Relative Sizes of Age Cohorts and Labor Force Participation of Older Workers

David Neumark [1,2,3,4] · Maysen Yen [1]

Abstract

We study the effects of the size of older cohorts on labor force participation and wages of older workers in the United States. We use panel data on states, treating the age structure of the population as endogenous, owing to migration. When older cohorts (50–59 or 60–69) are large relative to a young cohort (aged 16–24), the evidence fits the relative supply hypothesis. However, when older cohorts are large relative to 25- to 49-year-olds, the evidence points to a relative demand shift. Thus, we need a more nuanced view than simply whether the older cohort is large relative to the population: the cohort that they are large *relative to* matters.

Introduction

In light of population aging in the United States—a development shared with many countries—future employment rates of older individuals will be important determinants of the financial solvency of Social Security, mainly because higher employment implies a continued inflow of Social Security payroll taxes. Illustrating the importance of the employment of older individuals, assumptions about labor force participation (LFP) by age play a key role in the 2016 annual report of the federal Old-Age Survivors

✉ David Neumark
   dneumark@uci.edu

1   Department of Economics, UCI, Irvine, CA, USA

2   Center for Population, Inequality, and Policy, UCI, Irvine, CA, USA

3   National Bureau of Economic Research, 1050 Massachusetts Avenue, Cambridge, MA 02138, USA

4   Institute of Labor Economics (IZA), Schaumburg-Lippe-Strasse 5-9, 53113 Bonn, Germany

Insurance and Disability Insurance trust funds (OASDI 2016: chapter V.B.5). Moreover, policy responses to population aging seek in one way or another to increase employment of older individuals, such as the increases in the Social Security Administration (SSA)–defined full retirement age and changes for early retirees reflected in the Social Security Amendments of 1983. The effects of these reforms and future reforms hinge in large part on employment prospects of individuals at older ages.

Our analysis in this article studies what the changing demographic structure of the United States population implies for the likelihood of employment at older ages. In particular, the overriding question that motivates our analysis is whether increases in the relative shares of the population at older ages are likely to substantially change employment of older individuals. Likely changes in employment independent of Social Security reforms may, for example, lead to increased employment of older individuals that mitigates the increase in the dependency ratio we might otherwise expect from population aging; employment changes also condition how we view the anticipated costs or burdens on older individuals from raising the retirement age and reducing benefits for early retirement and claiming Social Security benefits.

The Baby Boom and other, less-dramatic fluctuations in the sizes of birth cohorts have generated substantial shifts in the relative sizes of older versus younger cohorts. Existing work on the effects of cohort size on labor markets in the United States has tended to focus on the effects of *own* cohort size on wages (e.g., Welch 1979) and sometimes on employment or unemployment (e.g., Korenman and Neumark 2000). These studies (as well as work for other countries, such as Canada; see Morin 2015) have tended to focus on the effects on youths of entering the labor market as part of a large cohort. In general, past studies found that youths entering the labor market as part of large cohorts fare worse at least initially in terms of earning lower wages and therefore having lower employment rates. These effects are interpreted as "relative supply" or "cohort crowding" effects of a cohort's relative size, with a large cohort shifting out the labor supply curve, depressing wages, and hence lowering employment or labor force participation rates (via the reservation wage effect). The evidence that larger cohorts experience relative earnings declines implies that workers in different age cohorts are only imperfectly substitutable, and some research (e.g., Morin 2015) suggests—as seems quite plausible—that the degree of substitutability between cohorts is lower the larger the age difference between them.

Our focus in this study is on older workers—in particular, the effects of the relative size of older cohorts on their LFP and wages. We concentrate on estimating effects among 50- to 59-year-olds and 60- to 69-year-olds. These are the age ranges in which LFP first starts to decline and when most people retire (see Table A1 in the online appendix). Those in the 60–69 age range, in particular, are the ones for whom (in light of population aging) policymakers are trying to increase employment, often through reforms to public pension systems (e.g., Gruber and Wise 2007). Moreover, policy may have considerable scope for increasing LFP in this age range because of low LFP rates (see Fig. 1, panel a).[1]

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[1] We explored grouping the 50- to 59-year-olds with 25- to 49-year-olds, but the data indicated that for our analyses, the behavior of 50- to 59-year-olds was similar to that of 60- to 69-year-olds and dissimilar to that of 25- to 49-year-olds.

a Labor force participation rates

b Cohort shares
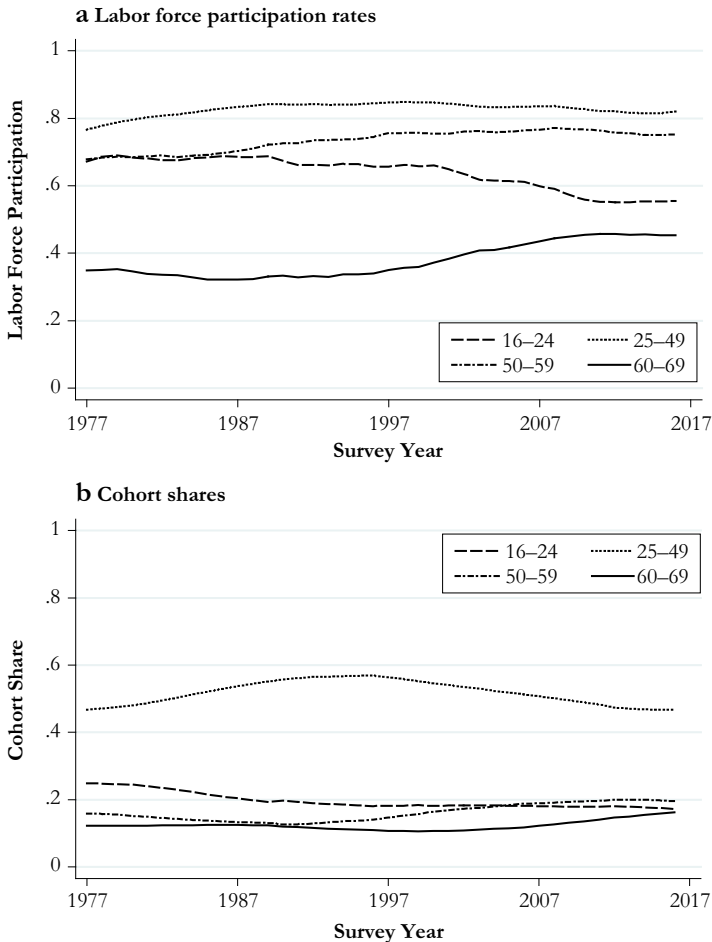
Fig. 1 Labor force participation rates and population share by age group, over time. In panel a, a state panel is first constructed from CPS monthly basic files by aggregating labor force participation for each state, year, and age group. The figure in the panel is created from weighted averages of all states' labor force participation rates, weighted by state population. In panel b, cohort share is constructed from the CPS monthly basic files by dividing the sum of the CPS survey weights for each age group in each state and year by the total sum of the survey weights for ages 16–69 in each state and year. The figure in the panel is constructed from weighted averages of all the states' cohort shares, weighted by state population. *Source:* Census Population Survey (CPS) 1977–2016.

In the standard relative supply framework applied to younger workers, we would simply view larger older cohorts as likely to experience lower wages and hence lower employment or LFP. Some past work has suggested that we should not expect much impact of relative cohort size on older workers. For example, Welch (1979) found evidence suggesting that the adverse effect of entering the job market in a large cohort weakens at older ages, although it does not dissipate. Wright (1991), for the United Kingdom, found that the effect fully dissipates. However, aside from being quite dated, these studies did not focus explicitly on older individuals. Moreover, if the degree of substitution is quite high between older cohorts and other, more experienced workers, consistent with the flattening of earnings-experience

profiles by middle age (Heckman et al. 2006), we might not expect much effect on wages or LFP of being in a large cohort of older workers.

Despite these considerations, there are reasons to expect that the effects of cohort size could be sizable for older workers. Older individuals in their 50s or 60s have low employment rates relative to those in their 40s or 30s, in part because of transitions to retirement, especially in their 60s (e.g., Munnell 2015). At the same time, retirement is quite fluid because many seniors transition to part-time or shorter-term partial retirement—so-called bridge jobs—at the end of their careers (e.g., Johnson et al. 2009) or return to work after a period of retirement (Maestas 2010). Together, these facts suggest that older workers may have quite elastic labor supply on the extensive margin in contrast with workers (especially men) of other ages.[2] If so, the effects of large cohort size on LFP or employment, stemming from wage effects, could be sizable. Moreover, if older workers in partial retirement are leaving career jobs and perhaps taking lower-skilled or less-demanding jobs, they may not be so substitutable with prime-aged workers (aged 25–49), implying potentially larger effects of cohort size on wages for older workers like for young labor market entrants.

To this point, we have focused on the usual relative supply hypothesis about cohort size, which predicts negative effects of large relative cohorts on LFP and wages. However, two factors could push in the opposite direction, toward a positive effect. First, we might expect the age structure of the population to affect the composition of consumption and hence labor demand.[3] The age structure of employment may be such that relative labor demand for an age cohort increases when the relative size of that cohort increases. For an example particularly pertinent to older workers, Cohen (2006) documented the aging of the U.S. nursing workforce, for which demand will surely grow as the population ages.

Second, a relative cohort size measure is just that: a relative measure. Thus, an increase, say, in the size of the 60- to 69-year-old cohort relative to the population means that the cohort is large relative to at least some other narrowly defined age cohorts. If two age cohorts are substitutable, then a decline in the relative size of one of them can imply an increase in the relative demand for the other. For example—again with particular relevance to older workers—the partial/bridge retirement phenomenon may mean that post-retirement workers take lower-skilled jobs more similar to those held by younger workers. In this case, older workers could be substitutable with young workers, and a large cohort of 60- to 69-year-olds relative to young workers could increase demand for 60- to 69-year-olds. Alternatively, if older workers are more substitutable for workers in the prime-aged/middle-aged cohort (aged 25–49), we might find this positive demand response for the size of the older cohort relative to this cohort.

We explore the effects of the relative sizes of age cohorts on LFP and wages, focusing on the effects on older individuals. We use long-term data on cohort size and cohort LFP rates and wages over many decades, exploiting variation across states in a panel data setting that controls for other influences on employment of older workers, as well as for some measures related to longer-term, life cycle responses to cohort size, such as education and marriage.

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[2] The meta-analysis in Evers et al. (2008) pointed to a very low extensive margin labor supply elasticity for men generally. For evidence suggesting sizable extensive margin labor supply elasticities for older workers in the United States, see French and Jones (2012).

[3] For example, Reinhardt (2003: exhibit 1) reported that per capita health spending for 55- to 64-year-olds is double that for 25- to 34-year-olds.

We pay careful attention to the endogeneity of the contemporaneous age structure of a state's potential workforce. Given distinct and persistent patterns of internal migration related to age (e.g., migration to Florida and Arizona) as well as more variable changes in internal migration with respect to economic conditions (and international immigration), we might expect the effects of the relative sizes of different age cohorts to be hard to detect in ordinary least squares (OLS) estimates. For example, an adverse effect of a large cohort on LFP may be obscured because the cohort is large owing to in-migration in response to strong labor demand. We instrument for contemporaneous relative cohort size measures using historical birth data by state and cohort, which should be an exogenous source of variation in states' current demographic structures.

Relevant Prior Work

There is long-standing interest in factors affecting the employment of older workers, often motivated by implications for retirement systems. Perhaps the largest body of research focuses on work incentives created by the Social Security system itself, including the level of benefits (e.g., Burtless 1986), the early retirement age (e.g., Gustman and Steinmeier 2005), the structure of the earnings test (e.g., Friedberg 2000), and the impact of reforms to delay retirement (e.g., Neumark and Song 2013).

Research has also focused on other factors affecting employment of older workers. For example, an outpouring of research has focused on factors that appeared to have slowed the growth in employment and LFP of older workers since the Great Recession, such as changes in age discrimination (Neumark and Button 2014) and increases in Social Security Disability Insurance awards (Mueller et al. 2016).

The effect of the relative sizes of age cohorts on the LFP of older individuals is a potentially important factor to study for at least two reasons. First, variation in cohort size can be used to improve predictions of long-run changes because the sizes of age cohorts can be quite reliably projected far into the future. Second, research on the effects of cohort size on young workers has established that cohort size can be influential. Welch (1979) showed that within schooling groups, the large cohort size of Baby Boomers reduced wages, with a larger impact on highly educated workers and workers early in their career.[4,5] Korenman and Neumark (2000) studied variation over countries and across time to estimate the effect of the relative size of youth cohorts on youth unemployment rates. They used an instrumental variables (IV) approach (as we do here) based on births by cohort and country to account for the endogeneity of cohort size with respect to labor market conditions (via migration) and found that larger youth cohorts are associated with higher unemployment rates.

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[4] Like many others studying cohort size, Welch (1979) focused on wages, but the effects of cohort size on wages should translate into effects on employment and LFP rates, with lower wages reducing these rates and vice versa. Berger (1984) built on Welch's work by looking at effects on earnings profiles, which he interpreted as reflecting human capital investment. This channel of influence is less relevant for older workers.

[5] Macunovich (1999) tried to separate labor supply and labor demand effects of cohort size, suggesting that relative sizes of birth cohorts (and changes in sizes of birth cohorts, to capture leading and lagging effects of a boom) affect supply, whereas relative sizes of current cohorts (and changes) reflect demand. It is not clear why this distinction isolates supply and demand effects; indeed, we use data on births to construct instrumental variables for contemporaneous cohort sizes, without taking a position on whether births drive supply or demand.

We focus not only on the relative cohort size of the cohort of interest—in this case, older individuals—but also on a more detailed characterization of the sizes of other cohorts in different age ranges. Incorporating information on the sizes of other age cohorts can matter because substitutability between cohorts may vary with distance in age. Stapleton and Young (1988) paid more attention to sizes of multiple cohorts, although they focused more on incentives to invest in education owing to the variation in substitutability between cohorts by education, a question further removed from the focus of our study. Our research also differs in focusing on how cohort size affects LFP (and wages) of older individuals.

Empirical Specifications and Strategy

We begin with a standard relative cohort size specification used to estimate the effects of a large cohort of older individuals on their LFP. This specification takes the following form:

$$LFP_{st}^O = \alpha + \beta^{O/T} RCS_{st}^{O/T} + \mathbf{X}_{st}\gamma + \boldsymbol{\lambda}_s + \boldsymbol{\theta}_t + \varepsilon_{st}. \tag{1}$$

The O superscript denotes older cohorts aged either aged 50–59 or 60–69. RCS is a relative cohort size measure, and the O/T superscript denotes that this is computed for older cohorts relative to all working-age cohorts (aged 16–69).[6] $\mathbf{X}$ is vector of controls, including the unemployment rate for 16- to 49-year-olds;[7] the rate of state GDP growth from the previous year to the current one; the shares married and living together, divorced or widowed, and spouse absent or separated;[8] the shares female (when we estimate regressions for men and women combined), Hispanic, black, urban, and union members; the share with less than a high school degree; and the share with a bachelor's degree or higher. The marital status and education controls may reflect some of the life cycle responses to variation in cohort size that reflect decisions taken at earlier ages but also other changes that have a large exogenous component; with the Current Population Survey (CPS) data we use, we are unable to measure years spent in different marital status states. The s and t subscripts denote state and year, and $\boldsymbol{\lambda}_s$ and $\boldsymbol{\theta}_t$ are vectors of fixed state and year effects.

The year effects we can incorporate into the state-level panel data we study are potentially very important. Many economy-wide changes, including factors such as technology, trade liberalization, increases in women's LFP, and declines in marriage, can influence employment of individuals of different ages. In aggregate data, there would be no way to control for these potentially confounding influences.

LFP is the state-by-year average. The LFP and RCS variables are entered in logs, so $\beta^{O/T}$ is an elasticity. Because we use sample estimates of state-level averages to construct our data, we always use generalized least squares, weighting by average state

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[6] We verify that estimating Eq. (1) for the size of the 60–69 cohort or the 50–59 cohort relative to the 16–49 cohort (or the 16–59 cohort, for the analysis of 60- to 69-year-olds) yields very similar results to defining the size of the older cohorts relative to 16- to 69-year-olds.

[7] This is defined for men, women, or both sexes, corresponding to the sample used in the regression.

[8] Until 1989, the data combined divorced and widowed, and combined spouse absent and separated.

population measured over the sample period. Our IV estimates (described shortly) are similarly weighted. We also estimate versions of Eq. (1) for the state-by-year log of average hourly wages.[9]

The estimate of $\beta^{O/T}$ measures the impact of the size of the older cohort relative to the workforce on LFP (or wages) of that older cohort. We would expect similar qualitative results for cohorts of other ages, viewed through the simple mechanism of supply shifts. We hence also estimate Eq. (1) for younger cohorts (aged 16–24, denoting the cohort size variable $RCS^{Y/T}$) and prime-aged cohorts (aged 25–49, denoting the cohort size variable $RCS^{P/T}$, with the corresponding coefficients defined as $\beta^{Y/T}$ and $\beta^{P/T}$).

The relative cohort size measures may be endogenous. One possibility is that people migrate to where labor market conditions for their age group are better. This would create a bias against finding evidence, predicted by the relative supply hypothesis, that a larger relative cohort size reduces LFP or wages because the cohort size may expand in response to high labor demand (which boosts LFP and wages). We might expect this kind of migration to be more common for younger cohorts.

In contrast, older individuals may be more likely to migrate for retirement-related reasons. States that are retirement destinations will tend to have larger relative older cohort sizes but lower LFP rates, not because of cohort-size effects on labor supply but through selective in-migration of older retirees. And similarly, states *from* which retirees (or near-retirees) migrate will tend to have lower relative cohort sizes at older ages but high LFP because of selective out-migration of retirees. The endogeneity bias from retirement-induced migration is thus in the opposite direction to the endogeneity bias from employment-induced migration, with retirement-induced migration biasing the LFP results *in favor* of evidence for the relative supply hypothesis. Of course, some older people may migrate based on labor market conditions if they entertain the possibility of some post-retirement work, so the direction of bias is ultimately an empirical question. Finally, in contrast to LFP, there is no clear prediction about bias in the estimates of Eq. (1) for older cohorts' wages based on retirement-related migration.

Migration flows appear to be large enough to matter, and this is borne out in our IV estimates. Figs. A1–A4 in the online appendix show data on interstate in-migration rates for retirement-related and work-related reasons, based on CPS Annual Social and Economic Supplement (ASEC) data. Fig. A1 shows data for 60- to 69-year-olds, with states ordered by retirement related in-migration rates. The states near the top of this list (e.g., Arizona, Florida, and Nevada) are not surprising. For these states, the *one-year* in-migration rates are near 0.4%. Thus, interstate in-migration could, over a number of years, result in sizable changes in the cohort share. A good deal of work-related interstate migration is reported for this age group, suggesting that the direction of bias when we estimate Eq. (1) for the older cohort is unclear and depends on the magnitudes and endogeneity of the migration flows.

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[9] We estimated all our main specifications without weighting (available upon request). We put less store in the unweighted estimates because the first stage is much weaker, likely because of the greater weight put on smaller states with less accurate estimates. (Correspondingly, the reduced-form relationships are also much weaker.) That said, a comparison of the OLS estimates shows no indication of substantive differences in the results.

Figure A2 in the online appendix shows the data for 50- to 59-year-olds. This group exhibits less retirement-related migration: for the states with the highest rates, the level is about one-half (0.2 percentage points) that for 60- to 69-year-olds. Interestingly, the states for which retirement-related migration is highest are somewhat different for 50- to 59-year-olds than for 60- to 69-year-olds. For 50- to 59-year-olds, far more migration is work-related.

Figures A3 and A4 in the online appendix show the data for the other two younger cohorts, now with states ordered by work-related in-migration rates. Not surprisingly, these cohorts show very little retirement-related migration. However, work-related in-migration rates are often quite high, with one-year rates well above 1% for 25- to 59-year-olds and 1.5% for 16- to 24-year-olds. Thus, again, over many years, in-migration could have substantial effects on the cohort share.

To address the potential endogeneity of relative cohort size, we instrument for the relative cohort size variables using predicted relative cohort sizes based on past births in the state for the years in which members of a cohort would have been born. Thus, for example, the IV for $RCS^{O/T}$ in 2000—in 2000, the number of people currently in the state aged 60–69 divided by the number aged 16–69—is the ratio of the number of people born in the state between 1931 and 1940 to the number of people born in the state between 1931 and 1984. The logic of this IV is clear. The instrument for relative birth-cohort size should predict the contemporaneous relative cohort size quite well— and it does. Further, it is hard to fathom a reason why the instrument for relative birth-cohort size, often constructed from very long lags, would affect current labor market outcomes conditional on the contemporaneous relative cohort size variable; hence, the instrument should satisfy the exclusion restriction.[10] Thus, the instrument for relative birth-cohort size should purge the contemporaneous relative cohort size variable of variation attributable to migration. It should also help correct for other sources of bias, such as measurement error in the estimation of the contemporaneous relative birth cohort variables; the latter are estimated from the CPS, whereas the birth cohort variables are constructed from the universe of birth records.

The standard expectation, based on the relative supply hypothesis regarding cohort size, is that the effects of $RCS^{O/T}$ on LFP and wages will be negative, as will the effects of $RCS^{Y/T}$ and $RCS^{P/T}$ for the younger cohorts. However, if older cohorts have extensive-margin labor supply responses that are more elastic, we might find larger negative estimates for LFP of older cohorts. In contrast, the effects of relative cohort size could go in the other direction because of the effects of age structure on the age composition of labor demand, and other differences between cohorts could arise because of substitution between workers in different age cohorts.

Our main analysis extends beyond Eq. (1). In particular, we explore whether the effects of age structure on LFP and wages of older workers are more complex than simply an effect of their cohort size relative to the working-age population, owing to more complex spillovers between cohorts of different ages. These complexities could arise through the demand side, depending on how the relative sizes of other cohorts

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[10] As indirect evidence, we verified that our IV does not predict contemporaneous state GDP growth, regardless of whether we condition on the contemporaneous relative cohort size variable. This holds true across age groups and in the richer specifications described later with two relative cohort size variables and two IVs. (These results are available upon request.)

affect demand for older workers. They could also arise through the supply side, given that a large relative cohort of older workers could be driven by a smaller cohort of very young workers or of prime-/middle-aged workers, and there may be different degrees of substitutability between these cohorts and older workers.

To address these questions, we modify Eq. (1) and instead estimate a model with separate effects of the size of the older cohort relative to the two younger age cohorts:[11]

$$LFP_{st}^{O} = \alpha + \beta^{O/Y}RCS_{st}^{O/Y} + \beta^{O/P}RCS_{st}^{O/P} + \mathbf{X}_{st}\gamma + \lambda_s + \theta_t + \varepsilon_{st}^{'}. \qquad (2)$$

The estimate of $\beta^{O/Y}$ captures the effect of the size of the older cohort relative to the younger cohort, and the estimate of $\beta^{O/P}$ captures the effect of the size of the older cohort relative to the prime-aged cohort.[12] Eq. (2) can tell us, for example, whether the effect of a large older cohort on LFP varies with whether the older cohort is large relative to the cohort of workers distant in age (i.e., the young) or relative to the cohort of those closer in age. As we do for Eq. (1), we estimate versions of Eq. (2) for the state-by-year log of average hourly wages.[13]

We also address endogeneity bias in Eq. (2). Indeed, differential responsiveness of migration across age groups could be particularly problematic in estimating Eq. (2). For example, suppose there is strong retirement-related migration of older individuals. We would not expect any such response among the younger cohort; in contrast, at least some retirement-related migration could occur in the prime-aged group. In that case, the negative correlation between $\varepsilon^{'}$ and $RCS^{O/Y}$ in Eq. (2) could be particularly strong. We use the same overall strategy but now use two IVs for the two relative cohort size variables in Eq. (2). For example, the IV for $RCS^{O/Y}$ in 2000—in 2000, the number of people currently in the state aged 60–69 divided by the number aged 16–24—is the ratio of the number of people born in the state between 1931 and 1940 to the number of people born in the state between 1976 and 1984. And the IV for $RCS^{O/M}$ in 2000—the number of people currently in the state aged 60–69 in 2000 divided by the number aged 25–59—is the ratio of the number of people born in the state between 1931 and 1940 to the number of people born in the state between 1941 and 1975.

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[11] Because results for Eq. (1) are very similar for the size of the 60–69 cohort or the 50–59 cohort relative to the 16–49 (or 16–59) cohort, the differences in results we find using Eq. (2) have to do only with differences in the sizes of the older cohorts relative to the sizes of the 16–24 or 25–49 cohorts.

[12] From here, we use "prime" to refer to ages 25–49. This is not meant to reflect a judgment about age, but use of "middle age" for 25- to 49-year-olds is likely to create more confusion.

[13] In research explaining wage differences between groups, a similar specification is sometimes estimated for relative wages. For example, Card and Lemieux (2001) assumed a production function including constant elasticity of substitution (CES) subaggregates, by education, of workers of different ages. The analyses in Stapleton and Young (1988) and Welch (1979) have a similar flavor. In contrast to understanding the evolution of wage differences between groups, our primary focus is on understanding the influence of relative cohort size on LFP, and the effects of relative cohort size on wage levels are most important for understanding this influence. Moreover, more recent work on cohort size has focused on the effects on wage levels (e.g., Berger 1984; Korenman and Neumark 2000; Macunovich 1999; Morin 2015). In addition, the relative outcome specifications would make sense for the estimates of Eq. (1) but not Eq. (2) because the latter includes two different relative cohort size variables on the right-hand side. For these reasons, and because Eq. (2) provides our key results (which, regardless, have to do more with effects on LFP than on wages), we focus on specifications of the effect of relative cohort size on wage levels. However, for completeness, we estimated the log average wage models corresponding to Eq. (1) using relative measures instead, and these results are available upon request.

Data

Our contemporaneous population and LFP data come from CPS monthly basic files from 1977 to 2016.[14] The microdata are aggregated to create state-by-year measures. Cohort sizes are constructed by weighting individuals by the survey weights used to aggregate up to population estimates in order to make the estimates population representative. For example, for the oldest cohort of 60- to 69-year-olds, $RCS_{st}^{O/T}$ is constructed by summing the survey weights in state s at time t for ages 60–69 and dividing it by the sum of the survey weights in state s at time t for the entire 16–69 age group. LFP rates are constructed using the same survey weights.

Wage data come from the CPS merged outgoing rotation group files, which are available from 1979. The hourly wage is measured directly as earnings per hour when available (for those paid hourly). For those not paid hourly, we construct this variable by dividing earnings per week by the usual hours worked.[15] We trim the computed hourly wages by removing hourly wages below half the state minimum wage or above $200/hour (in 2016 dollars). We then average hourly wages by state and year, using the survey weights.

The IV construction is considerably more involved. We use historical series on births by state, based on U.S. vital statistics reports published by the National Center for Health Statistics (NCHS). The data are available in two forms, either through *Births: Final Data* reports (NCHS 1974–1975, 1976–1977, 1978–1980, 1982–1996, 1997–1998, 1999–2017) retrievable online from 1970,[16] or in U.S. vital statistics reports (Grove and Hetzel 1968; Linder and Grove 1947; NCHS 1934, 1934–1938, 1939–2005, 1984); both reports are typically published two years after the reported year. The U.S. vital statistics reports have been produced since 1890, although birth information was not captured until 1915, when 10 states and the District of Columbia adopted the birth-registration system (Linder and Grove 1968). Other states began to trickle in, with the final participating states of Texas joining in 1933 and then Alaska joining in 1945 (see Table A2, online appendix). The *Births: Final Data* series is more recent. It started in 1971 and was published concurrently with the U.S. vital statistics reports, but the latter was phased out by 2003. The two reports are not completely identical but do not have large discrepancies.[17] We use the reported numbers of births in the *Births: Final Data* reports as our source back to and including 1971, and we use the U.S. vital statistics reports for prior years back to 1931.[18] Prior to 1931, the number of births is not available, so we reconstruct the level from the crude birth rates, defined as the number of births per 1,000 population.[19]

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[14] Our data come from the Integrated Public Use Microdata Series (Flood et al. 2017).

[15] Observations are not used if earnings are not reported, or if only weekly earnings but not hours are reported.

[16] *Births: Final Data* is the name of this report in later years. In earlier years the name varied, as reflected in the NCHS documents for the corresponding publication dates listed in the References.

[17] This is based on personal communications with Michelle Osterman, a health statistician at NCHS (5/2/17 and 5/8/17). The *Births: Final Data* series is easier to navigate and seems to be cited more often.

[18] One exception is the year 1979, in which the final report is not available online. For this year, we use birth numbers from the U.S. vital statistics report.

[19] *Vital Statistics Rates in the United States 1900–1940* (Linder and Grove 1947) contains birth rates and estimated population sizes for 1915 to 1930, which allows us to estimate the number of births.

There is surely some measurement error in the birth instruments we construct, and the accuracy of reporting is worse in the earlier data. For example, our constructed number of births from 1915 to 1930 from crude birth rates and estimated population sizes suggests a sharp decrease in the number of births from 1930 to 1931, which implies that we are overstating the number of births from 1915 to 1930, likely because crude birth rates are inconsistent because of unclear adjustments for underregistration.[20] Overall, the general issues with crude birth rates contributed to our decision to use the number of births from the individual yearly files either from *Births: Final Data* or U.S. vital statistics reports, whenever available.

Despite these concerns, measurement error in IVs is of less concern than measurement error in the variables of interest. Indeed, if the measurement error in the instrument is uncorrelated with the variable(s) for which we are instrumenting, and uncorrelated with the error term in the equation of interest, the measurement error does not introduce any inconsistency in the IV estimation, although it can weaken the instrument and make the IV estimate less precise. This is true even if the measurement error is worse in earlier periods (i.e., heteroskedastic). Therefore, although we note these potential issues with the early birth data, we do not believe that these issues pose substantive challenges to our empirical analysis.

To have data on the birth instrument for the oldest people in our sample (age 69), we shorten the CPS panel we use to begin in 1984 rather than in 1977 (for LFP) or 1979 (for wages). Even then, our panel with the instrument is unbalanced because we do not have the requisite birth data for all states from the earliest year because states started reporting births in different years. However, there are no gaps between years. For example, for 1984, data are available for 10 states and Washington, DC because the number of births in the old cohort is drawn from the number of births in the years 1915–1924. In later years, more states are added as their number of births are reported. For example, Georgia, which first started collecting birth data in 1928, has data available beginning with 1997, when the number of births for 69-year-olds was recorded.

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[20] For example, the crude birth rates from 1915 to 1929 do not have birth rates adjusted for underregistration, but the 1930 to 1940 crude birth rates do. In general, these earlier adjustments to crude birth rates are not well documented or transparent except for the dates of the adjustments. The most egregious example is that the crude birth rates recorded for 1940 are different between the vital statistics for 1900–1940 (Linder and Grove 1947) and those for 1940–1960 (Grove and Hetzel 1968). Michelle Osterman and her colleague, Brady Hamilton, were unable to reconcile this difference but believe that the more recent vital statistics (for 1940–1960) are accurate (personal communication, 5/8/17).

Results

Descriptive Statistics

In Fig. 1, panel a shows LFP rates by age group, and panel b shows cohort population shares. (Figures A5 and A6 in the online appendix show the corresponding information for wages.) We do not want to infer much from these aggregate time series,[21] but the rise in LFP of 60- to 69-year-olds in the latter part of the sample period (Fig. 1, panel a) coincides with an increase in their relative cohort size (Fig. 1, panel b) (correlation = .595). On the surface, this is inconsistent with the usual relative supply cohort size hypothesis, in which a large cohort size depresses LFP. Moreover, Fig. A5 shows that the rising LFP of the older cohort was accompanied by rising real wages (correlation = .627), also inconsistent with the relative supply hypothesis.

We next explore the relationships between relative cohort size and both LFP and wages in more detail, providing similar evidence for different age cohorts and showing both the time series and the within-state variation and covariation between these three variables. First, to avoid having to compare across the panels in Fig. 1, we graph in Fig. 2 the time series on LFP rates and relative cohort size for each of the four age cohorts. Panel a of Fig. 2, for 16- to 24-year-olds, parallels the evidence for 60- to 69-year-olds in that LFP rates and relative cohort size tend to move in the same direction, rather than the opposite direction as predicted by the relative supply hypothesis. The evidence for those aged 25–49 and 50–59 is less clear.

Figure A6 in the online appendix shows the same type of evidence but for real wages. Here, the evidence for the younger cohorts is mixed. The evidence for 25- to 49-year-olds shows rising wages in the latter part of the sample period, when relative cohort size is declining, which is consistent with the relative supply effect of cohort size. However, in the earlier part of the sample, wages are flat as relative cohort size rises, and the correlation is negative, as reported in the notes to the figure. For 16- to 24-year-olds, in contrast, the wage and relative cohort size series track each other in the early part of the sample, which is inconsistent with the relative supply effect of a larger cohort; thereafter, both series are largely flat. The correlations for this age group, as well as the two older cohorts, are positive (see the figure notes).

The final two online appendix figures (Figs. A7 and A8) provide information on changes over time at the state level, showing scatterplots of the 1977–2016 changes for LFP and 1979–2016 changes for wages for each state. Thus, the data points summarize the overall changes over the sample period in contrast to the year-by-year changes graphed for the aggregate time series. Fig. A7 shows evidence of negative relationships between LFP rates and relative cohort size for all four age cohorts: 16–24, 25–49, 50–59, and 60–69. The slope coefficient, however, is particularly large for 16- to 24-year-olds (-0.979) and near 0 for 50- to 59-year-olds. (The correlation is statistically significantly different from 0 only for 16- to 24-year-olds.) These findings contrast with the positive correlations in the time-series data shown in Fig. 2 and are more

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[21] However, one potential advantage of the national time-series data, relative to more disaggregated data, is that they should not be influenced by bias from endogenous migration across states. On the other hand, with national time-series data, we cannot control for aggregate trends or changes in age-specific labor demand, which could bias our results. These trends/changes may underlie some of the apparent inconsistencies in the aggregate data.
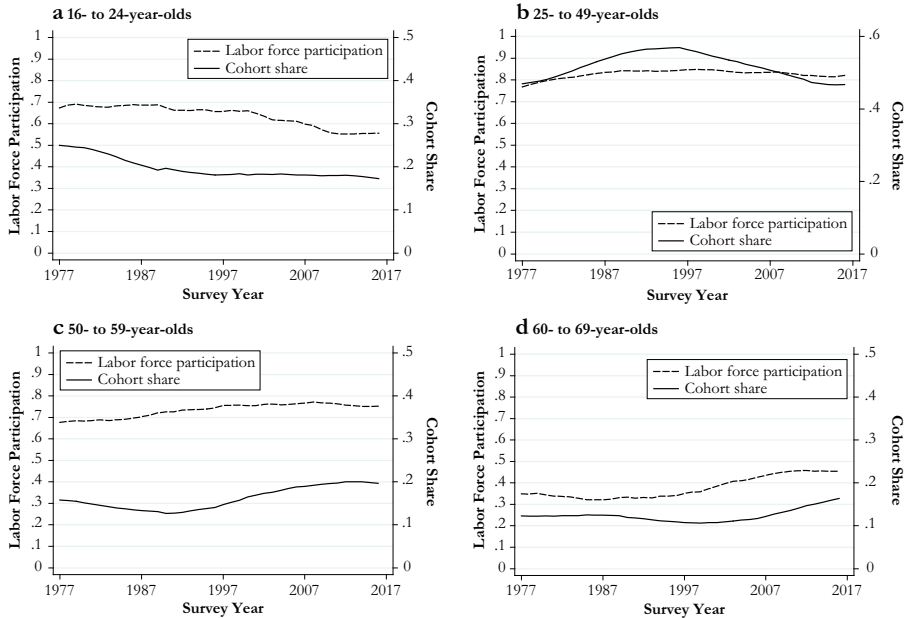
Fig. 2 Labor force participation rates and cohort shares by age group, over time. Pearson correlation coefficients for 16- to 24-year-olds, 25- to 49-year-olds, 50- to 59-year-olds, and 60- to 69-year-olds are .640, .784, .628, and .595, respectively. *Source:* Data source and series construction are explained in notes to Fig. 1.

consistent with the relative supply effect of cohort size. Fig. A8 of the online appendix shows evidence of a positive relationship for wages for cohorts aged 16–24, 25–49, and 50–59, inconsistent with the relative supply effect of cohort size, but the evidence for those aged 60–69 is more consistent with this effect.

Thus, the time-series evidence is largely inconsistent with the relative supply effect of cohort size (Fig. 2 and Fig. A6). The state-level evidence is consistent with this effect for LFP for all age cohorts (Fig. A7) but is not consistent for wages in three of four cases (Fig. A8). However, this evidence is suggestive at best, and the state-level evidence may be particularly prone to endogeneity bias, with the bias for the older cohorts for LFP likely in the negative direction. Hence, we next turn to the regression estimates, with the IV estimates most likely to uncover the true effects of relative cohort size.

LFP: OLS Regression Estimates

Table 1 reports OLS regression estimates of the effects of relative cohort size on LFP for each age group; these are estimates of Eq. (1). We present results for both sexes combined and then for men and women separately.

Over the sample period and ages considered, there are potential reasons to prefer one approach or the other. For the older cohorts, LFP and careers of men and women were quite different, and men and women were likely not viewed as highly substitutable labor inputs. The younger cohorts, in contrast, experienced rising LFP of married women and some convergence in the occupational distribution. Unlike, say, in the

Table 1 OLS regressions of log labor force participation rate on log cohort share, 1977–2016

	Both Sexes				Men				Women			
	Ages 16–24	Ages 25–49	Ages 50–59	Ages 60–69	Ages 16–24	Ages 25–49	Ages 50–59	Ages 60–69	Ages 16–24	Ages 25–49	Ages 50–59	Ages 60–69
ln(Cohort Size/Population Aged 16–69)	0.101***	0.042	0.046*	−0.131*	0.033	−0.002	0.032†	−0.077	0.101*	0.039	0.062†	−0.099
	[0.036]	[0.032]	[0.021]	[0.063]	[0.033]	[0.016]	[0.017]	[0.059]	[0.038]	[0.057]	[0.035]	[0.082]
Mean Labor Force Participation	.64	.83	.73	.38	.67	.93	.83	.45	.61	.73	.64	.31
R^2	.92	.91	.90	.89	.92	.91	.81	.80	.89	.93	.93	.88
Incremental R^2	.029	.033	.008	.011	.013	.015	.024	.011	.052	.037	.006	.006

Notes: The table reports estimates of Eq. (1). All specifications include fixed state and year effects, controls for the age 16–49 unemployment rate, state GDP growth, a constant, share married and live together, share divorced or widowed, share spouse absent or separated, share Hispanic, share non-Hispanic black, share with a bachelor's degree or more, share high school dropouts, and share urban. The columns under "Both Sexes" also include the share female. Regression is weighted by average state population through the sample period. Estimates are weighted by average state population through the sample period. Incremental R^2 refers to addition to R^2 beyond fixed state and year effects. Standard errors are clustered by state. $N = 2{,}040$. Data sources are described in notes to Fig. 1.

†$p < .10$; *$p < .05$; **$p < .01$

research literature on the impacts of relative supplies of workers by educational level, we are unable to disentangle the effects of variation in male and female relative cohort size on outcomes for men and women because the relative cohort size variables are so highly correlated (partial correlations greater than .99, conditioning on the other control variables in our models). Thus, we simply report the results separately by sex and for both sexes combined, and note that the findings are generally robust.

For both sexes combined, we find a positive and significant effect of relative cohort size for 16- to 24-year-olds, with an elasticity of 0.101. The estimate for 25- to 49-year-olds is also positive but insignificant and smaller (an elasticity of 0.042). The estimated elasticity for 50- to 59-year-olds is a bit larger (0.046) and statistically significant. For the oldest cohort (aged 60–69), the estimate is significant and negative, with a larger absolute estimated elasticity (−0.131). The sign pattern of the estimates is almost always the same for men and women separately (with one minor exception for an estimate very close to 0 for 25- to 49-year-old men). In addition, some of the estimates for men and women separately are smaller in absolute value than the estimates for both sexes combined, and in conjunction with larger standard errors, the separate estimates are less likely to be statistically significant.

The negative estimates for the oldest cohort are consistent with the relative supply effect of a larger cohort. The positive estimates for two younger cohorts and the older (50–59) cohort are not. Recall, though, that there is a potential positive bias in the estimates for cohorts for which migration is more related to labor market conditions, with in-migration to areas with stronger labor demand boosting both relative cohort size and LFP. At the same time, the estimates for the older cohorts could be biased in the opposite direction from retirement-related endogenous migration.

LFP: IV Estimates

Table 2 reports the IV estimates of Eq. (1) for LFP, based on the level of the LFP rate for the cohort. Recall that constructing the IV causes us to lose the earliest years of the sample (plus some other earlier observations for some states). Thus, in Table 2, we first report OLS estimates for the same sample we can use for the IV estimation. The OLS estimates are consistent with Table 1. For both sexes combined, we continue to find a positive and significant effect for the youngest cohort, a weaker positive effect for the 50- to 59-year-old cohort, and a negative although no longer significant effect for the oldest cohort. For 25- to 49-year-olds, however, the estimate is now negative. For men and women separately, the sign pattern is always the same, but there is some variation in which estimates are statistically significant.

The IV estimates tell a strikingly different story. For the two younger cohorts (aged 16–24 and 25–49), the IV estimates point to a negative effect of relative cohort size on LFP, which is significant for the 25- to 49-year-old cohort, overall and for women. The estimated elasticities range from −0.114 to −0.411. These estimates are consistent with the standard relative supply hypothesis about the effect of relative cohort size. In every case (six estimations), the direction of change relative to the OLS estimates is consistent with positive bias induced by in-migration to stronger labor markets: that is, the IV estimates become negative or become more negative.

In contrast, for the two older cohorts (aged 50–59 and 60–69), we find strong evidence of a large positive effect of relative cohort size, for both sexes combined

Table 2 OLS and IV regressions of log labor force participation rate on log cohort share, 1984–2016

	Ages 16–24		Ages 25–49		Ages 50–59		Ages 60–69	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Both Sexes								
ln(cohort size / population aged 16–69)	0.081*	−0.222	−0.036	−0.257*	0.032	0.295*	−0.074	0.385†
	[0.037]	[0.178]	[0.039]	[0.111]	[0.024]	[0.122]	[0.052]	[0.231]
IV: ln(cohort births / births aged 16–69)								
Reduced-form coefficient (dependent variable = ln(LFPR))		−0.061		−0.086*		0.055*		0.103†
		[0.048]		[0.034]		[0.021]		[0.054]
First-stage coefficient (dependent variable = ln(cohort size / population aged 16–69))		0.276**		0.336**		0.188**		0.267**
		[0.048]		[0.063]		[0.041]		[0.064]
Incremental R^2 (first stage)		.046		.015		.005		.011
First-stage F statistic		33.00		28.10		21.05		17.48
Durbin-Wu-Hausman test for endogeneity (p value)		.068		.006		.017		.014
Men								
ln(cohort size/population aged 16–69)	0.014	−0.200	−0.031*	−0.114	0.012	0.241	−0.046	0.255
	[0.036]	[0.193]	[0.014]	[0.077]	[0.020]	[0.224]	[0.053]	[0.223]
IV: ln(cohort births/births 16–69)								
Reduced-form coefficient (dependent variable = ln(LFPR))		−0.054		−0.041		0.031		0.071
		[0.052]		[0.027]		[0.026]		[0.064]
First-stage coefficient (dependent variable = ln(cohort size/ population aged 16–69))		0.272**		0.364**		0.130**		0.279**
		[0.051]		[0.064]		[0.047]		[0.058]
Incremental R^2 (first-stage)		.042		.016		.003		.010
First-stage F statistic		28.51		32.59		7.75		23.57
Durbin-Wu-Hausman test for endogeneity (p value)		.242		.238		.241		.152

Table 2 (continued)

	Ages 16–24		Ages 25–49		Ages 50–59		Ages 60–69	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Women								
ln(cohort size/population aged 16–69)	0.085†	−0.277	−0.059	−0.411*	0.071†	0.477**	−0.013	0.627†
	[0.043]	[0.200]	[0.067]	[0.187]	[0.042]	[0.183]	[0.044]	[0.338]
IV: ln(cohort births/births 16–69)								
Reduced-form coefficient (dependent variable = ln(LFPR))		−0.079		−0.132*		0.099*		0.138**
		[0.054]		[0.057]		[0.039]		[0.050]
First-stage coefficient (dependent variable = ln(cohort size/population 16–69))		0.284**		0.321**		0.207**		0.221**
		[0.054]		[0.068]		[0.041]		[0.075]
Incremental R^2 (first-stage)		.040		.010		.005		.007
First-stage F statistic		27.63		22.31		25.97		8.66
Durbin-Wu-Hausman test for endogeneity (p value)		.047		.011		.019		.005

Notes: Data source is described in notes to Fig. 1, and specification details are described in notes to Table 1. The table reports estimates of Eq. (1). Regression is weighted by average state population through the sample period. Standard errors are clustered by state. The IV used for the cohort share is the total number of births within an age cohort by state and year divided by the total number of births for 16- to 69-year-olds by state and year. We exclude certain years and states with missing birth data for the cohorts because only a handful of states started reporting births in 1915. $N = 1,326$.

†$p < .10$; *$p < .05$; **$p < .01$

and for men and women separately (the estimates are significant for both sexes combined and for women). The estimated elasticities range from 0.241 to 0.627. This evidence is inconsistent with the relative supply effect of a large cohort and instead suggests that labor demand effects from large older cohorts more than offset any supply effects. Like for the two younger cohorts, the IV estimates are quite different from the OLS estimates. However, for the older cohorts, the direction of the change relative to the OLS estimates is in every case (again, six estimations) consistent with negative bias in the OLS estimates from endogenous migration related to retirement; the IV estimates become positive or become more positive. Thus, the IV versus OLS estimates are consistent with the kinds of biases we might expect—related to the job market for younger cohorts and to retirement for older cohorts.

Table 2 also presents additional information about the IV estimates. First, in each panel, we report the reduced-form estimates: the effects on LFP of the relative cohort size variables defined based on births only. These always share the sign and significance of the IV estimates.[22] The reduced-form estimates tend to be roughly one-fourth the magnitude of the IV estimates (as is true in the upcoming analysis of wages). As usual, the IV and reduced-form estimates answer different questions. For the purposes of asking what the behavioral response of LFP of older workers is to exogenous variation in relative cohort size, the IV estimate measures the parameter of interest. The reduced-form estimates capture solely the effects of variation in cohort size driven by the relative sizes of birth cohorts. Because there can be other sources of exogenous variation in cohort size (e.g., related to immigration and changes in industry structure, although not all this variation is exogenous), we regard the IV estimate as more relevant to asking, for example, what population aging implies for the likely LFP of older individuals.

Next, we report the first-stage coefficient estimates and F statistics. The first-stage estimates are always positive and strongly statistically significant. The magnitudes are in the 0.13 to 0.36 range. The F statistics are generally very large, ranging from approximately 8 to 33 (and below 10 in only two cases). Finally, we report p values from the Durbin-Wu-Hausman endogeneity test. For the combined results and the results for women, these are below .05 in all cases but one, consistent with significant evidence of endogeneity bias.[23]

Finally, we also report the incremental R^2 from adding the instrument to the first-stage equations (including the fixed effects). For the older cohorts, it ranges from about 0.5% to 1.1%. (It is also high for the youngest cohort, consistent with a good deal of migration at young ages.) Consistent with the differences between the OLS and IV estimates, this suggests the potential for a good deal of endogenous variation, although of course much of the unexplained variation may not be associated with endogenous responses.

Wages: OLS Regression Estimates

We next turn to estimates of Eq. (1) for the effects of relative cohort size on wages. As reported in Table 3, for both sexes combined, we find a negative and significant effect

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[22] Sometimes the significance level varies, but the same estimates are significant at the 10% level or lower.

[23] As noted earlier, we estimate versions of the models in Table 2 (and in upcoming Table 4) defining the size of the older cohorts relative to the younger cohorts (e.g., 16- to 49-year-olds). The results are very similar (available upon request).

Table 3 OLS regressions of log average hourly wage on log cohort share, 1979–2016

	Both Sexes				Men				Women			
	Ages 16–24	Ages 25–49	Ages 50–59	Ages 60–69	Ages 16–24	Ages 25–49	Ages 50–59	Ages 60–69	Ages 16–24	Ages 25–49	Ages 50–59	Ages 60–69
ln(Cohort Size/Population Aged 16–69)	−0.056†	0.226**	−0.066†	−0.029	−0.054†	0.181**	−0.040	−0.015	−0.009	0.209**	−0.050	−0.023
	[0.030]	[0.075]	[0.038]	[0.061]	[0.032]	[0.064]	[0.032]	[0.068]	[0.026]	[0.066]	[0.042]	[0.050]
Mean Hourly Wage (2016 dollars)	12.05	21.88	23.83	21.78	12.63	24.19	27.55	25.33	11.42	19.21	19.69	17.89
R^2	.986	.995	.993	.983	.978	.993	.989	.967	.985	.996	.993	.981
Incremental R^2	.003	.002	.003	.003	.003	.003	.004	.003	.002	.002	.003	.002

Notes: See notes to Table 1. $N = 1,938$. Data source and other details about the data are described in notes to Fig. A5 in the online appendix.

†$p < .10$; *$p < .05$; **$p < .01$

of relative cohort size for 16- to 24-year-olds, with an elasticity of –0.056, consistent with the relative supply hypothesis. In contrast, for 25- to 49-year-olds, the estimate is large and positive (an elasticity of 0.226). For the two older cohorts (aged 50–59 and 60–69), the estimates are negative, fairly small, and statistically significant (at the 10% level) only for 50- to 59-year-olds (elasticity of –0.066). The magnitudes are similar and the sign pattern of the estimates is the same for men and women separately.[24]

Wages: IV Estimates

Table 4 reports the IV estimates for wages. As discussed earlier, expectations regarding endogeneity bias in the estimated effects of relative cohort size on wages are less clear. First, although the younger and prime-aged cohorts may migrate to strong labor markets, the outward supply shift in these states may not do much to lower wages, and there can be offsetting effects from agglomeration externalities and/or compensating differentials for congestion (e.g., Richardson 1995). Second, for the older cohorts, as noted earlier, there is no clear prediction about bias from retirement-related migration.

In the IV estimates, which are of most interest, we again receive a sharp message. For the two younger cohorts, we find evidence of a positive effect of relative cohort size, for men and women combined, and for men. The elasticities range from 0.20 to 0.45 and are always statistically significant for both sexes combined and for men (in one case at the 10% level). For the two older cohorts, in contrast, the IV estimates always point to a negative effect and are always significant for 50- to 59-year-olds (once at the 10% level) but not significant for 60- to 69-year-olds, which is consistent with the relative supply effect of a large cohort. The elasticities range from −0.16 to −0.56.

Like Table 2, Table 4 also reports diagnostic information about the IV estimates. The first-stage results are the same as for the LFP estimates and hence are not reported again (see Table 2). The reduced-form estimates always share the sign and significance of the IV estimates. And the *p* values from the Durbin-Wu-Hausman endogeneity test indicates evidence of endogeneity bias in just over half the specifications. Although we did not have strong *a priori* expectations of endogeneity bias in wage estimates, the evidence suggests that sometimes such bias occurs.

As it stands, then, the evidence on the estimated effects of a larger cohort on wages cannot be fully reconciled with the estimated effects on LFP. Referring to the IV estimates, the LFP effects for the older cohorts point to a positive demand shift toward older workers when the older cohort is larger, whereas the wage effects are negative (albeit insignificant for 60- to 69-year-olds); only the latter is consistent with the relative supply hypothesis. For the younger cohorts, in contrast, the LFP

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[24] The closest estimates in the existing literature are for the young cohort. Welch (1979) estimated elasticities of "entry" wages with respect to cohort size, for less-educated workers, in the –0.1 to –0.2 range. Macunovich (1999) reported an elasticity of about –0.07 with respect to size of birth cohort for young, less-educated workers. Morin's (2015) elasticities ranged from about –0.05 to –0.09 across age groups. However, estimation methods differ. Welch used relative wages, Morin and Macunovich used the level, and our estimates using a relative wage measure are less consistent with Welch (results available upon request). Our estimates also change when we instrument. Moreover, the results are not comparable for many reasons pertaining to differences in the analysis, including the much earlier data used in many of these other studies.

Table 4 OLS and IV regressions of log average hourly wage on log cohort share, 1984–2016

	Ages 16–24		Ages 25–49		Ages 50–59		Ages 60–69	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Both Sexes								
ln(cohort size/population aged 16–69)	0.041	0.323*	0.089	0.450*	–0.012	–0.526**	0.019	–0.191
	[0.025]	[0.154]	[0.069]	[0.195]	[0.040]	[0.188]	[0.060]	[0.199]
IV: ln(cohort births/births aged 16–69)								
Reduced-form coefficient (dependent variable = ln(average hourly wage))		0.089*		0.151*		–0.099**		–0.051
		[0.044]		[0.061]		[0.032]		[0.054]
Durbin-Wu-Hausman test for endogeneity (p value)		.062		.011		.003		.229
Men								
ln(cohort size/population aged 16–69)	0.017	0.382†	0.048	0.397*	0.015	–0.559†	0.041	–0.155
	[0.030]	[0.204]	[0.059]	[0.178]	[0.030]	[0.314]	[0.076]	[0.230]
IV: ln(cohort births/births aged 16–69)								
Reduced-form coefficient (dependent variable = ln(average hourly wage))		0.104†		0.144*		–0.073*		–0.043
		[0.054]		[0.061]		[0.033]		[0.065]
Durbin-Wu-Hausman test for endogeneity (p value)		.053		.014		.024		.317
Women								
ln(cohort size/population aged 16–69)	0.042	0.198	0.056	0.308	0.020	–0.504**	0.004	–0.236
	[0.030]	[0.154]	[0.064]	[0.240]	[0.053]	[0.190]	[0.045]	[0.199]
IV: ln(cohort births/births aged 16–69)								
Reduced-form coefficient (dependent variable = ln(average hourly wage))		0.056		0.099		–0.104**		–0.052
		[0.047]		[0.078]		[0.037]		[0.046]
Durbin-Wu-Hausman test for endogeneity (p value)		.349		.216		.002		.236

Notes: The first-stage estimates (and F statistics) are the same as in Table 2. See notes to Table 2. $N = 1,326$. Data source is described in notes to Fig. A5 in the online appendix.

† $p < .10$; * $p < .05$; ** $p < .01$

effects are most consistent with a negative relative supply effect, although the evidence is statistically significant only for 25- to 49-year-olds, whereas the wage effects are in the opposite direction. These contradictory findings are summarized later in Table 7.

Separate Effects of the Size of the Older Cohort Relative to Younger or Prime-Aged Cohort

When we estimate the richer model (Eq. (2)) allowing for separate effects of the size of the older cohort relative to the two younger cohorts, we obtain a more coherent set of findings. These estimates are reported in Tables 5 (for 50- to 59-year-olds) and 6 (for 60- to 69-year-olds). Here, we report the OLS and IV estimates for the consistent sample for which we can compute both. The IV estimations in Tables 5 and 6 are more demanding because there are now two endogenous variables. The first-stage F statistics

Table 5 OLS and IV regressions of log labor force participation rate and hourly wages of 50- to 59-year-olds on log relative cohort sizes for 16- to 24-year-olds and 25- to 49-year-olds, 1984–2016

	LFP		Hourly Wages	
	OLS	IV	OLS	IV
Both Sexes				
ln(cohort size aged 50–59 / cohort size aged 16–24)	–0.020	–0.061	0.019	–0.114
	[0.021]	[0.058]	[0.023]	[0.132]
ln(cohort size aged 50–59 / cohort size aged 25–49)	0.038†	0.255**	–0.030	–0.318**
	[0.020]	[0.085]	[0.035]	[0.099]
First-stage F statistic				
Dependent variable = ln(cohort size aged 50–59 / cohort size aged 16–24)		12.32		12.32
Dependent variable = ln(cohort size aged 50–59 / cohort size aged 25–49)		11.61		11.61
Durbin-Wu-Hausman test for endogeneity (p value)		.001		.002
Men				
ln(cohort size aged 50–59 / cohort size aged 16–24)	–0.027†	–0.103	0.023	–0.179
	[0.014]	[0.088]	[0.018]	[0.210]
ln(cohort size aged 50–59 / cohort size aged 25–49)	0.030	0.190	–0.014	–0.336*
	[0.019]	[0.120]	[0.029]	[0.157]
First-stage F statistic				
Dependent variable = ln(cohort size aged 50–59 / cohort size aged 16–24)		5.83		5.83
Dependent variable = ln(cohort size aged 50–59 / cohort size aged 25–49)		7.76		7.76
Durbin-Wu-Hausman test for endogeneity (p value)		.056		3.026
Women				
ln(cohort size aged 50–59 / cohort size aged 16–24)	–0.019	–0.011	0.013	–0.092
	[0.029]	[0.100]	[0.030]	[0.136]
ln(cohort size aged 50–59 / cohort size aged 25–49)	0.074*	0.392**	0.002	–0.293*
	[0.031]	[0.116]	[0.040]	[0.134]
First-stage F statistic				
Dependent variable = ln(cohort size aged 50–59 / cohort size aged 16–24)		12.31		12.31
Dependent variable = ln(cohort size aged 50–59 / cohort size aged 25–49)		10.71		10.71
Durbin-Wu-Hausman test for endogeneity (p value)		.002		.002

Notes: Data source is described in notes to the figures, and specification details are described in notes to Table 1. The table reports estimates of Eq. (2). Regression is weighted by average state population through the sample period. Standard errors are clustered by state. The two IVs used are total number of births for 50- to 59-year-olds divided by the total number of births for 16- to 24-year-olds by state and year and the total number of births for 50- to 59-year-olds divided by the total number of births for the 25- to 49-year-olds by state and year. We exclude certain years and states with missing birth data for the cohorts because only a handful of states started reporting births in 1915. $N = 1,326$. (Note that we could use more observations in this table than in Table 6 for 60- to 69-year-olds because the absence of early birth data is less of a constraint. However, we keep the samples the same in the two tables to make the estimates most comparable).

†$p < .10$; *$p < .05$; **$p < .01$

Table 6 OLS and IV regressions of log labor force participation rate and hourly wages of 60- to 69-year-olds on log relative cohort sizes for 16- to 24-year-olds and 25- to 49-year-olds, 1984–2016

	LFP		Hourly Wages	
	OLS	IV	OLS	IV
Both Sexes				
ln(cohort size aged 60–69 / cohort size aged 16–24)	−0.120*	−0.459*	0.024	−0.239†
	[0.047]	[0.188]	[0.039]	[0.135]
ln(cohort size aged 60–69 / cohort size aged 25–49)	0.077	0.597**	−0.010	−0.074
	[0.055]	[0.192]	[0.058]	[0.174]
First-stage *F* statistic				
Dependent variable = ln(cohort size aged 60–69 / cohort size aged 16–24)		10.51		10.51
Dependent variable = ln(cohort size aged 60–69 / cohort size aged 25–49)		9.72		9.72
Durbin-Wu-Hausman test for endogeneity (*p* value)		.006		.083
Men				
ln(cohort size aged 60–69 / cohort size aged 16–24)	−0.073	−0.480**	0.034	−0.299
	[0.047]	[0.163]	[0.043]	[0.183]
ln(cohort size aged 60–69 / cohort size aged 25–49)	0.031	0.464*	0.007	−0.066
	[0.054]	[0.187]	[0.061]	[0.195]
First-stage *F* statistic				
Dependent variable = ln(cohort size aged 60–69 / cohort size aged 16–24)		9.20		9.20
Dependent variable = ln(cohort size aged 60–69 / cohort size aged 25–49)		14.20		14.20
Durbin-Wu-Hausman test for endogeneity (*p* value)		.013		.101
Women				
ln(cohort size aged 60–69 / cohort size aged 16–24)	−0.104*	−0.444	−0.045	−0.242
	[0.049]	[0.271]	[0.041]	[0.149]
ln(cohort size aged 60–69 / cohort size aged 25–49)	0.124*	0.770**	0.052	−0.056
	[0.058]	[0.252]	[0.057]	[0.180]
First-stage *F* statistic				
Dependent variable = ln(cohort size aged 60–69 / cohort size aged 16–24)		8.43		8.43
Dependent variable = ln(cohort size aged 60–69 / cohort size aged 25–49)		4.85		4.85
Durbin-Wu-Hausman test for endogeneity (*p* value)		.002		.129

Notes: Data source is described in notes to the figures, and specification details are described in notes to Table 1. The table reports estimates of Eq. (2). Regression is weighted by average state population through the sample period. Standard errors are clustered by state. The two IVs used are the total number of births for 60- to 69-year-olds divided by the total number of births for 16- to 24-year-olds by state and year and the total number of births for 60- to 69-year-olds divided by the total number of births for 25- to 49-year-olds by state and year. We exclude certain years and states with missing birth data for the cohorts because only a handful of states started reporting births in 1915. $N = 1{,}326$.

†$p < .10$; *$p < .05$; **$p < .01$

are often fairly large in both tables, although in some cases they do not exceed 10.[25] The IV results are qualitatively similar for all three samples: pooled, men only, and women only.

Looking first at the 50- to 59-year-old cohort, the IV estimates for LFP indicate a weak negative effect or no effect of the size of the 50- to 59-year-old cohort relative to the youngest cohort (aged 16–24), with elasticities ranging from −0.011 to −0.103. However, for the size of the older cohort relative to the prime-aged cohort (aged 25–49), the estimated effect is larger and positive in all three cases and is statistically significant in two of them; the elasticities range from 0.19 to 0.39. For wages, the effect of the size of the 50- to 59-year-old cohort relative to the youngest cohort is negative but not statistically significant, with elasticities ranging from −0.11 to −0.18. The estimated effect of the size of the older cohort relative to the prime-aged cohort is more strongly negative and is statistically significant in all cases, with elasticities ranging from −0.29 to −0.34.

Table 6 presents similar estimates for the oldest cohort of 60- to 69-year-olds. In the IV estimates, the sign pattern is identical to that for 50- to 59-year-olds. For LFP, the results are stronger statistically. The size of the 60- to 69-year-old cohort relative to the 16- to 24-year-old cohort has a large and statistically significant negative effect, with elasticities ranging from −0.44 to −0.48. And the size of the 60- to 69-year-old cohort relative to the 25- to 49-year-old cohort has a large and statistically significant positive effect, with elasticities ranging from 0.46 to 0.77. For wages, only the estimated effect of cohort size relative to 16- to 24-year-olds for both sexes combined is statistically significant (at the 10% level); the elasticities range from −0.24 to −0.30.

Interestingly, then, when we look at the size of the two older cohorts (aged 50–59 and 60–69) relative to the youngest cohort (aged 16–24), the evidence is essentially fully consistent with the relative supply effect of a larger cohort, with negative effects on both LFP and wages. These results are summarized for men and women combined in Table 7. In contrast, when we look at the size of the older cohorts relative to the prime-aged cohort (aged 25–49), we find relatively little statistical evidence for the relative supply effect of a larger older cohort. In particular, the LFP effect is positive for both older cohorts (in Tables 5 and 6), and the wage effect is not significant and fairly close to 0 for 60- to 69-year-olds in Table 6. The exception is for 50- to 59-year-olds in Table 5, where the wage effects are negative; but these results are in the opposite direction of the LFP effects.

The striking finding here, in our view, is that when we break up the cohorts to which we compare the size of the older cohort, we find far less contradictory evidence of the effects of a larger older cohort. Table 7 helps illustrate this point in one table. When simply looking at the relative size of the older cohorts (in Tables 2 and 4), using the simpler specification in Eq. (1), we find that the LFP effects of large older cohorts point to a positive demand shift toward older workers when the older cohort is larger, which is inconsistent with the relative supply hypothesis. The wage effects, in contrast, point to a negative relative supply effect.

In contrast, when we break up the cohorts to which we compare the size of the older cohort, using Eq. (2), all the evidence for the size of older cohorts relative to the youngest cohort fits the relative supply hypothesis. In contrast, almost none of the evidence for the size of the older cohorts relative to the 25- to 49-year-old cohort fits this hypothesis, and none of the evidence for LFP does. We highlight these results in Table 7.

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[25] With multiple instruments, the preferred diagnostic is Shea's partial R^2, although these measures do not have a clear interpretation. And the Stock and Yogo (2005) critical values for minimum eigenvalues do not apply beyond homoskedastic independent, identically distributed errors.

Table 7 Summary of evidence on relationship between relative cohort size and labor force participation rates and wages, IV estimates

Type of Evidence	Ages 16–24	Ages 25–49	Ages 50–59	Ages 60–69
Labor Force Participation				
Table 2, Eq. (1)				
Men and women	−	−*	+*	+†
Consistent with supply effect of relative cohort size?	Yes	Yes	No	No
Relative to two younger cohorts (Tables 5 and 6), Eq. (2)				
Men and women aged 16–24			−	−**
Consistent with supply effect of relative cohort size?			**Yes**	**Yes**
Men and women aged 25–49			+**	+**
Consistent with supply effect of relative cohort size?			**No**	**No**
Wages				
Table 4, Eq. (1)				
Men and women	+*	+*	−**	−
Consistent with supply effect of relative cohort size?	No	No	**Yes**	**Yes**
Relative to two younger cohorts (Tables 5 and 6), Eq. (2)				
Men and women aged 16–24			−	−†
Consistent with supply effect of relative cohort size?			**Yes**	**Yes**
Men and women aged 25–49			−**	−
Consistent with supply effect of relative cohort size?			**Yes**	**No**

Note: See notes to Tables 2 and 4–6.

†$p < .10$; *$p < .05$; **$p < .01$

How do we interpret the findings? The evidence of large negative effects on both LFP and wages for older workers aged 60–69 when this cohort is large relative to the youngest cohort indicates that the oldest and the youngest workers are not very substitutable. Rather, a large older cohort of 60- to 69-year-olds relative to 16- to 24-year-olds creates traditional, supply-side cohort crowding effects for older workers. This finding suggests that the effects are not driven by whether older workers taking post-retirement jobs move into jobs otherwise held by young people.

The results for the size of the 60- to 69-year-old cohort relative to the prime-aged cohort (aged 25–49), however, are more consistent with a relative demand shift. We find a strong positive effect on LFP, suggesting that when the older cohort is large relative to the prime-aged cohort, demand for older workers is strong. When prime-aged workers are relatively scarce, firms may try to retain older workers. It is true that we do not find a corresponding positive wage effect for the older cohort; the estimates (in Table 6) are not significantly different from 0, and they are small, although they are negative rather than positive.

Although we cannot explain negative estimates via the demand side, if older workers' labor supply on the extensive margin is quite elastic, that could militate against finding a positive wage effect. And it is possible that the absence of wage effects or even negative effects, despite a positive demand shift, could arise from older workers entering into different kinds of employment relationships with their prior employers or new employers that are more flexible and pay less and in which they work fewer hours,[26] or from negative selection on wages of those who remain employed at older ages.

For 50- to 59-year-olds, the evidence for the effects of cohort size relative to the size of the youngest cohort (aged 16–24) is also no longer contradictory, because the estimated effects on LFP are negative (although not significant). The negative estimates are consistent with the conventional relative cohort size effect, like we find for 60- to 69-year-olds relative to 16- to 24-year-olds (although the evidence was much stronger in this case). Only for the estimates for 50- to 59-year-olds relative to 25- to 49-year-olds does a contradiction remain, given that we find positive estimates of the relative size of the older cohort on LFP but negative and significant estimates on wages. However, the positive effects on LFP are the same as for 60- to 69-year-olds, although the magnitudes are smaller for 50- to 59-year-olds.

Thus, the disaggregation of the younger cohorts largely resolves most of the contradictory evidence we find when lumping all "non-old" cohorts together. We find strong evidence, when compared with the size of younger cohorts, of traditional cohort crowding for workers aged 60–69. When compared with the size of prime-aged cohorts, we find more evidence that large relative size of the oldest cohort is associated with a shift in demand toward older workers, although we cannot fully explain both the LFP and wage effects for the effects of the size of older relative to prime-aged cohorts in a simple demand and supply framework.

What Do Workers in Older Cohorts Do When Their Younger Cohorts Are Smaller?

The evidence from Tables 5 and 6 suggests that when the older cohorts of 50- to 59-year-olds or 60- to 69-year-olds are large relative to the 25- to 49-year-old cohort, LFP of the older cohorts is higher. This is consistent with an increase in demand for members of the older cohorts. Yet wages do not rise—partly because, we suspect, the older workers induced to participate in the labor force when the younger prime-age cohort is smaller are entering into different kinds of employment relationships, possibly with lower pay. In this subsection, we present some evidence on this conjecture.

Panel A of Table 8 reports IV estimates of specifications similar to those in Tables 5 and 6, except that we estimate models for the share of the labor force working part-time or self-employed. If the LFP response among the older cohorts occurs via different kinds of employment relationships, then we might expect the shares working part-time or self-employed to increase. Moreover, a self-employment response of this nature would be more likely to be for an unincorporated self-employed business, such as someone taking on a consulting role for a former employer. Hence, we also report specifications for the

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[26] For example, Johnson et al. (2009: Table 1) reported that among workers aged 51–55 in 1992, as of 2006, 14.2% remained at the same employer, 15.7% changed employer and stayed in the same occupation, and 26.9% changed employer and occupation. (The remainder were not employed.) Average wages are considerably lower on the new job (see Johnson's Table 17), which is typically less physically demanding, especially for those for changed occupations (see Johnson's Table 2).

Table 8 IV regressions of part-time work and self-employment of older cohorts, and OLS regressions of log average hourly wages on log part-time and self-employed workforce shares, 1984–2016

	50- to 59-Year-Olds				60- to 69-Year-Olds			
	Part-Time	Self-employed	Self-employed, Incorporated	Self-employed, Unincorporated	Part-Time	Self-employed	Self-employed, Incorporated	Self-employed, Unincorporated
A. IV Regressions for Log Shares of Labor Force, Both Sexes								
ln(older cohort size / cohort size aged 16–24)	0.306	0.554†	−1.408*	1.194*	0.062	0.037	0.125	−0.005
	[0.363]	[0.308]	[0.644]	[0.510]	[0.219]	[0.210]	[0.362]	[0.266]
ln(older cohort size / cohort size aged 25–49)	0.371	0.511	−0.513	1.084*	0.614**	0.115	0.877	−0.030
	[0.251]	[0.323]	[0.465]	[0.528]	[0.232]	[0.291]	[0.676]	[0.329]
B. OLS Regressions for Log Average Hourly Wages, Both Sexes								
Regression for part-time and for self-employed	−0.056**	0.004			−0.124**	−0.008		
	[0.013]	[0.012]			[0.023]	[0.014]		
Regression for part-time, self-employed incorporated, and self-employed unincorporated	−0.055**		0.007	−0.000	−0.123**		0.007	−0.016
	[0.012]		[0.007]	[0.010]	[0.023]		[0.009]	[0.009]

Notes: For panel A, notes from Tables 5 and 6 apply. Each column is a separate specification. Part-time and self-employed come from different questions and are not mutually exclusive. Part-time and self-employment status are based on current employment only. In panel B, each row reports results from two separate regressions for log average hourly wages, for 50- to 59-year-olds or 60- to 69-year-olds.

†$p < .10$; *$p < .05$; **$p < .01$

shares of the labor force in self-employment broken down by incorporation status. Aside from that, the approach is exactly as in Tables 5 and 6, with the same first stage, and so on.

The estimates indicate that the margin of response for 50- to 59-year-olds to a smaller relative 25- to 49-year-old cohort is an increase in the share of the labor force working as self-employed. This effect is statistically significant (and is larger) only for the unincorporated self-employed, as hypothesized.[27] For 60- to 69-year-olds, panel A of Table 8 indicates that the margin of response to a smaller 25- to 49-year-old cohort is an increase in the share of the labor force working part-time. Together, this evidence is consistent with older cohorts participating in the labor force at a higher rate, when their cohorts are large relative to 25- to 49-year-olds, in employment relationships that differ from common full-time wage and salary arrangements. That is what we might expect given that the increase in LFP when old cohorts are relatively large comes from those less attached to the labor force (and hence not participating when the relative size of older cohorts is not large).

Do these participation responses of older cohorts also explain the absence of positive wage effects (or even negative wage effects for 50- to 59-year-olds) in response to large cohorts of older workers relative to 25- to 49-year-olds? To explore this, panel B of Table 8 simply reports regressions of our log average hourly wage measure on the shares of older workers in these alternative work arrangements. The evidence suggests that part-time work is associated with lower wages, although self-employment is not. Thus, these wage results provide a partial explanation for why the increase in LFP of older cohorts, when they are large relative to the 25- to 49-year-old cohort, is not accompanied by higher wages, as we would expect from a pure labor demand story. The explanation works for 60- to 69-year-olds—for whom the response occurs in part-time work—but not for 50- to 59-year-olds.[28]

Conclusions

Our study is motivated by the question of how employment (or LFP) and wages of older individuals are likely to change as the U.S. population ages, with a rising share of the population in the age ranges in which most people are retired. Couched in terms of the prior literature, this question concerns the effect of "cohort crowding" for older workers. When there is a relatively large cohort of older individuals, do we find that wages and LFP are lower because of the relative supply effect? Such evidence would be consistent with other research on younger workers. Or do we find different effects, perhaps because the age composition of the population affects the age composition of consumption and hence labor demand, or because a large relative cohort of older workers implies a small relative cohort of younger workers, which can itself affect demand for older workers?

We explore these effects of relative cohort size, taking account of the potential endogeneity of population structure owing to both work-related and retirement-related migration. We use as IVs relative cohort size measures predicted by historical data on births

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[27] We also find a smaller positive effect, significant at the 10% level, in response to a smaller 16- to 24-year-old cohort, although Table 5 does not point to an increase in LFP for 50- to 59-year-olds when their cohort is large relative to 16- to 24-year-olds.

[28] We also estimated all models including two controls for the percentage of observations in either the leading (1946–1955) or trailing (1956–1964) edges of the Baby Boom (see Colby and Ortman 2014). This had virtually no effect on the results (available upon request).

in each state, by year. In general, we find evidence consistent with the kinds of biases we would expect from these two types of migration, and hence we emphasize the IV results.

When we study the effects of a large relative older cohort (aged 50–59 or 60–69) relative to the working-age population as a whole, we find contradictory evidence. For LFP, we find evidence that is inconsistent with the relative supply or cohort crowding hypothesis and that instead suggests an increase in demand for older workers when the older cohort is relatively large (with higher LFP). But we find negative wage effects, consistent with the relative supply hypothesis.

However, when we look at the size of the older cohorts relative to a young cohort (aged 16–24) and a cohort spanning the prime/middle range of ages (aged 25–49), we find a more coherent set of results. When the older cohort is large relative to the younger cohort, the evidence is much more consistent with the relative supply hypothesis, with a larger relative older cohort reducing LFP and wages. But when the older cohorts are large relative to the cohort of 25- to 49-year-olds, LFP of older workers is higher, and it is less clear that wages are affected.

These results for the size of older cohorts relative to prime-aged cohorts are more consistent with a relative demand shift. When prime-aged workers are scarce relative to older workers, firms may try to retain or hire older workers. Older workers' extensive margin labor supply elasticity may be quite high. Moreover, older workers often enter into different jobs or employment relationships with more flexible, lower-paying work. Data on part-time work and self-employment provide some evidence that the increase in older workers' LFP when their cohort is large relative to the 25- to 49-year-old cohort comes via self-employment or part-time work. Moreover, for 60- to 69-year-olds, this may help explain why average wages do not rise despite the increase in LFP; for 50- to 59-year-olds, in contrast, there remains more of a contradiction between higher LFP but lower wages when their cohort is large relative to 25- to 49-year-olds.

Together, the results suggest that cohort size may have important implications for the LFP (and wages) of older workers. However, our evidence suggests that we need a more nuanced view than simply whether the older cohort is large relative to the population: the cohort that they are large *relative to* matters. Our evidence also suggests the value of additional work to understand the behavior underlying our findings, both to better understand the labor market decisions of older workers and to assess the validity of the interpretation of the results we find in our study.

Nonetheless, as it stands, the pattern of projected population aging is most consistent with rising shares of 50- to 59-year-olds and 60- to 69-year-olds relative to the broad group in their 20s, 30s, and 40s, rather than an increase relative to a particularly small young cohort (U.S. Census Bureau n.d.). As such, our results suggest that population aging is likely to be accompanied by rising LFP and hence employment of older individuals.

That said, our analysis has some potential limitations, which remain for future research. We motivate our analysis by asking what changes in the age structure of the population imply, first and foremost for the LFP of older individuals. Our estimates based on the IV we use are informative for this interpretation—an interpretation that can also be thought of as extrapolating from our estimates to project the likely effects of population aging in the aggregate data, which can be viewed as exogenous. Our estimates do not, however, disentangle or decompose the labor supply and demand responses of those at different ages to population aging. Moreover, there is potentially a rich set of life cycle responses in which large cohorts who are now older engaged in

when they were younger, such as increased educational investments owing to lower wages (e.g., Berger 1984), leading to higher employment at older ages to recoup the earlier educational investments. That is, there is a potentially rich and interesting "black box" of behavioral responses to changes in age structure that we do not explore, although doing so goes well beyond the purpose of this article.

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