Relative Education and the Advantage of a College Degree

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Abstract

What is the worth of a college degree when higher education expands? The relative education hypothesis posits that when college degrees are rare, individuals with more education have less competition to enter highly skilled occupations. When college degrees are more common, there may not be enough highly skilled jobs to go around; some college-educated workers lose out to others and are pushed into less skilled jobs. Using new measurements of occupation-level verbal, quantitative, analytic skills, this study tests the changing effect of education on skill utilization across 70 years of birth cohorts from 1971-2010, net of all other age, period, and cohort trends. Higher education expansion erodes the value of a college degree, and college-educated workers are at greater risk for underemployment in less cognitively demanding occupations. This raises questions about the sources of rising income inequality, skill utilization across the working life course, occupational sex segregation, and how the returns to education have changed across different life domains.

Relative Education and the Advantage of a College Degree

Over the past eighty years, the United States experienced a dramatic increase in college enrollment and completion. Only 7.7% of Americans aged 25-29 had a bachelor's degree or more in 1950, but this number tripled to 22.5% in 1980, and further increased to 31.7% by 2010 (Snyder, de Brey and Dillow 2018). With increased state support for universities during the 20th century (Fischer and Hout 2006), the increasing cultural value that individuals and employers attach to college education (Baker 2011), the diffusion of educational expectations across class lines (Goyette 2008), and visible economic returns to education stimulating university attendance (Goldin and Katz 2008), college is increasingly an institutionalized part of the life course (Rosenbaum 2001). Furthermore, the advantages of college appear to be strengthening, as the monetary returns to education increased dramatically in the last part of the 20th century (Autor 2014; Fischer and Hout 2006; Goldin and Katz 2008). With a strong empirical case that “education makes life better”—both directly and by leading to better jobs (Hout 2012: 394; Kalleberg 2011)—researchers typically treat increases in college education as a positive outcome.

The positive news about education has largely displaced older concerns that the expansion of college education might not have been paralleled by an increased demand for skilled labor, leading to an erosion of the competitive advantage of a college degree (Berg 1970; Bills 2016; Brown 1995; Collins 1979; Freeman 1976; Hirsch 1976; Smith 1986). Indeed, several decades ago, scholars were concerned about the possibility of *relative education effects*: When college degrees are rare, individuals with more education have less competition to enter highly skilled occupations. When college degrees are more common, there may not be enough skilled jobs to go around—some college-educated workers may lose out to others and be pushed

into low-skilled jobs instead. Many scholars dismiss the relative education hypothesis in light of research on wage differentials, but this may be premature. The relative education argument applies to *skill utilization* in the labor market (Hirsch 1976), and only indirectly to the wage premium. Wages are a separate outcome subject to institutional as well as market forces, such as returns to a specific occupation or industry, union power, and favorable regulatory environments for high-income workers (Cappelli 2015; Jacobs and Myers 2014; Jacobs and Dirlam 2016; Kalleberg 2011; Lin and Tomaskovic-Devey 2013; Tomaskovic-Devey and Lin 2011).

This study combines 40 years of the Current Population Survey data and new measures of cognitive skill utilization to re-examine the relative education hypothesis (Flood et al. 2015). I begin with a brief overview of differing perspectives on recent trends in returns to a college education, explain why wage premiums are not a good proxy for skill utilization, and develop new measures of cognitive skill utilization at work based on an analysis of O*NET data (see also: Liu and Grusky 2013). I then test the relative education hypothesis with these direct measures of skill utilization—net of all age, period, and cohort trends that could confound the hypothesis. I interpret the results graphically and in substantive terms, addressing the extent to which rising educational attainment across birth cohorts pushes college-educated individuals into less skilled jobs. The findings provide new directions for the study of labor market mismatch, occupational sex segregation, and labor market careers across the life course. The study also calls into question the prevailing explanations for rising income inequality, how the returns to education have changed across different life domains, and whether sending more students to college is a viable strategy to reduce inequality.

The Relative Education Hypothesis: A Brief Synopsis

The heart of this study is a test of relative education effects. Relative education theory can be summarized by the following points: (1) all else equal, for an individual to obtain a scarce resource such as a good job, s/he needs to have more education than other potential applicants; (2) as more individuals attend college in order to obtain better jobs and higher social status, these educational credentials no longer help employers narrow down their job searches. Since a “crowd” of individuals competes for the same position, employers raise the educational requirements to screen for better-qualified applicants, and sometimes there are not enough jobs to absorb all of the well-educated workers in the population. As a result, (3) the advantage a college degree confers to an individual depends on the educational level of his or her peers. A person who earns a college degree in the context of a less-educated labor force may be seen as highly desirable, while that same person in a highly-educated labor pool is seen as minimally qualified rather than elite.

Ultimately, the value of a college degree is not absolute, but *relative* to the amount of education held by peers and depreciates as it becomes more common in the labor market unless skilled jobs increase at a similar or greater rate. With the dramatic increase in educational attainment over successive birth cohorts, if all else is equal we would expect the value of a college degree to drop. Put another way, we may be witnessing degree inflation in the United States, where each individual college degree is less effective at helping a worker obtain skilled employment. If this is correct, individuals with college degrees born in later cohorts are less likely to take jobs that utilize the verbal, quantitative, and analytic skills they develop in school.

Education as Positional Good

Educational attainment signals to employers that a job seeker is desirable (Spence 1973). Screening workers for a minimum level of educational attainment is a low-cost strategy for

removing undesirable candidates that complies with anti-discrimination regulations, while exceptionally well-educated workers may be flagged for further review. But while education represents acquisition of skills, it is also a positional good which means that at least some of its value is relative—whether you have more or less of it than your competition. Hirsch (1976) provides the most vivid explanation of how positional goods work with the metaphor of the job queue (see also: Reskin and Roos 2009; Thurow 1976). Imagine that you are in a line for a job in your town, where all job candidates are ordered from most qualified to least qualified; if you gain more education, you will probably move forward in the line and are more likely to land the job. However, if you move to a new metropolitan area that has better-educated job candidates than your previous town, you will find yourself further back in line than before. Without any change in your own absolute educational attainment, your degree's power in the labor market has lost value because a greater proportion of applicants are ahead of you in the labor queue. In economies like the United States, relative position in the queue is usually more relevant to landing the job than the absolute value provided by the educational training (Di Stasio 2017; Goldthorpe 2014).

Relative education theorists disagree about what signals college completion sends to potential employers. Those working within a broad human capital tradition emphasize that education signals productive capacity in the workplace. Workers learn skills from a variety of sources, but academic college skills are particularly prized by employers and society as a whole (Baker 2011). Furthermore, in the absence of concrete knowledge about an applicant's future productivity, education completion may signal the capacity to learn new tasks (Spence 1973).

In contrast, the credentialist strain of relative education theory suggests that the relationship between education, skills, and hiring is spurious: Workplace skills are often learned

through on-the-job training. Instead, credentialist theorists suggest that employers prefer college-educated workers for two related cultural reasons. First, employers are looking for workers with high levels of cultural capital, and educational attainment signals mastery of dominant American cultural norms (Collins 1979). Thus, when firms look for employees, they believe that college-educated workers have greater work ethic and more poised presentation of self; in other words, they seek workers whose cultural training and background matches their own (Berg 1970; Brown 2001). Second, by setting an artificial educational bar for new hires, workplaces announce their own prestige and status by excluding those with lower social standing and reinforcing its own elite cultural position (Collins 1979).

In both strands of relative education theory, workers who earn a degree are able to better signal their desirable qualities to potential employers. The signal helps applicants distinguish themselves from others: It moves them further towards the front of the labor market queue. Therefore, a young adult with sufficient resources and ability will usually stay in school until they reach a desirable position in the labor market queue to advance his or her career (Goldthorpe 2014; Thurow 1976; Van de Werfhorst and Andersen 2005).

The Dynamics and Consequences of Degree Inflation

As young adults and their families increasingly realize that education is an effective path towards economic advancement, financial aid and the university system expands, and high schools adopt policies encouraging all students to attend college, the proportion of individuals attending college rapidly increases over birth cohorts (Goldin and Katz 2008; Goyette 2008; Rosenbaum 2001). Thus, over time, to maintain the desired position in the labor market a prospective worker in later cohorts must distinguish themselves with more education, creating a feedback loop where each individual needs to obtain more education to protect against others

leapfrogging them in the labor market queue (Collins 1979; Collins 2002; Freeman 1976; Hirsch 1976; Thurow 1976; Van de Werfhorst and Andersen 2005).

The expansion of higher education has a number of effects on the labor market. From the human capital perspective, college education increases an individual's skills¹, but mass education increases the number of skilled individuals, making it harder for each degree-holder to stand out² (Smith 1986). From the credentialist perspective, if everyone has the same cultural qualifications, employers insist on higher-prestige or more advanced degrees to maintain the same selectivity (Collins 1979; Collins 2002). Furthermore, just as the economy cannot absorb particularly large birth cohorts (Easterlin 1987; Pampel and Peters 1995; Slack and Jensen 2008), it may not be able to absorb larger numbers of college-educated individuals in the contemporary labor market. But from all perspectives, adding a large number of educated individuals to the queue without a comparable increase in the number of jobs means larger numbers of individuals are unable to turn their college degree into skilled work; these individuals are said to be

¹ Human capital theorists in this tradition tend to emphasize "hard" skills such as verbal, mathematic, and analytic skills over "soft" skills such as interpersonal demeanor.

² It is possible that the human capital approach pays too much attention to the advantages conferred by college and not enough on its relationship with latent traits, or on heterogeneity within colleges themselves. In a draft of this study, one reviewer suggested that latent personality traits or cognitive abilities are activated and certified by college education. In other words, there is an interaction between "good latent traits" (GLTs) and college education, and increasing the number of college-bound undergraduates may mean a larger number of college-educated job seekers with worse cognitive skills or personality traits. It is important to note that this is only true if GLTs are fixed as a proportion of the population and not modifiable by the college environment, and if GLTs are unrelated to college completion.

Similarly, another reviewer suggested that perhaps the explosion of lower-tier college options led to college graduates with fewer skills. It is likely that there is greater heterogeneity in college-educated workers today, but it is unlikely that they would explain a divergence between skill utilization and income.

Under either scenario, college-educated job seekers would increasingly find worse jobs and have lower incomes because the population of college-educated job seekers has become less desirable. GLTs and college preparation likely have similar effects on obtaining high-skilled work and income; this is because high-skilled work is a major determinant of income (Cappelli 2015) and because traits and college preparation should both be rewarded in workplace contexts. If GLTs or college preparation had an effect on income but not occupation, this would result in rising within-occupation inequality, which has not increased steadily over time (Mouw and Kalleberg 2010). However, future research should be conducted to investigate on increasing heterogeneity among college graduates, and whether it could interact with the relative education levels.

“overeducated” or “underemployed by skill underutilization” (Burris 1983: 454; Clogg 1979: 9).

Thus, academic degrees are subject to inflation in the labor market: Just as an oversupply of money makes each dollar less valuable, an excess of degrees makes each one less important (Collins 2002). Or, as Hirsch (1976: 5) says, “If everyone stands on tiptoe, no one sees better.”

Research on skill underutilization suggests that it is a special type of labor market mismatch, in which a worker has a poor fit with their job (Handel 2003). Just as an individual may struggle to find a job close to home, or to find a job that provides an appropriate number of work hours, obtaining a position that does not fully utilize a worker’s collegiate skills is a poor outcome (Kalleberg 2008). At the individual level, low-skilled work often results in worse extrinsic and intrinsic rewards for the employee (Kalleberg 2011). Educational mismatch at work also has a scarring effect on employees, so that skill underutilization early in a career hurts subsequent attempts to earn later jobs (Pedulla 2016). Finally, a nation’s economy cannot take advantage of the skills of its workers, keeping the nation below full working capacity and rendering educational training unnecessary (Clogg 1979; Hirsch 1976).

There is some recent evidence suggesting that the relative education theory is correct, and that college degrees have lost power in the labor market. Overeducation has increased dramatically since 1972, with individuals steadily taking jobs for which they are overqualified (Vaisey 2006). Bol (2015) finds that increases in education have eroded the absolute earning power of college degrees in industrialized nations, Boylan (1993) finds that college-educated workers displace those with a high school diploma from lower-skilled jobs, and Bernstein (2007) argues that sex work has become an avenue for well-educated women to supplement incomes. Finally, newspaper columnists and trade groups show numerous cases where companies increasingly hire workers with college degrees, even for jobs that should not require one

(Burning Glass 2014; Rampell 2013). However, the most extensive evidence for the relative education hypothesis in the United States is approximately forty years old (Berg 1970; Collins 1979; Freeman 1976; but see Brown 1995), with recent research arguing against relative education effects in the United States (Goldin and Katz 2008; Hout 2012).

The Monetary Returns to Education

Relative education theory is a simple, theoretically robust account of the effects of educational expansion on obtaining skilled work. However, there is a major set of contrary findings: While relative education theory suggests that the value of a college degree is on the decline, evidence shows that the dollar return to college degree has *increased* over the past few decades in the United States (Goldin and Katz 2008). With few exceptions, sociologists largely agree that this evidence refutes relative education theory as it applies to the United States³ (e.g., Baker 2011; Handel 2003; Hout 2012; Pfeffer and Hertel 2015; Rauscher 2015; see Collins 2002 for a dissenting view).

The primary theory and evidence against the relative education hypothesis in the United States is “skill-biased technological change” (SBTC), and consists of the following argument: Growth in educational attainment in the United States led to major technological advances and business productivity starting in the late 20th century (Goldin and Katz 2008; Liu and Grusky 2013; see also Rauscher 2015). The sharp increase in the demand for college-educated workers allegedly outstripped the supply of college graduates, leading to a skills shortage in the United States labor market (Goldin and Katz 2008; but see Cappelli 2015). The evidence for the

³ Many of these assertions have not been subtle. Baker (2011: 10) writes: “[T]he notion of credentialism should be laid to rest so that broader hypotheses about widespread educational credentialing in postindustrial society can be considered.” In a similar vein, Hout (2012: 391) argues that: “As evidence of a true causal effect of education on pay accumulates, the discussions of credentialing, training robbery, and overeducation become irrelevant.”

hypothesized skills shortage is demonstrated by the increased monetary return to education, which indicates there are not enough skilled workers to fill all of the job positions.

Recent research suggests revisions to the SBTC thesis but does not contradict it. Liu and Grusky (2013) demonstrate that the increasing dollar return to education is driven mostly by the increasing return to occupation-level analytic skill; net of skill, education provides only a slightly greater advantage in 2010 as it did in 1985. This finding echoes Mouw and Kalleberg (2010), who find that wage inequality is disproportionately driven by changes to a few occupations, such as managers and computer systems analysts, and that the effect of education has not changed since 1992. Many occupations that supposedly have a shortage of skilled workers—such as STEM fields—have seen no income gains at all (Teitelbaum 2014).

However, there is a flaw in skill-biased technological change theory: It is only valid if changes in wages are primarily due to changes in supply and demand for skills. This is a major problem because increasing income polarization by occupation is due to a large host of factors beyond increased demand for skilled workers (Boylan 1993; Cappelli 2015; Kalleberg 2011). Starting in the 1980s, neoliberal policies favored higher-paid occupations, unions became less powerful and able to set wages, and the white-collar financial industry began to capture an increasing share of America's income (Jacobs and Myers 2014; Jacobs and Dirlam 2016; Kristal and Cohen 2016; Lin and Tomaskovic-Devey 2013; Lin 2015; Tomaskovic-Devey and Lin 2011; Volscho and Kelly 2012). If the United States has shifted towards a winner-take-all economy—where income is highly polarized between good and bad paying jobs (Hacker and Pierson 2010)—being on top of the occupational pyramid is far more lucrative than in the past, and educated workers would stand to benefit⁴.

⁴ Furthermore, the growth of neoliberal policy in the United States explains why the relative education hypothesis holds in the developed world overall, but not in the United States (Bol 2015).

If relative education theory is correct, then individuals with college degrees are increasingly being shuffled into lower-skill jobs (Hirsch 1976). However, the dramatic increase in monetary returns to certain occupations means that those who do obtain high-skill jobs are compensated more than ever before (Liu and Grusky 2013; Mouw and Kalleberg 2010). And because individuals with greater educational credentials often land jobs closer to the top of the occupational hierarchy, the dollar returns to education may rise even as a smaller proportion of college-educated individuals benefit (Boylan 1993; Smith 1986). This logic does not assume that wages are a proxy for demand, as Goldin and Katz (2008) imply: The relationship between the demand for skilled workers and salaries is not only imperfect and limited, but also varies over time. Although we do not yet know for certain, it is therefore possible that the wage premium is increasing *despite* a glut of college educated workers.

Furthermore, it is important to note that in the present United States economy, a failure to land at the top of the occupational hierarchy poses major problems for workers. Many middle-class occupations have shrunk in the United States due to technological advances, offshoring, and economic restructuring (Kalleberg 2011). The result is that in the years following World War II, college-educated workers who failed to translate their degrees into high-skilled work may have been able to find middle-class jobs anyway. With stagnant and declining middle-class occupational growth and rapid expansions of the low-skilled service sector, college-educated workers may slide far down the occupational hierarchy in the present economic circumstances.

While studies of the educational wage premium are important, they cannot disprove the relative education hypothesis because wages and salaries are governed by institutional forces beyond labor queuing (Hirsch 1976; see also: Smith 1986). If analysts wish to evaluate the relative education hypothesis, they should investigate whether college educated individuals are

increasingly likely to take lower-skilled jobs. In particular, we would expect changes in verbal, quantitative, and analytic skill utilization at work, since college directly improves these academic skills and are prized in the job market (Baker 2011).

Formulating Hypotheses

Both SBTC and relative education theory argue that the supply of educated workers and the growth of high-skilled jobs change at different rates. Under the SBTC hypothesis, a shortage of skilled workers would occur as more technological change and economic growth—which is often driven by increasing rates of education—generates a boom in high-skilled jobs (Acemoglu and Autor 2012; Goldin and Katz 2008; Rauscher 2015). With more available high-skilled jobs, college degree holders will increasingly find it easier to find jobs that require more verbal, quantitative, and analytic skills. This means *as the proportion of college-educated individuals rises, individual degree-holders will end up in jobs that require more cognitive skills*, and the relationship between education and skill utilization will strengthen.

Alternatively, if relative education theory is correct higher education expansion leads to a greater amount of new college graduates than new highly-skilled jobs, and other factors that lead to the growth of high-skilled jobs do not make up the difference. Because the rate of college graduates increases faster than the rate of skilled jobs, there is more competition for each skilled job; with increased competition, it is harder to obtain highly-skilled work and the relationship between education and finding skilled work should weaken. Therefore, we would expect: *As the proportion of college-educated individuals increases across birth cohorts, individuals with college degrees will end up in jobs that require fewer cognitive skills such as verbal, quantitative, and analytic reasoning capabilities.*

The result of this process is twofold. First, we would expect individuals with college degrees to work in less cognitively demanding jobs when more of their peers have graduated from college. Second, we would expect that as college-educated individuals work less cognitively demanding jobs, that the gap in skill-utilization between college and non-college educated workers narrows. As a result, we would expect the “advantage” of a college degree to decline in both absolute and relative terms; college graduates should find less skilled work compared to previous eras, and college graduates will have less of an advantage over non-college graduates in obtaining skilled work.

Accounting for Different Temporal and Sex-Based Trends

Researchers studying changes in the relationship between education and work should be especially sensitive to two analytic issues. First, researchers should remove potential sources of spuriousness across age, period, and cohort (Clogg 1979; Glenn 2003). Second, researchers should analyze men and women separately to account for changing male-female gaps in education and occupational participation since 1960 (e.g., Buchmann and DiPrete 2006; England and Li 2006; Reskin and Roos 2009; Tomaskovic-Devey et al. 2006).

The Advantages of an Age-Period-Cohort Design

Because individuals who enter the labor force primarily compete against people a few years earlier and later than them, cohort-level education should increase competition and make it harder to obtain skilled work (Easterlin 1987; Freeman 1976; Pampel and Peters 1995; Tenn 2005); because employment outcomes at the start of the career also affect later work trajectories, especially tight labor markets and widespread underemployment may scar entire cohorts of workers (Elder 1999; Pedulla 2016). However, other trends across birth cohorts could bias the results and should be accounted for, such as the effects of cohort size on employment

opportunities (Easterlin 1987; Slack and Jensen 2008). Because age, period, and cohort are perfectly correlated with each other⁵, *any* time-varying trend that affects changes in skill-utilization and is also correlated with educational attainment could lead to spurious results (Clogg 1979; Glenn 2003). Normally, age-period-cohort (APC) models are used to disentangle these effects; these models can be repurposed to partial out spurious age, period, and cohort effects that otherwise may confound the relationship between education and skill utilization.

What are some of the age, period, and cohort changes that could occur at the same time as changes in educational attainment and skill utilization? First, there are a number of institutional changes to workplaces and the labor market that occurred between 1970 and 2010 that affected population-level skill utilization. These include the increasing tendency for companies to lay off unwanted workers during robust economies, employer reluctance to retrain workers, and offshoring jobs to other countries (Cappelli 2015; Kalleberg 2011). These changes, along with macro-level economic booms and busts, have a potential relationship with the growth and loss of certain occupations and therefore affects the total amount of skill utilization (Clogg 1979; McKee-Ryan and Harvey 2011). Institutional and demographic changes also may exert a cohort effect on skill-utilization. Workers entering labor markets in the mid-20th century benefitted from internal labor markets while later cohorts did not, the especially large Baby Boom cohorts experienced increased labor market competition for jobs (Easterlin 1987; Slack and Jensen 2008), and curricular changes in K-12 school systems led to different skill sets in later cohorts (Cappelli 2001; Schoenfeld 2007; Slack and Jensen 2008; Thornton 2008). Finally, there should be substantial relationships between age, cohort-level education, and skill utilization; later cohorts of workers are simultaneously the most likely to get a college degree,

⁵ Period minus Age always equals Cohort, meaning that they form a perfect linear relationship and will always be correlated with each other.

but also the youngest and least likely to have advanced far in their career (Clogg 1979). This gives them less time to accumulate human capital at work, or to advance into mid-career jobs with higher skill requirements. Age-Period-Cohort models are ideal in this case because they can simultaneously partial out the variance associated with birth cohort size, macro-economic booms and busts, and any other time-varying trend that could bias the results.

In addition, age-period-cohort models can accommodate the changing effects of race, marital status, and region on skill utilization that would otherwise bias the present findings. For example, we would expect that the passage of anti-discrimination laws and their implementation in the 20th century generated cohort differences in the return to education by race (del Río and Alonso-Villar 2015; Dobbin 2009). However, if employer preferences have not become more egalitarian over time (see: Quillian et al. 2017), then we should expect more African-American and Latino workers in under-skilled jobs. Employment outcomes also differ substantively by region and the type of metro area, partially because certain regions have more vibrant economies but also because cost-of-living differences lead to differences in wages (Bishop, Formby and Thistle 1992; McLaughlin and Perman 1991). Furthermore, it is also important to remove variation associated with regional economies that change over time, such as the flight of manufacturing jobs (and their associated skills) to developing countries or other parts of the United States. The Age-Period-Cohort design accommodates these compositional shifts.

Accounting for Sex-Specific Trends

Additionally, any observed change in the relationship between education and labor market outcomes could be due to women's increased prominence in higher education institutions and the labor force. Sex stratification in relation to higher education and the workplace happens at several distinct points in the life course including differential access to higher education,

different educational and social experiences on campus, and differentiated economic returns to college (Jacobs 1996). Moreover, the sex differentials in occupational attainment and the broad economic benefits of a college education have changed markedly over the past 50 years (e.g., Buchmann and DiPrete 2006; England and Li 2006; Tomaskovic-Devey et al. 2006). Put another way, women's educational and labor market experience is different than men's, and the relationship between women's and men's educational and labor market experiences have evolved over the past 50 years in different ways (Cotter, Hermsen and Vanneman 2001; Reskin 1993; Reskin and Roos 2009; Reskin, McBrier and Kmec 1999). Separate models by sex are therefore necessary to capture men and women's positions in labor market queues, and how they change over time.

One major change in sex stratification over the prior fifty years is the dramatic increase in women attending and graduating from college. While American college campuses were formerly dominated by men, there is currently a slight edge in women's college completion rates (Buchmann and DiPrete 2006). However, women are still less likely to apply to and attend elite academic institutions (Bielby et al. 2014; Jacobs 1996; Turley, Santos and Ceja 2007). Furthermore, mass expansion of college education led more women from disadvantaged families to apply to college, but men from those families are less likely to attend college (Buchmann and DiPrete 2006). Thus, women are more likely to go to college in later birth cohorts, but they may also increasingly suffer from disadvantages in family background or institutional prestige.

When women do attend college, they historically majored in female-typed disciplines such as nursing and education. These majors provide less analytic and quantitative skill development, but more verbal skill development (see: England and Li 2006 for examples). Although differences have not been erased, women today are more likely than before to major in

male-typed fields such as business, life sciences, and mathematics (Jacobs 1996; but see England and Li 2006; Mann and DiPrete 2013), and have also made progress entering male-dominated, high-skilled workplaces (Baunach 2002; Queneau 2006; Tomaskovic-Devey et al. 2006).

Because of these factors, there are three major reasons why male and female trajectories should differ. First, college-educated women should have greater access to high-status jobs today than in the 1970s, which opens up more opportunities to use college degrees in skilled occupations. Second, the increased number of women entering high-skilled work means that highly-educated men increasingly have more job market competition from both educated men *and* women. Thus, women should have more opportunities today than in the past to partially counteract rising competition, while men face a steeper increase in labor market competition as women enter male-typed jobs; these should lead to a greater decline in skill utilization among college-educated men than for women. Third, the demand for female-typed occupations has grown dramatically at the same time that growth in male-typed occupations plateaued (Cotter, Hermesen and Vanneman 2001; Kalleberg 2011); as a result, women should have less difficulty obtaining work than men as college education rates rise. This trend may be exacerbated by increasing feminization of previously male-typed working class occupations, which limits the fallback options for college-educated men who cannot find skilled work (Reskin and Roos 2009).

Methods

The present study asks whether individuals from better-educated cohorts are pushed into less-skilled work by increasing cohort competition. In contrast to research that uses wages as a proxy for skill utilization, I develop occupational skill ratings using O*NET data alongside a measure of substantive significance to interpret the effect of education on skill utilization as the higher education system expands. Using the skill ratings and the Annual Social and Economic

Supplement from 1971-2010, I then examine how an individual's educational level affects skill utilization when education rises in their birth cohort and region net of age, period, cohort, and other demographic trends.

Data

The Annual Social and Economic Supplement (ASEC)—conducted as part of the Current Population Survey (CPS) every March—is an ideal data set for this analysis (Flood et al. 2015). The ASEC includes a large number of demographic and occupational variables, repeats them across a long period of time in a comparable way, and is large enough to produce stable results in an APC analysis⁶. The variables in this analysis are all consistently available from 1971 to the present, and thus I use all ASEC data between 1971 and 2010. I exclude part-time workers who prefer part-time work to a full-time job⁷. Individuals who choose part-time work may not necessarily be getting less of a return on their education, in contrast to those who have part-time work because they cannot obtain full-time employment. I also limit the analysis to individuals older than 25, since 4-year degree completion usually occurs by then, and exclude individuals older than 62 because they may move into retirement jobs that are substantially different from their earlier career.

The detailed occupational codes in ASEC are especially important to this analysis because they permit linking each employed individual to his or her likely job tasks. Until 2003,

⁶ APC models remove time trends associated with aging, cohort change, and historical changes, all of which could be correlated with rising levels of educational attainment. But because age, period, and cohort together form a perfect linear combination, additional constraints are necessary to identify the model. When coupled with a large number of dummy variables for period and cohort—which, depending on the constraints applied, can number between 18 and 100 before any interactions or other variables are included —APC models become very unstable in smaller samples.

⁷ In the Current Population Survey, all participants who worked part-time were asked why they worked part-time. If an individual who wants full-time work cannot get it then they are getting a lower return on their degree, and thus are comparable to full-time workers. However, many workers stated that they actually preferred a part-time job, which would mean that obtaining a part-time job would actually be a labor market reward in itself. Since this represents a qualitatively different set of job seeking behavior, I exclude these participants from the analysis.

the US Department of Labor used trained, independent raters to assess the skill levels of all occupation categories listed by the US Census, published in either the Dictionary of Occupational Titles or the Department of Labor's O*NET database (Boese et al. 2001). With multiple measurements for verbal, quantitative, and analytic skills, the Department of Labor's skill utilization ratings are the most comprehensive database of occupational skills in the United States. They are therefore the best option for large-scale, quantitative analyses of occupational skill in both economics and sociology (see: Autor and Dorn 2013; Liu and Grusky 2013).

I construct the dependent variable with confirmatory factor analysis to predict verbal, quantitative, and analytic skill factor scores for each occupation (see also: Liu and Grusky 2013; Yu and Kuo 2017). The indicators come from the 2003 O*NET database; I follow Boese et al's (2001) recommendation and use a one-year database instead of multiple years; in any case, prior analyses show very little change in skill ratings over time (Liu and Grusky 2013). With little change in skill, the findings in this study represent changes in the amount of people entering an occupation, not changes to the jobs themselves.

I estimate factor scores for verbal skills from observed skill-ratings for oral comprehension, written comprehension, and written expression; quantitative skills from indicators for mathematics knowledge and mathematical reasoning skill; and analytic skills from measurements of on-the-job learning and complex problem solving skill. These skill ratings comprise the dependent variables for the analysis. I then use consistent occupational codes developed by Autor and Dorn (2013) and Dorn (2009) to harmonize job ratings across multiple years. As Liu and Grusky (2013) note, skill is multi-dimensional; individuals may have high levels of mathematical knowledge, but not analytical reasoning, and the same is true of jobs. Therefore, separate measures for each skill level is the best approach. More information on the

construction of these variables is located in Appendix A. The skill ratings used in this study are posted at a public research archive and also in Appendix D⁸.

To test the relative education and skill-biased technological change hypotheses, I report three sets of independent variables: Variables for cohort-level educational attainment, individual-level educational attainment, and an interaction between the two to show how the returns to education change as educational attainment increases across cohorts. I calculate cohort-specific relative education within region and time period (see also: Goldin and Katz 2008; Horowitz 2015; Tenn 2005). To reflect different levels of education across the country, I calculate the proportion of individuals with a 4-year degree by subdividing the sample into the nine different census regions, subdividing each region by five-year periods, and then calculating the number of individuals with a 4-year degree for five-year birth cohorts. This approach captures competition more accurately than an overall measure of period-based educational attainment, since people are generally in competition with people who are a few years older or younger in a specific labor market (Easterlin 1987; Freeman 1976; Horowitz 2015; Pampel and Peters 1995). All control variables are included in all models.

Figure 1 shows the proportion of individuals who have earned a college degree for each census division over cohorts. There is substantial heterogeneity in educational attainment across different census divisions, although all regions show a secular increase in educational attainment. For example, only 7.3% respondents in the 1915-1919 cohort within the East-South Central Division⁹ have a 4-year college degree, which more than triples to 28.7% by the 1980-1984 cohort. Change is even more dramatic in the Northeast Division, where the of respondents cohort

⁸ The skill scores may be accessed in Appendix D, or may be downloaded at the following link: <https://dx.doi.org/10.17605/OSF.IO/FNQV7>

⁹ This census division consists of Alabama, Kentucky, Mississippi, and Tennessee.

having a four year degree increases more than fourfold from 10.1 % in the 1915-1919 cohort to 46.8% in the 1980-1984 cohort. In general, the largest increases came along the Atlantic coast (Northeast, Mid-Atlantic) while the smallest increases came in the western portion of the United States (Pacific, Mountain West).

FIGURE 1 ABOUT HERE

I use the measure of relative education, dummy variables for educational attainment, and an interaction between the two to identify how the returns to education change as educational attainment rises. I use three dummy variables for educational attainment—individuals who have completed some college, a 4-year degree, and those who have earned more education than a four year degree, with individuals who have completed no college as the reference category. Using dummy variables is the best solution for estimating nonlinear returns to education, since in the United States degree completion has more substantial impacts on job market outcomes than years of education (Bills 2016). I then interact the proportion of individuals with a college degree with the series of dummy variables for educational level. The key independent variables, dependent variables, and additional covariates are listed in Table 1.

TABLE 1 ABOUT HERE

Analysis

I estimate six separate regression models in this study which test the effects of education on verbal, quantitative, and analytic skill utilization, separately for men and for women. The key independent variables are the cohort-, period- and census division-specific proportion of individuals with a 4-year degree, and the interaction between educational attainment and the proportion of 4-year degree holders. I also include a large number of additional covariates to partial out potentially spurious explanations. The most important covariates are variables for age,

period, and cohort. I operationalize age, period, and cohort using different time scales; I estimate age effects using linear, quadratic, and cubed functional forms, period effects using one-year intervals, and five-year cohort groupings (Yang and Land 2008). Yang and Land (2008: 301) refer to this as a “fixed effects” model, since age and period are treated as dummy variables instead of random effects. However, the term “fixed effects” model also leads to some confusion, since the term also describes an econometric regression model that only measures within-unit changes over time (Halaby 2004: 514). Thus, some researchers may prefer the term “dummy variable model,” since period and cohort are represented as dummy variables instead of random effects.

It is important to note that there is considerable disagreement over the best way to estimate APC models. To break the linear relationship between age, period, and cohort, a constraint needs to be added to the APC model, which sometimes leads to inconsistent age, period, or cohort estimates (Luo and Hodges 2016; Luo et al. 2016; Yang, Fu and Land 2004). Although the age, period, and cohort trends in this study are not the primary focus of the analysis, inaccurate estimation of temporal trends could bias the key independent variables. Thus, it is important to demonstrate that the present findings are robust to alternative models for age, period, and cohort. Appendix B presents five different APC models with different philosophical underpinnings, discuss their strengths and weaknesses, and use each to evaluate the hypotheses in the paper (Harding and Jencks 2003; Luo 2015; Riebler, Held and Rue 2012; Yang and Land 2008). Different age-period-cohort modeling strategies provide the same support for the hypotheses. While the age-period-cohort trends are not the main focus of this paper, readers may find the overall trends useful. For this purpose, I present graphs of age, period, and cohort trends in skill utilization in Appendix C.

I also include several other dummy variables and interactions to account for other potential demographic changes across cohorts: Race (white, black, Hispanic, other); metro area (central city, outside central city, not in a metro area, not identifiable); census division; and marital status (married, never married, separated/widowed/divorced). I also include race-cohort and marital status-cohort interactions. This accounts for changing social conditions that could lead to more or fewer dual-earner couples, as well as accounting for changing anti-discrimination laws and enforcement. Finally, while it is important to capture changes in relative education change across cohorts, there may be features of regional economies that are correlated with absolute educational level or historical trends. I therefore include interactions between census division and educational level, as well as between census division and time period. Thus, the full regression model is represented by the following equation (Model 1):

$$\begin{aligned}
 y = & \beta_0 + \beta_1(X_{collprop}) + \beta_2(X_{somecollege}) + \beta_3(X_{fouryeardegree}) + \beta_4(X_{morethanfouryears}) + \\
 & \beta_5(X_{somecollege \times collprop}) + \beta_6(X_{fouryeardegree \times collprop}) + \beta_7(X_{more4yrs \times collprop}) + \beta_8(X_{age}) + \\
 & \beta_9(X_{age^2}) + \beta_{10}(X_{age^3}) + \sum \delta_j (D_{year}) + \sum \delta_k (D_{cohort}) + \sum \delta_m (D_{maritalstatus}) + \sum \delta_r (D_{race}) + \\
 & \sum \delta_a (D_{metroarea}) + \sum \delta_d (D_{censusdivision}) + \sum \delta_{km} (D_{cohort \times maritalstatus}) + \sum \delta_{kr} (D_{cohort \times race}) + \\
 & \sum \delta_{de} (D_{censusdivision \times education}) + \sum \delta_{dj} (D_{censusdivision \times year}) + \varepsilon_i
 \end{aligned}$$

Where X represents specific independent variables and β represents a single coefficient, and where D represents a vector of dummy variables and δ represents a set of coefficients for each dummy variable. The findings are robust to models with fewer additional covariates, and models with fewer independent variables are available upon request. I conduct all analysis in Stata 13. To further validate the model, I also estimate changes in wages as educational levels rise, and identified similar findings to previous research: In recent birth cohorts, the dollar return to education has increased compared with less-educated individuals.

One of the benefits of this dataset is its size: With over one million men and over 750,000 women, the standard errors are unusually small: This is good, because it means that the estimates in this model are likely to be stable and close to the true population parameters. It also means that nearly every key independent variable is statistically significant at $p < 0.001$. While it is rarely appropriate to interpret statistical significance as an effect size, this is especially problematic in such a large data set (Kline 2004). A better solution is to develop standards for “substantive” or “practical” significance to interpret effect sizes (Bernard, Chakhaia and Leopold 2017).

TABLE 2 ABOUT HERE

To develop these standards, I identified the qualitative differences between the description of job responsibilities on O*NET and compared them to the quantitative differences in skill utilization in my model.¹⁰ The results of my qualitative analysis suggest that it is very difficult to identify gaps between occupations less than 0.2 points away from each other; thus, differences in skill rating less than 0.2 points are deemed not substantively meaningful. However, I was able to consistently identify qualitative differences between occupations that were 0.2-0.4 points apart in skill rating, and jobs that were 0.4-1.0 points apart in skill rating had large qualitative differences. Occupations more than one point apart in quantitative skill rating had dramatically different qualitative task responsibilities. I present a selection of occupational titles, skill scores, and qualitative descriptors of job skills in Table 2.

Findings

¹⁰ I developed these standards using the following coding scheme: First, I sorted the full list of 316 occupations by verbal skill utilization. I then selected an occupation, and downloaded the qualitative description of job responsibilities from O*NET. I then read the job descriptions for jobs that had slightly higher and lower verbal skill utilization scores, and recorded whether I noticed no difference, some difference, a large difference, or very large difference in verbal skill utilization. I continued reading job descriptions and recording the qualitative size of the skill difference between jobs until I consistently identified “very large” differences. I repeated this process several times for several different occupations, and then for quantitative and analytic skill utilization.

Table 3 presents three key sets of covariates from the fixed effects/dummy variable models. The first are the dummy variables for educational attainment, and each one represents the effect of a certain amount of college compared to not having any college at all. The second is the “Proportion of Cohort with a 4-Year Degree.” The third are the interactions between college and college proportion in a given birth cohort—these represent the relative education effect of education on skill utilization. When this coefficient is negative and the others positive, it means that the advantages of going to college decline as educational attainment rises. When the coefficient is negative *and* larger than the effect size for college proportion, individuals with college degrees are increasingly ending up in lower-skilled jobs as the proportion of college-educated rises. For example, male participants with college experience are more likely to use verbal skills at work than those without college experience, shown by the positive and statistically significant dummy variables for some college, four-year degree, and more than a 4-year degree. Meanwhile, the positive and statistically significant effect of college proportion shows that as the number of college-educated individuals increases over birth cohorts, so does the verbal skill utilization of workers with no college degree. Finally, the interaction terms between education level and college proportion are all negative; thus, while individuals with college experience use verbal skills more than those without college background, this advantage declines as the proportion of college-educated individuals increases.

TABLE 3 ABOUT HERE

The findings across all six fixed effects/dummy variable models are listed in Table 3, and are broadly similar across both men and women, as well as across all three skill domains. While individuals with college degrees utilize more skills on the job than those without any college, the interaction between educational level and college proportion is negative in nearly all cases,

indicating that individuals with college degrees lose their advantages over those without a college degree when there are relatively more of them¹¹. Additionally, in most cases the interaction between education level and college proportion is negative *and* larger than the skill level increases for all workers; generally speaking, individuals with college degrees are taking less-skilled jobs as cohort-level education rises¹². In other words, while greater education generally leads to more skill usage, this effect weakens when more people earn college degrees. This finding is consistent with the relative education hypothesis, and inconsistent with skill-biased technological change.

There are two ways to interpret the effect of education on skill utilization: An “absolute” interpretation and a “contextual” or “gap” interpretation. To make each interpretation clearer, I show how the relationship between education and skill utilization depends on the proportion of college degree-holders for a hypothetical job occupant, holding all other variables constant. I assume that the hypothetical individual is 27 years old; this is because the effects of an overcrowded labor market should be most apparent as individuals are transitioning into the labor force. I also assume the individual lives in 1999 within a central city in the Mid-Atlantic census division and is white and married. Changing these characteristics alters the overall predicted skill levels by shifting the intercept up or down, but not the interaction between individual educational attainment and relative education levels, which the primary concern. The only independent variable that changes is the proportion of college educated individuals, which is defined from 8% to 40% since this is the approximate range in the present data¹³. Individuals who belong to

¹¹ The only exception is that men with more than a four-year degree do not use any more or less quantitative skills as college proportion increases; it is the only non-significant coefficient in Table 3.

¹² One exception is women with a four-year degree, who are using approximately the same amount of quantitative skills as in the past. This is because the education by college proportion interaction (-0.384) is approximately the same size as the coefficient for college proportion (0.398).

¹³ The full range is from 2.2% to 48.7%, but there are very few cohort-period-region combinations below 8% or above 40%.

cohorts with a low percentage of college educated individuals—about 8%--were largely born prior to 1930. Individuals who belong to cohorts with high degrees of college completion—around 40%--were almost entirely born in 1975 or later.

The “absolute” interpretation judges whether individuals with college degrees end up in less-skilled jobs as cohort competition increases. Figure 2 shows the absolute decline in skill utilization that occurs as the proportion of college-educated individuals increases; I also list the decline from 8% college-educated cohorts to 40% college-educated cohorts in Table 4, where I list guidelines on interpreting changes in skill levels. Generally speaking, most of the declines in skill utilization are noticeable but not especially large—in the range of 0.2-0.4 point declines. For example, our hypothetical male with a college degree loses 0.32 points for verbal skill utilization, and this drop in verbal skill utilization is roughly the difference between a news reporter and a librarian; between a librarian and a clergy member; or between a physical therapist and a bank teller. The decline in verbal skill utilization for the hypothetical female with a college degree is slightly smaller (-0.22 points). This is roughly the difference in verbal skill utilization from a middle school teacher to a physical therapist; from a secretary¹⁴ to a social worker; or from a social worker to a customer service representative.

FIGURE 2 ABOUT HERE

Overall, in the present simulation men with four year degrees have noticeable but moderate declines in verbal and quantitative skill utilization, while women with four year degrees have similar declines in verbal and analytic skill utilization. The largest decline is for men’s analytic skill utilization, which declines 0.45 points as the proportion of college-educated population increases from 8% to 40%. While skill differences between 0.2 and 0.4 points are

¹⁴ Secretarial positions require substantial verbal skills, despite the stigma attached to them as pink-collar jobs.

noticeable, a skill difference between 0.4 and 1.0 is substantively larger. For example, the 0.45 decline in analytic skill utilization is about the same as the decline from an actuary to a middle school teacher; from a budget analyst to a news reporter; or from a physical therapist to a clergy member. In this scenario, there is no decline in quantitative skill utilization for women with college degrees; women with four year degrees are just as likely to utilize quantitative skills in well-educated cohorts as in cohorts with less educational attainment.

It is important to note that the results in Figure 2 are due to both uneven growth across different types of occupations, as well as the skills of the college-educated population. Specifically, the decline in analytic skill utilization is larger than for verbal and quantitative skills, which suggests either too many analytically skilled workers or too few analytically-oriented jobs. Because Liu and Grusky (2013) report more growth in analytic skill usage than verbal or quantitative usage, this is likely due to an oversupply of analytic skill.

TABLE 4 ABOUT HERE

The second way of determining how much the effect of education on skill utilization has declined is a “contextual” or “gap” interpretation. This approach involves predicting the skill utilization gap between college-educated and no-college workers in low-education cohorts, and then comparing it to the same gap in high-education cohorts. The contextual interpretation therefore demonstrates how the advantage of a college degree changes as the proportion of college-educated individuals rises over birth cohort. Diminishing advantages in skill utilization can reflect either a rise in skill utilization among low-education workers, a decline in skill utilization among high-education workers, or both. As noted previously, changing the assumptions of the present scenario¹⁵ leads to larger or smaller overall gaps between college-

¹⁵ Just as before, I assume that the individual is 27 years old, the individual lives in 1999 within a central city in the Mid-Atlantic census division, and is white and married

educated and non-college educated workers; however, changes to the gap between college- and non-college-educated workers remains the same.

FIGURE 3 ABOUT HERE

In Figure 3, I graph the skill-utilization advantage under the assumption that 8% and 40% of the participants' birth cohort is college-educated, and provide the gap differences in Table 4. The first finding is that while both the hypothetical male and female college-educated worker is using fewer verbal skills at work as the proportion of college-educated individuals rises, there has also been a small increase in verbal skill utilization for individuals without college degrees. The result is that while the absolute declines in skill utilization for four-year degree holders are only moderate, the decline in the *advantage* over non-college educated workers is substantively larger. The approximate 0.5 point decline in verbal skill utilization for both men and women is equivalent to the difference between librarians and physical therapists, or from a middle school teacher to a bank teller. Similarly, while the increase in quantitative skill usage for our hypothetical non-college educated man is below 0.2, the gap between the college educated and non-college educated man declines by -0.38, which is a noticeable and moderate decline. This is approximately the difference between an accountant and an insurance underwriter, or the difference between insurance underwriters and middle school teachers¹⁶.

Finally, holding constant other factors, there is a relatively large decline in the amount of analytic tasks that college-educated workers perform as the share of college-educated individuals grows, which is combined with an increase in analytic skill usage by the non-college educated. The result is that the gap between four-year degree holders and non-college educated analytic skill usage shrinks by approximately 0.6 for both men and women. A 0.6 change in analytic skill

¹⁶ On the other hand, among women the difference in quantitative skill usage between college-educated and non-college-educated is not substantively meaningful.

is substantively large. At the lower end of the scale, it is the difference between the analytic usage of a customer service representative and a grounds maintenance worker. Among occupations with more analytic skill requirements, it is larger than the difference between occupational therapists and customer service representatives; larger than the gap between middle school teachers and secretaries; and larger than the gap between budget analysts and news reporters. In other words, as college education rises, workers with no college degree are entering occupations with more analytic skill requirements concurrently with college-educated individuals entering less analytically-oriented occupations. The result is that the advantage a college-educated individual has in analytic skill utilization declines as cohorts become better-educated.

That said, it is important to note that despite the decline in the advantage of a college degree, individuals who earn a college degree still take more skilled jobs than those with no college experience. For example, if 40% of the cohort-specific population has a college degree, men with college degrees have a 1.3 point advantage in analytic skill utilization compared to men without any college experience. This is substantively very large; it is approximately the difference between an insurance underwriter and a tool and die maker; or the difference between a librarian and an automotive mechanic. However, while the present findings indicate that a college degree still provides a large advantage in the labor market today, further increases in the rate of college completion may continue to erode the relative advantage of a college degree.

One key question is whether men and women have similar levels of skill utilization, and whether this changed at the same time as education rose in the population. The tables and figures presented here suggest that the relationship between education and skill utilization at work varies across skill domain and sex, and are consistent with sex-specific differences in college major

choice and occupational sex segregation. Figure 2 shows that men with college degrees have greater overall quantitative and analytic skill usage holding all other variables constant, while women have higher verbal skill-utilization at work. This is due to men's over-representation in STEM majors, as well as long-standing skill differences between traditional male- and female-typed occupations.

While men do utilize more quantitative and analytic skills at work than women, Figure 2 shows that college-educated men's skill utilization declines faster than women's skill utilization, net of other demographic and temporal differences. This is likely because more (1) college-educated women entered male-typed occupations and (2) there was a large increase in the demand for female-typed labor at the same time as higher education expansion; being able to join more skilled labor queues increases women's skill utilization, so that college-educated women's increased competition is partially offset by increased opportunities. Furthermore, declining occupational sex segregation creates more competition for men, shuffling even more college-educated men into less-skilled jobs and leading to a steeper decline in skill utilization. However, it is important to note that while educational expansion is likely correlated with sex desegregation in collegiate major and occupation, it is quite possible that they have separate and independent effects on sex-specific labor queues.

Discussion

In the present study, I use ASEC data and skill ratings derived from O*NET to test the relative education hypothesis, investigating whether college degree-holders increasingly find themselves in lower-skilled jobs as more of their peers attend college. The findings support the hypothesis that as educational attainment rises in the population, individuals with college degrees are increasingly shuffled into lower-skilled jobs. The hypothesis holds across verbal,

quantitative, and analytic skill utilization; in both male and female samples, and is net of all confounders from age, period, and cohort trends, as well other demographic and geographic changes to the labor force.

Previous research failed to find this effect because it tested for gaps in wages instead of directly measuring skill utilization. Prior researchers argue that because wages and demand for skills are correlated, increasing dollar returns to education means that workers are utilizing their skills at work. But the relationship between wages and skills is not constant over time, and the dollar return to education reflects both the demand for skills and institutional factors (Cappelli 2015; Hirsch 1976; Kalleberg 2011). The result is that even though more college-educated individuals are being squeezed into less-skilled work, the large gains made by workers at the top of the occupational pyramid can counterbalance and sometimes outweigh those who cannot find work appropriate for their skills. Meanwhile, college-educated individuals who cannot find skilled work have fewer palatable options. There are fewer middle-class and middle-skilled jobs today compared with low-skilled service jobs (Kalleberg 2011), meaning that college-educated workers who cannot find skilled jobs are likely to find menial service jobs instead.

There are several ways that future researchers could extend these findings. For example, positional crowding likely has the largest effects on less prestigious bachelor's degrees; alumni of schools like Harvard and Stanford continue to differentiate themselves on the job market (Rivera 2011), and expansion of less prestigious universities may produce workers who are less attractive to employers. Additionally, not all academic disciplines have grown proportionally with the expansion of the university, and it is possible that the proliferation of certain majors (such as psychology or political science) is more important than the total number of college degrees; it is theoretically possible to have an oversupply of physicists but an undersupply of

chemists. Future research should also investigate the geographical variation in underemployment by skills, since there is evidence that overeducation is often most severe in areas with universities (Davia, McGuinness and O'Connell 2017). It is likely that there is substantial heterogeneity within the college-educated workforce, and this heterogeneity could affect both occupation-level skill utilization and income. Furthermore, mismatch by university prestige, academic major, geography, and the interactions between them may lead to much greater underemployment in specific areas or within certain occupations, and may partially explain wage increases in a few occupations.

In addition to the study of labor market mismatch, this study points to four new research directions. First, this study's findings suggest that institutional factors, rather than skill-biased technological changes, are the driving factors behind wage and salary changes. There is no doubt that there has been increasing monetary returns to education to a college degree over the last few years¹⁷, with increasing income polarization between “good” and “bad” jobs (Kalleberg 2011; Mouw and Kalleberg 2010). However, the proposed mechanism for this increase is that high-skilled jobs have expanded faster than the rate of college graduates, which meant that young adults entering the labor force increasingly found more skilled work (Goldin and Katz 2008). The findings in the present study demonstrate that individuals with college degrees are *not* moving into more skilled work as overall education increases, and in fact are entering lower-skilled work instead. This means that the wage premium to education is increasing *despite* increased competition, and not because of it. This runs contrary to theories of skill-biased

¹⁷ Using the analysis described in the analysis section, I also estimated whether education's effect on income has declined or increased, and found similar patterns to those reported in other publications (e.g., Fischer and Hout 2006; Goldin and Katz 2008).

technological change, and re-opens the question of why college-educated workers suddenly began earning more money.

If college-educated workers are not in greater demand today for their cognitive skills, what caused the monetary returns to education to dramatically increase? The answer is beyond the scope of this study. However, the most likely factors are three institutional effects on income polarization, which are especially pronounced in the United States and which could counteract relative education effects on income seen in other countries (Bol 2015; Cappelli 2015; Kalleberg 2011). First, labor unions play a critical role in setting wage standards across industries and the country, and the weakness of unions has led to increased inequality as the economy has expanded (Brady, Baker and Finnigan 2013; Jacobs and Myers 2014; Kristal and Cohen 2016; Volscho and Kelly 2012; Western and Rosenfeld 2011). Second, the deregulation of the financial industry has led to dramatic income increases in the white-collar financial sector, creating large inequalities in wealth and income (Lin and Tomaskovic-Devey 2013; Lin 2015; Tomaskovic-Devey and Lin 2011; Volscho and Kelly 2012). Third, there is evidence that neoliberal policies widened income inequality, with a greater share of income going to a smaller segment of the population (Jacobs and Dirlam 2016; Volscho and Kelly 2012). None of these explanations are perfect; some research suggests that the effects of labor unions on income inequality are independent of the returns to education (e.g., Kristal and Cohen 2016; Western and Rosenfeld 2011), and the mechanism connecting conservative politics to income shares is not well-specified. However, these changes likely increase between-occupation wage inequality, and could easily explain the apparent increases in the return to analytical skill and to white-collar management (Liu and Grusky 2013; Mouw and Kalleberg 2010). Future research should directly

test for institutional mechanisms that connect occupations and wage inequality, net of decreasing skill utilization in the labor market.

Second, researchers should refine the present analyses to distinguish between skill utilization at the beginning and end of the work career, which is beyond the scope of the present study. We might expect that individuals entering a highly competitive labor market would also struggle to improve their prospects later in life (Elder 1999). It is possible that using few work skills early in the career might lead to cumulative disadvantage; since early work skills help develop future job prospects, individuals who enter low-skill jobs early in their careers may not develop the human capital to move into higher-skill jobs later in life. Alternatively, it is also possible that cohort competition is most pronounced when individuals enter the labor market and do not yet have work histories. Further research on this topic could help us understand whether overcrowded labor markets “scar” entire cohorts of workers for the remainder of their careers (Pedulla 2016), or whether the effects gradually dissipate as they obtain more experience and their labor market competition changes.

Third, the present study shows that the returns to education over time differ somewhat by sex: College-educated women show no substantive decline in quantitative skill utilization at work, and are penalized less by relative education effects in other skill domains. Based on previous research, it is likely that sex differences in the education-skill relationship are at least partially due to declining sex segregation by academic major and occupation (Jacobs 1996; Reskin and Roos 2009). The increased ability of women to move into high status professions would create more competition for men at the same time it improves women’s task responsibilities. However, the proposed explanation is not tested in this study. Future research should explicitly test for the relationship between higher education expansion, sex desegregation,

and the shape of sex- and education-specific labor queues; these processes may be causally linked, or could operate independently of each other.

Fourth, the present study shows the need to further research the returns to a college degree and occupation across different life domains. As Hermanowicz (2010) demonstrates, individuals spend an extraordinary amount of time of their lives at work, and thus our jobs impact us in a variety of different ways: Jobs help structure our social lives, affect our health, impact our happiness, and place limits on our free time (Horowitz 2016). In theory, as individuals obtain more education, we would expect individuals to obtain better jobs and therefore have better social outcomes. If individuals with college degrees are being squeezed into less desirable jobs, then we would expect that the social returns to education would also decline; for example, there is increasing evidence that the civic returns to education have dropped as more individuals earn college degrees (Campbell 2009; Horowitz 2015; Nie, Junn and Stehlik-Barry 1996). However, because education leads to less complex jobs but results in more money, the social returns to education could vary substantially by outcome. Future research is needed to disentangle the effect of education, income, and task responsibilities on outcomes such as marriage and divorce, social networks and social capital, and health. And at a more general level, sociologists should also include positional measures of education in their research: Very few sociologists believe that education is an absolute good across contexts, and yet research that looks at time-varying effects of education is rare (Shavit and Park 2016).

Finally, the current study highlights the question: Is sending more students to college a viable strategy to reduce inequality? Economists suggest that sending more individuals to college leads to concrete technological advances and economic growth (Acemoglu and Autor 2012; Goldin and Katz 2008); from this perspective sending more high school students to college is

justified. However, this study confirms that even as the average dollar return to college increases, college-educated workers are less likely to enter the highly skilled jobs that take advantage of this increased monetary return. Although the number of high-skilled jobs is not fixed, it is also not infinite and likely outstripped by the supply of well-educated adults entering the labor force.

Of course, it is still rational to encourage individual students to complete college; Hout (2012) convincingly points out sending an *individual* to college improves his or her life outcomes, and college degrees are worth a substantial amount in the job market in the present study as well. But absent a much larger increase in high-skilled work, sending *everyone* to college guarantees that more college-educated workers end up in less-skilled work, and there is no guarantee that a college degree will retain its value if enrollment continues to rise. Put another way, sending more people to college to improve their life circumstances leads to diminishing returns for occupational advancement, and is not an ideal intervention to reduce population-level inequality. Or, as Hirsch (1976: 5) argues: “If everyone stands on tiptoe, no one sees better.”

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Figure 1. Proportion of College-Educated Individuals per Birth Cohort and Census Division

Source: Annual Social and Economic Supplement of the Current Population Survey: 1971-2010 (Flood et al. 2015)

Figure 2. Predicted Changes in Absolute Skill Utilization of College-Educated Individuals as Proportion of College-Educated Rises over Birth Cohorts

Note: Predictions are for 27 year old individuals who live in 1999 within a central city in the Mid-Atlantic census division, is white and married.
 Source: Table 3

Figure 3. Predicted Gaps in Skill Level Between Four-Year Degree and No College: Differences between a Cohort with 8% College-Educated Proportion and a Cohort with 40% College-Educated Proportion.

Source: Annual Social and Economic Supplement of the Current Population Survey: 1971-2010 (Flood et al. 2015)

Findings are net of age (linear, squared, cubed), cohort (5 year dummy variables), year, marital status (married, never married, separated/widowed/divorced), race (white, black, Hispanic, other), metro area (central city, outside central city, not in metro area, not identifiable), census division, and interactions by race-cohort, marital status-cohort, census division-education, and census division-year. Predictions here are for 27 year old individuals who live in 1999 within a central city in the Mid-Atlantic census division, is white and married.

Table 1. Descriptive Statistics

Variable	Male		Female		Both Sexes	
	Mean	SD	Mean	SD	Minimum	Maximum
<i>Dependent Variables</i>						
Verbal Skill Utilization	2.36	1.16	2.28	1.29	0	6.2
Quant. Skill Utilization	3.19	1.31	2.81	1.50	0.2	6.2
Analytic Skill Utilization	2.75	1.28	2.61	1.38	0.33	6.33
<i>Independent Variables</i>						
Age	40.91	10.00	40.70	10.04	25	62
4-Year Degree Proportion	0.26	0.07	0.25	0.07	0.02	0.49
Metro Area: Not Identified	0.16	0.36	0.16	0.36	0	1
Metro Area: Not in Central City	0.23	0.42	0.24	0.43	0	1
Metro Area: Central City	0.25	0.43	0.23	0.42	0	1
Metro Area: Outside Central City	0.36	0.48	0.37	0.48	0	1
Geographic Region: Northeast	0.09	0.28	0.09	0.28	0	1
Geographic Region: Mid-Atlantic	0.13	0.34	0.13	0.34	0	1
Geographic Region: E. North Central	0.14	0.35	0.15	0.36	0	1
Geographic Region: W. North Central	0.10	0.30	0.10	0.29	0	1
Geographic Region: S. Atlantic	0.18	0.38	0.16	0.37	0	1
Geographic Region: E. South Central	0.05	0.23	0.05	0.22	0	1
Geographic Region: W. South Central	0.09	0.29	0.09	0.28	0	1
Geographic Region: Mountain West	0.09	0.29	0.10	0.30	0	1
Geographic Region: Pacific	0.12	0.32	0.13	0.33	0	1
White	0.76	0.43	0.78	0.41	0	1
Black	0.13	0.34	0.09	0.28	0	1
Hispanic	0.12	0.32	0.13	0.34	0	1
Married	0.62	0.49	0.75	0.43	0	1
Never Married	0.16	0.36	0.15	0.35	0	1
Widowed/Divorced/Separated	0.23	0.42	0.11	0.31	0	1
No College	0.63	0.48	0.65	0.48	0	1
Some College	0.11	0.31	0.10	0.29	0	1
Four Year Degree	0.18	0.38	0.16	0.37	0	1
More than Four Year Degree	0.08	0.27	0.09	0.28	0	1

Source: Annual Social and Economic Supplement of the Current Population Survey: 1971-2010 (Flood et al. 2015)

Four-Year Degree Proportion is by cohort, period, and time period.

Table 2. Sample of Common Occupations and their Skill Ratings using Confirmatory Factor Analysis

Occupation	Verbal Skill Score	Quantitative Skill Score	Analytic Skill Score	Sample Activities
Baggage Porters and Bellhops	2	1.4	1.16	Handle luggage or other possessions for patrons; Greet customers, patrons, or visitors.
Cashiers	3	3.16	1.5	Process sales or other transactions; Answer customer questions about goods or services
Tellers	3.66	3.16	1.5	Verify accuracy of financial or transactional data; Execute financial transactions; Enter information into databases
Agricultural/Food Science Technicians	2.8	2.6	1.83	Record research or operational data; Analyze chemical compounds; Test quality of materials or finished products
Grounds Maintenance Workers	1	0.6	2	Operate grounds maintenance equipment
Nursing and Home Health Aides	1.4	0.6	2	Encourage patients during therapeutic activities; maintain medical records
Carpenters	1.4	1.8	2	Review blueprints or specifications; Mark reference points on construction materials
Customer Service Representatives	3.4	2.4	2.6	Discuss goods or services information with customers or patrons; Respond to customer problems or complaints

Occupational Therapists	3.4	2.4	3.16	Analyze patient data to determine patient needs or treatment goals; Record patient medical histories
Insurance Sales Agents	3.6	2	3.33	Gather information to determine customer needs; Explain financial information to customers; Sell products or services
Insurance Underwriters	3.6	3.4	3.5	Explain regulations or procedures; Assess financial status of clients; Assess risks to business operations
Secretaries	3.8	2	3.66	Answer telephones and email to direct inquiries or provide information; Enter information into databases; Prepare documentation for reports
Librarians	4.5	2.83	3.66	Help patrons use library or archival resources; Search information sources to find specific data; Classify materials according to standard systems
Clergy	4.2	0.6	3.83	Lead classes or community events; Counsel clients or patients regarding personal issues; Develop educational programs
Social Workers	3.6	2.4	3.83	Counsel clients or patients on substance abuse, health, or interpersonal issues; Advocate for individual or community needs
News Analysts and Reporters	4.8	0.6	4	Analyze information obtained from news sources; Edit written materials; Report news to the public
Elementary/Middle School Teachers	4.2	3	4.16	Modify teaching methods or materials to accommodate student needs; Establish rules governing student behavior; Evaluate student work
Physical Therapists	4	1	4.33	Develop medical treatment plans; Record patient medical histories; Analyze patient data to determine patient needs or treatment goals

Budget Analysts	4	4.6	4.5	Analyze budgetary or accounting data; Advise others on financial matters; Verify accuracy of financial information
Accountants and Auditors	4.8	3.8	4.83	Examine financial records or processes; Analyze business or financial data; Calculate tax information
Actuaries	4.2	4.4	4.83	Manage financial activities; Develop organizational goals or objectives; Analyze data to identify trends or relationships among variables

Source: O*NET 4.0 Database (Boese et al. 2001)

Note: The skill scores are occupation-level variables representing how much each detailed occupational category utilizes cognitive skill. The scores range from range from 0 to 6.20 (for Verbal Skill Utilization), 0.20 to 6.20 (for Quantitative Skill Utilization), and from 0.33 to 6.33 (for Analytic Skill Utilization). Each score is measured using a predicted factor score from a measurement model. The observed indicators are independent skill ratings from O*NET. For more details on the measurement model, see Appendix A.

Table 3. Fixed Effects/Dummy Variable APC Models, Showing Differential Effects of Education on Skill Utilization as Proportion of College Educated Rises

	Model 1		Model 2		Model 3	
	Verbal Skill Utilization		Quantitative Skill Utilization		Analytic Skill Utilization	
	Male	Female	Male	Female	Male	Female
Proportion of Cohort with 4-Year Degree	0.630	0.746	0.475	0.398	0.672	0.803
	(0.0621)	(0.0669)	(0.0587)	(0.0642)	(0.0566)	(0.0647)
<i>Educational Attainment (ref: No College)</i>						
Some College	1.553	1.396	1.198	0.847	1.245	1.109
	(0.0266)	(0.0279)	(0.0251)	(0.0267)	(0.0242)	(0.0270)
Four-Year Degree	2.179	1.651	1.633	0.894	2.132	1.791
	(0.0215)	(0.0238)	(0.0203)	(0.0228)	(0.0196)	(0.0230)
More than 4-Year Degree	2.543	1.944	1.193	1.125	2.545	2.093
	(0.0270)	(0.0343)	(0.0255)	(0.0329)	(0.0246)	(0.0331)
<i>Relative Educational Effects by Attainment</i>						
Some College * College Proportion	-2.183	-1.894	-1.602	-1.181	-1.658	-1.163
	(0.0686)	(0.0718)	(0.0648)	(0.0689)	(0.0625)	(0.0694)
Four-Year Deg. * College Proportion	-1.648	-1.424	-1.173	-0.384	-2.075	-1.772
	(0.0555)	(0.0611)	(0.0525)	(0.0586)	(0.0506)	(0.0590)
More than 4-Year Deg. * College Proportion	-1.256	-1.281	-0.0884	-0.875	-1.242	-1.132
	(0.0707)	(0.0879)	(0.0668)	(0.0842)	(0.0644)	(0.0849)
Constant	1.483	3.030	1.451	2.187	1.289	1.776
	(0.108)	(0.116)	(0.102)	(0.111)	(0.0985)	(0.112)
Observations	1,003,679	759,162	1,003,679	759,162	1,003,679	759,162
R-Squared	0.339	0.255	0.211	0.131	0.351	0.272

All coefficients statistically significant at $p < 0.001$ except More than 4-Year Deg.*College Proportion (Male Quant) which is not significant.
Source: Annual Social and Economic Supplement of the Current Population Survey: 1971-2010 (Flood et al. 2015)

Coefficients for age (linear, squared, cubed), cohort (5 year dummy variables), year, marital status (married, never married, separated/widowed/divorced), race (white, black, Hispanic, other), metro area (central city, outside central city, not in metro area, not identifiable), census division, and interactions by race-cohort, marital status-cohort, census division-education, and census division-year included in model but not listed here; full results are available by request.

Table 4. Differences in Skill Utilization by Educational Level: Between 8% College-Educated and 40% College-Educated Cohorts

	Verbal Skill Utilization		Quantitative Skill Utilization		Analytic Skill Utilization	
	Male	Female	Male	Female	Male	Female
Decline in Skill Utilization of Four Year Degree from 8% to 40%	-0.32	-0.22	-0.22	0.00	-0.45	-0.31
Decline in the Advantage of Four Year Degree vs. No College Degree going from 8% to 40%	-0.53	-0.46	-0.38	-0.12	-0.66	-0.57
Interpretation of Skill Differences:	0-0.2: Not A Substantively Meaningful Difference 0.2-0.4: Noticeable/Moderate Substantive Difference 0.4-1.0: Large Substantive Difference Above 1: Very Large Substantive Difference					

Source: Annual Social and Economic Supplement of the Current Population Survey: 1971-2010 (Flood et al. 2015)

Findings are net of age (linear, squared, cubed), cohort (5 year dummy variables), year, marital status (married, never married, separated/widowed/divorced), race (white, black, Hispanic, other), metro area (central city, outside central city, not in metro area, not identifiable), census division, and interactions by race-cohort, marital status-cohort, census division-education, and census division-year.

Appendix A

In this section, I provide more detail on the construction of the dependent variables: Skill ratings for verbal, quantitative, and analytic skills. Overall, the dependent variables are comparable to previous measures constructed by Autor and Dorn (2013) and Liu and Grusky (2013). These skill ratings may be accessed in the online supplement to this article, and may also be downloaded at the SocArXiv website¹.

Autor and Dorn (2013) use skill ratings from the Dictionary of Occupational Titles to identify the degree of abstract reasoning and routine work for each occupation, and merged it into an occupational categorization scheme based on the 1990 census occupation categories. They operationalize abstract task activities as an average of the quantitative reasoning requirements of the job and the amount that the job involves planning and coordinating activities. Routine tasks are measured by an average of the ability to execute precise actions and manual finger dexterity requirements. However, the validity of this measurement is unknown, and averaging indicators together is a less desirable method than predicting factor scores.

Meanwhile, Liu and Grusky (2013) analyze data from O*NET, a more recent web-based version of the Dictionary of Occupational Titles; they create scales for several different skill types including verbal, quantitative, and analytic abilities. They identify an eight-factor skill model, comprising 45 separate indicator variables. Using confirmatory factor analysis, Liu and Grusky's develop separate scales for verbal, quantitative, and analytic skill, which together comprise a fairly strong operationalization of task complexity. As a result, Liu and Grusky's (2013) approach is more theoretically and empirically robust than Autor and Dorn's (2013); it uses a broader range of cognitive skills and estimates a scale using factor analysis instead of

¹ The data may be found at the following link: <https://dx.doi.org/10.17605/OSF.IO/FNQV7>

simple averaging. However, replication of Liu and Grusky's (2013) cognitive scales reveals that the fit statistics for these three constructs are poor. Thus, while Autor and Dorn's (2013) measurements are too narrow, Liu and Grusky's (2013) scale is too broad.

FIGURE A1 ABOUT HERE

While the present measurement of skill is comparable to those constructed by Autor and Dorn (2013) and Liu and Grusky (2013), I also make several improvements in the present paper. Like Liu and Grusky (2013), I use data from the O*NET, but include fewer indicators. The path diagram for this measurement model is presented in Figure 1, with the factor loadings presented in Table 1. The factor structure for verbal and quantitative skills is relatively simple. I estimate the latent variable for verbal skill utilization with independent ratings of oral comprehension, written comprehension, and written expression, and I estimate quantitative skill using mathematics knowledge and mathematics reasoning skill. The factor structure for analytic skills is more complex, since it is represented by a latent variable for on-the-job learning and an observed measurement of complex problem solving skill. All of these categories, with the exception of mathematics knowledge, are skill ratings; because knowledge is technically the result of a learning process rather than a skill, I allow the errors for mathematical knowledge to correlate with the errors for learning and complex problem-solving.

The fit statistics for this model are either excellent (CFI: 0.994; TLI: 0.989) or good (RMSEA: 0.069), indicating that this factor structure converges on consistent constructs. Using the measurement model, I predict factor scores for each occupation for three different latent variables: verbal, quantitative, and analytic skill utilization (as in Liu and Grusky 2013). I then use Autor and Dorn's (2013) occupational coding scheme to harmonize the occupational skill ratings over time. However, just as Liu and Grusky (2013) welcome further attempts to develop

their scale, I welcome further attempts to develop the one presented here. In particular, it would be beneficial to incorporate more of Liu and Grusky's (2013) indicators into this model.

For all analyses, I use the 2003 update of the O*NET database (Boese et al., 2001). The 2003 O*NET update is the last time that the Department of Labor used independent task raters; all contemporary O*NET ratings are the result of surveys sent to workers currently at their job. For this purpose, surveys are inferior to using independent task raters; workers can only compare their skill utilization at their present job to ones they have already had. It also means that several rare occupations are only rated by a handful of respondents.

Unlike Liu and Grusky, I use only one O*NET database year. The authors of O*NET strongly recommend only using a single year of occupation rating at a time, perhaps because of changes to rating protocols from year to year. Furthermore, although Liu and Grusky (2013) present a compelling rationale for looking at skill changes over time, they find almost no change in non-computer skills. They describe the change in skills as “glacial” (Liu and Grusky 2013: 1346) and concluded that only intra-occupational change for computing skills is noticeable—which, according to Liu and Grusky (2013), are also not major drivers of increasing wage inequality. Their conclusion: “[I]f a major upgrading in skill is to be found, it will either be confined to computing alone or be driven by changes in the mix of occupations rather than within-occupation changes in skill content.” (Liu and Grusky 2013: 1346).

If Liu and Grusky (2013) are incorrect—and cognitive skill content of occupations has changed over time—then is it possible that the present findings are biased? It is possible, but only if two other conditions are true. First, cognitive skill upgrading would have to *disproportionately affect occupations* with a high proportion of better-educated workers. This is inconsistent with the tenets of SBTC, which posit that skill upgrading occurred across all

occupations; furthermore, there is not much evidence to support the notion that highly skilled occupations require even more cognitive skill than in the past. Second, cognitive skill upgrading would have to occur *within occupational categories*, and not via the growth and decline of occupational groups. It is possible that Liu and Grusky (2013) are incorrect that cognitive skill has remained stable over time. It is also possible that skill upgrading primarily affects already highly-skilled occupations, or that cognitive skill upgrading happens within occupations instead of changes to the occupational structure. However, the likelihood that *all three* of these conditions are true is much less likely, lending support to the analyses in the present study.

Figure A1. Measurement Model used for Factor Scores, using Indicators from O*NET

Table A1. Measurement Model Factor Loadings, for Factor Score Prediction

Factor	Indicator	Coefficient	Standard Error
Verbal	Oral Comprehension	1	(0)
	Written Comprehension	1.102***	(0.0238)
	Written Expression	1.365***	(0.0288)
Quantitative Reasoning	Mathematical Reasoning	1	(0)
	Mathematics	0.896***	(0.0276)
Analytic Reasoning	Complex Problem Solving	1	(0)
	Ongoing Learning (Latent)	1.123***	(0.0158)
Ongoing Learning	Critical Thinking	1	(0)
	Active Learning	1.043***	(0.0140)

Standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05

Appendix B

In this appendix, I demonstrate that the key findings of this study are robust to alternative age-period-cohort (APC) model specifications. APC modeling is very difficult because age, period, and cohort are perfectly linearly related; whenever the analyst knows two of age, period, and cohort, the third is known as well. In order to identify the model, some constraint must be placed to break the perfect linear relationship (Glenn 2003). Unfortunately, choosing a constraint that does not match the underlying trend can bias temporal results. There is no agreement on the best constraint to impose on the model, since underlying trends are different from one case to the next, and a constraint that is justified in one context may bias the results in another.

The major concern here is with the relationship between the relative education interaction and the dependent variables of interest; the age, period, and cohort variables are included primarily to remove spurious relationships. That said, an inaccurately specified cohort trend could bias the interaction between individual- and population-level educational attainment. Consequently, it is most important to demonstrate that the APC modeling strategy does not change the relative education interaction coefficient in any substantive way. Therefore, I present cohort and period as dummy variables in the main section of this paper, but also provide four additional types of APC models, and report the key independent variables for each one.

Each of the APC models uses a different method to identify age, period and cohort; they include the fixed-effects/dummy variable approach from the main study (Yang and Land 2008), a type of related populations model (Riebler, Held, and Rue 2012), a model that identifies cohort as an age-period interaction (Luo 2015), an inductive approach to building a parsimonious APC model (Harding and Jencks 2003), and a cross-classified random effects model (Yang and Land

2008). *All* of these models have advantages and disadvantages, and which I discuss in the following section.

The results are located in Tables B1-B6, with each table including the results for a different measure of skill utilization, and separate tables for male and female respondents. Within each table, I include all five models to easily compare differences in the key independent variables. The coefficients are not substantively different across different APC modeling approaches. Thus, the key findings of this study are robust to different APC modeling solutions, and are therefore not likely affected by choice of constraint in the APC model. I also replicate figures from the main study using coefficients from Model 2.

TABLES B1-B6 HERE

Fixed Effects/Dummy Variable Model

The first model that I present in Appendix B is the fixed-effects APC model, which represents age as a linear, quadratic, and cubic trend, and period and cohort as unequal-width dummy variables (Yang and Land 2008). This is the model presented in the main manuscript, and is reproduced here for comparison purposes. Model 1 is represented by the equation:

$$\begin{aligned}
 y = & \beta_0 + \beta_1(X_{collprop}) + \beta_2(X_{somecollege}) + \beta_3(X_{fouryeardegree}) + \beta_4(X_{morethanfouryears}) + \\
 & \beta_5(X_{somecollegeXcollprop}) + \beta_6(X_{fouryeardegreeXcollprop}) + \beta_7(X_{more4yrsXcollprop}) + \beta_8(X_{age}) + \\
 & \beta_9(X_{age^2}) + \beta_{10}(X_{age^3}) + \sum \delta_j (D_{year}) + \sum \delta_k (D_{cohort}) + \sum \delta_m (D_{marital\ status}) + \sum \delta_r (D_{race}) + \\
 & \sum \delta_a (D_{metroarea}) + \sum \delta_d (D_{censusdivision}) + \sum \delta_{km} (D_{cohortXmaritalstatus}) + \sum \delta_{kr} (D_{cohortXrace}) + \\
 & \sum \delta_{de} (D_{censusdivisionXeducation}) + \sum \delta_{dj} (D_{censusdivisionXyear}) + \varepsilon_i
 \end{aligned}$$

Where X represents specific independent variables and β represents a single coefficient, and where D represents a vector of dummy variables and δ represents a set of coefficients for

each dummy variable. This is the model from the main study, and is located in the first column of Tables B1-B6.

There are numerous advantages to representing cohort and period as unequal width and fixed-effects dummy variables. Interpretation is straightforward, with the omitted period and cohort representing the reference group and each dummy variable as the deviation from the reference category. Using dummy variables (or random effects) to test cohort and period differences permits the ability to capture multiple functional forms within the model parameters. Because age, period, and cohort are related to each other by formal definition, using dummy variables avoids violating the assumptions of a random effects model (see: Luo and Hodges 2016); additionally, the slightly larger standard errors lead to more conservative statistical tests than the cross-classified random effects model. Finally, estimation is straightforward and simple, and the model's weaknesses are well-known in comparison with other approaches that have not been subjected to as much scrutiny.

However, there are two concerns with unequal-width APC models. Luo and Hodges (2016) note that collapsing years or birth cohorts into groups is equivalent to imposing restrictions on parameters in the model. In other words, grouping individuals born in 1930-1934 into a single cohort bin is similar to constraining the parameters for each of the cohorts (1930, 1931, 1932, 1933, 1934) to be equal to each other. Although not as damaging as constraining five-year blocks to be equal to each other, biased cohort estimates are possible. Additionally, by accounting for age, period, and cohort as separate variables, the model assumes that cohort effects do not change (Luo 2015). Thus, changes in cohort effects can be mistaken for age trends.

APC-Related Populations Model

The APC-Related Populations model is a method that permits age, period, and cohort to be measured in equally-spaced intervals (Riebler, Held, and Rue 2012). The model achieves identification by estimating several different populations of interest, and constraining one temporal effect to be equal across populations. For example, the analyst could evaluate lung cancer deaths across Sweden, Norway, and Denmark; age groupings could be constrained to be equal across all three regions, while period and cohort are allowed to vary by country. This method is usually performed in a Bayesian framework, although frequentist inference is also possible (Riebler, Held, and Rue 2012). In the frequentist estimation used here, both period and cohort interact with geographic region, but no main effect for period and cohort effect is estimated.

In the second column of Tables B1-B6, I report the key independent variables from the related populations model. Age, period, and cohort are all represented by five-year dummy variables. Age is assumed to be constant across geographical regions, while cohort and period interact with geographical region but the main effect of cohort and period is omitted.

Model 2 is therefore represented by the following equation:

$$\begin{aligned}
 y = & \beta_0 + \beta_1(X_{collprop}) + \beta_2(X_{somecollege}) + \beta_3(X_{fouryeardegree}) + \beta_4(X_{morethanfouryears}) + \\
 & \beta_5(X_{somecollege \times collprop}) + \beta_6(X_{fouryeardegree \times collprop}) + \beta_7(X_{more4yrs \times collprop}) + \beta_8(X_{age}) + \\
 & \beta_9(X_{age^2}) + \beta_{10}(X_{age^3}) + \sum \delta_{dj} (D_{censusdivision \times year}) + \sum \delta_{dj} (D_{censusdivision \times cohort}) \\
 & + \sum \delta_m (D_{marital\ status}) + \sum \delta_r (D_{race}) + \sum \delta_a (D_{metroarea}) + \sum \delta_d (D_{censusdivision}) + \\
 & \sum \delta_{km} (D_{cohort \times maritalstatus}) + \sum \delta_{de} (D_{censusdivision \times education}) + \varepsilon_i
 \end{aligned}$$

Where X represents specific independent variables and β represents a single coefficient, and where D represents a vector of dummy variables and δ represents a set of coefficients for each dummy variable.

The related populations model has some advantages over other APC approaches. Unlike other APC models, no single age, period, or cohort interval is assumed to be equal to any other, circumventing the potential problems of unequal-width intervals (see Luo and Hodges 2016). In many contexts such as in health and mortality, age effects are less region specific than cohort and period trends, and thus the constraint is theoretically justified.

However, there are disadvantages to the related populations model. For one, constraining age effects to be equal but not constraining cohort or period effects is justified within health and mortality research, but since working life courses can vary by geographic region it is not as obvious a choice for modeling skill utilization. Furthermore, while the model can accurately and reliably detect cohort and period changes within each geographical region, it is not able to adequately estimate overall cohort and period effects. Riebler, Held, and Rue (2012) are correct that this is sufficient for many research questions, but the inability to estimate overall cohort and period effects would have unknown impacts on other variables in the regression model.

APC-Interaction Model

Luo (2015) proposes an alternative APC modeling strategy that represents cohorts as an age-by-period interaction effect. Cohort dummy variables are not included; instead, age and period variables are included, as is a full interaction of age by period variables. Cohort effects are identified by the interaction effect. The model is identified because the cohort effect varies by combination of age and period, breaking the linear relationship between the three variables.

In the third column of Tables B1-B6, I estimate the APC-Interaction model. I interact one-year time periods with age, age-squared, and age-cubed variables; when the model calls for interactions between cohorts and other variables, I utilize the age-period interaction in place of cohort. Thus, Model 3 is given by the equation:

$$\begin{aligned}
y = & \beta_0 + \beta_1(X_{collprop}) + \beta_2(X_{somecollege}) + \beta_3(X_{fouryeardegree}) + \beta_4(X_{morethanfouryears}) + \\
& \beta_5(X_{somecollegeXcollprop}) + \beta_6(X_{fouryeardegreeXcollprop}) + \beta_7(X_{more4yrsXcollprop}) + \beta_8(X_{age}) + \\
& \beta_9(X_{age^2}) + \beta_{10}(X_{age^3}) + \sum \delta_j D_{year} + \sum \zeta_{aj} (I_{ageXyear}) + \sum \zeta_{a2j} (I_{age^2Xyear}) + \sum \zeta_{a3j} (I_{age^3Xyear}) + \\
& \sum \delta_m (D_{maritalstatus}) + \sum \delta_r (D_{race}) + \sum \delta_a (D_{metroarea}) + \sum \delta_d (D_{censusdivision}) + \\
& \sum \zeta_{ajm} (I_{ageXyearXmaritalstatus}) + \sum \zeta_{ajr} (I_{ageXyearXrace}) + \sum \zeta_{a2jm} (I_{age^2XyearXmaritalstatus}) + \\
& \sum \zeta_{a2jr} (I_{age^2XyearXrace}) + \sum \zeta_{a3jm} (I_{age^3XyearXmaritalstatus}) + \sum \zeta_{a3jr} (I_{age^3XyearXrace}) + \\
& \sum \delta_{de} (D_{censusdivisionXeducation}) + \sum \delta_{dj} (D_{censusdivisionXyear}) + \varepsilon_i
\end{aligned}$$

Where X represents specific independent variables and β represents a single coefficient, where D represents a vector of dummy variables and δ represents a set of coefficients for each dummy variable, and where I represents a vector of interactions and ζ represents the interaction between a continuous variable and a dummy variable. The results from this model are located in the third column of Tables B1-B6.

There are two major advantages to the APC-Interaction model. First, this model does not constrain any cohort groupings to be equal to each other, meaning that it does not introduce constraints that may introduce error into the analysis (Luo and Hodges 2016). Additionally, unlike all of the other models presented here, cohort effects vary depending on age, which is a useful characteristic if cohort effects are not constant across the life course.

Unlike models that represent cohort effects as multi-year group variables, Luo's (2015) approach is relatively untested and the weaknesses are not well-known. In particular, it seems that Luo's (2015) model might be less effective at capturing sharp breaks in between cohorts, since cohort trends are posited as an interaction of two other forces. Another obvious weakness of Luo's (2015) model is that it is not parsimonious (Harding and Jencks 2003); where cohort effects are the same across ages, the model duplicates this effect across many different variables.

Finally, interpretation of cohort effects is not clear, since each cohort may have a large number of different effects across the lifespan; it is difficult to select a cohort effect to estimate total effect sizes.

Inductive Parsimony Model

Harding and Jencks (2003) take a different approach to APC Modeling: While most APC models are primarily concerned with identifying the model, Harding and Jencks (2003) are most concerned with parsimony, and that the model be as simple as possible. Using this guiding principle, they suggest careful examination of descriptive data to justify the inclusion of period effects, to determine whether the model requires age-by-cohort interactions, and whether age can be fit as a linear term or with a spline. The model is then estimated using continuous variables for age, and dummy variables for cohort and (if necessary) either an unequal-width period effect or cohort-by-age interaction. As a result, despite the differences in philosophy it is possible to use Harding and Jencks' (2003) inductive approach and arrive at a substantively similar model to either the fixed-effects/dummy variable APC model (Model 1) or the APC-Interaction model (Model 3).

FIGURE B1 HERE

The first step in Harding and Jencks' approach is to use graphical methods to identify whether there is an obvious period trend that works similarly across cohorts. To answer this question, I graph men's skill utilization by cohort and across period in Figure B1, and women's skill utilization by cohort and across period in Figure B2. The graphs suggest that there is a linear trend by period, and that it operates similarly across cohorts. Furthermore, a period trend can sometimes manifest as a lagged cohort effect. Thus, if a period effect is occurring so that skill is increasing across time periods, we would expect to see later cohorts enter the sample with greater

skill utilization. In Figures B3 and B4, individuals who enter the labor force during later periods utilize greater skill, further suggesting the need to measure period effects.

FIGURE B2 HERE

The second step is to investigate whether there need to be any age by cohort interactions included in the model. This would be necessary if the age trajectories in Figures B3 and B4 were noticeably different across cohorts. However, age effects appear similar across birth cohorts, suggesting that interactions between age and cohort are not required. Because using dummy variables is more parsimonious than interactions, I am able to opt for a smaller set of cohort dummies than a full age by cohort interaction.

FIGURE B3 HERE

The third and final step is to capture any nonlinear trends in aging, preferably using splines. In Figure B3, men's skill utilization tends to rapidly increase until about age 35, with the rate of growth slowing thereafter. On the other hand, women's skill utilization increases more slowly until age 50, at which point skill utilization stops increasing. Thus, men and women require different spline locations.

FIGURE B4 HERE

Because a period effect is needed but an interaction but can be represented adequately with dummy variables instead of interactions, the result is a model that is substantively similar to Model 1. While philosophically Harding and Jencks (2003) would prefer to omit the period variables entirely, it is not justified by the descriptive graphs. The only major difference between Model 1 and Model 4 is that instead of modeling age as quadratic and cubic effects, Model 4 is represented by an age spline that differs by sex (age 35 for men, age 50 for women). Model 4 is thus represented by the equation:

$$\begin{aligned}
y = & \beta_0 + \beta_1(X_{collprop}) + \beta_2(X_{somecollege}) + \beta_3(X_{fouryeardegree}) + \beta_4(X_{morethanfouryears}) + \\
& \beta_5(X_{somecollegeXcollprop}) + \beta_6(X_{fouryeardegreeXcollprop}) + \beta_7(X_{more4yrsXcollprop}) + \\
& \beta_8(X_{age_underspline}) + \beta_9(X_{age_overspline}) + \sum \delta_{dj} (D_{censusdivisionXyear}) + \\
& \sum \delta_{dj} (D_{censusdivisionXcohort}) + \sum \delta_m (D_{maritalstatus}) + \sum \delta_r (D_{race}) + \sum \delta_a (D_{metroarea}) + \\
& \sum \delta_d (D_{censusdivision}) + \sum \delta_{km} (D_{cohortXmaritalstatus}) + \sum \delta_{de} (D_{censusdivisionXeducation}) + \varepsilon_i
\end{aligned}$$

These results are located in the fourth column of Tables B1-B6.

The advantage of this method is that in some cases, it may be possible to identify models that only require two time trends instead of three. On the other hand, Harding and Jencks (2003) place a great deal of faith in the ability to identify period and cohort effects in graphs; given that any two of age, period, and cohort effects can mimic the third, identifying visual trends from is not always possible. However, it is important to note that while Model 4 is philosophically quite different than Model 1, substantively they are quite similar. As a result, other models (especially Model 2 and 3) are required to test whether the APC design has biased the results.

Cross-Classified Random Effects Models

Cross-Classified Random Effects models (CCREMs) are a variation of the fixed-effects dummy variable approach to APC modeling. Instead of providing unequal-width dummy variables, period and cohort are treated as random intercepts (Yang and Land 2008). Interactions with period and cohort are designated by randomly varying slopes. The level-one specification for the CCREM (Model 5) is as follows:

$$\begin{aligned}
y = & \beta_{0jk} + \beta_{1jk}(X_{collprop}) + \beta_{2jk}(X_{somecollege}) + \beta_{3jk}(X_{fouryeardegree}) + \\
& \beta_{4jk}(X_{morethanfouryears}) + \beta_{5jk}(X_{somecollegeXcollprop}) + \beta_{6jk}(X_{fouryeardegreeXcollprop}) + \\
& \beta_{7jk}(X_{more4yrsXcollprop}) + \beta_{8jk}(X_{age}) + \beta_{9jk}(X_{age^2}) + \beta_{10jk}(X_{age^3}) + \sum \delta_{mjk} (D_{maritalstatus}) +
\end{aligned}$$

$$\sum \delta_{rjk} (D_{race}) + \sum \delta_{ajk} (D_{metroarea}) + \sum \delta_{djk} (D_{censusdivision}) + \\ \sum \delta_{dejk} (D_{censusdivision \times education}) + \varepsilon_{ijk}$$

And by the following level-2 equations:

$$\beta_0 = t_{0j} + c_{0k}$$

$$\delta_{mjk} = \gamma_m + c_{mk}$$

$$\delta_{rjk} = \gamma_r + c_{rk}$$

$$\delta_{djk} = \gamma_d + t_{dj}$$

Where X represents specific independent variables and β represents a single coefficient, where D represents a vector of dummy variables and δ represents a set of coefficients for each dummy variable, where t represents random effects by period and c represents random effects by cohort, and where the fixed effects for each dummy variable are represented by γ . This model is located in the fifth column of Tables B1-B6.

The random effects specification has some advantages over other models. Like the fixed effects/dummy variable model, interpretation is straightforward, where the mean and the slope vary depending on the time period and cohort where an individual lives. The random effects specification also means that the model captures sharp breaks across cohorts and time periods. The standard errors are distributed slightly more efficiently in moderate sample sizes, provided a non-significant Hausman test (Yang and Land 2008).

However, there are several weaknesses of the CCREM. Practically, treating period and cohort as random effects means assuming that the error from period, cohort, and age is uncorrelated with each other and with any other variables in the model (Luo and Hodges 2016). This is unlikely to be true in several cases, especially given that age, period, and cohort are related to each other by mathematical definition. The CCREM is also vulnerable to the same

problems that plague unequal-width dummy variables, where the choice of constraint can bias estimates, and where cohort effects remain stable across the life course.

There is another issue which appeared during the analysis of the present data, although there is no published research suggesting it would be a problem: CCREMs with random slopes² are sometimes empirically underidentified. From a theoretical perspective, this makes sense when the following is taken into account: (a) Random effects, like all error structures, are latent variables, (b) increasing the number of latent variables in the model without dramatically increasing the number of endogenous variables can sometimes result in underidentified models, and (c) age-period-cohort models are *already* perilously close to underidentification because age, period, and cohort represent a perfect linear relationship³. There no mention of this in the published literature, although it is likely that other researchers have independently discovered the same issues. Thus, while Yang and Land (2008) are entirely correct that CCREMs are a useful way to estimate random intercepts, there is some as-yet unexplained problem with random-coefficient CCREMs. As a result, while the results from Model 5 are generally very similar to Models 1-4, the model estimating men's quantitative skill utilization is empirically underidentified and also provides much more support for the hypotheses than the other models.

Replication of Graphs: Related Populations Model

To emphasize the point that there are very few substantive differences in the key independent variables across models, I replicate Figure 2 and Figure 3 from the main section of this study with coefficients from the related populations model. Figure B5 is the predicted changes in skill utilization as the proportion of college-educated rises, while Figure B6 is how

² In cases where there are only random intercepts, CCREMs are identified (as in: Yang and Land 2008).

³ The casual analyst can observe this simply by running a model with random slopes in widely-used statistical programs. In R and Stata the analysis is rarely estimable; meanwhile SAS deals with empirical underidentification by occasionally fixing one or more random effects to have zero variance, leading to unstable model results.

the predicted skill level gaps between college educated and non-college educated individuals changes at low and high levels of education in the population.

B5 ABOUT HERE

I replicate the related populations model (Model 2) because it involves a substantively different set of methodological strategies and has more conservative estimates in contrast to Model 1. If there are any challenges to the findings in Model 1, they are most likely to occur using coefficients from Model 2. The results from Figure B5 show a smaller decline in quantitative skill utilization among college-educated men than in Figure 2. The differences in quantitative skill utilization is moderate using coefficients from Model 1 (0.22 change), but are not substantively meaningful using coefficients from Model 2 (0.17 change).

All other declines in skill utilization are of similar magnitude over time. The difference in men's quantitative skill utilization is the largest—and only substantively meaningful—difference in interpretation between Model 2 and Model 1. Moreover, when comparing the results from Model 1 and Model 2, *there are no differences in the advantage* that college education provides in skill utilization. As shown in Figure B6, this is even true for men's quantitative skill utilization; while men with a college degree had a smaller decline in quantitative skill utilization, this is entirely due to changes in skill utilization among all men.

B6 ABOUT HERE

To explicate this further, I report the exact differences in verbal skill utilization among men across Model 1 and Model 2. Using Model 1, the difference in verbal skill utilization between men with a college degree and those with no college experience is 1.99 at 8% and 1.46 at 40%. Meanwhile, using the coefficients from Model 2, the difference in verbal skill utilization between men with a college degree and those with no college experience is 1.98 at 8% and 1.46

at 40%. Similar results are found across all six analyses, such that the gap between individuals with a four year degree and those with no college experience is almost exactly identical using coefficients from Model 1 as they are from Model 2. Thus, the overall advantage a college degree provides in skill utilization and how it changes over time is almost exactly the same.

Table B1. Alternative Age-Period Cohort Estimation Procedures Demonstrate Similar Effects of Education on Men's Verbal Skill Utilization

	Model 1: Fixed Period and Cohort Dummies	Model 2: Related Populations Model	Model 3: APC Interaction Model	Model 4: Inductive/ Parsimony Model	Model 5: Cross- Classified Random Effects Model
Prop. of Cohort with 4-Year Degree	0.630 (0.0621)	0.827 (0.0733)	0.671 (0.0570)	0.668 (0.0618)	0.7021 -0.05028
Some College	1.553 (0.0266)	1.554 (0.0270)	1.563 (0.0266)	1.553 (0.0266)	1.5541 -0.02652
Four-Year Degree	2.179 (0.0215)	2.167 (0.0218)	2.192 (0.0215)	2.178 (0.0215)	2.1807 -0.02141
More than 4-Year Degree	2.543 (0.0270)	2.535 (0.0275)	2.561 (0.0270)	2.543 (0.0270)	2.5446 -0.02695
Some College * College Proportion	-2.183 (0.0686)	-2.175 (0.0702)	-2.221 (0.0686)	-2.182 (0.0686)	-2.1795 -0.06849
Four-Year Deg. * College Proportion	-1.648 (0.0555)	-1.617 (0.0570)	-1.700 (0.0556)	-1.647 (0.0555)	-1.6445 -0.05542
More than 4-Year Deg.* Coll. Prop.	-1.256 (0.0707)	-1.217 (0.0725)	-1.324 (0.0708)	-1.256 (0.0707)	-1.2505 -0.07059
Constant	1.483 (0.108)	2.207 (0.0296)	1.576 (0.853)	1.987 (0.0547)	1.2719 -0.09751

Standard errors in parentheses

All coefficients statistically significant at $p < 0.001$

Table B2. Alternative Age-Period Cohort Estimation Procedures Demonstrate Similar Effects of Education on Women's Verbal Skill Utilization

	Model 1: Fixed Period and Cohort Dummies	Model 2: Related Populations Model	Model 3: APC Interaction Model	Model 4: Inductive/ Parsimony Model	Model 5: Cross- Classified Random Effects Model
Prop. of Cohort with 4-Year Degree	0.746 (0.0669)	0.671 (0.0789)	0.862 (0.0611)	0.733 (0.0655)	0.9115 -0.05805
Some College	1.396 (0.0279)	1.383 (0.0284)	1.404 (0.0279)	1.396 (0.0279)	1.4028 -0.02785
Four-Year Degree	1.651 (0.0238)	1.648 (0.0244)	1.661 (0.0239)	1.652 (0.0238)	1.6578 -0.02373
More than 4-Year Degree	1.944 (0.0343)	1.952 (0.0351)	1.961 (0.0343)	1.944 (0.0343)	1.9616 -0.03415
Some College * College Proportion	-1.894 (0.0718)	-1.857 (0.0738)	-1.928 (0.0718)	-1.895 (0.0718)	-1.8994 -0.07175
Four-Year Deg. * College Proportion	-1.424 (0.0611)	-1.444 (0.0630)	-1.459 (0.0612)	-1.425 (0.0611)	-1.4294 -0.06096
More than 4-Year Deg.* Coll. Prop.	-1.281 (0.0879)	-1.287 (0.0907)	-1.338 (0.0880)	-1.281 (0.0879)	-1.3052 -0.08764
Constant	3.030 (0.116)	2.710 (0.0319)	3.009 (0.958)	2.828 (0.0576)	2.6273 -0.1098

Standard errors in parentheses

All coefficients statistically significant at $p < 0.001$

Table B3. Alternative Age-Period Cohort Estimation Procedures Demonstrate Similar Effects of Education on Men's Quantitative Skill Utilization

	Model 1: Fixed Period and Cohort Dummies	Model 2: Related Populations Model	Model 3: APC Interaction Model	Model 4: Inductive/ Parsimony Model	Model 5: Cross- Classified Random Effects Model
Prop. of Cohort with 4-Year Degree	0.475 (0.0587)	0.619 (0.0693)	0.415 (0.0538)	0.502 (0.0584)	1.2195 -0.09169
Some College	1.198 (0.0251)	1.197 (0.0255)	1.208 (0.0251)	1.198 (0.0251)	0.5065 -0.04532
Four-Year Degree	1.633 (0.0203)	1.623 (0.0206)	1.647 (0.0203)	1.632 (0.0203)	1.1991 -0.02505
More than 4-Year Degree	1.193 (0.0255)	1.189 (0.0260)	1.211 (0.0255)	1.193 (0.0255)	1.6345 -0.02023
Some College * College Proportion	-1.602 (0.0648)	-1.585 (0.0663)	-1.636 (0.0648)	-1.602 (0.0648)	1.1954 -0.02546
Four-Year Deg. * College Proportion	-1.173 (0.0525)	-1.150 (0.0538)	-1.223 (0.0526)	-1.172 (0.0525)	-1.5991 -0.0647
More than 4-Year Deg.* Coll. Prop.	-0.0884 (0.0668)	-0.0651 (0.0685)	-0.156 (0.0670)	-0.0876 (0.0668)	-1.1739 -0.05234
Constant	1.451 (0.102)	1.908 (0.0280)	1.756 (0.806)	1.785 (0.0517)	-0.09121 -0.06667

Standard errors in parentheses

All coefficients statistically significant at $p < 0.001$ except:

More than 4-Year Deg.* Coll. Prop: Not statistically significant in M1, M2, M4; Significant at $p < 0.05$ in M3

Table B4. Alternative Age-Period Cohort Estimation Procedures Demonstrate Similar Effects of Education on Women's Quantitative Skill Utilization

	Model 1: Fixed Period and Cohort Dummies	Model 2: Related Populations Model	Model 3: APC Interaction Model	Model 4: Inductive/ Parsimony Model	Model 5: Cross- Classified Random Effects Model
Prop. of Cohort with 4-Year Degree	0.398 (0.0642)	0.318 (0.0756)	0.460 (0.0586)	0.402 (0.0628)	0.5437 -0.0539
Some College	0.847 (0.0267)	0.838 (0.0272)	0.856 (0.0268)	0.847 (0.0267)	0.8519 -0.02669
Four-Year Degree	0.894 (0.0228)	0.895 (0.0234)	0.910 (0.0229)	0.894 (0.0228)	0.9004 -0.02274
More than 4-Year Degree	1.125 (0.0329)	1.134 (0.0336)	1.146 (0.0329)	1.125 (0.0329)	1.1415 -0.03273
Some College * College Proportion	-1.181 (0.0689)	-1.157 (0.0707)	-1.217 (0.0689)	-1.181 (0.0688)	-1.1865 -0.06878
Four-Year Deg. * College Proportion	-0.384 (0.0586)	-0.408 (0.0603)	-0.437 (0.0587)	-0.384 (0.0586)	-0.3971 -0.05842
More than 4-Year Deg.* Coll. Prop.	-0.875 (0.0842)	-0.896 (0.0869)	-0.945 (0.0844)	-0.875 (0.0842)	-0.9062 -0.084
Constant	2.187 (0.111)	2.057 (0.0306)	4.079 (0.918)	2.105 (0.0552)	1.947 -0.1006

Standard errors in parentheses

All coefficients statistically significant at $p < 0.001$

Table B5. Alternative Age-Period Cohort Estimation Procedures Demonstrate Similar Effects of Education on Men's Analytic Skill Utilization

	Model 1: Fixed Period and Cohort Dummies	Model 2: Related Populations Model	Model 3: APC Interaction Model	Model 4: Inductive/ Parsimony Model	Model 5: Cross- Classified Random Effects Model
Prop. of Cohort with 4-Year Degree	0.672 (0.0566)	0.829 (0.0668)	0.662 (0.0519)	0.711 (0.0564)	0.7399 -0.04546
Some College	1.245 (0.0242)	1.244 (0.0246)	1.253 (0.0242)	1.245 (0.0242)	1.2469 -0.02417
Four-Year Degree	2.132 (0.0196)	2.124 (0.0199)	2.143 (0.0196)	2.131 (0.0196)	2.1346 -0.01951
More than 4-Year Degree	2.545 (0.0246)	2.538 (0.0250)	2.560 (0.0246)	2.545 (0.0246)	2.5476 -0.02456
Some College * College Proportion	-1.658 (0.0625)	-1.652 (0.0639)	-1.690 (0.0625)	-1.658 (0.0625)	-1.657 -0.06242
Four-Year Deg. * College Proportion	-2.075 (0.0506)	-2.060 (0.0519)	-2.118 (0.0507)	-2.074 (0.0506)	-2.0747 -0.0505
More than 4-Year Deg.* Coll. Prop.	-1.242 (0.0644)	-1.207 (0.0660)	-1.302 (0.0646)	-1.242 (0.0644)	-1.2407 -0.06433
Constant	1.289 (0.0985)	2.002 (0.0270)	1.315 (0.777)	1.846 (0.0499)	1.1255 -0.08876

Standard errors in parentheses

All coefficients statistically significant at $p < 0.001$

Table B6. Alternative Age-Period Cohort Estimation Procedures Demonstrate Similar Effects of Education on Women's Analytic Skill Utilization

	Model 1: Fixed Period and Cohort Dummies	Model 2: Related Populations Model	Model 3: APC Interaction Model	Model 4: Inductive/ Parsimony Model	Model 5: Cross- Classified Random Effects Model
Prop. of Cohort with 4-Year Degree	0.398 (0.0642)	0.318 (0.0756)	0.460 (0.0586)	0.402 (0.0628)	0.5437 -0.0539
Some College	0.847 (0.0267)	0.838 (0.0272)	0.856 (0.0268)	0.847 (0.0267)	0.8519 -0.02669
Four-Year Degree	0.894 (0.0228)	0.895 (0.0234)	0.910 (0.0229)	0.894 (0.0228)	0.9004 -0.02274
More than 4-Year Degree	1.125 (0.0329)	1.134 (0.0336)	1.146 (0.0329)	1.125 (0.0329)	1.1415 -0.03273
Some College * College Proportion	-1.181 (0.0689)	-1.157 (0.0707)	-1.217 (0.0689)	-1.181 (0.0688)	-1.1865 -0.06878
Four-Year Deg. * College Proportion	-0.384 (0.0586)	-0.408 (0.0603)	-0.437 (0.0587)	-0.384 (0.0586)	-0.3971 -0.05842
More than 4-Year Deg. * Coll. Prop.	-0.875 (0.0842)	-0.896 (0.0869)	-0.945 (0.0844)	-0.875 (0.0842)	-0.9062 -0.084
Constant	2.187 (0.111)	2.057 (0.0306)	4.079 (0.918)	2.105 (0.0552)	1.947 -0.1006

Standard errors in parentheses

All coefficients statistically significant at $p < 0.001$

Figure B1. Men's Skill Utilization by Cohort and Across Period

Figure B2. Women's Skill Utilization by Cohort and Across Period

Figure B3. Men's Age-Related Changes in Skill Utilization, by Cohort

Figure B4. Women's Age-Related Changes in Skill Utilization, by Cohort

Figure B5. Predicted Changes in Absolute Skill Utilization of College-Educated Individuals as Proportion of College-Educated Rises over Birth Cohorts

Note: Predictions are for 27 year old individuals who live in 1999 within a central city in the Mid-Atlantic census division, is white and married.
Source: Table 3

Figure B6. Predicted Gaps in Skill Level Between Four-Year Degree and No College: Differences between a Cohort with 8% College-Educated Proportion and a Cohort with 40% College-Educated Proportion.

Source: Annual Social and Economic Supplement of the Current Population Survey: 1971-2010 (Flood et al. 2015)

Findings are net of age (linear, squared, cubed), cohort (5 year dummy variables), year, marital status (married, never married, separated/widowed/divorced), race (white, black, Hispanic, other), metro area (central city, outside central city, not in metro area, not identifiable), census division, and interactions by race-cohort, marital status-cohort, census division-education, and census division-year. Predictions here are for 27 year old individuals who live in 1999 within a central city in the Mid-Atlantic census division, is white and married.

Appendix C

In this study, the main purpose is to show how the return to education changes as educational attainment rises across cohorts. As a result, I do not present the individual age, period, and cohort model in the main section of this manuscript; age, period, and cohort are included only additional covariates to account for any changes that could be correlated with educational attainment over time. However, many readers may find the temporal trends in skill utilization substantively interesting or useful. Therefore, I present the age-period-cohort model here, as well as several graphs outlining these changes.

The following model captures the age, period, and cohort trends for each dependent variable, represented by the following equation:

$$y = \beta_0 + \beta_1(X_{age}) + \beta_2(X_{age^2}) + \beta_3(X_{age^3}) + \sum \delta_j (D_{year}) + \sum \delta_k (D_{cohort}) + \varepsilon_i$$

Where age is represented by linear, quadratic, and cubic trends and period and cohort are represented by a series of dummy variables. Time period is represented by one-year intervals, while cohort is represented by 5-year groupings.

For the period and cohort graphs, I set the age equal to 40.79 because this is the mean age in this dataset. For the age and cohort graphs, I use the period effect for 1999. For the age and period graphs, I assume that the person was born between 1940 and 1944 since those individuals are likely to be in the labor force between 1971 and 2010. For every graph, I assume that the person lives in West North Central⁴ census division because its educational attainment is not particularly high or low.

⁴ This census division includes Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota and South Dakota.

Figure C1, C2, and C3 indicate the age, period, and cohort trends for verbal and quantitative skill utilization. The most notable change is a rapid increase in skill utilization over period, with women's skill utilization rising much faster than men's. Men have a slight increase in both all skill usage between ages 25 and 40, although it levels off thereafter, with marginal decreases in women's verbal and quantitative skill utilization in late adulthood. Verbal skill utilization for both men and women increased slightly over birth cohorts until the early baby boom cohorts, after which there is a secular decline that continues over time. Quantitative skill utilization also declines for men in the late baby boom cohorts, although women's quantitative skill usage only declines for cohorts born between 1975 and 1984. The cohort changes in analytic skill follow a similar pattern, although to a lesser degree.

Figure C1. Age, Period, and Cohort Trends in Verbal Skill Utilization

Figure C2. Age, Period, and Cohort Trends in Quantitative Skill Utilization

Figure C3. Age, Period, and Cohort Trends in Analytic Skill Utilization

Appendix D

To use the occupation skill scores listed here, first harmonize your occupation census codes from the CPS, ACS, or other data source using the coding scheme developed by David Dorn. Go to the following webpage: <http://www.ddorn.net/data.htm>. Then, download files A1-A7, depending the occupation codes your dataset uses. Merge the files into your dataset, matching your current file's coding scheme to the variable "occ" in Dorn's data file. For example, if your occupation codes are from the 1980 census, merge your data file with file A4, with the "occ" file as a linking code. Multiple merges are necessary if occupation codes change over time. Once every record that lists an occupation is merged with an occ1990dd code, then merge the skill score data using occ1990dd as a linking code.

Please cite the most recent version of this paper, as well as the following paper by David Autor and David Dorn:

David Autor and David Dorn. "The Growth of Low Skill Service Jobs and the Polarization of the U.S. Labor Market." *American Economic Review*, 103(5), 1553-1597, 2013.

Table D1. Occupation Skill Scores

Occupation Label	occ1990dd	ANALYTIC	QUANT	VERBAL
Chief Executives	4	5.5	4.66	5.33
Financial Managers	7	4.83	5.16	5.16
Human Resources Managers	8	4.4	4	4.8
Advertising and Promotions Managers	13	4.5	3.66	5.33
Education Administrators	14	4.5	3.91	5.25
Medical and Health Services Managers	15	3.66	4	4.6
Property, Real Estate, and Community Association Managers	18	3.2	4	4.2
Funeral Directors	19	1.5	1.2	2.8
Accountants and Auditors	23	4.83	3.8	4.8
Insurance Underwriters	24	3.5	3.4	3.6
Budget Analysts	25	4.5	4.6	4
Management Analysts	26	4.36	2.7	4.2
Human Resources, Training, and Labor Relations Specialists	27	4.16	3.4	4.4
Purchasing Agents and Buyers, Farm Products	28	2.4	3.6	4.4
Wholesale and Retail Buyers, Except Farm Products	29	3.83	3.33	4
Purchasing Managers	33	2.8	3.8	4
Agents and Business Managers of Artists, Performers, and Athletes	34	3	3	3.4
Construction and Building Inspectors	35	4.16	3.2	4.2
Compliance Officers	36	3.6	2.8	4.6
Architects, Except Naval	43	4.83	3	3.6
Aerospace Engineers	44	5.83	5.8	5.8
Materials Engineers	45	5	3.8	4.4
Petroleum Engineers	47	4.2	5	5.2
Chemical Engineers	48	6.16	6	5.33
Civil Engineers	53	4.2	5.4	5.6
Computer Hardware Engineers	55	5.83	5.5	5.33
Industrial Engineers, Including Health and Safety	56	4	4.4	4.2
Mechanical Engineers	57	5.12	5.12	4.83
Computer Scientists and Systems Analysts	64	4.5	4.83	5.66
Actuaries	66	4.83	4.4	4.2
Miscellaneous Mathematical Science Occupations	68	5.5	6.2	4.4
Astronomers and Physicists	69	5.16	5.2	4.8
Chemists and Materials Scientists	73	5.8	4.4	5

Atmospheric and Space Scientists	74	5.33	2.6	4.2
Environmental Scientists and Geoscientists	75	5	4	5.4
Agricultural and Food Scientists	77	5	2.8	4
Biological Scientists	78	4.2	3.6	5.2
Conservation Scientists and Foresters	79	4.83	2.4	4.8
Medical Scientists	83	6.33	5	6.2
Physicians and Surgeons	84	5.2	3.4	4.8
Dentists	85	4.5	1.4	3.4
Veterinarians	86	5.66	1.6	4.8
Optometrists	87	3.66	2.8	4.2
Podiatrists	88	5.5	1.4	3.2
Chiropractors	89	4.83	1.2	2.8
Registered Nurses	95	4.08	3.33	4.66
Pharmacists	96	4.5	3.6	4
Dietitians and Nutritionists	97	4.66	3.2	5
Respiratory Therapists	98	4.16	1.4	2.6
Occupational Therapists	99	3.16	2.4	3.4
Physical Therapists	103	4.33	1	4
Audiologists	104	4.66	3	4.4
Radiation Therapists	105	2.8	2	3.4
Physician Assistants	106	4.5	1.8	3.4
Postsecondary Teachers	154	5.16	3.4	5.4
Preschool and Kindergarten Teachers	155	2.66	2.16	3.83
Elementary and Middle School Teachers	156	4.16	3	4.2
Secondary School Teachers	157	4.16	3	4.2
Special Education Teachers	158	3.66	2.4	4.2
Other Teachers and Instructors	159	3.6	2.2	4.2
Counselors	163	4.5	3.2	4
Librarians	164	3.66	2.83	4.5
Archivists, Curators, and Museum Technicians	165	3.4	2.4	3.6
Economists	166	5	4.8	6
Psychologists	167	4.4	3.6	5
Sociologists	169	5.16	3.2	5.4
Urban and Regional Planners	173	4.83	3.4	4.8
Social Workers	174	3.83	2.4	3.6
Clergy	176	3.83	0.6	4.2

Miscellaneous Community and Social Service Specialists	177	3.16	2.2	3.2
Lawyers	178	5	1.2	4.8
Writers and Authors	183	2.2	2.4	4.4
Technical Writers	184	5.16	2	6.2
Designers	185	2.16	1.2	3.2
Musicians, Singers, and Related Workers	186	3.16	1.4	4
Actors	187	3.2	1.2	3
Artists and Related Workers	188	4.33	3	3.8
Photographers	189	2.66	2.83	2.83
Dancers and Choreographers	193	2.6	1	2.8
News Analysts, Reporters and Correspondents	195	4	0.6	4.8
Announcers	198	2	1.2	2
Athletes, Coaches, Umpires, and Related Workers	199	2.1	1	3
Clinical Laboratory Technologists and Technicians	203	2.66	3.16	3.33
Dental Hygienists	204	3.5	0.4	1.6
Medical Records and Health Information Technicians	205	2.83	2.6	3.8
Diagnostic Related Technologists and Technicians	206	3	2	2.8
Licensed Practical and Licensed Vocational Nurses	207	3.33	1	2.6
Emergency Medical Technicians and Paramedics	208	3.16	2	2.6
Nuclear Technicians	214	3.4	4.2	4.8
Drafters	217	3.4	3.6	3.6
Surveyors, Cartographers, and Photogrammetrists	218	3.5	3.8	3.8
Agricultural and Food Science Technicians	223	1.83	2.6	2.8
Chemical Technicians	224	3.2	4	5.4
Geological and Petroleum Technicians	225	3.4	3.8	3.6
Aircraft Pilots and Flight Engineers	226	3.16	3.6	3.2
Air Traffic Controllers and Airfield Operations Specialists	227	3.6	2.4	3.4
Broadcast and Sound Engineering Technicians and Radio Operators	228	3.33	2.6	3.6
Computer Programmers	229	4.66	4.33	5.33
Computer Control Programmers and Operators	233	1.4	2.4	2.6
Paralegals and Legal Assistants	234	4.33	2.6	5
Other Life, Physical, and Social Science Technicians	235	2.4	3.8	4.8
First-Line Supervisors/Managers of Retail Sales Workers	243	3.16	3	3.8
Insurance Sales Agents	253	3.33	2	3.6
Appraisers and Assessors of Real Estate	254	2.4	3.8	4
Securities, Commodities, and Financial Services Sales Agents	255	4.83	4	4.8

Advertising Sales Agents	256	3.16	2	3.8
Sales Engineers	258	4.66	4.83	5.16
Sales Representatives, Wholesale and Manufacturing	274	3.83	3.33	3.83
Counter and Rental Clerks	275	1	2.66	3
Cashiers	276	1.5	3.16	3
Door-To-Door Sales Workers, News and Street Vendors, and Related	277	1.4	1.6	2.8
Models, Demonstrators, and Product Promoters	283	2	0.6	0.6
First-Line Supervisors/Managers of Office and Administrative Support	303	3.33	3.5	4.33
Computer Operators	308	3.16	2.5	3.5
Secretaries and Administrative Assistants	313	3.66	2	3.8
Word Processors and Typists	315	0.66	2.5	3
Credit Authorizers, Checkers, and Clerks	316	1.66	1.2	2
Hotel, Motel, and Resort Desk Clerks	317	1.33	1.8	2
Travel Agents	318	2.16	2.4	2.8
Receptionists and Information Clerks	319	1.33	2.5	2.83
Order Clerks	326	1.2	3.4	3.2
Human Resources Assistants, Except Payroll and Timekeeping	328	2.8	2.4	3.8
Library Technicians	329	1.8	1.6	3.2
File Clerks	335	1.83	0.6	2
Brokerage Clerks	336	2.2	4	3.8
Bookkeeping, Accounting, and Auditing Clerks	337	2.83	4	4.33
Payroll and Timekeeping Clerks	338	1.5	3.4	2.4
Billing and Posting Clerks and Machine Operators	344	2.16	1.4	2.6
Office Machine Operators, Except Computer	347	1	0.4	2.2
Switchboard Operators, Including Answering Service	348	1	0.6	2.2
Postal Service Clerks	354	1.2	1.4	1.8
Postal Service Mail Carriers	355	1.16	1.2	1.8
Mail Clerks and Mail Machine Operators, Except Postal Service	356	1	1.6	2.4
Couriers and Messengers	357	1.33	1.4	2
Dispatchers	359	2.8	0.4	2.8
Cargo and Freight Agents	364	2.16	0.8	2
Procurement Clerks	365	3.16	1.6	3.6
Meter Readers, Utilities	366	1.5	0.8	2
Weighers, Measurers, Checkers, and Samplers, Recordkeeping	368	1.6	2.2	2.4
Transportation, Storage, and Distribution Managers	373	3.5	3.6	3.8
Claims Adjusters, Appraisers, Examiners, and Investigators	375	2.5	3.4	3.4

Customer Service Representatives	376	2.6	2.4	3.4
Eligibility Interviewers, Government Programs	377	2	0.8	3.4
Bill and Account Collectors	378	1.4	1.4	3.4
Office Clerks, General	379	1.16	3	3.33
Tellers	383	1.5	3.16	3.66
Proofreaders and Copy Markers	384	2	1.4	3.6
Data Entry Keyers	385	1.33	1.6	2.2
Statistical Assistants	386	2	4.6	3.2
Court, Municipal, and License Clerks	389	2.33	1.4	3.4
Maids and Housekeeping Cleaners	405	1.16	0.2	0.2
Laundry and Dry-Cleaning Workers	408	1.66	0.6	1.2
First-Line Supervisors/Managers of Fire Fighting and Prevention	417	4	3.2	4
First-Line Supervisors/Managers of Police and Detectives	418	4.33	3.5	4.66
Crossing Guards	425	0.6	0.6	2.2
Private Detectives and Investigators	426	3.5	2.6	3.8
Animal Control Workers	427	2.66	2.2	3.6
First-Line Supervisors/Managers of Food Preparation and Serving	433	3.2	2.8	3.4
Bartenders	434	0.83	1.83	1.33
Waiters and Waitresses	435	0.5	1.83	2
Chefs and Head Cooks	436	3.4	2.4	2.8
Food Preparation Workers	439	0.83	1.33	1.66
Combined Food Preparation and Serving Workers, Including Fast Food	444	1	1.33	1.33
Dental Assistants	445	2.33	3.16	3.66
Nursing, Psychiatric, and Home Health Aides	447	2	0.6	1.4
First-Line Supervisors/Managers of Housekeeping and Janitorial	448	2.66	2	3.2
First-Line Supervisors/Managers of Landscaping, Lawn Service, and	450	3	2	3
Grounds Maintenance Workers	451	2	0.6	1
Janitors and Building Cleaners	453	1	1.83	2
Pest Control Workers	455	2.66	1.6	1.8
Barbers	457	1.33	0.8	1.4
Hairdressers, Hairstylists, and Cosmetologists	458	1.8	1.4	2.2
First-Line Supervisors/Managers of Gaming Workers	459	3.4	3.6	2.4
Tour and Travel Guides	461	1.2	1.8	2.8
Ushers, Lobby Attendants, and Ticket Takers	462	0.83	0.4	0.8
Baggage Porters, Bellhops, and Concierges	464	1.16	1.4	2
Recreation and Fitness Workers	466	2.75	2.16	3.58

Motion Picture Projectionists	467	0.8	0.8	1.4
Child Care Workers	468	1.83	0.2	1
Funeral Service Workers	469	2.66	0.4	1.6
First-Line Supervisors/Managers of Personal Service Workers	470	2.8	2	3.6
Transportation Attendants	471	1.4	0.8	1.8
Animal Trainers	472	3.16	1	1.2
Farmers and Ranchers	473	2.6	2.4	2.4
Farm, Ranch, and Other Agricultural Managers	475	3.33	3.2	4
Miscellaneous Agricultural Workers	479	2	2.2	1.8
Graders and Sorters, Agricultural Products	488	1.2	2	1.8
Agricultural Inspectors	489	2.6	3	4.6
First-Line Supervisors/Managers of Farming, Fishing, and Forestry	496	3.2	2.2	2.6
Fishers and Related Fishing Workers	498	2	1.4	1.4
First-Line Supervisors/Managers of Mechanics, Installers, and	503	3	2.6	2.8
Automotive Service Technicians and Mechanics	505	2	2.16	2.33
Bus and Truck Mechanics and Diesel Engine Specialists	507	2.33	0.6	1.2
Aircraft Mechanics and Service Technicians	508	2.8	2.8	1.8
Small Engine Mechanics	509	2.33	1.2	1.6
Automotive Body and Related Repairers	514	2.16	2.2	1.6
Heavy Vehicle and Mobile Equipment Service Technicians and Mechanics	516	1.27	1.2	1.8
Industrial and Refractory Machinery Mechanics	518	1.83	1.6	1.4
Maintenance Workers, Machinery	519	1.33	1.8	2.2
Electrical and Electronics Repairers, Industrial and Utility	523	2.2	2.4	2.6
Computer, Automated Teller, and Office Machine Repairers	525	1.5	1.8	2
Home Appliance Repairers	526	1.5	0.6	1
Radio and Telecommunications Equipment Installers and Repairers	527	2.16	1.4	1.4
Avionics Technicians	533	2.4	3	1.8
Heating, Air Conditioning, and Refrigeration Mechanics and Installers	534	1.5	1.4	1.4
Precision Instrument and Equipment Repairers	535	3.83	1.6	3.2
Locksmiths and Safe Repairers	536	1.83	0.4	0.8
Control and Valve Installers and Repairers	539	2	2	2
Elevator Installers and Repairers	543	2.33	1	2
Millwrights	544	1.5	2.66	2.5
Maintenance and Repair Workers, General	549	1.83	2.33	2.16
First-Line Supervisors/Managers of Construction Trades and Extraction	558	3.2	2.8	3.2
Brickmasons, Blockmasons, and Stonemasons	563	1	0.8	1

Carpenters	567	2	1.8	1.4
Drywall Installers, Ceiling Tile Installers, and Tapers	573	1.5	1	0.4
Electricians	575	2.2	2.6	2.4
Electric Motor, Power Tool, and Related Repairers	577	2.16	0.6	2
Painters, Construction and Maintenance	579	1.33	0.6	0.2
Paperhangers	583	1.16	1	1
Plasterers and Stucco Masons	584	1.66	0.8	0.6
Pipelayers, Plumbers, Pipefitters, and Steamfitters	585	2	2.75	1
Cement Masons, Concrete Finishers, and Terrazzo Workers	588	1.66	1.4	1.2
Glaziers	589	1.66	2.2	1.4
Insulation Workers	593	1.33	1.2	0.4
Paving, Surfacing, and Tamping Equipment Operators	594	1	0.6	0.6
Roofers	595	1.16	0.6	0.4
Structural Iron and Steel Workers	597	1.16	2.16	1.66
Earth Drillers, Except Oil and Gas	598	1	1.83	1.83
Fence Erectors	599	1	0.8	0.6
Derrick, Rotary Drill, and Service Unit Operators, Oil, Gas, and	614	1.66	0.8	1.8
Explosives Workers, Ordnance Handling Experts, and Blasters	615	1.5	2.4	2
Shuttle Car Operators	616	0.5	0.6	1
Tank Car, Truck, and Ship Loaders	617	0.83	1	1.4
First-Line Supervisors/Managers of Production and Operating Workers	628	2.83	3.2	3.8
Tool and Die Makers	634	2.2	3.4	1.8
Machinists	637	3.75	3.83	3.58
Boilermakers	643	2.16	0.8	0.6
Tool Grinders, Filers, and Sharpeners	644	2.16	2.4	1.4
Model Makers and Patternmakers, Metal and Plastic	645	2.16	2.6	1.2
Etchers and Engravers	649	1.41	2	1.4
Sheet Metal Workers	653	2.33	1.4	1.8
Cabinetmakers and Bench Carpenters	657	1.66	1.8	1.2
Model Makers and Patternmakers, Wood	658	1.66	1.6	0.8
Tailors, Dressmakers, and Sewers	666	1.2	1.2	0.8
Upholsterers	668	2	2.2	1.6
Shoe and Leather Workers and Repairers	669	1.83	1.8	1.4
Molders, Shapers, and Casters, Except Metal and Plastic	675	1.16	1.2	1
Opticians, Dispensing	677	3.16	1.8	2.2
Health Diagnosing and Treating Practitioner Support Technicians	678	3.33	0.8	1.8

Bookbinders and Bindery Workers	679	1.33	0.6	0.8
Butchers and Other Meat, Poultry, and Fish Processing Workers	686	0.66	1	1.33
Bakers	687	1.5	1	0.2
Food Batchmakers	688	2.2	2	2
Water and Liquid Waste Treatment Plant and System Operators	694	2	2	3
Power Plant Operators, Distributors, and Dispatchers	695	1.83	1.8	1.8
Stationary Engineers and Boiler Operators	696	1.2	1.2	1.6
Miscellaneous Plant and System Operators	699	1.66	1.2	2.2
Lathe/Turning Machine Tool Setters, Operators, and Tenders, Metal and Plastic	703	2.5	3	2.2
Cutting, Punching, and Press Machine Setters, Operators, Metal and Plastic	706	1.4	1.4	1.6
Rolling Machine Setters, Operators, and Tenders, Metal and Plastic	707	1.83	1.8	2
Drilling and Boring Machine Tool Setters, Operators, Metal and Plastic	708	2.33	2.2	1.6
Grinding/Lapping/Buffering Machine Tool Setters, Operators, Metal and Plastic	709	2.2	2.8	1.6
Forging Machine Setters, Operators, and Tenders, Metal and Plastic	713	1.8	1.8	1.4
Molders and Molding Machine Setters, Operators, and Tenders, Metal and Plastic	719	1.66	1.4	1.2
Plating and Coating Machine Setters, Operators, and Tenders, Metal and Plastic	723	1.83	1	0.6
Heat Treating Equipment Setters, Operators, and Tenders, Metal and	724	1.6	1.6	1.8
Sawing Machine Setters, Operators, and Tenders, Wood	727	0.66	1	1.2
Woodworking Machine Setters, Operators, and Tenders, Except Sawing	729	1	1.2	1.2
Job Printers	734	1.83	0.8	1.4
Prepress Technicians and Workers	736	1	0.6	2
Textile Winding/Drawing Out Machine Setters, Operators, and Tenders	738	2	1.8	1.8
Textile Knitting and Weaving Machine Setters, Operators, and Tenders	739	2	1.8	1.8
Textile Bleaching and Dyeing Machine Operators and Tenders	743	2	1	2
Sewing Machine Operators	744	1.8	1.2	1.4
Shoe Machine Operators and Tenders	745	0.8	0.8	1.4
Pressers, Textile, Garment, and Related Materials	747	1	0.8	0.6
Cementing and Gluing Machine Operators and Tenders	753	1	2	2
Packaging and Filling Machine Operators and Tenders	754	1	1.5	1.83
Machine Setters, Operators, and Tenders, Synthetic and Glass Fibers	755	1.6	1.2	1.6
Crushing, Grinding, Polishing, Mixing, and Blending Workers	756	1	1.4	2.2
Chemical Processing Machine Setters, Operators, and Tenders	757	2.5	2.4	2.4
Food and Tobacco Roasting, Baking, and Drying Machine Operators and	763	1.66	1.8	2.2
Cleaning, Washing, and Metal Pickling Equipment Operators and Tenders	764	0.8	2	1.8
Paper Goods Machine Setters, Operators, and Tenders	765	1.58	0.8	1
Metal Furnace and Kiln Operators and Tenders	766	1.6	1.4	1.6

Food Cooking Machine Operators and Tenders	769	1	1.4	2.2
Photographic Process Workers and Processing Machine Operators	774	2	1.2	0.8
Tire Builders	779	0.5	0.4	0.6
Welding, Soldering, and Brazing Workers	783	2.4	0.8	1.4
Aircraft Structure, Surfaces, Rigging, and Systems Assemblers	785	2	1.6	1.4
Painting Workers	789	1.4	1.2	2
Inspectors, Testers, Sorters, Samplers, and Weighers	799	3.16	3.33	3.5
Supervisors, Transportation and Material Moving Workers	803	2.5	2.6	2.8
Driver/Sales Workers and Truck Drivers	804	2	1.2	2.2
Bus Drivers	808	0.83	1.16	1.83
Taxi Drivers and Chauffeurs	809	1	2.2	1.4
Parking Lot Attendants	813	1.83	0.8	2.8
Railroad Conductors and Yardmasters	823	2	1.4	3.8
Locomotive Engineers and Operators	824	1.83	0.6	1.6
Railroad Brake, Signal, and Switch Operators	825	1.4	1.6	1.4
Sailors and Marine Oilers	829	1.5	1	1.8
Other Transportation Workers	834	3.83	3	3
Operating Engineers and Other Construction Equipment Operators	844	1.4	0.4	0.6
Crane and Tower Operators	848	1.8	1.8	2
Dredge, Excavating, and Loading Machine Operators	853	1	0.4	0.6
Pumping Station Operators	859	1.2	1	2
Helpers--Installation, Maintenance, and Repair Workers	865	1.6	1.4	1.4
Helpers, Construction Trades	866	1	0.6	1.2
Construction Laborers	869	0.33	0.4	0.4
Helpers--Production Workers	873	1.4	1.2	1.4
Refuse and Recyclable Material Collectors	875	0.33	0	0.2
Machine Feeders and Offbearers	878	1.6	1	1.6
Service Station Attendants	885	0.8	1.8	2.4
Cleaners of Vehicles and Equipment	887	1.2	1.4	1.6
Packers and Packagers, Hand	888	1.6	1.2	1.4
Rail-Track Laying and Maintenance Equipment Operators	889	0.66	0.6	0.4