decision variables with the grandchildren measures under the hypothesis that grandparenthood induces an upward shift in reservation wages.

4.2 National-Level Estimation Results

Table 14 presents the results of estimating Equation (8) with and without interactions with the employment decision variables, both for the fraction that are grandfathers and with the average number of grandchildren as the key variables of interest. I present four different specifications for controlling for the impact of birth cohort: birth year as a second order polynomial, birth cohort fixed effects, 2 year birth cohort fixed effects and the birth year squared, and 4 year birth cohort fixed effects and the birth year squared. Like Blau and Goodstein, my results are sensitive to how the birth cohort effect is controlled for.

All fractions are multiplied by 100 before the regressions are run, so coefficients for the remaining regressions (unless otherwise noted) are interpreted as the amount the LFP rate changes on the scale of (% in LF) $*$ 100. The coefficients in the first four rows represent the change in older workers' LFP rate in response to a 1 point increase in the fraction of older men who are grandfathers. Columns 1, 3, and 4 show that 1 point increase in the

[26]The first order birth year is perfectly collinear with year and age fixed effects, so I omit it.

fraction of older men who are grandparents lowers the labor force participation rate by between 0.19-0.63 points. Adding interactions for retirement eligibility changes this range to a drop of 0.66 to 0.95 points. The marginal grandchild likewise decrease older male workers' LFP, either ranging from a 2.4 rate point drop (Column (3)) to a 7.8 point drop (Column (1)). Adding interactions with retirement eligibility changes this range from the marginal grandchild causing a 6.8 point drop in LFP (Column (3)) to a 12.7 point drop (Column (1)).

The clear outlier here is Column 2, which finds labor supply drops 29.9 points with no interactions with each 1 point increase in the fraction grandparent. The results for grandchild count are likewise implausible: each additional grandchild is found to decrease labor force participation by 369.5 points. The problem is that the remaining identification after birth year fixed effects are included comes through year-by-birth cohort, education group-by-birth cohort, or education group-by-birth year-by-year variation, such that unobserved shocks only impacting certain segments of a birth cohort or a birth cohort only in certain years are neither well-motivated nor well-understood in this empirical framework.[27] *Pace* Blau and Goodstein, 2 year birth cohort effects also leave little between-cohort variation, so I will only present results that either have the birth year squared or 4 year birth cohort fixed effects for the remainder of the paper.

In Table 15, I present the remaining coefficient estimates for the quadratic birth cohort and 4 year birth cohort effects models from Equation (8). Notably, there is practically no difference in coefficients whether I use $\% \, Grandfather$ (Column (1) and (2)) or *Grandchild Count* (Columns (5) and (6)) as my grandparent control. This suggests that grandparenthood is both an important factor in predicting LFP but that to a striking degree the two measures capture much the same variation. Also notable is that with a few exceptions, the coefficients here share the same signs as those reported in Blau and Goodstein. The social security and monthly disability benefit amounts are scaled down by 100, so that the coefficients represent the point change in the LFP rate in response to a $100 increase in these benefits. This interpretation is also true for the lifetime average monthly earnings coefficients. The wealth effect from Social Security benefits causes the sign on $SSB65$ to be negative, and a smaller gap between the PIA and benefit levels available at 62 causes greater labor force detachment, although this is not statistically significant in my model. Likewise, a greater credit for remaining in the labor force past the FRA ($SSB70 - SSB65$) prompts a higher LFP rate.

I also find that a 1 point increase in being in bad health lowers the LFP rate by between

[27] Blau and Goodstein also find that using birth cohort fixed effects yields counterintuitive and implausible results, because the remaining policy variation in Social Security benefits comes through non-exogenous rule changes in benefit calculations.

0.71 to 0.79 points, and that a 1 point rise in the fraction married or fraction previously married raises LFP by between 0.11 to 0.17 points and 0.07 to 0.18 points, respectively. A rise in the fraction who are veterans also decreases labor force participation, but this effect is small and not always statistically significant, with the significant values ranging between 0.06 to 0.07 points.

In the eligibility interactions (Columns (3), (4), (7), and (8)), combined with the interactions in Table 14, shed some light on whether grandparenthood prompts an employment decision response commensurate with the findings from the PSID. Namely, that their value of leisure increases, so that grandfathers would disproportionately respond to increases in their wealth or reservation wages by dropping out of the labor force. While in these tables cannot be interpreted causally, they do largely agree with this intuition. The coefficients on the interactions with the social security benefit levels all have the expected signs: negative (and significant) on the FRA amount, negative (but not significant) on difference between the early retirement and FRA amounts, and positive (and significant) on the difference between the delayed retirement and FRA amounts. Similarly, the lifetime average earnings interaction is negative, suggesting that grandfathers respond to increased wealth by disproportionately leaving the labor force. The log predicted wage interaction is positive and significant, although the *ex ante* hypothesis is more ambiguous, it implies that increased wages draw marginally more grandfathers to remain in (or enter) the labor force.

The only puzzling result is on the interaction with the monthly disability benefit, which is positive, significant, and of essentially the same magnitude across all specifications. Since disability benefits can also raise reservation wages, the positive sign is hard to explain in context of the other results. As the results are descriptive, not causal, it seems likely an artifact of some heretofore unexplored source of bias. The PSID results from Section 3.5 would condition us to expect either no effect or a slight, negative one. However, the outcome used is self-reported disability, not SSDI receipt. Taking these results at face value, they suggest that grandfathers are less likely to uptake disability benefits than the grandchildless. This means that grandfathers are saving their grandchild leisure time consumption for retirement, and perhaps are mindful of the impact on their extended families while being out of the labor force during their prime working years.

Compared to the regressions without interactions, the main effects on the employment decision variables (presented in Table 15) flip signs but retain their significance. Since these are the main effects after the interaction between two continuous variables, the true net effect is going to reflect the effect at the mean of the interacted variable. Table 16 thus presents the marginal effects of both the grandparent measures and the employment decision variables.

It's important to note that the net marginal impact of the grandparent measures is only

1/3 to 1/2 the size of the grandparent effect reported in the first row of Table 14, with the net marginal effect from grandchildren consistently smaller in the regressions with 4-year birth cohort fixed effects, the net marginal effect of a \$100 increase in the FRA amount is positive in these regressions. Since this is highly implausible, I will focus from here on out on the regressions with just birth cohort time trends (Columns (1) and (3)).

The net marginal effect on the fraction grandparent from Column (1) compares favorably with the point estimate found from the PSID 2SLS estimates: a 1% increase in the fraction grandparent decreases the LFP rate by 0.19%. As noted in Section 3.2.3, the PSID results imply that a 10% rise in the number of grandfathers will induce approximately 2% more men to retire. Here, I find that a 10% increase in the fraction grandparent would decrease the LFP rate by 1.9%. The net marginal impact of all variables are as expected, with the exception of the log predicted wage, which is negative and statistically significant at the 1% level. In a non-causal context, this could be correlated with greater wealth accumulation and could be entering the results as a wealth effect.

Given that grandparenthood seems to have had a robust effect across specifications, it is worth seeing whether trends in grandparenthood may have played a role in trends in LFP rates, even if the net effect was not huge. I present in the next sections simulations based on the results from Tables 14 and 15's Columns (1) and (5) that iterate over different counterfactual fertility scenarios to see how LFP rate trends would have been historically altered.

4 Counterfactual Simulations

Given that labor force attachment changes from grandfatherhood seem to be concentrated around retirement, how did the shrinking in extended families seen since the 1960's and 1970's change overall labor force participation? To put some context on the results in Tables 5, 14, 15, and 16, I simulate older worker's labor force participation from 1962 onward using 4 different scenarios:

1. **No Baby Boom**: I assume that the post-WWII "boom" never happened, so that the birth rate was essentially unchanged from 1939 to 1965.

2. **No *Roe***: I assume that abortion was never nationally legalized, and extend the birth rates observed in 1970-1972 outwards to the present.

3. **Ultra Low Fertility**: I assume that the birth rate has been the same as the minimum one observed, which nationally was 2015's value of 12.4.

4. **Ultra High Fertility**: I assume that the birth rate has been the same as the maximum one observed, which nationally was 1957's value of 24.9.

To put the greatest possible weight on the influence of grandchildren and for overall model plausibility, I use the specifications with the employment decision interactions but without 4 year birth cohort fixed effects, corresponding to Columns (1) and (5) in Table 14. If by using the results "friendliest" to grandparenthood's LFP impact, I fail to show that trends in grandchildren made little impact in older worker's labor force attachment overall, then I can conclude that grandchildren's aggregate impact is not very material to understanding these LFP trends.

Figure **??** shows the counterfactual simulation results for men 55-69. Figure 5a shows the fraction of men aged 55-69 who were grandfathers from 1962-2015, their observed LFP rate, the model's predicted LFP rate, and then the predicted LFP rate for the 4 counterfactual scenarios for historical fraction grandparent values. Figure 5b is structured similarly. Both figures show that, absent the Baby Boom, the LFP rate would have been much 2-4 points higher through 1990, before slowly converging to the model's predicted LFP rate in 1994. Surprisingly, the *Roe* scenario shows that at least in this crude counterfactual, the fall in fertility after *Roe* matters little for the LFP rate for older workers. The greatest gap is observed only in the most recent year of data, where the observed LFP rate is 2 points higher than the simulated LFP rate without *Roe* in the average grandchild count and fraction grandparent versions.

The ultra-low and ultra-high scenarios reveal two interesting LFP trends. Sustained high fertility would have yielded a substantially LFP rate 3-5 points for older workers until around 1975 - nearly 20 years after the 1957 fertility peak. Thereafter, the gap grows between observed and simulated LFP rates to almost 10 points by the 1994 nadir, before exhibiting some convergence in both graphs. The fraction grandparent graphs shows that by 2015, an ultra-high fertility world would have had an LFP rate about 5 points lower. The average grandchild count scenarios shows a much larger gap of about 9 points in 2015, reflecting most likely that most people still get grandchildren in both scenarios, but an ultra-high fertility scenario would have meant much larger extended families than what was actually observed.

Conversely, a sustained ultra low fertility rate would have risen the LFP rate by 4 points in the mid-1960's in the fraction grandparent version or 6 points in the average grandchild count version, but these trends would have completely converged by 2015. These contrasts suggest that the grandparenthood elasticity of labor supply was likely bigger in the 1960's than it is today, possibly because other forces not previously at play are now more strongly shaping older worker's labor force attachment decisions, such as joint spousal retirement decisions.

One phenomenon clearly demonstrated by the fertility scenarios is that altering the grandparenthood assumptions does not meaningfully change the fall and rise pattern in older men's LFP during this period. The counterfactuals show that certain stylized facts about the LFP rate are unrelated to grandchildren trends. No matter the grandchild scenario and both for the 62-64 and 65-69 group, 1994 remains as the nadir for older men's LFP rate, although different grandchild levels would have slightly changed what that nadir was. Despite several significant changes in extended family composition between 1962 to the present, grandparenthood seems to have played only a minor role in shifting LFP trends among older workers.

One important factor that this exercise does not consider is that dependency ratio forecasts were a key factor in the Social Security Amendments of 1965, 1972, 1977, and 1983, which increased and then decreased benefits. Since I do not control for what Social Security benefits likely would have been under these alternative scenario, it is possible that even in the context of this exercise, the grandparenthood effect on these trends is being understated.

5 Discussion and Conclusion

This paper explores grandparents' labor force attachment by testing various labor outcomes along the extensive and intensive margins of grandparenthood. The first-stage weak instrument hypothesis is n The evidence presented here is that there is a grandparenthood effect on labor force attachment, and that grandparents of both sexes tend to lower their labor force attachment as their extended families grow. Both grandfathers and grandmothers have lower labor force attachment, and this labor supply reduction increases with family size.

I find that along the extensive margin of grandparenthood, I find that grandmothers are 12.6%-14.5% more likely to report being too disabled to work relative to the grandchildless, and this effect seems to be driven exclusively by maternal grandmothers.

Along the intensive margin of grandparent, the most conservative estimate is that they work 120 fewer hours per year with each additional grandchild. All specifications grouping maternal and paternal grandmothers together find a reduction in annual hours worked if non-retired, but this effect is larger for maternal grandmothers. Grandmothers also become 8.5% more likely to be retired in response to the marginal grandchild, whose effect appears to be driven more by paternal grandmothers. Maternal grandmothers are 5.8% more likely to self-report being too disabled to work.

A key finding of this paper is that grandfathers have a significant retirement response to the marginal grandchild across all specifications, with a midpoint estimate of 12.1%. The most conservative estimates still find that grandfathers become 5.7% more likely to be retired. Grandfathers do not exhibit a substantially different response to their sons' versus their daughters' children.

In the limited literature on this question, grandfathers' roles have not received as much attention as grandmothers', but the findings here indicate that this is an oversight, because grandfathers are shown to decrease their labor force response across multiple specifications. I therefore also examine how the post-Baby Boom increase in grandchildren affected the fall in older men's labor force participation seen between 1970 and 1994, and then it's subsequent rise from 1994 to the Great Recession's advent by using the CPS to estimate representative yearly samples of male workers, aged 50-69. I find significant interactions between grandchildren, retirement eligibility, and retirement benefits, but these interactions do not explain why the labor force participation rate fell, even though it did somewhat coincide with "peak" grandparenthood. Across all alternative historical grandparenthood scenarios, I find that the fall and rise would have occurred regardless, although the depth of that change would have been more gradual if the 1970's had not seen a grandparenthood peak.

There are some currently unaddressed issues that future drafts will consider. These include using the PSID to study how grandparents react specifically to the youngest grandchildren, in an effort to better understood what role grandchildren play in the grandparent's lifecycle. Another extension is using the CPS data to see what role the decline in grandparenthood has played in the rising labor force participation among older female workers. Lastly, given the significance of Social Security and grandparenthood interactions, exploring other simulations that make changes to both parameters. This will also account for how policymakers make decisions about Social Security, such that a permanent boom or permanent bust in fertility would almost certainly provoke a response by policymakers to adjust Social Security accordingly.

This paper uses estimates adult children's fertility impact on grandparents' labor market outcomes using exogenous variation in access to reproductive technology. Grandchildren's influence is examined through the effect of being a grandparent and the total number of grandchildren per household. Although much of the policy variation in the fertility instrument is historical, the results complement existing findings on grandparent aid to new parents by determining that the time transfers from grandparent to adult child are likely coming out of the labor supply of the grandmother. This paper is also the first in the literature to document a grandfather labor market response to grandchildren, an important contribution because policymakers wishing to model how new generations affect old ones need a clearer understanding of how both older worker types may change their behavior.

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TABLE 1
Time Transfers (in Hours) By Number of Grandchildren

Number of Grandchildren ⇒	**0**	**1**	**2**	**3**	**Any Child**
Mother's Parents					
Married Grandparents	21.48	58.25	52.01	51.35	55.40
Grandfather Remarried	3.51	12.88	9.63	7.38	9.23
Grandmother Remarried	40.18	59.78	19.28	42.88	43.64
Single Grandfathers	25.02	19.42	5.18	2.14	13.79
Single Grandmothers	21.24	83.17	25.21	44.13	45.31
All Mother's Parents	**19.35**	**57.31**	**27.07**	**36.31**	**37.98**
Father's Parents					
Married Grandparents	21.32	16.79	51.33	68.22	47.24
Grandfather Remarried	3.04	7.21	2.39	2.12	4.21
Grandmother Remarried	27.64	173.41	11.48	35.03	64.59
Single Grandfathers	1.92	1.49	6.06	2.31	5.76
Single Grandmothers	16.04	36.22	10.15	2.22	14.20
All Father's Parents	**15.59**	**33.42**	**18.90**	**21.41**	**22.89**
All Grandparents	**34.95**	**90.72**	**45.97**	**57.72**	**60.86**

Table 1 shows average time transfer in hours from parents to adult children, separated out on the basis of how many children the adult children have.
Source: 2013 PSID Family Rosters and Transfers.

TABLE 2
Summary Statistics for Grandparent Sample, 1968

	Grandfathers		Grandmothers	
	Mean	**St Dev**	**Mean**	**St Dev**
Age	47.7	14.52	46.5	15.4
Number of Children	2.59	1.50	2.61	1.53
Age When First Grandchild Born	52.4	8.65	49.5	9.3
Age When First Retired	57.5	9.84	56.7	11.5
Fraction Retired	21.2%	0.409	20.6%	0.40
Fraction Disabled	14.0%	0.347	16.7%	.373
Annual Hours Worked	1,899.7	1,029.58	872.9	966.7
In Labor Force (ILS)[a]	74.9%	0.433	40.5%	0.49
Fraction Married	73.0%	0.444	72.0%	0.45
Grandparents	5,205		7,611	
Adult Children	12,738		19,361	
Obs. in Grandparenthood Sample	94,687		136,393	
Obs. in Grandchild Count Sample	206,412		326,638	

Table 2 shows selected summary statistics for in-sample men and women who were candidates to become either a grandfather or a grandmother, namely that they have at least one child in the PSID.

[a] Labor force status for the spouse is first reported until 1976, and first reported for individuals in 1979. Thus, for women, I instead use whether they report working more than 0 hours last year, which has a correlation coefficient with the observed labor force status of 0.55 to ensure that I can consistently observe a proxy for women's labor force participation from at least 1968 onwards.

TABLE 3
Panel Fixed Effects Estimation of Grandparents' Labor Response to Grandchildren

Grandchild Measure ⇓	**Grandfathers**				**Grandmothers**			
	Retired	Disabled	Cond. Hrs Worked	In Labor Force	Retired	Disabled	Cond. Hrs Worked	Non-Zero Hours
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
				Grandparent Status Regressions[a]				
$\mathbb{1}$**{Grandparent}**	0.048***	0.006	29.16	-0.014	0.020**	0	-74.37**	-0.030***
	(0.009)	(0.010)	(20.64)	(0.009)	(0.010)	(0.010)	(29.01)	(0.011)
Adj. R^2	0.77	0.46	0.49	0.65	0.74	0.41	0.55	0.54
F	192.7	10.5	11.4	234.8	101.3	8.7	82.8	90.7
N	59,814	51,829	34,712	57,947	95,427	84,072	58,095	95,427
				Child Count Regressions[a]				
Child Count	0.036***	-0.001	-8.42	-0.012**	0.018***	0.002	-47.53***	-0.016***
	(0.007)	(0.004)	(9.51)	(0.004)	(0.006)	(0.004)	(13.15)	(0.004)
Adj. R^2	0.77	0.48	0.50	0.66	0.74	0.42	0.57	0.55
F	278.8	11.2	10.9	300.2	144.1	11.3	75.5	94.0
N	163,804	139,206	92,989	159,531	268,240	236,374	164,131	268,240

Table 3 shows the panel fixed effects regression estimates for the $\mathbb{1}${Grandparent} measure of the effect being a grandparent on labor force attachment outcomes. Likewise, the coefficients for *Child Count* measure the marginal effect of an additional grandchild on each outcome. All regressions include individual-level and state-by-year fixed effects. Regressions are weighted with the core family sampling weights provided by the PSID, where the child count regressions are adjusted for the number of adult children each grandparent has. Robust standard errors clustered at the state level are reported. All regressions include the adult child and grandparent demographic variables listed in Section 3.1.

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

TABLE 4
First-Stage Estimates of Grandchild Measures from PSID

	Grandfathers				Grandmothers			
Access Measure ⇓	Retired	Disabled	Cond. Hrs Worked	In Labor Force	Retired	Disabled	Cond. Hrs Worked	Non-Zero Hours
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Grandparent Status Regressions[a]								
Pill Access	-0.008	-0.023	-0.014	-0.011	-0.002	-0.014	-0.012	-0.002
	(0.027)	(0.023)	(0.021)	(0.026)	(0.025)	(0.022)	(0.022)	(0.025)
Abortion Access	-0.032	-0.023	-0.027	-0.031	0.008	0.013	0	0.008
	(0.028)	(0.026)	(0.028)	(0.029)	(0.023)	(0.019)	(0.031)	(0.023)
Abortion $\leq$ 250 Mi.	-0.097***	-0.086***	-0.075***	-0.097***	-0.105***	-0.091***	-0.068***	-0.105***
	(0.017)	(0.020)	(0.017)	(0.018)	(0.015)	(0.015)	(0.018)	(0.015)
Adj. R^2	0.78	0.76	0.76	0.78	0.78	0.75	0.78	0.78
N	59,814	51,829	34,712	57,947	95,427	84,072	58,095	95,427
Child Count Regressions[a]								
Pill Access	-0.150***	-0.160***	-0.117***	-0.158***	-0.124***	-0.151***	-0.106***	-0.124***
	(0.028)	(0.020)	(0.018)	(0.027)	(0.034)	(0.022)	(0.023)	(0.034)
Abortion Access	-0.109**	-0.079**	-0.036	-0.105**	-0.059	-0.042	-0.009	-0.059
	(0.041)	(0.038)	(0.039)	(0.043)	(0.042)	(0.034)	(0.046)	(0.042)
Abortion $\leq$ 250 Mi.	-0.220***	-0.218***	-0.151***	-0.223***	-0.189***	-0.184***	-0.098	-0.189***
	(0.075)	(0.068)	(0.032)	(0.076)	(0.049)	(0.050)	(0.058)	(0.049)
Adj. R^2	0.77	0.72	0.73	0.77	0.78	0.73	0.79	0.78
N	163,804	139,206	92,989	159,531	268,240	236,374	164,131	268,240

Table 4 shows the first-stage regression results estimating Equation (4) for the grandchild measure *Child Count* and Equation (5) for $\mathbb{1}\{Grandparent\}$. “Pill Access” and “Abortion Access” are the treatment variables for whether the adult daughter had access to the reproductive technology in year t. “Abortion $\leq$ 250 Mi.” is a dummy for whether individual i’s 1968 state was within 250 miles of a state with legalized abortion prior to *Roe*. All regressions include state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has. Robust standard errors clustered at the state level are reported.
* p<0.10, ** p<0.05, *** p<0.01
[a] All regressions include the adult child and grandparent demographic variables listed in Section 3.1.

TABLE 5
2nd-Stage IV Results of Grandparents' Labor Response to Grandchildren

	Grandfathers				Grandmothers			
Grandchild Measure ⇓	Retired	Disabled	Cond. Hrs Worked	In Labor Force	Retired	Disabled	Cond. Hrs Worked	Non-Zero Hours
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Grandparent Status Regressions[a]								
$\mathbb{1}\{$**Grandparent**$\}$	0.668***	0.103	-1,210.97	-0.704***	-0.009	-0.002	-535.34	-1.869***
	(0.223)	(0.175)	(737.34)	(0.254)	(0.198)	(0.134)	(964.26)	(0.365)
N	59,814	51,829	34,712	57,947	95,427	84,072	58,095	95,427
Montiel-Pflueger F	6.52	5.39	3.69	6.62	6.16	5.09	3.28	6.16
5% Crit. Value	*26.28*	*27.06*	*29.70*	*25.71*	*20.50*	*21.29*	*22.18*	*20.61*
10% Crit. Value	*16.06*	*16.45*	*18.02*	*15.72*	*12.67*	*13.16*	*13.61*	*12.68*
Child Count Regressions[a]								
Child Count	0.185***	0.014	-316.71*	-0.199**	0.265***	0.027	-483.85*	-0.516***
	(0.050)	(0.041)	(161.31)	(0.038)	(0.063)	(0.061)	(248.92)	(0.110)
N	163,804	139,206	92,989	159,531	268,240	236,374	164,131	268,240
Montiel-Pflueger F	97.94	98.48	45.75	99.06	69.93	79.02	34.49	69.93
5% Crit. Value	*26.92*	*25.20*	*27.13*	*27.13*	*24.58*	*22.66*	*26.31*	*24.43*
10% Crit. Value	*16.37*	*15.47*	*16.69*	*16.49*	*15.00*	*13.85*	*16.09*	*14.92*

Table 5 shows the second-stage regression estimates of grandparents' labor force characteristics. Second-stage treatment variables are from Equation (4) and the grandparent flag from Equation (5). All regressions include individual-level and state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has where necessary. Standard errors are clustered at the state level.

[a] All regressions include the adult child and grandparent demographic variables listed in Section 3.1.

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

TABLE 6
Complier Characteristics Across Samples
Treatment (D)=Becoming a Grandparent ($\mathbb{1}\{$Grandparent$\}$)

Instrument and Sample	P[D=1]	First-Stage $P[D_1 < D_0]$	P[Z=1]	Compliance Probabilities $P[D_1 < D_0 \mid D = 1]$	$P[D_1 < D_0 \mid D = 0]$
<u>Pill Access</u>					
All Grandfathers	0.610	0.008	0.723	0.009	0.006
Maternal Grandfathers	0.595	0†	0.824	0	0
Paternal Grandfathers	0.624	0.045	0.628	0.045	0.045
All Grandmothers	0.608	0.002	0.736	0.002	0.003
Maternal Grandmothers	0.589	0†	0.827	0	0
Paternal Grandmothers	0.627	0†	0.644	0	0
<u>Abortion Access</u>					
All Grandfathers	0.610	0.032	0.696	0.037	0.025
Maternal Grandfathers	0.595	0.045	0.793	0.060	0.023
Paternal Grandfathers	0.624	0†	0.606	0	0
All Grandmothers	0.608	0†	0.711	0	0
Maternal Grandmothers	0.589	0†	0.795	0	0
Paternal Grandmothers	0.627	0†	0.626	0	0
<u>Abortion $\leq$ 250 Mi.</u>					
All Grandfathers	0.610	0.097***	0.010	0.002	0.246
Maternal Grandfathers	0.595	0.074**	0.012	0.001	0.181
Paternal Grandfathers	0.624	0.173***	0.009	0.002	0.456
All Grandmothers	0.608	0.105***	0.012	0.002	0.265
Maternal Grandmothers	0.589	0.084***	0.014	0.002	0.202
Paternal Grandmothers	0.627	0.155***	0.010	0.002	0.411

Table 6 reports estimates of the complier population drawn from the first-stage estimates reported in Table 4, and unreported runs on the sex of the adult child.
* p<0.10, ** p<0.05, *** p<0.01

TABLE 7
Complier Characteristics Across Samples
Treatment (S)=Having a Marginal Child ($\Delta ChildCount_{it} \geq 0$)

Instrument and Sample	$P[Z=1]$	For $0 \leq S \leq 1$: $P[S=1]$	Weighted[a] First-Stage $P[S_1 < S_0]$	Compliance Probabilities: $P[S_1 < S_0 \mid S=1]$	$P[S_1 < S_0 \mid S=0]$	For $1 \leq S \leq 2$: $P[S=2]$	Weighted[a] First-Stage $P[S_1 < S_0]$	Compliance Probabilities: $P[S_1 < S_0 \mid S=2]$	$P[S_1 < S_0 \mid S=1]$	**Total Compliers**
Pill Access										
All Grandfathers	0.604	0.095	0.040***	0.254	0.018	0.103	0.016***	0.092	0.007	0.371
Maternal Grandfathers	0.738	0.106	0.033***	0.232	0.010	0.115	0.014***	0.086	0.004	0.332
Paternal Grandfathers	0.483	0.085	0.054***	0.307	0.031	0.091	0.021***	0.088	0.012	0.438
All Grandmothers	0.629	0.105	0.033***	0.199	0.014	0.107	0.014***	0.082	0.006	0.301
Maternal Grandmothers	0.750	0.118	0.026***	0.166	0.007	0.118	0.011***	0.072	0.003	0.248
Paternal Grandmothers	0.518	0.092	0.048***	0.255	0.026	0.097	0.019***	0.103	0.010	0.394
Abortion Access										
All Grandfathers	0.659	0.095	0.027**	0.188	0.010	0.103	0.011**	0.073	0.004	0.275
Maternal Grandfathers	0.702	0.106	0.022*	0.147	0.007	0.115	0.010*	0.058	0.003	0.215
Paternal Grandfathers	0.458	0.085	0.023**	0.124	0.014	0.091	0.009**	0.047	0.006	0.191
All Grandmothers	0.607	0.105	0.015	0.085	0.006	0.107	0.006	0.036	0.003	0.130
Maternal Grandmothers	0.724	0.118	0.004	0.025	0.001	0.118	0.017	0.002	0.001	0.041
Paternal Grandmothers	0.499	0.092	0.018	0.097	0.010	0.097	0.008	0.092	0.010	0.209
Abortion $\leq$ 250 Mi.										
All Grandfathers	0.011	0.095	0.006***	0.001	0.007	0.103	0.017***	0.002	0.019	0.029
Maternal Grandfathers	0.012	0.106	0.007***	0.007	0.007	0.115	0.015***	0.002	0.017	0.033
Paternal Grandfathers	0.010	0.085	0.006*	0.001	0.007	0.091	0.018*	0.002	0.020	0.030
All Grandmothers	0.011	0.105	0.001***	0.001	0.012	0.107	0.016***	0.002	0.018	0.033
Maternal Grandmothers	0.012	0.118	0.002***	0.000	0.002	0.118	0.012***	0.001	0.016	0.019
Paternal Grandmothers	0.010	0.092	0.013***	0.001	0.014	0.097	0.015***	0.002	0.021	0.038

Table 7 reports estimates of the complier population drawn from the first-stage estimates reported in Table 4, and unreported runs on the sex of the adult child. Complier probabilities are estimated for each *Child Count* level.

[a] Weighted first stage coefficient is the product of the results reported in Table 4 or unreported estimates on the first-stage runs done by the sex of the adult child multiplied by weights for the unit causal responses (Angrist and Imbens, 1995). These weights are used to calculate the weighted average of the LATEs, reflecting that each level of treatment intensity (i.e., number of children) has a different LATE. The total complier population is thus the sum of the compliers for each LATE.

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

TABLE 8
2nd-Stage IV Results: Maternal Versus Paternal Grandparents

	Grandfathers				**Grandmothers**			
Familial Relation ⇓	Retired	Disabled	Cond. Hrs Worked	In Labor Force	Retired	Disabled	Cond. Hrs Worked	Non-Zero Hours
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
				Grandparent Status Regressions[a]				
Maternal	0.232	0.251	-814.65	-0.400	-0.701	-0.289	2,300.30	-0.289
	(0.273)	(0.194)	(836.38)	(0.271)	(0.424)	(0.263)	(2,014.07)	(0.720)
N	34,811	30,638	21,500	33,914	55,199	49,162	34,941	55,199
Montiel-Pflueger F	4.64	2.81	2.40	4.54	5.52	3.16	1.45	5.52
5% Crit. Value	*25.37*	*26.49*	*29.87*	*24.98*	*23.79*	*24.09*	*23.56*	*23.42*
10% Crit. Value	*15.50*	*16.14*	*18.14*	*15.26*	*14.55*	*14.79*	*14.49*	*14.35*
Paternal	0.449*	0.079	-751.11	-0.801**	0.586***	-0.140	-17.62	-1.818***
	(0.254)	(0.228)	(700.06)	(0.345)	(0.161)	(0.184)	(846.51)	(0.697)
N	24,952	21,134	13,143	23,981	40,174	34,844	23,097	40,174
Montiel-Pflueger F	5.35	4.78	5.90	5.28	11.58	8.76	8.04	11.58
5% Crit. Value	*33.66*	*33.55*	*32.39*	*33.55*	*26.57*	*27.17*	*29.91*	*26.52*
10% Crit. Value	*20.69*	*20.59*	*19.85*	*20.62*	*16.23*	*16.56*	*18.27*	*16.20*
				Child Count Regressions[a]				
Maternal	0.213***	0.007	-183.25	-0.217***	0.297***	0.030	-1,058.83*	-0.800***
	(0.060)	(0.056)	(208.05)	(0.051)	(0.103)	(0.069)	(535.32)	(0.234)
N	96,366	83,329	58,000	94,218	154,854	138,225	98,160	154,854
Montiel-Pflueger F	45.42	52.82	27.28	46.71	18.16	28.77	6.82	18.16
5% Crit. Value	*25.90*	*20.88*	*24.76*	*25.94*	*25.19*	*24.67*	*27.82*	*25.14*
10% Crit. Value	*15.89*	*12.84*	*15.24*	*15.88*	*15.29*	*15.12*	*16.99*	*15.26*
Paternal	0.196**	0.028	-473.04**	-0.239***	0.214***	0.033	-184.89	-0.410***
	(0.076)	(0.062)	(226.62)	(0.071)	(0.067)	(0.067)	(173.24)	(0.087)
N	67,422	55,863	34,979	65,295	113,374	98,137	65,956	113,374
Montiel-Pflueger F	77.67	64.98	45.62	80.39	141.27	113.58	95.78	141.27
5% Crit. Value	*28.21*	*29.47*	*30.23*	*28.01*	*25.80*	*25.52*	*27.12*	*25.80*
10% Crit. Value	*17.33*	*18.15*	*18.71*	*17.23*	*15.81*	*15.59*	*16.71*	*15.81*

Table 8 shows the second-stage regression estimates of grandparents' labor force characteristics for maternal versus paternal grandparents. Second stage estimates are from Equation (4) and the grandparent flag from Equation (5). All regressions include individual-level and state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has where necessary. Standard errors are clustered at the state level.

[a] All regressions include the adult child and grandparent demographic variables listed in Section 3.1.

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

TABLE 9

First-Stage Estimates with 10 Lags on Each Policy

	$\mathbb{1}\{$**Grandparent**$\}$		**Child Count**	
	Grandfathers	Grandmothers	Grandfathers	Grandmothers
Access Policy $\Downarrow$	(1)	(2)	(3)	(4)
	(b/se)	(b/se)	(b/se)	(b/se)
Pill Access				
No Lag	-0.036	-0.025	-0.085**	-0.079**
	(0.023)	(0.020)	(0.038)	(0.037)
Lag (t-1)	-0.001	0	-0.075*	-0.048
	(0.019)	(0.016)	(0.043)	(0.042)
Lag (t-2)	0.022	0.023*	0.008	0.006
	(0.014)	(0.012)	(0.019)	(0.018)
Lag (t-3)	0.027*	0.036***	-0.007	0.019
	(0.015)	(0.010)	(0.015)	(0.016)
Lag (t-4)	0.026*	0.022*	0.041**	0.039**
	(0.013)	(0.012)	(0.019)	(0.017)
Lag (t-5)	0.019*	0.021*	0.036**	0.033***
	(0.010)	(0.011)	(0.014)	(0.011)
Lag (t-6)	0.046***	0.041***	0.042***	0.038***
	(0.010)	(0.011)	(0.012)	(0.014)
Lag (t-7)	0.043***	0.029***	0.069***	0.069***
	(0.010)	(0.010)	(0.016)	(0.013)
Lag (t-8)	0.007	0.014*	0.057***	0.063***
	(0.009)	(0.007)	(0.015)	(0.013)
Lag (t-9)	0.029***	0.013**	-0.004	-0.000

Table 9 shows the first-stage regression results estimating Equation (6) for the grandchild measure *Child Count* and Equation (7) for $\mathbb{1}\{Grandparent\}$. "Pill Access" and "Abortion Access" are the treatment variables for whether the adult daughter had access to the reproductive technology in year t. "Abortion $\leq$ 250 Mi." is a dummy for whether individual i's 1968 state was within 250 miles of a state with legalized abortion prior to *Roe*. All regressions include state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has. Robust standard errors clustered at the state level are reported. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

TABLE 9

First-Stage Estimates with 10 Lags on Each Policy

	$\mathbb{1}\{$**Grandparent**$\}$		**Child Count**	
	Grandfathers	Grandmothers	Grandfathers	Grandmothers
Access Policy ⇓	(1)	(2)	(3)	(4)
	(b/se)	(b/se)	(b/se)	(b/se)
	(0.010)	(0.006)	(0.018)	(0.12)
Lag (t-10)	0.042***	0.057***	0.195***	0.178***
	(0.014)	(0.011)	(0.031)	(0.026)
Abortion Access				
No Lag	-0.022	-0.007	-0.085***	-0.056*
	(0.021)	(0.017)	(0.026)	(0.029)
Lag (t-1)	-0.006	0.012	-0.003	0.009
	(0.010)	(0.007)	(0.014)	(0.014)
Lag (t-2)	0.003	0.005	-0.017	-0.002
	(0.009)	(0.012)	(0.013)	(0.012)
Lag (t-3)	-0.003	0.021**	-0.006	0.006
	(0.011)	(0.010)	(0.015)	(0.014)
Lag (t-4)	0.008	0.004	-0.003	-0.005
	(0.013)	(0.018)	(0.020)	(0.016)
Lag (t-5)	0.010	0.006	0.001	0.018
	(0.019)	(0.017)	(0.029)	(0.018)
Lag (t-6)	0.002	0.047***	0.010	0.039**
	(0.020)	(0.013)	(0.023)	(0.016)
Lag (t-7)	0.016	-0.008	-0.006	-0.015
	(0.020)	(0.017)	(0.026)	(0.020)
Lag (t-8)	0.022	0.045***	0.041*	0.060***

Table 9 shows the first-stage regression results estimating Equation (6) for the grandchild measure *Child Count* and Equation (7) for $\mathbb{1}\{Grandparent\}$. "Pill Access" and "Abortion Access" are the treatment variables for whether the adult daughter had access to the reproductive technology in year t. "Abortion $\leq$ 250 Mi." is a dummy for whether individual i's 1968 state was within 250 miles of a state with legalized abortion prior to *Roe*. All regressions include state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has. Robust standard errors clustered at the state level are reported. * p<0.10, ** p<0.05, *** p<0.01

TABLE 9

First-Stage Estimates with 10 Lags on Each Policy

	$\mathbb{1}\{$**Grandparent**$\}$		**Child Count**	
	Grandfathers	Grandmothers	Grandfathers	Grandmothers
Access Policy $\Downarrow$	(1)	(2)	(3)	(4)
	(b/se)	(b/se)	(b/se)	(b/se)
	(0.022)	(0.016)	(0.021)	(0.019)
Lag (t-9)	0.003	-0.014	-0.003	-0.016
	(0.022)	(0.015)	(0.028)	(0.018)
Lag (t-10)	0.093**	0.083***	0.227***	0.216***
	(0.038)	(0.030)	(0.058)	(0.041)
Abortion $\leq$ **250 Mi.**				
No Lag	-0.058***	-0.077***	-0.196**	-0.160***
	(0.019)	(0.016)	(0.078)	(0.042)
Lag (t-1)	-0.005	-0.004	-0.038*	-0.027
	(0.015)	(0.012)	(0.021)	(0.023)
Lag (t-2)	0.022*	0.013	-0.036	-0.025
	(0.012)	(0.014)	(0.025)	(0.019)
Lag (t-3)	-0.003	-0.024	-0.106	-0.115
	(0.024)	(0.020)	(0.070)	(0.047)
Lag (t-4)	0.004	-0.014	-0.011	-0.031
	(0.032)	(0.024)	(0.074)	(0.058)
Lag (t-5)	-0.007	0.007	-0.028	-0.007
	(0.016)	(0.016)	(0.036)	(0.030)
Lag (t-6)	0.011	-0.010	-0.133***	-0.108***
	(0.010)	(0.014)	(0.041)	(0.033)
Lag (t-7)	-0.004	-0.017	-0.028	-0.022

Table 9 shows the first-stage regression results estimating Equation (6) for the grandchild measure *Child Count* and Equation (7) for $\mathbb{1}\{Grandparent\}$. "Pill Access" and "Abortion Access" are the treatment variables for whether the adult daughter had access to the reproductive technology in year t. "Abortion $\leq$ 250 Mi." is a dummy for whether individual i's 1968 state was within 250 miles of a state with legalized abortion prior to *Roe*. All regressions include state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has. Robust standard errors clustered at the state level are reported. * p<0.10, ** p<0.05, *** p<0.01

TABLE 9

First-Stage Estimates with 10 Lags on Each Policy

	$\mathbb{1}$\{**Grandparent**\}		**Child Count**	
	Grandfathers	Grandmothers	Grandfathers	Grandmothers
Access Policy $\Downarrow$	(1)	(2)	(3)	(4)
	(b/se)	(b/se)	(b/se)	(b/se)
	(0.023)	(0.020)	(0.051)	(0.041)
Lag (t-8)	-0.021	-0.008	-0.018	-0.008
	(0.028)	(0.026)	(0.029)	(0.028)
Lag (t-9)	0.022*	0.005	-0.095***	-0.062**
	(0.012)	(0.010)	(0.024)	(0.027)
Lag (t-10)	0.024	0.007	0.001	0.009
	(0.030)	(0.024)	(0.038)	(0.039)
Adj R^2	0.79	0.79	0.71	0.79
N	59,814	95,427	163,804	208,240

Table 9 shows the first-stage regression results estimating Equation (6) for the grandchild measure *Child Count* and Equation (7) for $\mathbb{1}\{Grandparent\}$. "Pill Access" and "Abortion Access" are the treatment variables for whether the adult daughter had access to the reproductive technology in year t. "Abortion $\leq$ 250 Mi." is a dummy for whether individual i's 1968 state was within 250 miles of a state with legalized abortion prior to *Roe*. All regressions include state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has. Robust standard errors clustered at the state level are reported. * p<0.10, ** p<0.05, *** p<0.01

TABLE 10
2nd-Stage IV Results from First Stage with 10 Lags on Each Policy

	Grandfathers				Grandmothers			
Grandchild Measure ⇓	Retired	Disabled	Cond. Hrs Worked	In Labor Force	Retired	Disabled	Cond. Hrs Worked	Non-Zero Hours
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Grandparent Status Regressions[a]								
$\mathbb{1}\{$**Grandparent**$\}$	0.020	0.064	-100.12	0.008	0.046	0.126***	-457.98***	-0.080
	(0.058)	(0.063)	(199.71)	(0.062)	(0.048)	(0.040)	(162.14)	(0.096)
N	59,814	51,829	34,712	57,947	95,427	84,072	58,095	95,427
Montiel-Pflueger F	20.97	16.21	8.71	20.60	30.07	29.60	15.07	30.07
5% Crit. Value	*31.76*	*31.24*	*31.17*	*31.70*	*30.48*	*30.06*	*31.80*	*30.48*
10% Crit. Value	*18.84*	*18.45*	*18.40*	*18.80*	*17.89*	*17.58*	*18.87*	*17.89*
Child Count Regressions[a]								
Child Count	0.106***	0.008	-58.88	-0.051**	0.116***	0.046**	-298.92***	-0.138***
	(0.019)	(0.024)	(40.64)	(0.024)	(0.023)	(0.022)	(74.17)	(0.042)
N	163,804	139,206	92,989	159,531	268,240	236,374	164,131	268,240
Montiel-Pflueger F	56.09	46.99	30.48	54.83	58.09	55.00	25.62	58.09
5% Crit. Value	*30.86*	*31.18*	*30.87*	*30.92*	*32.25*	*31.93*	*32.69*	*32.25*
10% Crit. Value	*18.18*	*18.41*	*18.18*	*18.23*	*19.20*	*18.97*	*19.53*	*19.20*

Table 10 shows the second-stage regression estimates of grandchildren's impact on grandparents' labor force characteristics after adding 10 lags on each policy access instrument. All regressions include individual-level and state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has where necessary. Standard errors are clustered at the state level.

[a] All regressions include the adult child and grandparent demographic variables listed in Section 3.1.

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

TABLE 11
2nd-Stage IV Results from First Stage with 10 Lags on Each Policy
Maternal Versus Paternal Grandparents

Familial Relation ⇓	**Grandfathers** Retired	Disabled	Cond. Hrs Worked	In Labor Force	**Grandmothers** Retired	Disabled	Cond. Hrs Worked	Non-Zero Hours
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Grandparent Status Regressions[a]								
Maternal	0.018	0.041	-163.96	-0.011	-0.035	0.200***	-401.46*	-0.079
	(0.071)	(0.079)	(230.36)	(0.080)	(0.060)	(0.042)	(221.92)	(0.090)
N	34,811	30,638	21,500	33,914	55,199	49,162	34,941	55,199
Montiel-Pflueger F	12.67	8.78	4.94	12.34	22.60	20.93	10.51	22.60
5% Crit. Value	*32.23*	*32.03*	*32.09*	*32.23*	*31.06*	*30.80*	*31.25*	*31.06*
10% Crit. Value	*19.19*	*19.04*	*19.08*	*19.19*	*18.32*	*18.13*	*18.46*	*18.32*
Paternal	0.063	0.092	67.74	-0.031	0.190**	0.003	-526.49**	-0.18
	(0.078)	(0.099)	(260.67)	(0.069)	(0.073)	(0.072)	(245.00)	(0.113)
N	24,952	21,134	13,143	23,981	40,174	34,844	23,097	40,174
Montiel-Pflueger F	17.18	13.08	7.62	16.74	21.35	19.66	9.87	21.35
5% Crit. Value	*31.08*	*30.53*	*30.77*	*31.13*	*29.58*	*29.37*	*31.95*	*29.58*
10% Crit. Value	*18.32*	*17.92*	*18.10*	*18.37*	*17.23*	*17.07*	*18.97*	*17.23*
Child Count Regressions[a]								
Maternal	0.103***	-0.006	-51.00	-0.057**	0.080***	0.058**	-339.28***	-0.138***
	(0.025)	(0.027)	(52.46)	(0.026)	(0.028)	(0.022)	(95.43)	(0.043)
N	96,366	83,329	58,000	94,218	154,854	138,225	98,160	154,854
Montiel-Pflueger F	36.63	33.47	21.05	34.87	36.69	35.59	22.24	36.69
5% Crit. Value	*30.22*	*30.66*	*30.59*	*30.30*	*31.82*	*31.70*	*31.96*	*31.82*
10% Crit. Value	*17.71*	*18.03*	*17.98*	*17.76*	*18.89*	*18.80*	*18.99*	*18.89*
Paternal	0.116***	0.032	-62.43	-0.064	0.147***	0.031	-193.32**	-0.162***
	(0.032)	(0.042)	(101.45)	(0.039)	(0.032)	(0.031)	(77.56)	(0.045)
N	67,422	55,863	34,979	65,295	113,374	98,137	65,956	113,374
Montiel-Pflueger F	45.26	26.84	16.43	45.64	85.73	64.02	41.00	85.73
5% Crit. Value	*31.68*	*32.87*	*32.15*	*31.82*	*30.39*	*30.54*	*31.07*	*30.39*
10% Crit. Value	*18.78*	*19.67*	*19.13*	*18.88*	*17.83*	*17.93*	*18.33*	*17.83*

Table 11 shows the second-stage regression estimates of grandparents' labor force characteristics for maternal versus paternal grandparents after adding 10 lags on each policy access instrument. All regressions include individual-level and state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has where necessary. Standard errors are clustered at the state level.

[a] All regressions include the adult child and grandparent demographic variables listed in Section 3.1.

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

TABLE 12
2nd-Stage IV Results For Grandparents with 2 Adult Children

Grandchild Measure ⇓	**Grandfathers**				**Grandmothers**			
	Retired	Disabled	Cond. Hrs Worked	In Labor Force	Retired	Disabled	Cond. Hrs Worked	Non-Zero Hours
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
				Grandparent Status Regressions[a]				
With Base Instruments	0.371	0.018	-434.25	-0.488	-0.399	-0.308	2,485.86	-0.491
	(0.314)	(0.691)	(1,266.61)	(0.475)	(0.503)	(0.242)	(1,780.25)	(0.446)
N	14,340	12,557	7,928	13,879	22,496	20,161	12,738	22,496
Montiel-Pflueger F	1.25	0.54	0.93	1.13	1.60	1.71	1.02	1.60
5% Crit. Value	*30.60*	*30.91*	*31.97*	*30.62*	*30.93*	*30.88*	*30.58*	*30.94*
10% Crit. Value	*18.16*	*18.35*	*19.09*	*18.21*	*18.53*	*18.50*	*18.23*	*18.54*
With 10 Lags[b]	-0.022	0.081	53.70	-0.083	0.169	0.177*	-133.09	-0.023
	(0.121)	(0.104)	(195.79)	(0.098)	(0.109)	(0.101)	(293.46)	(0.149)
N	14,340	12,557	7,928	13,879	22,496	20,161	12,738	22,496
				Grandchild Count Regressions[a]				
With Base Instruments	0.184***	0.116**	-285.70	-0.067	0.178***	0.002	-595.70	-0.067
	(0.046)	(0.046)	(216.04)	(0.048)	(0.062)	(0.056)	(456.30)	(0.083)
N	14,340	12,557	7,928	13,879	22,496	20,161	12,738	22,496
Montiel-Pflueger F	35.56	22.60	10.98	36.48	21.90	14.19	5.72	21.90
5% Crit. Value	*26.04*	*27.03*	*29.33*	*26.31*	*30.39*	*30.69*	*31.81*	*30.39*
10% Crit. Value	*15.41*	*15.88*	*17.29*	*15.47*	*17.97*	*18.20*	*19.08*	*17.97*
With 10 Lags[b]	0.099***	0.032	24.23	-0.061***	0.116***	0.051*	-229.44**	-0.056
	(0.028)	(0.025)	(91.78)	(0.021)	(0.026)	(0.028)	(88.56)	(0.041)
N	14,340	12,557	7,928	13,879	22,496	20,161	12,738	22,496

Table 12 shows the second-stage regression estimates of grandparents' labor force characteristics. The first stage has instruments for each adult child so that the variables of interest reflect the fertility behavior of all the adult children. All regressions include individual-level and state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has where necessary. Standard errors are clustered at the state level.

[a] All regressions include the adult child and grandparent demographic variables listed in Section 3.1.

[b] The Montiel-Pflueger F statistic cannot be calculated for cases where the number of excluded instruments (66) exceeds the number of clusters (39).

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

TABLE 13
2nd-Stage IV Results with Adult Child Age Range Fixed Effects
First Stage with and without 10 Lags on Each Policy

	Grandfathers				Grandmothers			
First-Stage Regression ⇓	Retired	Disabled	Cond. Hrs Worked	In Labor Force	Retired	Disabled	Cond. Hrs Worked	Non-Zero Hours
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
				Grandparent Status Regressions[a]				
Base Instruments	0.738***	0.086	-652.18	-0.174	0.187	-0.039	-1,040.48	-1.287*
	(0.215)	(0.180)	(731.84)	(0.159)	(0.403)	(0.273)	(732.18)	(0.717)
N	59,290	51,294	34,336	57,491	94,960	83,586	57,767	94,960
Montiel-Pflueger F	4.30	4.16	4.23	4.75	1.80	2.17	3.71	1.80
5% Crit. Value	*27.52*	*28.58*	*30.31*	*27.18*	*21.46*	*22.33*	*23.47*	*21.67*
10% Crit. Value	*16.76*	*17.33*	*18.35*	*16.55*	*13.24*	*13.76*	*14.35*	*13.36*
With 10 Lags[a]	-0.049	0.067	-245.01	0.048	0.008	0.145**	-327.97*	-0.042
	(0.082)	(0.069)	(190.33)	(0.059)	(0.056)	(0.056)	(174.02)	(0.066)
N	59,290	51,294	34,336	57,491	94,960	83,586	57,767	94,960
Montiel-Pflueger F	13.88	13.26	7.82	13.90	17.94	21.38	9.85	17.94
5% Crit. Value	*31.79*	*31.29*	*31.15*	*31.73*	*30.50*	*30.12*	*31.83*	*30.50*
10% Crit. Value	*18.86*	*18.49*	*18.39*	*18.82*	*17.91*	*17.62*	*18.88*	*17.91*
				Child Count Regressions[a]				
Base Instruments	0.189***	0.023	-309.49*	-0.100***	0.235***	-0.022	-132.98	-0.233***
	(0.046)	(0.033)	(173.86)	(0.031)	(0.065)	(0.066)	(174.56)	(0.096)
N	162,274	137,692	91,956	158,103	266,482	234,613	162,851	266,482
Montiel-Pflueger F	49.54	46.42	36.36	49.95	34.90	35.81	34.07	34.90
5% Crit. Value	*25.88*	*24.67*	*26.90*	*26.06*	*24.80*	*22.80*	*26.44*	*24.67*
10% Crit. Value	*15.78*	*15.19*	*16.54*	*15.87*	*15.12*	*13.95*	*16.17*	*15.05*
With 10 Lags[a]	0.057*	-0.002	-130.80***	-0.009	0.085***	0.036	-119.59*	-0.084***
	(0.032)	(0.022)	(41.06)	(0.026)	(0.029)	(0.024)	(67.97)	(0.029)
N	162,274	137,692	91,956	158,103	266,482	234,613	162,851	266,482
Montiel-Pflueger F	29.81	31.94	27.30	29.11	27.51	35.48	30.44	27.51
5% Crit. Value	*30.88*	*31.16*	*30.90*	*30.95*	*32.31*	*32.00*	*32.45*	*32.31*
10% Crit. Value	*18.19*	*18.39*	*18.20*	*18.24*	*19.25*	*19.10*	*19.35*	*19.25*

Table 13 shows the second-stage regression estimates of grandparents' labor force characteristics after adding fixed effects for 10 year age ranges for the adult children. All regressions include individual-level and state-by-year fixed effects and are weighted with the core family sampling weights provided by the PSID, adjusted for the number of adult children each grandparent has where necessary. Standard errors are clustered at the state level.

[a] All regressions include the adult child and grandparent demographic variables listed in Section 3.1.

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

TABLE 14

Panel Regression of Older Men's National Labor Force Participation Rates

	Birth Cohort Specification [a]							
	Time Trends	1 Year FE	2 Year FE	4 Year FE	Time Trends	1 Year FE	2 Year FE	4 Year FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
	Fraction Grandparent Regressions				**Average Grandchild Count Regressions**			
<u>Without interactions</u>								
GP_Measure	-0.627***	-29.882***	-0.190	-0.588***	-7.756***	-369.503***	-2.345	-7.269***
	(0.064)	(9.710)	(0.410)	(0.200)	(0.793)	(120.079)	(5.073)	(2.468)
<u>With interactions</u>								
GP_Measure	-0.946***	-34.156***	-0.657	-0.780***	-12.708***	-417.019***	-6.833	-9.530***
	(0.125)	(9.944)	(0.420)	(0.217)	(1.465)	(121.249)	(5.161)	(2.633)
×SSB65	-0.036***	-0.013	-0.023**	-0.038***	-0.339***	-0.073	-0.192**	-0.360***
	(0.006)	(0.011)	(0.009)	(0.008)	(0.071)	(0.122)	(0.096)	(0.090)
×(SSB62-SSB65)	-0.003	0.027	0.018	-0.015	0.032	0.487**	0.341**	-0.052
	(0.008)	(0.018)	(0.014)	(0.013)	(0.094)	(0.206)	(0.160)	(0.145)
×(SSB70-SSB65)	0.012**	0.021**	0.026***	0.028***	0.062	0.168*	0.248***	0.269***
	(0.006)	(0.008)	(0.007)	(0.007)	(0.070)	(0.094)	(0.083)	(0.076)
×Avg. Earnings	-0.003***	0.000	-0.000	-0.001	-0.038***	0.008	0.001	-0.007
	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)	(0.009)	(0.009)	(0.008)
×Disability Benefit	0.024***	0.029***	0.029***	0.029***	0.240***	0.279***	0.281***	0.285***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.028)	(0.030)	(0.029)	(0.029)
×Log Predicted Wage	0.456***	0.228***	0.239***	0.274***	5.095***	2.471***	2.614***	3.023***
	(0.029)	(0.032)	(0.032)	(0.032)	(0.359)	(0.388)	(0.394)	(0.391)
Adj. R^2	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98
F	187.017	163.331	161.246	163.484	182.300	160.168	157.835	160.005
N	4121	4120	4121	4121	4121	4120	4121	4121

Table 14 shows the panel fixed effects regression estimates of labor force characteristics, including grandparenthood status, on national labor force participation rates. The coefficients for % *Grandparent* measure the effect of an additional 1 point in the fraction of individuals in a given age-sex-year-birth year-educational attainment group cell who are grandparents. The coefficients for *Grandchild Count* measure the marginal effect of an additional grandchild on national LFP. These are second stage estimates after instrumenting for $Spouse_I nLF$ in Equation (C.1) as described in Section 4.1. All regressions are weighted by the number of individuals in each cell, and include age and education group-by-year fixed effects. All rate and fractional variables are multiplied by 100, so that that coefficients can be interpreted as how many points the LFP rate changes per one unit change in the variable. The regressions are estimated with heteroskedastic-robust standard errors.

[a] See Section 4.1 for list of variables included.

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

TABLE 15
Extended Estimates from National LFP Panel Regression

	% Grandfathers				Grandchild Count			
	No Elig. Interactions		Elig. Interactions		No Elig. Interactions		Elig. Interactions	
	Time Trends	4 Year FE	Time Trends	4 Year FE	Time Trends	4 Year FE	Time Trends	4 Year FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
SSB65	-0.689***	-0.383***	2.072***	2.421***	-0.689***	-0.383***	1.274***	1.577***
	(0.112)	(0.131)	(0.370)	(0.468)	(0.112)	(0.131)	(0.291)	(0.348)
(SSB62-SSB65)	-0.597***	-0.353*	0.078	0.754	-0.597***	-0.353*	-0.321	0.049
	(0.147)	(0.212)	(0.579)	(0.826)	(0.147)	(0.212)	(0.482)	(0.638)
(SSB70-SSB65)	0.794***	0.332***	-0.490	-1.661***	0.794***	0.332***	0.033	-1.023***
	(0.079)	(0.124)	(0.396)	(0.417)	(0.079)	(0.124)	(0.316)	(0.332)
Lifetime Avg. Monthly Earnings	0.016	-0.025**	0.311***	0.095**	0.016	-0.025**	0.233***	0.065*
	(0.011)	(0.011)	(0.038)	(0.044)	(0.011)	(0.011)	(0.030)	(0.036)
Monthly Disability Benefit	-0.115***	-0.198***	-1.657***	-2.071***	-0.115***	-0.198***	-1.173***	-1.435***
	(0.043)	(0.047)	(0.165)	(0.171)	(0.043)	(0.047)	(0.128)	(0.131)
Log Predicted Wage	2.427***	2.339***	-32.016***	-18.720***	2.427***	2.339***	-24.676***	-14.076***
	(0.808)	(0.796)	(2.115)	(2.310)	(0.808)	(0.796)	(1.835)	(1.988)
% Married	0.153***	0.111***	0.162***	0.157***	0.153***	0.111***	0.166***	0.157***
	(0.029)	(0.029)	(0.028)	(0.027)	(0.029)	(0.029)	(0.028)	(0.028)
% Previously Married	0.168***	0.072**	0.166***	0.124***	0.168***	0.072**	0.175***	0.128***
	(0.034)	(0.033)	(0.032)	(0.032)	(0.034)	(0.033)	(0.033)	(0.032)
% Veteran	-0.071***	-0.055***	-0.058***	-0.067***	-0.071***	-0.055***	-0.061***	-0.067***
	(0.007)	(0.009)	(0.008)	(0.009)	(0.007)	(0.009)	(0.008)	(0.009)
% White	-0.014	0.010	-0.011	0.011	-0.014	0.010	-0.025*	0.007
	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
% Black	0.024	0.043*	-0.005	0.027	0.024	0.043*	-0.024	0.020
	(0.025)	(0.025)	(0.024)	(0.024)	(0.025)	(0.025)	(0.025)	(0.025)
% In Bad Health	-0.730***	-0.793***	-0.707***	-0.754***	-0.730***	-0.793***	-0.712***	-0.768***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Birth Year^2	0.001*	0.002	-0.002	0.006**	0.000	0.000	-0.003	0.004*
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
% Spouse in LF	0.119***	0.092***	0.100***	0.092***	0.119***	0.092***	0.103***	0.096***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.009)

Table 15 shows the remaining coefficient estimates for the panel fixed effects regression estimates of labor force characteristics, including grandparenthood status, first shown in Table 14 (Equation (8)). All rate and fractional variables are multiplied by 100, so that that coefficients can be interpreted as how many points the LFP rate changes per one unit change in the variable. The regressions are estimated with heteroskedastic-robust standard errors. * p<0.10, ** p<0.05, *** p<0.01.

TABLE 16
Marginal Effects for Interacted Variables

	% Grandparent		Grandchild Count	
	(1)	(2)	(3)	(4)
	(b/se)	(b/se)	(b/se)	(b/se)
GP_Measure	-0.187***	-0.129***	-4.087***	-2.525***
	(0.087)	(0.199)	(1.024)	(2.455)
SSB65	-0.140***	0.114***	-0.136***	0.080***
	(0.126)	(0.138)	(0.128)	(0.139)
(SSB62-SSB65)	-0.128	-0.134	-0.187	-0.169
	(0.177)	(0.197)	(0.175)	(0.200)
(SSB70-SSB65)	0.251	0.067***	0.291	0.096***
	(0.097)	(0.119)	(0.098)	(0.121)
Lifetime Avg. Monthly Earnings	0.100***	0.051**	0.074***	0.035*
	(0.012)	(0.012)	(0.011)	(0.012)
Monthly Disability Benefit	-0.213***	-0.292***	-0.176***	-0.250***
	(0.039)	(0.039)	(0.039)	(0.040)
Log Predicted Wage	-4.277***	-2.081***	-3.494***	-1.509***
	(0.811)	(0.788)	(0.807)	(0.786)
Birth Cohort Time Trends	Y	Y	Y	Y
4-Year Birth Cohort FE's	N	Y	N	Y

Table 16 shows the net effect of increasing either $GP_Measure$ by one unit, or increasing each of the three Social Security benefit measures by $100, derived from the results in Table 14.
* p<0.10, ** p<0.05, *** p<0.01

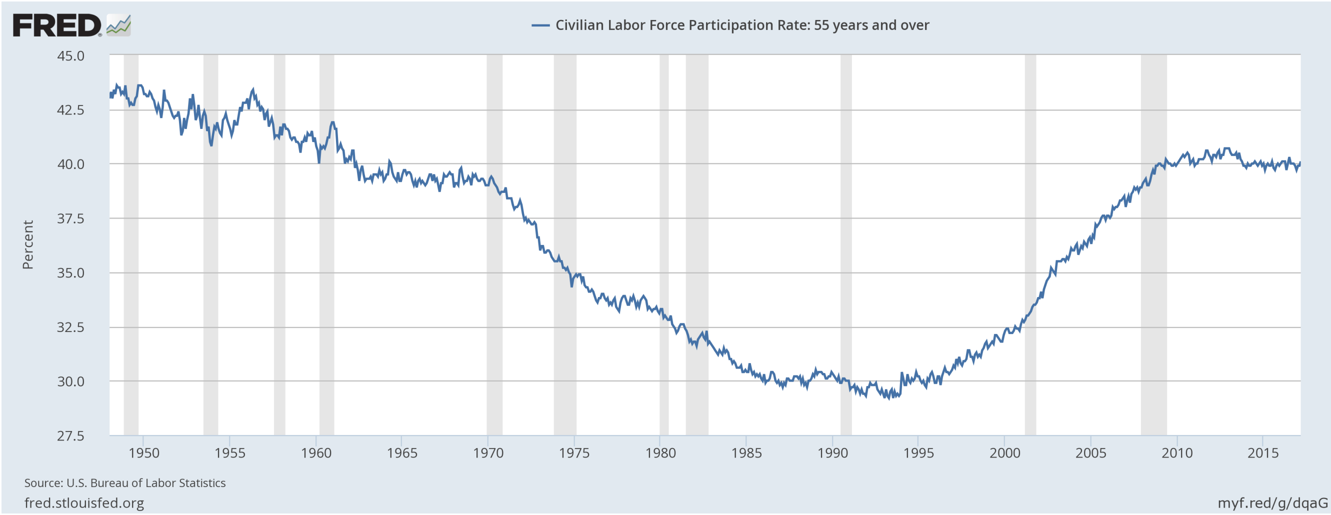


Figure 1 shows the civilian labor force participation rate for workers 55 and over from 1948 to April 2017.

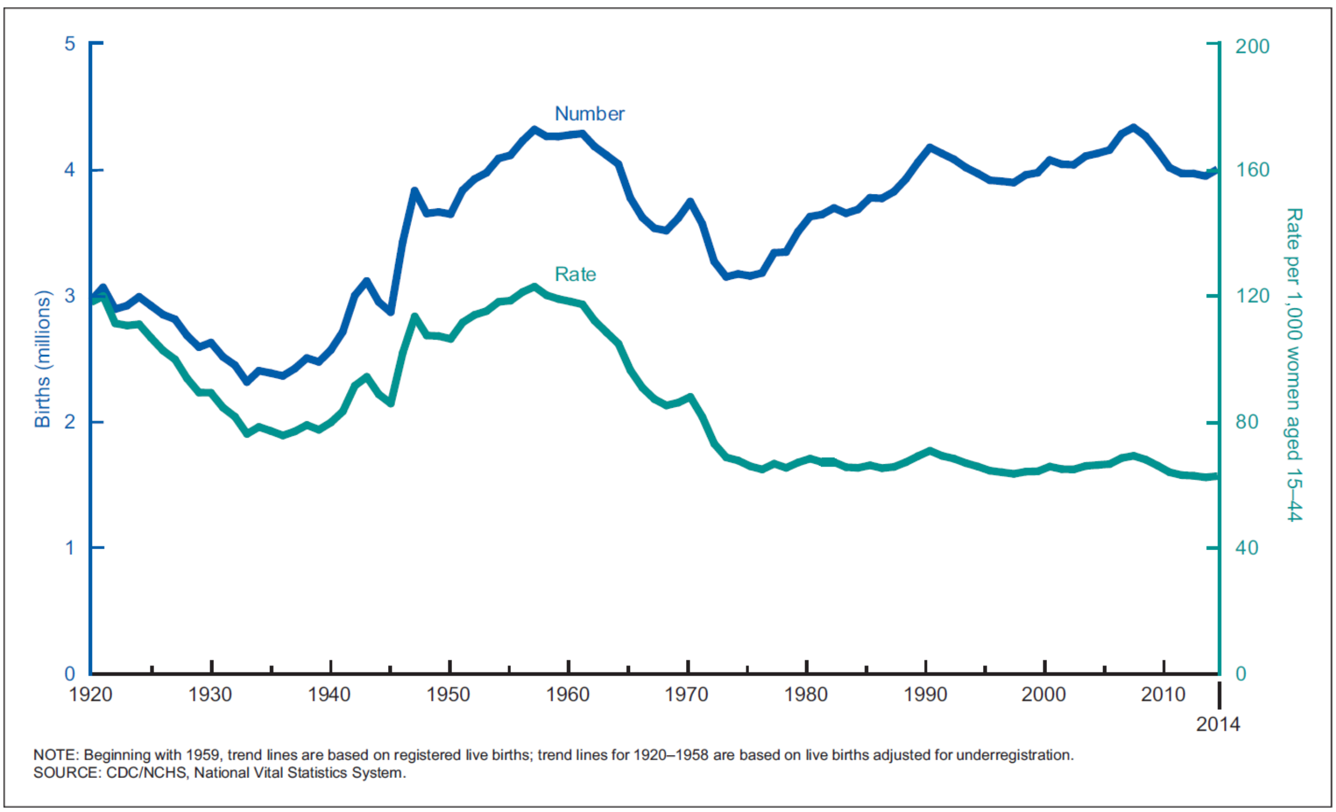


Figure 2 shows the birth rate and birth count by year for the United States from 1920 to 2014. Source: National Center for Health Statistics.

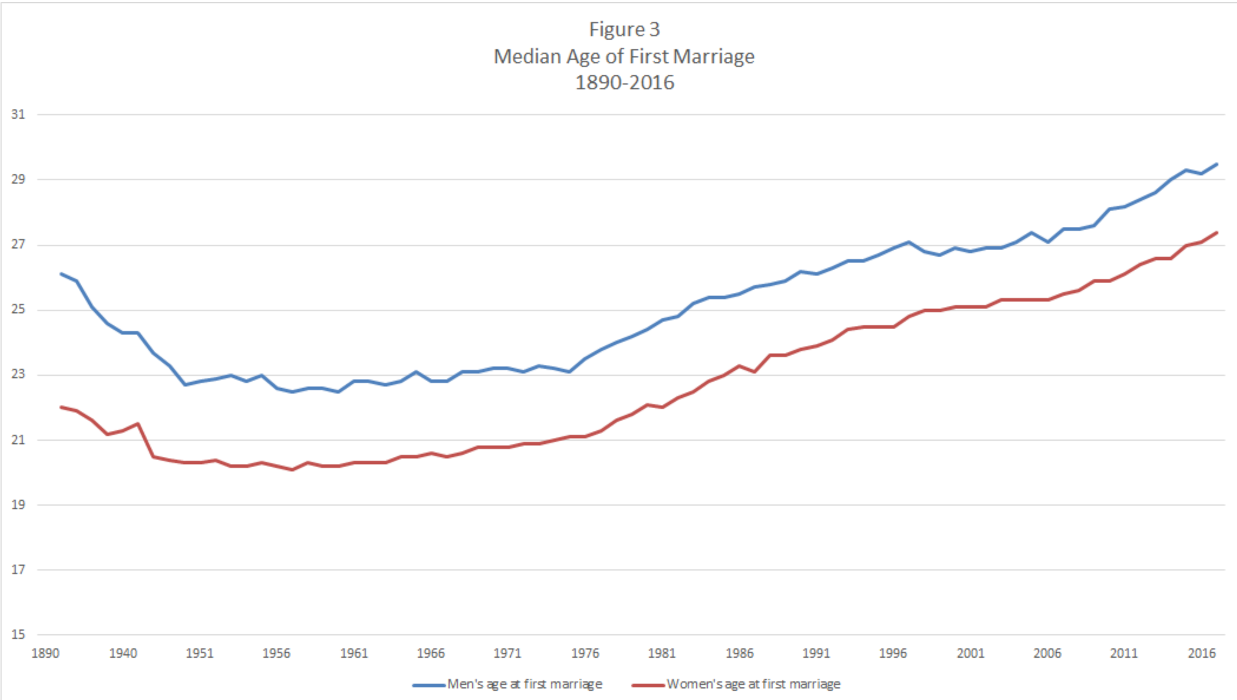
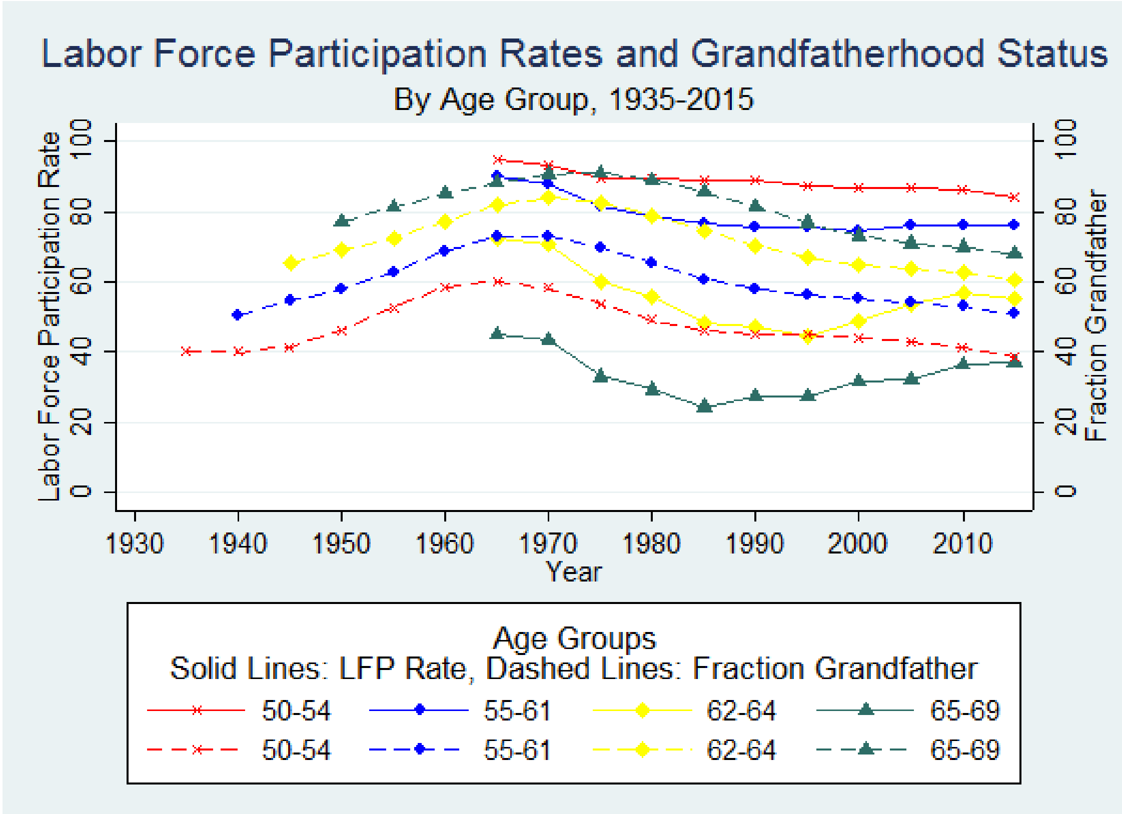
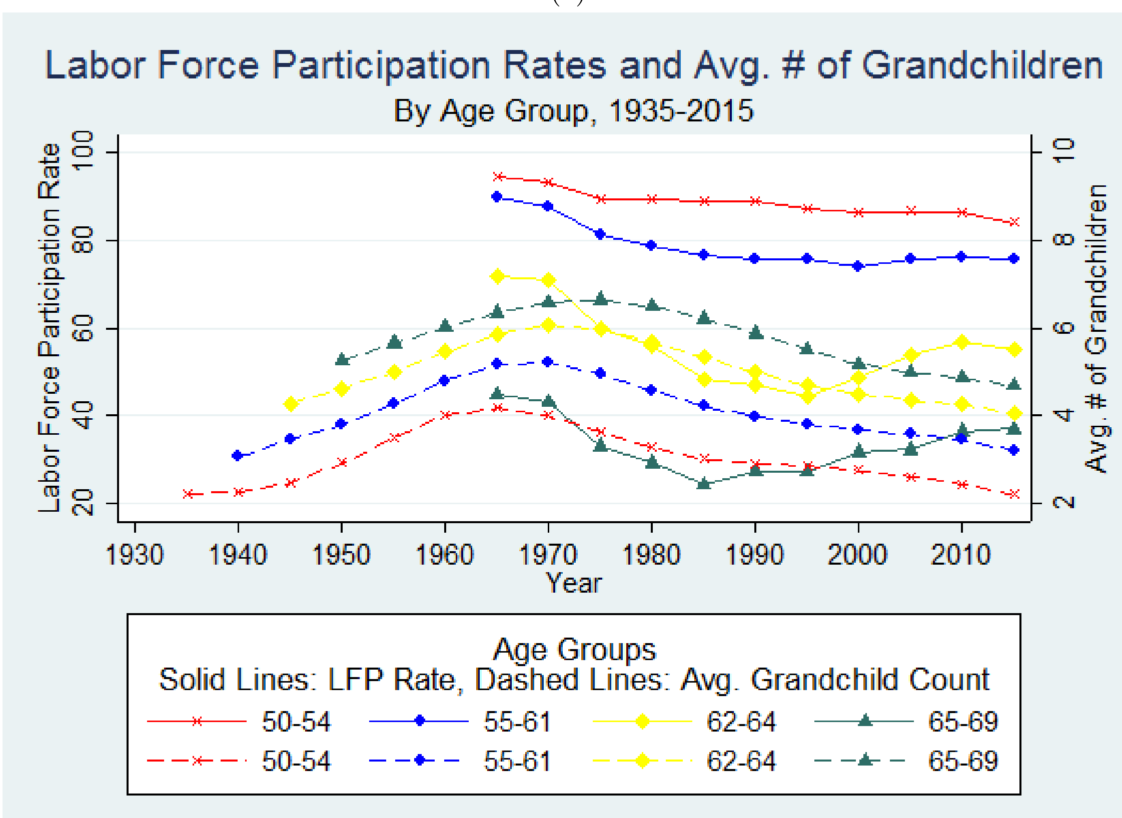


Figure 3 shows the median age of first marriage for men and for women from 1890 to 2016. Sources: U.S. Census Bureau, Current Population Survey, March and Annual Social and Economic Supplements.

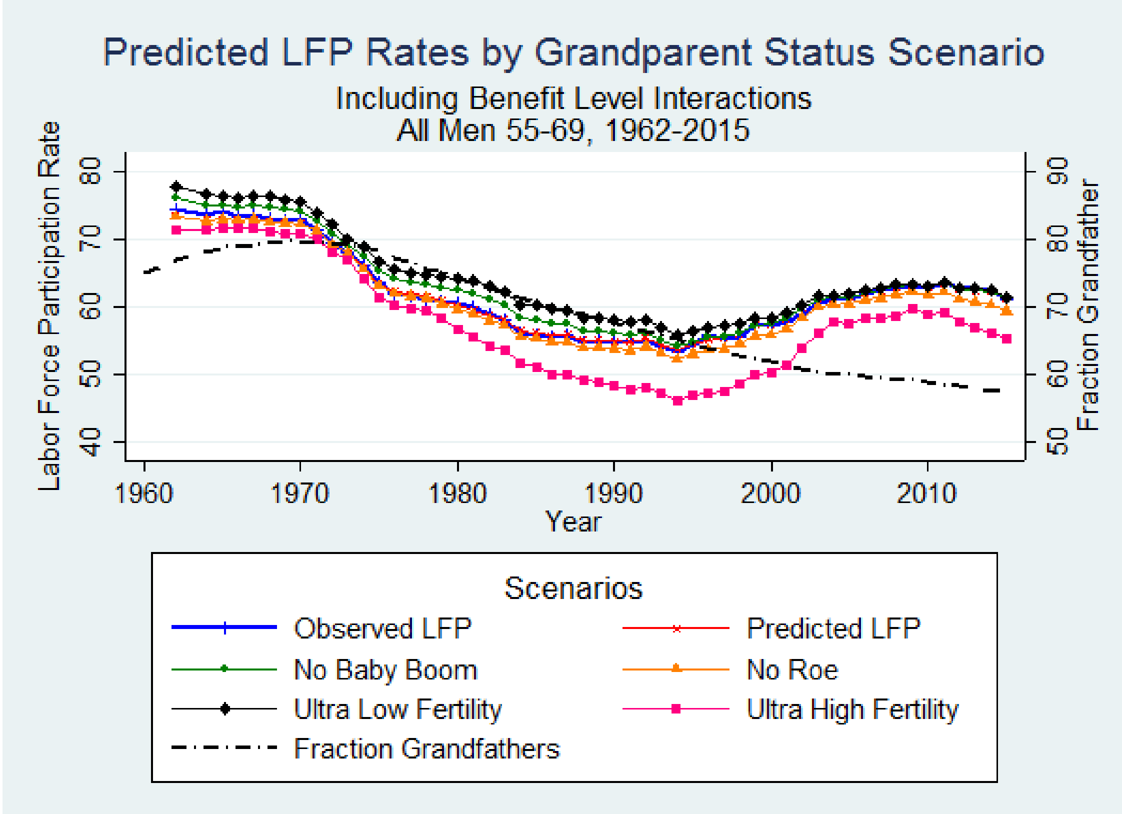


(a)

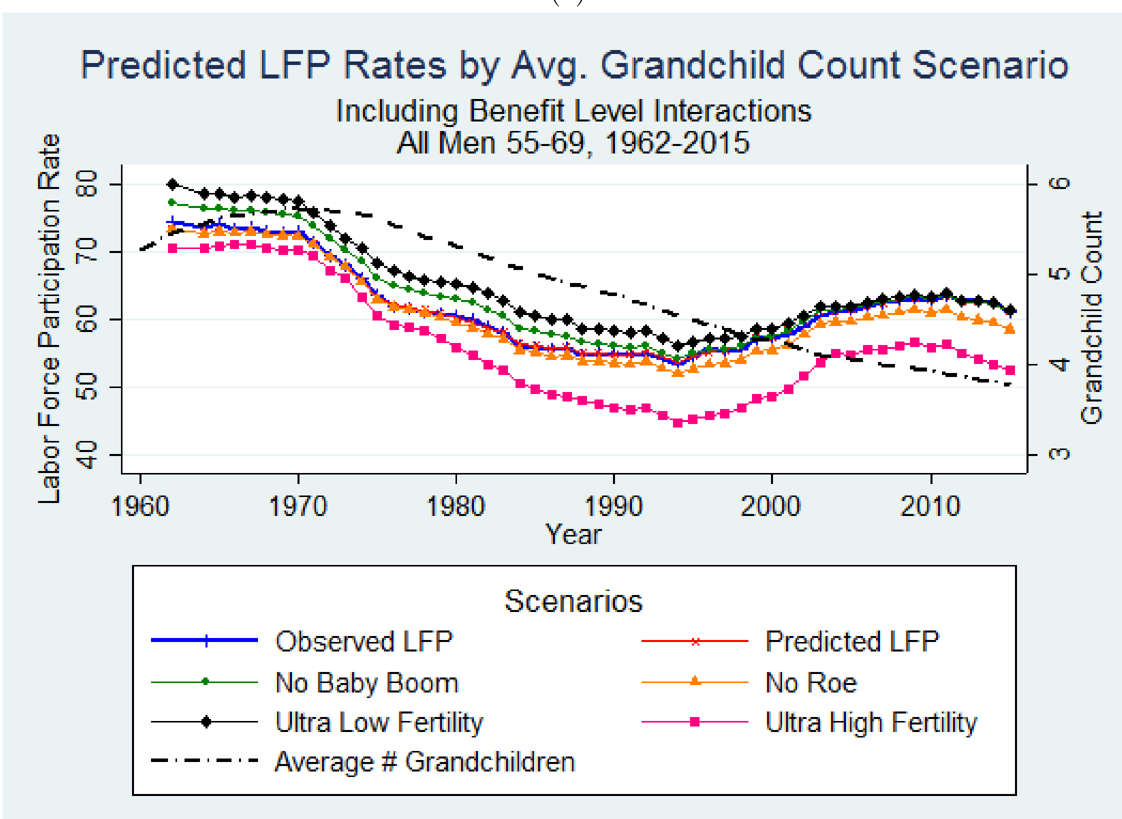


(b)

Figure 4: Figure 4a shows the fraction of men who are grandparents and their labor force participation (LFP) rate by age groups. LFP rates are in solid lines, and average grandchildren counts are in dashed lines. Figure 4b shows men's average number of grandchildren and their labor force participation (LFP) rate by age groups. LFP rates are in solid lines, and average grandchildren counts are in dashed lines.



(a)



(b)

Figure 5: Figure 5a shows the fraction of men who are grandparents according to different assumptions about fertility and the labor force participation (LFP) rate for ages 55-69 under these scenarios. Figure 5b shows men's average number of grandchildren according to different assumptions about fertility and the labor force participation (LFP) rate for ages 55-69 under these scenarios.

Appendix

A Legal History and Policy Coding Detail

I am indebted to the detailed research conducted by Middlebury's Caitlin Knowles Myers, without which, this paper would not be possible. For the instrument, access is coded as the fraction of the year that a conception could be blocked or aborted. Abortion was legalized by *Roe* and most other state statutes through the first trimester, so eligibility is backdated 93 days (or the equivalent for other eligibility periods) prior to the legalization date. For the pill, the policy is coded as is.

In the PSID, the first wife's age, year of birth, and other characteristics were used to code access for adult sons. Access is coded based on the state where the daughter or daughter-in-law was living in 1968, to avoid introducing potential endogeneity from women moving to states where contraception or abortion was legalized. Unless directly observed, the daughter-in-law's 1968 state is assumed to be the same as her husband's. Following the exhaustive reviews of the state laws on abortion and contraception given in Myers (2016, 2017) and Bailey et al. (2011) each daughter or daughter-in-law is coded as having access to abortion or access to contraception if there were no barriers to her access, such as spousal or parental consent requirements.[28]

The variation in state laws I use for the instrument is given in four tables. Tables A1-A3 shows the state-by-year policy variation. Table A1 shows the month and year of access to oral contraceptives for unmarried women between ages 18-20 and under 18. Table A2 shows the month and year of access to abortion on-demand for women 21 and over and between

[28]There are some cases where a fair reading of the law prohibited or potentially allowed access, but either the provision was not enforced or contemporary sources indicated that physicians did not perform reproductive services until the laws were clarified. I am indebted to Myers (2016, 2017) for identifying which provisions were likely enforced, and which were not, beyond a fair reading of the plain text. In those cases where Myers (2016, 2017) indicated that a law was not enforced or not followed, I have deferred to her work. Additional ambiguities were resolved with supplemental information from Joyce, Tan, and Zhang (2013), Levine (2003), Sabia and Anderson (2016), Bailey (2006), Levine et al. (1999), and Bitler and Zavodny (2001).

18-20. Table A3 shows the month and year of access to abortion by minors, and shows that there is substantial variation between and within states when minors did and did not have free access to abortion. Where exemptions existed for married individuals or minors who graduated from high school, I used PSID data to code these minors as having access for the individual-level estimates.

Table A4 shows the frequency distribution of birth years for the daughters and daughters-in-law. For adult sons who never married, the birth year of the mother of their oldest child is used. For adult sons who never married and never had children, their birth year minus 2 is used, reflecting the fact that on average, men marry women 2 years younger than themselves. Including them this way reflects their potential to provide grandchildren, even if it is never realized.

A.1 Access to Oral Contraceptives

The legislative history of access to oral contraceptives begins in 1960 when Enovid was approved by the Food and Drug Administration for the prevention of pregnancy (Junod and Marks (2002)). At this time, legal minors were largely defined as being under 21 and could not freely obtain hormonal birth control.[29] In fact, many states had complete or partial bans on contraceptive sales through a series of laws known as "Comstock Laws".[30] In 1965, the Supreme Court ruled in *Griswold v Connecticut* that Connecticut's Comstock law banning the sale of contraceptives to married couples was unconstitutional, holding that the Constitution ensured a right to privacy.[31] In practice, in every state except for Massachusetts, this meant that all women of the age of the legal majority could freely buy oral contraceptives. The right to privacy for unmarried woman of legal majority was formally established by the 1972 Supreme Court ruling *Eisenstadt v Baird*.[32] Here, the Supreme Court struck down a Massachusetts law that prohibited the sale of contraceptives to unmarried individuals. In

[29] Only in Arkansas and Alaska was 18 the age of full legal majority for women in 1960.
[30] For a history and discussion of the influence of the state Comstock Laws, see Bailey (2010)
[31] 405 U.S. 438 (1965)
[32] 405 U.S. 438 (1972)

between these rulings, states and courts directly or indirectly reshaped the laws governing access to contraception. Many studies have exploited this variation, including Goldin and Katz (2002), Bailey (2006), Bailey, Guldi, Davido, and Buzuvis (2011), Guldi (2008), Myers (2012), among others, to find that access to the pill allowed women to increase their labor supply, chiefly by delaying births.

For women between 18-20, states lowered the age of legal majority to 18 or 19 in waves, culminating in unimpeded access to oral contraceptives for all 18-20 year olds by 1976. Bailey (2006) establishes that the laws that permitted young women to purchase oral contraceptives were passed for reasons mostly orthogonal to expanding access to reproductive technology. Commonly, states in this period lowered their age of legal majority from 21 to 18, which incidentally allowed women of those ages to buy birth control. These legislative actions were taking place in the context of the debate over the draft, voting rights of soldiers, and the Vietnam War, and so had little connection to greater demands for reproductive freedom of choice. Other states had mature minor statutes, which hold that minors can consent to medical procedures and services if the minor clearly demonstrates they understand the implications. Often, these predated the introduction of the pill or court rulings that established the doctrine, usually for reasons have nothing to do with access to oral contraceptives. For example, the Ohio Supreme Court established a mature minor doctrine in 1956 (four years before the introduction of the pill) following *Lacey v Laird*, which was litigated over a nose surgery performed on a minor.[33] While the age of majority laws would open up access for women aged 18-20, the mature minor doctrines would often allow all minors to obtain contraceptives. The same rulings that would grant access to 18-20 year olds were often extended to all minors, creating further exploitable variation in access. There is thus substantial state-by-year variation in who could freely buy birth control pills that forms the basis for the empirical strategy used in this paper.

[33] Lacey v. Laird 166 Ohio St. 12, 139 N.E. 2d 25 (1956).

A.2 Access to Abortion

Prior to the January 1973 Supreme Court *Roe v Wade* decision legalizing abortion on-demand through the first trimester, 6 states had already done so: California in Sept 1969,[34] followed by Hawaii,[35] Alaska,[36] New York,[37] and Washington State[38] in 1970, and Washington D.C. in 1971, with *de facto* legalization occurring there in the wake of *United States v. Vuitch*.[39,40] Ananat, Gruber, and Levine (2007), Levine et al. (1999), Gruber, Levine, and Staiger (1999), Joyce, Tan, and Zhang (2013), and others have shown that live births declined for women in their prime childbearing years in the early repeal states compared to the non-repeal states in a manner consistent with a response to the change in policy.

Within the early repeal states, abortion on-demand was legalized inconsistently by age. California initially required minors (20 and younger) to obtain parental consent for an abortion, whereas hospitals in New York City announced they would perform them on minors between 17 and 20 without it. Further, Joyce, Tan, and Zhang (2013) demonstrate that the residency requirements (or lack thereof) acted as an exogenous shock on neighboring states, inducing women to travel to have an abortion, and lowering the birth rate of neighboring states, a finding also corroborated in Klerman (1999), Ananat, Gruber, and Levine (2007), and Levine et al. (1999) among others.

After *Roe*, some states acted to impose restrictions on abortion access, mostly requiring minors to obtain parental consent or to notify their parents before an abortion. These laws have been shown to effectively reduce access to abortion, such that variation in access

[34] People v. Belous 71 Cal. 2d 954 (September 5, 1969)

[35] Haw. Rev. Stat. § 453-16 (2010)

[36] Alaska Stat. § 18.16.010 (2010)

[37] Klerman (1999)

[38] Wash. Stat. § 9.02.100 et seq. Washington's statute permitted abortion through the first four months instead of just the first trimester

[39] 402 U.S. 62 (April 1, 1971)

[40] Myers(2014) and Klerman (1999) point out that in addition to the full-repeal states, 11 states had adopted the American Law Institutes' Model Penal Code (MPC) statutes on abortion, which permitted it if the progression of the pregnancy would cause mental or physical harm to the mother. The convention I use in this paper is to code access as being only those states that granted abortion on-demand, which the MPC statutes did not. Myers (2012, 2014) duly shows that while abortion rates in the MPC states were somewhat higher than in the non-reform states, they were significantly lower than the full-repeal states.

continues after 1973. They are included as a source of variation although their ultimate impact on pregnancy incidence is unclear.[41] This can also be exploited to identify changes in the likelihood to have a child exogenous to the labor force characteristics of either the potential grandparents or parents. Thus, a key innovation in this paper is to instrument for timing and number of grandchildren by using state-by-year differences in access to abortion and oral contraceptives.

[41]Bitler and Zavodny (2001) showed that requiring parental notification or consent did in fact lower the abortion rate among teens in the states that passed these laws. Levine (2003) also found that parental involvement laws lower the abortion rate but did not find a statistically significant reduction in the overall birth rate. The mechanism is itself unclear: Sabia and Anderson (2016) extend Levine's finding by testing specifically for the effect of the parental involvement laws on teen birth control use. Their findings suggest that parental involvement laws do increase the probability that sexually active minors use birth control, but Colman, Dee, and Joyce (2013) examine the same question and do not.

TABLE A1
Month and Year of Unhindered Access to Oral Contraception for Women Under 21

State	18-20	Under 18
Alabama[a]	10/1971	10/1971 (14)
Arizona	5/1972	10/1977
Arkansas	7/1873	3/1973
California	3/1972	1/1976
Colorado	4/1971	4/1971
Connecticut	10/1971	
District of Columbia	8/1971	8/1971
Florida[a]	7/1973	
Georgia	4/1971	7/1972
Illinois	10/1961	
Indiana	9/1973	
Iowa[b]	7/1973	
Kansas	5/1970	5/1970
Kentucky	6/1968	7/1972
Louisiana	8/1972	7/1975
Maine[a,b]	6/1972	
Maryland	7/1971	7/1971
Massachusetts[c]	1/1974	1/1977
Michigan	1/1972	2/1980
Minnesota[c]	6/1973	1/1976
Mississippi	5/1966	5/1966
Missouri[d]	7/1977	
Nebraska[b]	7/1972 (19)	
New Jersey[d]	1/1973	
New York[b]	9/1973	7/1975
North Carolina	7/1971	7/1977
Ohio	6/1965	6/1965
Oregona	9/1971	9/1971
Pennsylvania	4/1970	9/1997
South Carolina	6/1972	6/1972 (16)
South Dakota	7/1972	
Tennessee	5/1971	7/1971
Texas[d]	8/1973	
Utah	7/1960	7/1975
Virginia	11/1971	11/1971
Washington	7/1968	7/1968
West Virginia	7/1972	7/1992
Wisconsin[c]	3/1972	7/1978

Table A1 shows the date of access as the earliest year and month that unmarried, childless women under 21 could obtain contraception without parental or spousal consent.
[a] Access for minors under certain exemptions: being married, already being a parent, being a high school graduate, or the physician believes there is harm to the minor by not providing service.
[b] IA lowered its age of majority first to 19 in July 1972; ME lowered it to 20 first in 10/1969. NE lowered it to 20 first in 3/1969. NY first lowered age of access to 16 in 1973.
[c] Granted access to married minors before granting it to all: MA (1965), MN (1971), and WI (1960).
[d] Married minors can get access, year effective in parenthesis: NJ (1965), TX (1974).
Sources: Author's coding using the state statutes, Myers (2012, 2014), Bailey (2006), Bailey et al. (2011).

TABLE A2
Month and Year of Unhindered Access to Abortion On-Demand for Women 18 and Over

State	21 and Over	18-20
Alabama	1/1973	1/1973
Arizona	1/1973	1/1973
Arizona	1/1973	1/1973
California	9/1974	5/1971
Colorado	1/1973	7/1973
Connecticut	1/1973	1/1973
District of Columbia	4/1971	8/1974
Florida	1/1973	7/1973
Georgia	1/1973	1/1973
Illinois	1/1973	1/1973
Indiana	1/1973	1/1973
Iowa	1/1973	1/1973
Kansas	1/1973	1/1973
Kentucky	1/1973	1/1973
Louisiana	1/1973	1/1973
Maine	1/1973	1/1973
Maryland	1/1973	1/1973
Massachusetts	1/1973	1/1974
Michigan	1/1973	1/1973
Minnesota	1/1973	1/1973
Mississippi	1/1973	1/1973
Missouri[a]	7/1976	7/1976
Nebraska[b]	1/1973	1/1973
New Jersey[c]	1/1973	1/1973
New York	7/1970	7/1970
North Carolina	1/1973	1/1973
Ohio	1/1973	1/1973
Oregon	1/1973	1/1973
Pennsylvania	1/1973	1/1973
South Carolina	1/1973	1/1973
South Dakota	1/1973	1/1973
Tennessee	1/1973	1/1973
Texas	1/1973	1/1973
Utah	1/1973	1/1973
Virginia	1/1973	1/1973
Washington	12/1970	12/1970
West Virginia	1/1973	1/1973
Wisconsin[c]	1/1973	1/1973

Table A2 shows the date of access as the earliest year and month that unmarried, childless women under 21 could obtain an abortion without parental or spousal consent.
[a] Prior to the Supreme Court's *Danforth* decision, Missouri had a spousal consent requirement for married women seeking abortions.
[b] Minors in NE are 18 and under.
[c] New Jersey and Wisconsin had pending court cases challenging the validity of anti-abortion statutes and the legality of abortion on-demand prior to Roe is unclear. Most studies do not treat these as repeal states.
Sources: Author's coding using the state statutes, Myers (2012, 2014), Ananat, Gruber, and Levine (2007), Levine et al. (1999), Joyce, Tam, and Zhang (2013).

TABLE A3

Parental Involvement Laws for Legal Minors, Date Enjoined or Enforced, 1968-2013

State	Enjoined or Explicit Access	Enforced
Alabama	1/1973-9/1987	9/1987
Arizona	1/1973-7/1982	7/1982-10/1985
	10/1985-5/1986	5/1986-8/1987
	8/1987-2/2003	3/2003-9/2009
	10/2009-8/2011	2011-present
Arkansas[a]		1/1973-2/1976
	2/1976-2/1989	3/1989-present
California		9/1969-5/1971
Colorado		1/1973-2/1975
	2/1975-6/2003	6/2003-present
Connecticut[a]		1/1973-11/1998
District of Columbia		4/1971-8/1974
Florida		1/1973-1/1978
	1/1978-6/2005	7/2005-present

Table A3 gives the effect dates of free or conditional access to women under the age of 18 for 1968 PSID states that had changes in the law. Statute dates should be read left to right on down.

[a] Preexisting parental consent or notification law or attorneys general ruling whose legality was left unclear after *Roe* and *Danforth*.

[b] Spousal consent law in effect between 6/1974 and 2/1975.

[c] Abortions without parental involvement permitted for women 16 and over between 7/1973-11/1974, and 17 and over currently.

Sources: the author's coding based on state statutes, Myers (2012, 2014), Sabia and Anderson (2016), Levine (2003), and Bitler and Zavodny (2001).

TABLE A3

Parental Involvement Laws for Legal Minors, Date Enjoined or Enforced, 1968-2013

State	Enjoined or Explicit Access	Enforced
Georgia[a]		9/1991-present
Illinois	1/1973-8/2013	8/2013-present
Indiana	1/1973-4/1973	4/1973-1/1975
	2/1975-8/1984	9/1984-present
Iowa	1/1973-12/1996	1/1997-present
Kansas	1/1973-6/1992	7/1992-present
Kentucky[a]		1/1973-11/1974
	11/1974-3/1989	3/1989-7/1991
	7/1991-7/1994	7/1994-present
Louisiana	1/1973-6/1973	6/1973-1/1976
	1/1976-9/1978	9/1978-3/1980
	3/1980-7/1980	7/1980-present
Maryland	1/1973-5/1977	5/1977-12/1985
	1/1986-present	

Table A3 gives the effect dates of free or conditional access to women under the age of 18 for 1968 PSID states that had changes in the law. Statute dates should be read left to right on down.

[a] Preexisting parental consent or notification law or attorneys general ruling whose legality was left unclear after *Roe* and *Danforth*.

[b] Spousal consent law in effect between 6/1974 and 2/1975.

[c] Abortions without parental involvement permitted for women 16 and over between 7/1973-11/1974, and 17 and over currently.

Sources: the author's coding based on state statutes, Myers (2012, 2014), Sabia and Anderson (2016), Levine (2003), and Bitler and Zavodny (2001).

TABLE A3

Parental Involvement Laws for Legal Minors, Date Enjoined or Enforced, 1968-2013

State	Enjoined or Explicit Access	Enforced
Massachusetts		8/1974-6/1976
Michigan	1/1973-3/1991	3/1991-8/1992
	8/1992-3/1993	4/1993-present
Minnesota	1/1973-7/1981	8/1981-11/1986
	11/1986-8/1988	8/1988-present
Mississippi	1/1973-7/1993	7/1993-present
Missouri[b]	11/1973-6/1974	6/1974-2/1975
	2/1975-6/1983	6/1983-11/1983
	11/1983-8/1985	8/1985-present
Nebraska	1/1973-5/1973	5/1973-11/1975
	11/1975-6/1977	7/1977-12/1978
	1/1979-5/1981	5/1981-9/1983
	9/1983-9/1991	9/1991-present
North Carolina	5/1973-10/1995	10/1995-present

Table A3 gives the effect dates of free or conditional access to women under the age of 18 for 1968 PSID states that had changes in the law. Statute dates should be read left to right on down.

[a] Preexisting parental consent or notification law or attorneys general ruling whose legality was left unclear after *Roe* and *Danforth*.

[b] Spousal consent law in effect between 6/1974 and 2/1975.

[c] Abortions without parental involvement permitted for women 16 and over between 7/1973-11/1974, and 17 and over currently.

Sources: the author's coding based on state statutes, Myers (2012, 2014), Sabia and Anderson (2016), Levine (2003), and Bitler and Zavodny (2001).

TABLE A3

Parental Involvement Laws for Legal Minors, Date Enjoined or Enforced, 1968-2013

State	Enjoined or Explicit Access	Enforced
Ohio	1/1973-9/1974	3/1976-8/1976
	8/1976-10/1990	10/1990-present
Oklahoma	2/1973-5/1975	5/1975-6/1976
	7/1976-6/2001	7/2001-6/2002
	6/2002-11/2004	11/2004-present
Pennsylvanina	1/1973-3/1994	3/1994-present
South Carolina[c]		7/1973-11/1974
	11/1974-5/1990	5/1990-present
South Dakota	1/1973-3/1973	3/1973-6/1976
	7/1976-6/1997	7/1997-present
Tennessee	1/1973-11/1992	11/1992-7/1996
	7/1996-1/2000	1/2000-present
Texas	1/1973-12/1999	1/2000-present
Utah	1/1973-3/1973	3/1973-9/1973

Table A3 gives the effect dates of free or conditional access to women under the age of 18 for 1968 PSID states that had changes in the law. Statute dates should be read left to right on down.

[a] Preexisting parental consent or notification law or attorneys general ruling whose legality was left unclear after *Roe* and *Danforth*.

[b] Spousal consent law in effect between 6/1974 and 2/1975.

[c] Abortions without parental involvement permitted for women 16 and over between 7/1973-11/1974, and 17 and over currently.

Sources: the author's coding based on state statutes, Myers (2012, 2014), Sabia and Anderson (2016), Levine (2003), and Bitler and Zavodny (2001).

TABLE A3

Parental Involvement Laws for Legal Minors, Date Enjoined or Enforced, 1968-2013

State	Enjoined or Explicit Access	Enforced
	9/1973-4/2006	5/2006-present
Virginia[a]		1/1973-6/1976
	7/1976-6/1997	7/1997-present
Washington		11/1970-1/1975
West Virginia	1/1973-5/1984	5/1984-present
Wisconsin	1/1973-6/1992	6/1992-present

Table A3 gives the effect dates of free or conditional access to women under the age of 18 for 1968 PSID states that had changes in the law. Statute dates should be read left to right on down.

[a] Preexisting parental consent or notification law or attorneys general ruling whose legality was left unclear after *Roe* and *Danforth*.

[b] Spousal consent law in effect between 6/1974 and 2/1975.

[c] Abortions without parental involvement permitted for women 16 and over between 7/1973-11/1974, and 17 and over currently.

Sources: the author's coding based on state statutes, Myers (2012, 2014), Sabia and Anderson (2016), Levine (2003), and Bitler and Zavodny (2001).

TABLE A4
PSID In-Sample Daughter/Daughter-in-Law Year of Birth Distribution

	Grandfather Sample			Grandmother Sample		
Year of Birth ⇓	**Frequency**	**Percent**	**Cumulative Percent**	**Frequency**	**Percent**	**Cumulative Percent**
Before 1940	21	0.385	0.385	64	0.806	0.806
1940-1944	60	1.1	1.485	140	1.763	2.569
1945-1949	359	6.582	8.067	716	9.015	11.584
1950-1954	950	17.418	25.486	1,578	19.869	31.453
1955-1959	1,200	22.002	47.488	1,917	24.137	55.591
1960-1964	1,257	23.047	70.535	1,808	22.765	78.356
1965-1969	862	15.805	86.34	978	12.314	90.67
1970-1974	419	7.682	94.023	441	5.553	96.223
After 1974	326	5.977	100	300	3.777	100

Table A4 shows the birth year distribution of four types of adult children: adult daughters, the first wife of an adult son, the mother of the adult son's oldest child if the adult son did not marry, or if the adult son never married and never had a child, his birth year minus 2. The first frequency table is adult children in the grandfather sample and the second table is for the grandmother sample.

B CR2VE Adjustment to 2SLS Standard Errors

In the standard instrumental variables setup, there is a model where the OLS estimator $y = XB + u$ is inconsistent because $\mathbb{E}[u|X] \neq 0$. There exists a set of instruments Z satisfying $\mathbb{E}[u|Z] = 0$. Z must be of full rank and $dim[Z] \geq dim[X]$ and $Corr[Z, X] \neq 0$. Below, I derive a standard error adjustment for the cases where the number of clusters has not allowed the estimator to attain its asymptotic properties, but is plausibly more than the substantial finite sample biases that can be introduced when the number of clusters is small, commonly held to be less than 30.

Following from Bell and McCaffrey (2002) and Cameron and Miller (2015), the CR2VE standard error adjustment takes the form:

$$\tilde{u}_g = \left[I_{N_g} - H_{gg}\right]^{-1/2} \hat{u}_g \tag{9}$$

where G is the total number of clusters, $\hat{u}_g$ are the original predicted residuals for cluster g, and H_{gg} equals:

$$H_{gg} = X_g(X'X)^{-1}X_g'. \tag{10}$$

where H_{gg} is derived from noting that:

$$\hat{u} = \left[I - X(X'X)^{-1}X'\right] u \tag{11}$$

so that $A_g = \left[I_{N_g} - H_{gg}\right]^{-1/2}$ solves

$$\mathbb{E}\left[(X'X)^{-1}\left[\sum_{g=1}^{G} X_g' A_g \hat{u}_g \hat{u}_g' A_g' X_g\right](X'X)^{-1}\right] = V_{clu}[\hat{\beta}_{OLS}] \tag{12}$$

Thus, inserting Equation (9) into adjusted residuals can then be used to calculate the standard cluster-robust variance estimator (CRVE) developed by Liang and Zeger (1986) for unbalanced, finite-observation clusters:

$$V_{clu}[\hat{\beta_{OLS}}] = (X'X)^{-1}\left[\sum_{g=1}^{G} X'_g \tilde{u}_g \tilde{u}'_g X_g\right](X'X)^{-1} \tag{13}$$

As in OLS, IV standard errors can be biased by few clusters. The cluster-robust standard error for the point estimate in the exactly-identified case is:

$$\hat{V}_{clu}[\hat{\beta}_{2SLS}] = (Z'X)^{-1}\left[\sum_{g=1}^{G} Z'_g \hat{u}_g \hat{u}'_g Z_g\right](X'Z)^{-1} \tag{14}$$

and for the overidentified case, it takes the form:

$$\hat{V}_{clu}[\hat{\beta}_{2SLS}] = (X'ZWZ'X)^{-1}X'ZW\left[\sum_{g=1}^{G} Z'_g \hat{u}_g \hat{u}'_g Z_g\right]WZ'X(X'ZWZ'X)^{-1} \tag{15}$$

where, as in OLS, G that is too low can create downwardly biased estimates of the standard errors. This downward bias in turn can lead to overrejection of the null (Bertrand et al. (2004); Cameron and Miller (2015); many others).

I thus extend Equations (9) and (10) by noting that the projection matrix for two-stage least squares is:

$$H^{2SLS} = Z(Z'Z)^{-1}Z' \tag{16}$$

and the cluster-specific projection matrix is:

$$H^{2SLS}_{gg} = Z_g(Z'Z)^{-1}Z'_g \tag{17}$$

So that the corollary solutions to Equation (12) are

$$A_g^{2SLS,Exact} = \sqrt{\frac{G-1}{G}}\left[I_{N_g} - H^{2SLS}_{gg}\right]^{-1} \tag{18}$$

and

$$A_g^{2SLS,OverID} = \sqrt{\frac{G-1}{G}} \left[I_{N_g} - H_{gg}^{2SLS} X (X' H_{2SLS} X)^{-1} X' H_{gg}^{2SLS} \right]^{-1} \quad (19)$$

These are the CR2VE-adjusted standard errors in the spirit of Bell and McCaffrey (2002) and popularized by Angrist and Lavy (2009).

Appendix B References

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C Estimating Grandparenthood Measures

As far as I am aware, no one data source tracks longitudinally how many grandchildren respondents have. Thus, the average number of grandchildren and the fraction of each birth cohort that are grandparents has to be estimated from extant sources. Unfortunately, the PSID is not a broad enough sample to credibly estimate this figure at the state level or education group level by birth cohort. I thus used the Health and Retirement Study (HRS) and Retirement History Longitudinal Survey (RHLS) data,[42] which cover information from 1973-1979 (biennially) and from 1992-2014 (biennially).[43] Specifically, I used the RAND HRS files which compress the survey responses into a "wide" dataset of each's respondent's longitudinal responses.[44] These retirement surveys have large samples of older individuals

[42]The HRS (Health and Retirement Study) is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan.

[43]While the RHLS in fact covers 1969-1979 for the 1906-1911 birth cohort, the first three survey years (1969, 1971, and 1973) did not ask about grandchildren.

[44]The RAND HRS Data file is an easy to use longitudinal data set based on the HRS data. It was developed at RAND with funding from the National Institute on Aging and the Social Security Administration.

and provide the necessary sampling breadth to credibly calculate grandparent statistics.

I combined the HRS and RHLS responses into a synthetic panel covering bienially 1973-1979 and 1992-2014 that estimated by age, birth cohort, and education group the fraction who are grandfathers and their total number of grandchildren. However, this left many cells with missing information. Thus, the second step fits a simple model of either the fraction grandfather or number of grandchildren by age by birth year to extrapolate these results to missing years, ages, and birth cohorts:

$$\begin{aligned} GP\_Measure_{etab} &= \beta_0 + \beta_1 CumulativeBirthrate_{tab} + \beta_2 \mathbb{1}\{Age_{etab} \geq 33\} \\ &+ \beta_3 BirthYear_{db} + \beta_4 BirthYear_{db}^2 + \delta EducationGroup_e + \epsilon_{etab}, \end{aligned} \tag{C.1}$$

where $GP\_Measure_{etab}$ is either the fraction who are grandfathers in birth cohort b at age a in year t, and are in education group e or the number of grandchildren each grandfather has. $EducationGroup_e$ is a vector of education group dummies for the four categories: less than high school, high school, some college, and college degree. $CumulativeBirthrate_{tab}$ is the cumulative sum of the national crude birthrate that starts at age 33 for grandfathers and then monotonically increases until age 84 for each intervening age a in year t.

This model was calibrated only for the very earliest grandparent years. For men under 33, the fraction grandparent and the number of grandchildren was set to zero. For men 33 to 34, the fraction grandparent is set to 0.5% and the grandchild count is set to 0.005. For men aged 35, the fraction grandparent is set to 1% and the grandchild count is set to 0.01. Remaining values for other ages are set by the model.

The reasoning here is that for each birth cohort, higher birthrates in year t represent a higher likelihood of becoming grandparents and a higher likelihood of welcoming a new grandchild, so birthrates are an important control. Yet, simply lagging the birthrates would not work well here, because a 20 year lag on the birth rate for an individual at age 40 is meaningless while being meaningful for a man at age 60. Thus, the running sum of the birthrate for the individual captures both that higher birthrates mean higher chances of being a grandparent and more grandchildren while also accounting for the fact that sustained high birth rates over time should increase these measures monotonically with age.

In order to completely fill the synthetic panel by means of the above model, it was necessary to find information on birthrates then going back to at least 1925, when the members of the oldest in-sample birth cohort (1892) turned 33. For the 1925-1930 period, I used the "Vital Statistical Rates in the United States, 1900-1940" published by the National Office of Vital Statistics, which reports in Table 44 on p. 666-667 the crude birth rate for the birth registration states from 1915-1940. It is important to note that the national crude birth rate is computed just from participating states, which by 1933, included all states.[45]

For the 1941-1967 period, I used the annual National Vital Statistics of the United States reports, which listed the counts of births for each state. To generate the crude birthrates spanning 1941-1967, I then used for the population denominators the Census Bureau's "Annual Estimates of the Population for the U.S. and States, and for Puerto Rico".[46]

For the 1968-2004 period, I used the National Centers for Health Statistics publicly-available natality microdata.[47] These contain either a full or partial sample of all of the birth records down to the county level for all registry states from 1968-2004. For population denominators spanning 1969-2004, I then used the Surveillance, Epidemiology, and End Results (SEER) Program's county by sex by single-age yearly population estimates that I could then aggregate up to the national level. For the 1968 population denominator, I again used the Census Bureau's "Annual Estimates of the Population for the U.S. and States, and for Puerto Rico".

[45] By 1925, all states were participating except for 13 holdouts: Alabama, Arkansas, Colorado, Georgia, Louisiana, Missouri, Nevada, New Mexico, Oklahoma, South Carolina, South Dakota, Tennessee, and Texas. Alabama, Arkansas, Louisiana, Missouri, and Tennessee joined in 1926. Colorado, Georgia, Oklahoma, and South Carolina joined in 1927. Nevada and New Mexico joined in 1928. South Dakota did not join until 1932 and Texas was the last continental state to join the registry in 1933. Alaska and Hawaii joined upon statehood, with statistics being reported for Alaska in 1959 and Hawaii in 1960.

[46] Their total population estimate includes the Armed Forces serving overseas, so I instead aggregated their state population estimates to generate the national estimate of people resident in the United States in a given year.

[47] Most readily available courtesy of the National Bureau of Economic Research at `http://www.nber.org/data/vital-statistics-natality-data.html`

From 2005 to 2015, the publicly available natality microdata suppresses all geographic identifiers, so I used the published birthrates and counts made available in the National Vital Statistics Systems' annual publication on births. These publications include the state and national birth counts, and for population denominators, I again used the SEER population estimates.