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ABSTRACT

Essays in Labor Economics

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This dissertation contains three chapters on two broad topics in labor economics: the determinants of early career outcomes and the impact of an aging population (and related policies). The first chapter investigates how the retirement slowdown among older Americans has affected the labor market prospects of younger Americans in recent decades. Using an instrumental variables approach exploiting plausibly exogenous variation in the age composition of the old across U.S. commuting zones, I find that the retirement slowdown has had a negative impact on the composition of jobs among the young. In commuting zones where fewer older workers retire due to the initial age structure, youth employment in high-skill occupations declines while youth employment in low-skill occupations increases. The estimates imply that the retirement slowdown can account for up to 60 percent of the rise of youth employment in low-skill jobs between 1990 and 2007. This pattern of occupational downgrading is consistent with a model of the labor market featuring occupational choice, and the fact that older workers are increasingly concentrated in high-skill jobs. I also find evidence

of declining youth wages and a shift towards part-time employment among the young. Together, the results suggest that retirement trends have contributed to stagnant youth labor market prospects in recent years.

The second chapter, joint work with Daniel Fetter and Lee Lockwood, explores the relationship between government old-age support and transfers within the family by investigating the Old Age Assistance Program (OAA), a means-tested and state-administered pension program created by the Social Security Act of 1935. Using Census data on the entire U.S. population in 1930 and 1940, we exploit large differences in the generosity of OAA programs across state borders to estimate the effects of OAA on intergenerational living arrangements. Our results suggest that OAA reduced intergenerational co-residence among both elderly men and elderly women, enough to explain most or all of the aggregate decline between 1930 and 1940, and lay the foundation for future work using linked Census samples to investigate the impact of OAA on recipients' children and their families.

The third chapter, joint work with Enrico Berkes and Bledi Taska, investigates how initial skill-specific labor market conditions affect early career outcomes of college graduates. Using data on the near-universe of online job postings in the U.S. between 2010 and 2016, we build a new measure of skill mismatch which captures how well an individual's college major matches the occupational composition of local labor demand around the time of graduation. Intuitively, a college graduate experiences skill mismatch when only a small fraction of online job postings in her city are suitable for her major in the year she graduates. Exploiting variation in skill mismatch across majors, cities and graduation cohorts, we find that a one standard deviation increase in our measure leads to a 3 percent decline in initial wages. Skill mismatch is also associated with a greater probability of being initially unemployed or employed in a part-time job, as well as a lower probability of being employed in a college occupation or one of the top occupations by college major. While the effects on unemployment, part-time employment and employment in college occupations gradually fade over time, the effects on wages and major-occupation fit persist up to 6 years after graduation. Our findings highlight the importance of having the right skills in the right place at the right time.

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CHAPTER 1

The Impact of the Retirement Slowdown on the U.S. Youth Labor Market[1]

1.1. Introduction

One of the most striking developments in the U.S. labor market in recent decades has been the sharp rise in the labor supply of older Americans. As Figure 1.1 shows, the share of Americans aged 55+ that are employed has increased from 30 to 40 percent since the mid-1990s, mirroring a gradual decline in the 55+ retirement rate. The slowdown in retirements is not fully understood, but is generally attributed to a combination of greater financial incentives to work longer—due to changes in Social Security, a transition from defined-benefit to defined-contribution pension plans in the private sector, and rising life expectancy—as well as a greater capacity to work longer, thanks to a shift away from physically demanding jobs and improvements in late-life health (Friedberg, 2007; Quinn, 2010). An important but lesser-known aspect of recent retirement trends is that older workers are increasingly concentrated in high-skill jobs, which is closely related to changes in the educational composition of older workers. Indeed, not only have the high-educated expanded their labor supply by the largest amount, but the average educational attainment of older generations has

[1]This research was supported in part through the computational resources and staff contributions provided for the Quest high performance computing facility at Northwestern University. Part of the analysis is based on Statistics Canada 2006 Census Public Use Microdata Files. All computations, use and interpretation of these data are entirely my own.

steadily grown over time, as emphasized by Burtless (2013) and Goldin and Katz (2016).

At the same time, there is mounting evidence that the youth labor market has deteriorated since the 2000s. Beaudry et al. (2014) show that cohorts of college graduates who entered the labor market after 2000 had a lower probability of being employed in cognitive occupations along with flatter wage profiles, and argue that this reflects a decline in the demand for cognitive skills. Consistent with these findings, Abel et al. (2014) document how young college graduates are increasingly employed in low-paying jobs which do not require a college degree. Figure 1.2 illustrates the contrast between the fortunes of the young and old by plotting changes in employment by major occupation group between 1980 and 2007. While younger workers are increasingly concentrated in low-skill occupations (e.g. food preparation, personal services), older workers have enjoyed significant gains in high-skill occupations (e.g. professionals, managers). Another way to see this divergence is Figure 1.3, which displays the evolution of hourly wages since 1980.

Motivated by these facts, this paper investigates to what extent the rise in the labor supply of older Americans has affected the job prospects of the young in recent decades. To answer this question, I use a local labor market approach and compare the evolution of youth employment outcomes across U.S. commuting zones, which have experienced differential changes in the 55+ employment rate over the period 1980-2007. I primarily focus on outcomes of young adults aged 22 to 30, who are at the early stages of their career. A fundamental endogeneity problem arises due to the fact that changes in the employment of the old, which capture both hires/separations and retirements, not only reflect labor supply-side variation but also labor demand-side variation. That is, changes in unobservable local labor market conditions tend to push outcomes of all workers in the same direction, which leads to a mechanical relationship between changes in the 55+ employment rate and changes in youth employment outcomes.

To address this empirical challenge, I adopt an instrumental variables approach which exploits plausibly exogenous variation in the age composition of the old across commuting zones to predict retirement trends. Specifically, I construct an instrument by interacting the initial commuting zone-level 45+ age distribution with 10-year national retirement rates by age. The predictive power of this instrument derives from the fact that the propensity to retire varies over the life cycle. In the U.S., older workers tend to retire in their 60s, with a disproportionate number of people retiring at specific ages associated with eligibility for Social Security or Medicare (Gustman and Steinmeier, 2005). Assuming that the initial age composition among the old is orthogonal to the subsequent evolution of local labor demand conditions, IV estimates will identify the causal effect of retirement trends on youth employment outcomes.

The empirical results can be summarized as follows. In commuting zones where fewer older workers retire due to the initial age structure, youth employment in high-skill occupations declines while youth employment in low-skill occupations rises. A one percentage point increase in the 55+ employment rate is associated with a 0.7 percentage point decrease in youth employment in high-skill occupations and a 0.5 percentage point increase in youth employment in low-skill occupations, as a share of the youth population. This pattern of occupational downgrading also manifests itself through an increase in the share of younger workers that are “overeducated” for their job. Moreover, in commuting zones where fewer older workers retire, youth wages experience a significant decline and youth employment shifts from full-time to part-time jobs. Finally, I document evidence of greater school attendance and net out-migration among the young, both of which have been found to be important adjustment mechanisms in the face of declining labor market prospects.

To rationalize the key occupational patterns, I present a model of the labor market featuring occupational choice. The model allows for endogenous choices of both firms and workers and provides comparative statics for an increase in the labor supply of the old concentrated in high-skill jobs, mimicking recent retirement trends observed in the United States. On the labor demand side, production involves capital and two types of labor: low-skill and high-skill. On the labor supply side, workers differ in terms of age (young or old) and education (low-educated or high-educated). The model allows for imperfect substitutability between low-skill and high-skill labor, and between younger and older workers within skill types. It also allows for occupational choice: while low-educated workers are confined to low-skill jobs, high-educated workers can choose whether to supply their labor in low-skill or high-skill jobs depending on their individual comparative advantage and relative wages. I show that an increase in the labor supply of the old concentrated in high-skill jobs leads to a decline in youth high-skill wages relative to youth low-skill wages. In turn, this change in relative wages induces a self-selection response among the high-educated young: marginal-ability individuals reallocate towards low-skill jobs until equilibrium is restored. These comparative statics hinge on the assumption that the young and old are closer substitutes within skill types than low-skill and high-skill labor, which is supported by existing empirical evidence (Card and Lemieux, 2001).

This paper makes three contributions. First, using a novel empirical strategy, I shed new light on how changes in the labor supply of older workers affect youth employment outcomes in the United States. The existing literature on this question can be split into two strands. Studies that use aggregate or state-level data have found little evidence of a negative relationship between the employment rates of the old and young (Gruber and Wise, 2010; Munnell and Wu, 2012). In contrast, studies that use firm-level data and exploit specific retirement reforms in European countries provide compelling evidence that delayed retirements have a negative effect on youth hiring (Martins et al., 2009; Boeri et al., 2016; Bovini and Paradisi, 2017). For example, Boeri et al. (2016) estimate the effect of a 2011 pension reform in Italy, which raised the retirement age by up to five years for some categories of workers. They find that, for every five additional older workers staying on the job in response to the policy change, firms hired one fewer younger worker.

On the one hand, a firm-level perspective allows for more credible research designs. On the other hand, firm-level studies cannot capture what happens to the labor market as a whole and time horizons are often shorter. My paper strikes a balance between the two current approaches by using local labor markets as the unit of analysis and isolating variation in retirement trends due to age composition. Another key distinction between my paper and existing studies is the emphasis on the skill dimension. Considering the skill-biased nature of retirement trends is essential to understanding their consequences, much in the same way that low-skill immigration and high-skill immigration have very different implications. Correspondingly, the effects on younger workers may show up more clearly in the types of jobs that they hold rather than employment levels.

Second, this paper contributes to our understanding of how local labor markets adjust to skill-specific labor supply shocks. Past studies have examined the entry of women into the labor force (Acemoglu et al., 2004) and immigration (Card, 2001; Peri et al., 2015). In contrast, I study the effect of an increase in the supply of predominantly high-educated, experienced workers who have already climbed the job ladder. Another notable difference relative to immigration shocks is that older workers do not constitute an influx of new consumers into the local economy.

Finally, while the recent woes of young Americans have been well-documented, much less is known about the underlying causes. One exception is Beaudry et al. (2016), who argue that the decline in the demand for cognitive skills is related to trends in technology. They posit that IT technologies reached maturity around 2000, which subsequently reduced the demand for high-skill workers. My findings suggest the retirement slowdown among older Americans as an alternative but complementary hypothesis. In particular, the estimates imply that it can explain up to 60 percent of the rise of youth employment in low-skill occupations between 1990 and 2007.

The remainder of the chapter is organized as follows. In Section 1.2, I begin by presenting the conceptual framework. Section 1.3 describes the empirical strategy, including potential sources of endogeneity and the instrumental variables approach. In

Section 1.4, I describe the data before moving on to the empirical analysis in Section 1.5. Section 1.6 contains a broader discussion of the results. Section 1.7 concludes.

1.2. A Model of the Labor Market with Occupational Choice

To guide the interpretation of the empirical results, it is useful to go over some theoretical ideas. A natural framework to explore the economics of labor supply shocks is the so-called "canonical" labor demand model (Acemoglu and Autor, 2011). Despite being highly stylized, this framework delivers basic intuition on how supply and demand forces interact with production technology—in the form of complementarity/substitutability between inputs—to produce equilibrium wages and employment.

In this section, I present a version of the canonical labor demand model in which production involves capital and two types of labor, low-skill and high-skill. On the labor supply side, workers differ in terms of age (young or old) and education (low-educated or high-educated). Importantly, there are complementarities in production between low-skill and high-skill labor, and between the young and old within skill types. In a slight departure from the standard setup, I incorporate an element of occupational choice. Whereas low-educated workers are confined to low-skill occupations, high-educated workers can choose whether to supply their labor in low-skill or high-skill occupations depending on their ability to perform these jobs and the wages that they offer. Self-selection based on comparative advantage à la Roy (1951) is a common feature in models of automation (e.g. Autor et al., 2003).

In the context of the model, one can interpret the recent retirement slowdown observed in the U.S. as an increase in the supply of older workers concentrated in high-skill jobs. The goal is to understand how equilibrium wages and occupational composition of younger workers change in response to a labor supply shock of this nature. Assuming younger and older workers are more substitutable than low-skill and high-skill labor within skill types, as implied by the findings in Card and Lemieux (2001), I will show that high-skill youth wages must fall relative to low-skill youth wages to compensate firms for the change in marginal productivities. In turn, the change in relative wages prompts marginal-ability high-educated younger workers to reallocate from high-skill jobs towards low-skill jobs until equilibrium is restored. This central prediction will provide a rationale for the results on occupational composition presented in the empirical analysis.

Firm Production. Consider a representative firm combining capital K and labor L according to a Cobb-Douglas production function to produce a homogenous good Q:

$$Q = AL^{1-\alpha}K^{\alpha} \tag{1.1}$$

where $\alpha \in (0,1)$ is the output elasticity of capital and A represents total factor productivity. Labor can be decomposed into two types, low-skill (L_L) and high-skill (L_H), which I will also refer to as occupations or jobs throughout this section. These occupations can be performed by two types of workers, young (L_{Ly}, L_{Hy}) or old (L_{Lo}, L_{Ho}). The different labor inputs are aggregated according to a nested constant elasticity of substitution (CES) structure, similar to Card and Lemieux (2001):

$$L = [\theta_L L_L^{\beta} + \theta_H L_H^{\beta}]^{1/\beta} \tag{1.2}$$

$$L_g = [\theta_{gy} L_{gy}^{\gamma} + \theta_{go} L_{go}^{\gamma}]^{1/\gamma} \quad g \in \{L, H\} \tag{1.3}$$

where $\theta_L + \theta_H = 1$ and $\theta_{gy} + \theta_{go} = 1$ are relative labor productivities. The key parameters of the model are $\beta \leqslant 1$ and $\gamma \leqslant 1$, which respectively capture the degree of substitution between low-skill and high-skill labor, and the degree of substitution between younger and older workers within skill types. Higher values of β and γ imply greater substitutability between inputs. The firm optimally chooses labor inputs and capital to maximize profits, taking the output price p, wages w_{gk} and the rental rate of capital r as given (i.e. competitive input and output markets):

$$\max_{(K, L_{gk})} AL^{1-\alpha} K^{\alpha} - rK - \sum_{g \in \{L,H\}} \sum_{k \in \{y,o\}} w_{gk} L_{gk} \tag{1.4}$$

where the output price has been normalized to 1.

Occupational Choice and Capital Supply. On the labor supply side, younger and older workers are either low-educated or high-educated. The distinction between skill types and education types implies that the mapping between them is not one-to-one. In particular, I assume that high-educated workers can perform both low-skill and high-skill occupations, whereas low-educated workers are confined to low-skill occupations. Moreover, high-educated workers are endowed with heterogeneous abilities to perform high-skill occupations relative to low-skill occupations. Let u and z respectively denote the ability parameters of younger and older workers (interpreted in terms of efficiency units), distributed according to the cumulative distribution functions $\Gamma(u)$ and $\Lambda(z)$. A younger worker with ability u can earn $w_{Hy} \cdot u$ in high-skill occupations or w_{Ly} in low-skill occupations (efficiency units in low-skill occupations are implicitly normalized to 1). Similarly, an older worker with ability z can earn $w_{Ho} \cdot z$ or w_{Lo}. High-educated workers optimally sort into low-skill and high-skill jobs according to the thresholds u^* and z^* defined by the following indifference conditions:

$$w_{Ly} = u^* \cdot w_{Hy} \tag{1.5}$$

$$w_{Lo} = z^* \cdot w_{Ho} \tag{1.6}$$

As a result, a fraction $\Gamma(u^*)$ of high-educated younger workers and a fraction $\Lambda(z^*)$ of high-educated older workers supply their labor in low-skill jobs, with the remaining fractions $1 - \Gamma(u^*)$ and $1 - \Lambda(z^*)$ supplying their labor in high-skill jobs.

Denote the labor supply of young low-educated and high-educated workers by L_y^ℓ and L_y^h respectively, and analogously (L_o^ℓ, L_o^h) for older workers. I make two simplifying assumptions: (1) low-educated and high-educated workers are perfect substitutes in low-skill jobs, and (2) all workers supply their labor inelastically. This yields the following labor supply equations:

$$L_{Ly} = L_y^{\ell} + \Gamma(u^*) \cdot L_y^h \tag{1.7}$$

$$L_{Hy} = \left\{ \int_{u^*}^{u^{\max}} u \cdot \Gamma'(u) \cdot du \right\} \cdot L_y^h \tag{1.8}$$

$$L_{Lo} = L_o^{\ell} + \Lambda(z^*) \cdot L_o^h \tag{1.9}$$

$$L_{Ho} = \left\{ \int_{z^*}^{z^{\max}} z \cdot \Lambda'(z) \cdot dz \right\} \cdot L_o^h \tag{1.10}$$

The inelastic labor supply assumption implies that the only labor supply response to a change in wages is self-selection among high-educated workers, which is embedded in the cutoffs u^* and z^*. To close the model, I follow Dustmann et al. (2017) and assume that capital is supplied according to $r = K^{\lambda}$ where $\lambda \geqslant 0$ captures the elasticity of capital. Small values of λ imply that capital is very elastic.

Comparative Statics. Now that all the elements of the model are in place, we can turn to comparative statics. Following recent retirement trends in the U.S., consider an increase in the labor supply of older workers concentrated in high-skill jobs. In the context of the model, this is equivalent to assuming the *growth* in the labor supply of low-educated and high-educated older workers $(d \log L_o^{\ell}, d \log L_o^h)$ satisfies the following condition:[2]

$$s_{Ho} \cdot d \log L_o^h > s_{Lo} \cdot (s_o^h \cdot d \log L_o^h + s_o^{\ell} \cdot d \log L_o^{\ell}) \tag{1.11}$$

[2]It turns out that solving the comparative statics in log changes in analytically convenient.

The left-hand side of (1.11) is the *effective* labor supply increase of older workers in high-skill jobs, expressed as the product of the initial share of older workers in high-skill jobs $s_{Ho} \equiv (\theta_{Ho} L_{Ho}^{\gamma})^{1/\gamma} / L_H$ and the growth in the labor supply of high-educated older workers. Similarly, the right-hand side of (1.11) is the effective labor supply increase of older workers in low-skill jobs, which is the weighted average of the growth in the labor supply of high-educated and low-educated older workers ($s_o^h + s_o^\ell = 1$ is the initial mix of education types in low-skill jobs), scaled by the initial share of older workers in low-skill jobs $s_{Lo} \equiv (\theta_{Lo} L_{Lo}^{\gamma})^{1/\gamma} / L_L$. Assuming without loss of generality that $d \log L_o^\ell = \delta \cdot d \log L_o^h$, condition (1.11) can be restated more simply as $s_{Ho} > s_{Lo} \widetilde{\delta}$ where $\widetilde{\delta} = (s_o^h + s_o^\ell \delta)$.

How do equilibrium wages (w_{Ly}, w_{Hy}) and occupational composition (L_{Ly}, L_{Hy}) of younger workers change in response a labor supply shock of this nature? To understand what drives changes in the wages of younger workers, consider the totally differentiated first-order conditions for L_{Hy} and L_{Ly} in (1.4), where the first-order condition for K has already been combined with the capital supply equation and substituted in (see Appendix A.6 for details):

(1.12)

$$d \log w_{Hy} = \overbrace{\varphi \cdot d \log L}^{<0} + \overbrace{(\beta - 1) \cdot (d \log L_H - d \log L)}^{<0} + \overbrace{(\gamma - 1) \cdot (d \log L_{Hy} - d \log L_H)}^{\gg 0}$$

(1.13)

$$d \log w_{Ly} = \underbrace{\varphi \cdot d \log L}_{<0} + \underbrace{(\beta - 1) \cdot (d \log L_L - d \log L)}_{>0} + \underbrace{(\gamma - 1) \cdot (d \log L_{Ly} - d \log L_L)}_{>0}$$

where $\varphi = -\alpha\lambda/(1-\alpha+\lambda)$. These equations capture firm optimality on the labor demand side and neatly illustrate the forces at work. The first term captures *capital-labor complementarity*: unless capital is fully elastic ($\lambda = 0$), all wages must go down in response to an overall increase in labor as the marginal product of labor is now lower. The second term captures *skill complementarity*: assuming imperfect substitutability between skill types ($\beta < 1$), a labor supply increase "biased" towards high-skill labor has a positive effect on low-skill wages and a negative effect on high-skill wages. The reason is that low-skill and high-skill labor are q-complements under the CES assumption, so that a relative increase in one input raises the marginal productivity of the other while lowering its own, which from the firm's perspective requires wages to adjust accordingly. The greater the substitutability between skill types, the smaller the magnitude of this effect. Similarly, the third term captures *age complementarity*: assuming imperfect substitutability between age types ($\gamma < 1$), an increase in the supply of older workers has a positive effect on the wages of younger workers via q-complementarity between the young and old. However, because the labor supply increase is more pronounced in high-skill jobs, the effect on high-skill youth wages is stronger than the effect on low-skill youth wages. Note that the skill complementarity and age complementarity effects disappear if (1) the inputs are perfect substitutes ($\beta, \gamma = 1$), or if (2) the labor supply shock is skill-neutral ($d\log L_H = d\log L_L$) and age-neutral ($d\log L_{gy} = d\log L_{go}$). In other words, changes in wages in this model are induced by a combination of imperfect substitutability between inputs and non-neutral labor supply shocks.

To obtain the equilibrium change in wages, we have to take into account the labor supply response of high-educated workers via self-selection, which will have an indirect effect on wages. For expositional purposes, assume for now that self-selection among older workers is negligible and focus on the self-selection response of younger workers. This response is summarized in the following equation, obtained by totally differentiating the threshold condition (1.5):

$$d \log u^* = d \log w_{Ly} - d \log w_{Hy} \tag{1.14}$$

Therefore, what matters from the perspective of workers is the change in *relative* wages, which hinges on whether the skill complementarity effect dominates the age complementarity effect or vice-versa, since they exert opposite pressure on the cutoff u^* (the capital-labor effect cancels out). It turns out that the skill complementarity effect dominates as long as younger and older workers are more substitutable withing skill types than high-skill and low-skill workers, i.e. $\gamma > \beta$. Assuming this is the case, the decline in high-skill wages relative to low-skill wages prompts high-educated younger workers to reallocate away from high-skill jobs towards low-skill jobs as the latter become more attractive. This self-selection response, driven by marginal workers in the ability distribution, effectively dampens the change in relative wages. To see this formally, the following equation gives the equilibrium change in relative youth wages, in the special case of negligible self-selection among older workers:

(1.15)

$$d\log w_{Ly} - d\log w_{Hy} = \frac{\overbrace{-(\beta-1)\cdot(s_{Ho}-s_{Lo}\widetilde{\delta})}^{\text{skill complementarity effect }(>0)} + \overbrace{(\gamma-1)\cdot(s_{Ho}-s_{Lo}\widetilde{\delta})}^{\text{age complementarity effect }(<0)}}{1\underbrace{-(\beta-1)\cdot\eta_u C_2^y-(\gamma-1)\cdot\eta_u C_3^y}_{\substack{\text{dampening effect due to self-selection}\\ \text{among high-educated young }(>0)}}}\cdot d\log L_o^h$$

where $\eta_u > 0$ captures the elasticity of the cumulative distribution function $\Gamma(u)$ around the initial threshold u^*, and $C_2^y > 0$ and $C_3^y > 0$ are just functions of the model parameters and initial labor shares (see Appendix A.6 for exact definitions). The numerator captures the direct effect of the labor supply shock via firm optimality, whereas the denominator captures the indirect effect via self-selection among younger workers (greater than 1). Under the premise of condition (1.11) and the assumption that $\gamma > \beta$, the numerator will be strictly positive and the threshold u^* will go up.

The reallocation of older workers towards low-skill jobs constitutes another dampening effect since it essentially attenuates the skill-biasedness of the original labor supply shock.[3] For the intuition described above to go through, one of two assumptions is sufficient. We can keep assuming that self-selection among older workers is negligible, i.e. the elasticity η_z of the comparative advantage schedule $\Lambda(z)$ is small around the initial threshold z^*, which is not unrealistic given that occupational mobility rates have been found to be quite low at older ages (Kambourov and Manovskii, 2008). Alternatively, we can assume that the initial share of older workers is strictly greater in high-skill jobs than low-skill jobs. In practice, this is roughly equivalent to saying that

[3]Self-selection among older workers is unambiguous since the age complementarity effect operates in the opposite direction.

high-skill jobs (e.g. managers) are more likely to be held by older workers than low-skill jobs (e.g. cashiers), which is also empirically plausible. The arguments outlined in this subsection are summarized in the following proposition:

Proposition 1 (Comparative Statics of a Skill-Biased Retirement Slowdown)**.** *Consider an increase in the labor supply of older workers satisfying condition (1.11). Assuming either that (A1) self-selection among the old is negligible ($\eta_z \approx 0$), or that (A2) older workers initially make up a greater share of high-skill labor than low-skill labor ($s_{Ho} > s_{Lo}$), this leads to the following three equivalent predictions:*

(a) *Decrease in high-skill employment and increase in low-skill employment among the young:*

$$d \log L_{Hy} < 0 \quad \text{and} \quad d \log L_{Ly} > 0 \tag{1.16}$$

(b) *Reallocation of high-educated younger workers from high-skill to low-skill occupations:*

$$d \log u^* > 0 \tag{1.17}$$

(c) *Decrease in high-skill youth wages relative to low-skill youth wages:*

$$d \log w_{Hy} < d \log w_{Ly} \tag{1.18}$$

under the condition that younger and older workers are closer substitutes within skill types than high-skill and low-skill labor, i.e. $\gamma > \beta$.

Proof. See Appendix A.6. □

To recap, the conceptual framework developed in this section illustrates how an increase in the labor supply of older workers concentrated in high-skill jobs can result in occupational downgrading among the young through supply and demand forces. The mechanics of the model are actually similar to Beaudry et al. (2016), except that in their framework the source of the decline in the demand for high-skill workers is the "bust" phase of an IT productivity shock. The empirical strategy, which I now turn to, essentially compares local labor markets experiencing differential increases in the labor supply of the old. In Section 1.5, I will show that in places where fewer older workers retire: (a) youth employment in high-skill occupations declines while youth employment in low-skill occupations rises, (b) the share of "overeducated" younger workers goes up, and (c) wages of younger workers in high-skill occupations decline by more than wages of younger workers in low-skill occupations, in line with the predictions in Proposition 1.

1.3. Empirical Strategy

The empirical strategy in this paper consists in comparing the evolution of youth employment outcomes across U.S. local labor markets, some of which experience greater increases in the labor supply of older workers than others over the period spanning 1980 to 2007. Local labor markets are approximated using the concept of commuting zones (CZ), which are clusters of counties defined based on commuting patterns observed in the data. Comparing outcomes across U.S. commuting zones is a common approach in the literature, and has been used to study the local labor market effects of various economic trends, including the automation of routine tasks (Autor and Dorn, 2013), import competition from China (Autor et al., 2013), immigration (Smith, 2012), and more recently the adoption of industrial robots (Acemoglu and Restrepo, 2017).

I measure changes in the labor supply of the old using changes in the 55+ employment rate, i.e. the employment-to-population ratio among the old.[4] Although the literature typically defines the older workers as aged 55 to 64, I include people aged 65 and above given that working past 65 has become increasingly common in the United States.[5] To guard against potential confounding factors that could be correlated with retirement trends, I control for a number of initial commuting zone characteristics that may alternatively explain cross-sectional variation in the evolution of youth employment outcomes. I control for the employment share of manufacturing to account for the secular decline of the industry, which has been accelerated by the rise of Chinese import competition (Autor et al., 2013; Acemoglu et al., 2016). I control for the employment share of so-called "routine" occupations, as Autor and Dorn (2013) show that commuting zones initially specialized in routine tasks subsequently experience stronger job polarization—the simultaneous decline of middle-skill occupations and rise of low-skill and high-skill occupations (see Figure 1.2)—due to the diffusion of automation technology. I also control for an index which measures the extent to which occupations in an area are susceptible to offshoring (Firpo et al., 2011), the initial share of immigrants which is predictive of future immigrant inflows, as well as the initial female employment rate to capture the rise in female labor force participation. Finally,

[4] In Section 1.5.5, I show that the results are robust to alternative measures of labor supply shocks, such as log changes or dividing employment among those aged 55 or older by total population instead.

[5] The 65+ employment rate has increased from 11% to 15% between 1980 and 2007.

I control for initial demographic differences across commuting zones, in terms of age composition, gender composition, racial composition and educational composition.

Formally, let c denote commuting zones and t denote time periods. The basic regression specification stacks first-differences across three periods (1980-1990, 1990-2000, 2000-2007), controlling for period fixed effects (δ_t) and start-of-period CZ characteristics ($X_{c,t-1}$):

$$\Delta\, y_{ct} = \delta_t + \beta \cdot \Delta\, \text{emp}/\text{pop}_{ct}^{55+} + X'_{c,t-1} \cdot \chi + \varepsilon_{ct} \tag{1.19}$$

where y_{ct} is some youth employment outcome. The empirical strategy therefore relies on differences in trends across commuting zones, rather than level differences which are partialled out. The main coefficient of interest, β, measures the effect of a one percentage point increase in the local 55+ employment rate on changes in youth employment outcomes over 10 years. To get a sense of the variation in the data, Appendix Figure A.1 plots changes in the 55+ employment rate across commuting zones and Appendix Table A.1 displays summary statistics on the distribution of changes in the 55+ employment rate, for each period separately. The takeaway is that there is a substantial amount of heterogeneity in retirement trends across time and space. Note that first-differences for the period 2000-2007 are scaled by 10/7 so that outcomes are implicitly measured in terms of 10 $\times$ mean annual changes for comparability across periods, following Autor and Dorn (2013) and Autor et al. (2013).

1.3.1. Sources of Endogeneity

The ordinary least squares (OLS) estimate of β is likely to be biased for two reasons. First, changes in the 55+ employment rate not only capture flows from employment to inactivity (i.e. retirements), but also flows between employment and unemployment (i.e. hires and separations). This is problematic because hires, layoffs and voluntary quits all tend to be correlated with the state of the local economy. Since unobservable local economic conditions can also affect youth employment outcomes through the error term ε_{ct} in (1.19), the OLS estimate $\widehat{\beta}$ will likely be inconsistent. Put differently, in regions where the economy is booming (slumping), firms tend to hire (lay off) workers of all age groups resulting in a mechanical relationship between changes in the 55+ employment rate and changes in youth employment outcomes.

Second, even if we were able to perfectly measure retirement flows, retirement decisions can themselves be influenced by local labor market conditions. For example, Coile and Levine (2007, 2011) find that the retirement propensity of individuals eligible for Social Security increases during downturns, particularly among the low-educated. On the other hand, Goda et al. (2011) argue that asset losses during the Great Recession induced some individuals to delay their retirement plans. Moreover, Social Security benefits are calculated based on the highest 35 years of earnings so that high-earners have a financial incentive to delay retirement. Regardless of the direction, the sensitivity of retirement flows to local economic conditions reinforces the notion that changes in the 55+ employment rate may reflect labor demand-side variation rather than labor supply-side variation as intended.

In practice, the sign of the bias depends on the sign of the relationship between unobservable labor demand factors and both the 55+ employment rate and the outcome under consideration. For example, if the 55+ employment rate tends to increase when local conditions improve and the outcome of interest is youth employment (unemployment), then the OLS estimate $\widehat{\beta}$ will be biased upward (downward).

1.3.2. Instrumental Variable: Local Age Composition of the Old

To address this fundamental endogeneity problem, I employ an instrumental variables (IV) approach. The idea is to exploit the fact that the propensity to retire varies over the life cycle. As a result, areas with different initial age distributions will tend to experience differential retirement trends in subsequent years. Consider the following thought experiment. Two commuting zones (A and B) are identical apart from the fact that a disproportionate number of individuals aged 45 or older are clustered between the ages of 55 and 60 in commuting zone A. In contrast, the 45+ age distribution is relatively smooth in commuting zone B. Since Americans tend to retire in their 60s, one would expect the 55+ employment rate to decline in commuting zone A *relative* to commuting zone B over the next 10 years, as the unusually large 55-60 cohort in commuting zone A goes through the peak retirement ages. Assuming local labor market conditions evolve in the same way in both commuting zones, one can compare how youth employment outcomes evolve in commuting zones A versus B to obtain clean estimates of the effect of retirement trends on younger workers.

The empirical strategy is simply a generalization of the example above in that it exploits geographical variation in the initial age composition of the old to isolate labor supply-side variation in retirement trends. In practice, I construct an instrument capturing "predicted retirement intensity" ($\widetilde{\text{PRI}}$) by interacting the start-of-period CZ-specific 45+ age distribution with 10-year national retirement rates by age, which are defined as the difference between the employment rate of a birth cohort at the beginning of the period and the employment rate of the same birth cohort 10 years later. For example, the 10-year retirement rate of 45 year olds in 1990 is equal to the employment rate of 45 year olds in 1990 minus the employment rate of 55 year olds in 2000. In other words, it measures the proportion of 45 year olds in 1990 who have retired at some point between 1990 and 2000. Formally, the instrument is given by:

$$\widetilde{\text{PRI}}_{ct}^{45+} = \sum_{a=45}^{80} \frac{\text{pop}_{c,t-10}^{a}}{\text{pop}_{c,t-10}^{45\text{-}80}} \cdot \left(\text{emp}/\text{pop}_{(-c),t-10}^{a} - \text{emp}/\text{pop}_{(-c),t}^{a+10}\right) \tag{1.20}$$

where $\text{emp}/\text{pop}_{(-c)t}^{a}$ are national employment rates by age a, excluding individuals in the commuting zone c under consideration in order to avoid any mechanical correlation in the first-stage relationship.[6] For the period 2000-2007, 7-year retirement rates are converted into 10-year equivalents by scaling them by 10/7. In the empirical analysis, I will estimate equation (1.19) via two-stage least squares (2SLS) using $\widetilde{\text{PRI}}_{ct}^{45+}$ as an instrument for 10-year changes in the 55+ employment rate $\Delta\ \text{emp}/\text{pop}_{ct}^{55+}$. Appendix Figure A.2 displays the cross-sectional variation in predicted retirement intensity and Appendix Table A.2 displays summary statistics on the distribution of predicted

[6] I truncate the age distribution at 80 since age is truncated at 90 in the data. Very few people work in their 80s anyway.

retirement intensity, separately by period. Note that whether an area is characterized by high or low predicted retirement intensity varies over time since age composition is constantly shifting.[7]

As its definition makes clear, the instrument derives its predictive power from variation in 10-year retirement rates across the entire 45+ age distribution. As such, the instrument not only exploits the fact that people tend to retire in their 60s (as in the example), but also more subtle differences. For instance, many Americans retire at 62 or 65 due to eligibility for Social Security or Medicare (Gustman and Steinmeier, 2005). As a result, commuting zones with a disproportionate number of 52 or 55 year olds at the beginning of the period will experience above-average retirement intensity. Conversely, CZs with a disproportionate number of 62 or 65 year olds at the beginning of the period will experience below-average retirement intensity, since many of them have already retired. These discontinuities—as well as the general bell shape—are clearly visible in Figure 1.4, which plots 10-year retirement rates by birth cohort, separately for each time period.

In the sense that it exploits cross-sectional differences in demographic composition, the instrument is similar in spirit to the "immigrant-enclave" instrument commonly used in the immigration literature, which relies on the fact that immigrants tend to settle in areas containing large existing populations from their home country (Altonji and Card, 1991; Card, 2001). Accordingly, these instruments predict local inflows of immigrants based on the how the pre-existing population of immigrants is spatially distributed across local labor markets and national inflows of immigrants by country

[7] The (serial) correlation coefficients for the period pairs (1980-1990, 1990-2000), (1980-1990, 2000-2007) and (1990-2000, 2000-2007) are respectively 0.48, 0.08 and 0.31.

of origin. More closely related, Maestas et al. (2016) use cross-state variation in 50+ age composition combined with 10-year national survival rates by age to predict state-level 60+ population growth.

The identifying assumption underlying the IV approach is that the initial local age distribution among the old only affects youth outcomes through retirement trends. There are three broad threats to identification. The first threat relates to the origin of differences in age composition. Geographical disparities in age composition are fundamentally the product of past birth and migration patterns. While birth patterns in the distant past are arguably orthogonal to current labor demand conditions, recent migration patterns by age could potentially be driven by regional economic trends. For example, industrial decline in some areas might simultaneously lead to poor youth employment outcomes and rapid aging as the working-age population gradually out-migrates over time. Given that a large initial share of elderly implies low predicted retirement intensity (see Figure 1.4), this could lead us to overstate the impact of an increase in the 55+ employment rate.

To get a sense of how much variation comes from past birth rates versus recent migration flows, I predict the current local 45-80 age distribution using the lagged local 35-70 age distribution projected forward by 10 years.[8] Regressing CZ-year-specific age shares on a constant and corresponding predicted age shares yields an R^2 value of 0.73, where I pooled the years 1980, 1990 and 2000 together. This suggests a substantial portion of the variation in age composition stems from past birth patterns. Nevertheless,

[8]The lagged age distribution is projected forward using 10-year national survival rates by age, defined as the size of a birth cohort at the end of the period divided by the size of the same birth cohort a decade earlier.

I implement two robustness checks in Section 1.5.5 to address any remaining concerns about migration. First, to the extent that economic decline is geographically concentrated, I will show that the results are robust to allowing for state-specific time trends. Second, to the extent that local economic decline is persistent over time, I will conduct a falsification exercise which shows that *future* retirement trends are not predictive of contemporaneous changes in youth employment outcomes.

The second threat to identification is that, even if local age composition among the old is exogenous, it could potentially affect youth employment outcomes through consumption patterns by age. It has been well-documented that consumption profiles are hump-shaped over the life cycle, and that the types of goods and services that people consume change over time (Aguiar and Hurst, 2013). As they get older, individuals tend to reduce expenditures on goods for which home production is a substitute (e.g. food) as well as work-related goods (e.g. clothing), while demand for healthcare typically goes up. The worry is that this could potentially affect the demand for workers across sectors, and therefore provide an alternative explanation for changes in youth occupational composition. In other words, do younger workers end up in retail jobs because of skill-biased retirement trends, or because of rising consumer demand? I argue that one piece of evidence favors the labor supply story: namely the fact that youth wages decline *across all occupations* in commuting zones where fewer older workers retire (see Section 1.5.3). In the context of the labor demand model presented earlier, this finding can be rationalized by imperfect substitutability between capital and labor (assuming capital is not perfectly elastic), but is harder to square with a story based on local consumption patterns by age.

The third threat to identification stems from the fact that women tend to have children at certain ages. As a result, age composition among the old could potentially be correlated with age composition among the young via fertility patterns. This could be problematic as it may affect both the size of younger cohorts—and therefore the degree of labor market competition among the young—as well as the age distribution of younger cohorts, which mechanically affects labor market outcomes through the distribution of experience. The cohort size concern is alleviated by the fact that I control for the initial population share of young (16-30) versus prime-aged (31-54) versus older (55+) individuals. Moreover, I will explicitly show that the results are not driven by age composition among the young in Section 1.5.4.

1.4. Data

U.S. Census and American Community Survey. The main data sources used in the empirical analysis are the 1980, 1990 and 2000 U.S. Censuses 5% samples, as well as the 2007 American Community Survey (ACS) 1% sample (Ruggles et al., 2017). Following others (e.g. Smith, 2012; Autor et al., 2013), I truncate the sample period in 2007 to abstract away from the Great Recession, which was particularly devastating for younger workers. The Census and ACS are large-scale surveys of the U.S. population and contain detailed information on respondents, including demographic characteristics, employment status, income and geographic location. This allows me to compute a wide array of employment outcomes at the local labor market level for various demographic groups. The main drawback of the Census/ACS is that they are repeated cross-sections, which prevents us from following the same individuals over time. As a result, one weakness of the analysis is that changes in area-level outcomes can potentially reflect changes in the underlying population (both in terms of observables and unobservables), warranting caution when interpreting the results (a point which I will return to in Section 1.5.4). I focus on the non-institutional civilian population and exclude from the sample: (1) individuals confined to institutional group quarters, (2) unpaid family workers, and (3) individuals on active military duty. All aggregate outcomes are constructed using Census sampling weights.

As already mentioned, I adopt the concept of commuting zones to approximate local labor markets. Commuting zones, developed by Tolbert and Sizer (1996), are 741 clusters of counties characterized by strong commuting ties within CZs and weak commuting ties across CZs based on commuting patterns in the 1990 Census. The advantage of commuting zones over alternative geographic units is that they cover both urban and rural parts of the country, and are not based on arbitrary factors such as state boundaries or minimum population requirements. In the analysis, I drop Alaska and Hawaii and focus on the continental U.S., resulting in a total of 722 CZs. Since commuting zones are not directly identifiable in the Census, I follow standard practice and assign individuals living in areas that overlap with multiple commuting zones to each of those CZs according to weights that reflect how the area's population is distributed across CZs (see Appendix A.1 for more details).

In order to study changes in occupational structure over multiple decades, it is important to use a time-consistent classification scheme, given that new jobs have been introduced (e.g. computer programmer) while old ones have disappeared (e.g. switchboard operator). I adopt the occupational classification developed by Dorn (2009). It distinguishes between 330 individual occupations which I categorize into 12 occupation groups, loosely following Autor (2015). To get a broad sense of what these occupations are, where the young and old are concentrated and what they earn on average, Appendix Table A.3 documents employment shares and mean hourly wages in 2000 for the five most common occupations in each occupation group (Appendix A.2 describes how hourly wages are constructed).

In this paper, I mainly focus on the outcomes of young adults aged 22 to 30. These individuals have mostly completed their education and are at various stages of the school-to-work transition. How individuals fare during this phase can have a significant impact on their subsequent career path, a notion supported by studies showing that initial labor market conditions have long-lasting effects on college graduates (Kahn, 2010; Altonji et al., 2016). To provide a complete picture of the labor market, I also report outcomes for teenagers (16-21) and the prime-aged (31-44).

1.5. Results

In this section, I estimate the effect of retirement trends on youth employment, occupational composition and wages using the IV approach described in Section 1.3, highlighting the contrast with naive OLS estimates. I then explore two important margins of adjustment: school attendance and internal migration. Lastly, I subject the main results to a series of robustness checks.

1.5.1. Employment, Unemployment and Labor Force Participation

Throughout Section 1.5, I will estimate the equation (1.19) for a variety of outcomes, either via OLS or 2SLS. Observations are weighted by the start-of-period CZ share of national population to lend more weight to larger commuting zones, and standard errors are clustered at the state level to allow for within-state correlation in the error terms, both across CZs and over time. Descriptive statistics on the full set of start-of-period commuting zone controls are given in Appendix Table A.6.

Table 1.1 shows the effect of retirement trends on youth employment, unemployment and labor force non-participation, all expressed as a share of youth population for comparability across outcomes. To uncover intensive margin effects, I further partition employment into part-time and full-time employment, where part-time is defined as working less than 35 hours a week.[9] The OLS estimates in Panel A imply that the labor supply of older workers is positively correlated with youth employment: a one percentage point increase in the 55+ employment rate is associated with a 0.5 percentage point increase in the youth employment rate, with a corresponding decline in youth unemployment and non-labor force participation. Moreover, youth employment appears to shift from part-time to full-time jobs.

However, as discussed in Section 1.3.1, OLS estimates likely reflect unobservable labor demand conditions, which tend to push outcomes of all age groups in the same direction. I therefore instrument for changes in the 55+ employment rates with predicted retirement intensity based on the initial 45+ age composition, thereby isolating

[9]Note that, consistent with the findings for older women in Goldin and Katz (2016), most of the increase in the 55+ employment rate has been concentrated in full-time jobs (see Appendix Table A.5).

plausibly exogenous labor supply-side variation in retirement trends. The first-stage results are shown in Panel A of Appendix Table A.7, separately by period in columns (1)-(3) and pooling them together in column (4). The instrument has sufficient explanatory power, with all *F*-statistics exceeding the rule-of-thumb threshold of 10 so that weak instruments is not a concern (Stock and Yogo, 2005). In terms of magnitude, a one percentage point increase in the share of 45+ year olds predicted to retire over the next 10 years is associated with a 1.4 percentage point decline in the 55+ employment rate.

The corresponding 2SLS estimates are shown in Panel B of Table 1.1. While the coefficients for youth employment and labor force non-participation are not statistically significant, the coefficient on youth unemployment is *positive* and significant, unlike the OLS estimate. Moreover, the no-employment effect masks a simultaneous decline in full-time employment and rise in part-time employment among the young (even though the coefficient for full-time employment is not significant). The 2SLS results therefore suggest that increases in the labor supply of older workers have a negative impact on younger workers, if anything. Note that the disparity between the OLS and 2SLS estimates supports the view that labor demand factors bias the OLS estimates towards finding a "positive" relationship between changes in the 55+ employment rate and changes in youth outcomes. That is, thriving areas of the country are characterized by rising employment among the young and old alike.

Appendix Table A.8 splits the 2SLS results for the young (22-30) by gender and education, and shows results for teenagers (16-21) and the prime-aged (31-44). The main takeaway from this table is that teenage employment undergoes a sharp decline (about one percentage point), while the prime-aged are relatively unaffected.

1.5.2. Occupational Composition

I now turn to the main outcome of interest: youth occupational composition. To simplify the exposition, I combine the 12 occupation groups in Figure 1.2 into three skill groups: low-skill, middle-skill and high-skill occupations. Low-skill occupations are comprised of agriculture, food preparation/maintenance, personal services and unskilled sales occupations (e.g. cashiers). Middle-skill occupations include operators/laborers, clerical/administrative jobs, production workers and protective services. High-skill occupations include skilled sales occupations (finance, insurance and real estate), technicians, professionals and managers. Youth employment by skill group is measured in youth population shares so that the coefficients naturally add up to the total employment effect in column (1) of Table 1.1.

Panel A of Table 1.2 displays the OLS estimates. Consistent with the employment results, they suggest that an increase in the 55+ employment rate has a positive effect on the young, in the sense that employment shifts away from low-skill jobs towards middle-skill and high-skill jobs. In contrast, the 2SLS estimates in Panel B imply the exact opposite: youth occupational composition *worsens* in commuting zones where fewer older workers retire. In particular, a one percentage point increase in the 55+ employment rate reduces youth employment in high-skill occupations by 0.74 percentage points and raises youth employment in low-skill occupations by 0.52 percentage points, both highly statistically significant. The coefficient on middle-skill employment is positive but not statistically significant. Appendix Table A.9 reveals that the increase in low-skill jobs is concentrated among younger workers without a college degree, while the decrease in high-skill jobs is most pronounced among college graduates. Interestingly, employment in middle-skill jobs goes up among the prime-aged (31-44) and young college graduates, whereas younger workers without a college degree and teenagers (16-21) respectively experience an increase in low-skill jobs and an overall decline in employment as already mentioned. These patterns are broadly consistent with the notion of a "job ladder," in which workers with higher levels of education and/or experience displace those with lower levels of education/experience as they move down the ladder (Beaudry et al., 2016; Barnichon and Zylberberg, 2016). Appendix Figure A.3 decomposes the skill group effects at the occupation group level and shows that the decline in high-skill jobs primarily stems from managerial and professional occupations, where the labor supply of older workers has seen the largest increase in recent decades (see Figure 1.2).

To ensure that these findings do not hinge on the specific way in which I aggregated occupations, I show similar results using an alternative method. Following Autor and Dorn (2013), I rank the 330 individual occupations according to mean 1980 hourly wages and combine them into three equal-sized bins, each containing a third of total employment in 1980 (bottom, middle and top tercile). The results using this alternative grouping of occupations are displayed in Appendix Table A.10. Both the OLS and 2SLS estimates are relatively close to the corresponding estimates in Table 1.2, suggesting a fair amount of overlap between skill groups and skill terciles.

Another way to reveal occupational downgrading among the young is to measure the share of younger workers that are "overeducated" for their job, sometimes referred to as "underemployment." As mentioned in the introduction, studies have shown that college graduates in the U.S. are increasingly concentrated in low-skill jobs that do not require a college degree. In light of the results in Table 1.2, one might expect to observe an increase in this type of educational mismatch in places where fewer older workers retire. In order to determine whether workers are overeducated or not, we first need to assign a required level of education to each occupation. One option is to follow Abel et al. (2014) and exploit job descriptions from the Department of Labor's Occupational Information Network (O*NET). Appendix A.3 describes this procedure in more detail. An alternative approach is to assign the most common education level observed in the data, as in Clark et al. (2016). I build two measures of overeducation using the latter approach, one based on the modal education level in 1990 and another where the modal education level is allowed to vary by year. Appendix Table A.4 summarizes educational requirements by occupation group. As expected, occupations at the upper end of the spectrum tend to have higher educational requirements.

I define overeducation as one of two instances: (1) having at least a 4-year college degree and being employed in an occupation that does not require one, or (2) having some education beyond high school (Associate's degree, post-secondary certificate, college dropout) and being employed in an occupation that only requires a high school degree or less. Appendix Table 1.3 shows the impact of retirement trends on the share of workers with some education beyond high school that are overeducated for their job. The 2SLS estimates in the first column of the bottom three panels imply that a one percentage point increase in the 55+ employment rate is associated with a 1.1 to 1.3 percentage point increase in the share of workers that are overeducated, irrespective of the method used to compute education requirements. The magnitude of this effect is relatively comparable across genders and education groups, although the prime-aged (31-44) experience no change along this dimension.

In summary, the results in this section demonstrate that in commuting zones where fewer older workers retire due to the initial age structure, the occupational mix of younger workers shifts from high-skill to low-skill jobs. This finding is in line with Proposition 1, which states that an increase in the labor supply of older workers concentrated in high-skill jobs leads to occupational downgrading among the young. Recall that this prediction is valid under the key assumption that the young and old are more substitutable within skill types than low-skill and high-skill workers. Card and Lemieux (2001) provide the most relevant empirical evidence on this matter. Using Current Population Survey data from 1970 to 1997, they estimate a structural model of labor demand featuring a nested CES production function similar to the one presented in Section 1.2. They find an elasticity of substitution between age groups within skill types $\sigma_\gamma = 1/(1-\gamma)$ in the 4 to 6 range, and an elasticity of substitution between skill types $\sigma_\beta = 1/(1-\beta)$ in the 2 to 2.5 range. This respectively implies a value of γ between $3/4$ and $5/6$, and a value of β between $1/5$ and $1/2$, which is consistent with the premise of Proposition 1. Therefore, the fact that retirement trends have been skill-biased provides a coherent and empirically plausible explanation for the occupational patterns observed in the data.

1.5.3. Wages

Table 1.4 shows the impact of retirement trends on mean log hourly wages of young adults working full-time, excluding the self-employed. Column (1) pools all occupations together while columns (2)-(4) examine wages separately by skill group. The 2SLS estimates in Panel B are all negative and statistically significant. The baseline estimate in column (1) implies that wages of younger workers decline by 3% in response to a one percentage point increase in the 55+ employment rate. However, given the results in the previous section, does this simply reflect the fact that younger workers are more likely to be employed in low-skill jobs, which tend to pay less on average?

One way to assess this claim is to adjust wage differences for changes in the observable composition of workers, both in terms of demographics and the types of jobs that they hold. I generate "composition-adjusted" wage measures following the two-step procedure in Shapiro (2006) and Albouy (2016). First, using individual-level Census data, I regress log wages on a comprehensive set of individual controls (incl. gender, race, education, potential experience, industry, occupation) and commuting zone fixed effects, separately by year. I then extract the estimated year-specific CZ fixed effects and take first-differences to obtain mean log wage differences that are not mechanically driven by changes in the local composition of workers (see Appendix A.4 for additional details). Note that this method can only account for observable characteristics. We cannot rule out that the pool of workers has changed in terms of unobservables as well, a fundamental weakness of repeated cross-sectional data. The 2SLS estimates for the composition-adjusted wage measures are displayed in Panel C. They suggest that about 15% of the wage effect can be attributed to changes in the observable composition of workers. Nevertheless, they still imply that youth hourly wages decline by 2.5% in response to a one percentage point increase in the 55+ employment rate over a 10-year period.

In the context of the conceptual framework developed in Section 1.2, declining wages in response to an increase in total labor supply is consistent with the notion that capital is not perfectly elastic at the commuting zone level.[10] Proposition 1 also makes the following wage prediction: high-skill youth wages should decline relative to low-skill youth wages. However, this statement applies to wages *per efficiency unit*. The average wage among younger workers in high-skill jobs, which is arguably closer to what we observe in the data, depends on the latent ability distribution:

$$\overline{w_{Hy}} = \int_{u^*}^{u^{\max}} u \cdot w_{Hy} \cdot \Gamma'(u) \cdot du = w_{Hy} \cdot E(u|u > u^*) \tag{1.21}$$

Totally differentiating (1.21) yields:

$$d \log \overline{w_{Hy}} = d \log w_{Hy} + \eta_E \cdot d \log u^* \tag{1.22}$$

where $\eta_E > 0$ is the elasticity of $E(u|u > u^*)$ around the initial ability threshold u^*. The first term on the right-hand side of (1.22) is negative under the premise of Proposition 1. On the other hand, the second term is positive since $d \log u^* > 0$. Intuitively, it captures the fact that average ability among younger workers who remain in high-skill jobs goes up due to self-selection (recall that marginal ability workers optimally reallocate towards low-skill jobs). Going back to the empirical results, even though there

[10] Specifically, λ must be sufficiently positive in equations (1.12)-(1.13).

is no obvious parallel between the model and the data, this could potentially explain why the wage effects for low-skill versus middle-skill versus high-skill occupations in columns (2)-(4) of Panel C are relatively similar in terms of magnitude (one would not be able to statistically reject that they are equal). Appendix Table A.11 reports wage estimates for various demographic groups, ranging from -1.1% for prime-aged workers to -3.5% for middle-educated younger workers.

Note that the OLS estimates in Panel A of Table 1.4 are all positive and significant. Combined with the OLS results on youth employment and occupational composition, this lends further support to the claim that raw changes in the 55+ employment rate reflect variation in local economic conditions. For the rest of the analysis, I only report 2SLS estimates.

1.5.4. Margins of Adjustment

School Attendance. Past studies have documented that college attendance among the young tends to rise during downturns, as the opportunity cost of going to school falls. For instance, Betts and McFarland (1995) find a positive relationship between community college enrollment and the unemployment rate. More recently, Charles et al. (forthcoming) show that the housing boom of the 2000s reduced enrollments at 2-year colleges as youth labor markets prospects improved. Given the results so far, one may wonder whether the option to go to school similarly serves as an adjustment mechanism in response to rising labor supply among the old. In Table 1.5, I estimate the effect of retirement trends on school attendance. Column (1) implies that a one percentage point increase in the 55+ employment rate increases school attendance among young adults by 0.44 percentage points (as a share of population). The effect is more pronounced for females than for males, and is concentrated among individuals with some education beyond high school but less than a 4-year college degree. School attendance also rises significantly among teenagers (16-21), consistent with the large employment and wage declines documented earlier.

There are two ways to interpret these findings. First, one can view them as corroborating evidence that youth labor market prospects indeed deteriorate in commuting zones where fewer older workers retire. Second, to the extent that school attendance raises individuals' future productivity, higher educational attainment could potentially mitigate/offset the immediate labor market consequences of increases in the labor supply of the old. Note that this crucially hinges on whether the returns to early-career work experience exceed the returns to education for those individuals induced to return to (or stay in) school. However, predicted returns to education are unlikely to be the highest among "marginal" individuals who would not have gone to school otherwise.

Net Migration. Population typically adjusts in response to local labor demand shocks (Blanchard and Katz, 1992). This is especially true for young adults, who exhibit the highest mobility rates among all age groups (Molloy et al., 2014). Table 1.6 shows the effect of retirement trends on net migration, that is, changes in log population counts. I find strong evidence of net out-migration among the young: a one percentage point increase in the 55+ employment rate is associated with a 4% contraction in the youth population over 10 years. Although it is impossible to tell whether net out-migration reflects higher out-migration or reduced in-migration, Monras (2017) suggests that in the U.S. internal migration in response to labor demand shocks is mostly driven by in-migration rates. The fact that college graduates appear to be most responsive to local labor market conditions is consistent with similar findings in the literature (Bound and Holzer, 2000; Wozniak, 2010; Notowidigdo, 2011). Interestingly, there is no population change among the middle-educated young, teenagers or the prime-aged.

Net out-migration among college graduates raises an important concern regarding the results on youth occupational composition: does occupational downgrading among the young simply reflect changes in the underlying population? The fact that college graduates are more likely to be employed in high-skill jobs certainly suggests that changes in educational composition could account for some of the effect. However, the effect is unlikely to fade away completely since occupational downgrading occurs among all education groups (see Appendix Table A.9). Using the same two-step method as in Section 1.5.3, I generate changes in employment by skill group that are "adjusted" for age, gender, race and education.[11] Comparing the resulting estimates in Appendix Table A.12 with Panel B of Table 1.2 suggest that between 30 and 40 percent of occupational downgrading among the young can be attributed to changes in demographic composition, notably education. Nevertheless, youth employment in high-skill occupations still declines by 0.41 percentages points, while youth employment in low-skill occupations rises by 0.36 percentages points (both statistically significant). Therefore, the main empirical finding of the paper remains unchanged.

[11] That is, I first regress a dummy for employment by skill group on dummies for age, gender, race and education using individual-level Census data. I then extract the year-specific CZ fixed effects and apply first differences. Note that the average probability of being employed in a skill group can be thought of as the analog of the corresponding population share.

1.5.5. Robustness

Falsification Test. As discussed in Section 1.3.2, one potential concern with the empirical strategy is that recent migration patterns by age, which affect local age composition, could be driven by local labor market conditions. As a result, poor youth employment outcomes could reflect regional economic trends rather than increases in the labor supply of the old. Under that scenario, to the extent that these trends are persistent over time, one would expect contemporaneous changes in youth employment outcomes to be correlated with *future* retirement trends. In Table 1.7, I regress changes in youth employment outcomes in 1970-1980, 1980-1990 and 1990-2000 on changes in the 55+ employment rate in the next period (1980-1990, 1990-2000, 2000-2007), pooling all three periods together. All the coefficients are statistically insignificant, suggesting that recent migration patterns by age are unlikely to be a major concern.

Alternative Controls and Samples. Appendix Table A.13 assesses the robustness of the main findings to alternative controls. Panel A adds state fixed effects, implicitly relying on deviations from state-specific time trends for identification. Panel B includes initial employment shares in 13 broad industry groups and the 12 broad occupation groups to fully control for industrial/occupational differences across commuting zones. Panel C controls for finer age shares (16-21, 22-30, 31-44, 45-54, 55+), while Panel D controls for age-education group shares. Although the magnitude of the estimates varies somewhat across specifications, the patterns are broadly similar.

Similarly, Appendix Table A.14 evaluates the robustness of the main findings to alternative sample restrictions. Panel A adjusts all outcomes for demographic composition (age, gender, race, education), as in Section 1.5.4. Given the school attendance results, one might be worried that the findings are simply driven by the fact that students tend to hold worse jobs than full-time workers. Therefore, Panel B excludes students from the sample. To mitigate the impact of net migration across commuting zones which could potentially affect the underlying pool of younger workers in terms of unobservables, Panels C and D respectively exclude from the sample people born in another state than the state they currently reside in and people who recently migrated from another state. Finally, as a way to ensure that the results are not driven by any specific part of the country, Appendix Table A.15 excludes Census divisions one-by-one from the sample. Reassuringly, the results are remarkably stable across all samples.

Alternative Labor Supply Shock and Instrument Definitions. In our main specification, we measure labor supply increases among the old using changes in the 55+ employment rate. Appendix Table A.16 shows that alternative choices yield similar results. Panel A uses log changes (i.e. 55+ employment growth), Panel B divides 55+ employment at the start and end of the period by the 16+ rather than the 55+ population, while Panels C and D divide changes in 55+ employment by the start-of-period 55+ and 16+ population respectively. Naturally, alternative measures of labor supply shocks have different scales, which is why the estimates vary in terms of magnitude, but the overall patterns are very similar.

The baseline instrument described in Section 1.3.2 interacts start-of-period 45+ age composition at the CZ level with 10-year national retirement rates by age. An alternative way to harness the same source of variation is to predict both start-of-period and end-of-period 55+ employment rates, and take first differences. One can predict the start-of-period 55+ employment rate by interacting the initial CZ-specific 55+ age distribution with national employment rates by age. Similarly, one can predict the end-of-period 55+ employment rate by projecting the initial CZ-specific 45+ age distribution forward by 10 years (using 10-year national survival rates by age), and interacting the resulting distribution with end-of-period national employment rates by age.

I also construct a version of the instrument in which I replace 10-year retirement rates by age from the U.S. with corresponding 10-year retirement rates from Canada.[12] One potential concern with the current interpretation of the results is that national retirement trends by age could be driven by age-specific labor demand shocks, rather than labor supply-side factors such as improvements in late-life health, the decline in physically-demanding jobs and rising life expectancy. Therefore, to the extent that age-specific labor demand shocks are confined to the U.S. while trends in health/jobs are common across the U.S. and Canada, this instrument isolates variation in retirement rates that stems from labor supply-side factors. Conceptually, the spirit of this exercise is similar to Autor et al. (2013), who instrument imports between the U.S. and China with imports between other advanced countries and China, in order to isolate

[12]See Appendix A.5 for details on how 10-year retirement rates by age were computed using Canadian Census data.

the component of Chinese imports that comes from increased competitiveness of Chinese manufacturers rather than U.S. product demand shocks.

The first-stage results for these two alternative instruments are shown in Panels B and C of Appendix Table A.7. They both have sufficient predictive power, with *F*-statistics well above 10. Appendix Table A.17 shows the second-stage results for youth employment and occupational composition. The fact that the coefficients are fairly similar to the baseline estimates highlights just how robust the main results are.

1.6. Discussion

In this section, I take stock of the findings documented in this paper and discuss some broader implications. First off, the stark contrast between OLS and 2SLS estimates highlights the challenge associated with estimating the causal effect of labor supply decisions of one group of workers on another group of workers using cross-sectional variation in employment rates, as local labor demand factors naturally push outcomes of all workers in the same direction. In the case of older workers, taking advantage of the fact that they tend to retire at specific ages, I have shown that it is possible to exploit geographical variation in age composition to isolate labor supply-side variation in retirement trends.

My main finding is that in commuting zones where fewer older workers retire, youth employment in high-skill occupations declines while youth employment in low-skill occupations rises. Going back to the original motivation of the paper, this suggests that the slowdown in retirements since the mid-1990s has contributed to deteriorating labor market prospects among the young. To get a sense of magnitudes, I perform a national accounting exercise extrapolating the cross-sectional estimates to the aggregate. Between 1990 and 2007, the 55+ employment rate rose by 7.2 percentage points (see Appendix Table A.5). Over the same period, youth employment in low-skill occupations rose by 3.9 percentage points, while youth employment in high-skill occupations essentially stayed constant. Using the 2SLS estimates adjusted for demographic composition from Appendix Table A.12 (internal migration should be irrelevant for this exercise), this implies that the retirement slowdown can account for about 60 percent of the aggregate rise in youth low-skill employment between 1990 and 2007 ($0.319 \times 7.2 \approx 2.3$). Moreover, if the 55+ employment rate had not increased, youth employment in high-skill occupations would have actually risen by 3 percentage points between 1990 and 2007 ($-0.414 \times 7.2 \approx -3$).

The retirement slowdown therefore provides a novel explanation for the recent struggles of younger workers (and college graduates in particular), complementing the IT story in Beaudry et al. (2016). Note that the fact that youth outcomes improved during the 1990s and subsequently deteriorated during the 2000s—referred to as the "Great Reversal" in the demand for cognitive skills in Beaudry et al. (2016)—is not at odds with my hypothesis. It is entirely possible that rising demand in high-skill jobs due to the IT revolution outpaced declining demand due to skill-biased retirement trends during the 1990s, and that the latter became more prominent in the 2000s as IT technologies reached maturity.

Poor youth employment outcomes are significant because they can have long-lasting effects. For example, several studies have documented long-term earnings "scars" associated with graduating from college during a recession (Kahn, 2010; Oreopoulos et al., 2012; Altonji et al., 2016). More recently, Guvenen et al. (2017) use individual earnings histories spanning the period 1957-2013 to investigate patterns in lifetime income inequality. They find that early career outcomes are an important determinant of both cross-cohort and within-cohort lifetime income inequality. Table 1.8 provides some suggestive evidence on the long-term effects of retirement trends. Panel A reproduces the baseline results from Section 1.5, which essentially look at outcomes of people aged 22-30 today versus outcomes of people aged 22-30 ten years later.[13] Alternatively, we can follow the same cohort over time and look at outcomes of people aged 22-30 today versus outcomes of people aged 32-40 ten years later. The resulting estimates in Panel B reveal negative effects on occupational composition, overeducated employment and wages that are smaller in magnitude but still statistically significant.

The findings in this paper provide some useful insights in the context of retirement age policy. As is well-known, population aging is putting enormous pressure on dependency ratios, the ratio of "dependents" (aged 0-14 or 65+) to the working age population (aged 15-64). This growing imbalance poses a serious threat to the long-term solvency of pay-as-you-go (PAYG) pension schemes, in which the current generation of workers funds the current generation of retirees through payroll taxes.[14] To address

[13]In this table, we exclude individuals born out-of-state from the sample to mitigate the impact of internal mobility.

[14]The U.S. is in a better position than other countries thanks to higher inflows of immigrants (who tend to be of working age) and higher fertility rates. Nevertheless, the Social Security Administration projects that the Trust Fund's reserves will be depleted by 2034, at which point it will only be able to meet 79% of its obligations (OASDI Board of Trustees, 2016).

this problem, policymakers have overwhelmingly opted to expand the size of the labor force relative to the size of the retired population, mainly by raising the age at which individuals are eligible for partial or full retirement benefits and discouraging early retirements.[15] Indeed, in many European countries (e.g. France, Germany, Spain, U.K.) as well as the U.S., the normal retirement age is being gradually raised to 67 or 68.

While the fiscal benefits of these policies are evident, potential costs, if any, are less clear. One concern that is often brought up is the potential crowding-out of younger workers. A long-held belief among the public and some policymakers has been that the rate at which older workers retire directly determines the number of jobs available for the young, a classic example of the so-called "lump-of-labor" fallacy. This kind of zero-sum view of the labor market has been widely rejected by the economists (Börsch-Supan, 2013). As mentioned in the introduction, studies for the U.S. have found little evidence that the old crowd out the young (Gruber and Wise, 2010; Munnell and Wu, 2012). In light of this evidence, the current consensus in policy discussions seems to be that encouraging people to work longer will not have any repercussions for younger generations (United States Government Accountability Office, 2012; The PEW Charitable Trusts, 2012; Carnevale et al., 2013).

This paper offers a slightly different perspective. Delayed retirements should not simply be viewed as an increase in the number of older workers, but also as an increase in the labor supply of certain skill groups. At least in the U.S., it is clear that older Americans are increasingly concentrated at the upper end of the occupational

[15] In the U.S., complementary measures include the elimination of the Social Security earnings test for individuals who have reached the normal retirement age and delayed retirement credits, which compensate individuals who claim Social Security benefits past the normal retirement age (up to age 70).

spectrum. As illustrated in the empirical analysis, this implies that the consequences for younger workers may manifest themselves in the types of jobs that they hold, rather than employment levels. Therefore, a key consideration in understanding the potential consequences of raising the retirement age is whether low-skill or high-skill workers will respond more strongly to those changes.

1.7. Conclusion

A combination of health improvements, rising life expectancy, changing norms and a policy shift towards prolonged work lives implies that the old are likely to keep working longer, not just in the U.S. but around the world. How will this structural shift affect the labor market, and younger generations in particular?

This paper investigates whether—and how—the rise in the labor supply of older Americans has affected the job prospects of labor market entrants over the period 1980-2007. The empirical analysis compares the evolution of youth employment outcomes across U.S. commuting zones, isolating plausibly exogenous variation in retirement patterns due to differences in the initial age composition of the old. I find that in commuting zones where fewer older workers retire, youth employment in high-skill occupations declines while youth employment in low-skill occupations rises. Building on the fact that older Americans are increasingly concentrated in high-skill jobs, I show that occupational downgrading among the young can be rationalized using a model of the labor market featuring occupational choice. Specifically, these patterns are consistent with a world in which the old and young are more substitutable within skill types than low-skill and high-skill workers, a notion that is supported by empirical estimates in the literature. Commuting zones where fewer older workers retire also experience a significant decline in youth wages and youth employment shifts from full-time to part-time jobs. Finally, I document evidence of net out-migration and greater school attendance among the young, both of which have been found to be important adjustment mechanisms in the face of declining labor market prospects.

Overall, the findings in this paper suggest that retirement trends have contributed to stagnant youth outcomes in recent years. The estimates imply that the retirement slowdown can explain up to 60 percent of the rise in youth employment in low-skill occupations between 1990 and 2007. This offers a novel explanation for the declining fortunes of the young since 2000, complementing the IT hypothesis in Beaudry et al. (2016). Whether or not we should be concerned by deteriorating early career outcomes hinges on individuals' ability to catch up over time. Unfortunately, initial evidence on cohort-specific outcomes suggests the presence of long-term effects. Future work could exploit the increasing availability of longitudinal administrative data to directly estimate these long-run effects.

CHAPTER 2

The Intergenerational Incidence of Government Old-Age Support: Evidence from the Early Social Security Era[1]

2.1. Introduction

The efficiency and distributional consequences of government old-age support depend crucially on the nature and strength of the links between parents and their adult children. Absent such links, expansions of government old-age support redistribute from younger to older generations and crowd out life cycle saving for retirement and the capital stock (see, for example, Feldstein and Liebman, 2002). Strong links between parents and their adult children fundamentally transform the effects of government old-age support programs, since, with strong links, expansions of government old-age support tend to trigger offsetting changes in intergenerational transfers within families (Barro, 1974; Becker, 1974; Bernheim and Bagwell, 1988). This reduces the extent to which such expansions redistribute from younger to older generations and crowd out the capital stock, and it causes such expansions to redistribute from families with more to fewer children (since with family insurance the per-child cost of providing a given level of old-age support is decreasing in family size, whereas with government old-age support it is independent of family size). It also raises the possibility that government old-age support has a variety of effects that are not often considered, including on the

[1] This chapter is joint work with Daniel Fetter and Lee Lockwood.

labor supply and geographic mobility of recipients' adult children and their families. Understanding the effect of government old-age support on intergenerational transfers within families is therefore central for evaluating these policies.

In this paper, we provide new evidence on the effect of government old-age support on intergenerational transfers within families by investigating the Old Age Assistance Program (OAA). OAA was introduced alongside Social Security in the 1930s and provided means-tested support to the elderly. It was large both in absolute terms—22 percent of people 65 and older received OAA in 1940—and relative to Social Security, which made no regular payments until 1940 and remained smaller than OAA until 1950. An especially useful feature of OAA from an empirical perspective is that unlike Social Security and many other social insurance programs, OAA eligibility and benefit levels were set at the state level and exhibited considerable variation, from very small programs in some states to very large programs in others. This unusual cross-sectional variation, together with the fact that private pensions and other government programs targeting the elderly were relatively uncommon compared to later periods, makes OAA a promising setting to investigate the effects of these programs.

Our key measure of family transfers is co-residence (shared living arrangements) between the elderly and adult relatives other than spouses. This is a frequently used measure of intergenerational transfers, particularly in the literature focused on historical periods (e.g., Costa, 1997, 1998, 1999). Moreover, as we discuss in Section 2.3, co-residence likely responds differently to government old-age support than other types of family transfers due to the indivisibility of living arrangements (at any given time someone either lives independently or with others, not some of both), which has important implications for efficiency and distributional consequences of government old-age support. As we discuss in Section 2.5, in the time series, the legislative expansion of government old age support through OAA and Social Security coincided with large reductions in co-residence rates among the elderly, particularly co-residence arrangements where the elderly live in their children's household as dependents.

Our analysis combines large policy variation with data on the entire U.S. population from the 1930 and 1940 U.S. Censuses. The large sample size of this dataset and its precise geographic information enable a wide range of empirical tests that would have been difficult or impossible with previously available data. Our main empirical tests exploit differential expansions in OAA payments across states over the 1930s, using a simulated instrument based primarily on differences across states in legal maximum payments at the end of the 1930s. Our estimates indicate that OAA significantly reduced intergenerational co-residence among older individuals. States in which OAA expanded more over the 1930s saw differentially large reductions in co-residence rates of the elderly with other family members. The magnitude of the results is significant as a share of the observed change over the 1930s. Our baseline estimates suggest that for elderly men, for whom co-residence fell by 3 percentage points over the 1930s, co-residence rates would have risen by 1 percentage point in the absence of OAA. For elderly women, our estimates suggest that OAA accounts for about three quarters of the observed decline in co-residence of 4.6 percentage points. Further results on whether the co-residing elderly are recorded as being the head of the household or dependents, and whether they live with their sons or daughters, shed additional light on the nature of family transfers and the effects of government old-age support.

Our main contribution is to provide novel evidence on the extent to which family transfers, in the form of shared living arrangements, respond to changes in government old-age support. Even judged solely by the degree to which it advances our understanding of modern social insurance programs, a historical perspective on this question has three important advantages. First, this period precedes the large expansions in social insurance which took place during the mid-20th century in the United States, allowing us to study family transfers in a setting in which the government safety net was quite limited. Second, unlike most contemporary social insurance programs, OAA benefit levels were set at the state level. This unusual cross-sectional policy variation provides critical empirical leverage to estimate the causal effect of old-age support programs. Finally, we are able to exploit Census data on the entire U.S. population.

Closely related to this study is the literature on the historical decline of intergenerational co-residence. Costa (1999), who studies the effect of an expansion of OAA between 1940 and 1950 on co-residence rates of widowed women, is particularly closely related. Relative to that paper, some advantages of this analysis, in addition to shedding light on a different time period and on co-residence of men as well as women, are that we use statutory variation in payments rather than variation in observed payments, and that with the availability of complete count Census data from 1940 and earlier years, the 1930s offer the potential to examine indirect impacts of OAA on non-co-resident adult children (by using earlier Census waves to identify family links and following individuals over time). Other related work includes Costa (1997), who finds that Union Army pensions reduced co-residence among elderly men in the early 20th century, and McGarry and Schoeni (2000), who find that Social Security increased independent living arrangements among elderly widowed women from 1940 onwards. Engelhardt et al. (2005) show using more recent data that reduced Social Security income for the so-called "Notch" generation increased shared living arrangements. Another part of this literature (e.g. Ruggles, 2007) has tried to disentangle the determinants of co-residence, and in particular the extent to which it is driven by the income of elderly parents versus the income of their adult children. Also related is a literature on family transfers and living arrangements in developing countries, particularly focused on the South African Old Age Pension (e.g., Jensen, 2004; Edmonds et al., 2005; Hosegood et al., 2009; Hamoudi and Thomas, 2014), which has examined the response of family arrangements to receipt of old age pensions, as well as indirect effects of old age pensions on younger family members' labor supply.

The remainder of the chapter is organized as follows. In Section 2.2, we provide some background information on the Old Age Assistance program. In Section 2.3, we discuss some conceptual ideas to motivate and guide the interpretation of the empirical analysis. Section 2.4 describes the data and our empirical strategy. We then present our main findings in Section 2.5, before concluding in Section 2.6.

2.2. Background on the Old Age Assistance Program

The Old Age Assistance (OAA) program was introduced in the Social Security Act of 1935, alongside Old Age Insurance (OAI), the program that came to be known as Social Security. OAA provided federal matching grants to states for means-tested old age support programs for the low-income elderly, designed and administered at the state level. The early Social Security program was quite small, making payments to less than two percent of the elderly in 1940 (the first year of annual payments), but the introduction of OAA led to a rapid expansion in government old-age support. In 1930, only eight states, holding about 13 percent of the population 65 and older, made any payments under a state old age assistance program, and only about 1.2 percent of those 65 and older in these states received payments through these programs. By 1940, all states made payments under an OAA program, and nationwide about 22 percent of people aged 65 and older received OAA payments. In 1940, the average OAA payment was $232 per year ($3,615 in 2010 dollars), which was about 25 percent of 1939 median wage and salary earnings for 60–64-year-olds earning a wage, and slightly over half of the 25th percentile of wage earnings. As discussed extensively in Fetter and Lockwood (forthcoming), OAA was by far the largest source of old-age support around 1940, greatly exceeding both Social Security and employer pensions. Social Security became the larger program only in the 1950s, as legislation expanded eligibility and benefits and OAA was gradually phased out.

Critically, eligibility and benefit levels for OAA programs differed widely across states, due to significant discretion left to states in the design and administration of their OAA programs. Appendix Figure B.1 shows county-level data from U.S. Social Security Board (1940c) on total OAA payments in the month of December 1939, scaled by the population 65 and older in the 1940 Census. The substantial differences in payments across state borders suggests that different state policies led to large differences in payments for individuals in similar circumstances.

OAA programs were generally set up as either an income floor or a consumption floor (the latter of which takes into account all resources when determining payments). In practice, state or local OAA administrators evaluated the "needs" and "resources" of each applicant and the excess, if any, of needs over resources determined the size of the payment, up to a maximum level. In some states the level of "needs" could vary across people, while in others a common dollar amount was used. As in Fetter and Lockwood (forthcoming), the analysis uses measures of maximum payments to approximate variation across states in the level of the income or consumption floor. The statutory maximum monthly payment was \$30 in most states (\$470 in 2010 dollars), which was the federal matching cap, but ranged from \$15 to \$45, with eight states having no statutory maximum. The states that had no statutory maximum had a small number of very high payments, but the 99th percentile of payments were well in line with other states' legal maxima.[2] Treatment of married couples varied across states. When both spouses were eligible, payments were sometimes made through joint grants and sometimes through two individual grants.

Eligibility for OAA under state OAA laws depended on a variety of criteria. All states had a minimum age of eligibility, nearly always 65, and required that an applicant have little income. Nearly all states had residency requirements. Many states also imposed asset tests and restricted eligibility to U.S. citizens or long-term residents. Of particular relevance to this analysis, many states required that an applicant have no legally responsible relatives able to provide support, and support provided by relatives would be regarded as an applicant's available "resources." Which relatives were

[2] See Appendix Table B.1, which reports basic features of each state's payments.

legally responsible varied by state. Biological children would always be considered responsible, although Lansdale et al. (1939) note that sons-in-law were typically exempt, limiting the extent to which married daughters could be held responsible for supporting their parents. To the extent that relative responsibility laws changed the implicit taxation of co-residence in OAA, they should influence its effects on co-residence. For example, in states with no such laws, co-residence is taxed relative to separate living arrangements, but if relatives are legally bound to provide support regardless of living arrangements there is less implicit taxation of co-residence.

Table 2.1 shows summary statistics at the state level on recipiency and payments in December 1939. Recipiency rates and benefits per recipient both varied widely across states, and were not strongly correlated across states. Payments per person 65 and older varied more than 13-fold across states.

2.3. The Effect of OAA on Family Transfers and Living Arrangements: Predictions and Implications

To motivate and frame the empirical analysis below, in this section we discuss how different features of OAA might be expected to affect living arrangements and the implications for the efficiency and distributional consequences of OAA policy. We consider an unfunded (pay-as-you-go) government program that taxes the working-age population to give cash benefits to the elderly. For simplicity, we first consider the case of an unconditional cash benefit to the elderly, the size of which is independent of their labor supply, living arrangements, and other choices and characteristics.

The key prediction, common to many models of the family, including intergenerational altruism (e.g., Barro, 1974; Becker, 1974) and "family constitutions" (i.e., self-enforcing patterns of intergenerational transfers within families, Cigno, 1993), is that government pay-as-you-go programs trigger offsetting changes in family transfers, reducing net "upstream" family transfers from adult children to their elderly parents. Depending on the strength of this response, the induced changes in family transfers can fundamentally transform the efficiency and distributional effects of the government program. Whereas without family links, expansions of government old-age support confer large windfalls on the "initial old," with strong links such expansions trigger offsetting family transfers that, for reasons discussed below, might even leave the initial old worse off than they would have been without the expansion.[3] Whereas without family links, government old-age support crowds out life cycle saving and the capital stock, with strong links such expansions may have little effect on saving and the capital stock, since they are at least partially neutralized by offsetting changes in family transfers.[4] Whereas without family links, expansions of government old-age support are largely neutral with respect to family size, with strong links such expansions redistribute from families with more to fewer children.[5] Whereas without family links, the

[3]Samuelson (1958) shows how the windfall to the initial old need not come at the expense of younger generations, even without family links, if population and productivity growth are large enough relative to the interest rate, though Diamond (1965) shows that this condition cannot hold in a dynamically efficient economy.

[4]Government old-age support crowds out the capital stock because, although government old-age support substitutes for saving at the level of an individual household, it does not substitute for saving at the level of the economy as a whole, since the revenue raised by taxing workers is directly spent on current retirees, not saved and invested for the future.

[5]With family insurance, the per-child cost of providing a given level of old-age support is decreasing in family size, whereas with government old-age support it is independent of family size, since per-person taxes and benefits are independent of family size. Such intra-generational redistribution is

efficiency effects of government old-age support depend largely on the extent to which it improves consumption smoothing over the life cycle (if myopia or capital market imperfections limit consumption smoothing) or across states of the world with different lifespans (if annuity market imperfections limit consumption smoothing), with strong links the efficiency effects depend largely on the relative "administration costs" of government versus family pay-as-you-go programs.

Both historically and today, shared living arrangements are an important way in which different generations of the same family help each other. Shared living arrangements enable families to economize on the costs of housing and other household-level public goods (e.g., Lazear and Michael, 1980) and may reduce the cost of additional transfers by facilitating greater altruism and monitoring. One important feature of shared living arrangements is that they potentially create an indivisibility: at any given time, the family can either live together or independently, not some of both.[6] This indivisibility has three implications for the effects government old-age support. First, it creates a possibility of overshifting: the induced change in family transfers may be larger than the triggering change in government old-age support. For example, an adult daughter who in the absence of OAA would have taken in her elderly father to support him in his old age might no longer do so if her father receives OAA, and the resulting reduction in family transfers could well be larger than the OAA benefit. Second, the indivisibility raises the likelihood that the incidence of government

limited by the extent to which marriage unites family lines of different sizes. See Bernheim and Bagwell (1988).

[6]This indivisibility may be unimportant for families that wish to live together some of the time and independently at other times, in which case they can effectively convexify over time the non-convexity due to the inability to live both together and independently at a point in time.

old-age support on the adult children of recipients is highly uneven, with the children who live with their elderly parents in at least some states of the world bearing much more of the incidence than their siblings. Third, different living arrangements likely involve different mappings between spending and utility due to economies of scale from household-level public goods. As a result, conditioning benefits on living arrangements could potentially help target higher-marginal utility types of people or states of the world.

A government old-age support program with unconditional cash benefits is likely to affect living arrangements through two main channels. First through income effects, which for reasons discussed above are positive for low-fertility family lines and negative for others. In principle, independent living could be a normal or inferior good, depending on preferences and the context. In practice, independent living is usually assumed to be a normal good for most people, and empirical results are generally consistent with that (e.g., Costa, 1999; McGarry and Schoeni, 2000). That only a minority (22 percent) of elderly individuals received OAA means that most recipient families experienced positive net transfers, which would be expected to increase independent living, to the extent that it is a normal good. The second main channel through which unconditional government old-age support is likely to affect co-residence is by changing desired family transfers, in this case reducing desired net transfers from adult children to their elderly parents. Co-residence is a major way in which such transfers are delivered, so an expansion in government old-age support is predicted to reduce shared living arrangements whose main purpose is to facilitate upstream transfers from children to their elderly parents and to increase shared living arrangements whose main purpose is to facilitate downstream transfers from elderly parents to their children. A potential, likely highly imperfect, proxy for the average direction of transfers embedded in a particular shared living arrangement is reported headship (e.g., whether the co-residing elderly are recorded as being the head of the household or dependents). Another indicator of the direction of transfers might be the relative economic status of the two generations (e.g., if a relatively rich elderly individual lives with a child of his who is relatively poor, this arrangement is more likely to involve a net downstream transfer; see Ruggles, 2007).

Unlike the unconditional cash benefit whose consequences we have so far been discussing in this section, however, OAA benefits depended on an individual's labor earnings and, often, assets, living arrangements, and family characteristics. Most of these means tests seem likely to have implicitly taxed shared living arrangements (with the possible exception of relative responsibility laws, as discussed in the previous section) and so would likely further decrease shared living arrangements above and beyond any effects through income effects and decreasing the demand for upstream family transfers. On the other hand, the fact that OAA significantly reduced labor supply (Fetter and Lockwood, forthcoming) would likely work in the opposite direction, since older people were more likely to co-reside when out of the labor force.

2.4. Data and Empirical Approach

2.4.1. Data

The key data sources in this paper are the full-population microdata from the 1930 and 1940 U.S. Censuses. In the analysis, we focus on men and women aged 55 to 84 living in states in which the OAA eligibility age was 65 in 1939.[7] We further restrict the sample to individuals with non-missing demographic information (age, gender, race, citizenship status, marital status, state of birth, and education).[8] In addition to large sample sizes and precise geographic information, an advantage of the Census is that it allows us to obtain a detailed picture of shared living arrangements. For every household, Census enumerators identified a head of household and established relationships between that person and all other members of the household.

Unfortunately, the Census does not report sufficient information to identify OAA recipients. Therefore, as described in the next section, our empirical strategy consists in testing for differential changes in co-residence rates across states with differential expansions in OAA between 1930 and 1939. Data on OAA programs in 1939 comes from U.S. Social Security Board (1940b), which reports monthly data on total OAA dollar payments and the number of recipients at the state level.[9] Data on OAA programs in 1930 comes from Parker (1936), which reports OAA spending and number of recipients from the inception of then-existing state OAA programs through 1935.[10]

[7] Three states—Missouri, New Hampshire, and Pennsylvania—had an OAA eligibility age of 70 in 1939 but reduced the eligibility age to 65 on January 1, 1940 to meet a requirement to continue receiving federal matching funds. We also exclude Colorado, in which long-term residents became eligible at age 60.

[8] The share of individuals with non-missing demographics is 97.2% and 93.3% in 1930 and 1940, respectively.

[9] The reason we use 1939 OAA features rather than 1940 is that some states changed payment and eligibility levels at the beginning of 1940, shortly before the 1940 Census (which took place in April). To the extent that actual policy followed statutory changes only with a delay, or to the extent that living arrangements do not adjust quickly, 1939 features may be more closely related to outcomes observed in the 1940 Census. In practice, state-level payments per person 65 and older in December 1939 (our main right-hand side variable) and in either March or April 1940 are highly correlated (above 0.99).

[10] Although using OAA data from 1929 would more closely align with the fact that we use 1939 OAA data for 1940, this publication does not report 1929 data for all states. This likely does not matter much in practice, because even those OAA programs that existed in 1930 were quite small.

Neither of these sources contains details on recipients' characteristics. We do, however, have state-level tabulations on new OAA recipients and the payments approved for these recipients for each fiscal year from 1936 through 1940, drawn from research memoranda of the Social Security Board (U.S. Social Security Board, 1939a,b, 1941), which we use to determine how high payments tended to be in states without statutory maximum payments. Information on the features of OAA laws themselves, such as maximum payments and relative responsibility laws, comes from U.S. Social Security Board (1940a).

2.4.2. Empirical Approach

Our key source of variation is heterogeneous changes in state OAA policies between 1930 and 1939. The spirit of our empirical exercise is to compare the evolution of co-residence-by-age profiles across states with differential expansions in OAA over the 1930s, flexibly controlling for time-invariant location-specific factors common across ages as well as time-varying age-specific factors common across locations. Intuitively, absent any differential changes in OAA, we would expect these profiles to evolve in a similar fashion across all states. In contrast, differential changes in OAA across states should prompt co-residence-by-age profiles to diverge after age 65, the OAA eligibility age.[11] As mentioned in the introduction, other modern-day social insurance programs that use age 65 as an eligibility cutoff, including Social Security and Medicare, were

[11] In principle, these profiles could start diverging at an earlier age if there are anticipatory effects.

either small or non-existent at the time, and private pensions were still relatively uncommon. This implies that any “kinks” in the co-residence-by-age profiles after age 65 are mostly likely driven by OAA.

As a summary measure of OAA generosity, we use state-level OAA payments per person aged 65 and older, which capture both variation in recipiency rates and payments per recipients. In order to isolate variation in observed levels of OAA driven by policy rather than population characteristics, we adopt a simulated instruments strategy in the spirit of Currie and Gruber (1996). To simulate payments in 1939, we follow Fetter and Lockwood (forthcoming). Using the earnings distribution among the national population of men aged 60–64 in 1939, the oldest ineligible age group, we simulate OAA payments per person aged 65 and older treating a state’s maximum payment as an income floor and incorporating any earnings disregards.[12] The basic idea is that a state’s maximum payment should be correlated with its typical income floor and not be driven by underlying labor market conditions or population characteristics. For the eight states with no legal maximum payment, we measure variation in income floors using the 99th percentile payment among recipients accepted in fiscal year 1938–39 in each state (based on information in U.S. Social Security Board, 1939b).[13] In all but a few cases, the 99th percentile payment is the same as the legal maximum

[12]The national population we use for each state omits the state itself, although in practice this makes little difference. Earnings disregards existed in only a few states and were always at low levels. For the purposes of the simulated instrument, we impute earnings for the self-employed by drawing from the earnings distribution of wage earners with the same level of education and the same number of weeks worked.

[13]The idea is that with payments equal to the gap between “needs” and “resources,” payments near the top of the distribution tend to reflect payments to individuals with virtually no resources—present in every state—and therefore likely reflect administrative norms or rules. We use the 99th percentile payment rather than the observed maximum payment because the latter could be driven by outliers.

in those states that had legal maxima. Simulating payments in 1930 would be more problematic, as the 1930 Census did not collect information on income. However, even states with laws on the books had very low payments (see Appendix Table B.1), due in part to the lack of federal funding and correspondingly high state and local funding requirements (Fetter, 2017) and also, presumably, to administrative startup costs in the years immediately after laws were passed. Hence, as an approximation, our simulated payment is zero for all states in 1930. The Appendix contains additional details on the construction of the simulated instrument.

One important concern is the potential endogeneity of OAA policies if policy differences across states were correlated with other, unobserved determinants of changes in co-residence. For example, Fetter and Lockwood (forthcoming) document that in 1939, OAA payments per person aged 65 and older (and simulated OAA payments) tended to be greater in higher-income states, suggesting that these states tended to have more generous policies. Our approach here is similar to that of Fetter and Lockwood (forthcoming), who restrict comparisons to counties lying on opposite sides of state borders, but preserves sample size. In the analysis below, we partition the set of all counties into 106 "state border groups," each of which comprises all counties that are closest to a given state border, as measured by the distance from the geographic center of the county. These state border groups are displayed in Appendix Figure B.2. We then restrict comparisons to be within state border groups. In Appendix Table B.2 we show that in a cross-section based on the 1940 Census, both observed and simulated payments are systematically correlated with demographics and income when comparisons are unrestricted, but differences are considerably smaller (and statistically insignificant) when comparisons are restricted to state borders.

Consider an individual i, aged $a \in \{55, \ldots, 84\}$, observed in year $t \in \{1930, 1940\}$, living in state s and county c assigned to state border group b. In our main specification, we estimate equations of the form:

$$y_{iacbst} = \alpha_c + \beta_{abt} + \sum_a \gamma_a \cdot (\text{OAA per-65+ payments})_{st} + \varepsilon_{iacbst} \tag{2.1}$$

where y is an outcome of interest (e.g. indicator for co-residence with relatives). The county fixed effects α_c capture for time-invariant level differences across counties, while the age-border-year fixed effects β_{abt} flexibly control for time-varying age-specific factors common across counties belonging to the same state border group (e.g. age-specific local labor market shocks). For expositional purposes (and precision), we bin age into 5-year age groups (55-59, 60-64, 65-69, 70-74, 75-79, 80-84), and since our policy of interest varies at the state level, standard errors are clustered at the state level. The key explanatory variable, the level of OAA payments per person aged 65 and older in state s and year t, is allowed to have heterogeneous effects by age.[14] We instrument the OAA age interaction terms using our simulated instrument interacted with the same set of age fixed effects. Appendix Table B.3 reports "first-stage" results, where each OAA age interaction term is separately regressed on the full set of instruments, in each gender sample. As can be seen from the diagonals, the simulated OAA payments are

[14] As already mentioned, we use 1930 payments (measured in 1939 dollars) for 1930 and 1939 payments for 1940. In the baseline specification, we use the level rather than the log of OAA payments because most states had zero payments in 1930. In Section 2.5.3, we show that the baseline results are robust to alternative ways of accommodating zeros while also adjusting for the fact that the distribution of OAA payments is right-skewed.

clearly predictive of realized OAA payments, despite using only some of the eligibility and payment criteria. All models are just-identified, so bias from weak instruments is unlikely to be a problem (see, e.g., Angrist and Pischke, 2009).

2.5. Results

2.5.1. Descriptive Features of Co-residence

Before presenting the main results, we first document some descriptive facts on co-residence patterns among older Americans before and during the early expansions of government old-age support. Throughout the paper, we define co-residence with relatives as living with at least one family member (aged 18 or older) other than one's spouse. We also distinguish between two types of co-residence depending on household headship status. In the Census, a single individual is identified as the household head. While the Census enumeration instructions left the definition of a household head somewhat vague, it was likely associated with an individual's economic independence (Costa, 1998; Ruggles, 2007). We therefore distinguish between co-residence as the household head (or spouse of the household head) and co-residence as a "dependent." This allows us to differentiate between situations in which net transfers across generations likely take place from the older to the younger generation (i.e. when parents head the household) and situations in which net transfers likely go in the opposite direction (i.e. when parents are dependents).

Figure 2.1 shows a time series of co-residence over the period 1880-2016, separately for co-residence as a household head and as a dependent. Co-residence as a household head fell in the decades prior to 1930, with the decline slowing over the 1930s before declining rapidly after 1940. The decade from 1930 to 1940 stands out as a period in which co-residence as a dependent fell more rapidly than in the prior half-century (during which it had not decreased for men at all). Notably, the other major period of accelerating decline in co-residence as a dependent coincided with the legislative expansion of Social Security from 1950 onward.

To explore the patterns over the 1930s further, Figure 2.2 plots co-residence rates by age in 1920, 1930, and 1940, separately for men and women. About half of men aged 55 and older, and slightly more than half of women, lived with a relative other than their spouse. At younger ages, the elderly mostly co-reside as household heads, likely reflecting children not having left the household yet. The share of men and women co-residing as household heads falls steadily with age. At the same time, co-residence as a dependent rises with age, consistent with children providing care for their elderly parents, yielding a U-shaped pattern in the overall co-residence-by-age profile for both men and women.[15] Trends in co-residence by age over the period 1920-1940 support the notion that greater resources went to the elderly over the 1930s, displacing support through the family. Indeed, co-residence rates as a dependent changed little between 1920 and 1930, but fell noticeably between 1930 and 1940, coinciding with large expansions in OAA. Moreover, this decline was entirely concentrated at ages 65 and older for men, the most common OAA eligibility age. For women, co-residence rates started

[15] For interpreting these and other facts, it may be helpful to note that in 1940, remaining life expectancy for men reaching age 65 was about 12 years, and for women reaching age 65 was about 13.5 years (Grove and Hetzel, 1968). Appendix Figure B.3 displays population counts by age in 1930 and 1940. High mortality rates at older ages explains the strong decline with age. Also worth noting are the spikes at "round" ages (e.g. 55, 60, etc.), most likely due to reporting errors.

declining at earlier ages, possibly due to their husbands reaching the OAA eligibility age before them.[16]

Most co-residence at older ages—roughly four-fifths—was with children as opposed to other relatives, as shown in Appendix Table B.4. Among elderly women, co-residence with daughters was more common. Breaking down co-residence by household headship status (results not shown) reveals that this was due to a greater likelihood of living with a daughter when co-residing as a dependent. For men, there were only slight differences between co-residence rates with sons and daughters, regardless of household headship status.

Appendix Table B.5 shows that those out of the labor force co-resided at substantially higher rates than those still in the labor force, and were much more likely to co-reside as dependents; these facts have been emphasized by Costa (1998). Notably, for men, who were much more likely than women to be in the labor force, co-residence for those in the labor force fell little between 1930 and 1940, and household headship status when co-residing also changed little. Rather, the most significant reduction in co-residence among men came from those out of the labor force co-residing as a dependent.

Marital status was also an important determinant of co-residence, with co-residence at older ages significantly more common among men and women who were separated, divorced, or widowed (who were mostly widowed) than among those who were still

[16]In 1940, 90 percent of husbands aged 55-84 were older than their wives, and the average age gap was 5.5 years.

married.[17] Appendix Table B.6 shows that, among men, the most significant declines in co-residence between 1930 and 1940 came from older non-married men being less likely to co-reside as a dependent. For women, there were declines in the likelihood of co-residence as a dependent for both married and non-married women, but little decline in the likelihood of co-residing as the household head or the spouse of the household head.

2.5.2. The Effect of OAA on Co-residence Among the Elderly

Table 2.2 shows our main IV estimates of the effect of OAA payments per person 65 and older on co-residence (OLS results are shown in Appendix Table B.7). Figure 2.3 shows corresponding estimates allowing for interactions by individual years of age instead. The results strongly suggest that OAA reduced co-residence with relatives among the elderly. Most of the declines in co-residence occur after age 65, which is consistent with the fact that the OAA eligibility age was 65 in all states included in our sample. For women, the propensity to co-reside starts declining at slightly younger ages, which could be due to their husbands reaching the OAA eligible age before them, as mentioned earlier. At younger ages, around 65 to 74 for men and 60 to 74 for women, lower co-residence rates are largely due to a lower likelihood of co-residing as a household head. At older ages, beginning at around 70 and especially after 75, the key driver is lower co-residence as a dependent. One possible interpretation is that OAA had two separate effects: at younger ages, it allowed children to leave the household earlier

[17] Because of higher mortality rates, men were more likely to be married than women at every age: in 1930, 90 percent of men aged 55-59 were married compared to 73 percent of women aged 55-59, and at ages 80-84, 50 percent of men were married compared to 16 percent of women.

than they would have otherwise, while at older ages, it reduced the likelihood of parents moving in with their children as dependents.

Simple back-of-the-envelope calculations help to convey the magnitude of these effects in the context of the historical decline in co-residence among the elderly. Between 1930 and 1940, total OAA payments per person 65 and older in the United States, expressed in 1940 dollars, rose from 0.26 dollars per year to 52.35 dollars per year. This increase in OAA payments can explain a large share of the observed decline in co-residence between 1930 and 1940. First, we consider co-residence as a dependent. Our estimates imply that OAA can explain about 75 percent of the two percentage point drop in co-residence as a dependent for men 65 and older, and about 37 percent of the 4.8 percentage point drop for women 65 and older. Second, we consider co-residence as a whole (either as a dependent or a household head). For men, the predicted decline in co-residence is 4 percentage points, compared to the observed 3 percentage point drop, suggesting that co-residence overall would have risen over the 1930s in the absence of OAA. For women, the predicted decline in overall co-residence (3.5 percentage points) is about 76 percent of the observed decline (4.6 percentage points). One possible reason why OAA "over-explains" the overall decline in co-residence for men (under the assumption that a linear extrapolation based on the IV estimates is valid) is that other factors were pushing co-residence rates upward during this period, including perhaps poor labor market prospects among younger generations in the aftermath of the Great Depression. Taken together, these calculations suggest that OAA played a central role in falling co-residence rates over the 1930s, especially for men.

As shown in Table 2.3, OAA was associated with declines in co-residence for both married and non-married individuals, though the decline was somewhat more pronounced among non-married individuals, particularly non-married men. Appendix Table B.8 reports results separately for co-residence as a household head and co-residence as a dependent. Particularly among men, the overall decline in co-residence primarily reflects a fall in co-residence as a household head for married individuals, and a fall in co-residence as a dependent for non-married individuals. The fact that co-residence as a dependent sharply declines among non-married men and women is broadly consistent with the notion that these individuals—who were mostly widowed—more heavily relied on family members for support, and that OAA enabled them to live independently. That OAA reduced co-residence as a dependent among widowed women during the 1930s reinforces the conclusion of Costa (1999) about OAA and Social Security's important role in reducing these living arrangements, based on OAA expansions during the 1940s.

Having documented that OAA reduced co-residence for the elderly over this period, we take a first step toward shedding light on OAA's "indirect" effects on family members of the elderly by examining what types of co-residence arrangements are diminished by OAA. Table 2.4 decomposes the main co-residence effects into co-residence with children and co-residence with other relatives. Children implicitly refers to own (biological) children, while other relatives include children-in-law, grandchildren, siblings, parents, aunts/uncles, nieces/nephews, and cousins, among others. Co-residence with children means that at least one child, son or daughter, is present in the household. Co-residence with other relatives means that at least one other family member is present in the household, but no children. These two types of co-residence are therefore mutually exclusive by construction.

The results in Table 2.4 suggest that most of the co-residence effects are driven by a decline in co-residence with children as opposed to other family members, as might be expected given that co-residence was most often with children. Table 2.5 separately looks at co-residence with sons and co-residence with daughters.[18] The estimates in columns (1)-(6) suggest that for men, most of the decline in co-residence as a household head comes from co-residence with sons, while most of the decline in co-residence as a dependent comes from co-residence with daughters. As noted above, for elderly men co-residence rates with sons and daughters were similar, regardless of household headship status, suggesting that OAA had different effects on different types of co-residence arrangements. Elderly women exhibit patterns similar to elderly men, but it is worth noting that the decline in co-residence as a dependent among elderly women is driven both by co-residence with sons and co-residence with daughters (though mostly one or the other depending on age, with suggestive evidence that co-residence as a dependent with sons saw larger decreases in response to OAA, despite being less common for elderly women than co-residence with daughters).

To shed further light on the mechanisms through which OAA reduced co-residence, we estimate regressions separately for states with and without relative responsibility laws. As noted in Section 2.2, these laws likely changed the degree to which family transfers through co-residence were taxed by the means tests of OAA. In Appendix

[18]For simplicity, we do not define co-residence with sons and daughters to be mutually exclusive.

Table B.9, we estimate equation (2.1) separately for states with and without relative responsibility laws. To ease comparisons across samples, we plot these estimates and the baseline estimates from Table 2.2 together in Figure 2.4. A majority of states had relative responsibility laws so that in some specifications the estimates are quite imprecise due to the smaller number of comparisons. But for co-residence overall, the patterns are broadly similar across all states. For co-residence as a dependent, particularly for men, the results seem to be driven largely by states with relative responsibility laws. The suggestive evidence here that states with relative responsibility laws saw similar declines in co-residence due to OAA, and perhaps larger declines, is in contrast to the findings of Costa (1999). Studying widowed women between 1940 and 1950, she finds that the OAA-induced decline in co-residence was concentrated in states without relative responsibility laws. To the extent that states with these laws had less implicit taxation of co-residence relative to separate living arrangements, these results suggest that the income transfer component of OAA was important in reducing co-residence over the 1930s.

2.5.3. Robustness

The results are robust to a range of alternative empirical choices. First, although we measure OAA in levels in our main specification to accommodate the fact that most states had no OAA payments in 1930, alternative ways of handling zero values yield similar results. Appendix Table B.10 reports results in which we apply the log transformation $\log(1+x)$ to our measure of OAA payments (and simulated OAA payments). The resulting estimates are qualitatively similar to the baseline estimates in Table 2.2.

Our main specification restricts comparisons to counties within the same state border group in order to compare areas more likely to be similar in terms of unobservables (due to geographical proximity) but with different OAA policies. This may be a reasonable assumption for adjacent counties on either side of a state border, but may be less so for counties that lie further apart. For example, some counties in the peninsula of Florida are arguably quite far from the border with Georgia. To assess whether our comparisons are undermined by the inclusion of counties that are distant from state borders (which was motivated by statistical precision), we show results that rely on narrower geographic comparisons. We calculate the 90th, 75th, and 50th percentiles of the distribution of distances from the center of counties to the nearest state border—these distances are 162, 102, and 56 kilometers, respectively—and in Appendix Table B.11 we show results restricting the sample to counties located within each of these distance thresholds. Maps showing these alternative state border groups are displayed in Appendix Figure B.4. In practice, the inclusion of counties relatively far from state borders does not appear to bias our results: the estimates are remarkably stable across all samples.

Finally, to bolster our interpretation of the results, we test for differential trends in co-residence across states *prior* to the introduction of most OAA programs. In Appendix Table B.12, we report estimates from a variant of equation (2.1) where we replace data from 1930 and 1940 with corresponding data from 1920 and 1930, but keep using OAA data from 1930 and 1939 for the key OAA age interaction terms. The idea is to test whether changes in OAA payments between 1930 and 1939 are predictive of changes in co-residence patterns between 1920 and 1930. For this exercise, we exclude the eight states that had positive OAA payments in 1930 (see Appendix Table B.1). The results are encouraging. Across all age groups and dependent variables, the coefficients for men are generally small and not statistically significant at conventional levels. For women, although some coefficients (such as those on co-residence with relatives as a dependent) are similar in magnitude to the main estimates, they are mostly not statistically significant. Taken together, these results do not suggest that states that would eventually have higher levels of OAA were trending differentially prior to the introduction of OAA.

2.6. Conclusion

Many of the most important government programs transfer resources to older people. In this paper, we investigate the effects of the Old Age Assistance program on intergenerational co-residence in 1940. OAA was a large source of government old-age support at the time—nearly one quarter of all individuals 65 and older received OAA in 1940—and it helped pave the way for many of the important social insurance programs of the present day. Even independent of its historical importance, OAA presents a valuable opportunity for learning about the effects of government old-age support programs, since unlike many modern programs, it varied significantly across states and across otherwise-similar groups of people within states. The recent availability of Census data on the full U.S. population in 1940 makes studying OAA a particularly fruitful way to shed light on the effects of these programs.

Our results suggest that OAA reduced intergenerational co-residence among both elderly men and elderly women in 1940 significantly, enough to explain most or all of its aggregate decline between 1930 and 1940. The effects of OAA on living arrangements suggest that at least some of the incidence of OAA was on the adult children of OAA recipients. In ongoing work, we are taking advantage of the ability to link individuals over time across Censuses (building on the work of Feigenbaum, 2016; Bailey et al., 2017; Abramitzky et al., 2018, among others) to investigate the effect of OAA on recipients' adult children and their families, especially their geographic mobility and labor supply. Linking family members will also enable us to explore heterogeneity across and within families (e.g. based on income, proximity), shedding further light on the nature of family links and the effects of government old-age support.

CHAPTER 3

The Effect of Skill Mismatch on Early Career Outcomes of College Graduates: Evidence from Online Job Postings[1]

3.1. Introduction

A number of studies have documented the long-term impact of graduating from college during times of high unemployment (Kahn, 2010; Oreopoulos et al., 2012; Altonji et al., 2016). Cohorts who enter the labor market during downturns are more likely to be unemployed, more likely to start their career in worse jobs, and earn lower wages on average compared to cohorts who face more favorable economic conditions at entry. While these initial differences typically dissipate over time, some of them can persist for many years after graduation. These findings illustrate the cost of recessions for individual workers, and contribute to our understanding cross-cohort income inequality by highlighting the role of initial labor market conditions. They also shed light on the forces underlying career dynamics. In particular, the speed and the extent to which individuals who graduate during bad times are able to catch up with their more fortunate counterparts—as well as the channels through which they bridge the

[1]This chapter is joint work with Enrico Berkes and Bledi Taska. This research was supported in part through the computational resources and staff contributions provided for the Quest high performance computing facility at Northwestern University.

gap (e.g. by changing jobs versus on the job)—provide important clues that can inform various theories of career progression.[2]

Another important finding is that average effects of graduating during a recession mask substantial heterogeneity across college graduates. Notably, studies have found large disparities in the size and persistence of these effects across graduates from different fields of study. Graduates with high-paying majors tend to fare better than graduates with low-paying majors, both in the short-run and the long-run. While the differential impact of downturns across college majors could in principle reflect differential exposure, and subsequent response, to a common shock, it seems likely that graduates with different skills actually face different labor market conditions at entry. After all, there are not only large earnings differentials across college majors (Altonji et al., 2012), but there is also evidence that these gaps exhibit substantial variation across time (Altonji et al., 2014) and space (Phelan and Sander, 2017).

In this paper, we directly explore this possibility by studying how *skill-specific* initial labor market conditions affect early career outcomes of college graduates. To answer this question, we exploit data on the near-universe of online job postings in the U.S. since 2010 and construct a new measure of "skill mismatch," which essentially captures how well the skills that are embedded in college majors match the skills that are demanded by local employers in a given city and given year. Intuitively, college graduates with a specific major experience skill mismatch when only a small fraction of job openings in their local labor market are suitable for their major in the year that

[2]Leading examples include job search models, models of human capital accumulation, employer learning models, and models of long-term wage contracts.

they graduate.[3] For instance, a finance major who graduated in Detroit in 2013 will have experienced skill mismatch if there were relatively few job openings for financial occupations in that area at the time. Our empirical strategy consists in comparing individuals who faced different initial labor market conditions, as measured by skill mismatch, based on when and where they graduated, but also what field they majored in. This additional layer of variation allows us to control for cohort-location-specific factors, and implicitly compare individuals who faced the same overall labor market conditions, but whose skills were more or less in the demand when they graduated.

We find that skill mismatch leads to worse initial outcomes for college graduates: they are more likely to be unemployed or employed in a part-time job, less likely to be employed in an occupation that typically requires a college degree, less likely to be employed one of the top occupations for their college major, and they earn lower wages. In terms of magnitude, a one standard deviation increase in our skill mismatch measure—roughly equivalent to the average difference between a major in music and drama and a major in physics, or alternatively the difference between having a STEM degree in Providence, RI as opposed to San Francisco, CA in 2016—leads to a 0.4 percentage point increase in the probability of being unemployed, a 0.8 percentage point increase in the probability of being employed in a part-time job, a 1 percentage point decrease in the probability of being employed in a college occupation, a 1.8 percentage point decrease in the probability of being employed in one of the top 5 occupations by

[3] In our baseline definition, we quantify the suitability between majors and occupations using occupational employment shares by college major, but we will also show alternative results based on college major wage premiums by occupation.

college major, and a 3 percent decline in hourly wages among individuals 1-2 years out of college.

While the effects on unemployment, part-time employment and employment in college occupations gradually fade over time, the effects on wages and major-occupation fit persist up to 6 years after graduation. These medium-run effects are substantial: the wage and major-occupation fit penalties associated with a one standard deviation increase in initial skill mismatch are 2.6 percent and 1.6 percentage points respectively among individuals 5-6 years out of college. Focusing on wages, we also find that low-paying majors are more sensitive to skill mismatch than high-paying majors, and that the medium-run effects are largely driven by initial skill mismatch rather than skill mismatch experienced in subsequent years. This last result, combined with the persistent effect of skill mismatch on major-occupation fit, suggests that early career human capital accumulation plays an important role. All in all, our findings highlight the importance of having the right skills in the right place at the right time.

As mentioned already, our paper is closely related to the literature on the long-term effects of initial labor market conditions, as measured by local or national unemployment rates. Analyzing cohorts of white males in the National Longitudinal Survey of Youth who graduated from college during the 1980s, Kahn (2010) finds that poor initial economic conditions have a negative and persistent effect on wages. Using Canadian administrative data covering a large number of cohorts of male college graduates, Oreopoulos et al. (2012) find similar, though less persistent, effects on earnings. Exploiting the richness of their university-employer-employee linked data, they also document heterogeneity in the speed and nature of the recovery process across more or less disadvantaged college graduates. College graduates with high predicted earnings, based on their major and the school they attended, fully recover within a few years, mostly through job mobility. On the other hand, college graduates with low predicted earnings never fully recover, and their recovery mostly takes place within the firm. Liu et al. (2016) show using administrative data from Norway that early career "skill mismatch"—which in their study refers to college graduates obtaining jobs in industries that are ill-suited for their college major—is an important driver of both the short-term and long-term earnings effects of graduating during times of high unemployment. More recently, Altonji et al. (2016) reexamine the long-term impact of initial labor market conditions in the U.S. using pooled data on cohorts who graduated between 1974 and 2011. They find similar patterns of negative wage effects at entry that gradually fade over time, and show that high-paying majors are less affected than than low-paying majors.

While the findings in this paper might seem similar, it is important to emphasize that our estimates are effectively *net* of the impact of initial labor market conditions common across college majors (both local and national), including unemployment rates. In that sense, our findings are fundamentally distinct from those in the existing literature. To show how this leads to new insights, we revisit the effects of initial unemployment rates in our sample, and find that they are less persistent and poor predictors of major-occupation fit.

Given that the term "skill mismatch" has been used by others in different ways, it is also worth distinguishing our concept of skill mismatch with alternative ones in the literature. Perhaps the most common notion of skill mismatch is one of match quality between workers and their current occupation, based on the similarity between workers' ability profile and the task content of their occupation. Various studies have explored how skill mismatch affects wage growth and job mobility over the life cycle (Guvenen et al., 2015; Lise and Postel-Vinay, 2016; Fredriksson et al., forthcoming). Another related concept is "mismatch unemployment," which refers to a misallocation between job seekers and vacancies (Şahin et al., 2014; Marinescu and Rathelot, forthcoming). More specifically, mismatch unemployment occurs when a planner could theoretically reduce aggregate unemployment by reallocating workers across segments of the economy, as defined by locations, industries or occupations (or some combination of the three). Most closely related to our paper is the concept of "skill remoteness" introduced in Macaluso (2017), which refers to a mismatch between the skill profile of recently laid-off workers and local labor demand. However, whereas we infer workers' skills based on their college major (i.e. skills acquired in school), Macaluso (2017) infers workers' skills based on the task content of their previous occupation (i.e. skills acquired on the job). Moreover, local labor demand is proxied using an area's occupational structure, whereas we directly measure the skills demanded by local employers using the occupational composition of online job postings.

The remainder of the chapter is organized as follows. We begin by describing the data sources used in the empirical analysis in Section 3.2. In Section 3.3, we outline our empirical strategy. In Section 3.4, we present our main results. Section 3.5 contains a broader discussion of the results. Finally, Section 3.6 concludes.

3.2. Data

3.2.1. Burning Glass Technologies

The primary data source in this paper is a database of online job ads provided by Burning Glass Technologies (BGT), an employment analytics and labor market information firm. Burning Glass maintains a database covering the near-universe of online job postings in the U.S. by regularly scraping information from over 40,000 online jobs boards and company websites, providing a real-time snapshot of the labor market. Online job postings data is widely used by state and local workforce agencies, higher education institutions and employers to learn about the latest trends in the labor market, complementing more traditional sources on vacancies such as the Job Openings and Labor Turnover Survey (JOLTS) maintained by the Bureau of Labor Statistics.[4] More recently, these data sources have been increasingly used in academic research, for example to study routine-biased technological change over the business cycle (Hershbein and Kahn, forthcoming), the returns to skill requirements (Deming and Kahn, 2018), and monopsony in the labor market (Azar et al., 2017, 2018).

Figure 3.1 plots the total number of online job postings in Burning Glass and the total number of job openings in JOLTS at a quarterly frequency since 2010. Although there is a significant level difference between these two series—which is partly due to how job openings and job postings are defined—they otherwise track each other quite closely over time.[5] There is however a key difference between Burning Glass and

[4]JOLTS is a nationally-representative survey of roughly 16,000 randomly-sampled establishments conducted each month, and not only measures vacancies but also hires and separations.

[5]JOLTS defines *active* job openings as positions that are open on the last business day of the month, could start within 30 days, and are subject to active recruiting efforts. BGT identifies *new* job postings using a 60-day window tolerance. That is, during the data collection process, any job posting is flagged

JOLTS. Because not all jobs are posted online, the distribution of online job postings is not necessarily representative of actual distribution of vacancies in the economy. Perhaps unsurprisingly, jobs that are posted online tend to be higher-skill jobs. Carnevale et al. (2014) estimate that, while between 60 and 70 percent of all jobs are posted online, the coverage for jobs that require at least a Bachelor's degree is closer to 80-90 percent.

Table 3.1 compares the industry composition in JOLTS and Burning Glass. Clearly, some industries are overrepresented in Burning Glass, most notably manufacturing, finance, education, and healthcare. Other industries, which tend to employ a greater number of low-skill workers, are underrepresented (e.g. construction, accommodation and food services, government). Since occupations are not available in JOLTS, Table 3.2 compares the occupational composition of online job postings in Burning Glass to the occupational composition of employment in the American Community Survey in terms of 2-digit Standard Occupational Classification (SOC) codes. Consistent with the previous table, some occupations are overrepresented in BGT, including business and financial occupations, computer and math occupations, and healthcare occupations, while others are underrepresented, such as education occupations, construction occupations, and production occupations. The fact that Burning Glass is more representative of college-type jobs is actually convenient for our setting since we are primarily interested in the job opportunities that college graduates face when they enter the labor market.

One major advantage of Burning Glass over JOLTS is the size and scope of the data. Burning Glass contains over 145 million unique job postings covering the period

as a duplicate if it has the same characteristics as one which was originally identified less than 60 days ago.

2010 to 2016. For each job posting, Burning Glass extracts a variety of information, including the employer, the occupation, the industry, the location, and various job requirements (e.g. education, experience). The granularity of the data allows us to get a more detailed picture of labor demand and conduct analyses at a very fine level, both in terms of geography and sectors of the economy. For the purpose of this study, we only exploit the occupation associated with the job (6-digit SOC), the Metropolitan Statistical Area (MSA) in which the job is to take place, as well as the year in which the job ad was posted. As we describe in Section 3.3.1, we use this information to compute the occupational composition of online job postings for every MSA-year pair, which will serve as our proxy for the types of skills demanded by local employers when we construct our skill mismatch measure. Despite its richness, one drawback of Burning Glass is that it only goes as far back as 2010, which effectively restricts the number of graduating cohorts we are able study in the analysis.[6]

3.2.2. American Community Survey

To measure employment outcomes of college graduates, we use data from the 2010-2016 American Community Surveys (ACS) 1% samples (Ruggles et al., 2017). The ACS is a large-scale household survey of the U.S. population, and contains detailed information on respondents' demographic characteristics, employment status, income and geographic location. Crucially for our purposes, since 2009 the ACS has asked respondents who hold a 4-year Bachelor's degree about their college major. In this paper, we will use college majors as a summary measure of the skills that individuals

[6]Data is technically available for 2007, but our analysis relies on knowing individuals' college major, which is only available in the American Community Survey since 2009.

have acquired while in school. College majors are typically associated with a specific curriculum and therefore a specific set of skills. As certain skills tend to be valued in certain jobs more than others, college majors implicitly contain useful information regarding the set of suitable occupations for a given college graduate. For example, nursing majors presumably possess the necessary skills to become registered nurses or enter related healthcare occupations. One important advantage of college majors over alternative measures of skills, such as current/previous occupations or ability profiles, is that they are available for a large sample of the population, regardless of current or past employment status. This means we can not only look at individuals who have never held a job before, but the large sample sizes in the ACS enable us to fully exploit the granularity of the Burning Glass data.

In the analysis, we focus on college graduates with 1 to 6 years of potential experience, defined as the *estimated* number of years since graduation (see below), who graduated between 2010 and 2015, either hold a Bachelor's or Master's degree, are not currently enrolled in school, and currently reside in one of the 294 MSAs that are identifiable in the ACS.[7] The potential experience and graduation year restrictions are dictated by the fact that Burning Glass only goes as far back as 2010, and the fact that 2016 is the latest ACS release.[8] We exclude individuals with 0 years of potential experience since the reference period for the income question in the ACS is the past 12 months (relative to the time of the survey), so that income for those individuals could

[7]In order to focus on the non-institutional civilian population, we exclude from the sample: (1) individuals confined to institutional group quarters, (2) unpaid family workers, and (3) individuals on active military duty.

[8]Note that our sample is unbalanced in terms of years of potential experience: in 2011, we only observe the 2010 graduation cohort; in 2012, we observe the 2010 and 2011 graduation cohorts, and so on.

potentially reflect income earned while in school. We exclude individuals with professional or doctoral degrees for two reasons: (1) program duration—and hence year of graduation—is harder to determine for those individuals (see below), and (2) the ACS only measures undergraduate fields of study, which might be a worse proxy of skills for people with graduate degrees.[9] Lastly, we focus on MSAs since urban areas have better coverage in the Burning Glass data. Given that most college graduates live in MSAs anyway (roughly 85 percent), this choice only results in a minimal loss of sample size. In Section 3.4.6, we show that we obtain very similar results using states as the unit of geography instead.[10]

The main limitation of the ACS is that it is mostly intended to capture the current situation of respondents, and therefore contains little information on past experiences. In our context, this means we cannot know for sure when and where individuals graduated from college. Following Altonji et al. (2016), we impute MSA at graduation using current MSA of residence, and approximate the year of graduation using the year individuals were most likely aged 22 in May, which is year of birth plus 22 for individuals born in the first two quarters of the year and year of birth plus 23 for everyone else. For individuals who hold a Master's degree, year of graduation is year of birth plus 24 or 25 depending on quarter of birth given that Master's programs typically last two years.

Appendix Table C.1 displays basic summary statistics for college graduates with 1 year of potential experience. Recent college graduates are more likely to be female

[9]The results are robust to excluding individuals with Master's degrees as well (see Section 3.4.6).

[10]We could also use the concept of commuting zones (CZ) to approximate local labor markets, but one drawback is that the ACS does not allow you to uniquely identify the CZ of residence for individuals living in sparsely populated areas.

than male (57 percent), are majority non-Hispanic whites (67 percent in 2016, down from 73 percent in 2011), and roughly one out of five graduate also holds a Master's degree. We organized college majors in the ACS into 56 categories, loosely following the classification in Altonji et al. (2016). Appendix Table C.2 displays college major shares for college graduates with 1 year of potential experience. In 2016, the five most common majors were "Psychology," "Communications," "Biological sciences," "Computer science and IT," and "Accounting." The composition of college majors has been fairly stable since 2011, with the largest gains occurring in "Computer science and IT" and "Fitness, nutrition, and leisure," and the largest losses occurring in "Business management and administration" and "Elementary education."

3.3. Empirical Strategy

3.3.1. Measure of Skill Mismatch

The key novelty in this paper is how we measure initial skill-specific labor market conditions. We introduce a new measure of skill mismatch, which is meant to capture how well the skills that are embedded in college majors match the skills that are demanded by local employers in a given city and given year. Essentially, skill mismatch is low for a particular college graduate when there are many suitable job opportunities for someone with her college major in her local labor market in the year she graduates, and high otherwise. As an example, consider an individual who graduated from college in 2010 in Chicago with a major in accounting. Because many accounting majors go on to become accountants, this individual presumably had good job prospects if accountants happened to be in high demand in Chicago in 2010. Of course, accountant is not the only suitable occupation for accounting majors, many of them are employed as financial managers for example. However, few of them end up being employed as chemists. Therefore, the relative number of vacancies in Chicago in 2010 for accountants or financial managers versus chemists is ultimately what determines the extent to which this accounting major experienced skill mismatch at graduation.

In practice, our measure combines two ingredients: (1) a metric of fit between each college major and each occupation, and (2) the occupational distribution of online job postings, which is MSA and year-specific. Formally, skill mismatch for college major m in MSA ℓ at time t is defined as one minus the weighted average of the match coefficient between major m and every occupation k, where the weights correspond to the share of online job postings in MSA ℓ at time t that are for occupation k:[11]

(3.1)

$$\text{skill mismatch}_{m\ell t} = 1 - \sum_k \text{share of job postings}^{\text{BGT}}_{\ell t}(\text{occ}_k) \times \text{match}(\text{major}_m, \text{occ}_k)$$

To determine the extent to which a particular occupation k is suitable for college major m, our baseline definition uses the share of workers with college major m that are employed in occupation k nationally, based on pooled 2009-2016 ACS data:

$$\text{match}(\text{major}_m, \text{occ}_k) = \left. \frac{\text{emp}(\text{occ}_k)}{\sum_k \text{emp}(\text{occ}_k)} \right| \text{major}_m \tag{3.2}$$

The underlying assumption is that occupation k is a good fit for major m if a large share of workers with major m are employed in occupation k, a kind of "revealed preference"

[11]We use 4-digit SOC codes as our concept of occupations to compute skill mismatch because it is the most granular classification available in both the ACS and Burning Glass (109 distinct codes).

argument.[12] In Section 3.4.6, we will show results using an alternative definition of skill mismatch which uses college major wage premiums by occupation instead.

To facilitate the interpretation of our skill mismatch measure, we normalize it to have a mean of zero and a standard deviation of one across all college majors, MSAs and years. Table 3.3 shows average skill mismatch by college major between 2010 and 2016. In general, high-paying majors such as those in health and STEM fields seem to be in high-demand and are characterized by low skill mismatch (see Appendix Table C.3 for a ranking of majors in terms of wages). In contrast, low-paying majors, such as those in arts and humanities are characterized by high skill mismatch. Of course, these differences are partly due to the fact that certain jobs are overrepresented in Burning Glass. However, as explained in the next section, our empirical strategy will control for national differences in skill mismatch across college majors, and instead rely on within-MSA cross-major variation and within-major cross-MSA variation in skill mismatch. To get a sense of the cross-sectional variation in skill mismatch, Appendix Figure C.1 plots average skill mismatch by MSA (across all college majors), and Appendix Tables C.4-C.7 provide summary statistics on the distribution of average skill mismatch by college major group across MSAs for the four largest major groups: "Science, math, and technology," "Business," "Social sciences," and "Arts and humanities." The key takeaway from these tables and figures is that there is a tremendous amount of variation in skill mismatch, but that most of it is across college majors and across MSAs rather than over time.

[12] To avoid any mechanical correlation between our measure of skill mismatch and the outcomes of interest, we restrict the sample to individuals aged 32 or older to compute employment shares (college graduates with a Master's degree and 6 years of potential experience are at most 31 according to our definition).

While the intuition behind our concept of skill mismatch is clear, one might wonder whether it truly captures something meaningful about skill-specific labor market conditions. As suggestive evidence, we show that it is correlated with two important labor market outcomes, unemployment rates and mean hourly wages, both across college majors and across MSAs. Appendix Figures C.2 and C.3 plot average skill mismatch by college major against corresponding national unemployment rates and mean hourly wages, where the latter are based on recent college graduates aged 22-31. In every year, skill mismatch is positively correlated with unemployment rates and negatively correlated with mean hourly wages. In other words, majors characterized by high skill mismatch are also characterized by high unemployment rates and low wages, as we might have expected. Next, Appendix Figures C.4 and C.5 plot average skill mismatch by MSA against corresponding unemployment rates and mean hourly wages, separately for each of the four largest college major groups. Within each major group, skill mismatch is positively correlated with MSA-specific unemployment rates, and negatively correlated with MSA-specific mean hourly wages. Overall, these figures demonstrate that skill mismatch is predictive of adverse labor market outcomes, both when comparing individuals with different college majors and individuals with the same major but located in different cities. It is worth noting that there seems to be a tighter relationship between skill mismatch and wages than between skill mismatch and unemployment rates. We now turn to the empirical strategy, which allows us to explore the effect of skill mismatch on labor market outcomes in a more formal regression framework.

3.3.2. Empirical Specification

Our empirical strategy follows the literature on the long-term impact of graduating during a recession in that we compare outcomes of individuals who faced different initial labor market conditions, as measured by skill mismatch, depending on when and where they graduated, and allowing the effects to vary with potential experience. However, a unique feature of our context is that skill mismatch, unlike unemployment rates, also varies by college major. This allows us to exploit within-MSA cross-major variation, and implicitly compare individuals who graduated in the same city at the same time and therefore faced the same overall labor market conditions, but experienced different skill-specific labor market conditions by virtue of having different college majors.

Formally, consider an individual i with potential experience $e \in \{1, \ldots, 6\}$, residing in MSA ℓ at time $t \in \{2011, \ldots, 2016\}$, who graduated from college in year $g \in \{2010, \ldots, 2015\}$ with a major in m.[13] Our main empirical specification is given by:

$$y_{iem\ell gt} = \alpha_{mg} + \gamma_{\ell g} + \lambda_t + \varphi_e + \beta_e \cdot \text{skill mismatch}_{m\ell g} + \theta \cdot X_{it} + \varepsilon_{iem\ell gt} \tag{3.3}$$

where λ_t are year fixed effects, φ_e are potential experience fixed effects, and X_{it} are individual-level control variables.[14] For expositional purposes, the potential experience fixed effects β_e are combined into three groups: 1-2 years, 3-4 years, and 5-6 years (the main potential experience fixed effects φ_e are left in individual years). The

[13] The ACS asks respondents to list a primary and secondary field of study. Throughout the paper, college majors refer to primary field of study.

[14] Individual-level controls include a female indicator, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having Master's degree and an indicator for being a double major.

major-cohort fixed effects α_{mg} control for major-specific labor market conditions at the time of graduation that are common across MSAs. Among others, these fixed effects capture cross-major level differences in skill mismatch that may stem from the non-representativeness of Burning Glass in terms of occupations. The MSA-cohort fixed effects $\gamma_{\ell g}$ control for MSA-specific labor market conditions at the time of graduation that are common across college majors. In particular, these fixed effects absorb local overall unemployment rates, which have been the focus of the literature so far.

The ability to interpret β_e as the causal effect of skill mismatch hinges on several assumptions. First, we need to assume that students do not strategically graduate in years when demand for their major is high. This concern is somewhat alleviated by the fact that we assign skill mismatch based on *predicted* year of graduation rather *actual* year of graduation. Furthermore, studies have concluded that selective timing of graduation is unlikely to be a major factor (Oreopoulos et al., 2012; Liu et al., 2016; Altonji et al., 2016). Second, we need to assume that students do not self-select into college majors which they anticipate to be in high demand in the future *in their local labor market*. There are a two key reasons why this might be a reasonable assumption: (1) students would need to correctly anticipate (local) labor demand conditions 4 years in advance, which seems unlikely in light of the evidence on students' expectations regarding future earnings (Betts, 1996; Arcidiacono et al., 2012; Wiswall and Zafar, 2015b), and (2) studies have shown that, while future labor market prospects do matter, individual preferences and expected performance probably play a more prominent role in driving college major choices (Arcidiacono, 2004; Beffy et al., 2012; Stinebrickner and Stinebrickner, 2014; Wiswall and Zafar, 2015a).[15] Third, we need to assume that college graduates do not sort into MSAs based on local labor demand conditions. Although some studies offer encouraging evidence in other contexts (Oreopoulos et al., 2012; Liu et al., 2016), this is probably the biggest concern for our interpretation of the results, exacerbated by the fact that we cannot observe where people went to college. In Section 3.4.6, we try to address this concern in several different ways. Finally, on the labor demand side, we also need to assume that job postings are exogenous to the supply of college graduates, both in terms of timing and occupations. However, it seems reasonable to conjecture that employers post vacancies primarily based on their business needs at the time, and that there are few substitution possibilities between different occupations.

3.4. Results

3.4.1. Employment, Unemployment and Labor Force Participation

In Table 3.4, we start by looking at how skill mismatch affects the probability of being employed, unemployed or out of the labor force. We also split employment into part-time and full-time work, where part-time is defined as working less than 35 hours a week. The first row shows that skill mismatch has a negative effect on the probability of being initially employed and a positive effect on the probability of being initially unemployed. In terms of magnitude, a one standard deviation increase in skill

[15] Relatedly, there is evidence that college major choice varies over the business cycle (Blom et al., 2017), though it is unclear whether these findings extend to local skill-specific labor market conditions.

mismatch—which is roughly equivalent to the average difference between a major in music and drama and a major in physics (see Table 3.3), or alternatively the difference between having a STEM degree in Providence, RI as opposed to San Francisco, CA in 2016 (see Appendix Table C.4)—reduces the probability of being employed by 0.6 percentage points and raises the probability of being unemployed by 0.4 percentage points for individuals 1-2 years out of college. Columns (2) and (3) show that these effects reflect a simultaneous decline in full-time employment and (smaller) rise in part-time employment.

The second and third rows reveal that, while the positive effects on part-time employment and unemployment gradually fade over time (i.e. with experience), the negative effect on employment remains roughly constant. As a result, for individuals with 5-6 years of potential experience, the negative employment effect is associated with a greater probability of being out of the labor force rather than being unemployed.

3.4.2. Occupations

Next, we explore the effect of skill mismatch on occupations. Poor initial labor market conditions may not only increase the risk of not finding a job among young college graduates, but also force some of them to settle for worse jobs. This is a common finding in the literature (e.g., Oreopoulos et al., 2012). In Table 3.5, we examine the effect of skill mismatch on the probability of being employed in an occupation that typically requires a college degree and, following Altonji et al. (2016), the probability of being employed in one of the top 5 or top 10 occupations by college major, conditional on being employed.

We define "college" occupations in two complementary ways: (1) based on educational requirements in the Department of Labor's Occupational Information Network (O*NET) database (Abel et al., 2014), or (2) based on education levels observed in the ACS (Clark et al., 2016). In the O*NET version, we define college occupations as those for which a majority of respondents in the O*NET surveys indicated that they require one.[16] In the ACS version, we define college occupations as those for which the most common education level among incumbent workers is a Bachelor's degree or more. The top 5 or top 10 occupations by college major are determined based on employment shares in the ACS. As in Section 3.3.1, for each college major, we compute the occupational distribution among workers who hold that major, and identify the 5 or 10 most common jobs. Appendix Table C.8 lists the top 3 occupations for each college major.[17]

As can be seen from Panel D in Appendix Table C.1, about two thirds of college graduates are initially employed in college occupations, while respectively 35 percent and 45 percent of recent college graduates are employed in one of the top 5 and top 10 occupations associated with their college major. Appendix Table C.3 shows that there is a tremendous amount of variation in these outcomes across college majors, with high-paying majors typically faring better than low-paying ones. For example, 86 percent of electrical engineering majors start their career in a college job, and 62 percent of them

[16]For each occupation, O*NET surveys incumbent workers and occupational experts to understand the nature of the job, including educational requirements. Rather than a unique education level, O*NET reports the distribution of responses (e.g. 55% Bachelor's degree and 45% Associate's degree).

[17]The only difference with Section 3.3.1 is how we classify occupations: to compute occupational outcomes in this section we the use Census occupational classification from Dorn (2009) instead of 4-digit SOC. It is worth noting that "Managers and administrators, n.e.c." happens to be the most common occupation in the U.S. under this scheme (around 5% of employment), which is why it is one of the top occupations for many college majors.

in one of the top 5 occupations. In contrast, the corresponding numbers for psychology majors are 59 percent and 20 percent respectively.

The estimates in Table 3.5 show that skill mismatch has negative effect on the probability of being initially employed in a college occupation or one of the top occupations by college major. A one standard deviation increase in skill mismatch reduces the probability of being initially employed in a college occupation by 1 percentage point, and reduces the probability of being initially employed in a top 5 or top 10 occupation by 1.8 percentage points. However, while the effect on college employment is half as large 5-6 years after graduation, the effect on major-occupation fit does not diminish over time. The fact that workers who experience initial skill mismatch are more likely to be "stuck" in occupations that do not fit their college major could potentially reflect human capital depreciation, a point we return to in Section 3.5.

3.4.3. Earnings and Wages

We now turn to the effect of skill mismatch on earnings and wages. Hourly wages are computed by dividing annual wage income by the product of weeks worked last year and usual hours worked per week. Nominal income and wages are then converted into 2014 dollars using the Personal Consumption Expenditures chain-type price index released by the Bureau of Economic Analysis (BEA). In addition, we adjust income and wages for cost-of-living differences across MSAs using the BEA's Regional Price Parity index. Finally, we winsorize the distribution of real earnings and real wages at the top and bottom percentiles separately by year to neutralize the influence of outliers.

Table 3.6 shows the effect of skill mismatch on log earnings and log wages, conditional on having positive income. Skill mismatch has a negative and lasting impact on income and wages. In the first two years after graduation, a one standard deviation in skill mismatch is associated with a 5 percent decline in annual income, which partly reflects a decline in hours (see column (3) in Table 3.4), and a 3 percent decline in hourly wages. Strikingly, even 5-6 years after graduation, these penalties remain large and statistically significant, at -3.4 percent and -2.6 percent respectively. In contrast, the literature on the long-term effects of initial unemployment rates tend to find wage effects that decay at a faster rate. In Section 3.4.5, we estimate the effect of unemployment rates in our sample and show that they are indeed less persistent. Given the results in the previous section, a natural question is how much of the wage effects reflect major-occupation mismatch, since being employed in one of the top 5 or top 10 occupations by college major is associated with a 15 percent wage premium on average. However, a simple back-of-the-envelope calculation suggests that only about 14 percent of the wage effect can be attributed to major-occupation fit.

Figure 3.2 illustrates heterogeneity in the wage effects across college major groups. Specifically, we estimate a single regression (3.3) where the dependent variable is log hourly wages, and interact initial skill mismatch not only with potential experience fixed effects but also with major group fixed effects. The figure plots the resulting OLS estimates and corresponding 95% confidence intervals for the 8 college major groups (roughly in increasing order). The largest wage declines, occur among the four lowest-paying major groups: "Arts and humanities," "Public and social services," "Multi/interdisciplinary studies" and "Social sciences." There are also negative and statistically significant wage declines among high-paying major groups such as business or STEM, both of which are important drivers of the average effects, given that they can account for around 40 percent of all college graduates. Interestingly, the wage effects among graduates with degrees in health care majors are close to zero and precisely estimated. The fact that low-paying majors are more affected by initial labor market conditions is broadly consistent with the findings in Oreopoulos et al. (2012) and Altonji et al. (2016). Another interesting feature of Figure 3.2 is that the wage effects seem to be persistent across all majors, whereas one might have expected some differences in convergence patterns.

3.4.4. The Effect of Current vs. Initial Labor Market Conditions

As discussed in Oreopoulos et al. (2012), the medium-run estimates we have documented so far can be thought of as the effect of initial skill mismatch *plus* the weighted sum of the effect of subsequent skill mismatch, to the extent that skill mismatch is serially correlated.[18] In this section, we explore how much of the medium-run effects is due to initial skill mismatch. We start by augmenting our main specification (3.3) with the effect of *current* skill mismatch interacted with potential experience fixed effects (also grouped into 2-year bins):

$$\begin{aligned} y_{iem\ell gt} = {} & \alpha_{mg} + \gamma_{\ell g} + \lambda_t + \varphi_e + \beta_e \cdot \text{skill mismatch}_{m\ell g} \\ & + \delta_e \cdot \text{skill mismatch}_{m\ell t} + \theta \cdot X_{it} + \varepsilon_{iem\ell gt} \end{aligned} \tag{3.4}$$

[18] Skill mismatch exhibits strong serial correlation: within MSA-major pairs 1 year apart, the Pearson correlation coefficient is 0.95 on average. The corresponding 5-year correlation coefficient is 0.9 on average.

Table 3.7 shows the resulting estimates. Columns (1) and (2) illustrate that, although both current and initial skill mismatch have negative effects, what matters for earnings and wages is initial skill mismatch. Figure 3.3 plots the wage results from column (2) using individual instead of grouped years of potential experience, and leads to a similar conclusion. The patterns for the other outcomes are slightly more mixed, with the effects for individuals with 3-4 years of potential experience seemingly being driven by current rather than initial skill mismatch while the opposite being true for individuals with 5-6 years of potential experience. However, the fact that skill mismatch is strongly serially correlated implies that it might be difficult to disentangle these two effects. In more restrictive models where current and initial skill mismatch are not interacted with potential experience fixed effects, only the coefficients corresponding to initial skill mismatch are statistically significant.

Digging deeper into the wage results, we estimate the effect of initial skill mismatch *net* of all subsequent skill mismatch, analogous to the exercise proposed in Oreopoulos et al. (2012). This involves controlling for the full history of skill mismatch that individuals face over the first 6 years of potential experience, allowing skill mismatch at every stage to have persistent effects. Because estimates can get noisy, we follow Oreopoulos et al. (2012) and average skill mismatch across consecutive years. Let skill mismatch$_{m\ell,01}$ denote skill mismatch for major m in MSA ℓ averaged across the year of graduation and the following year. Define skill mismatch$_{m\ell,23}$

and skill mismatch$_{m\ell,45}$ analogously. We then estimate the following model:

$$\begin{aligned} \text{log wage}_{iem\ell gt} = \alpha_{mg} + \gamma_{\ell g} + \lambda_t + \varphi_e &+ \beta_{e,01} \cdot \text{skill mismatch}_{m\ell,01} \\ &+ \beta_{e,23} \cdot \text{skill mismatch}_{m\ell,23} \\ &+ \beta_{e,45} \cdot \text{skill mismatch}_{m\ell,45} + \theta \cdot X_{it} + \varepsilon_{iem\ell gt} \end{aligned} \tag{3.5}$$

where $\beta_{e,y_1y_2} = 0 \;\; \forall e < y_2$. Figure 3.4 plots the estimates from our main specification (3.3) (baseline), the estimates from specification (3.5) where we only include the effect of skill mismatch$_{m\ell,01}$ (no history), and the estimates from specification (3.5) without any restrictions (full history). First, note that averaging skill mismatch across years 0 and 1 of potential experience yields estimates that are extremely similar to the baseline estimates. Second, the effect of initial skill mismatch net of subsequent skill mismatch are not too dissimilar to the baseline estimates, except for potential experience year 4. One reason why the estimate are less precise for more experienced individuals is the unbalanced nature of our sample. The estimate for 6 years of potential experience is only based on individuals who graduated in 2010 and are observed in the 2016 ACS. Similarly, the estimate for 5 years of potential experience is based on the 2010 and 2011 graduation cohorts, observed in 2015 and 2016 respectively. Hopefully, as more data becomes available in the ACS, this exercise will yield a clearer picture. But overall, our takeaway from the results in this section is that skill mismatch experienced in the year of graduation seems to be an important driver of our main results.

3.4.5. The Effect of Skill Mismatch vs. Overall Unemployment Rates

In order to contrast our findings on the effect of skill-specific labor market conditions with findings in the literature on the effect of overall labor market conditions, we directly estimate the effect of initial unemployment rates in our sample. Specifically, we estimate models of the following form:

$$y_{iem\ell gt} = \alpha_{mg} + \gamma_{\ell} + \lambda_t + \varphi_e + \beta_e \cdot \text{unemp}_{\ell g} + \theta \cdot X_{it} + \varepsilon_{iem\ell gt} \tag{3.6}$$

where $\text{unemp}_{\ell g}$ are MSA-cohort-specific unemployment rates extracted from the Bureau of Labor Statistics' Local Area Unemployment Statistics. Aside from replacing skill mismatch with unemployment rates, the only other difference relative to our main specification (3.3) is that we control for MSA fixed effects γ_{ℓ} instead of MSA-cohort fixed effects.

Table 3.8 displays the resulting estimates. The first row shows that unemployment rates have a negative impact on initial labor market outcomes. Among individuals with 1-2 years of potential experience, a one percentage point increase in the local unemployment rate reduces earnings by 2.4 percent and wages by 0.9 percent. It also raises the probability of being unemployed by 0.5 percentage points and reduces the probability of being employed in a college occupation by 0.5 percentage points. Although we examine a different period, the magnitude of the estimates are in line with the literature. Studying U.S. college graduating classes of 1974-2011, Altonji et al. (2016) find using a similar empirical exercise that a one percentage point increase in the Census division-cohort-specific unemployment rate leads to a 2.9 percent decline in earnings and a 1 percent decline in wages among individuals with 1 year of potential experience.

What really stands out in Table 3.8 is how fast the impact of unemployment rates fades with experience. While the effects are only slightly smaller and still statistically significant for individuals with 3-4 years of potential experience, they essentially vanish after 5 years.[19] Another interesting feature is that unemployment rates are poor predictors of major-occupation fit, as can be seen from the last two columns. These qualitative patterns stand in stark contrast to our main findings, which shows that skill-specific labor market conditions have fundamentally different consequences for college graduates. This is due to the fact that unemployment rates and skill mismatch capture distinct sources of variation. To illustrate this point, Appendix Table C.9 augments specification (3.6) with our skill mismatch measure interacted with potential experience fixed effects. Both the coefficients for unemployment rates and skill mismatch are virtually indistinguishable from the corresponding ones in Tables 3.4-3.6 and Table 3.8.

3.4.6. Robustness Checks

Alternative Sample Restrictions. Our main results are robust to a variety of alternative sample restrictions. First, many studies restrict attention to men (Kahn, 2010; Oreopoulos et al., 2012), presumably to abstract away from the fact that career dynamics may differ across men and women, for example due to birth-related career interruptions for women. Table 3.9 shows the main results for men and women separately.

[19] The estimates in the second and third row of Table 3.8 are also similar to corresponding ones in Altonji et al. (2016).

Although the estimates differ slightly in magnitude, the patterns are broadly similar. It is worth noting that the wage effects are slightly more negative for men, with a 2.9 percent wage penalty in response to a one standard deviation increase in skill mismatch for men with 5-6 years of potential experience versus a corresponding 1.7 percent wage penalty for women. In addition, skill mismatch has a persistent, albeit small, effect on the probability of being unemployed for men, whereas women are largely unaffected. Some of these differences could reflect the fact that men and women sort into different college majors and that majors are differentially sensitive to skill mismatch as we showed in Figure 3.2. Another common empirical choice in the literature is to focus on individuals with exactly a 4-year Bachelor's degree. We include individuals with a Master's degree in our baseline sample, but results excluding them are extremely similar (see Appendix Table C.12).

As Appendix Table C.10 shows, there is a substantial amount of variation in the number of online job postings per capita across MSAs, ranging from fewer than 1 for every 100 individuals in San Juan, PR, to 15 for every 100 individuals in San Francisco, CA, in 2016. Therefore, one might be concerned about the accuracy of our skill mismatch measure in smaller cities. In Appendix Table C.11, we restrict the sample to the top 100 MSAs in terms of online job postings per capita, and find very similar results.

Alternative Definition of Skill Mismatch. In our baseline definition of skill mismatch, we use employment shares (3.2) to assess the fit between college majors and occupations. However, employment shares may be a poor measure of major-occupation fit in certain cases. As an example, consider history majors. Although not shown in Appendix Table C.8, the fifth most common occupation for individuals with this major is "Retail salespersons and sales clerks." Arguably, this is not because history majors possess skills that are essential for a sales clerk position, but probably simply reflects the fact that many of them end up in low-skill jobs.

To address this concern, we construct an alternative measure of skill mismatch in which employment shares are replaced with a different notion of major-occupation fit: college major wage premiums by occupation. Specifically, for each occupation, we regress log hourly wages of college-educated workers currently employed in that occupation on demographic controls and college major fixed effects.[20] The estimated college major fixed effects then serve as a proxy for how "valuable" the set of skills embedded in college majors are in a specific occupation. We borrow this idea from Liu et al. (2016), who use an analogous procedure to assess the fit between college majors and industries. Skill mismatch is then defined as in equation (3.1) except that the match between college majors and occupations is given by wage premiums instead of employment shares:

$$\widetilde{\text{match}}(\text{major}_m, \text{occ}_k) = \text{wage premium}(\text{major}_m)|\text{occ}_k \tag{3.7}$$

The rationale is that occupations associated with large wage premiums are a good fit for a particular college major. Coming back to the example above, while the 4-digit

[20] As with employment shares, we restrict the sample to individuals aged 32 or older to avoid any mechanical correlation between the outcomes and our measure of skill mismatch. Moreover, for each occupation, we convert the corresponding major wage premiums into "shares" (by normalizing them by the sum of all wage premiums), to account for the fact that certain jobs tend to pay more on average. This normalization has no bearing on the results.

SOC code 4120 encompassing sales clerks ranks 12th in terms of employment shares among history majors, it only ranks 81st in terms of wage premiums (out of 109 4-digit SOC occupations). While college major wage premiums capture something distinct from employment shares, they are nonetheless positively correlated with one another. As a result, the baseline and alternative measures of skill mismatch are also positively correlated (correlation coefficient of 0.29). This is illustrated graphically in Appendix Figure C.6, which plots average skill mismatch by college major under both definitions, separately by year.

Appendix Table C.13 shows the main results using the alternative measure of skill mismatch. Although the magnitude of the estimates differs—in part because a one standard deviation increase in skill mismatch has a different interpretation under this alternative definition—the basic patterns are the same: there are negative persistent effects on wages, the probability of being employed and the probability of being employed in one of the top occupations by college major.

Endogenous Migration. As mentioned in Section 3.2.2, one of the main drawback of using the ACS is that it contains little information about past location. As a result, we must impute individuals' MSA at graduation using the MSA they currently reside in. This could potentially be a poor approximation, especially for individuals several years out of college. Moreover, this raises the concern that we might be overstating the impact of skill mismatch if individuals endogenously sort into MSAs.

We try to mitigate this concern in three different ways. First, we can simply focus on individuals with 1 year of potential experience, excluding those who migrated from a different state in the last year. For this subpopulation, we can at least be confident that we are accurately estimating the short-term effects of skill mismatch. The estimates in Appendix Table C.14 are quite close to the corresponding estimates in the first rows of Tables 3.4-3.6, with the exception of part-time employment and employment in non-college occupations, which are smaller and statistically insignificant.

Alternatively, following others (e.g., Charles et al., forthcoming), we can restrict the sample to individuals born in the state they currently reside in, for which mobility is probably less of a concern. Incidentally, this restriction also excludes foreign born college graduates who exhibit higher mobility rates, presumably because they have fewer social ties to specific areas. The resulting estimates in Appendix Table C.15 reveal slightly smaller effects but broadly similar patterns. For example, the wage coefficient for 5-6 years of potential experience is half as large as the corresponding baseline estimate in Table 3.6, but still statistically significant. On the other hand, the effects on major-occupation fit are not persistent for this subpopulation.

Finally, we can use states instead of MSAs as the unit of geography, which at least addresses the threat within-state cross-MSA migration. This comes at the price of less cross-sectional variation in skill mismatch, but a slightly larger sample size since we can include individuals who do not live in MSAs. Appendix Table C.16 shows the resulting estimates, which are remarkably similar to the baseline estimates.

Exploiting Time Variation in Skill Mismatch. Our main regression specification (3.3) features MSA-cohort fixed effects, which control for location-specific factors common across college majors, as well as major-cohort fixed effects, which control for major-specific factors common across MSAs. In principle, we could also include major-MSA fixed effects to control for time invariant location-major-specific factors. For example, certain schools might have a better track record at producing certain kinds of majors than other schools, due to the quality of instruction or the quality of research facilities. In turn, if local employers tend to heavily hire—and therefore advertise for—graduates from these fields, this would push us towards finding that low skill mismatch is associated with better outcomes. By "saturating" the regression with major-MSA fixed effects, identification of the main effects relies heavily on within-MSA variation in skill mismatch across cohorts.

The resulting estimates for log wages are shown in column (4) of Table 3.10, and show *positive* but statistically insignificant coefficients. The estimates for the other outcomes are also small and insignificant (not shown). The results are however fully robust to the inclusion of major-state fixed effects and major group-MSA fixed effects, as can be seen from column (3). In contrast to the saturated regression, this specification still exploits cross-sectional variation, both within-MSA across college majors belonging to the same major group and within-major across MSAs in the same state.

In light of these results, one might therefore be tempted to conclude that the main results presented in Section 3.4 are simply driven by cross-sectional variation in the quality of college majors. However, there are two reasons why time variation in the occupational composition of online job postings is hard to interpret and potentially misleading. First, the sophisticated algorithm used by Burning Glass to scrape job ads from various online sources is constantly evolving. In particular, it is getting better at capturing low-skill jobs, creating a spurious shift in the composition of online job postings. Second, and more importantly, the extent to which various sectors of the economy hire workers online has also changed over time. Consider the transportation and warehousing industry. Table 3.1 shows that the share of online job postings in Burning Glass for this industry has tripled over the last few years, from 3.78 percent in 2010 to 10.88 percent in 2016. In contrast, the corresponding share of job openings in JOLTS has only increased from 2.74 percent to 3.55 percent over the same period. This can also been seen in Table 3.2, where the share of online job postings for transportation and material moving occupations has risen from 3.95 percent to 10.45 percent between 2010 and 2016, while employment in these occupations has been relatively stable in the ACS at around 6 percent. Therefore, the sharp rise of transportation jobs in Burning Glass probably reflects a shift in the way this industry hires new workers, rather than a real shift in labor demand.

Addressing the representativeness of Burning Glass over time is a challenging task. Following Şahin et al. (2014), one option is to assume that the industry composition of job openings in JOLTS reflects the true composition of job openings in the economy and adjust the composition of online job postings in Burning Glass accordingly. For each industry-year pair (j, t), we compute the JOLTS adjustment factor which equalizes the industry composition in JOLTS and Burning Glass:

$$\text{share of job openings}_t^{\text{JOLTS}}(\text{ind}_j) = \text{JOLTS factor}_{jt} \times \text{share of job postings}_t^{\text{BGT}}(\text{ind}_j) \tag{3.8}$$

We then adjust the MSA-year-specific occupational shares in the definition of skill mismatch (3.1) by first computing shares at the occupation-industry level, scaling them by the industry-specific JOLTS adjustment factor, and summing over all industries for each occupation k:

(3.9)

$$\widetilde{\text{share of job postings}}^{\text{BGT}}_{\ell t}(\text{occ}_k) = \sum_j \text{share of job postings}^{\text{BGT}}_{\ell t}(\text{occ}_{kj}) \times \text{JOLTS factor}_{jt}$$

To get a sense of what this adjustment does, the last three columns in Table 3.2 display the JOLTS-adjusted occupational composition of online job postings in Burning Glass, i.e. equation (3.9) where occupation-industry shares are computed at the national rather than MSA level. While it is impossible to know the true occupational composition of job openings, the JOLTS-adjusted shares do get closer to employment shares in the ACS on average, although not for every occupation (e.g. office and administrative support occupations).

Columns (5) and (6) in Table 3.10 show the regression results for log wages using this JOLTS-adjusted measure of skill mismatch, respectively without and with major-MSA fixed effects. The estimates are extremely similar to the corresponding estimates in columns (1) and (4). This is perhaps not so surprising given that we include major-cohort fixed effects, which effectively account for the fact that some majors tend to have lower skill mismatch than others on average due to the occupational composition of job postings in Burning Glass. Therefore, a national-level adjustment is unlikely to make much of a difference.

In principle, we could do a finer adjustment if JOLTS data was available at an MSA level.[21] However, the fundamental problem is that the adjustment can only be done at the industry level. The simple example in Appendix Table C.17 illustrates why. Suppose that there are only two industries in the economy: IT and construction. Within IT, there are two types of jobs: software developers and database administrators. Similarly, within construction, the two types of jobs are construction managers and construction workers. For simplicity, suppose that there are 100 job openings in the economy in 2010, distributed according to column (4). The number of job openings increases to 120 in 2016, but the increase is proportional across jobs so that the composition of job openings in column (5) stays unchanged. Columns (6) and (7) show the corresponding number and composition of job postings in Burning Glass, where differences stem from the fact that not all jobs are posted online and the fact that IT and high-skill jobs are overrepresented in Burning Glass. However, between 2010 and 2016, the composition of jobs in Burning Glass shifts from IT towards construction, and from construction managers towards construction workers (within the construction sector). Based on column (7), one would conclude that labor demand has shifted away from IT, with a particularly strong increase in the demand for construction workers. Of course, these changes are spurious and simply reflect changes in the extent to which different industries hire workers online as well as improvements in Burning Glass' data collection technology.

The question now is whether an industry-level adjustment can solve this issue. Columns (8)-(12) apply the JOLTS adjustment as described above, again assuming that

[21] The lowest level of geography in JOLTS is Census regions, but they are not available at the industry level (at least in the public data).

JOLTS perfectly captures job openings in the economy. The are two things to notice from the resulting JOLTS-adjusted shares in column (13). First, because the relative representativeness of software developers versus database administrators in Burning Glass has not changed over time, the JOLTS-adjusted shares are accurate for those two jobs. However, because this is not the case for construction managers versus construction workers, the shares for those two jobs are still off in both periods, even though their sum is now correct. Therefore, based on the JOLTS-adjusted shares we would still conclude that labor demand has shifted away from construction managers towards construction workers. This illustrates the fundamental challenge in trying to exploit time variation in the occupational composition of online job postings, and why we should perhaps not put too much stock in the estimates in Table 3.10 that try to do exactly that.

3.5. Discussion

In this section, we discuss what our findings imply for models of career dynamics. Two alternative mechanisms are often proposed as potential explanations for the presence of persistent wage effects in response to poor initial labor market conditions. The first class of models are job search models in which workers continuously make draws from some underlying wage distribution, slowly climbing the job ladder by obtaining better jobs. As argued in Oreopoulos et al. (2012), in order to generate persistent wage effects in response to a temporary deterioration in the wage distribution, the basic framework needs to be augmented with job mobility costs that are increasing with age or tenure. Assuming the existence of such costs, workers who start their career in worse jobs are at risk of being stuck at the bottom of the job ladder as time goes by. Mobility costs that are increasing with age can originate from several sources, including firm-specific human capital and family-related constraints. Oreopoulos et al. (2012) argue that a search model along those line is consistent with their findings, in particular the fact that part of the recovery process takes the form of mobility from lower towards higher-quality employers, especially for college graduates with high predicted earnings.

The second class of models are models of human capital accumulation, in which wage growth over the life cycle stems from the accumulation productivity-enhancing human capital over time, either at the firm, industry or occupation level. In those models, differential career paths as a function of initial labor market conditions can be attributed to starting jobs with differential opportunities for human capital accumulation, with lower-level jobs implicitly associated with lower rates of human capital accumulation.

Since we do not have longitudinal data on workers, we cannot directly explore how much of our effects are driven by job (im)mobility and thereby provide direct evidence in favor of (or against) the job search hypothesis in the context of skill mismatch. Nevertheless, we argue that the human capital story offers a simple and intuitive explanation for our findings. In particular, it is consistent with one piece of evidence we have documented: namely the impact of skill mismatch on major-occupation fit. We posit that college majors are associated with a certain mix of skills and that different occupations make use of those skills with different intensities. Human capital accumulation is then a function of how well those skills are exploited on the job.[22] Therefore, to the extent that major-occupation fit is an indicator of how well skills acquired in school are exploited in a certain occupation, then the fact that skill mismatch has a negative effect on initial major-occupation fit would seem to support this hypothesis. Our finding that skill mismatch experienced in the year of graduation seems to be crucial is also consistent with this story. The importance of starting jobs for subsequent wage growth has been emphasized by Devereux (2002), and Kinsler and Pavan (2015) have shown that working in an occupation related to one's major is associated with a significant wage premium. Moreover, as mentioned in the introduction, Liu et al. (2016) find that initial major-industry fit can explain a large portion of the long-term impact of recessions among Norwegian college graduates. Shedding further light on this particular mechanism is something we are exploring in ongoing work.[23]

Before concluding, we end with a brief policy note. Our findings provide useful insights in the context of initiatives geared towards helping students make more informed college major choices. In recent years, policymakers have made efforts to increase transparency in higher education by making data available to the public on cost and performance metrics of higher education institutions. A prominent example is the U.S. Department of Education's College Scorecard, which provides information on tuition costs, financial aid, graduation rates, and earnings of past students for nearly

[22] One could also imagine that skills depreciate over time if unused. This basic premise is similar to the one in Lise and Postel-Vinay (2016).

[23] One promising direction is to further investigate heterogeneity in the effect of skill mismatch across college majors, as major-specific human capital accumulation may be more important for some majors than others.

every college or university in the United States. Concurrently, a literature has emerged exploring the effectiveness of these kinds of initiatives, particularly when it comes to college major decisions (Hastings et al., 2015; Wiswall and Zafar, 2015b; Baker et al., 2017). Our findings on the effect of skill mismatch illustrate that what matters for future labor market prospects is the interaction of field of study *and* location. Therefore, average earnings by college major should be made available at subnational levels if possible for maximum informativeness.

3.6. Conclusion

A number of recent studies have documented the persistent effects of graduating from college during times of high unemployment, and how these effects vary across graduates from different fields of study. In this paper, we explore a related question: how do skill-specific initial labor market conditions affect early career outcomes of college graduates? Exploiting data on the near-universe of online job postings in the U.S. since 2010, we construct a new measure of skill mismatch which captures how well the skills that are embedded in college majors match the skills that are demanded by local employers in a given city and given year. Intuitively, college graduates with a specific major experience skill mismatch when only a small fraction of job openings in their local labor market are suitable for their major in the year that they graduate. Conceptually, this additional layer of variation allows us to compare individuals who faced the same overall labor market conditions, but whose skills were more or less in demand when they graduated.

We find that skill mismatch leads to worse initial outcomes for college graduates: they are more likely to be unemployed or employed in a part-time job, less likely to be employed in a college occupation or one of the top occupations for their college major, and they earn lower wages. While the effects on unemployment, part-time employment and employment in college occupations gradually fade over time, the effects on wages and major-occupation fit persist up to 6 years after graduation. These medium-run effects are substantial: a one standard deviation increase in initial skill mismatch leads to a 1.6% point decline in the probability of being employed in one of the top 5 occupations by college major and a 2.6% decline in hourly wages, 6 years after graduation. This contrasts with the effects of overall unemployment rates at graduation—the focus of past studies—which completely dissipate within 4 years of graduation in our sample.

Our findings highlight the importance of having the right skills in the right place at the right time. In particular, the persistent effects on major-occupation fit, combined with the fact that initial skill mismatch seems to matter more for wages than skill mismatch experienced in subsequent years, suggest that early career human capital accumulation is a key determinant of college graduates' long-term career path. From a policy perspective, our findings suggest that efforts to inform students' college major choices should take into account the interaction between college majors and locations.

Table 1.1. The Effect of Retirement Trends on Youth Employment, Unemployment and Labor Force Participation

	Dependent variable: Youth outcome (22-30)				
	Δ Emp/pop				
	All	Part-time	Full-time	Δ Unemp/pop	Δ NLFP/pop
	(1)	(2)	(3)	(4)	(5)
Panel A: OLS estimates					
Δ Emp/pop (55+)	0.519***	-0.200***	0.719***	-0.186***	-0.333***
	(0.074)	(0.050)	(0.107)	(0.043)	(0.056)
Panel B: 2SLS estimates					
Δ Emp/pop (55+)	0.112	0.516***	-0.404	0.318**	-0.430
	(0.438)	(0.133)	(0.491)	(0.159)	(0.463)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 1.2. The Effect of Retirement Trends on Youth Occupational Composition

	Dependent variable: Δ Emp/pop (22-30)		
	Low-skill occupations	Middle-skill occupations	High-skill occupations
	(1)	(2)	(3)
Panel A: OLS estimates			
Δ Emp/pop (55+)	-0.120***	0.554***	0.086
	(0.046)	(0.063)	(0.053)
Panel B: 2SLS estimates			
Δ Emp/pop (55+)	0.518***	0.335	-0.740***
	(0.176)	(0.300)	(0.250)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 1.3. The Effect of Retirement Trends on Overeducated Employment

	Dependent variable: Δ Overeducated/emp					
	Young (22-30)					
	All	Male	Female	Some college	⩾ College grad	Prime-aged (31-44)
	(1)	(2)	(3)	(4)	(5)	(6)
*Panel A: OLS estimates (O*NET)*						
Δ Emp/pop (55+)	0.213***	0.291***	0.117	0.166**	0.106	0.066
	(0.077)	(0.103)	(0.085)	(0.076)	(0.096)	(0.052)
*Panel B: 2SLS estimates (O*NET)*						
Δ Emp/pop (55+)	1.386***	1.377***	1.480***	0.874***	0.898***	-0.120
	(0.262)	(0.342)	(0.279)	(0.273)	(0.315)	(0.160)
Panel C: 2SLS estimates (Census: 1990 basis)						
Δ Emp/pop (55+)	1.319***	1.272***	1.451***	1.004***	0.832**	0.005
	(0.283)	(0.344)	(0.310)	(0.336)	(0.374)	(0.190)
Panel D: 2SLS estimates (Census: yearly basis)						
Δ Emp/pop (55+)	1.150***	1.110***	1.271***	1.204***	0.703**	-0.424*
	(0.263)	(0.334)	(0.290)	(0.395)	(0.314)	(0.248)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 1.4. The Effect of Retirement Trends on Youth Wages

	Dependent variable: Δ log hourly wage (22-30)			
	All occupations	Low-skill occupations	Middle-skill occupations	High-skill occupations
	(1)	(2)	(3)	(4)
Panel A: OLS estimates				
Δ Emp/pop (55+)	1.058***	1.032***	1.280***	0.679***
	(0.172)	(0.199)	(0.200)	(0.157)
Panel B: 2SLS estimates				
Δ Emp/pop (55+)	-2.958***	-2.781***	-2.160**	-3.526***
	(0.842)	(0.739)	(0.950)	(0.716)
Panel C: 2SLS estimates (composition-adjusted)				
Δ Emp/pop (55+)	-2.510***	-2.742***	-2.187**	-2.952***
	(0.749)	(0.702)	(0.922)	(0.639)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). Sample excludes part-time workers and the self-employed. All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 1.5. The Effect of Retirement Trends on School Attendance

	Dependent variable: Δ School/pop							
	Young (22-30)							
	All	Male	Female	⩽ High school grad	Some college	⩾ College grad	Teenagers (16-21)	Prime-aged (31-44)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Emp/pop (55+)	0.444***	0.387***	0.500**	-0.238	1.015***	0.394	1.180***	0.003
	(0.158)	(0.139)	(0.211)	(0.155)	(0.364)	(0.302)	(0.283)	(0.119)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 1.6. The Effect of Retirement Trends on Net Migration

	Dependent variable: Δ log population count							
	Young (22-30)							
	All	Male	Female	⩽ High school grad	Some college	⩾ College grad	Teenagers (16-21)	Prime-aged (31-44)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Emp/pop (55+)	-3.919***	-3.870***	-3.957***	-3.898***	-1.026	-7.178***	0.192	-0.553
	(1.308)	(1.479)	(1.162)	(1.156)	(1.273)	(2.536)	(0.791)	(0.896)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 1.7. Falsification Test: The Effect of Future Retirement Trends on Past Youth Outcomes

	Dependent variable: Youth outcome (22-30)									
	Δ Emp/pop									
	All	Part-time	Full-time	Low-skill occupations	Middle-skill occupations	High-skill occupations	Δ Unemp/ pop	Δ NLFP/ pop	Δ Overeduc/ emp (O*NET)	Δ log hourly wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Δ Emp/pop (55+)	0.159	-0.005	0.164	-0.040	0.491	-0.292	-0.276	0.117	-0.138	-0.414
(10 years later)	(0.294)	(0.091)	(0.314)	(0.164)	(0.315)	(0.275)	(0.218)	(0.176)	(0.231)	(0.771)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects (1970-1980, 1980-1990, 1990-2000) and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation off-shoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 1.8. The Effect of Retirement Trends on Cohort-Specific Outcomes

	Dependent variable: Youth outcome (22-30)							
	Δ Emp/pop							
	All	Part-time	Low-skill occupations	Middle-skill occupations	High-skill occupations	Δ Unemp/pop	Δ Overeduc/emp (O*NET)	Δ log hourly wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 22-30 today vs. 22-30 ten years later (baseline results)								
Δ Emp/pop (55+)	0.004	0.298**	0.349*	0.325	-0.670***	0.271	1.167***	-3.006***
	(0.259)	(0.138)	(0.202)	(0.219)	(0.214)	(0.174)	(0.266)	(0.785)
Panel B: 22-30 today vs. 32-40 ten years later (cohort results)								
Δ Emp/pop (55+)	0.285	0.015	0.245**	0.321*	-0.281*	0.233	0.566**	-0.575***
	(0.177)	(0.139)	(0.105)	(0.168)	(0.169)	(0.244)	(0.244)	(0.187)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). Sample excludes individuals born out-of-state. All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 2.1. Summary Statistics on State OAA Programs, December 1939

	Mean	Median	SD	Min	Max	*N*
OAA recipiency rate, December 1939	0.23	0.23	0.09	0.08	0.49	49
OAA payment per recipient, December 1939	17.93	18.90	6.49	6.01	32.97	49
OAA payment per person 65+, December 1939	4.16	3.59	2.59	1.01	13.17	49
Legal maximum payment (per month)	29.37	30	5.34	15	45	41
99th percentile payment (per month)	29.43	30	6.22	12	45	49
99th percentile payment, states with legal maximum	28.78	30	4.85	15	45	41

Notes: Includes the 48 states and the District of Columbia. 99th percentile payment is for new recipients in fiscal year 1938-39. Eight states had no legal maximum payment. Recipiency rate and payments per person 65+ are normalized by state population from 1940 Census. Data on OAA dollar payments and number of recipients from U.S. Social Security Board (1940b), data on legal maximum payments from U.S. Social Security Board (1940a), data on 99th percentile payment from U.S. Social Security Board (1939b).

Table 2.2. The Effect of OAA on Co-residence

	Co-residence with relatives		As household head		As dependent	
	(1)	(2)	(3)	(4)	(5)	(6)
OAA per-65+ × (55-59)	-0.034	-0.012	-0.027	-0.034	-0.008	0.022
	(0.027)	(0.022)	(0.024)	(0.025)	(0.010)	(0.018)
OAA per-65+ × (60-64)	-0.022	-0.037*	-0.018	-0.060**	-0.005	0.023*
	(0.020)	(0.022)	(0.017)	(0.030)	(0.007)	(0.013)
OAA per-65+ × (65-69)	-0.063**	-0.044*	-0.056**	-0.046*	-0.007	0.001
	(0.025)	(0.025)	(0.027)	(0.024)	(0.010)	(0.009)
OAA per-65+ × (70-74)	-0.084***	-0.087***	-0.051**	-0.046*	-0.033**	-0.041***
	(0.029)	(0.032)	(0.025)	(0.027)	(0.013)	(0.015)
OAA per-65+ × (75-79)	-0.101***	-0.065	-0.032	-0.005	-0.068***	-0.059**
	(0.034)	(0.040)	(0.031)	(0.022)	(0.025)	(0.023)
OAA per-65+ × (80-84)	-0.071**	-0.098**	-0.018	0.036	-0.053*	-0.134***
	(0.030)	(0.042)	(0.022)	(0.026)	(0.028)	(0.045)
Observations	12,273,735	11,806,357	12,273,735	11,806,357	12,273,735	11,806,357
Mean of dep. var.	0.511	0.592	0.418	0.377	0.093	0.215
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
Sample	Men	Women	Men	Women	Men	Women

Notes: Annual OAA payments per person 65+ are in hundreds of 1939 dollars. The Kleibergen-Paap rk Wald *F*-statistic is 1.98 in the men sample and 2.37 in the women sample. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 2.3. The Effect of OAA on Co-residence: Heterogeneity by Marital Status

	Co-residence with relatives			
	(1)	(2)	(3)	(4)
OAA per-65+ × (55-59)	−0.028	−0.044	−0.019	−0.014
	(0.026)	(0.030)	(0.024)	(0.022)
OAA per-65+ × (60-64)	−0.027	−0.022	−0.031	−0.065***
	(0.018)	(0.028)	(0.027)	(0.022)
OAA per-65+ × (65-69)	−0.059***	−0.090***	−0.042	−0.062**
	(0.022)	(0.034)	(0.027)	(0.025)
OAA per-65+ × (70-74)	−0.066**	−0.140***	−0.068**	−0.091***
	(0.029)	(0.041)	(0.033)	(0.031)
OAA per-65+ × (75-79)	−0.105***	−0.117***	−0.048	−0.062
	(0.037)	(0.038)	(0.042)	(0.040)
OAA per-65+ × (80-84)	−0.042	−0.116***	−0.095*	−0.085**
	(0.040)	(0.042)	(0.057)	(0.042)
Observations	9,032,441	2,095,142	6,041,997	4,787,247
Mean of dep. var.	0.52	0.561	0.514	0.695
State border × age group × year FEs	✓	✓	✓	✓
County FEs	✓	✓	✓	✓
Sample	Men (married)	Men (non-married)	Women (married)	Women (non-married)

Notes: Sample excludes single and never married individuals. Non-married individuals are those that are separated, divorced or widowed. Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 2.4. The Effect of OAA on Co-residence with Children vs. Other Relatives

	Co-residence with child(ren) as:			Co-residence with other relative(s) as:			Co-residence with child(ren) as:			Co-residence with other relative(s) as:		
	Any	HHH	Dep.	Any	HHH	Dep.	Any	HHH	Dep.	Any	HHH	Dep.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
OAA per-65+ × (55-59)	-0.020	-0.018	-0.002	-0.014	-0.009*	-0.006	-0.002	-0.024	0.023	-0.010	-0.009	-0.001
	(0.027)	(0.024)	(0.009)	(0.009)	(0.005)	(0.005)	(0.025)	(0.025)	(0.017)	(0.008)	(0.006)	(0.005)
OAA per-65+ × (60-64)	-0.016	-0.014	-0.002	-0.006	-0.003	-0.003	-0.028	-0.047	0.018	-0.008	-0.013***	0.005
	(0.019)	(0.018)	(0.005)	(0.007)	(0.005)	(0.005)	(0.025)	(0.029)	(0.012)	(0.008)	(0.005)	(0.006)
OAA per-65+ × (65-69)	-0.047**	-0.045*	-0.002	-0.016***	-0.011**	-0.005	-0.016	-0.026	0.009	-0.028***	-0.020***	-0.008*
	(0.020)	(0.024)	(0.010)	(0.006)	(0.004)	(0.004)	(0.021)	(0.020)	(0.009)	(0.008)	(0.005)	(0.004)
OAA per-65+ × (70-74)	-0.065***	-0.044*	-0.021*	-0.019**	-0.007*	-0.012*	-0.065**	-0.041*	-0.024*	-0.022**	-0.005	-0.017**
	(0.023)	(0.024)	(0.012)	(0.008)	(0.004)	(0.006)	(0.026)	(0.022)	(0.013)	(0.011)	(0.006)	(0.007)
OAA per-65+ × (75-79)	-0.080***	-0.030	-0.050**	-0.021	-0.003	-0.018*	-0.044	0.007	-0.050***	-0.021	-0.012*	-0.009
	(0.030)	(0.029)	(0.025)	(0.013)	(0.009)	(0.010)	(0.030)	(0.020)	(0.017)	(0.015)	(0.007)	(0.012)
OAA per-65+ × (80-84)	-0.035	0.002	0.037	-0.036*	-0.020*	-0.015	-0.073*	0.026	-0.099**	-0.025	0.009	-0.034*
	(0.034)	(0.023)	(0.029)	(0.019)	(0.011)	(0.011)	(0.044)	(0.026)	(0.039)	(0.023)	(0.009)	(0.018)
Observations	12,273,735	12,273,735	12,273,735	12,273,735	12,273,735	12,273,735	11,806,357	11,806,357	11,806,357	11,806,357	11,806,357	11,806,357
Mean of dep. var.	0.421	0.365	0.056	0.089	0.052	0.037	0.474	0.326	0.148	0.118	0.051	0.067
State border × age group × year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample	Men	Men	Men	Men	Men	Men	Women	Women	Women	Women	Women	Women

Notes: Children refer to own (biological) children. Other relatives include children-in-law, parents, siblings, grandchildren, and other relatives (e.g. aunts/uncles, nephews/nieces, cousins, etc..). Co-residence with children means that at least one child is present in the household. Co-residence with other relatives means that at least one other family member is present in the household, but no children. Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 2.5. The Effect of OAA on Co-residence with Sons vs. Daughters

	Co-residence with son(s) as:			Co-residence with daughter(s) as:			Co-residence with son(s) as:			Co-residence with daughter(s) as:		
	Any	HHH	Dep.	Any	HHH	Dep.	Any	HHH	Dep.	Any	HHH	Dep.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
OAA per-65+ × (55-59)	-0.045*	-0.046*	0.001	-0.012	-0.008	-0.003	-0.027	-0.045**	0.018	0.002	-0.002	0.004
	(0.026)	(0.026)	(0.005)	(0.019)	(0.017)	(0.006)	(0.021)	(0.023)	(0.011)	(0.018)	(0.016)	(0.011)
OAA per-65+ × (60-64)	-0.035*	-0.038*	0.003	0.003	0.008	-0.005	-0.040**	-0.052**	0.011	-0.004	-0.011	0.007
	(0.020)	(0.021)	(0.004)	(0.014)	(0.014)	(0.004)	(0.019)	(0.022)	(0.008)	(0.017)	(0.017)	(0.007)
OAA per-65+ × (65-69)	-0.052**	-0.055**	0.003	-0.011	-0.006	-0.005	-0.030*	-0.035**	0.004	0.004	-0.001	0.005
	(0.022)	(0.022)	(0.004)	(0.013)	(0.015)	(0.007)	(0.017)	(0.018)	(0.007)	(0.011)	(0.010)	(0.005)
OAA per-65+ × (70-74)	-0.043**	-0.043**	-0.0003	-0.040**	-0.019	-0.021***	-0.039**	-0.021	-0.018**	-0.029*	-0.023	-0.006
	(0.020)	(0.020)	(0.007)	(0.017)	(0.015)	(0.008)	(0.019)	(0.016)	(0.009)	(0.017)	(0.016)	(0.007)
OAA per-65+ × (75-79)	-0.045*	-0.037*	-0.009	-0.061***	-0.019	-0.042**	-0.027	0.006	-0.033**	-0.024	-0.007	-0.017
	(0.024)	(0.020)	(0.009)	(0.023)	(0.023)	(0.020)	(0.019)	(0.014)	(0.014)	(0.023)	(0.015)	(0.013)
OAA per-65+ × (80-84)	-0.029	-0.026	-0.003	-0.011	0.023	-0.034	0.007	0.010	-0.002	-0.083**	0.014	-0.097***
	(0.025)	(0.019)	(0.016)	(0.034)	(0.020)	(0.025)	(0.022)	(0.018)	(0.022)	(0.038)	(0.020)	(0.031)
Observations	12,273,735	12,273,735	12,273,735	12,273,735	12,273,735	12,273,735	11,806,357	11,806,357	11,806,357	11,806,357	11,806,357	11,806,357
Mean of dep. var.	0.272	0.246	0.027	0.239	0.21	0.029	0.275	0.209	0.067	0.274	0.193	0.082
State border × age group × year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample	Men	Men	Men	Men	Men	Men	Women	Women	Women	Women	Women	Women

Notes: Sons and daughters refer to own (biological) children. Co-residence with sons means that at least one son is present in the household. Co-residence with daughters is defined analogously. Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 3.1. Industry Composition: JOLTS vs. Burning Glass, 2010-2016

	Job opening/job posting shares (%)					
	JOLTS			Burning Glass		
JOLTS industry	2010	2013	2016	2010	2013	2016
Mining and Logging	0.57	0.51	0.22	0.72	0.59	0.23
Construction	2.53	3.01	3.36	0.91	1.15	1.24
Manufacturing	6.49	6.75	6.05	9.41	9.58	8.26
Wholesale Trade	2.91	3.30	3.26	0.98	1.08	0.82
Retail Trade	9.26	11.65	10.98	9.66	12.13	11.27
Transportation, Warehousing and Utilities	2.74	3.49	3.55	3.78	4.98	10.88
Information	2.76	2.46	1.49	4.40	3.87	3.54
Finance and Insurance	6.20	5.63	4.43	9.51	9.53	8.69
Real Estate and Rental and Leasing	1.13	1.43	1.37	3.31	2.42	1.88
Professional and Business Services	18.79	17.70	19.63	18.95	15.67	14.61
Educational Services	2.00	1.70	1.75	5.02	5.90	4.44
Health Care and Social Assistance	16.80	15.57	17.69	21.54	19.35	22.00
Arts, Entertainment, and Recreation	1.18	1.55	1.50	0.84	0.95	0.74
Accommodation and Food Services	8.76	11.55	11.79	5.99	7.40	7.44
Other Services	4.76	3.71	3.74	2.14	2.46	1.73
Government	13.12	9.99	9.19	2.84	2.94	2.22

Notes: Job postings belonging to the industry "Agriculture, Forestry, Fishing and Hunting" (NAICS code 11) are excluded from the Burning Glass sample in this table since JOLTS does not cover agricultural establishments.

Source: Job Openings and Labor Turnover Survey, Burning Glass Technologies.

Table 3.2. Occupational Composition: American Community Survey vs. Burning Glass, 2010-2016

		Employment/job posting shares (%)								
		ACS			Burning Glass			Burning Glass (JOLTS-adjusted)		
SOC code	SOC occupation group	2010	2013	2016	2010	2013	2016	2010	2013	2016
11-0000	Management Occupations	9.67	9.82	10.30	12.00	11.40	9.90	12.16	11.28	10.13
13-0000	Business and Financial Operations Occupations	4.67	4.80	4.85	7.45	7.50	6.97	7.01	6.57	6.08
15-0000	Computer and Mathematical Occupations	2.47	2.67	2.96	14.58	11.58	10.00	12.01	9.40	8.28
17-0000	Architecture and Engineering Occupations	1.81	1.84	1.84	3.10	3.05	2.44	3.04	2.87	2.35
19-0000	Life, Physical, and Social Science Occupations	0.88	0.86	0.88	1.12	1.03	0.99	1.40	1.11	1.11
21-0000	Community and Social Service Occupations	1.70	1.64	1.74	1.06	1.12	1.01	1.38	1.26	1.17
23-0000	Legal Occupations	1.18	1.16	1.12	1.00	1.23	0.54	0.98	1.38	0.67
25-0000	Education, Training, and Library Occupations	6.24	6.08	5.98	1.96	2.58	2.21	1.51	1.48	1.58
27-0000	Arts, Design, Entertainment, Sports, and Media Occupations	1.87	1.92	2.01	2.64	2.90	2.29	2.37	2.64	2.19
29-0000	Healthcare Practitioners and Technical Occupations	5.50	5.60	5.97	12.32	9.78	13.95	13.18	10.43	14.26
31-0000	Healthcare Support Occupations	2.51	2.59	2.34	2.48	2.04	2.13	2.54	2.04	2.07
33-0000	Protective Service Occupations	2.25	2.21	2.08	0.91	1.02	1.11	1.59	1.61	1.93
35-0000	Food Preparation and Serving Related Occupations	5.68	5.84	5.89	3.26	4.45	4.31	5.30	7.68	7.50
37-0000	Building and Grounds Cleaning and Maintenance Occupations	4.00	4.02	3.93	1.09	1.19	1.08	1.21	1.40	1.48
39-0000	Personal Care and Service Occupations	3.58	3.72	3.77	1.60	2.44	1.30	2.02	2.89	1.81
41-0000	Sales and Related Occupations	11.08	10.82	10.49	12.78	13.22	12.16	12.66	13.76	13.29
43-0000	Office and Administrative Support Occupations	13.91	13.36	12.83	10.61	11.67	10.83	9.68	10.47	10.20
45-0000	Farming, Fishing, and Forestry Occupations	0.73	0.70	0.68	0.05	0.07	0.06	0.04	0.06	0.07
47-0000	Construction and Extraction Occupations	5.06	4.99	5.03	1.00	1.07	1.03	1.16	1.35	1.52
49-0000	Installation, Maintenance, and Repair Occupations	3.30	3.20	3.09	2.78	3.04	2.75	3.12	3.44	3.42
51-0000	Production Occupations	5.91	6.01	5.79	2.24	2.69	2.49	2.04	2.29	2.38
53-0000	Transportation and Material Moving Occupations	6.01	6.15	6.42	3.95	4.94	10.45	3.53	4.61	6.31

Notes: ACS employment shares are based on all individuals aged 16 or older. JOLTS-adjusted job posting shares are calculated according to (3.9), using occupation-industry shares at the national rather than MSA level. Job postings belonging to the industry "Agriculture, Forestry, Fishing and Hunting" (NAICS code 11) are excluded from the Burning Glass sample in this table since JOLTS does not cover agricultural establishments.

Source: American Community Survey, Burning Glass Technologies.

Table 3.3. Average Skill Mismatch by College Major, 2010-2016

	Average skill mismatch									Average skill mismatch							
College major	2010	2011	2012	2013	2014	2015	2016	2010-16	College major	2010	2011	2012	2013	2014	2015	2016	2010-16
Nursing	-5.33	-4.13	-3.90	-3.00	-3.41	-5.34	-6.13	-4.46	Sociology	0.15	0.17	0.17	0.19	0.18	0.15	0.18	0.17
Medical and health services	-2.27	-1.78	-1.66	-1.26	-1.39	-2.26	-2.57	-1.88	All other social sciences	0.17	0.16	0.16	0.20	0.20	0.16	0.20	0.18
Computer science and IT	-1.93	-2.18	-2.27	-1.62	-1.42	-1.74	-1.25	-1.77	Agricultural sciences	0.19	0.13	0.20	0.20	0.22	0.20	0.22	0.19
Medical support	-1.25	-0.99	-1.12	-0.76	-0.79	-1.18	-1.27	-1.05	Public administration	0.18	0.14	0.14	0.20	0.24	0.20	0.27	0.20
Physics	-0.60	-0.76	-0.80	-0.45	-0.35	-0.51	-0.26	-0.53	Family and consumer sciences	0.20	0.23	0.24	0.20	0.17	0.18	0.18	0.20
Mathematics	-0.47	-0.59	-0.64	-0.37	-0.29	-0.42	-0.20	-0.42	Human resources	0.22	0.16	0.15	0.19	0.24	0.20	0.27	0.21
Electrical engineering	-0.44	-0.63	-0.66	-0.30	-0.19	-0.33	-0.06	-0.37	Social work	0.17	0.23	0.22	0.25	0.23	0.17	0.18	0.21
Multidisciplinary or general science	-0.39	-0.32	-0.30	-0.18	-0.20	-0.38	-0.39	-0.31	History	0.24	0.21	0.23	0.22	0.23	0.23	0.26	0.23
Biological sciences	-0.35	-0.26	-0.25	-0.09	-0.11	-0.31	-0.33	-0.24	Area, ethnic, and civilization studies	0.22	0.22	0.22	0.25	0.26	0.24	0.28	0.24
All other engineering	-0.28	-0.44	-0.46	-0.18	-0.10	-0.21	0.00	-0.24	Linguistics	0.26	0.27	0.26	0.27	0.26	0.25	0.28	0.26
Engineering technologies	-0.23	-0.36	-0.38	-0.15	-0.09	-0.18	-0.01	-0.20	Fine arts	0.27	0.30	0.30	0.24	0.21	0.26	0.31	0.27
Fitness, nutrition, and leisure	-0.25	-0.16	-0.10	-0.07	-0.12	-0.25	-0.31	-0.18	English literature	0.30	0.30	0.30	0.30	0.31	0.30	0.34	0.31
Accounting	-0.16	-0.25	-0.29	-0.27	-0.03	-0.18	0.00	-0.17	All other physical sciences	0.32	0.25	0.24	0.36	0.39	0.34	0.42	0.33
Marketing	-0.22	-0.24	-0.18	-0.18	-0.14	-0.11	-0.04	-0.16	Journalism	0.34	0.34	0.35	0.36	0.39	0.39	0.45	0.37
General business	-0.15	-0.20	-0.16	-0.14	-0.08	-0.11	-0.02	-0.12	Film and visual arts	0.40	0.42	0.41	0.37	0.35	0.39	0.44	0.40
Business mgmt and administration	-0.10	-0.16	-0.14	-0.10	-0.02	-0.06	0.03	-0.08	Music and drama	0.45	0.45	0.44	0.42	0.42	0.44	0.48	0.44
Economics	-0.10	-0.17	-0.15	-0.10	-0.01	-0.06	0.05	-0.08	Environmental studies	0.44	0.40	0.41	0.47	0.48	0.45	0.50	0.45
Finance	-0.06	-0.15	-0.14	-0.10	0.06	-0.02	0.11	-0.04	Civil engineering	0.46	0.31	0.33	0.48	0.54	0.49	0.61	0.46
Psychology	-0.07	-0.01	0.00	0.07	0.05	-0.06	-0.06	-0.01	Philosophy and religion	0.51	0.49	0.50	0.51	0.50	0.46	0.48	0.49
All other business	0.01	-0.04	-0.01	0.01	0.07	0.06	0.13	0.03	Hospitality	0.51	0.49	0.59	0.46	0.47	0.58	0.50	0.51
Chemistry	0.05	0.03	0.03	0.16	0.18	0.10	0.16	0.10	Legal studies	0.52	0.51	0.50	0.49	0.54	0.54	0.58	0.53
Communications	0.07	0.06	0.09	0.11	0.13	0.12	0.17	0.11	All other education	0.63	0.63	0.61	0.53	0.45	0.52	0.47	0.55
Mechanical engineering	0.09	-0.08	-0.08	0.15	0.21	0.17	0.33	0.11	General education	0.66	0.67	0.65	0.55	0.45	0.55	0.49	0.57
International relations	0.09	0.05	0.06	0.12	0.17	0.13	0.22	0.12	Criminal justice and fire protection	0.61	0.58	0.58	0.59	0.59	0.56	0.58	0.58
Political science	0.12	0.08	0.10	0.13	0.16	0.14	0.21	0.13	Precision production and industrial arts	0.63	0.54	0.58	0.61	0.63	0.62	0.65	0.61
Liberal arts and humanities	0.14	0.15	0.16	0.14	0.13	0.12	0.14	0.14	Elementary education	0.81	0.82	0.78	0.66	0.54	0.68	0.60	0.70
Chemical engineering	0.11	-0.01	-0.01	0.18	0.25	0.20	0.34	0.15	Architecture	0.78	0.75	0.76	0.75	0.76	0.77	0.82	0.77
Commercial art and graphic design	0.16	0.21	0.19	0.09	0.05	0.17	0.25	0.16	Library science	0.84	0.83	0.83	0.83	0.83	0.83	0.89	0.84
Total	-0.04	-0.04	-0.03	0.05	0.07	-0.02	0.02	0	Total	-0.04	-0.04	-0.03	0.05	0.07	-0.02	0.02	0

Notes: Skill mismatch is defined according to equation (3.1), and normalized to have a mean of zero and standard deviation of one (across all college majors, MSAs and years). This table shows average skill mismatch, separately by college major and by year.

Table 3.4. The Effect of Skill Mismatch on Employment, Unemployment and Labor Force Participation

	Dependent variable:				
	Employed				
	Any	Part-time	Full-time	Unemployed	Out of labor force
	(1)	(2)	(3)	(4)	(5)
Skill mismatch ×	-0.006***	0.008***	-0.014***	0.004***	0.002
1-2 years of potential exp.	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Skill mismatch ×	-0.006***	0.003**	-0.008***	0.003***	0.003***
3-4 years of potential exp.	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Skill mismatch ×	-0.008***	0.000	-0.008***	0.002**	0.005***
5-6 years of potential exp.	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
MSA × cohort FEs	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓
R^2	0.061	0.052	0.074	0.031	0.072
Observations	162,508	162,508	162,508	162,508	162,508

Notes: Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 3.5. The Effect of Skill Mismatch on Occupations

	Dependent variable:			
	Employed in college/top 5/top 10 occupation			
	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)
Skill mismatch × 1-2 years of potential exp.	-0.010***	-0.008***	-0.018***	-0.017***
	(0.002)	(0.002)	(0.003)	(0.004)
Skill mismatch × 3-4 years of potential exp.	-0.006***	-0.005***	-0.016***	-0.016***
	(0.002)	(0.002)	(0.004)	(0.004)
Skill mismatch × 5-6 years of potential exp.	-0.004**	-0.003*	-0.016***	-0.018***
	(0.002)	(0.002)	(0.005)	(0.004)
MSA × cohort FEs	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓
R^2	0.124	0.132	0.173	0.136
Observations	146,566	146,566	146,566	146,566

Notes: Sample excludes the non-employed. Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 3.6. The Effect of Skill Mismatch on Earnings and Wages

	Dependent variable:	
	Log annual income	Log hourly wage
	(1)	(2)
Skill mismatch × 1-2 years of potential exp.	-0.049***	-0.031***
	(0.005)	(0.004)
Skill mismatch × 3-4 years of potential exp.	-0.036***	-0.027***
	(0.005)	(0.004)
Skill mismatch × 5-6 years of potential exp.	-0.034***	-0.026***
	(0.006)	(0.005)
MSA × cohort FEs	✓	✓
College major × cohort FEs	✓	✓
Year FEs	✓	✓
Potential experience FEs	✓	✓
Individual controls	✓	✓
R^2	0.181	0.199
Observations	150,844	150,844

Notes: Sample restricted to individuals with positive wage income. Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 3.7. The Effect of Skill Mismatch in Current vs. Graduation Year

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Skill mismatch (grad. year) × 1-2 years of potential exp.	-0.037** (0.017)	-0.016* (0.009)	-0.007 (0.004)	0.006 (0.004)	0.004 (0.002)	-0.006 (0.006)	-0.005 (0.006)	-0.016** (0.007)	-0.009 (0.006)
Skill mismatch (grad. year) × 3-4 years of potential exp.	-0.024*** (0.006)	-0.027*** (0.006)	-0.004 (0.003)	-0.004 (0.003)	0.003* (0.001)	0.000 (0.005)	0.002 (0.005)	-0.007 (0.005)	-0.004 (0.005)
Skill mismatch (grad. year) × 5-6 years of potential exp.	-0.044*** (0.012)	-0.033*** (0.009)	-0.007** (0.003)	0.007 (0.005)	-0.001 (0.002)	-0.007 (0.004)	-0.004 (0.004)	-0.020*** (0.007)	-0.019*** (0.006)
Skill mismatch (current year) × 1-2 years of potential exp.	-0.014 (0.021)	-0.018 (0.012)	0.001 (0.004)	0.003 (0.005)	0.001 (0.002)	-0.005 (0.007)	-0.004 (0.007)	-0.003 (0.009)	-0.009 (0.008)
Skill mismatch (current year) × 3-4 years of potential exp.	-0.016* (0.008)	-0.000 (0.005)	-0.003 (0.003)	0.008*** (0.003)	0.000 (0.002)	-0.008 (0.005)	-0.009* (0.005)	-0.010* (0.006)	-0.014** (0.006)
Skill mismatch (current year) × 5-6 years of potential exp.	0.012 (0.014)	0.008 (0.007)	-0.001 (0.004)	-0.009 (0.007)	0.004* (0.002)	0.003 (0.004)	0.000 (0.004)	0.005 (0.005)	0.001 (0.006)
MSA × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.181	0.199	0.061	0.052	0.031	0.124	0.132	0.173	0.136
Observations	150,844	150,844	162,508	162,508	162,508	146,566	146,566	146,566	146,566

Notes: Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 3.8. The Effect of Overall Unemployment Rates at the MSA Level in Graduation Year

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment rate (%) × 1-2 years of potential exp.	-0.024***	-0.009**	-0.010***	0.005**	0.005***	-0.006**	-0.005*	-0.005	-0.001
	(0.007)	(0.004)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.004)	(0.003)
Unemployment rate (%) × 3-4 years of potential exp.	-0.014**	-0.008**	-0.005***	0.002	0.004**	-0.005**	-0.004	-0.005	-0.004
	(0.005)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Unemployment rate (%) × 5-6 years of potential exp.	-0.003	-0.003	-0.003	-0.002	0.001	-0.002	-0.001	-0.002	-0.001
	(0.005)	(0.004)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.004)	(0.003)
MSA FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.169	0.187	0.050	0.040	0.020	0.112	0.120	0.164	0.126
Observations	140,041	140,041	151,104	151,104	151,104	136,092	136,092	136,092	136,092

Notes: Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 3.9. The Effect of Skill Mismatch on Males vs. Females

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Men									
Skill mismatch ×	-0.054***	-0.035***	-0.008***	0.006***	0.005***	-0.004	-0.004	-0.023***	-0.018***
1-2 years of potential exp.	(0.006)	(0.005)	(0.002)	(0.002)	(0.001)	(0.004)	(0.004)	(0.004)	(0.004)
Skill mismatch ×	-0.036***	-0.030***	-0.007***	-0.001	0.003**	-0.001	-0.000	-0.020***	-0.017***
3-4 years of potential exp.	(0.008)	(0.005)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.004)	(0.004)
Skill mismatch ×	-0.032***	-0.029***	-0.005***	-0.002	0.003**	-0.000	0.002	-0.019***	-0.017***
5-6 years of potential exp.	(0.008)	(0.006)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.004)	(0.004)
R^2	0.219	0.225	0.066	0.084	0.054	0.149	0.156	0.170	0.139
Observations	65,686	65,686	69,559	69,559	69,559	64,052	64,052	64,052	64,052
Panel B: Women									
Skill mismatch ×	-0.042***	-0.026***	-0.002	0.011***	0.002*	-0.014***	-0.010***	-0.012**	-0.014***
1-2 years of potential exp.	(0.008)	(0.007)	(0.003)	(0.003)	(0.001)	(0.004)	(0.003)	(0.005)	(0.004)
Skill mismatch ×	-0.033***	-0.019***	-0.003	0.007***	0.001	-0.010***	-0.007**	-0.010*	-0.014***
3-4 years of potential exp.	(0.005)	(0.004)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.006)	(0.004)
Skill mismatch ×	-0.025***	-0.017***	-0.006*	0.001	-0.000	-0.007***	-0.007**	-0.011	-0.018***
5-6 years of potential exp.	(0.005)	(0.004)	(0.004)	(0.003)	(0.001)	(0.003)	(0.003)	(0.008)	(0.005)
R^2	0.171	0.199	0.103	0.059	0.041	0.139	0.146	0.208	0.168
Observations	85,158	85,158	92,949	92,949	92,949	82,514	82,514	82,514	82,514

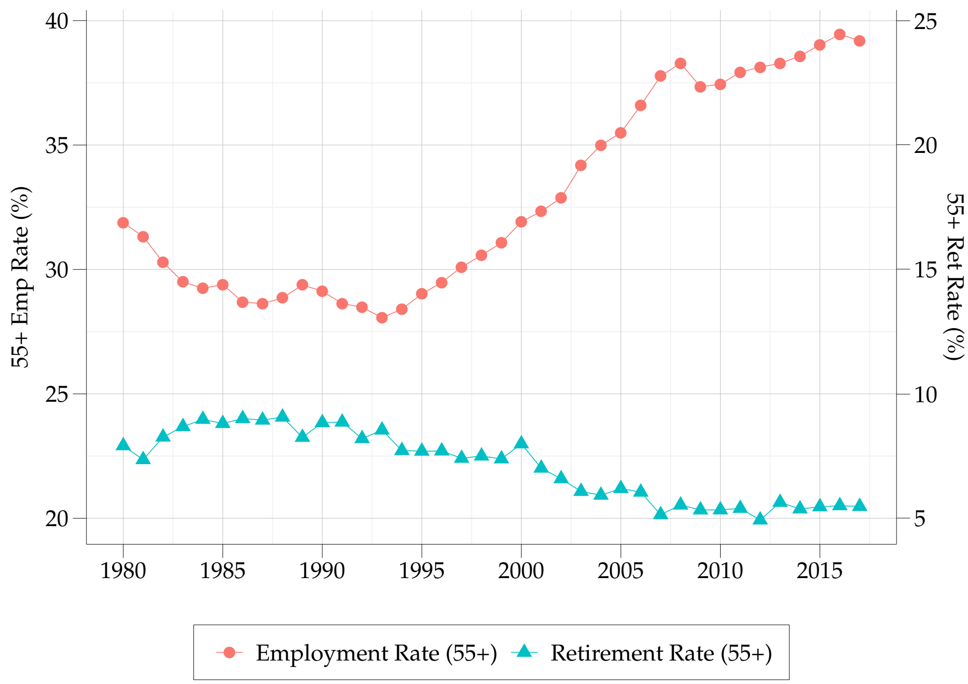
Notes: Individual controls include race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table 3.10. Wage Regressions: Alternative Specifications

	Dependent variable: Log hourly wage					
	Baseline				JOLTS-adjusted	
	(1)	(2)	(3)	(4)	(5)	(6)
Skill mismatch ×	-0.031***	-0.030***	-0.035***	0.013	-0.035***	0.008
1-2 years of potential exp.	(0.004)	(0.004)	(0.004)	(0.014)	(0.006)	(0.010)
Skill mismatch ×	-0.027***	-0.028***	-0.031***	0.017	-0.031***	0.013
3-4 years of potential exp.	(0.004)	(0.004)	(0.004)	(0.014)	(0.006)	(0.011)
Skill mismatch ×	-0.026***	-0.028***	-0.032***	0.017	-0.029***	0.014
5-6 years of potential exp.	(0.005)	(0.004)	(0.005)	(0.013)	(0.007)	(0.010)
MSA × cohort FEs	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓		✓	✓	✓	✓
MSA × year FEs		✓				
College major × year FEs		✓				
College major × state FEs			✓			
College major group × MSA FEs			✓			
College major × MSA FEs				✓		✓
Potential experience FEs	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
R^2	0.199	0.212	0.234	0.273	0.199	0.273
Observations	150,844	150,844	150,844	150,844	150,844	150,844

Notes: The JOLTS-adjusted measure of skill mismatch is described in Section 3.4.6. Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

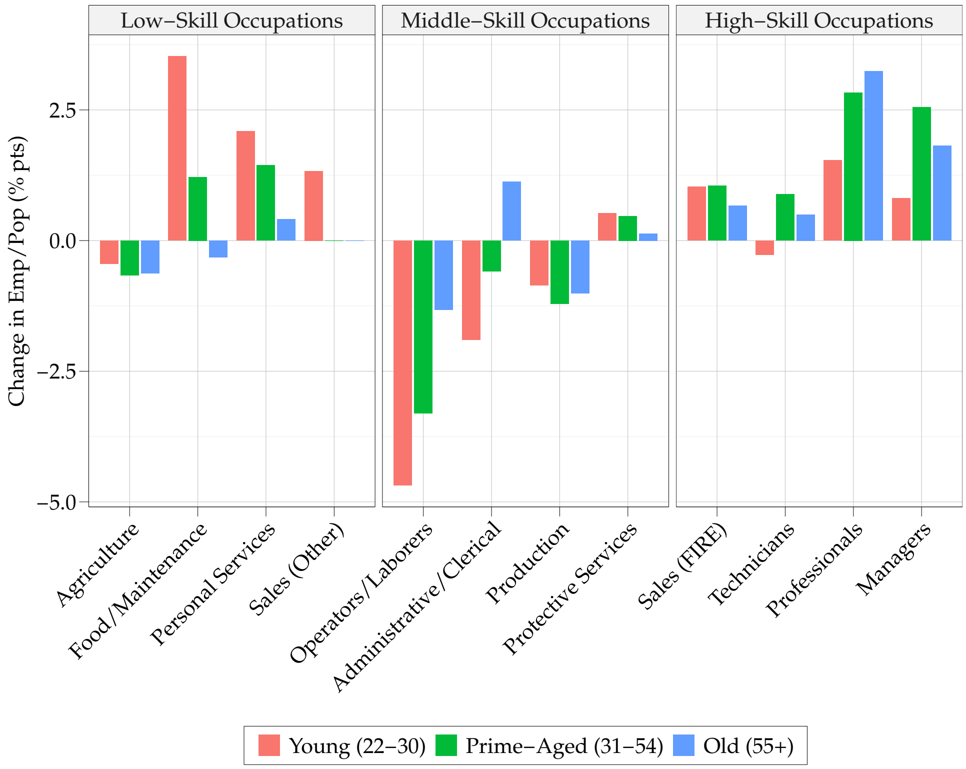
Figure 1.1. Employment and Retirement Rates Among Americans Aged 55+, 1980-2017



Notes: In this graph, the retirement rate is defined as the number of individuals who worked 26+ weeks in the previous year and are currently out of the labor force, divided by the number of individuals who worked 26+ weeks in the previous year, regardless of current labor force status. The employment rate is the employment-to-population ratio.

Source: 1980-2017 Annual Social and Economic Supplement of the Current Population Survey (Flood et al., 2017).

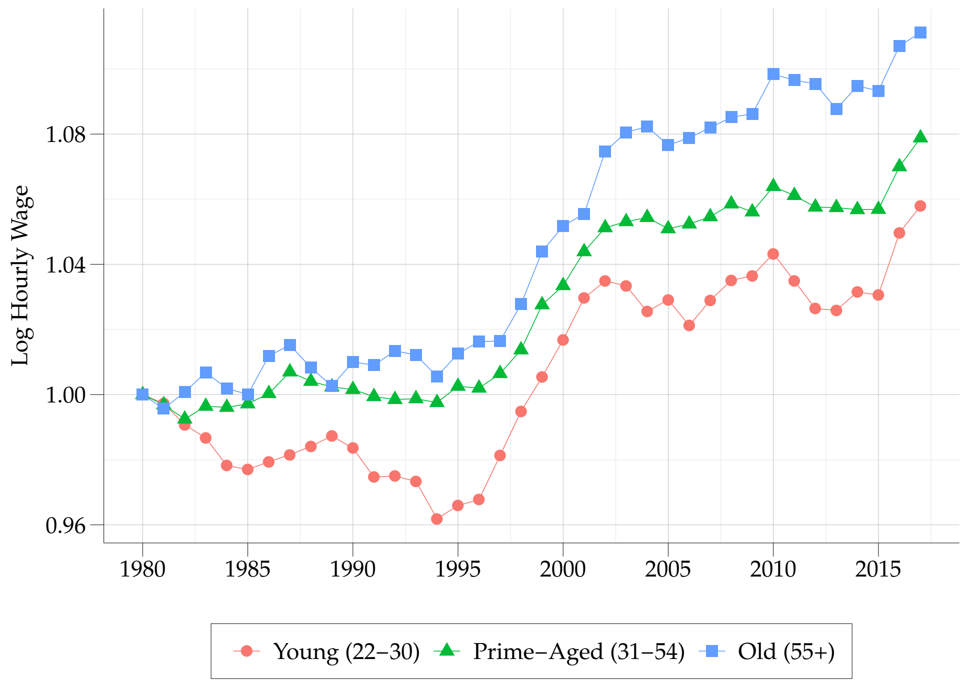
Figure 1.2. Changes in Occupational Composition by Age Group, 1980-2007



Notes: Employment rates are defined as the ratio of occupation group-specific employment to total population, separately by age group. Occupation groups (x-axis) are ranked according to mean hourly wage in 2000.

Source: 1980 Census, 2007 American Community Survey.

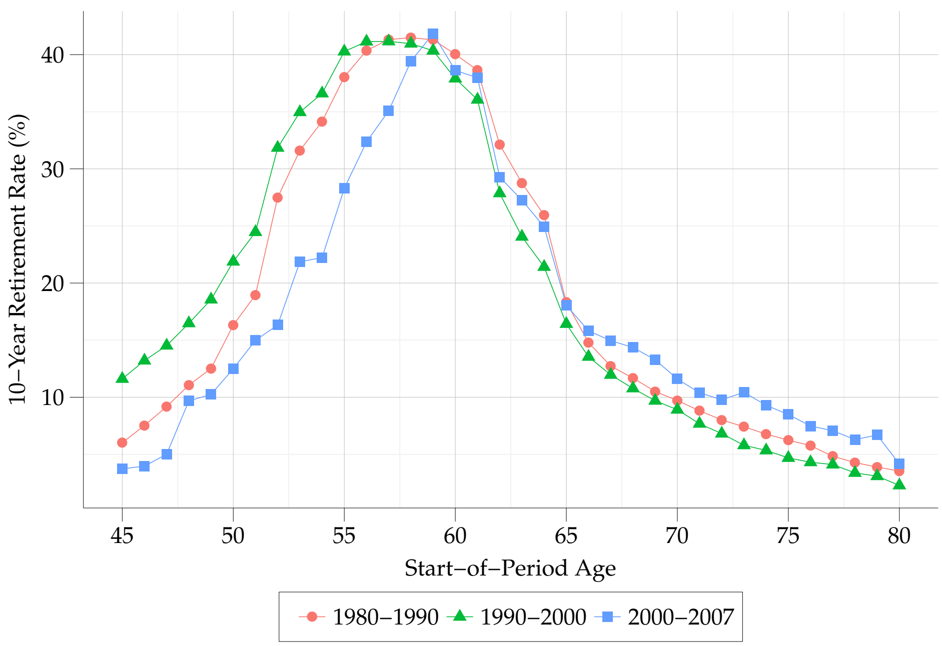
Figure 1.3. Mean Hourly Wages by Age Group, 1980-2017



Notes: Real log hourly wages are normalized to 1 in 1980 (see Appendix A.2 for wage construction details).

Source: 1980-2017 Annual Social and Economic Supplement of the Current Population Survey.

Figure 1.4. 10-Year Retirement Rates by Birth Cohort, 1980-2007

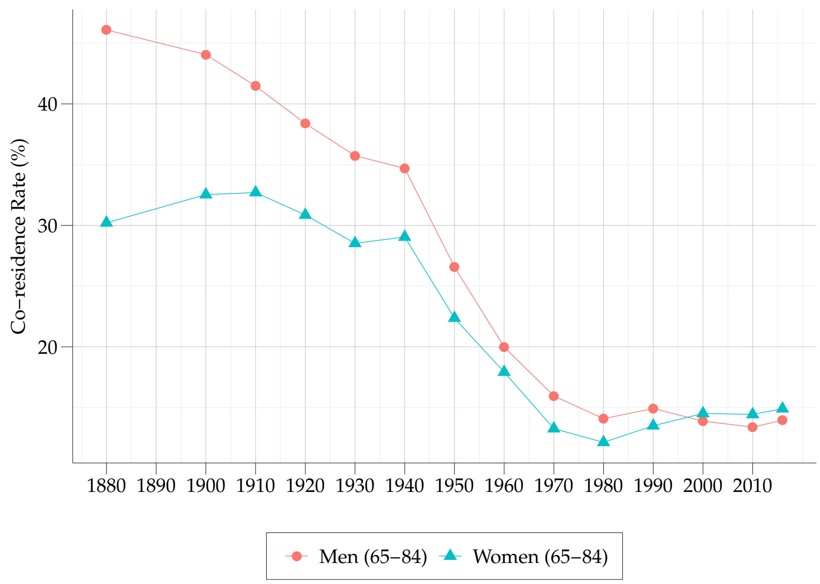


Notes: The 10-year retirement rate for a specific birth cohort (x-axis) is defined as the start-of-period employment rate of this cohort minus the end-of-period employment rate of the same cohort, at the national level. 7-year retirement rates in 2000-2007 are converted into 10-year equivalents by scaling them by 10/7.

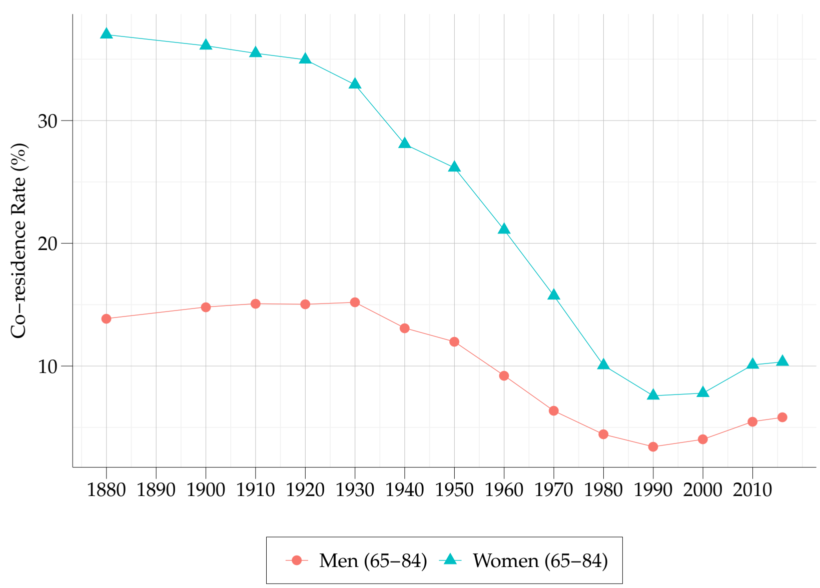
Source: 1980, 1990, 2000 Census, 2007 American Community Survey.

Figure 2.1. Co-residence as Household Head vs. Dependent Among Men and Women Aged 65-84, 1880-2016

Panel A: As household head

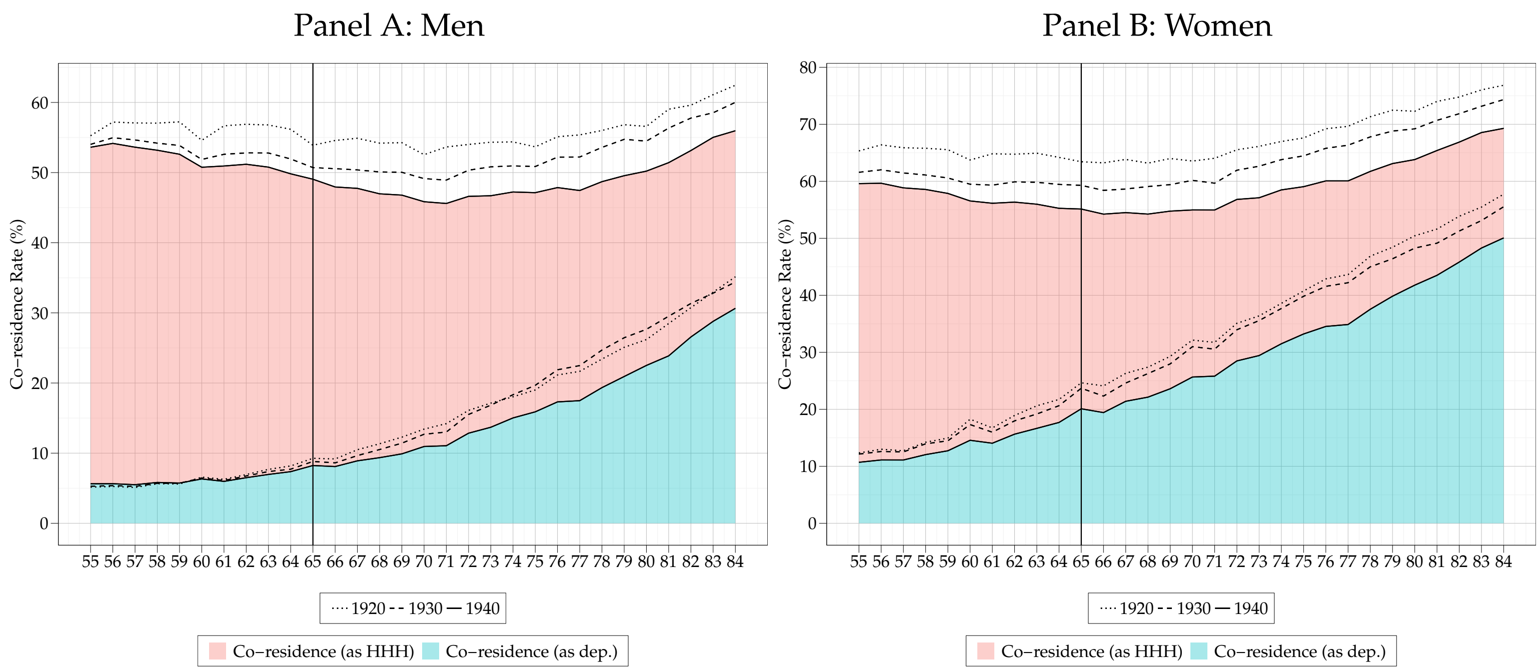


Panel B: As dependent



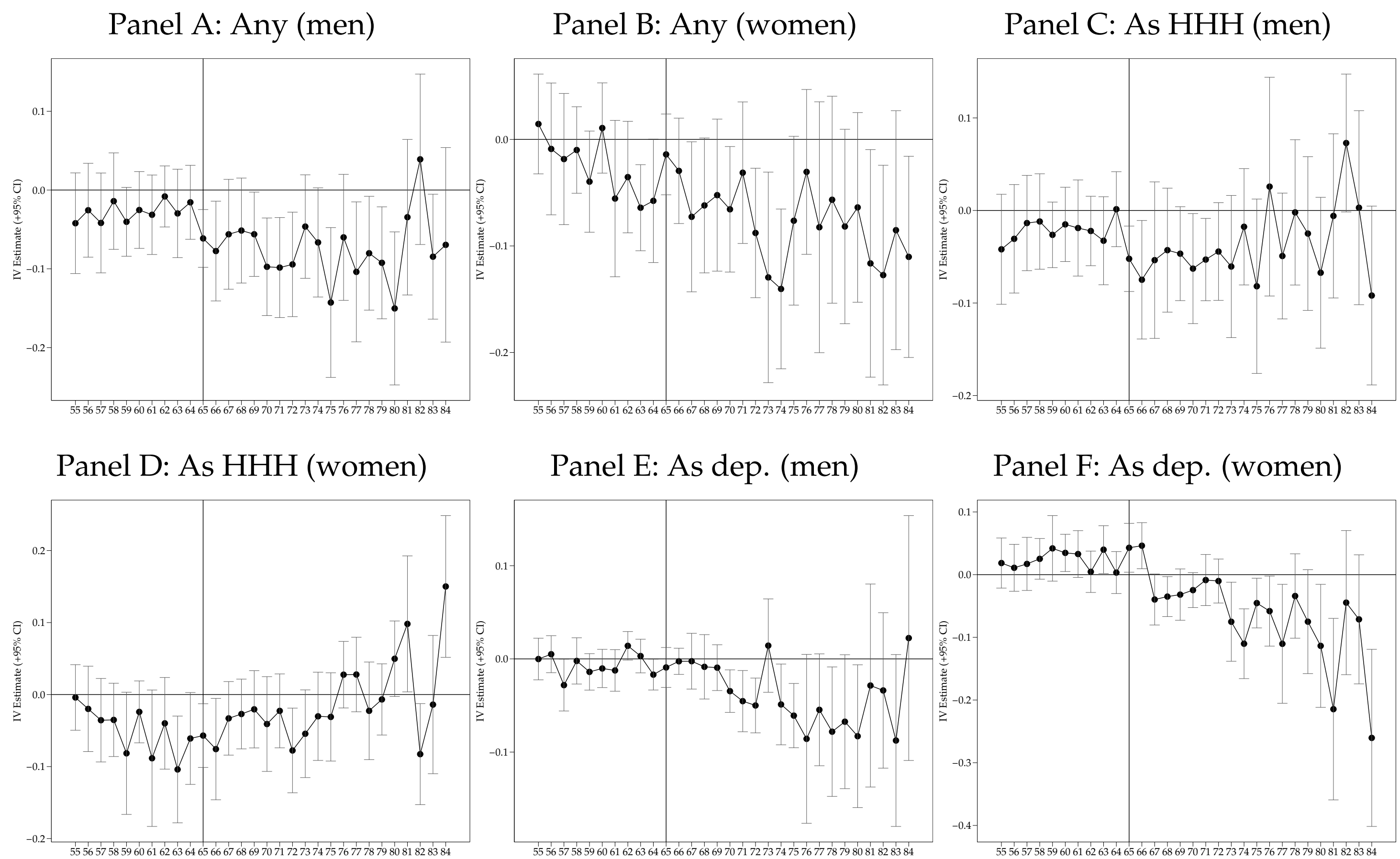
Notes: This figure plots the share of Americans aged 65-84 who are co-residing with relatives as household heads and dependents over the period 1880-2016, separately by gender.

Figure 2.2. Co-residence Rates by Age, 1920-1940



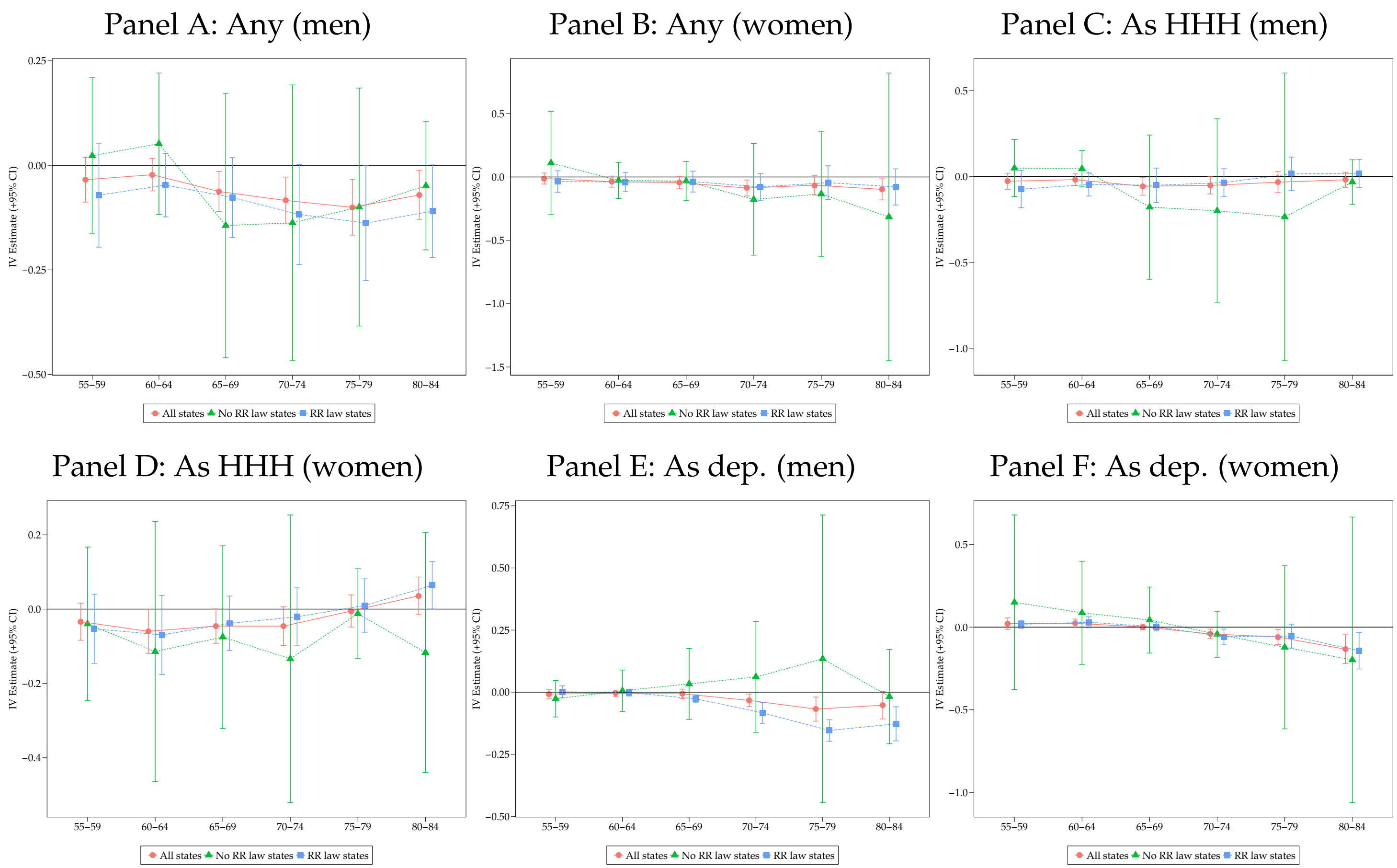
Notes: This figure plots co-residence rates by age, separately by gender. The upper three lines depict overall co-residence rates, while the bottom three lines depict co-residence rates as a dependent, for 1920 (dotted lines), 1930 (dashed lines) and 1940 (solid lines). The red-shaded and blue-shaded areas decompose overall co-residence rates in 1940 into co-residence as a household head and as a dependent respectively.

Figure 2.3. The Effect of OAA on Co-residence by Age



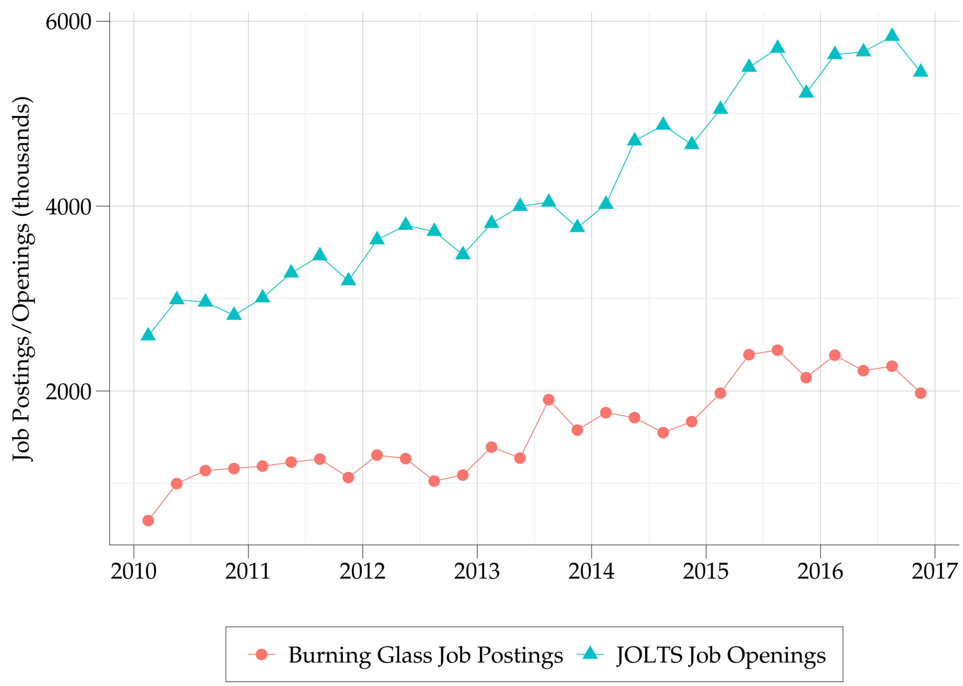
Notes: Each dot represents the IV coefficient corresponding to OAA per-65+ payments interacted with the relevant age dummy (x-axis) from regression (2.1), where the dependent variable and gender sample is indicated in the panel title. The error bars represent the corresponding 95% confidence intervals based on robust standard errors clustered at the state level.

Figure 2.4. The Effect of OAA on Co-residence: States with vs. without Relative Responsibility Laws



Notes: Each dot/triangle/square represents the IV coefficient corresponding to OAA per-65+ payments interacted with the relevant age group dummy (x-axis) from regression (2.1), where the dependent variable and gender sample is indicated in the panel title, separately for all states (red dots), states with relative responsibility laws (blue squares) and states without relative responsibility laws (green triangles). The error bars represent the corresponding 95% confidence intervals based on robust standard errors clustered at the state level.

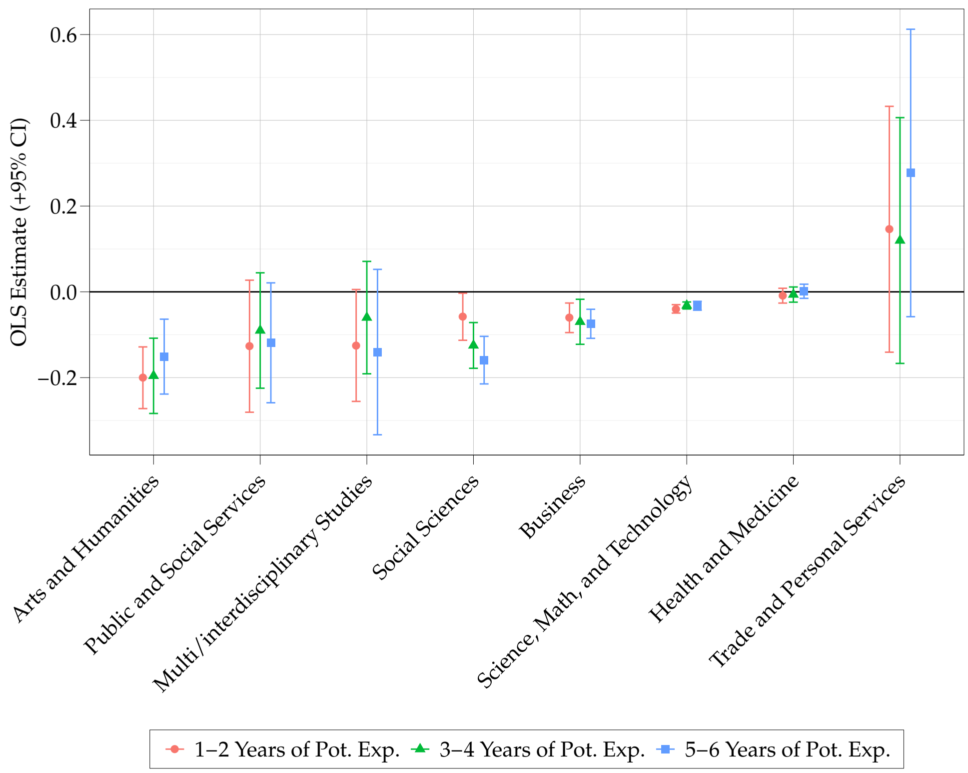
Figure 3.1. Quarterly Job Postings/Openings: JOLTS vs. Burning Glass, 2010-2016



Notes: Monthly job posting and job opening counts are averaged by quarter.

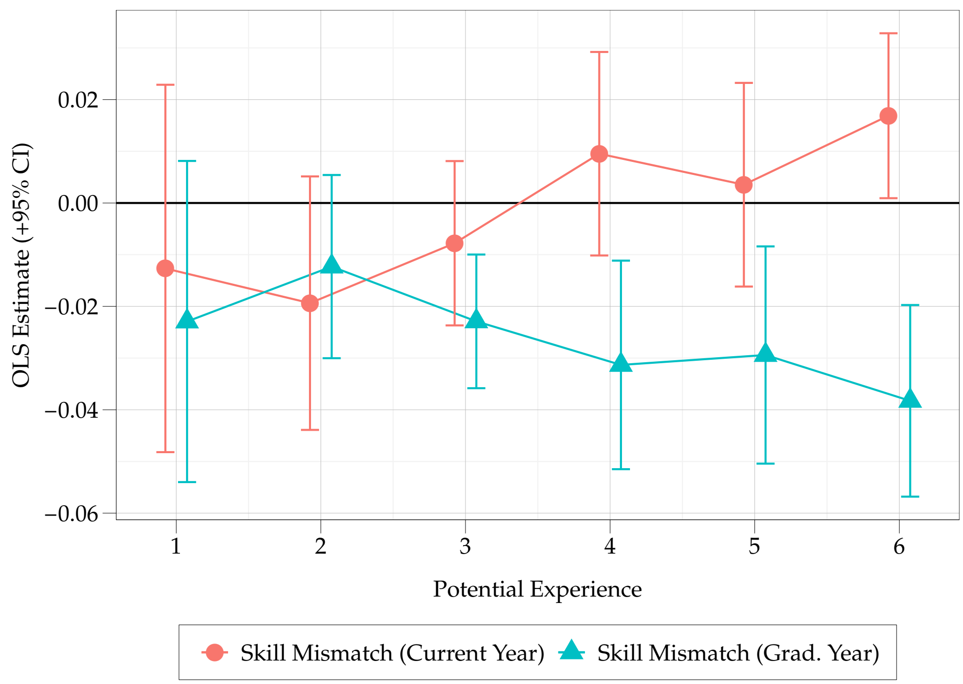
Source: Job Openings and Labor Turnover Survey, Burning Glass Technologies.

Figure 3.2. The Effect of Skill Mismatch on Wages: Heterogeneity by College Major Group



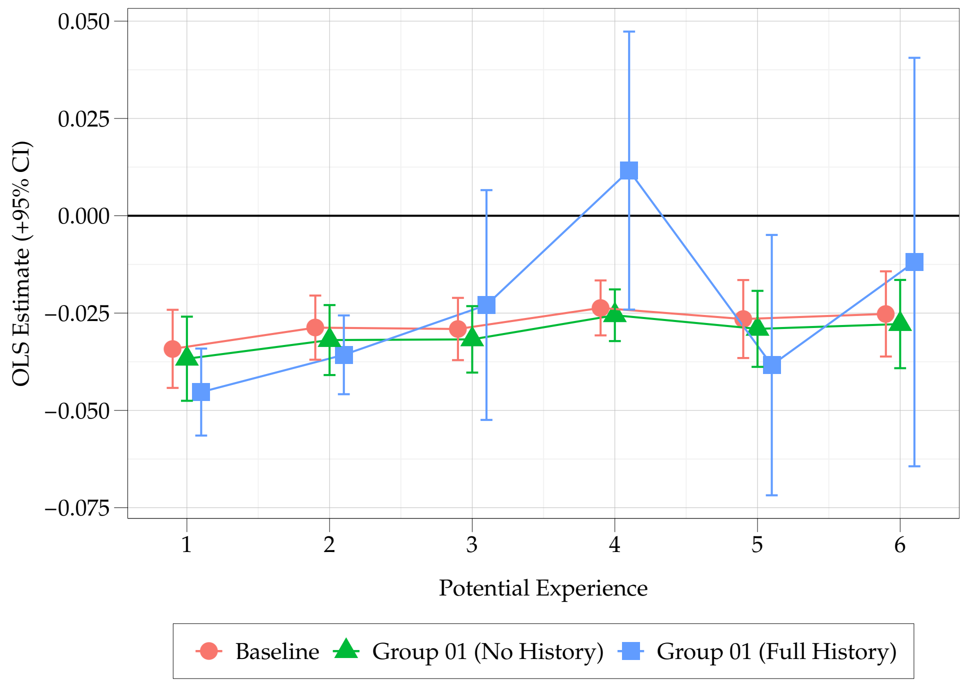
Notes: Each dot/triangle/square represents the OLS coefficient corresponding to the interaction between skill mismatch, a potential experience group (see legend) and one of the eight college major groups (x-axis) in regression (3.3) where the dependent variable is log hourly wage. The vertical error bars represent the corresponding 95% confidence intervals, based on robust standard errors clustered at the state level.

Figure 3.3. The Effect of Skill Mismatch on Wages in Current vs. Graduation Year



Notes: Each dot/triangle represents the OLS coefficient corresponding to the interaction between skill mismatch in current or graduation year (see legend) and a potential experience dummy (x-axis) in regression (3.4) where the dependent variable is log hourly wage. The vertical error bars represent the corresponding 95% confidence intervals, based on robust standard errors clustered at the state level.

Figure 3.4. The Effect of Skill Mismatch on Wages: Baseline vs. Full History



Notes: Each dot/triangle/square represents the OLS coefficient corresponding to the interaction between skill mismatch and a potential experience dummy (x-axis) in regression (3.3) or (3.5) (see legend) where the dependent variable is log hourly wage. The vertical error bars represent the corresponding 95% confidence intervals, based on robust standard errors clustered at the state level.

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APPENDIX A

Appendix to Chapter 1

A.1. Mapping Census/ACS Geography to Commuting Zones

The smallest geographic unit available in the Census and ACS varies by year. In the 1980 Census, so-called county groups—typically metropolitan areas plus surrounding counties—are identifiable. Since 1990, the most disaggregated geographic unit reported in the Census/ACS are Public Use Microdata Areas (PUMA), which are subareas comprising between 100,000 and 200,000 residents. When a county group or PUMA overlaps with multiple CZs, individuals in those areas are assigned to each of those CZs with weights that add up to one. These weights are based on how the county group/PUMA population is distributed across CZs, and implicitly assume individuals have been sampled at random. For instance, if a PUMA overlaps with two CZs and its population is equally split between them, individuals in this PUMA are assigned to both CZs with half weights. As a result, they will contribute to aggregate outcomes in both locations. County groups and PUMAs were mapped to CZs using crosswalks made available by David Dorn on his website.[1]

A.2. Computing Hourly Wages

Hourly wages are computed by dividing annual wage income by the product of weeks worked last year and usual hours worked per week. In the Census, top-coded

[1] `http://ddorn.net/data.htm`.

wage incomes are automatically replaced by the state median value above the threshold, except in 1980. For that year, I multiply top-coded incomes by 1.5, following Autor and Dorn (2013). Nominal wages are then converted into 2014 dollars using the Personal Consumption Expenditures chain-type price index released by the Bureau of Economic Analysis. Finally, I truncate the distribution of real wages at the top and bottom percentiles separately by year to neutralize the influence of outliers. All wage measures exclude the self-employed and part-time workers, defined as working less than 35 hours a week.

Hourly wages in Figure 1.3, which are based on Current Population Survey data, are computed analogously.

A.3. Assigning Educational Requirements to Occupations using O*NET

I assign a required level of education to each occupation using descriptions from the U.S. Department of Labor's Occupational Information Network (O*NET). O*NET uses its own variant of the Standard Occupational Classification to identify over 1,000 detailed occupations. For each occupation, O*NET surveys incumbent workers and occupational experts to understand the nature of the job, and among others includes a question on educational requirements. However, rather than a unique education level, O*NET reports the distribution of responses (e.g. 55% Bachelor's degree and 45% Associate's degree). I assign the education level with the highest response rate to every O*NET occupation. I then match each of the 330 Census occupations to the set of corresponding O*NET occupations using crosswalks published by the Bureau of Labor Statistics (exact one-to-many matching). When a Census occupation corresponds to multiple O*NET occupations with different educational requirements, I conservatively assign the highest education level to that occupation. The only exceptions to this rule are cases in which all but a few O*NET occupations have the same educational requirement, typically residual categories that lump together miscellaneous occupations (e.g. "Personal service occupations, not elsewhere classified"). In those instances, I assign the most common education level.

A.4. Generating Composition-Adjusted Wages

The procedure described here follows Shapiro (2006) and Albouy (2016). To generate composition-adjusted wage measures, I first run the following OLS regression using individual-level Census data (excl. the self-employed and part-time workers):

$$\log(\text{wage})_{ict} = \alpha_{ct} + X'_{it} \cdot \Gamma_t + \varepsilon_{ict} \tag{A.1}$$

where α_{ct} are year-specific CZ fixed effects and X'_{it} is a comprehensive set of individual-level controls, including gender, race, education, immigrant status, a quadratic in potential experience, industry of employment and occupation of employment. To make this regression implementable, every individual needs to be assigned to a unique commuting zone. In the same way that individuals in some areas are assigned to multiple CZs with "probabilistic" weights in order to construct CZ-level outcomes, here I randomly assign them to one of those CZs using the same probabilities. Regression (A.1) is run separately by demographic group, weighting observations by Census sampling weights. The estimated CZ fixed effects $\widehat{\alpha_{ct}}$ are then used to compute CZ-specific mean log wage differences $\Delta\ \widehat{\alpha_{ct}}$ that are not driven by changes in the local composition of workers, at least in terms of observables.

A.5. Constructing the Instrument based on Retirement Rates from Canada

This version of the instrument simply replaces U.S. retirement rates in (1.20) with corresponding retirement rates from Canada, which are constructed as described in Section 1.3.2 using Canadian Census data from 1981, 1991, 2001 and 2006 (Minnesota Population Center, 2018). Since age is truncated at 85 in the Canadian Census, this instrument only exploits CZ-level age composition within the 45 to 75 age range (instead of 45 to 80). Moreover, age is only available in 5-year bins in 2006. As an approximation, I compute bin-specific employment rates and "interpolate" them by regressing the resulting step function on a constant and a polynomial of degree 5 in age. For the 2001-2006 period, 5-year retirement rates are converted into 10-year equivalents by scaling them by 2.

A.6. Proof of Proposition 1

PROOF OF PROPOSITION 1. The proof proceeds in three steps: (1) derive the labor demand equations on the firm side (net of capital), (2) derive the labor supply equations on the worker side, and (3) combine them to obtain the equilibrium wage response, which determines the equilibrium occupational choice response.

Labor Demand. Start by totally differentiating the capital supply equation:

$$d \log r = \lambda \cdot d \log K \tag{A.2}$$

Next, totally differentiate the first-order condition for K in (1.4) and substitute for $d\log r$ using (A.2):

$$d\log K = \frac{1-\alpha}{1-\alpha+\lambda} d\log L \tag{A.3}$$

Totally differentiate the first-order condition for L_{gk} in (1.4) and substitute for $d\log K$ using (A.3) to obtain the labor demand equations:

(A.4)

$$d\log w_{gk} = \varphi \cdot d\log L + (\beta - 1)\cdot(d\log L_g - d\log L) + (\gamma - 1)\cdot(d\log L_{gk} - d\log L_g)$$

where $\varphi = -\alpha\lambda/(1-\alpha+\lambda)$, and the subscripts $g \in \{L, H\}$ and $k \in \{y, o\}$ respectively denote skill types and age types.

Labor Supply/Occupational Choice. Totally differentiate the CES aggregates (1.2) and (1.3):

$$d\log L = s_L \cdot d\log L_L + s_H \cdot d\log L_H \tag{A.5}$$

$$d\log L_g = s_{gy} \cdot d\log L_{gy} + s_{go} \cdot d\log L_{go} \tag{A.6}$$

where $s_g \in [0, 1]$ and $s_{gk} \in [0, 1]$ are initial (effective) labor shares given by:

$$s_g = \frac{\theta_g L_g^\beta}{\theta_L L_L^\beta + \theta_H L_H^\beta} \quad \text{and} \quad s_{gk} = \frac{\theta_{gk} L_{gk}^\gamma}{\theta_{gy} L_{gy}^\gamma + \theta_{go} L_{go}^\gamma}$$

Since the labor supply (L_y^ℓ, L_y^h) of younger workers is assumed to stay unchanged, totally differentiating the labor supply equations (1.7) and (1.8) yields:

$$d \log L_{Ly} = s_y^h \eta_u \cdot d \log u^* \tag{A.7}$$

$$d \log L_{Hy} = -s_u \eta_u \cdot d \log u^* \tag{A.8}$$

where $\eta_u > 0$ is the elasticity of the cumulative distribution function $\Gamma(\cdot)$ around the initial ability threshold u^*:

$$\eta_u = \frac{\partial \Gamma(u^*)}{\partial u^*} \cdot \frac{u^*}{\Gamma(u^*)} \tag{A.9}$$

and $s_y^h \in [0,1]$ is the initial share of youth low-skill labor that is high-educated while $s_u > 0$ is just a scaling factor:

$$s_y^h = \frac{\Gamma(u^*) \cdot L_y^h}{L_y^\ell + \Gamma(u^*) \cdot L_y^h} \quad \text{and} \quad s_u = \frac{u^* \cdot \Gamma(u^*)}{\int_{u^*}^{u^{\max}} u \cdot \Gamma'(u) \cdot du}$$

We can repeat the same steps for older workers, except that their labor supply is assumed to exogenously increase according to $d \log L_o^h \geqslant d \log L_o^\ell > 0$. Without loss of generality, let $d \log L_o^\ell = \delta \cdot d \log L_o^h$ where $\delta \leqslant 1$ by assumption. Then, we have:

$$d \log L_{Lo} = s_o^h \eta_z \cdot d \log z^* + (s_o^\ell \delta + s_o^h) \cdot d \log L_o^h \tag{A.10}$$

$$d \log L_{Ho} = -s_z \eta_z \cdot d \log z^* + d \log L_o^h \tag{A.11}$$

where $\eta_z > 0$ is the elasticity of the cumulative distribution function $\Lambda(\cdot)$ around the initial ability threshold z^*:

$$\eta_z = \frac{\partial \Lambda(z^*)}{\partial z^*} \cdot \frac{z^*}{\Lambda(z^*)} \tag{A.12}$$

and $s_o^\ell \in [0,1]$ and $s_o^h \in [0,1]$ are the initial shares of old low-skill labor that are respectively low-educated and high-educated while $s_z > 0$ is just a scaling factor:

$$s_o^\ell = \frac{L_o^\ell}{L_o^\ell + \Lambda(z^*) \cdot L_o^h} \quad \text{and} \quad s_o^h = \frac{\Lambda(z^*) \cdot L_o^h}{L_o^\ell + \Lambda(z^*) \cdot L_o^h} \quad \text{and} \quad s_z = \frac{z^* \cdot \Lambda(z^*)}{\int_{z^*}^{z^{\max}} z \cdot \Lambda'(z) \cdot dz}$$

Lastly, totally differentiate the threshold conditions (1.5)-(1.6):

$$d \log u^* = d \log w_{Ly} - d \log w_{Hy} \tag{A.13}$$

$$d \log z^* = d \log w_{Lo} - d \log w_{Ho} \tag{A.14}$$

These expressions can be inserted into (A.7)-(A.8) and (A.10)-(A.11) to yield the labor supply equations.

Equilibrium. In the competitive equilibrium, labor supply and labor demand have to be equal. In practice, this amounts to combining the four labor supply and four labor demand equations together and solving for wages. To simplify the notation, define the following set of constants:

$$C_0 \equiv s_L s_{Lo} \widetilde{\delta} + s_H s_{Ho},\ C_1^y \equiv s_L s_{Ly} s_y^h - s_H s_{Hy} s_u,\ C_1^o \equiv s_L s_{Lo} s_o^h - s_H s_{Ho} s_z$$

$$C_2^y \equiv s_{Ly} s_y^h + s_{Hy} s_u,\ C_2^o \equiv s_{Lo} s_o^h + s_{Ho} s_z,\ C_3^y \equiv s_{Lo} s_y^h + s_{Ho} s_u$$

$$C_3^o \equiv s_{Ly} s_o^h + s_{Hy} s_z,\ C_4^y \equiv s_{Hy} - s_{Ly} \widetilde{\delta},\ C_4^o \equiv s_{Ho} - s_{Lo} \widetilde{\delta}$$

where $\widetilde{\delta} \equiv s_o^\ell \delta + s_o^h \leqslant 1$.[2] Moreover, let $\Delta_y = d \log w_{Ly} - d \log w_{Hy}$ and $\Delta_o = d \log w_{Lo} - d \log w_{Ho}$. First, compute the change in high-skill, low-skill and overall labor by substituting the labor supply equations into (A.5)-(A.6):

$$d \log L_H = -s_{Hy} s_u \eta_u \cdot \Delta_y - s_{Ho} s_z \eta_z \cdot \Delta_o + s_{Ho} \cdot d \log L_o^h \tag{A.15}$$

$$d \log L_L = s_{Ly} s_y^h \eta_u \cdot \Delta_y + s_{Lo} s_o^h \eta_z \cdot \Delta_o + s_{Lo} \widetilde{\delta} \cdot d \log L_o^h \tag{A.16}$$

$$d \log L = \eta_u C_1^y \cdot \Delta_y + \eta_z C_1^o \cdot \Delta_o + C_0 \cdot d \log L_o^h \tag{A.17}$$

[2] Note that all constants are strictly positive, except C_4^y and C_4^o whose signs depend on $\widetilde{\delta}$ and initial labor shares.

Next, compute the following set of intermediate expressions:

$$d\log L_H - d\log L = -s_L\eta_u C_2^y \cdot \Delta_y - s_L\eta_z C_2^o \cdot \Delta_o + s_L C_4^o \cdot d\log L_o^h \tag{A.18}$$

$$d\log L_L - d\log L = s_H\eta_u C_2^y \cdot \Delta_y + s_H\eta_z C_2^o \cdot \Delta_o - s_H C_4^o \cdot d\log L_o^h \tag{A.19}$$

$$d\log L_{Ho} - d\log L_H = s_{Hy}s_u\eta_u \cdot \Delta_y - s_{Hy}s_z\eta_z \cdot \Delta_o + s_{Hy} \cdot d\log L_o^h \tag{A.20}$$

$$d\log L_{Lo} - d\log L_L = -s_{Ly}s_y^h\eta_u \cdot \Delta_y + s_{Ly}s_o^h\eta_z \cdot \Delta_o + s_{Ly}\widetilde{\delta} \cdot d\log L_o^h \tag{A.21}$$

$$d\log L_{Hy} - d\log L_H = -s_{Ho}s_u\eta_u \cdot \Delta_y + s_{Ho}s_z\eta_z \cdot \Delta_o - s_{Ho} \cdot d\log L_o^h \tag{A.22}$$

$$d\log L_{Ly} - d\log L_L = s_{Lo}s_y^h\eta_u \cdot \Delta_y - s_{Lo}s_o^h\eta_z \cdot \Delta_o - s_{Lo}\widetilde{\delta} \cdot d\log L_o^h \tag{A.23}$$

Substitute (A.17)-(A.21) into the first-order conditions (A.4) for L_{Ho} and L_{Lo} to get:

$$\begin{aligned} d\log w_{Ho} = {} & [\varphi \cdot \eta_u C_1^y - (\beta-1) \cdot s_L\eta_u C_2^y + (\gamma-1) \cdot s_{Hy}s_u\eta_u] \cdot \Delta_y \\ & + [\varphi \cdot \eta_z C_1^o - (\beta-1) \cdot s_L\eta_z C_2^o - (\gamma-1) \cdot s_{Hy}s_z\eta_z] \cdot \Delta_o \\ & + [\varphi \cdot C_0 + (\beta-1) \cdot s_L C_4^o + (\gamma-1) \cdot s_{Hy}] \cdot d\log L_o^h \end{aligned} \tag{A.24}$$

$$\begin{aligned} d\log w_{Lo} = {} & [\varphi \cdot \eta_u C_1^y + (\beta-1) \cdot s_H\eta_u C_2^y - (\gamma-1) \cdot s_{Ly}s_y^h\eta_u] \cdot \Delta_y \\ & + [\varphi \cdot \eta_z C_1^o + (\beta-1) \cdot s_H\eta_z C_2^o + (\gamma-1) \cdot s_{Ly}s_o^h\eta_z] \cdot \Delta_o \\ & + [\varphi \cdot C_0 - (\beta-1) \cdot s_H C_4^o + (\gamma-1) \cdot s_{Ly}\widetilde{\delta}] \cdot d\log L_o^h \end{aligned} \tag{A.25}$$

Subtracting (A.24) from (A.25), we get:

$$\Delta_o = \frac{(\beta-1)\cdot\eta_u C_2^y - (\gamma-1)\cdot\eta_u C_2^y}{1-(\beta-1)\cdot\eta_z C_2^o - (\gamma-1)\cdot\eta_z C_3^o}\cdot\Delta_y - \frac{(\beta-1)\cdot C_4^o + (\gamma-1)\cdot C_4^y}{1-(\beta-1)\cdot\eta_z C_2^o - (\gamma-1)\cdot\eta_z C_3^o}\cdot d\log L_o^h \tag{A.26}$$

Applying analogous steps to the first-order conditions for L_{Hy} and L_{Ly}:

$$\begin{aligned} d\log w_{Hy} = {} & [\varphi\cdot\eta_u C_1^y - (\beta-1)\cdot s_L\eta_u C_2^y - (\gamma-1)\cdot s_{Ho}s_u\eta_u]\cdot\Delta_y \\ & + [\varphi\cdot\eta_z C_1^o - (\beta-1)\cdot s_L\eta_z C_2^o + (\gamma-1)\cdot s_{Ho}s_z\eta_z]\cdot\Delta_o \\ & + [\varphi\cdot C_0 + (\beta-1)\cdot s_L C_4^o - (\gamma-1)\cdot s_{Ho}]\cdot d\log L_o^h \end{aligned} \tag{A.27}$$

$$\begin{aligned} d\log w_{Ly} = {} & [\varphi\cdot\eta_u C_1^y + (\beta-1)\cdot s_H\eta_u C_2^y + (\gamma-1)\cdot s_{Lo}s_y^h\eta_u]\cdot\Delta_y \\ & + [\varphi\cdot\eta_z C_1^o + (\beta-1)\cdot s_H\eta_z C_2^o - (\gamma-1)\cdot s_{Lo}s_o^h\eta_z]\cdot\Delta_o \\ & + [\varphi\cdot C_0 - (\beta-1)\cdot s_H C_4^o - (\gamma-1)\cdot s_{Lo}\widetilde{\delta}]\cdot d\log L_o^h \end{aligned} \tag{A.28}$$

Subtracting (A.27) from (A.28), we get:

$$\Delta_y = \frac{(\beta-1)\cdot\eta_z C_2^o - (\gamma-1)\cdot\eta_z C_2^o}{1-(\beta-1)\cdot\eta_u C_2^y - (\gamma-1)\cdot\eta_u C_3^y}\cdot\Delta_o - \frac{(\beta-1)\cdot C_4^o - (\gamma-1)\cdot C_4^o}{1-(\beta-1)\cdot\eta_u C_2^y - (\gamma-1)\cdot\eta_u C_3^y}\cdot d\log L_o^h \tag{A.29}$$

Finally, we can solve for the change in equilibrium youth wages Δ_y by combining (A.26) with (A.29):

(A.30)

$$\Delta_y = \underbrace{\frac{(\beta-1)\cdot\eta_z C_2^o - (\gamma-1)\cdot\eta_z C_2^o}{1-(\beta-1)\cdot\eta_u C_2^y - (\gamma-1)\cdot\eta_u C_3^y}\cdot\frac{(\beta-1)\cdot\eta_u C_2^y - (\gamma-1)\cdot\eta_u C_2^y}{1-(\beta-1)\cdot\eta_z C_2^o - (\gamma-1)\cdot\eta_z C_3^o}\cdot\Delta_y}_{\text{"amplifying" effect due to dependence of } \Delta_o \text{ on } \Delta_y \text{ and vice-versa (always positive)}}$$
$$\underbrace{-\frac{(\beta-1)\cdot\eta_z C_2^o - (\gamma-1)\cdot\eta_z C_2^o}{1-(\beta-1)\cdot\eta_u C_2^y - (\gamma-1)\cdot\eta_u C_3^y}\cdot\frac{(\beta-1)\cdot C_4^o + (\gamma-1)\cdot C_4^y}{1-(\beta-1)\cdot\eta_z C_2^o - (\gamma-1)\cdot\eta_z C_3^o}\cdot d\log L_o^h}_{\text{indirect effect of labor supply increase via } \Delta_o \text{ (negative if } \gamma>\beta)}$$
$$\underbrace{-\frac{(\beta-1)\cdot C_4^o - (\gamma-1)\cdot C_4^o}{1-(\beta-1)\cdot\eta_u C_2^y - (\gamma-1)\cdot\eta_u C_3^y}\cdot d\log L_o^h}_{\text{direct effect of labor supply increase (positive if } \gamma>\beta)}$$

Rearranging, we get $\Delta_y = (N/D)\times d\log L_o^h$ where

(A.31) $$N = -(\beta-\gamma)\cdot C_4^o + (\beta-\gamma)\cdot(\gamma-1)\cdot\eta_z\cdot(s_{Ho}-s_{Lo})\cdot(s_o^h + s_z\widetilde{\delta})$$

(A.32) $$\begin{aligned} D = {} & 1-(\gamma-1)\cdot[\eta_u\cdot(C_2^y+C_3^y)+\eta_z\cdot(C_2^o+C_3^o)] \\ & +(\gamma-1)^2\cdot\eta_u\eta_z\cdot(C_2^y+C_3^y)\cdot(C_2^o+C_3^o)-(\beta-\gamma)\cdot[\eta_u C_2^y+\eta_z C_2^o] \\ & +(\beta-\gamma)\cdot(\gamma-1)\cdot\eta_u\eta_z\cdot[C_2^y\cdot(C_2^o+C_3^o)+C_2^o\cdot(C_2^y+C_3^y)] \end{aligned}$$

To complete the proof, it suffices to show that $\Delta_y > 0$ if $\gamma > \beta$, which proves part (c) of the proposition, since parts (a) and (b) immediately follow in light of (A.13) and (A.7)-(A.8). In that case, $D > 1$ so that $\Delta_y > 0$ if and only if $N > 0$:

(A.33) $$(\gamma-\beta)\cdot[(s_{Ho}-s_{Lo}\widetilde{\delta})+(1-\gamma)\cdot\eta_z\cdot(s_{Ho}-s_{Lo})\cdot(s_o^h+s_z\widetilde{\delta})] > 0$$

which is guaranteed to hold under condition (A1) $\eta_z \approx 0$ or (A2) $s_{Ho} > s_{Lo}$. □

A.7. Appendix Tables and Figures

Table A.1. Distribution of Change in 55+ Employment Rate Across CZs, 1980-2007

	Period		
	1980-1990	1990-2000	2000-2007
	Panel A: Percentiles		
90th	0.005	0.050	0.120
75th	-0.007	0.039	0.094
50th	-0.022	0.027	0.069
25th	-0.038	0.012	0.034
10th	-0.053	-0.004	0.013

Panel B: Rank (40 largest CZs)

1	Orlando, FL	0.018	Minneapolis, MN	0.053	Pittsburgh, PA	0.120
2	Arlington CDP, VA	0.018	Denver, CO	0.048	Tampa, FL	0.114
3	Tampa, FL	0.016	Portland, OR	0.041	Portland, OR	0.111
4	Miami, FL	0.014	Tampa, FL	0.038	Miami, FL	0.106
5	San Diego, CA	0.013	Phoenix, AZ	0.037	Kansas, MO	0.103
20	Seattle, WA	-0.023	Houston, TX	0.022	San Diego, CA	0.088
21	Columbus, OH	-0.025	Detroit, MI	0.021	Atlanta, GA	0.087
36	Chicago, IL	-0.050	Philadelphia, PA	0.001	Columbus, OH	0.060
37	Pittsburgh, PA	-0.053	Los Angeles, CA	-0.013	San Antonio, TX	0.060
38	New Orleans, LA	-0.054	Bridgeport, CT	-0.015	Dallas, TX	0.059
39	Cleveland, OH	-0.058	Newark, NJ	-0.024	Detroit, MI	0.040
40	Houston, TX	-0.075	New York, NY	-0.034	San Jose, CA	0.037

Table A.2. Distribution of Predicted Retirement Intensity Across CZs, 1980-2007

	Period					
	1980-1990		1990-2000		2000-2007	
Panel A: Percentiles						
90th	0.220		0.215		0.180	
75th	0.217		0.212		0.178	
50th	0.212		0.207		0.175	
25th	0.208		0.203		0.172	
10th	0.205		0.200		0.170	
Panel B: Rank (40 largest CZs)						
1	West Palm Beach, FL	0.199	West Palm Beach, FL	0.180	West Palm Beach, FL	0.166
2	Tampa, FL	0.201	Tampa, FL	0.189	Pittsburgh, PA	0.169
3	Miami, FL	0.202	Miami, FL	0.199	Tampa, FL	0.169
4	Phoenix, AZ	0.213	Providence, RI	0.201	San Diego, CA	0.170
5	Orlando, FL	0.213	Pittsburgh, PA	0.202	Portland, OR	0.171
20	Kansas, MO	0.219	Baltimore, MD	0.212	Boston, MA	0.173
21	Providence, RI	0.219	Kansas, MO	0.212	Cincinnati, OH	0.173
36	Pittsburgh, PA	0.224	Atlanta, GA	0.217	Baltimore, MD	0.175
37	Arlington CDP, VA	0.224	Fort Worth, TX	0.217	Fort Worth, TX	0.176
38	Newark, NJ	0.224	Arlington CDP, VA	0.218	Dallas, TX	0.176
39	Baltimore, MD	0.224	Dallas, TX	0.219	New York, NY	0.178
40	Detroit, MI	0.225	Houston, TX	0.221	Arlington CDP, VA	0.178

Table A.3. Employment Share and Mean Hourly Wage by Occupation Group, 2000

	Employment share (%)			Mean wage (2014$)		
Top 5 occupations by occupation group	22-30	31-54	55+	22-30	31-54	55+
Panel A: Low-skill occupations						
Agriculture (5/8)	**1.19**	**1.22**	**2.33**	**11.62**	**14.31**	**16.00**
Farm workers, including nursery farming	0.62	0.38	0.42	10.62	12.21	13.40
Farmers (owners and tenants)	0.15	0.39	1.33	—	—	—
Farm managers	0.10	0.15	0.28	14.33	20.36	24.51
Timber, logging, and forestry workers	0.13	0.13	0.12	14.53	17.92	20.80
Animal caretakers, except on farms	0.12	0.10	0.09	12.71	14.55	16.37
Food/Maintenance (5/11)	**7.82**	**5.28**	**6.24**	**12.49**	**15.11**	**16.37**
Cooks	1.61	1.11	1.09	12.14	13.77	14.41
Janitors	0.99	1.36	2.34	13.46	16.53	17.61
Waiters and waitresses	1.95	0.64	0.41	11.96	13.13	14.20
Miscellaneous food preparation and service workers	0.82	0.52	0.74	11.41	13.12	13.97
Gardeners and groundskeepers	0.86	0.58	0.60	12.67	15.73	16.47
Personal Services (5/18)	**6.24**	**5.44**	**6.73**	**13.31**	**15.93**	**16.73**
Health and nursing aides	2.26	1.92	2.21	13.47	15.92	16.97
Child care workers	1.16	0.83	1.07	11.40	13.10	13.74
Housekeepers, maids, butlers, and cleaners	0.66	0.87	1.20	11.05	13.19	14.24
Hairdressers and cosmetologists	0.71	0.62	0.53	13.62	15.03	16.01
Recreation facility attendants	0.22	0.15	0.24	16.47	21.19	20.11
Sales (Other) (4/4)	**5.54**	**3.72**	**5.65**	**15.46**	**21.98**	**20.86**
Retail salespersons and sales clerks	3.34	2.46	3.89	17.62	25.27	22.90
Cashiers	2.04	1.11	1.43	11.84	14.66	15.45
Door-to-door sales, street sales and news vendors	0.12	0.12	0.21	15.19	18.44	21.17
Sales demonstrators, promoters and models	0.03	0.02	0.12	19.68	25.93	21.14

Table A.3. (cont.) Employment Share and Mean Hourly Wage by Occupation Group, 2000

	Employment share (%)			Mean wage (2014$)		
Top 5 occupations by occupation group	22-30	31-54	55+	22-30	31-54	55+
Panel B: Middle-skill occupations						
Operators/Laborers (5/59)	**13.04**	**12.20**	**11.40**	**15.48**	**19.54**	**21.32**
Truck, delivery and tractor drivers	2.67	2.89	3.05	15.93	19.81	21.49
Laborers, freight, stock and material handlers, n.e.c.	1.59	1.11	0.93	14.66	18.76	20.30
Assemblers of electrical equipment	1.32	1.16	0.95	15.50	18.74	20.57
Machine operators, n.e.c.	1.14	1.06	0.78	15.54	19.67	20.92
Construction laborers	1.24	0.84	0.53	15.74	19.97	22.10
Administrative/Clerical (5/41)	**17.03**	**16.10**	**16.91**	**15.78**	**19.99**	**21.37**
Secretaries and stenographers	2.38	3.01	3.58	15.92	19.13	20.74
Cust. service reps, investigators, adjusters, excl. insurance	2.40	1.40	0.93	16.41	21.01	21.73
Bookkeepers, and accounting and auditing clerks	1.11	1.34	1.79	15.80	18.72	19.85
Office supervisors	1.09	1.50	1.22	18.05	24.15	27.09
General office clerks	1.02	0.95	1.30	14.88	18.32	19.47
Production (5/68)	**11.38**	**12.28**	**9.68**	**17.43**	**23.53**	**26.72**
Carpenters	1.20	1.08	0.72	16.63	21.28	24.45
Production supervisors or foremen	0.70	1.20	0.96	19.02	25.39	30.58
Automobile mechanics and repairers	0.88	0.79	0.55	16.57	20.95	22.14
Supervisors of construction work	0.49	0.86	0.67	20.78	27.16	31.93
Mechanics and repairers, n.e.c.	0.51	0.62	0.67	16.60	21.78	22.85
Protective Services (5/7)	**2.15**	**2.03**	**1.94**	**19.20**	**26.18**	**24.00**
Police and detectives, public service	0.77	0.76	0.29	22.82	30.92	32.77
Guards and police, excluding public service	0.66	0.48	1.08	15.62	19.90	19.10
Sheriffs, bailiffs, correctional institution officers	0.39	0.36	0.20	18.88	24.18	25.19
Fire fighting, fire prevention and fire inspection	0.22	0.30	0.10	19.49	27.49	32.43
Protective service, n.e.c.	0.05	0.03	0.04	12.77	19.15	18.02

Table A.3. (cont.) Employment Share and Mean Hourly Wage by Occupation Group, 2000

	Employment share (%)			Mean wage (2014$)		
Top 5 occupations by occupation group	22-30	31-54	55+	22-30	31-54	55+
Panel C: High-skill occupations						
Sales (FIRE) (5/7)	**5.32**	**5.96**	**6.76**	**20.43**	**30.41**	**32.20**
Sales supervisors and proprietors	2.83	3.05	2.76	17.87	25.89	28.49
Salespersons, n.e.c.	1.30	1.42	1.63	21.84	32.47	32.68
Real estate sales occupations	0.26	0.58	1.35	21.82	33.96	30.99
Insurance sales occupations	0.29	0.41	0.60	20.11	32.72	36.60
Financial service sales occupations	0.39	0.30	0.24	31.67	55.31	60.22
Technicians (5/19)	**4.38**	**4.03**	**2.48**	**23.56**	**30.36**	**32.23**
Computer software developers	1.41	1.18	0.45	32.48	40.88	43.82
Licensed practical nurses	0.39	0.50	0.48	16.82	19.93	21.94
Legal assistants and paralegals	0.51	0.40	0.29	20.81	25.31	27.78
Engineering technicians	0.31	0.38	0.27	20.55	26.98	31.32
Clinical laboratory technologies and technicians	0.27	0.25	0.17	17.99	24.50	28.23
Professionals (5/67)	**15.66**	**17.24**	**16.15**	**22.58**	**32.72**	**37.80**
Primary school teachers	2.47	2.69	2.37	20.70	28.28	34.34
Registered nurses	1.24	2.16	1.63	24.85	29.90	32.00
Computer systems analysts and computer scientists	1.82	1.37	0.53	26.13	34.57	39.56
Subject instructors, college	0.87	0.84	1.45	17.02	30.65	41.40
Lawyers and judges	0.47	0.86	0.90	33.64	55.51	66.92
Managers (5/21)	**10.26**	**14.49**	**13.73**	**23.11**	**36.69**	**42.31**
Managers and administrators, n.e.c.	3.16	5.16	4.38	21.89	36.15	42.65
Accountants and auditors	1.55	1.51	1.37	23.49	30.83	32.52
Managers and specialists in marketing, advertising and PR	1.14	1.18	0.76	25.44	41.31	44.79
Chief executives, public administrators and legislators	0.20	1.04	1.59	34.12	65.12	70.63
Financial managers	0.59	0.89	0.64	24.88	39.63	42.85

Table A.4. Occupation Group Characteristics

			O*NET			1990 Census			
	Routine	Mean offshoring score	High school degree or less	Post-secondary certificate or associate's deg.	College degree or more	High school degree or less	Post-secondary certificate or associate's deg.	College degree or more	Total
Agriculture	0	-0.018	7	0	1	8	0	0	8
Food/Maintenance	2	-0.187	10	1	0	11	0	0	11
Personal Services	6	-0.190	9	7	2	15	3	0	18
Sales (Other)	1	1.038	4	0	0	4	0	0	4
Operators/Laborers	26.46	0.201	51	7	1	59	0	0	59
Administrative/Clerical	35	1.086	31	4	6	29	11	1	41
Production	16	-0.683	52	16	0	63	5	0	68
Protective Services	1	-1.034	6	1	0	3	4	0	7
Sales (FIRE)	4	0.318	0	1	6	1	2	4	7
Technicians	5	-0.031	1	7	11	1	15	3	19
Professionals	15	-0.066	2	2	63	2	7	58	67
Managers	5	0.334	0	2	19	2	5	14	21
Total	116.46	0.017	173	48	109	198	52	80	330

Notes: Following Autor and Dorn (2013), routine occupations are defined as the top third of occupations in terms of 1980 employment share, ranked according to an index of task content ($\log \text{routine} - \log \text{manual} - \log \text{abstract}$). Offshoring scores come from Firpo et al. (2011), and measure the extent to which occupations are susceptible to offshoring based on task content (face-to-face contact, on-site support). See Section 1.5.2 for a description of how required education levels by occupation are determined.

Source: Occupational Information Network (O*NET), 1990 Census.

Table A.5. Descriptive Statistics by Age Group, 1980-2007

	1980			1990			2000			2007		
	22-30	31-54	55+	22-30	31-54	55+	22-30	31-54	55+	22-30	31-54	55+
Panel A: Employment, unemployment and labor force participation (in population shares)												
Employment	0.73	0.74	0.32	0.77	0.80	0.29	0.75	0.77	0.31	0.76	0.79	0.36
Unemployment	0.06	0.04	0.01	0.06	0.04	0.01	0.05	0.03	0.01	0.06	0.04	0.01
Non-labor force participation	0.21	0.23	0.67	0.17	0.16	0.69	0.20	0.19	0.68	0.18	0.18	0.62
Panel B: Employment composition (in population shares)												
Part-time	0.11	0.11	0.08	0.13	0.11	0.08	0.14	0.11	0.09	0.14	0.10	0.09
Full-time	0.62	0.63	0.24	0.63	0.68	0.21	0.61	0.67	0.22	0.62	0.69	0.28
Low-skill occupations	0.12	0.11	0.08	0.15	0.12	0.07	0.16	0.12	0.07	0.18	0.13	0.07
Middle-skill occupations	0.38	0.36	0.14	0.36	0.35	0.12	0.33	0.33	0.12	0.31	0.31	0.13
High-skill occupations	0.23	0.27	0.10	0.26	0.33	0.11	0.27	0.32	0.12	0.26	0.34	0.16
Panel C: Overeducated employment (in employment shares of individuals with some education beyond high school)												
O*NET	0.40	0.28	0.29	0.40	0.31	0.30	0.40	0.32	0.32	0.42	0.31	0.32
Census: 1990 basis	0.51	0.38	0.38	0.52	0.42	0.41	0.50	0.43	0.42	0.54	0.43	0.43
Census: Yearly basis	0.65	0.56	0.57	0.52	0.42	0.41	0.40	0.33	0.33	0.43	0.33	0.33
Panel D: Educational attainment (in population shares)												
Attending school	0.13	0.04	0.01	0.17	0.07	0.02	0.18	0.05	0.01	0.20	0.05	0.01
High school or less	0.51	0.62	0.79	0.47	0.47	0.71	0.42	0.42	0.60	0.42	0.41	0.52
Some college	0.29	0.19	0.11	0.32	0.29	0.16	0.33	0.30	0.21	0.32	0.29	0.24
College grad or more	0.20	0.19	0.10	0.21	0.25	0.13	0.25	0.27	0.19	0.26	0.30	0.25
Panel E: Mean hourly wages (in 2014$)												
All	16.0	21.6	21.5	16.0	22.6	23.9	18.0	25.7	28.0	18.1	27.3	29.4
Low-skill occupations	11.7	13.4	13.4	11.6	14.0	14.9	13.5	17.0	17.7	13.0	16.7	17.2
Middle-skill occupations	15.9	19.7	20.1	15.0	19.6	20.7	16.3	21.2	22.8	16.2	21.5	22.8
High-skill occupations	18.2	27.3	29.3	19.5	28.6	32.2	22.5	33.6	38.3	23.3	36.5	39.9

Notes: Part-time employment is defined as working less than 35 hours a week. Low-skill occupations include food/maintenance, personal services, and sales (other). Middle-skill occupations include operators/laborers, administrative/clerical, production and protective services. High-skill occupations include sales (FIRE), technicians, professionals and managers. Overeducated workers are defined as either (1) having at least a 4-year college degree and being employed in an occupation that does not require one, or (2) having some education beyond high school (Associate's degree, post-secondary certificate, college dropout) and being employed in an occupation that only requires a high school degree or less. Hourly wages are conditional on full-time employment and exclude the self-employed.

Table A.6. Mean and Standard Deviation of CZ Characteristics, 1980-2007

	1980	1990	2000	2007
Manufacturing employment share	0.226	0.178	0.148	0.119
	(0.081)	(0.065)	(0.059)	(0.050)
Routine occupation employment share	0.332	0.324	0.316	0.299
	(0.036)	(0.026)	(0.021)	(0.020)
Mean occupation offshoring index	0.068	0.062	0.039	0.004
	(0.082)	(0.080)	(0.089)	(0.088)
Age 16-30 population share	0.357	0.305	0.267	0.266
	(0.033)	(0.030)	(0.033)	(0.029)
Age 55+ population share	0.275	0.270	0.270	0.297
	(0.043)	(0.045)	(0.044)	(0.039)
Female employment rate	0.471	0.540	0.548	0.556
	(0.053)	(0.055)	(0.051)	(0.044)
Immigrant population share	0.074	0.095	0.133	0.153
	(0.071)	(0.099)	(0.123)	(0.127)
Female population share	0.528	0.525	0.521	0.517
	(0.011)	(0.010)	(0.010)	(0.011)
Black population share	0.106	0.110	0.111	0.116
	(0.091)	(0.091)	(0.094)	(0.096)
Asian population share	0.013	0.026	0.037	0.045
	(0.018)	(0.031)	(0.041)	(0.046)
Other non-whites population share	0.009	0.041	0.077	0.078
	(0.019)	(0.052)	(0.070)	(0.067)
Middle-educated population share	0.200	0.258	0.279	0.278
	(0.043)	(0.043)	(0.036)	(0.035)
College-educated population share	0.138	0.181	0.217	0.245
	(0.038)	(0.053)	(0.063)	(0.068)
Number of commuting zones	722	722	722	722

Notes: All variables are based on the population aged 16 or older. Means and standard deviations (in parentheses) are weighted by CZ population.

Table A.7. 2SLS: First-Stage Regressions

	Dependent variable: Δ Emp/pop (55+)					
	80-90	90-00	00-07	80-07	80-07	80-07
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline IV						
Predicted retirement intensity	-1.215***	-1.834***	-3.171***	-1.442***		
	(0.313)	(0.223)	(0.501)	(0.178)		
Panel B: Project start-of-period age distribution forward						
Predicted retirement intensity					0.681***	
					(0.097)	
Panel C: 10-year retirement rates from Canada						
Predicted retirement intensity						-1.594***
						(0.230)
F-stat	15.06	67.69	40.08	65.82	49.69	48
Partial R^2	0.069	0.143	0.140	0.069	0.077	0.082
Period fixed effects				✓	✓	✓
Observations	722	722	722	2,166	2,166	2,166

Notes: All regressions include start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.8. The Effect of Retirement Trends on Employment, Unemployment and Labor Force Participation: Heterogeneity by Age, Gender and Education

	Dependent variable:				
	Δ Emp/pop				
	All	Part-time	Full-time	Δ Unemp/pop	Δ NLFP/pop
	(1)	(2)	(3)	(4)	(5)
Panel A: Young (22-30) $\times$ gender					
Male	0.014	0.463***	-0.449	0.324	-0.338
	(0.341)	(0.146)	(0.436)	(0.206)	(0.351)
Female	0.213	0.574***	-0.360	0.312**	-0.526
	(0.558)	(0.155)	(0.572)	(0.132)	(0.573)
Panel B: Young (22-30) $\times$ education groups					
$\leqslant$ High school grad	0.765	0.455***	0.310	0.306	-1.071*
	(0.577)	(0.152)	(0.631)	(0.227)	(0.601)
Some college	-0.038	0.511***	-0.549	0.326*	-0.288
	(0.266)	(0.179)	(0.341)	(0.187)	(0.324)
$\geqslant$ College grad	-0.491*	0.344	-0.835**	0.261**	0.230
	(0.286)	(0.228)	(0.394)	(0.107)	(0.251)
Panel C: Other age groups					
Teenagers (16-21)	-0.935***	0.094	-1.028***	0.548***	0.386
	(0.343)	(0.151)	(0.283)	(0.210)	(0.236)
Prime-aged (31-44)	0.120	0.049	0.071	0.151	-0.271
	(0.288)	(0.073)	(0.313)	(0.098)	(0.289)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). Each cell represents the coefficient corresponding to the change in the 55+ employment rate from a separate 2SLS regression, for different subgroups (rows) and outcomes (columns). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.9. The Effect of Retirement Trends on Occupational Composition: Heterogeneity by Age, Gender and Education

	Dependent variable: Δ Emp/pop		
	Low-skill occupations	Middle-skill occupations	High-skill occupations
	(1)	(2)	(3)
Panel A: Young (22-30) × gender			
Male	0.381*	0.450	-0.817***
	(0.211)	(0.310)	(0.277)
Female	0.664***	0.190	-0.640**
	(0.211)	(0.306)	(0.264)
Panel B: Young (22-30) × education groups			
⩽ High school grad	0.588***	0.184	-0.007
	(0.226)	(0.468)	(0.149)
Some college	0.504**	-0.054	-0.488**
	(0.208)	(0.326)	(0.194)
⩾ College grad	0.093	0.352	-0.937***
	(0.153)	(0.247)	(0.247)
Panel C: Other age groups			
Teenagers (16-21)	0.008	-0.782***	-0.161**
	(0.164)	(0.266)	(0.076)
Prime-aged (31-44)	0.093	0.343	-0.316**
	(0.087)	(0.237)	(0.143)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). Each cell represents the coefficient corresponding to the change in the 55+ employment rate from a separate 2SLS regression, for different subgroups (rows) and outcomes (columns). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.10. The Effect of Retirement Trends on Youth Occupational Composition: 1980 Skill Terciles

	Dependent variable: Δ Emp/pop (22-30)		
	Bottom tercile of 1980 skill distribution	Middle tercile of 1980 skill distribution	Top tercile of 1980 skill distribution
	(1)	(2)	(3)
Panel A: OLS estimates			
Δ Emp/pop (55+)	-0.059	0.332***	0.246***
	(0.047)	(0.046)	(0.071)
Panel B: 2SLS estimates			
Δ Emp/pop (55+)	0.751***	0.290	-0.928***
	(0.197)	(0.199)	(0.293)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.11. The Effect of Retirement Trends on Wages: Heterogeneity by Age, Gender and Education

	Dependent variable: Δ log hourly wage (22-30)			
	All occupations	Low-skill occupations	Middle-skill occupations	High-skill occupations
	(1)	(2)	(3)	(4)
Panel A: Young (22-30) × gender				
Male	-2.867***	-2.609**	-2.133**	-3.340***
	(1.027)	(1.051)	(1.083)	(0.847)
Observations	2,166	2,164	2,166	2,166
Female	-3.129***	-3.129***	-2.231***	-3.697***
	(0.695)	(0.741)	(0.824)	(0.686)
Observations	2,166	2,166	2,166	2,166
Panel B: Young (22-30) × education groups				
⩽ High school grad	-1.874**	-2.591***	-1.623	-1.750**
	(0.923)	(0.794)	(1.032)	(0.842)
Observations	2,166	2,166	2,166	2,144
Some college	-3.501***	-3.267***	-3.191***	-4.032***
	(0.845)	(0.813)	(0.865)	(0.964)
Observations	2,166	2,166	2,166	2,166
⩾ College grad	-2.786***	-2.264*	-3.015***	-2.596***
	(0.571)	(1.289)	(0.942)	(0.584)
Observations	2,166	2,044	2,149	2,166
Panel C: Other age groups				
Teenagers (16-21)	-2.714***	-2.345**	-2.931***	-2.532**
	(0.862)	(0.942)	(0.885)	(1.050)
Observations	2,166	2,166	2,166	2,129
Prime-aged (31-44)	-1.088***	-2.380***	-1.417***	-0.717**
	(0.404)	(0.711)	(0.464)	(0.289)
Observations	2,166	2,166	2,166	2,166

Notes: Each cell represents the coefficient corresponding to the change in the 55+ employment rate from a separate 2SLS regression, for different subgroups (rows) and outcomes (columns). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.12. The Effect of Retirement Trends on Youth Occupational Composition: Composition-Adjusted

	Dependent variable: Δ Emp/pop (22-30) — composition-adjusted		
	Low-skill occupations	Middle-skill occupations	High-skill occupations
	(1)	(2)	(3)
Δ Emp/pop (55+)	0.319**	-0.009	-0.414***
	(0.155)	(0.269)	(0.130)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). See Section 1.5.4 for a description of the composition adjustment. All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.13. Robustness: Alternative Controls

	Dependent variable: Youth outcome (22-30)									
	Δ Emp/pop									
	All	Part-time	Full-time	Low-skill occupations	Middle-skill occupations	High-skill occupations	Δ Unemp/ pop	Δ NLFP/ pop	Δ Overeduc/ emp (O*NET)	Δ log hourly wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: State fixed effects (First-stage F-stat = 66.03)										
Δ Emp/pop (55+)	0.117	0.523***	-0.406	0.417**	0.231	-0.531**	0.289*	-0.406	1.259***	-2.461***
	(0.461)	(0.126)	(0.519)	(0.162)	(0.296)	(0.220)	(0.168)	(0.513)	(0.237)	(0.768)
Panel B: Full industry & occupation group shares (First-stage F-stat = 61.34)										
Δ Emp/pop (55+)	0.150	0.516***	-0.366	0.431***	0.272	-0.552**	0.258	-0.408	1.273***	-2.561***
	(0.491)	(0.158)	(0.544)	(0.164)	(0.299)	(0.254)	(0.166)	(0.506)	(0.216)	(0.793)
Panel C: Finer age group shares (First-stage F-stat = 43.94)										
Δ Emp/pop (55+)	-0.227	0.734***	-0.962**	0.624***	0.067	-0.918***	0.383*	-0.156	1.441***	-3.848***
	(0.404)	(0.170)	(0.444)	(0.207)	(0.283)	(0.259)	(0.215)	(0.448)	(0.318)	(0.799)
Panel D: Age group × education group shares (First-stage F-stat = 90.91)										
Δ Emp/pop (55+)	0.169	0.370***	-0.202	0.431***	0.350	-0.612**	0.323**	-0.492	1.346***	-1.990***
	(0.503)	(0.116)	(0.553)	(0.147)	(0.319)	(0.251)	(0.160)	(0.494)	(0.249)	(0.708)

Notes: All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Panel A adds state fixed effects. Panel B replaces manufacturing employment shares with detailed industry and occupation shares. Panel C replaces population shares of young and old with detailed age shares (16-21, 22-30, 31-44, 45-54, 55+). Panel D replaces age group shares with age-education group shares. Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.14. Robustness: Alternative Samples

	Dependent variable: Youth outcome (22-30)									
	Δ Emp/pop									
	All	Part-time	Full-time	Low-skill occupations	Middle-skill occupations	High-skill occupations	Δ Unemp/ pop	Δ NLFP/ pop	Δ Overeduc/ emp (O*NET)	Δ log hourly wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Composition-adjusted outcomes										
Δ Emp/pop (55+)	-0.104	0.470***	-0.574*	0.319**	-0.009	-0.414***	0.286*	-0.182	0.844***	-2.510***
	(0.251)	(0.140)	(0.324)	(0.155)	(0.269)	(0.130)	(0.161)	(0.261)	(0.216)	(0.749)
Panel B: Excl. students										
Δ Emp/pop (55+)	0.210	0.331**	-0.121	0.535***	0.435	-0.759***	0.339*	-0.549	1.278***	-2.918***
	(0.479)	(0.129)	(0.539)	(0.184)	(0.314)	(0.264)	(0.175)	(0.499)	(0.267)	(0.847)
Panel C: Excl. those born out-of-state										
Δ Emp/pop (55+)	0.004	0.298**	-0.294	0.349*	0.325	-0.670***	0.271	-0.275	1.167***	-3.006***
	(0.259)	(0.138)	(0.325)	(0.202)	(0.219)	(0.214)	(0.174)	(0.306)	(0.266)	(0.785)
Panel D: Excl. recent out-of-state in-migrants										
Δ Emp/pop (55+)	0.154	0.459***	-0.305	0.435**	0.318	-0.599***	0.270*	-0.424	1.173***	-2.916***
	(0.427)	(0.134)	(0.475)	(0.181)	(0.326)	(0.207)	(0.156)	(0.445)	(0.269)	(0.821)

Notes: All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.15. Robustness: Excluding Census Divisions One-by-One

	Excluded Census division								
	New England	Middle Atlantic	East North Central	West North Central	South Atlantic	East South Central	West South Central	Mountain	Pacific
Youth outcome (22-30)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ Part-time/pop	0.461***	0.519***	0.466***	0.520***	0.560***	0.556***	0.563***	0.512***	0.503***
	(0.128)	(0.158)	(0.132)	(0.141)	(0.166)	(0.167)	(0.145)	(0.134)	(0.158)
Δ Unemp/pop	0.368**	0.291*	0.233*	0.342*	0.336*	0.397*	0.245*	0.286*	0.306*
	(0.163)	(0.167)	(0.133)	(0.182)	(0.191)	(0.213)	(0.143)	(0.159)	(0.171)
Δ Low-skill occupations/pop	0.474***	0.548***	0.477***	0.437**	0.585***	0.646***	0.467***	0.525***	0.512***
	(0.172)	(0.189)	(0.179)	(0.179)	(0.210)	(0.230)	(0.170)	(0.174)	(0.187)
Δ High-skill occupations/pop	-0.740***	-0.705**	-0.699***	-0.715***	-0.427*	-0.892***	-0.717***	-0.721***	-0.969***
	(0.256)	(0.275)	(0.245)	(0.253)	(0.248)	(0.302)	(0.222)	(0.261)	(0.263)
Δ Overeducated/emp (O*NET)	1.395***	1.306***	1.363***	1.380***	1.113***	1.681***	1.308***	1.472***	1.363***
	(0.273)	(0.295)	(0.274)	(0.263)	(0.298)	(0.295)	(0.256)	(0.271)	(0.275)
Δ Overeducated/emp (Census: 1990 basis)	1.350***	1.254***	1.259***	1.193***	1.257***	1.552***	1.234***	1.406***	1.332***
	(0.286)	(0.276)	(0.319)	(0.270)	(0.308)	(0.368)	(0.303)	(0.287)	(0.303)
Δ Overeducated/emp (Census: yearly basis)	1.163***	1.168***	0.998***	1.025***	1.179***	1.305***	1.087***	1.278***	1.179***
	(0.261)	(0.251)	(0.282)	(0.254)	(0.298)	(0.351)	(0.284)	(0.261)	(0.274)
Δ log hourly wage	-2.568***	-2.925***	-2.862***	-2.869***	-2.527***	-3.350***	-2.770***	-2.995***	-3.617***
	(0.797)	(0.959)	(0.841)	(0.899)	(0.867)	(0.992)	(0.876)	(0.915)	(0.942)
First-stage *F*-stat	62.52	45.43	61.63	58.05	33.89	54.57	90.63	60.45	52.88
Observations	2,118	2,085	1,911	1,665	1,848	1,947	1,836	1,884	2,034

Notes: All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table A.16. Robustness: Alternative Measures of Labor Supply Shocks

	Dependent variable: Youth outcome (22-30)									
	Δ Emp/pop									
	All	Part-time	Full-time	Low-skill occupations	Middle-skill occupations	High-skill occupations	Δ Unemp/ pop	Δ NLFP/ pop	Δ Overeduc/ emp (O*NET)	Δ log hourly wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Log changes (First-stage F-stat = 33.21)										
% Δ Emp (55+)	0.031	0.142***	-0.111	0.142***	0.092	-0.203***	0.087*	-0.118	0.381***	-0.812***
	(0.120)	(0.036)	(0.135)	(0.046)	(0.086)	(0.078)	(0.047)	(0.128)	(0.078)	(0.232)
Panel B: Change in 16+ population shares (First-stage F-stat = 43.15)										
Δ (Emp^{55+}/pop^{16+})	0.338	1.552***	-1.214	1.556***	1.006	-2.224***	0.955*	-1.293	4.169***	-8.896***
	(1.342)	(0.369)	(1.388)	(0.539)	(0.981)	(0.690)	(0.526)	(1.502)	(0.898)	(2.074)
Panel C: Divide by lagged 55+ population (First-stage F-stat = 20.16)										
(Δ Emp^{55+})/pop$^{55+}_{-1}$	0.074	0.338***	-0.264	0.339***	0.219	-0.484**	0.208*	-0.281	0.907***	-1.936***
	(0.285)	(0.098)	(0.328)	(0.116)	(0.208)	(0.205)	(0.121)	(0.307)	(0.226)	(0.583)
Panel D: Divide by lagged 16+ population (First-stage F-stat = 18.79)										
(Δ Emp^{55+})/pop$^{16+}_{-1}$	0.392	1.798***	-1.406	1.804***	1.166	-2.577**	1.107*	-1.499	4.831***	-10.308***
	(1.523)	(0.511)	(1.734)	(0.659)	(1.084)	(1.048)	(0.650)	(1.649)	(1.201)	(3.216)

Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

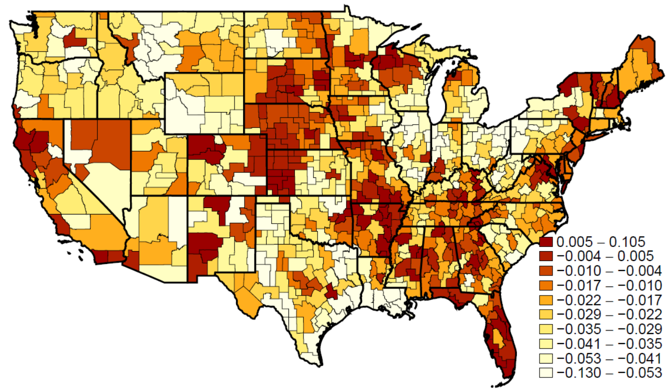
Table A.17. Robustness: Alternative Definitions of Instrument

	Dependent variable: Youth outcome (22-30)									
	Δ Emp/pop									
	All	Part-time	Full-time	Low-skill occupations	Middle-skill occupations	High-skill occupations	Δ Unemp/ pop	Δ NLFP/ pop	Δ Overeduc/ emp (O*NET)	Δ log hourly wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Project start-of-period age distribution forward (First-stage F-stat = 49.69)										
Δ Emp/pop (55+)	-0.291	0.542***	-0.833**	0.315*	-0.018	-0.587***	0.190	0.101	0.693**	-2.158***
	(0.271)	(0.158)	(0.350)	(0.164)	(0.233)	(0.201)	(0.147)	(0.213)	(0.279)	(0.757)
Panel B: 10-year retirement rates from Canada (First-stage F-stat = 48)										
Δ Emp/pop (55+)	0.160	0.359**	-0.199	0.445***	0.305	-0.591**	0.276**	-0.435	1.074***	-2.276***
	(0.370)	(0.141)	(0.415)	(0.166)	(0.202)	(0.280)	(0.126)	(0.368)	(0.215)	(0.835)

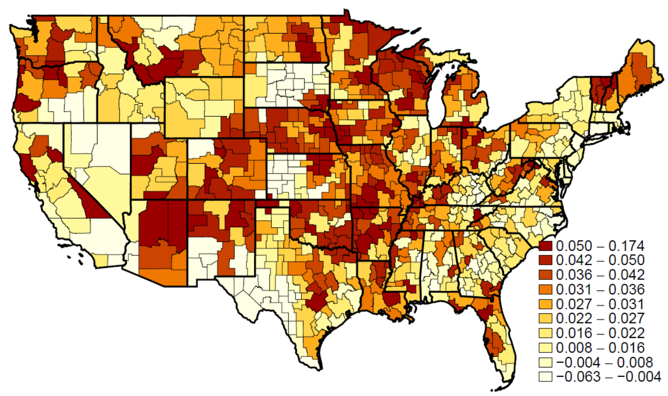
Notes: $N = 2,166$ (722 CZs $\times$ 3 time periods). All regressions include period fixed effects and start-of-period CZ controls (manufacturing employment share, employment share of routine occupations, mean occupation offshoring index, female employment rate, population share of immigrants/young (16-30)/old (55+)/females/Blacks/Asians/other non-whites/some college-educated/college-educated). Observations are weighted by start-of-period CZ share of national population. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Figure A.1. Change in 55+ Employment Rate by CZ, 1980-2007

Panel A: 1980-1990



Panel B: 1990-2000



Panel C: 2000-2007

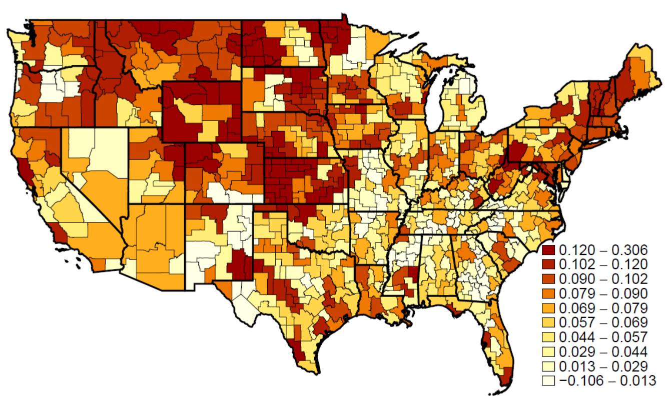
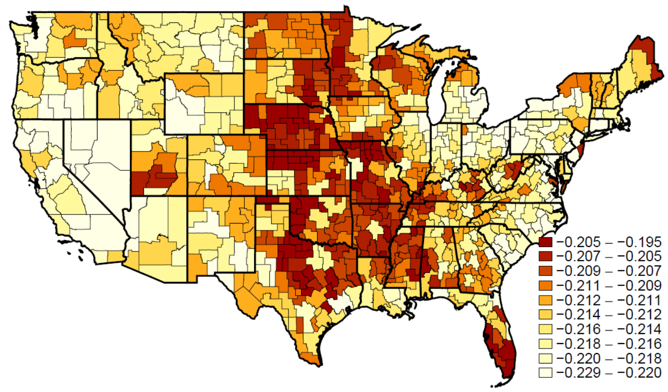
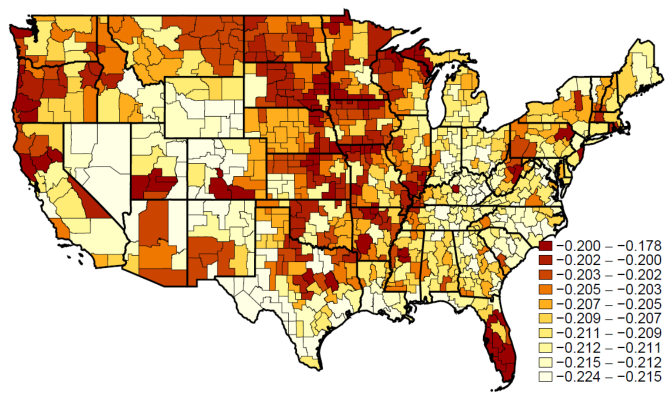


Figure A.2. Predicted Retirement Intensity by CZ, 1980-2007

Panel A: 1980-1990



Panel B: 1990-2000



Panel C: 2000-2007

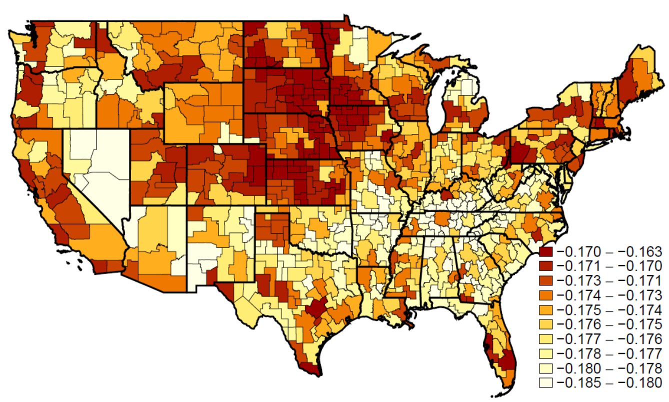
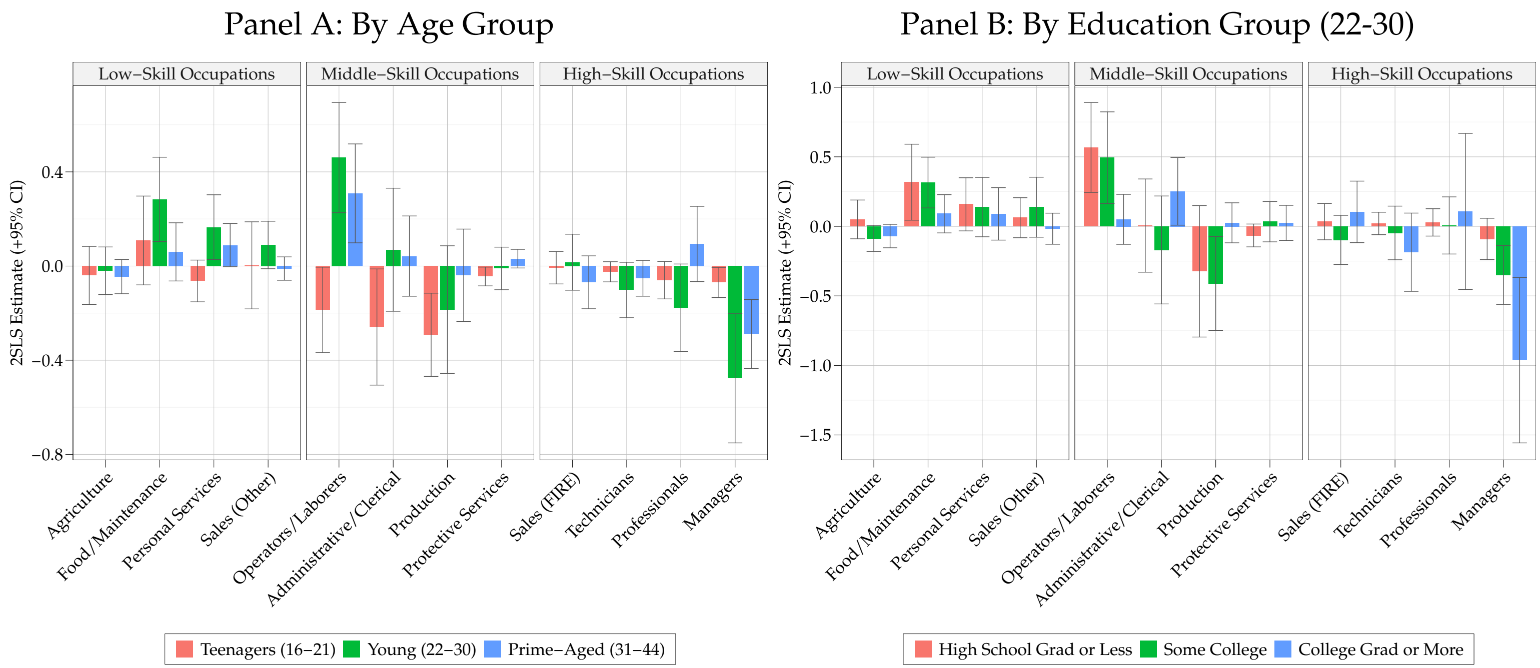


Figure A.3. The Effect of Retirement Trends on Occupational Composition: Occupation Group-Specific Effects



Notes: Each bar represents the coefficient corresponding to the change in the 55+ employment rate from a separate 2SLS regression, where the dependent variable is the change in occupation group-specific employment as a share of population (separately by subgroup). The error bars represent the corresponding 95% confidence intervals, based on robust standard errors clustered at the state level.

APPENDIX B

Appendix to Chapter 2

B.1. Constructing the Simulated Instrument

This appendix describes the construction of the simulated instrument, which follows the approach of Fetter and Lockwood (forthcoming). Our procedure constructs an instrument for actual OAA payments per person 65 and older in each state by simulating OAA payments among a national sample of men aged 60-64. As noted in Section 2.4, we set the simulated payment to zero for all states in 1930. For 1939, our procedure uses a measure of each state's maximum payment and any income disregards (which existed in five of the 45 states with an eligibility age of 65) to construct an income floor, and applies this floor to a national population of men to calculate a predicted payment per person in the sample. The national population used for each state omits the state itself, although in practice this has almost very little impact on the estimates. We use statutory maximum payments in 1939 as a measure of maximum payments for all states that had statutory maxima, and the 99th percentile payment to new recipients in fiscal year 1938-39 for the eight states that had no statutory maximum in 1939.

Given a measure of an individual i's earnings in 1939, for each state s we calculate a predicted OAA payment per person under that state's law as the mean over all individuals of

$$\text{(B.1)} \qquad \text{payment}_{is} = \max\{0, \min\{(\text{max payment})_s, \\ (\text{max payment})_s + (\text{income disregard})_s - (\text{income})_i\}$$

To minimize the possibility of endogenous earnings responses, we use the population of men 60-64 (just under the eligibility for OAA) in the 45 states that had an eligibility age of 65 to construct the instrument. Self-employment earnings are not reported in the 1940 Census, so for any person who reported being self-employed at the time of the Census and who worked a positive number of weeks in 1939, we impute earnings by randomly drawing 1939 earnings amounts from the population of non-self-employed men with the same number of years of education and the same number of weeks worked in 1939.

B.2. Appendix Tables and Figures

Table B.1. Characteristics of State OAA Programs, 1930-1939

State	First year of OAA payments	Characteristics of OAA programs in 1930: Recipiency rate	Payments per recipients	Per-65+ payments	Characteristics of OAA programs in 1939: Eligibility age	Relative respons. laws	Recipiency rate	Payments per recipients	Maximum payment	99th percentile payment	Per-65+ payments	Simulated IV
Alabama	1936	—	—	—	65	1	0.13	9.42	30	30	1.27	8.06
Arizona	1933	—	—	—	65	0	0.33	26.58	30	30	8.64	8.04
Arkansas	1936	—	—	—	65	0	0.17	6.01	—	12	1.03	2.73
California	1930	0.020	22.56	0.372	65	0	0.24	32.97	35	35	7.95	12.34
Colorado	1928	—	—	—	60	1	0.46	28.44	45	45	13.17	13.78
Connecticut	1936	—	—	—	65	1	0.13	27.04	39	30	3.55	11.38
Delaware	1931	—	—	—	65	1	0.12	10.98	25	25	1.37	6.40
D.C.	1936	—	—	—	65	1	0.08	25.08	30	30	2.02	8.05
Florida	1936	—	—	—	65	1	0.28	11.70	30	30	3.23	8.02
Georgia	1937	—	—	—	65	0	0.14	8.07	30	25	1.16	8.06
Idaho	1931	—	—	—	65	0	0.27	21.47	30	30	5.84	10.30
Illinois	1936	—	—	—	65	1	0.24	20.03	30	30	4.89	8.05
Indiana	1934	—	—	—	65	0	0.23	17.55	30	30	4.02	8.09
Iowa	1934	—	—	—	65	1	0.24	20.13	25	25	4.75	7.62
Kansas	1937	—	—	—	65	0	0.17	19.07	—	40	3.16	11.75
Kentucky	1928	0.0001	5.39	0.0007	65	0	0.24	8.66	15	15	2.07	3.50
Louisiana	1936	—	—	—	65	0	0.26	14.10	—	30	3.66	8.04
Maine	1936	—	—	—	65	1	0.17	20.64	30	30	3.59	8.06
Maryland	1930	0.0001	12	0.002	65	1	0.15	17.31	30	30	2.52	8.06
Massachusetts	1931	—	—	—	65	1	0.22	28.91	—	45	6.46	13.86
Michigan	1934	—	—	—	65	1	0.23	16.47	30	30	3.86	8.08
Minnesota	1931	—	—	—	65	0	0.31	20.64	30	30	6.42	9.28
Mississippi	1935	—	—	—	65	1	0.17	7.51	15	15	1.29	3.51
Missouri	1935	—	—	—	70	0	0.24	18.90	30	30	4.57	8.07
Montana	1923	0.033	14.09	0.465	65	0	0.34	17.99	—	30	6.05	9.52

Notes: Monthly OAA payments are in 1939 dollars. See Section 2.4.2 for a description of the simulated IV.

Source: Parker (1936), U.S. Social Security Board (1939a), U.S. Social Security Board (1939b), U.S. Social Security Board (1940b), U.S. Social Security Board (1941).

Table B.1. (cont.) Characteristics of State OAA Programs, 1930-1939

		Characteristics of OAA programs in 1930			Characteristics of OAA programs in 1939							
State	First year of OAA payments	Recipiency rate	Payments per recipients	Per-65+ payments	Eligibility age	Relative respons. laws	Recipiency rate	Payments per recipients	Maximum payment	99th percentile payment	Per-65+ payments	Simulated IV
Nebraska	1934	—	—	—	65	1	0.26	15.61	30	30	4.05	8.06
Nevada	1928	0.001	25	0.016	65	0	0.33	26.64	—	30	8.84	8.05
New Hampshire	1931	—	—	—	70	1	0.09	20.95	30	30	1.98	8.06
New Jersey	1932	—	—	—	65	1	0.11	20.22	40	30	2.22	11.77
New Mexico	1936	—	—	—	65	0	0.17	13.43	—	30	2.33	8.05
New York	1931	—	—	—	65	1	0.12	25.20	—	45	3.13	13.67
North Carolina	1937	—	—	—	65	0	0.22	9.99	30	30	2.23	8.07
North Dakota	1934	—	—	—	65	1	0.23	17.78	30	30	4.00	8.06
Ohio	1934	—	—	—	65	1	0.23	22.82	30	30	5.31	8.06
Oklahoma	1936	—	—	—	65	0	0.49	17.59	30	30	8.54	8.04
Oregon	1934	—	—	—	65	1	0.22	21.33	30	30	4.78	8.06
Pennsylvania	1934	—	—	—	70	1	0.12	21.77	30	30	2.52	8.00
Rhode Island	1935	—	—	—	65	1	0.12	19.20	30	30	2.40	8.05
South Carolina	1937	—	—	—	65	0	0.26	7.98	20	20	2.07	4.89
South Dakota	1936	—	—	—	65	1	0.32	17.67	30	30	5.65	8.06
Tennessee	1937	—	—	—	65	0	0.24	10.06	25	25	2.39	6.40
Texas	1936	—	—	—	65	0	0.35	8.75	30	30	3.04	8.05
Utah	1930	0.049	7.37	0.352	65	0	0.46	21.06	30	30	9.67	8.05
Vermont	1935	—	—	—	65	1	0.16	15.60	30	30	2.53	8.06
Virginia	1938	—	—	—	65	0	0.10	9.65	20	20	1.01	4.90
Washington	1934	—	—	—	65	1	0.27	22.04	30	30	5.97	8.05
West Virginia	1936	—	—	—	65	1	0.17	12.34	30	30	2.12	8.04
Wisconsin	1925	0.005	13.19	0.068	65	1	0.21	21.65	30	30	4.44	8.07
Wyoming	1930	0.009	13.21	0.121	65	1	0.26	23.29	30	30	6.15	8.06

Notes: Monthly OAA payments are in 1939 dollars. See Section 2.4.2 for a description of the simulated IV.

Source: U.S. Social Security Board (1939a), U.S. Social Security Board (1939b), U.S. Social Security Board (1940b), U.S. Social Security Board (1941).

Table B.2. Variation in OAA Per-65+ Payments: Unrestricted vs. Restricted Comparisons

Dependent variable	Share 65 and above	Share foreign born	Share nonwhite	Median years of schooling	Log median earnings
Panel A: Observed payments variable, no state border group fixed effects					
Annual OAA payments	0.024**	0.086**	-0.348***	2.573***	0.728**
per person 65+	(0.009)	(0.025)	(0.081)	(0.477)	(0.225)
Observations	2,606	2,606	2,606	2,587	2,587
Panel B: Observed payments variable, state border group fixed effects					
Annual OAA payments	0.007	0.006	0.029	-0.303	0.072
per person 65+	(0.005)	(0.007)	(0.018)	(0.346)	(0.127)
Observations	2,606	2,606	2,606	2,587	2,587
Panel C: Simulated payments variable, no state border group fixed effects					
Simulated annual OAA	0.019**	0.089***	-0.248*	1.582***	0.692***
payments per person 65+	(0.006)	(0.016)	(0.096)	(0.297)	(0.091)
Observations	2,606	2,606	2,606	2,587	2,587
Panel D: Simulated payments variable, state border group fixed effects					
Simulated annual OAA	0.001	0.003	-0.048	-0.390	0.099
payments per person 65+	(0.003)	(0.003)	(0.026)	(0.325)	(0.066)
Observations	2,606	2,606	2,606	2,587	2,587

Notes: Annual OAA payments per person 65+ are annualized December 1939 payments, in hundreds of 1939 dollars. Simulated payments are based on earnings in 1939 and measured in hundreds of 1939 dollars. Unit of observation is a county in 1940. All outcomes are measured in the 1940 Census.

Table B.3. "First-Stage" Regressions

Panel A: Men

	Age 55-59	Age 60-64	Age 65-69	Age 70-74	Age 75-79	Age 80-84
	(1)	(2)	(3)	(4)	(5)	(6)
Simulated IV × (55-59)	0.318***	−0.001	−0.001	−0.001	−0.0005	−0.0002
	(0.079)	(0.001)	(0.001)	(0.001)	(0.0003)	(0.0002)
Simulated IV × (60-64)	−0.002	0.317***	−0.001	−0.001	−0.0004	−0.0002
	(0.001)	(0.079)	(0.001)	(0.001)	(0.0003)	(0.0002)
Simulated IV × (65-69)	−0.002	−0.001	0.317***	−0.001	−0.0004	−0.0002
	(0.002)	(0.001)	(0.079)	(0.001)	(0.0004)	(0.0002)
Simulated IV × (70-74)	−0.002	−0.001	−0.001	0.318***	−0.0004	−0.0002
	(0.001)	(0.001)	(0.001)	(0.083)	(0.0002)	(0.0001)
Simulated IV × (75-79)	−0.0004	−0.0004	−0.0003	−0.0001	0.310***	0.00000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.087)	(0.0002)
Simulated IV × (80-84)	0.002	0.001	0.001	0.001	0.001	0.298***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.086)
Observations	12,273,735	12,273,735	12,273,735	12,273,735	12,273,735	12,273,735
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
Sample	Men	Men	Men	Men	Men	Men

Panel B: Women

	Age 55-59	Age 60-64	Age 65-69	Age 70-74	Age 75-79	Age 80-84
	(1)	(2)	(3)	(4)	(5)	(6)
Simulated IV × (55-59)	0.325***	0.001	0.0005	0.0002	0.0002	0.0001
	(0.080)	(0.002)	(0.001)	(0.001)	(0.0005)	(0.0003)
Simulated IV × (60-64)	0.0002	0.328***	0.0001	0.00002	0.00004	0.00000
	(0.002)	(0.080)	(0.001)	(0.001)	(0.0004)	(0.0002)
Simulated IV × (65-69)	0.0001	0.0001	0.328***	0.00001	0.00002	−0.00000
	(0.002)	(0.001)	(0.080)	(0.001)	(0.0004)	(0.0002)
Simulated IV × (70-74)	−0.001	−0.0005	−0.0003	0.331***	−0.0001	−0.0001
	(0.001)	(0.001)	(0.001)	(0.082)	(0.0003)	(0.0002)
Simulated IV × (75-79)	0.0003	0.0003	0.0002	0.0002	0.327***	0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.084)	(0.0002)
Simulated IV × (80-84)	0.002	0.002	0.001	0.001	0.001	0.317***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)	(0.084)
Observations	11,806,357	11,806,357	11,806,357	11,806,357	11,806,357	11,806,357
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
Sample	Women	Women	Women	Women	Women	Women

Notes: The dependent variable in each column is the interaction of OAA per-65+ payments with the corresponding age group dummy. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table B.4. Co-residence Rates with Different Family Members, 1930-1940

	Men					Women				
	Co-residence with:					Co-residence with:				
Age group	All relatives	Child(ren)	Son(s)	Daughter(s)	Other(s)	All relatives	Child(ren)	Son(s)	Daughter(s)	Other(s)
	1930					1930				
55-59	0.54	0.45	0.31	0.26	0.10	0.61	0.50	0.33	0.29	0.12
60-64	0.52	0.43	0.29	0.25	0.09	0.60	0.48	0.29	0.28	0.12
65-69	0.50	0.42	0.26	0.24	0.08	0.59	0.47	0.26	0.28	0.12
70-74	0.50	0.41	0.24	0.24	0.08	0.61	0.49	0.24	0.29	0.12
75-79	0.52	0.44	0.23	0.25	0.08	0.66	0.54	0.25	0.32	0.12
80-84	0.57	0.48	0.24	0.28	0.09	0.71	0.58	0.25	0.35	0.13
	1940					1940				
55-59	0.53	0.44	0.30	0.25	0.10	0.59	0.47	0.32	0.27	0.12
60-64	0.51	0.42	0.28	0.24	0.09	0.56	0.44	0.27	0.25	0.12
65-69	0.48	0.39	0.25	0.22	0.09	0.55	0.42	0.23	0.24	0.12
70-74	0.46	0.37	0.22	0.21	0.09	0.56	0.43	0.21	0.26	0.13
75-79	0.48	0.39	0.21	0.23	0.09	0.61	0.47	0.22	0.29	0.13
80-84	0.53	0.43	0.22	0.25	0.09	0.66	0.52	0.22	0.33	0.14

Notes: Children, sons, and daughters refer to own (biological) children. Other relatives include children-in-law, parents, siblings, grandchildren, and other relatives (e.g. aunts/uncles, nephews/nieces, cousins, etc..). Co-residence with children means that at least one child is present in the household. Co-residence with other relatives means that at least one other family member is present in the household, but no children. Co-residence with sons and co-residence daughters are defined analogously, but are not mutually exclusive.

Table B.5. Co-residence Rates by Labor Force Status, 1930-1940

	Men						Women					
	In labor force			Out of labor force			In labor force			Out of labor force		
	Co-residence as:			Co-residence as:			Co-residence as:			Co-residence as:		
Age group	Any	HHH	Dep.	Any	HHH	Dep.	Any	HHH	Dep.	Any	HHH	Dep.
	1930						1930					
55-59	0.55	0.50	0.05	0.51	0.35	0.16	0.50	0.33	0.17	0.64	0.52	0.12
60-64	0.52	0.47	0.05	0.53	0.35	0.18	0.46	0.30	0.16	0.62	0.44	0.18
65-69	0.50	0.43	0.07	0.53	0.33	0.21	0.44	0.27	0.16	0.61	0.35	0.26
70-74	0.47	0.39	0.08	0.54	0.29	0.26	0.42	0.26	0.16	0.63	0.28	0.35
75-79	0.46	0.37	0.10	0.57	0.25	0.32	0.44	0.27	0.17	0.68	0.24	0.44
80-84	0.48	0.36	0.11	0.61	0.23	0.38	0.48	0.28	0.20	0.72	0.20	0.52
	1940						1940					
55-59	0.54	0.49	0.05	0.50	0.37	0.13	0.48	0.32	0.16	0.61	0.51	0.10
60-64	0.51	0.46	0.05	0.49	0.36	0.13	0.44	0.28	0.16	0.58	0.43	0.16
65-69	0.49	0.43	0.05	0.47	0.33	0.14	0.43	0.26	0.17	0.56	0.34	0.22
70-74	0.46	0.40	0.06	0.46	0.30	0.17	0.44	0.25	0.19	0.57	0.29	0.28
75-79	0.46	0.38	0.08	0.49	0.27	0.21	0.50	0.24	0.26	0.61	0.25	0.36
80-84	0.47	0.36	0.11	0.54	0.25	0.29	0.58	0.22	0.36	0.67	0.21	0.45

Table B.6. Co-residence Rates by Marital Status, 1930-1940

	Men						Women					
	Married			Separated, divorced or widowed			Married			Separated, divorced or widowed		
	Co-residence as:			Co-residence as:			Co-residence as:			Co-residence as:		
Age group	Any	HHH	Dep.	Any	HHH	Dep.	Any	HHH	Dep.	Any	HHH	Dep.
	1930						1930					
55-59	0.57	0.55	0.02	0.49	0.32	0.17	0.58	0.55	0.03	0.71	0.42	0.28
60-64	0.55	0.52	0.02	0.53	0.31	0.22	0.53	0.48	0.04	0.71	0.37	0.35
65-69	0.51	0.47	0.04	0.57	0.29	0.28	0.48	0.41	0.07	0.71	0.31	0.41
70-74	0.47	0.41	0.06	0.61	0.26	0.35	0.45	0.35	0.10	0.73	0.26	0.47
75-79	0.46	0.37	0.09	0.66	0.23	0.44	0.46	0.31	0.15	0.74	0.23	0.52
80-84	0.46	0.34	0.13	0.71	0.22	0.49	0.49	0.29	0.21	0.77	0.20	0.57
	1940						1940					
55-59	0.56	0.54	0.02	0.50	0.33	0.16	0.56	0.53	0.03	0.67	0.43	0.24
60-64	0.52	0.50	0.03	0.52	0.32	0.19	0.50	0.46	0.04	0.67	0.37	0.29
65-69	0.48	0.45	0.04	0.53	0.29	0.23	0.45	0.39	0.06	0.65	0.31	0.34
70-74	0.44	0.39	0.05	0.56	0.27	0.29	0.41	0.34	0.08	0.66	0.27	0.39
75-79	0.43	0.36	0.07	0.61	0.25	0.35	0.42	0.31	0.11	0.68	0.24	0.44
80-84	0.43	0.33	0.10	0.66	0.23	0.43	0.45	0.28	0.16	0.72	0.21	0.51

Notes: Sample excludes single and never married individuals.

Table B.7. The Effect of OAA on Co-residence: OLS Estimates

	Co-residence with relatives		As household head		As dependent	
	(1)	(2)	(3)	(4)	(5)	(6)
OAA per-65+ × (55-59)	0.007	0.001	0.010	0.007	-0.002	-0.007
	(0.008)	(0.009)	(0.010)	(0.016)	(0.007)	(0.016)
OAA per-65+ × (60-64)	-0.002	-0.005	0.005	0.001	-0.007	-0.006
	(0.006)	(0.006)	(0.010)	(0.015)	(0.005)	(0.012)
OAA per-65+ × (65-69)	-0.023***	-0.029***	-0.003	-0.007	-0.020***	-0.022**
	(0.006)	(0.007)	(0.009)	(0.009)	(0.005)	(0.009)
OAA per-65+ × (70-74)	-0.037***	-0.046***	-0.002	-0.010	-0.035***	-0.036***
	(0.007)	(0.009)	(0.009)	(0.007)	(0.007)	(0.005)
OAA per-65+ × (75-79)	-0.063***	-0.043***	-0.001	0.002	-0.062***	-0.044***
	(0.008)	(0.012)	(0.007)	(0.009)	(0.008)	(0.011)
OAA per-65+ × (80-84)	-0.074***	-0.045***	-0.008	-0.001	-0.066***	-0.044**
	(0.010)	(0.015)	(0.008)	(0.011)	(0.009)	(0.018)
Observations	12,273,735	11,806,357	12,273,735	11,806,357	12,273,735	11,806,357
Mean of dep. var.	0.511	0.592	0.418	0.377	0.093	0.215
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
Sample	Men	Women	Men	Women	Men	Women

Notes: Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table B.8. The Effect of OAA on Co-residence: Heterogeneity by Marital Status and Type of Co-residence

Panel A: As household head

	Co-residence with relatives (as HHH)			
	(1)	(2)	(3)	(4)
OAA per-65+ × (55-59)	−0.025	−0.013	−0.021	−0.055
	(0.024)	(0.027)	(0.021)	(0.038)
OAA per-65+ × (60-64)	−0.029*	0.019	−0.036	−0.117**
	(0.017)	(0.023)	(0.027)	(0.047)
OAA per-65+ × (65-69)	−0.064**	−0.046	−0.053*	−0.055*
	(0.027)	(0.031)	(0.027)	(0.029)
OAA per-65+ × (70-74)	−0.054*	−0.051**	−0.060*	−0.051*
	(0.029)	(0.023)	(0.035)	(0.028)
OAA per-65+ × (75-79)	−0.070*	0.001	−0.040	−0.015
	(0.038)	(0.028)	(0.027)	(0.028)
OAA per-65+ × (80-84)	−0.064**	−0.005	−0.007	0.026
	(0.032)	(0.032)	(0.044)	(0.027)
Observations	9,032,441	2,095,142	6,041,997	4,787,247
Mean of dep. var.	0.486	0.281	0.463	0.315
State border × age group × year FEs	✓	✓	✓	✓
County FEs	✓	✓	✓	✓
Sample	Men (married)	Men (non-married)	Women (married)	Women (non-married)

Panel B: As dependent

	Co-residence with relatives (as dep.)			
	(1)	(2)	(3)	(4)
OAA per-65+ × (55-59)	−0.002	−0.031	0.002	0.041
	(0.006)	(0.020)	(0.007)	(0.038)
OAA per-65+ × (60-64)	0.002	−0.041	0.005	0.052
	(0.005)	(0.027)	(0.005)	(0.033)
OAA per-65+ × (65-69)	0.004	−0.043**	0.011	−0.007
	(0.009)	(0.020)	(0.008)	(0.018)
OAA per-65+ × (70-74)	−0.011	−0.090***	−0.007	−0.040**
	(0.007)	(0.026)	(0.009)	(0.020)
OAA per-65+ × (75-79)	−0.035**	−0.118***	−0.008	−0.047**
	(0.016)	(0.028)	(0.023)	(0.020)
OAA per-65+ × (80-84)	0.022	−0.111***	−0.088*	−0.111***
	(0.024)	(0.033)	(0.045)	(0.038)
Observations	9,032,441	2,095,142	6,041,997	4,787,247
Mean of dep. var.	0.034	0.28	0.051	0.38
State border × age group × year FEs	✓	✓	✓	✓
County FEs	✓	✓	✓	✓
Sample	Men (married)	Men (non-married)	Women (married)	Women (non-married)

Notes: Sample excludes single and never married individuals. Non-married individuals include the separated, divorced and widowed. Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table B.9. The Effect of OAA on Co-residence: States with vs. without Relative Responsibility Laws

Panel A: States with relative responsibility laws

	Co-residence with relatives		As household head		As dependent	
	(1)	(2)	(3)	(4)	(5)	(6)
OAA per-65+ × (55-59)	-0.071	-0.036	-0.073	-0.053	0.001	0.017
	(0.063)	(0.043)	(0.055)	(0.047)	(0.012)	(0.012)
OAA per-65+ × (60-64)	-0.047	-0.040	-0.046	-0.070	-0.002	0.029*
	(0.039)	(0.039)	(0.034)	(0.054)	(0.007)	(0.017)
OAA per-65+ × (65-69)	-0.077	-0.036	-0.050	-0.038	-0.027***	0.002
	(0.049)	(0.042)	(0.051)	(0.037)	(0.008)	(0.012)
OAA per-65+ × (70-74)	-0.117*	-0.078	-0.034	-0.021	-0.083***	-0.058**
	(0.061)	(0.054)	(0.041)	(0.040)	(0.021)	(0.023)
OAA per-65+ × (75-79)	-0.138**	-0.045	0.016	0.010	-0.154***	-0.054
	(0.070)	(0.068)	(0.050)	(0.037)	(0.022)	(0.037)
OAA per-65+ × (80-84)	-0.110*	-0.079	0.018	0.064**	-0.127***	-0.143**
	(0.056)	(0.073)	(0.042)	(0.032)	(0.035)	(0.056)
Observations	6,999,252	6,873,223	6,999,252	6,873,223	6,999,252	6,873,223
Mean of dep. var.	0.517	0.59	0.421	0.38	0.095	0.21
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
Relative responsibility laws	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Men	Women	Men	Women	Men	Women

Panel B: States without relative responsibility laws

	Co-residence with relatives		As household head		As dependent	
	(1)	(2)	(3)	(4)	(5)	(6)
OAA per-65+ × (55-59)	0.023	0.111	0.050	-0.040	-0.027	0.150
	(0.095)	(0.208)	(0.085)	(0.105)	(0.037)	(0.270)
OAA per-65+ × (60-64)	0.052	-0.027	0.046	-0.114	0.006	0.087
	(0.086)	(0.073)	(0.054)	(0.179)	(0.042)	(0.159)
OAA per-65+ × (65-69)	-0.144	-0.032	-0.177	-0.075	0.033	0.043
	(0.161)	(0.079)	(0.214)	(0.125)	(0.073)	(0.102)
OAA per-65+ × (70-74)	-0.138	-0.177	-0.199	-0.134	0.061	-0.043
	(0.168)	(0.225)	(0.273)	(0.198)	(0.114)	(0.071)
OAA per-65+ × (75-79)	-0.100	-0.134	-0.234	-0.012	0.134	-0.122
	(0.145)	(0.251)	(0.426)	(0.062)	(0.295)	(0.252)
OAA per-65+ × (80-84)	-0.049	-0.315	-0.031	-0.117	-0.018	-0.198
	(0.078)	(0.580)	(0.066)	(0.165)	(0.097)	(0.441)
Observations	5,274,483	4,933,134	5,274,483	4,933,134	5,274,483	4,933,134
Mean of dep. var.	0.503	0.594	0.413	0.373	0.09	0.222
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
Relative responsibility laws	No	No	No	No	No	No
Sample	Men	Women	Men	Women	Men	Women

Notes: Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table B.10. Robustness: Log Specification

	Co-residence with relatives		As household head		As dependent	
	(1)	(2)	(3)	(4)	(5)	(6)
log(1+OAA per-65+) × (55-59)	−0.042	−0.018	−0.027	−0.039	−0.015	0.021
	(0.031)	(0.026)	(0.026)	(0.029)	(0.012)	(0.021)
log(1+OAA per-65+) × (60-64)	−0.026	−0.048*	−0.017	−0.072**	−0.008	0.024
	(0.024)	(0.026)	(0.020)	(0.033)	(0.009)	(0.015)
log(1+OAA per-65+) × (65-69)	−0.079***	−0.061**	−0.073***	−0.059**	−0.006	−0.002
	(0.026)	(0.029)	(0.028)	(0.027)	(0.013)	(0.011)
log(1+OAA per-65+) × (70-74)	−0.104***	−0.115***	−0.066**	−0.061**	−0.037**	−0.054***
	(0.029)	(0.032)	(0.027)	(0.028)	(0.016)	(0.018)
log(1+OAA per-65+) × (75-79)	−0.121***	−0.084**	−0.046	−0.010	−0.075**	−0.074***
	(0.034)	(0.042)	(0.034)	(0.025)	(0.031)	(0.025)
log(1+OAA per-65+) × (80-84)	−0.083**	−0.131***	−0.026	0.041	−0.057*	−0.172***
	(0.034)	(0.045)	(0.027)	(0.032)	(0.033)	(0.051)
Observations	12,273,735	11,806,357	12,273,735	11,806,357	12,273,735	11,806,357
Mean of dep. var.	0.511	0.592	0.418	0.377	0.093	0.215
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
Sample	Men	Women	Men	Women	Men	Women

Notes: Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table B.11. Robustness: Narrower Geographic Comparisons

	Co-residence with relatives					
	(1)	(2)	(3)	(4)	(5)	(6)
OAA per-65+ × (55-59)	−0.038	−0.020	−0.042	−0.021	−0.051*	−0.023
	(0.027)	(0.022)	(0.026)	(0.022)	(0.029)	(0.023)
OAA per-65+ × (60-64)	−0.027	−0.042*	−0.027	−0.041*	−0.027	−0.035*
	(0.019)	(0.023)	(0.018)	(0.022)	(0.017)	(0.021)
OAA per-65+ × (65-69)	−0.062***	−0.046*	−0.060***	−0.049*	−0.055***	−0.038
	(0.024)	(0.025)	(0.022)	(0.025)	(0.021)	(0.024)
OAA per-65+ × (70-74)	−0.082***	−0.087***	−0.070***	−0.075***	−0.063***	−0.062**
	(0.028)	(0.032)	(0.024)	(0.028)	(0.024)	(0.028)
OAA per-65+ × (75-79)	−0.102***	−0.067*	−0.095***	−0.065*	−0.080**	−0.063
	(0.034)	(0.040)	(0.031)	(0.038)	(0.033)	(0.040)
OAA per-65+ × (80-84)	−0.068**	−0.095**	−0.058*	−0.087**	−0.061*	−0.092**
	(0.029)	(0.042)	(0.030)	(0.037)	(0.031)	(0.036)
Observations	10,644,509	10,237,189	9,118,862	8,839,016	6,766,538	6,654,569
Mean of dep. var.	0.524	0.603	0.531	0.608	0.537	0.614
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
State border distance	90th pct	90th pct	75th pct	75th pct	50th pct	50th pct
Sample	Men	Women	Men	Women	Men	Women

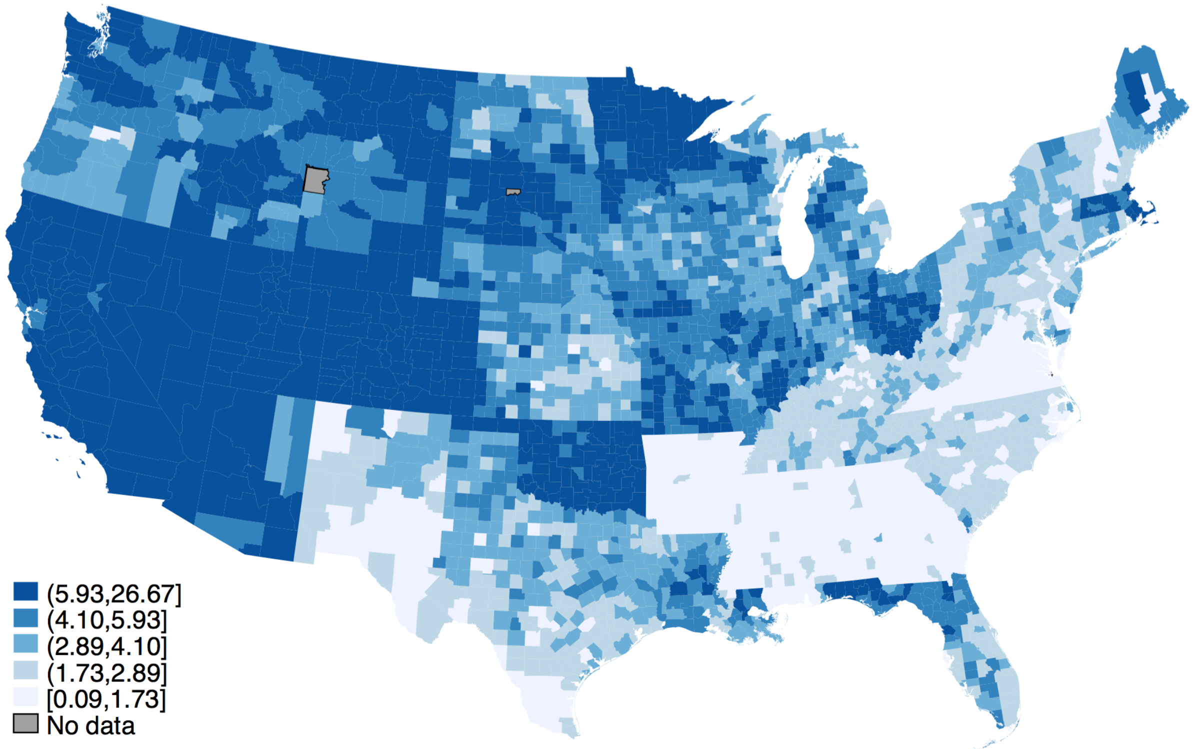
Notes: Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table B.12. 1920-1930 Placebo Regressions

	Co-residence with relatives		As household head		As dependent	
	(1)	(2)	(3)	(4)	(5)	(6)
OAA per-65+ × (55-59)	0.0003	0.035	-0.010	-0.001	0.010	0.036
	(0.029)	(0.029)	(0.030)	(0.024)	(0.016)	(0.027)
OAA per-65+ × (60-64)	-0.011	0.007	-0.023	-0.054	0.012	0.061*
	(0.031)	(0.028)	(0.034)	(0.043)	(0.012)	(0.036)
OAA per-65+ × (65-69)	-0.009	0.009	-0.019	-0.020	0.010	0.029
	(0.030)	(0.022)	(0.028)	(0.028)	(0.013)	(0.032)
OAA per-65+ × (70-74)	-0.055	-0.032	-0.036	-0.007	-0.018	-0.025
	(0.051)	(0.035)	(0.046)	(0.024)	(0.030)	(0.036)
OAA per-65+ × (75-79)	-0.015	-0.030	-0.003	0.042	-0.011	-0.072
	(0.035)	(0.037)	(0.036)	(0.036)	(0.032)	(0.052)
OAA per-65+ × (80-84)	-0.069	-0.043	-0.039	0.047	-0.030	-0.090
	(0.076)	(0.051)	(0.052)	(0.047)	(0.062)	(0.082)
Observations	7,248,821	6,708,818	7,248,821	6,708,818	7,248,821	6,708,818
Mean of dep. var.	0.547	0.639	0.449	0.399	0.098	0.239
State border × age group × year FEs	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓
Sample	Men	Women	Men	Women	Men	Women

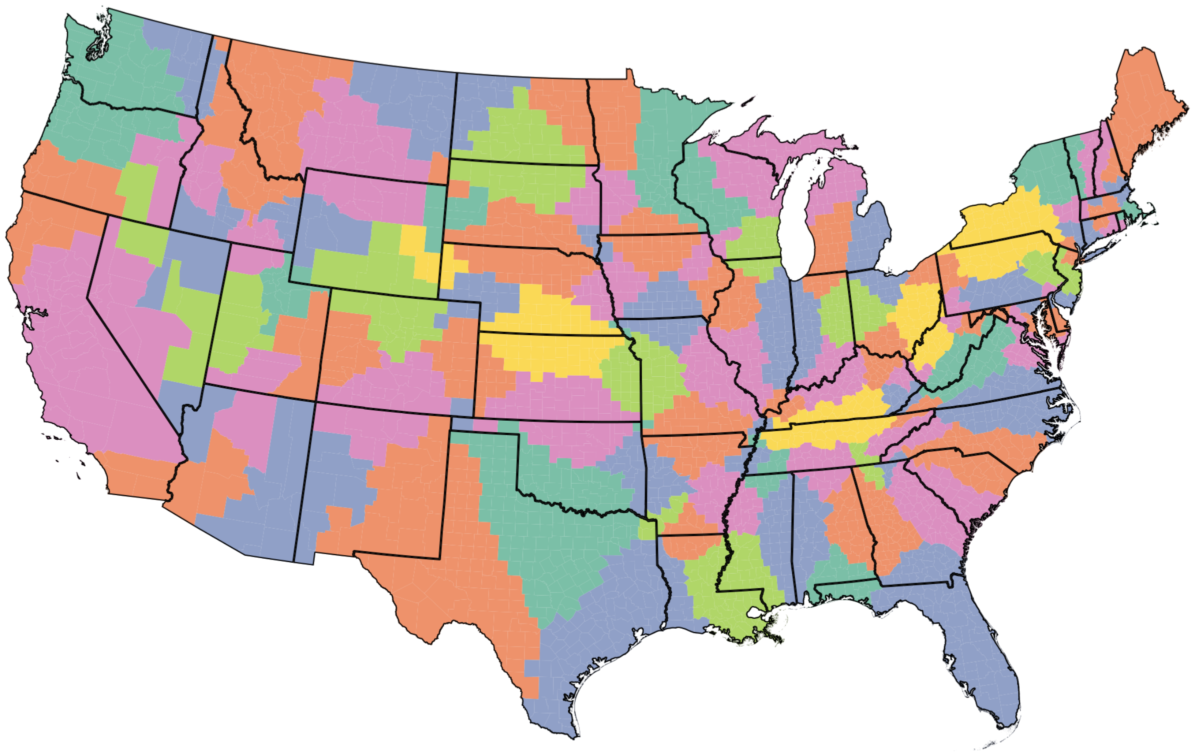
Notes: In this table, 1920 and 1930 outcomes are regressed on 1930 and 1939 OAA per-65+ payments (see Section 2.5.3 for details). The sample excludes counties in states with positive OAA payments in 1930 (CA, KY, MD, MT, NV, UT, WI, and WY), as well as counties belonging to the corresponding state border groups. Annual OAA payments per person 65+ are in hundreds of 1939 dollars. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Figure B.1. OAA Payments Per Person Aged 65+, December 1939



Notes: Figure shows total OAA payments for the month of December 1939 scaled by the population aged 65+ in the 1940 Census. OAA payments data come from U.S. Social Security Board (1940c).

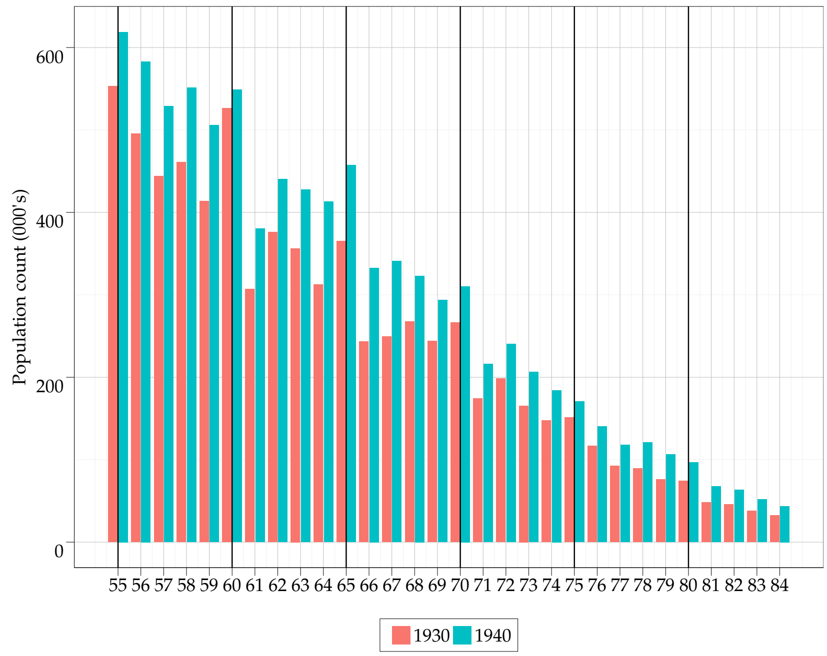
Figure B.2. State Border Groups Used in the Estimation



Notes: This figure shows state border groups used in the analysis, each comprising the set of counties closest to a given state border (based on distance to the geographic center of the county). The baseline sample in the analysis excludes Colorado, Missouri, New Hampshire, and Pennsylvania (as well as the counties belonging to the relevant border groups).

Figure B.3. Population Counts by Age, 1930-1940

Panel A: Men



Panel B: Women

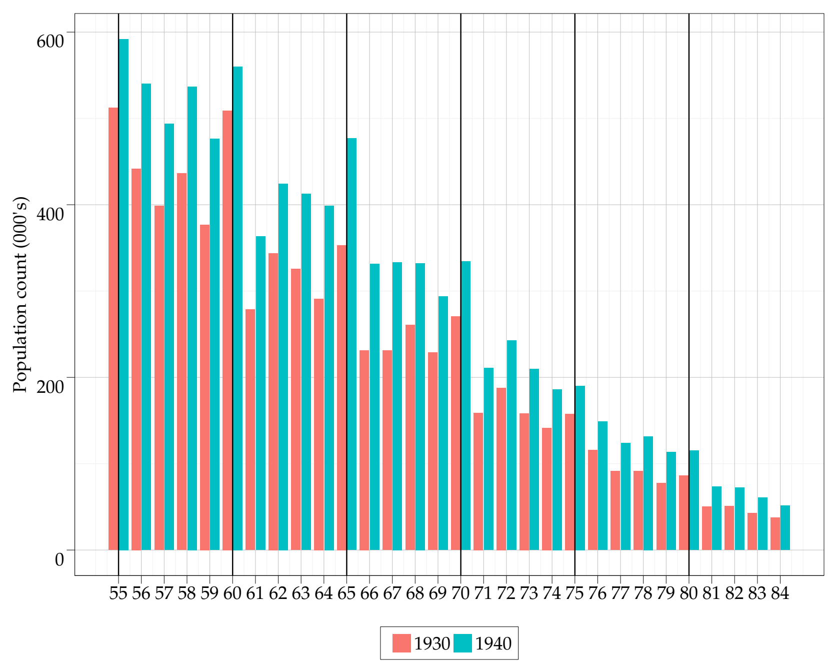
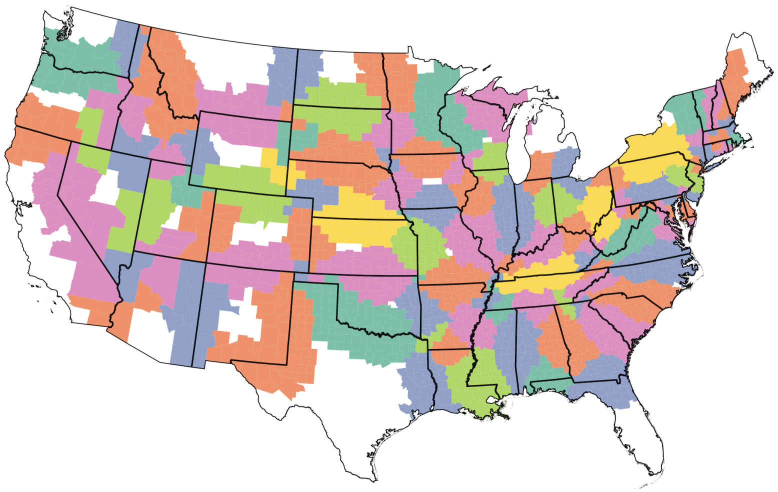
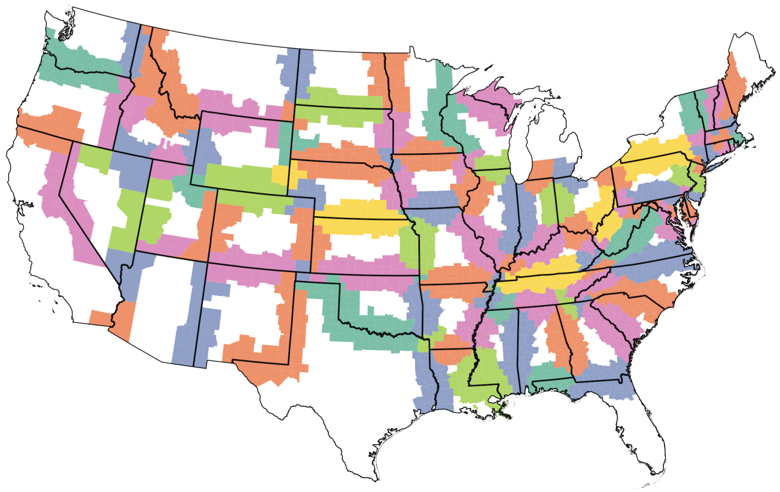


Figure B.4. Alternative State Border Comparison Groups

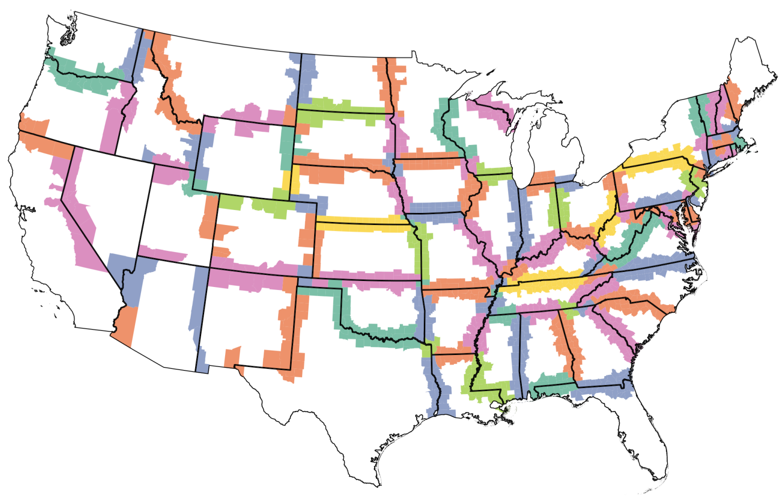
Counties within 90th percentile distance (162km)



Counties within 75th percentile distance (102km)



Counties within 50th percentile distance (56km)



APPENDIX C

Appendix to Chapter 3

C.1. Appendix Tables and Figures

Table C.1. Summary Statistics Among Recent College Graduates, 2011-2016

	2011	2012	2013	2014	2015	2016
Panel A: Demographics						
Age	23.91	23.93	23.90	23.89	23.91	23.94
Female	0.575	0.585	0.577	0.583	0.569	0.570
Hispanic	0.057	0.057	0.057	0.069	0.060	0.070
Black	0.074	0.084	0.085	0.087	0.081	0.087
Asian	0.095	0.100	0.108	0.105	0.115	0.110
Other race	0.043	0.057	0.055	0.061	0.063	0.062
Master's degree	0.175	0.192	0.172	0.171	0.187	0.197
Double major	0.128	0.117	0.112	0.119	0.117	0.117
Foreign born	0.100	0.111	0.104	0.111	0.112	0.109
Panel B: Migration						
Born in different state than current state of residence	0.440	0.434	0.430	0.429	0.433	0.427
Migrated in MSA within the last year	0.128	0.119	0.108	0.127	0.119	0.120
Panel C: Employment status						
Employed	0.878	0.893	0.892	0.884	0.895	0.901
Employed part-time (less than 35 hours/week)	0.150	0.143	0.151	0.156	0.145	0.135
Unemployed	0.062	0.055	0.054	0.055	0.048	0.044
Panel D: Occupations (conditional on employment)						
Employed in college occupation (O*NET)	0.640	0.640	0.650	0.649	0.654	0.674
Employed in college occupation (ACS)	0.590	0.603	0.607	0.625	0.625	0.639
Employed in top 5 occupation for own college major	0.357	0.364	0.355	0.352	0.354	0.356
Employed in top 10 occupation for own college major	0.468	0.475	0.471	0.466	0.462	0.475
Panel E: Income (MSA price parity-adjusted 2014$)						
Annual wage income	30,573	30,874	30,846	29,711	31,945	33,585
Hourly wage	17.14	16.95	16.66	16.56	17.25	18.09

Notes: Recent college graduates are defined as having 1 year of potential experience. Non-college occupations and top occupations by college major are defined in Section 3.4.2.

Table C.2. College Major Shares Among Recent College Graduates, 2011-2016

	Share (%)			Share (%)	
College major	2011	2016	College major	2011	2016
Arts and humanities:	*11.85*	*10.83*	*Science, math, and technology:*	*18.55*	*23.05*
English literature	3.57	3.05	Biological sciences	4.47	4.95
Music and drama	1.56	1.70	Computer science and IT	2.70	4.23
Fine arts	1.61	1.48	All other engineering	2.76	3.16
Commercial art and graphic design	1.84	1.38	Mechanical engineering	1.45	1.87
Film and visual arts	1.23	1.15	Mathematics	1.01	1.27
Philosophy and religion	1.12	1.04	Electrical engineering	1.08	1.25
Linguistics	0.93	1.03	Environmental studies	0.79	1.17
			Civil engineering	0.92	1.01
Business:	*21.70*	*18.78*	Agricultural sciences	0.52	1.00
Accounting	3.18	3.89	Chemistry	0.57	0.71
General business	3.85	3.58	All other physical sciences	0.32	0.57
Business mgmt and administration	5.07	3.52	Architecture	0.48	0.55
Marketing	3.57	3.25	Chemical engineering	0.43	0.48
Finance	3.11	2.63	Physics	0.29	0.44
All other business	2.45	1.66	Engineering technologies	0.78	0.40
Human resources	0.47	0.26			
			Social sciences:	*31.27*	*28.87*
Health and medicine:	*6.17*	*6.53*	Psychology	6.27	6.07
Nursing	3.20	3.13	Communications	5.36	5.80
Medical and health services	2.62	2.97	All other education	3.45	2.84
Medical support	0.35	0.44	Political science	2.54	2.74
			General education	2.21	1.87
Multi/interdisciplinary studies:	*6.07*	*6.98*	Economics	2.24	1.83
Fitness, nutrition, and leisure	1.92	3.10	All other social sciences	1.16	1.61
Multidisciplinary or general science	1.55	1.47	History	2.00	1.57
Liberal arts and humanities	1.12	1.08	Elementary education	2.49	1.52
Family and consumer sciences	0.89	0.94	Sociology	1.80	1.35
Area, ethnic, and civilization studies	0.59	0.38	Journalism	1.26	1.20
			International relations	0.46	0.43
Public and social services:	*3.88*	*4.29*	Library science	0.03	0.04
Criminal justice and fire protection	2.34	2.51			
Social work	1.16	1.33	*Trades and personal services:*	*0.51*	*0.67*
Legal studies	0.17	0.26	Precision production and industrial arts	0.38	0.57
Public administration	0.20	0.19	Hospitality	0.13	0.10

Notes: Recent college graduates are defined as having 1 year of potential experience.

Table C.3. Summary Statistics by College Major Among Recent College Graduates

	Average outcomes								
	Employed			Occupations (conditional on employment)					
College major	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10	Annual income	Hourly wage
Electrical engineering	0.88	0.04	0.04	0.87	0.86	0.62	0.67	55,056	28.93
Mechanical engineering	0.92	0.05	0.05	0.84	0.83	0.5	0.65	49,113	24.93
Public administration	0.90	0.14	0.08	0.62	0.47	0.07	0.26	37,815	24.93
Computer science and IT	0.88	0.09	0.04	0.78	0.76	0.59	0.64	45,470	24.43
All other engineering	0.90	0.06	0.04	0.83	0.82	0.47	0.58	48,354	24.31
Chemical engineering	0.94	0.08	0.03	0.85	0.83	0.4	0.52	49,328	24.25
Nursing	0.91	0.14	0.04	0.87	0.86	0.87	0.88	37,122	22.18
Civil engineering	0.96	0.05	0.02	0.86	0.83	0.64	0.7	44,747	22.07
Precision production and industrial arts	0.88	0.12	0.09	0.62	0.6	0.32	0.37	43,208	21.43
Mathematics	0.86	0.13	0.07	0.84	0.81	0.39	0.5	38,608	21.15
Accounting	0.93	0.07	0.04	0.84	0.82	0.66	0.76	42,930	20.64
Physics	0.84	0.13	0.06	0.88	0.85	0.29	0.35	38,415	20.29
Medical support	0.81	0.21	0.10	0.39	0.35	0.4	0.61	34,559	19.76
Finance	0.91	0.07	0.05	0.72	0.66	0.37	0.53	38,921	19.54
Engineering technologies	0.89	0.11	0.06	0.65	0.62	0.29	0.41	34,142	19.40
Economics	0.90	0.08	0.06	0.73	0.69	0.26	0.47	37,969	18.64
Medical and health services	0.89	0.17	0.04	0.68	0.6	0.24	0.37	32,824	18.20
General business	0.90	0.10	0.05	0.57	0.53	0.3	0.43	34,215	17.80
General education	0.93	0.13	0.03	0.83	0.81	0.62	0.72	30,264	17.07
Library science	0.90	0.20	0.00	1	1	1	1	30,872	16.84
Political science	0.87	0.13	0.08	0.55	0.54	0.19	0.29	29,237	16.67
Linguistics	0.85	0.18	0.05	0.65	0.58	0.15	0.3	26,746	16.67
Business mgmt and administration	0.90	0.10	0.05	0.57	0.52	0.27	0.42	31,732	16.55
All other business	0.89	0.12	0.06	0.53	0.49	0.26	0.35	31,264	16.52
Architecture	0.88	0.10	0.07	0.69	0.68	0.57	0.65	30,181	16.43
All other physical sciences	0.92	0.17	0.06	0.66	0.63	0.24	0.34	29,522	16.38
International relations	0.88	0.14	0.06	0.7	0.66	0.21	0.31	28,873	16.31
Social work	0.93	0.11	0.03	0.76	0.74	0.6	0.67	29,008	16.10

Notes: Recent college graduates are defined as having 1 year of potential experience. Non-college occupations and top occupations by college major are defined in Section 3.4.2. Annual income and hourly wages are expressed in MSA price parity-adjusted 2014 dollars.

Table C.3. (cont.) Summary Statistics by College Major Among Recent College Graduates

	Average outcomes								
	Employed			Occupations (conditional on employment)					
College major	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10	Annual income	Hourly wage
Chemistry	0.87	0.13	0.06	0.7	0.65	0.23	0.33	29,002	16.05
Marketing	0.93	0.11	0.04	0.58	0.53	0.38	0.47	31,002	15.91
Human resources	0.87	0.09	0.07	0.65	0.59	0.46	0.53	32,078	15.74
Multidisciplinary or general science	0.86	0.17	0.06	0.56	0.51	0.13	0.24	27,145	15.44
Journalism	0.92	0.19	0.05	0.66	0.64	0.31	0.43	26,800	15.43
All other education	0.91	0.18	0.04	0.8	0.79	0.63	0.73	27,247	15.36
Elementary education	0.93	0.17	0.03	0.78	0.77	0.68	0.77	26,677	15.36
Biological sciences	0.83	0.18	0.05	0.61	0.52	0.16	0.24	25,466	15.13
Commercial art and graphic design	0.87	0.19	0.07	0.67	0.66	0.56	0.67	26,788	15.06
All other social sciences	0.86	0.20	0.09	0.51	0.47	0.14	0.24	25,499	15.04
Agricultural sciences	0.90	0.10	0.04	0.41	0.36	0.23	0.34	28,006	15.03
History	0.87	0.21	0.06	0.53	0.48	0.25	0.31	25,853	14.84
Communications	0.91	0.15	0.05	0.6	0.57	0.26	0.42	27,053	14.79
Area, ethnic, and civilization studies	0.91	0.18	0.05	0.63	0.58	0.24	0.34	24,518	14.67
Family and consumer sciences	0.89	0.17	0.05	0.64	0.62	0.29	0.41	24,323	14.63
Fitness, nutrition, and leisure	0.91	0.21	0.05	0.56	0.51	0.23	0.39	24,975	14.56
Psychology	0.88	0.20	0.06	0.59	0.56	0.23	0.32	24,984	14.35
Criminal justice and fire protection	0.91	0.17	0.05	0.38	0.32	0.26	0.36	24,815	14.25
Sociology	0.89	0.16	0.06	0.58	0.54	0.2	0.29	25,854	14.16
English literature	0.86	0.20	0.07	0.57	0.55	0.22	0.34	23,618	14.02
Liberal arts and humanities	0.81	0.20	0.07	0.52	0.48	0.26	0.32	21,970	13.69
Environmental studies	0.87	0.17	0.08	0.49	0.43	0.15	0.23	22,654	13.57
Philosophy and religion	0.86	0.23	0.06	0.55	0.54	0.22	0.36	22,789	13.26
Film and visual arts	0.89	0.29	0.06	0.5	0.49	0.11	0.37	21,678	13.19
Legal studies	0.92	0.20	0.02	0.5	0.38	0.29	0.43	26,308	13.08
Fine arts	0.88	0.24	0.07	0.46	0.45	0.22	0.37	21,480	12.89
Music and drama	0.88	0.31	0.07	0.51	0.48	0.2	0.36	20,021	12.49
Hospitality	0.89	0.12	0.11	0.14	0.14	0.47	0.63	24,513	11.92

Notes: Recent college graduates are defined as having 1 year of potential experience. Non-college occupations and top occupations by college major are defined in Section 3.4.2. Annual income and hourly wages are expressed in MSA price parity-adjusted 2014 dollars.

Table C.4. Distribution of Skill Mismatch Across MSAs: Science, Math, and Technology, 2010-2016

	College major group: Science, math, and technology					
	2010		2013		2016	
	Panel A: Percentiles					
90th	0.49		0.47		0.56	
75th	0.32		0.35		0.43	
50th	0.06		0.12		0.18	
25th	-0.35		-0.27		-0.16	
10th	-1.04		-0.75		-0.53	
	Panel B: Rank (40 largest MSAs)					
1	Riverside-San Bernardino-Ontario, CA	0.15	Riverside-San Bernardino-Ontario, CA	0.32	Riverside-San Bernardino-Ontario, CA	0.38
2	Las Vegas-Henderson-Paradise, NV	-0.04	San Juan-Carolina-Caguas, PR	0.07	Las Vegas-Henderson-Paradise, NV	0.04
3	San Juan-Carolina-Caguas, PR	-0.10	Las Vegas-Henderson-Paradise, NV	0.04	San Juan-Carolina-Caguas, PR	-0.05
4	Providence-Warwick, RI-MA	-0.32	Miami-Fort Lauderdale-West Palm Beach, FL	-0.23	Providence-Warwick, RI-MA	-0.13
5	Miami-Fort Lauderdale-West Palm Beach, FL	-0.40	Orlando-Kissimmee-Sanford, FL	-0.27	Indianapolis-Carmel-Anderson, IN	-0.18
20	Detroit-Warren-Dearborn, MI	-1.04	Chicago-Naperville-Elgin, IL-IN-WI	-0.78	St. Louis, MO-IL	-0.51
21	Milwaukee-Waukesha-West Allis, WI	-1.04	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	-0.81	Chicago-Naperville-Elgin, IL-IN-WI	-0.54
36	New York-Newark-Jersey City, NY-NJ-PA	-1.77	San Francisco-Oakland-Hayward, CA	-1.36	Austin-Round Rock, TX	-1.03
37	Seattle-Tacoma-Bellevue, WA	-1.79	Charlotte-Concord-Gastonia, NC-SC	-1.42	Charlotte-Concord-Gastonia, NC-SC	-1.08
38	San Francisco-Oakland-Hayward, CA	-2.12	Austin-Round Rock, TX	-1.43	San Francisco-Oakland-Hayward, CA	-1.11
39	Washington-Arlington-Alexandria, DC-VA-MD-WV	-2.33	Washington-Arlington-Alexandria, DC-VA-MD-WV	-1.70	Washington-Arlington-Alexandria, DC-VA-MD-WV	-1.71
40	San Jose-Sunnyvale-Santa Clara, CA	-3.55	San Jose-Sunnyvale-Santa Clara, CA	-2.84	San Jose-Sunnyvale-Santa Clara, CA	-2.28

Notes: Skill mismatch is defined according to equation (3.1), and normalized to have a mean of zero and standard deviation of one (across all college majors, MSAs and years). This table shows average skill mismatch for college majors belonging to the group "Science, math, and technology," separately by MSA and by year. Panel A shows percentiles of the distribution of average skill mismatch across MSAs, separately by year. Panel B shows average skill mismatch for the 40 largest MSAs (in terms of population), separately by year.

Table C.5. Distribution of Skill Mismatch Across MSAs: Business, 2010-2016

	College major group: Business					
	2010		2013		2016	
	Panel A: Percentiles					
90th	0.24		0.17		0.37	
75th	0.15		0.08		0.25	
50th	-0.01		-0.02		0.12	
25th	-0.24		-0.21		-0.08	
10th	-0.48		-0.46		-0.30	
	Panel B: Rank (40 largest MSAs)					
1	Riverside-San Bernardino-Ontario, CA	-0.11	Riverside-San Bernardino-Ontario, CA	0.02	Riverside-San Bernardino-Ontario, CA	0.13
2	Virginia Beach-Norfolk-Newport News, VA-NC	-0.17	Las Vegas-Henderson-Paradise, NV	-0.08	Virginia Beach-Norfolk-Newport News, VA-NC	-0.02
3	Las Vegas-Henderson-Paradise, NV	-0.18	Virginia Beach-Norfolk-Newport News, VA-NC	-0.12	Las Vegas-Henderson-Paradise, NV	-0.10
4	San Antonio-New Braunfels, TX	-0.25	Orlando-Kissimmee-Sanford, FL	-0.27	Providence-Warwick, RI-MA	-0.12
5	Providence-Warwick, RI-MA	-0.30	Indianapolis-Carmel-Anderson, IN	-0.28	Sacramento–Roseville–Arden-Arcade, CA	-0.17
20	Miami-Fort Lauderdale-West Palm Beach, FL	-0.50	Minneapolis-St. Paul-Bloomington, MN-WI	-0.46	San Diego-Carlsbad, CA	-0.31
21	St. Louis, MO-IL	-0.52	Sacramento–Roseville–Arden-Arcade, CA	-0.47	Denver-Aurora-Lakewood, CO	-0.32
36	San Francisco-Oakland-Hayward, CA	-0.88	San Francisco-Oakland-Hayward, CA	-0.73	Charlotte-Concord-Gastonia, NC-SC	-0.54
37	Chicago-Naperville-Elgin, IL-IN-WI	-0.93	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.75	San Jose-Sunnyvale-Santa Clara, CA	-0.65
38	San Jose-Sunnyvale-Santa Clara, CA	-0.94	Charlotte-Concord-Gastonia, NC-SC	-0.78	San Francisco-Oakland-Hayward, CA	-0.66
39	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.95	San Jose-Sunnyvale-Santa Clara, CA	-0.82	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.67
40	New York-Newark-Jersey City, NY-NJ-PA	-1.02	New York-Newark-Jersey City, NY-NJ-PA	-0.97	New York-Newark-Jersey City, NY-NJ-PA	-0.72

Notes: Skill mismatch is defined according to equation (3.1), and normalized to have a mean of zero and standard deviation of one (across all college majors, MSAs and years). This table shows average skill mismatch for college majors belonging to the group "Business," separately by MSA and by year. Panel A shows percentiles of the distribution of average skill mismatch across MSAs, separately by year. Panel B shows average skill mismatch for the 40 largest MSAs (in terms of population), separately by year.

Table C.6. Distribution of Skill Mismatch Across MSAs: Social Sciences, 2010-2016

	College major group: Social sciences					
	2010		2013		2016	
	Panel A: Percentiles					
90th	0.52		0.49		0.53	
75th	0.44		0.43		0.44	
50th	0.33		0.32		0.35	
25th	0.20		0.19		0.20	
10th	0.05		0.05		0.09	
	Panel B: Rank (40 largest MSAs)					
1	San Juan-Carolina-Caguas, PR	0.36	San Juan-Carolina-Caguas, PR	0.39	San Juan-Carolina-Caguas, PR	0.37
2	Las Vegas-Henderson-Paradise, NV	0.32	Las Vegas-Henderson-Paradise, NV	0.34	Milwaukee-Waukesha-West Allis, WI	0.32
3	Virginia Beach-Norfolk-Newport News, VA-NC	0.30	Virginia Beach-Norfolk-Newport News, VA-NC	0.31	Indianapolis-Carmel-Anderson, IN	0.31
4	Pittsburgh, PA	0.28	Orlando-Kissimmee-Sanford, FL	0.28	Orlando-Kissimmee-Sanford, FL	0.31
5	San Antonio-New Braunfels, TX	0.26	Riverside-San Bernardino-Ontario, CA	0.27	Cincinnati, OH-KY-IN	0.29
20	Phoenix-Mesa-Scottsdale, AZ	0.13	Houston-The Woodlands-Sugar Land, TX	0.13	Atlanta-Sandy Springs-Roswell, GA	0.15
21	Sacramento–Roseville–Arden-Arcade, CA	0.12	Providence-Warwick, RI-MA	0.12	Tampa-St. Petersburg-Clearwater, FL	0.14
36	Minneapolis-St. Paul-Bloomington, MN-WI	-0.09	Austin-Round Rock, TX	-0.09	New York-Newark-Jersey City, NY-NJ-PA	-0.01
37	New York-Newark-Jersey City, NY-NJ-PA	-0.14	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.12	Boston-Cambridge-Newton, MA-NH	-0.07
38	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.18	New York-Newark-Jersey City, NY-NJ-PA	-0.12	San Francisco-Oakland-Hayward, CA	-0.08
39	San Francisco-Oakland-Hayward, CA	-0.18	Boston-Cambridge-Newton, MA-NH	-0.14	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.09
40	San Jose-Sunnyvale-Santa Clara, CA	-0.24	San Jose-Sunnyvale-Santa Clara, CA	-0.15	San Jose-Sunnyvale-Santa Clara, CA	-0.15

Notes: Skill mismatch is defined according to equation (3.1), and normalized to have a mean of zero and standard deviation of one (across all college majors, MSAs and years). This table shows average skill mismatch for college majors belonging to the group "Social sciences," separately by MSA and by year. Panel A shows percentiles of the distribution of average skill mismatch across MSAs, separately by year. Panel B shows average skill mismatch for the 40 largest MSAs (in terms of population), separately by year.

Table C.7. Distribution of Skill Mismatch Across MSAs: Arts and Humanities, 2010-2016

	College major group: Arts and humanities					
	2010		2013		2016	
	Panel A: Percentiles					
90th	0.50		0.47		0.52	
75th	0.44		0.42		0.46	
50th	0.36		0.33		0.38	
25th	0.26		0.23		0.29	
10th	0.15		0.13		0.20	
	Panel B: Rank (40 largest MSAs)					
1	San Juan-Carolina-Caguas, PR	0.41	Las Vegas-Henderson-Paradise, NV	0.38	Milwaukee-Waukesha-West Allis, WI	0.37
2	Las Vegas-Henderson-Paradise, NV	0.38	Riverside-San Bernardino-Ontario, CA	0.33	Indianapolis-Carmel-Anderson, IN	0.37
3	San Antonio-New Braunfels, TX	0.32	San Juan-Carolina-Caguas, PR	0.33	San Juan-Carolina-Caguas, PR	0.37
4	Virginia Beach-Norfolk-Newport News, VA-NC	0.31	Milwaukee-Waukesha-West Allis, WI	0.33	Riverside-San Bernardino-Ontario, CA	0.37
5	Pittsburgh, PA	0.30	Virginia Beach-Norfolk-Newport News, VA-NC	0.32	Orlando-Kissimmee-Sanford, FL	0.36
20	Portland-Vancouver-Hillsboro, OR-WA	0.17	Detroit-Warren-Dearborn, MI	0.18	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	0.23
21	Denver-Aurora-Lakewood, CO	0.17	Pittsburgh, PA	0.15	Tampa-St. Petersburg-Clearwater, FL	0.23
36	Seattle-Tacoma-Bellevue, WA	-0.05	Boston-Cambridge-Newton, MA-NH	-0.03	New York-Newark-Jersey City, NY-NJ-PA	0.08
37	New York-Newark-Jersey City, NY-NJ-PA	-0.10	San Francisco-Oakland-Hayward, CA	-0.03	Boston-Cambridge-Newton, MA-NH	0.06
38	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.14	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.07	San Francisco-Oakland-Hayward, CA	0
39	San Francisco-Oakland-Hayward, CA	-0.17	New York-Newark-Jersey City, NY-NJ-PA	-0.07	Washington-Arlington-Alexandria, DC-VA-MD-WV	-0.03
40	San Jose-Sunnyvale-Santa Clara, CA	-0.30	San Jose-Sunnyvale-Santa Clara, CA	-0.20	San Jose-Sunnyvale-Santa Clara, CA	-0.13

Notes: Skill mismatch is defined according to equation (3.1), and normalized to have a mean of zero and standard deviation of one (across all college majors, MSAs and years). This table shows average skill mismatch for college majors belonging to the group "Arts and humanities," separately by MSA and by year. Panel A shows percentiles of the distribution of average skill mismatch across MSAs, separately by year. Panel B shows average skill mismatch for the 40 largest MSAs (in terms of population), separately by year.

Table C.8. Top 3 Occupations by College Major

College major	Top 3 occupations: 1	2	3
Accounting	Accountants and auditors	Managers and administrators, n.e.c.	Financial managers
Agricultural sciences	Farmers (owners and tenants)	Managers and administrators, n.e.c.	Sales supervisors and proprietors
All other business	Managers and administrators, n.e.c.	Sales supervisors and proprietors	Accountants and auditors
All other education	Primary school teachers	Secondary school teachers	Managers in education and related fields
All other engineering	Managers and administrators, n.e.c.	Computer software developers	Engineers and other professionals, n.e.c.
All other physical sciences	Managers and administrators, n.e.c.	Geologists	Computer software developers
All other social sciences	Managers and administrators, n.e.c.	Primary school teachers	Computer systems analysts and computer scientists
Architecture	Architects	Managers and administrators, n.e.c.	Designers
Area, ethnic, and civilization studies	Managers and administrators, n.e.c.	Primary school teachers	Subject instructors, college
Biological sciences	Managers and administrators, n.e.c.	Primary school teachers	Registered nurses
Business mgmt and administration	Managers and administrators, n.e.c.	Accountants and auditors	Sales supervisors and proprietors
Chemical engineering	Managers and administrators, n.e.c.	Chemical engineers	Engineers and other professionals, n.e.c.
Chemistry	Managers and administrators, n.e.c.	Chemists	Primary school teachers
Civil engineering	Managers and administrators, n.e.c.	Civil engineers	Engineers and other professionals, n.e.c.
Commercial art and graphic design	Designers	Managers and administrators, n.e.c.	Painters, sculptors, craft-artists and print-makers
Communications	Managers and administrators, n.e.c.	Managers and specialists in mktg advertising and PR	Primary school teachers
Computer science and IT	Computer software developers	Computer systems analysts and computer scientists	Managers and administrators, n.e.c.
Criminal justice and fire protection	Police and detectives, public service	Managers and administrators, n.e.c.	Guards and police, excluding public service
Economics	Managers and administrators, n.e.c.	Other financial specialists	Accountants and auditors
Electrical engineering	Managers and administrators, n.e.c.	Computer software developers	Electrical engineers
Elementary education	Primary school teachers	Managers in education and related fields	Kindergarten and earlier school teachers
Engineering technologies	Managers and administrators, n.e.c.	Computer systems analysts and computer scientists	Computer software developers
English literature	Primary school teachers	Managers and administrators, n.e.c.	Subject instructors, college
Environmental studies	Managers and administrators, n.e.c.	Foresters and conservation scientists	Primary school teachers
Family and consumer sciences	Primary school teachers	Managers and administrators, n.e.c.	Social workers
Film and visual arts	Managers and administrators, n.e.c.	Primary school teachers	Photographers
Finance	Managers and administrators, n.e.c.	Other financial specialists	Accountants and auditors
Fine arts	Designers	Managers and administrators, n.e.c.	Primary school teachers

Notes: The top 3 occupations by college major are defined in terms of employment share, based on pooled 2009-2016 ACS data and the sample of individuals aged 32 or older with a Bachelor's or Master's degree.

Table C.8. (cont.) Top 3 Occupations by College Major

	Top 3 occupations		
College major	1	2	3
Fitness, nutrition, and leisure	Managers and administrators, n.e.c.	Primary school teachers	Dieticians and nutritionists
General business	Managers and administrators, n.e.c.	Sales supervisors and proprietors	Accountants and auditors
General education	Primary school teachers	Secondary school teachers	Managers in education and related fields
History	Managers and administrators, n.e.c.	Primary school teachers	Sales supervisors and proprietors
Hospitality	Cooks	Managers and administrators, n.e.c.	Funeral directors
Human resources	Personnel, HR, training and labor relations specialists	Managers and administrators, n.e.c.	Human resources and labor relations managers
International relations	Managers and administrators, n.e.c.	Managers and specialists in mktg, advertising and PR	Management analysts
Journalism	Editors and reporters	Managers and administrators, n.e.c.	Managers and specialists in mktg, advertising and PR
Legal studies	Legal assistants and paralegals	Managers and administrators, n.e.c.	Secretaries and stenographers
Liberal arts and humanities	Primary school teachers	Managers and administrators, n.e.c.	Sales supervisors and proprietors
Library science	Librarians	Primary school teachers	Managers in education and related fields
Linguistics	Primary school teachers	Managers and administrators, n.e.c.	Secondary school teachers
Marketing	Managers and administrators, n.e.c.	Salespersons, n.e.c.	Managers and specialists in mktg, advertising and PR
Mathematics	Managers and administrators, n.e.c.	Computer software developers	Computer systems analysts and computer scientists
Mechanical engineering	Managers and administrators, n.e.c.	Mechanical engineers	Engineers and other professionals, n.e.c.
Medical and health services	Pharmacists	Physical therapists	Speech therapists
Medical support	Clinical laboratory technologies and technicians	Dental hygienists	Radiologic technologists and technicians
Multidisciplinary or general science	Managers and administrators, n.e.c.	Primary school teachers	Registered nurses
Music and drama	Musicians and composers	Teachers, n.e.c.	Managers and administrators, n.e.c.
Nursing	Registered nurses	Managers of medicine and health occupations	Health and nursing aides
Philosophy and religion	Clergy and religious workers	Managers and administrators, n.e.c.	Primary school teachers
Physics	Managers and administrators, n.e.c.	Computer software developers	Computer systems analysts and computer scientists
Political science	Managers and administrators, n.e.c.	Primary school teachers	Sales supervisors and proprietors
Precision production and industrial arts	Managers and administrators, n.e.c.	Airplane pilots and navigators	Chief executives, public administrators and legislators
Psychology	Managers and administrators, n.e.c.	Vocational and educational counselors	Primary school teachers
Public administration	Managers and administrators, n.e.c.	Police and detectives, public service	Chief executives, public administrators and legislators
Social work	Social workers	Managers and administrators, n.e.c.	Vocational and educational counselors
Sociology	Managers and administrators, n.e.c.	Social workers	Primary school teachers

Notes: The top 3 occupations by college major are defined in terms of employment share, based on pooled 2009-2016 ACS data and the sample of individuals aged 32 or older with a Bachelor's or Master's degree.

Table C.9. The Effect of Skill Mismatch vs. Overall Unemployment Rates at the MSA Level in Graduation Year

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment rate (%) ×	-0.024***	-0.009**	-0.010***	0.005**	0.005***	-0.006**	-0.005*	-0.005	-0.001
1-2 years of potential exp.	(0.007)	(0.004)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.004)	(0.003)
Unemployment rate (%) ×	-0.014**	-0.008**	-0.005***	0.002	0.004**	-0.005**	-0.004	-0.005	-0.004
3-4 years of potential exp.	(0.005)	(0.004)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)
Unemployment rate (%) ×	-0.003	-0.003	-0.003	-0.002	0.001	-0.002	-0.001	-0.002	-0.001
5-6 years of potential exp.	(0.005)	(0.004)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.004)	(0.003)
Skill mismatch ×	-0.049***	-0.032***	-0.005***	0.008***	0.004***	-0.009***	-0.008***	-0.019***	-0.017***
1-2 years of potential exp.	(0.005)	(0.004)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)
Skill mismatch ×	-0.038***	-0.028***	-0.006***	0.003**	0.003***	-0.006***	-0.005***	-0.015***	-0.015***
3-4 years of potential exp.	(0.005)	(0.004)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)
Skill mismatch ×	-0.035***	-0.027***	-0.007***	-0.000	0.002*	-0.004*	-0.003	-0.018***	-0.019***
5-6 years of potential exp.	(0.006)	(0.005)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)
MSA FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.170	0.188	0.050	0.041	0.020	0.112	0.120	0.165	0.126
Observations	140,041	140,041	151,104	151,104	151,104	136,092	136,092	136,092	136,092

Notes: Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table C.10. Distribution of Online Job Postings Per Capita Across MSAs, 2010-2016

	2010		2013		2016	
			Panel A: Percentiles			
90th	0.05		0.08		0.10	
75th	0.04		0.07		0.08	
50th	0.03		0.05		0.07	
25th	0.02		0.04		0.05	
10th	0.02		0.03		0.04	
			Panel B: Rank (40 largest MSAs)			
1	San Jose-Sunnyvale-Santa Clara, CA	0.09	San Jose-Sunnyvale-Santa Clara, CA	0.12	San Francisco-Oakland-Hayward, CA	0.15
2	Washington-Arlington-Alexandria, DC-VA-MD-WV	0.07	San Francisco-Oakland-Hayward, CA	0.11	Denver-Aurora-Lakewood, CO	0.15
3	Boston-Cambridge-Newton, MA-NH	0.06	Boston-Cambridge-Newton, MA-NH	0.10	San Jose-Sunnyvale-Santa Clara, CA	0.15
4	San Francisco-Oakland-Hayward, CA	0.06	Denver-Aurora-Lakewood, CO	0.10	Portland-Vancouver-Hillsboro, OR-WA	0.14
5	Denver-Aurora-Lakewood, CO	0.06	Washington-Arlington-Alexandria, DC-VA-MD-WV	0.09	Washington-Arlington-Alexandria, DC-VA-MD-WV	0.13
20	Milwaukee-Waukesha-West Allis, WI	0.04	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	0.06	Dallas-Fort Worth-Arlington, TX	0.10
21	Nashville-Davidson–Murfreesboro–Franklin, TN	0.04	Phoenix-Mesa-Scottsdale, AZ	0.06	San Diego-Carlsbad, CA	0.09
36	Houston-The Woodlands-Sugar Land, TX	0.03	Providence-Warwick, RI-MA	0.05	Miami-Fort Lauderdale-West Palm Beach, FL	0.06
37	Virginia Beach-Norfolk-Newport News, VA-NC	0.03	Miami-Fort Lauderdale-West Palm Beach, FL	0.05	San Antonio-New Braunfels, TX	0.06
38	Miami-Fort Lauderdale-West Palm Beach, FL	0.03	San Antonio-New Braunfels, TX	0.04	Houston-The Woodlands-Sugar Land, TX	0.06
39	Riverside-San Bernardino-Ontario, CA	0.02	Riverside-San Bernardino-Ontario, CA	0.03	Riverside-San Bernardino-Ontario, CA	0.05
40	San Juan-Carolina-Caguas, PR	0.00	San Juan-Carolina-Caguas, PR	0.00	San Juan-Carolina-Caguas, PR	0.00

Source: Burning Glass Technologies.

Table C.11. Robustness: Top 100 MSAs in Terms of Online Job Postings Per Capita

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Skill mismatch × 1-2 years of potential exp.	-0.053***	-0.036***	-0.009***	0.007***	0.005***	-0.003	-0.002	-0.015***	-0.014**
	(0.008)	(0.005)	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)	(0.006)	(0.006)
Skill mismatch × 3-4 years of potential exp.	-0.035***	-0.026***	-0.009***	0.001	0.003***	0.000	0.001	-0.014***	-0.014***
	(0.006)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.005)	(0.005)
Skill mismatch × 5-6 years of potential exp.	-0.035***	-0.025***	-0.011***	0.000	0.003**	0.002	0.004	-0.015*	-0.016***
	(0.007)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.008)	(0.006)
MSA × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.179	0.205	0.065	0.050	0.027	0.121	0.129	0.177	0.137
Observations	80,842	80,842	86,571	86,571	86,571	78,771	78,771	78,771	78,771

Notes: Sample restricted to top 100 MSAs in terms of online job postings per capita (separately by year). Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table C.12. Robustness: Excluding Master's Degree Holders

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Skill mismatch × 1-2 years of potential exp.	-0.052***	-0.035***	-0.002	0.009***	0.004***	-0.015***	-0.013***	-0.020***	-0.018***
	(0.005)	(0.005)	(0.002)	(0.002)	(0.001)	(0.003)	(0.004)	(0.004)	(0.004)
Skill mismatch × 3-4 years of potential exp.	-0.035***	-0.027***	-0.002	0.002	0.002	-0.011**	-0.010**	-0.014***	-0.015***
	(0.007)	(0.006)	(0.001)	(0.002)	(0.001)	(0.004)	(0.004)	(0.005)	(0.004)
Skill mismatch × 5-6 years of potential exp.	-0.032***	-0.026***	-0.004**	-0.000	0.002	-0.011***	-0.010***	-0.017***	-0.021***
	(0.007)	(0.006)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.005)	(0.004)
MSA × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.168	0.173	0.061	0.056	0.037	0.104	0.111	0.179	0.138
Observations	116,913	116,913	126,080	126,080	126,080	113,369	113,369	113,369	113,369

Notes: Sample excludes individuals who hold a Master's degree. Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table C.13. Robustness: Alternative Definition of Skill Mismatch

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Skill mismatch ×	-0.147***	-0.064**	-0.030***	0.027***	0.013*	0.005	0.008	-0.056**	-0.045*
1-2 years of potential exp.	(0.030)	(0.031)	(0.011)	(0.009)	(0.007)	(0.018)	(0.017)	(0.026)	(0.024)
Skill mismatch ×	-0.135***	-0.064**	-0.036***	0.010	0.014*	0.011	0.010	-0.054**	-0.047*
3-4 years of potential exp.	(0.030)	(0.030)	(0.010)	(0.009)	(0.007)	(0.017)	(0.016)	(0.026)	(0.024)
Skill mismatch ×	-0.124***	-0.061**	-0.044***	0.004	0.011*	0.017	0.014	-0.060**	-0.055**
5-6 years of potential exp.	(0.030)	(0.030)	(0.010)	(0.010)	(0.007)	(0.018)	(0.016)	(0.027)	(0.025)
MSA × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.180	0.198	0.061	0.052	0.031	0.124	0.132	0.173	0.135
Observations	150,844	150,844	162,508	162,508	162,508	146,566	146,566	146,566	146,566

Notes: The alternative definition of skill mismatch is described in Section 3.4.6. Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table C.14. Robustness: Restricting to "Non-Movers" with 1 Year of Potential Experience

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Skill mismatch	-0.040***	-0.034***	-0.006*	0.002	0.005***	-0.007	-0.004	-0.022***	-0.020***
	(0.014)	(0.009)	(0.004)	(0.003)	(0.002)	(0.005)	(0.004)	(0.006)	(0.006)
MSA × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.206	0.224	0.108	0.117	0.089	0.195	0.206	0.246	0.207
Observations	30,795	30,795	32,886	32,886	32,886	29,586	29,586	29,586	29,586

Notes: Sample excludes individuals with more than 1 year of potential experience and individuals who migrated from a different state in the last year (i.e. "movers"). Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table C.15. Robustness: Excluding Individuals Born Out-of-State

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Skill mismatch ×	-0.025***	-0.013***	-0.005**	0.005*	0.004***	-0.009***	-0.008**	-0.011***	-0.009**
1-2 years of potential exp.	(0.005)	(0.005)	(0.002)	(0.003)	(0.001)	(0.003)	(0.004)	(0.003)	(0.004)
Skill mismatch ×	-0.014**	-0.011**	-0.005***	-0.000	0.002	-0.006	-0.005	-0.003	-0.004
3-4 years of potential exp.	(0.007)	(0.004)	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)	(0.004)	(0.004)
Skill mismatch ×	-0.013*	-0.013**	-0.004*	-0.006**	0.002	-0.006	-0.005	-0.003	-0.004
5-6 years of potential exp.	(0.008)	(0.005)	(0.002)	(0.003)	(0.001)	(0.005)	(0.004)	(0.005)	(0.006)
MSA × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.191	0.202	0.056	0.065	0.049	0.139	0.147	0.192	0.154
Observations	81,277	81,277	86,096	86,096	86,096	78,952	78,952	78,952	78,952

Notes: Sample excludes individuals born in a different state than their current state of residence. Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

Table C.16. Robustness: State-Level Regressions

	Dependent variable:								
			Employed			Occupations			
	Log income	Log wage	Any	Part-time	Unemployed	College (O*NET)	College (ACS)	Top 5	Top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Skill mismatch ×	-0.052***	-0.033***	-0.007***	0.011***	0.004***	-0.011**	-0.009**	-0.015***	-0.015***
1-2 years of potential exp.	(0.007)	(0.006)	(0.002)	(0.003)	(0.001)	(0.005)	(0.004)	(0.004)	(0.005)
Skill mismatch ×	-0.038***	-0.027***	-0.006***	0.004	0.003***	-0.006	-0.006	-0.013***	-0.014**
3-4 years of potential exp.	(0.006)	(0.006)	(0.002)	(0.003)	(0.001)	(0.004)	(0.004)	(0.005)	(0.006)
Skill mismatch ×	-0.035***	-0.027***	-0.010***	-0.000	0.002*	-0.004	-0.005	-0.015**	-0.016***
5-6 years of potential exp.	(0.009)	(0.007)	(0.002)	(0.003)	(0.001)	(0.003)	(0.003)	(0.006)	(0.006)
State × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
College major × cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Potential experience FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.162	0.183	0.046	0.037	0.018	0.114	0.122	0.164	0.126
Observations	177,043	177,043	190,714	190,714	190,714	172,090	172,090	172,090	172,090

Notes: In this table, skill mismatch is defined at the state level (rather than at the MSA level), and the sample includes individuals who live outside of MSAs. Individual controls include an indicator for being female, race fixed effects (Hispanic, Black, Asian, other race), an indicator for being foreign born, an indicator for having a Master's degree and an indicator for being a double major. Observations are weighted by Census sampling weights. Robust standard errors in parentheses, clustered at the state level. *** 1%, ** 5%, * 10% significance.

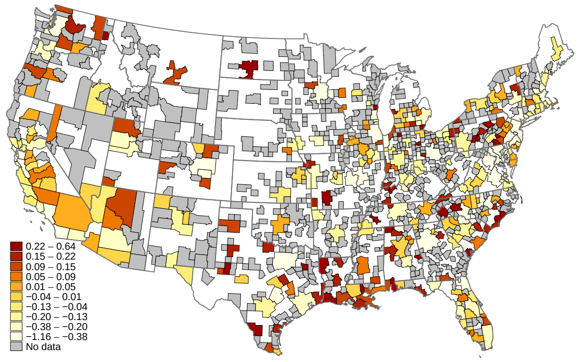
Table C.17. JOLTS Adjustment: Stylized Example

Year	Occupation	Industry	Real vs. BGT vacancies: Real vacancies	Real vacancy share	BGT vacancies	BGT vacancy share	JOLTS adjustment: BGT industry share	JOLTS vacancies	JOLTS industry share	JOLTS adjustment factor	Adjusted BGT vacancy share
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2010	Software Developer	IT	30	30%	15	45.5%	75.8%	50	50%	0.66	30%
2010	Database Administrator	IT	20	20%	10	30.3%					20%
2010	Construction Manager	Construction	30	30%	6	18.2%	24.2%	50	50%	2.06	37.5%
2010	Construction Worker	Construction	20	20%	2	6.1%					12.5%
	Total		100	100%	33	100%	100%	100	100%		100%
2016	Software Developer	IT	36	30%	24	38.1%	63.5%	60	50%	0.79	30%
2016	Database Administrator	IT	24	20%	16	25.4%					20%
2016	Construction Manager	Construction	36	30%	15	23.8%	36.5%	60	50%	1.37	32.6%
2016	Construction Worker	Construction	24	20%	8	12.7%					17.4%
	Total		120	100%	63	100%	100%	120	100%		100%

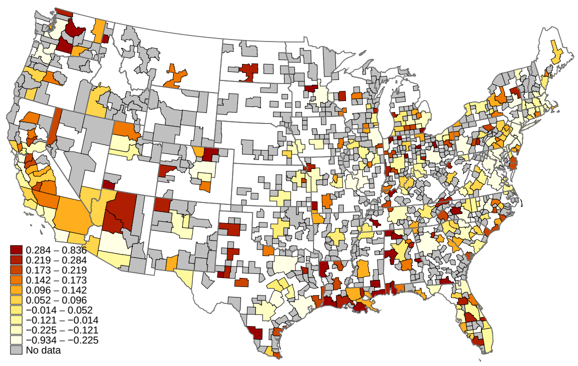
Notes: The numbers in this table are purely hypothetical (see Section 3.4.6 for details).

Figure C.1. Average Skill Mismatch by MSA, 2010-2016

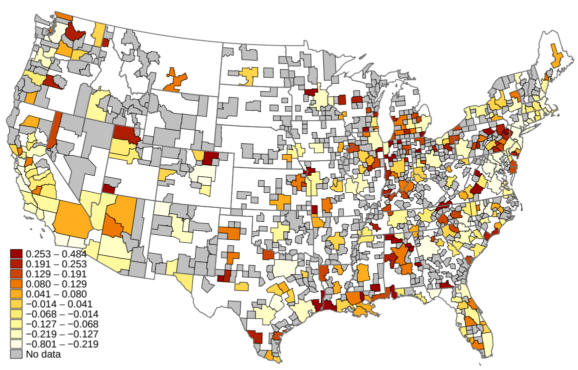
Panel A: 2010



Panel B: 2013



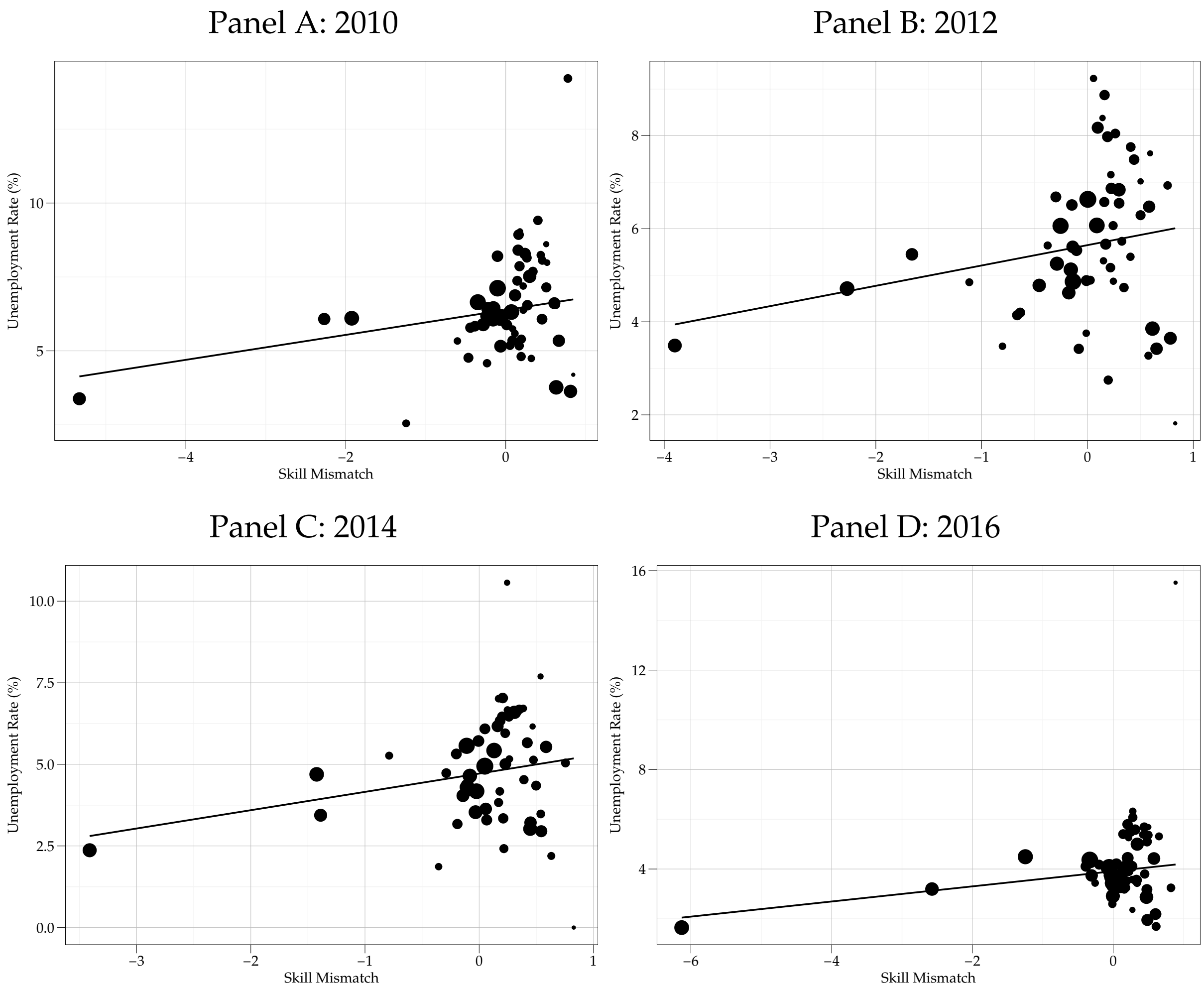
Panel C: 2016



Notes: Each map plots skill mismatch averaged across college majors (unweighted) for each of the 294 MSAs in the ACS, organized into year-specific deciles.

Source: American Community Survey, Burning Glass Technologies.

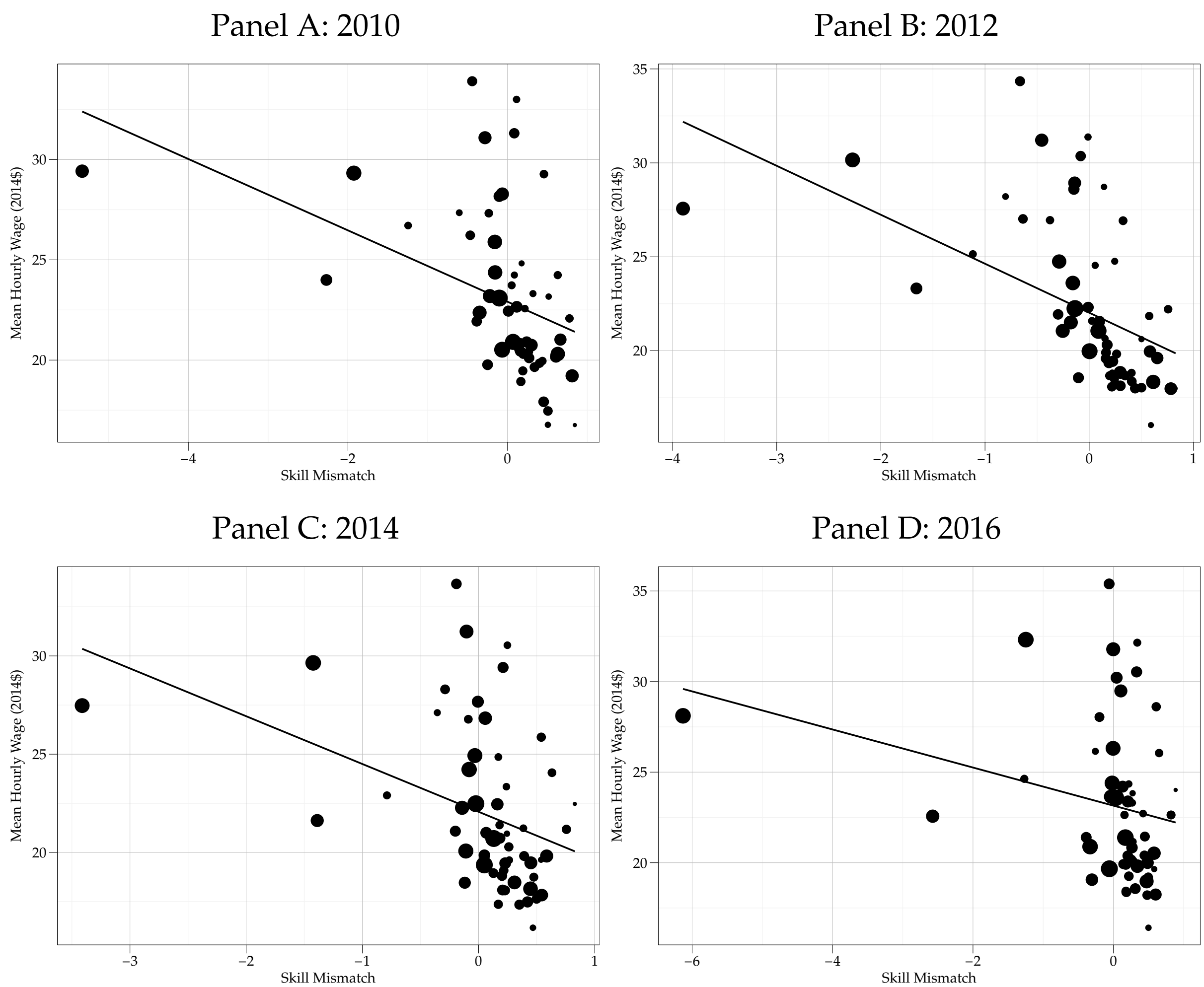
Figure C.2. Average Skill Mismatch vs. Unemployment Rates Across College Majors, 2010-2016



Notes: Each dot represents average skill mismatch (*x*-axis) and the national unemployment rate (*y*-axis) for one of the 56 college majors, separately by year. National unemployment rates are based on recent college graduates aged 22 to 31 with a Bachelor's or Master's degree. The size of each dot is proportional to the number of recent college graduates holding the corresponding major nationally, and the solid line represents the slope from a weighted linear regression.

Source: American Community Survey, Burning Glass Technologies.

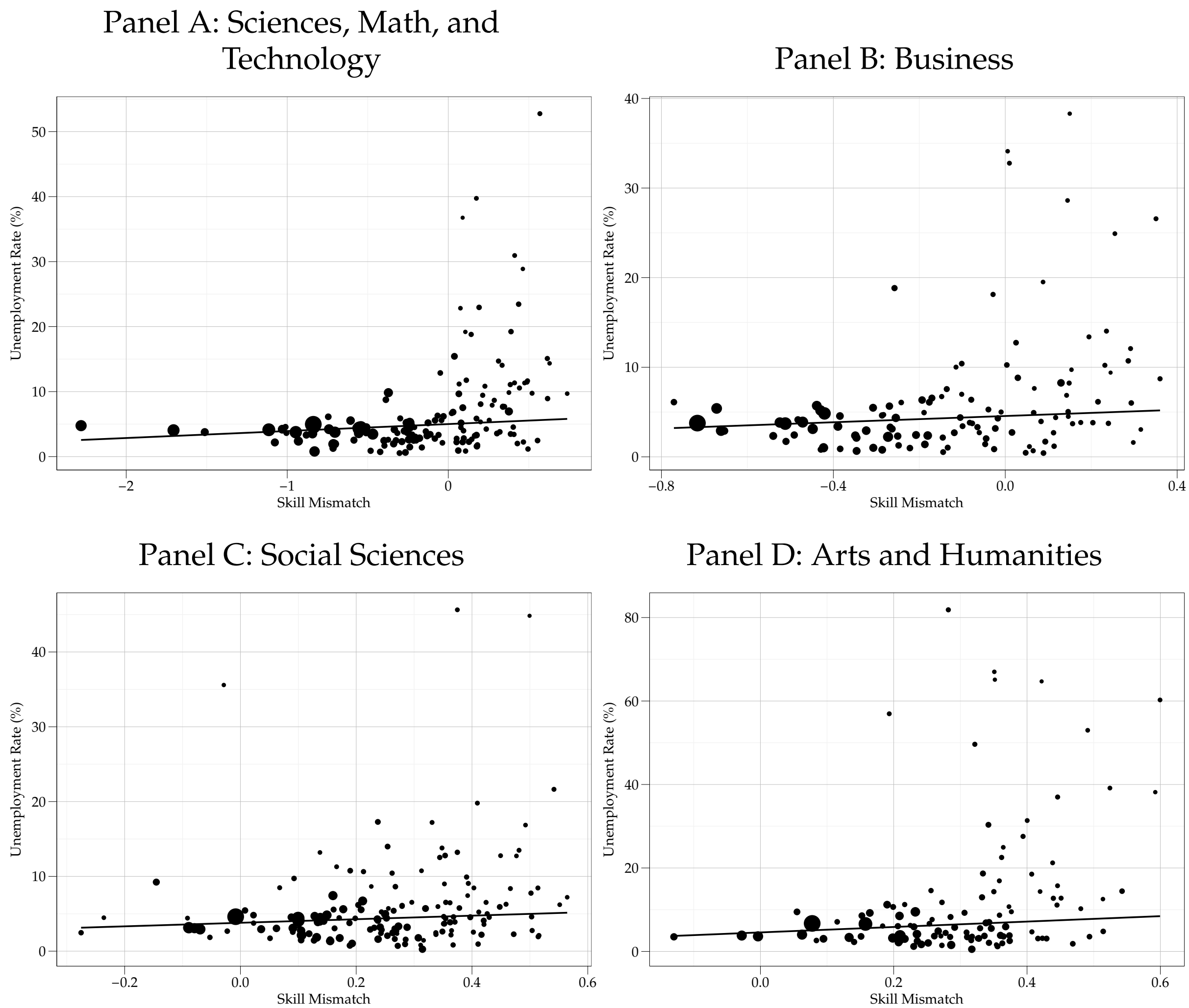
Figure C.3. Average Skill Mismatch vs. Mean Hourly Wages Across College Majors, 2010-2016



Notes: Each dot represents average skill mismatch (*x*-axis) and the mean hourly wage (*y*-axis) for one of the 56 college majors, separately by year. Mean hourly wages at the national level are based on recent college graduates aged 22 to 31 with a Bachelor's or Master's degree, earning a positive wage, and not attending school. The size of each dot is proportional to the number of recent college graduates holding the corresponding major nationally, and the solid line represents the slope from a weighted linear regression.

Source: American Community Survey, Burning Glass Technologies.

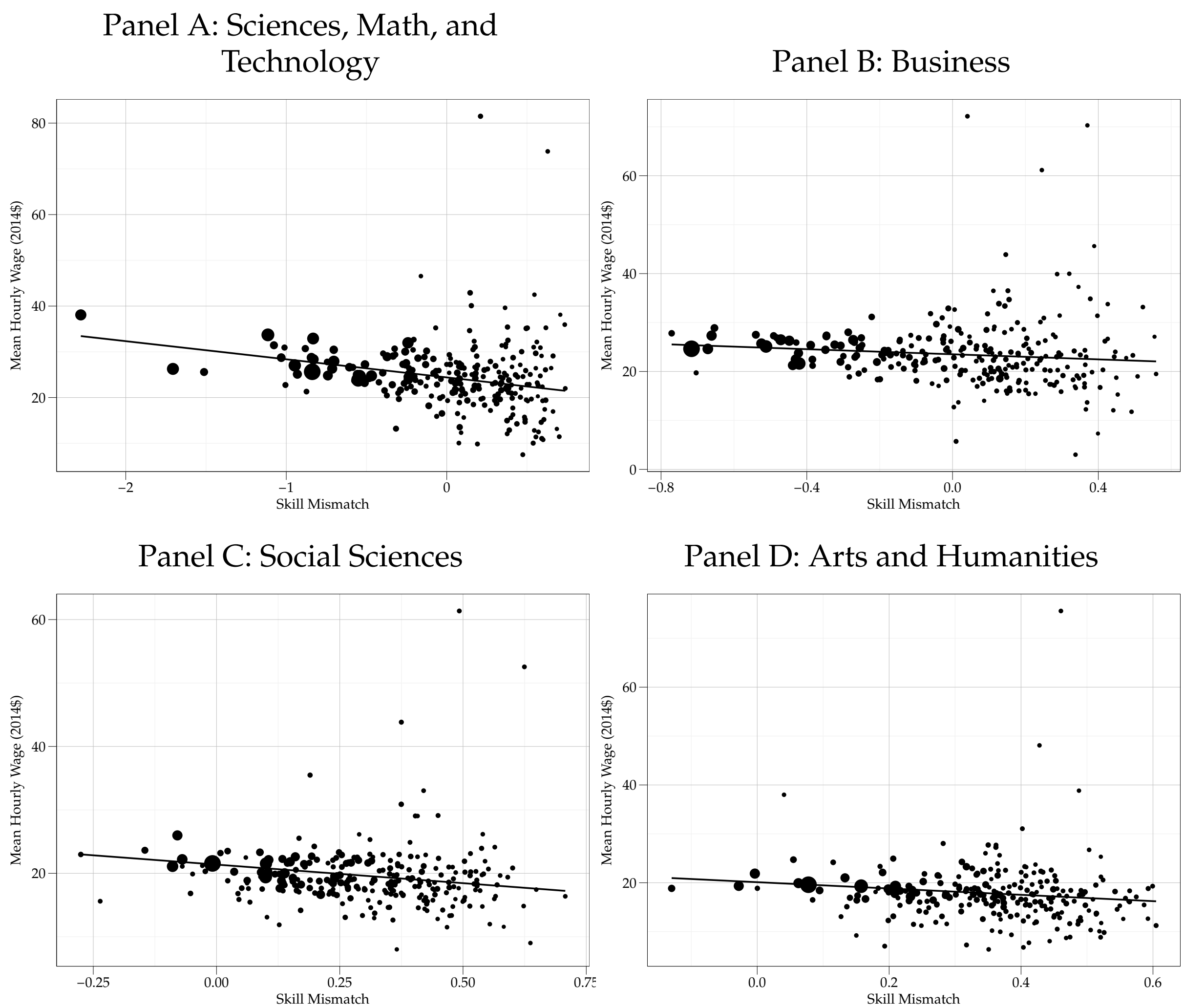
Figure C.4. Average Skill Mismatch vs. Unemployment Rates Across MSAs, 2016



Notes: Each dot represents average skill mismatch (*x*-axis) and the unemployment rate (*y*-axis) for a specific college major group (panel) and a specific MSA. Unemployment rates at the MSA level are based on recent college graduates aged 22 to 31 with a Bachelor's or Master's degree. The size of each dot is proportional to the number of recent college graduates holding a major belonging to the corresponding major group and living in the corresponding MSA, and the solid line represents the slope from a weighted linear regression.

Source: American Community Survey, Burning Glass Technologies.

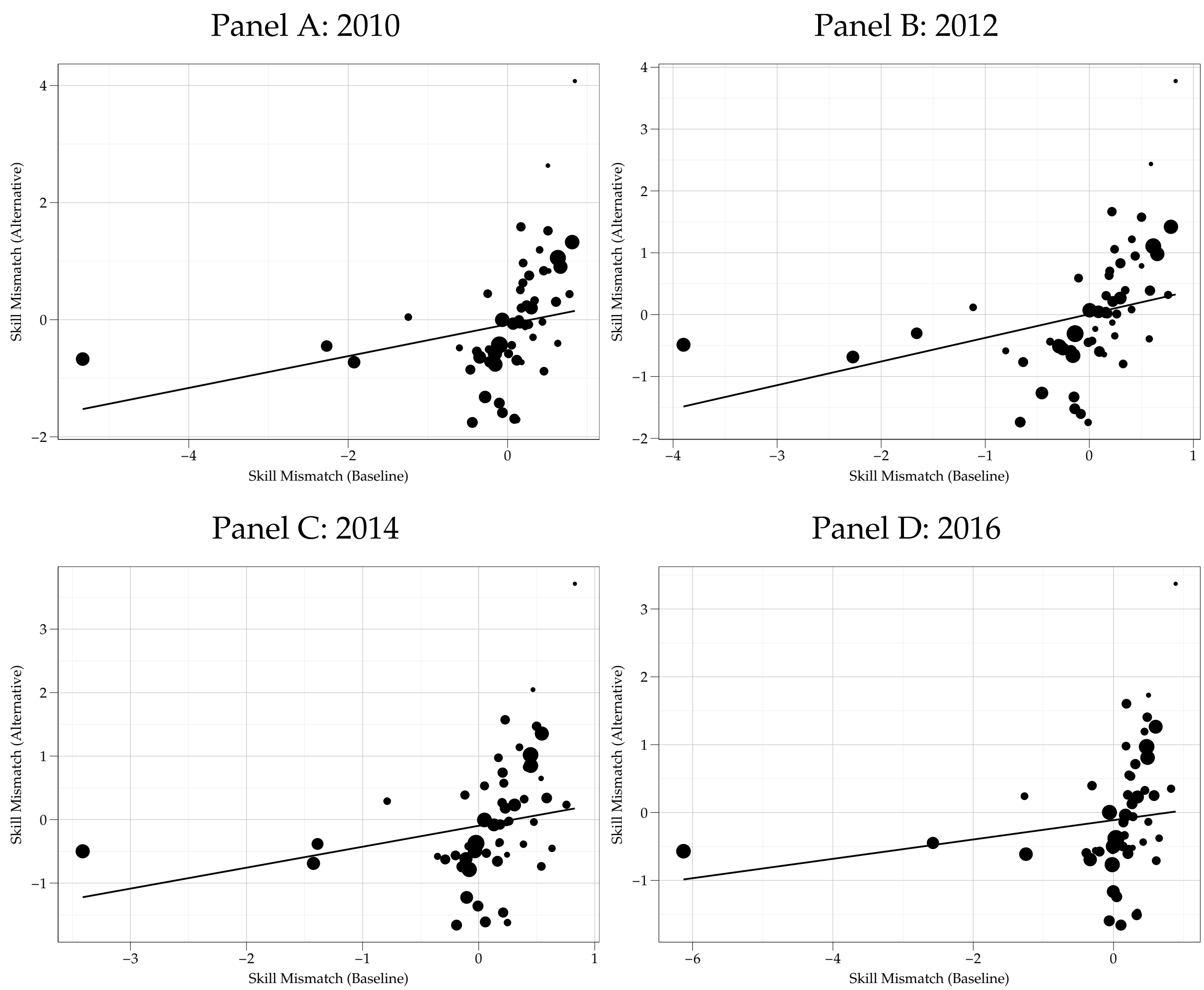
Figure C.5. Average Skill Mismatch vs. Mean Hourly Wages Across MSAs, 2016



Notes: Each dot represents average skill mismatch (x-axis) and the mean hourly wage (y-axis) for a specific college major group (panel) and a specific MSA. Mean hourly wages at the MSA level are based on recent college graduates aged 22 to 31 with a Bachelor's or Master's degree, earning a positive wage, and not attending school. The size of each dot is proportional to the number of recent college graduates holding a major belonging to the corresponding major group and living in the corresponding MSA, and the solid line represents the slope from a weighted linear regression.

Source: American Community Survey, Burning Glass Technologies.

Figure C.6. Average Skill Mismatch by College Major: Baseline vs. Alternative Definition, 2010-2016



Notes: Each dot represents average skill mismatch for one of the 56 college majors, separately by year. The baseline definition of skill mismatch (x-axis) uses employment shares (3.2) to assess the match between college majors and occupations, while the alternative definition of skill mismatch (y-axis) uses college major wage premiums (3.7) instead. The size of each dot is proportional to the number of recent college graduates holding that major nationally, and the solid line represents the slope from a weighted linear regression.

Source: American Community Survey, Burning Glass Technologies.