NBER WORKING PAPER SERIES

EXPLAINING THE DECLINE IN THE U.S. EMPLOYMENT-TO-POPULATION RATIO: A REVIEW OF THE EVIDENCE

Katharine G. Abraham
Melissa S. Kearney

Working Paper 24333
http://www.nber.org/papers/w24333

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2018

The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research. The paper has benefited from comments by Alex Gelber, Judy Hellerstein, Chinhui Juhn, David Lindauer, Robert Moffitt, Matthew Notowidigdo, and David Neumark, as well as participants in a June 2017 Brookings roundtable discussion. The authors are grateful to Elisa Jacome, Terry Pack, Colin Wick and George Zuo for excellent research assistance. Financial support from the Smith Richardson Foundation is gratefully acknowledged.

Explaining the Decline in the U.S. Employment-to-Population Ratio: A Review of the Evidence
Katharine G. Abraham and Melissa S. Kearney
NBER Working Paper No. 24333
February 2018
JEL No. J01,J21

ABSTRACT

This paper first documents trends in employment rates and then reviews what is known about the various factors that have been proposed to explain the decline in the overall employment-to-population ratio between 1999 and 2016. Population aging has had a notable effect on the overall employment rate over this period, but within-age-group declines in employment among young and prime age adults have been at least as important. Our review of the evidence leads us to conclude that labor demand factors, in particular trade and the penetration of robots into the labor market, are the most important drivers of observed within-group declines in employment. Labor supply factors, most notably increased participation in disability insurance programs, have played a less important but not inconsequential role. Increases in the real value of the minimum wage and in the share of individuals with prison records also have contributed modestly to the decline in the aggregate employment rate. In addition to these factors, whose effects we roughly quantify, we also identify a set of potentially important factors about which the evidence is too preliminary to draw any clear conclusion. These include improvements in leisure technology, changing social norms, increased drug use, growth in occupational licensing, and the costs and challenges associated with child care. Our evidence-driven ranking of factors should be useful for guiding future discussions about the sources of decline in the aggregate employment-to-population ratio and consequently the likely efficacy of alternative policy approaches to increasing employment rates.

Katharine G. Abraham
Department of Economics and
Joint Program in Survey Methodology
University of Maryland
1218 LeFrak Hall
College Park, MD 20742
and NBER
kabraham@umd.edu

Melissa S. Kearney
Department of Economics
University of Maryland
3105 Tydings Hall
College Park, MD 20742
and NBER
kearney@econ.umd.edu

I. INTRODUCTION

For several decades now, the employment rate among prime-age U.S adults has been falling. Less-educated males have experienced the largest drop in employment, but the troubling trends in participation are not limited to this group. Employment rates among women, which had been rising since the late 1960s, have stagnated and in some recent years declined. These worrisome developments were exacerbated by the Great Recession, but their roots preceded its onset. Understanding the reasons behind these long-term trends remains a priority for labor economists and policy makers alike.

In this paper, we review the evidence regarding the role of various potential factors in driving the structural decline in employment-to-population ratios over the period 1999 to 2016, with an emphasis on the experiences of prime-age individuals.[1] Our review is guided by two questions. First, what is the evidence on the causal relationship between a particular factor or set of factors and employment rates? Second, can changes in these underlying factors explain the trend in employment? Throughout our discussion of existing evidence, we highlight open questions on which more research is needed.

Based on our survey of the existing literature, we produce a ranking of the likely contribution of various factors to the ongoing declines in the employment rate. In instances where the literature has produced a credible causal estimate of the effect of a particular factor on employment, we apply that estimated effect to data on actual changes in that factor and thereby produce a plausible guess as to how much that factor has contributed to the decline in the employment-to-population ratio from 1999 to 2016. This approach is very different from that the approach taken by other recent papers that have used a cohort-based modeling approach to explaining changing labor force participation over time (see, for example, Aaronson, Davis and Hu 2012 and Aaronson et al 2014). Cohort models have considerable appeal for analyses that are undertaken in the context of developing macroeconomic or budget forecasts, but they are less well suited to drawing conclusions about the relative importance of the various labor demand, labor supply and institutional explanations that have been suggested for falling participation.

II. DESCRIBING THE TRENDS

We begin our discussion with an examination of some basic facts about the trends in the employment-to-population ratio in the U.S. labor market. Tables 1A, 1B and 1C display simple tabulations for the overall, male, and female population age 16 and older, showing annual average employment-to-population ratios and population shares by age and education. The reported numbers are based on monthly Current Population Survey data for 1999 (the year

[1] Some papers on trends in workforce attachment focus on the labor force participation rate rather than the employment-to-population ratio as the outcome of interest (e.g., Juhn and Potter 2006). Although the two measures behave differently and convey different information at a cyclical frequency, over the longer run, they generally have moved together.

before the start of the dot-com recession of the early 2000s) and 2016 (seven years into the post-Great-Recession economic recovery).[2] Over this period, the overall annual employment-to-population ratio fell from 64.3 percent to 59.7 percent, a decline of 4.5 percentage points.[3] Employment rates fell for both sexes, though the decline was steeper for men (5.9 percentage points) than for women (3.3 percentage points). As shown in Figure 1, the finding of a decline in the overall employment-to-population ratio is not specific to our choice of starting year or ending year. Had we been conducting our examination a few years earlier, however, the cumulative decline to be explained would have been considerably larger. This is because the overall employment-to-population ratio dropped sharply during the 2007-2009 recession and, as can be seen in Figure 1, has subsequently recovered, though not to its pre-recession level.

The marked declines in employment rates among prime age workers that are apparent in Figure 1 have prompted growing discussion and concern. The employment rate for each of the reported 10-year age groups within the 25 to 54 year old age band dropped 3 to 4 percentage points between 1999 and 2016. The decline for men age 25 to 34 (5.6 percentage points) was more than twice as large as the decline for women the same age (2.3 percentage points); among those age 35 to 44 and those age 45 to 54, the declines for men and women were more similar.

Among 16 to 24 year olds, the overall employment rate fell by 9.6 percentage points between 1999 and 2016, from 59.0 percent in 1999 to 49.4 percent in 2016. The employment rate for young men fell by 11.0 percentage points and that for young women by 8.2 percentage points. The decline for teenagers and young adults enrolled in school (11.6 percentage points) has been much larger than the decline for those in the same age range who are not enrolled in school (4.6 percentage points).

In contrast to the declines within the prime-age and young groups between 1999 and 2016, there was an *increase* in the employment-to-population ratio of 4.1 percentage points for those age 55 to 64, from 57.7 percent to 61.8 percent. This was attributable primarily to increasing employment among women; the corresponding employment rate for men changed much less. The overall employment rate among those age 65 and older rose even more—from 11.9 percent to 18.6 percent, an increase of 6.6 percentage points—with similar increases recorded for both men and women.

Despite the rise in employment at older ages, those age 55 to 64 and, especially, those 65 and older remain much less likely to be employed than those in their prime working years. As shown in the tables, the share of the population age 55 and older increased substantially between 1999 and 2016. Taken together, these facts imply that population aging has contributed to the reduction in the overall employment-to-population ratio.

---

[2] We thank Sarah Flood of the IPUMS-CPS staff for providing us with the composite weights used by the Bureau of Labor Statistics to generate the official employment statistics.

[3] The discrepancy between the change obtained by subtracting the two reported levels and the reported change is due to rounding.

To quantify the contributions of changing within-group employment rates and changing population shares to the overall decline in the employment-to-population ratio, we perform a simple decomposition exercise. For any disaggregation into mutually exclusive groups, the overall change in the employment-to-population ratio can be written as:

$$(1)\quad \Delta(E/P)_{t_0,t_1} = \sum_i s_{i,t_0}\,\Delta(E/P)_{i,t_0,t_1} + \sum_i (E/P)_{i,t_0}\Delta s_{i,t_0,t_1} + \sum_i \Delta s_{i,t_0,t_1}\,\Delta(E/P)_{i,t_0,t_1}$$

where E is employment, P is population, s is share of the overall population, *i* indexes groups, and t_0 and t_1 are the start and end of the time period over which the change is measured. This can be written equivalently as:

$$(2)\quad \begin{aligned}\Delta(E/P)_{t_0,t_1} &= \sum_i s_{i,t_0}\,\Delta(E/P)_{i,t_0,t_1} + \sum_i [(E/P)_{i,t_0} - (E/P)_{t_0}]\Delta s_{i,t_0,t_1} \\ &\quad + \sum_i \Delta s_{i,t_0,t_1}\,\Delta(E/P)_{i,t_0,t_1}\end{aligned}$$

The first set of terms in equation (2) captures the contribution of within-group employment rate changes to the change in the overall employment rate; the second set of terms, the contribution of changes in group population shares; and the third set of terms, the contribution of interactions between employment rate changes and population share changes.

Table 2A reports the results of this decomposition for the period from 1999 through 2016 using data disaggregated into 26 age-sex groups for the overall column and 13 age groups for the male and female columns.[4] A common narrative regarding the recent decline in the employment-to-population ratio is that it has been driven predominantly by the aging of the population and population aging indeed has had an effect. The numbers in the second panel of Table 2A imply that, had within-group employment rates remained at their 1999 levels, changes in the distribution of the population across age-sex categories between 1999 and 2016 would have caused the overall employment-to-population ratio to fall by 68.9 percent as much as the net overall decline that was actually observed. This translates into a drop in the aggregate employment rate of about 3.1 percentage points.

Because the net change in the overall employment-to-population ratio reflects both negative and positive influences, however, this does not mean that other factors have been unimportant. In fact, the effects of within-group employment rate declines among young and prime age adults on the overall employment-to-population ratio have been even larger than the effects of population aging. The numbers in the first two rows of Table 2A imply that, had the distribution of the

[4] Note that the age groups used in the calculations are more disaggregated than the age groups for which estimates are reported in the table; the numbers reported were derived by aggregating across the more disaggregated cells used in the calculations.

population across age-sex groups stayed the same as in 1999, within-group declines in employment rates among those in the 16 to 54 year old age range between 1999 and 2016 would have caused the overall employment-to-population ratio to fall by about 80.8 percent of the net observed overall decline. This translates into a drop of about 3.6 percentage points.

Partially offsetting these large negative effects are two factors that worked to raise the overall employment rate between 1999 and 2016. First, increases in employment rates among those age 55 and older raised the overall employment rate by 29.6 percent of the net overall decline, or about 1.3 percentage points. Second, shifts in population away from groups with falling employment rates and towards groups with rising employment rates—the interaction effects captured by the third set of terms in equation (2)—raised the overall employment rate by 20.1 percent of the net overall decline, or about 0.9 percentage point. The effects of rising employment rates among those age 55 and older are shown in the third and fourth rows of Table 2A; the interaction effects are shown in the table's bottom panel.

Table 2B reports the results of a decomposition similar to that displayed in Table 2A, but for groups that are further disaggregated by educational attainment in addition to age and (if applicable) sex. Absent other changes, the declines we observe in employment among in-school 16 to 24 year olds would have produced a decline in the overall employment rate equal to 22.6 percent of the observed net decline; declines in employment among out-of-school 16 to 24 year olds have played a smaller role. Among those age 25 to 54, declines in employment among those who are high school graduates or have some college together would have produced a decline in the overall employment rate equal to 44.2 percent of the observed net decline; declines among high school dropouts and college graduates in this age group have been less important. Changes in employment rates within just three groups—in-school 16 to 24 year olds plus those age 25 to 54 who are high school graduates or have some college—can account for more than 65 percent of the net overall decline in the employment-to-population ratio. This translates into a drop in the aggregate employment rate of about 3.0 percentage points. Much the same statements can be made about the changes observed for men and for women.

As in the Table 2A decompositions, increasing employment rates in the disaggregated cells for adults age 55 and older boost overall employment in the Table 2B decompositions, but the effect is considerably more modest. At older ages, more educated people have higher employment rates than less educated people. A substantial portion of the increase in employment among those age 55 and older shown in Table 2A can be tied to rising education levels at these older ages. Putting this slightly differently, when educational attainment is used to define the calculation cells, as is done in Table 2B, within-group employment rate changes at older ages have a smaller positive effect on the overall employment rate.

Changes in the distribution of the population across the groups used in the decomposition analysis also matter for the overall decline in the employment-to-population ratio in the Table 2B decomposition, but the effects of composition changes are smaller than in the Table 2A calculations. This is because the population not only is becoming older, which works to lower the overall employment rate, but also is becoming more educated, which works to raise the overall employment rate. Similar to the Table 2A decompositions, the interaction terms in the Table 2B calculations also work to raise the employment rate, reflecting shifts in the distribution of employment towards cells in which the employment-to-population ratio has risen.

In sum, our examination of the data on changes in the employment-to-population ratio leads to several conclusions:

1. First, in a decomposition by age and sex, decreases in within-age-group employment rates among those age 16 to 54 can account for 80.8 percent of the net overall decline in the employment-to-population ratio between 1999 and 2016, or approximately a 3.6 percentage point drop.

2. Second, declines in employment among school enrollees account for the majority of the contribution of those age 16 to 24 to the overall employment rate decline.

3. Third, declines in employment rates for those with a high school degree and some college account for the largest shares of the contribution of those age 25 to 54 to the overall decline in the employment-to-population ratio. Declines among high school dropouts and college graduates in this age range have made more modest contributions to the overall decline.

4. Fourth, increases in employment rates among those age 55 and older have worked to raise employment, making the net decline in the aggregate employment-to-population ratio smaller than it otherwise would have been.

5. Fifth, while within-group employment rate changes have been very important, population aging also made a significant contribution to the decline in the overall employment-to-population ratio between 1999 and 2016. Accounting only for changes in the age-sex composition of the population may overstate the importance of changes in population composition, however, since the population also has become more educated and those with higher educational attainment are more likely to be employed.

Our central concern in the remainder of the paper lies with understanding the factors that have been responsible for the within-group employment rate declines observed for young and prime-age adults over the 1999 to 2016 period. Although employment rates have been rising for those age 55 and older, we note that some of the same factors that have caused employment at younger ages to fall also could have dampened the growth in employment among this older population.

III. FACTORS BEHIND THE TRENDS

Our next task is to review available evidence on the factors that might be important drivers of the observed reductions in employment rates. These declines could have been driven by shifts in labor demand; by shifts in labor supply; or by changes in institutional factors or in the severity of labor market frictions. We consider in turn specific explanations for falling employment rates in each of these categories.

The obvious potential sources of adverse shifts in labor demand that could have contributed to falling employment rates are increased exposure to trade and the development of labor-saving technology. To the extent that these factors were responsible for inward shifts in the labor demand curve, we would expect them to have produced reductions in both wages and employment.

Alternatively, some of the observed decline in employment rates could be the result of inward shifts in the labor supply curve, resulting either from improvements in the options available to non-workers or from increases in the costs of participating in the labor force that deter some people from seeking employment. Supply-side explanations for low or falling U.S. employment rates that postulate increases in the attractiveness of the options available to non-workers have included growth in the availability and/or generosity of social insurance programs including disability insurance, the Supplemental Nutrition Assistance Program (SNAP) and publicly provided or subsidized health insurance; for men, increases in the number who have a working spouse; and growth in the availability of inexpensive entertainment options such as video games that make staying home more attractive for some people relative to working. Others have argued that the lack of workplace and childcare support for working parents makes it costly for them to hold a job, depressing their supply of labor to the market. In addition, increases in the number of immigrants in the workforce could have contributed to declines in employment among groups of workers for whom immigrants are a close substitute.

Institutional factors such as increases in the effective minimum wage and increases in the prevalence of occupational licensing requirements also have been cited as contributors to falling employment rates. Finally, some have suggested that increasing mismatch between available jobs and available workers, across both skill type and geographic space, could have played a role in driving down rates of employment. The remainder of this section of the paper considers the likely roles of a variety of labor demand, labor supply, institutional and labor market mismatch explanations for falling employment rates.

A. Labor Demand Factors

To the extent that adverse shifts in labor demand have driven declines in employment, we would expect falling employment rates to have been accompanied by falling wages. Moffitt (2012) examines the role of wages as a proximate cause of the falling employment rates observed over the period from 1999 to 2007. He concludes that falling wages can explain much of the decrease in employment rates observed for men and for both married women and unmarried women without children, though not the decline in employment rates for unmarried women with children, whose wages actually rose over the period he studied. While clearly partial equilibrium in nature, Moffitt's findings suggest that shifts in labor demand were likely to have been an important contributor to the observed declines in the employment rates for many groups over the period he studied. In more recent years, data on the real median earnings of full-time workers suggest that wages generally have been stagnant, with only a modest uptick, concentrated among college-educated workers, since 2014.

The outstanding question is what might have caused the adverse shifts in labor demand, especially for less educated workers. Two factors that have received extensive attention in the literature are trade and technology. Both are widely agreed to have adversely affected the demand for moderate- and low-skilled labor – shifting the demand curve for these workers to the left – though there is considerably less agreement about the magnitude and relative importance of these effects.

<u>Trade</u>

One of the major economic questions of recent years has been the extent to which the increased openness of the U.S. economy to trade, especially trade with China, has put downward pressure on U.S. wages and employment. Much of this literature is motivated by the sharp decline in manufacturing employment from about 17.3 million in 1999 to about 12.3 million in 2016, a loss of 5 million manufacturing jobs. Interestingly, Charles, Hurst, and Notowidigdo (2016) document that the decline in manufacturing jobs during the period 2000-2007 was almost entirely offset by increases in employment in the housing sector that masked the loss of manufacturing jobs. Between 2007 and 2011, the housing boom abated, but the decline in manufacturing jobs continued. Charles, Hurst and Notowidigdo (2016) estimate that roughly 40 percent of the decline in employment in the period 2007 to 2011 is attributable to losses in manufacturing.

A number of recent papers have linked the decline in manufacturing sector employment to international trade pressures. In an analysis that looks at the period from 1999 through 2007, Autor, Dorn, and Hanson (2013) document that growth in imports from China led to higher unemployment, lower labor force participation, and reduced wages in local labor markets that had a larger share of their initial employment in import-competing manufacturing industries and thus were more exposed to import competition. An earlier paper by Bernard, Jensen, and Schott (2006) similarly found that imports from low-income countries (including China) led to reductions in U.S. employment rates during the period 1977 to 1997. Autor et al. (2014) build on the work of Autor, Dorn, and Hanson (2013) by looking at individual level data. They define exposure to trade as the growth in U.S. imports from China from 1991 to 2007 that occurred in a worker's initial industry. Over the 1992 to 2007 period, individuals who worked in 1991 in manufacturing industries where the exposure to growth in imports from China was larger experienced lower cumulative earnings, were more likely to obtain disability benefits, and were more likely to work outside their narrowly-defined manufacturing industry and outside manufacturing altogether. Earnings losses were larger for those with low initial wages, low initial tenure, and low attachment to the labor force.

More recent work by Pierce and Schott (2016) links the large decline in U.S. manufacturing employment after 2000 to the change in U.S. trade policy that granted Permanent Normal Trade Relations (PNTR) to China, thereby eliminating potential tariff increases on Chinese imports, effective in 2001 with China's accession to the World Trade Organization (WTO). The fact motivating their paper is the large decline in U.S. manufacturing employment after 2000, following decades of relative stability. Using a difference-in-differences strategy, the authors find that employment fell by more in industries that were more exposed to the change in policy. The authors capture exposure as the difference between the NTR tariff (applied after WTO accession) and the non-NTR tariff (potentially applied before WTO accession). In practice China was granted the NTR tariff rates annually between 1980 and 2010, so exposure to the policy change is not about a change in tariff rates per se, but rather a reduction in the threat of higher tariffs.[5] These findings imply substantial employment losses owing to the policy change, but as Pierce and Schott acknowledge, their difference-in-differences identification strategy precludes an estimate of the effect of the policy change on overall U.S. employment. This is because the estimated effects are all about *relative* job losses and there is not an obvious way to translate their findings into an estimate of overall *absolute* job losses.

The papers just described are focused primarily on manufacturing and how import competition has affected the manufacturing sector. Even if the direct effects of increases in global competition fall primarily on manufacturing, the resulting employment effects may be much broader. Building on some of the research described above, Acemoglu et al. (2016) quantify how much of the reduction in manufacturing employment between 2000 and 2007 is attributable to rising import competition from China and also trace out that competition's broader effects. They find that the surge in import competition from China after the year 2000 was a driving force

[5] Drawing on the literature on investment under uncertainty, the authors consider a number of potential channels through which this policy change could have negatively affected U.S. manufacturing employment. In brief, they argue, the removal of this uncertainty did three things: (1) it increased the incentive for U.S. firms to incur the sunk costs associated with shifting operations to China or establishing a relationship with a Chinese producer; (2) it provided greater incentives for Chinese firms to invest in entering the U.S. market; and (3) it increased the attractiveness of investments in capital- or skill-intensive technologies at home that are more consistent with a U.S. comparative advantage. Using U.S. trade data, they find that PNTR is associated with relative increases in the value of Chinese imports as well as in the relative number of U.S. importers. Using U.S. microdata, they confirm that PNTR is associated with a relative increase in the number of pairs of U.S. and Chinese firms in trading agreements (per mechanism (1)). Using microdata from China, they confirm that PNTR is associated with relatively more Chinese exports from foreign-owned firms (per mechanism (2)). And using plant-level U.S. data, the authors document that the associated decline in U.S. manufacturing is heightened by input-output linkages and shifts toward less labor-intensive production (per mechanism (3)).

behind reductions in U.S. manufacturing employment, and more generally, for weak overall job growth, owing to input-output linkages.

The first part of the Acemoglu et al. (2016) paper estimates employment across four-digit manufacturing industries from 1991 to 2011 as a function of industry exposure to Chinese import competition. The authors use an IV estimation strategy, instrumenting for industry exposure with industry exposure to Chinese import competition in eight other high-income countries.[6] Their results imply that greater Chinese import penetration accounts for approximately 10 percent of the decline in U.S. manufacturing employment.

The authors then consider employment losses associated with a contraction of U.S. manufacturing through both upstream and downstream industry effects. The contraction of U.S. manufacturing in response to exposure to Chinese import competition could lead to a reduction in demand for intermediate inputs produced in the United States (upstream effects) as well as affecting the industries that purchase U.S. manufacturing goods (downstream effects). The upstream effects on the suppliers to U.S. manufacturing are unambiguously negative, but the downstream impact on the industry's customers could be either positive or negative, since it depends on how those firms interact with the imports from China. Using data from the 1992 U.S. input-output tables to measure linkages across industries, the authors confirm empirically negative employment effects on "upstream" industries and find no discernible employment effects on "downstream" industries.

The second part of the Acemoglu et al. (2016) paper considers a general equilibrium treatment of potential job losses coming through reallocation effects (which would offset the losses captured with their industry exposure analysis) or aggregate demand effects (which would amplify the losses). They find no empirical support for a reallocation effect. At the commuting zone (CZ) level, there is no discernible effect of import exposure in a CZ on employment in non-exposed industries. There is, however, evidence of negative aggregate demand effects. Inclusive of direct industry exposure effects, linked industry exposure effects, and local level reallocation and aggregate demand effects, the authors estimate that import competition with China caused a reduction in employment of 2.37 million workers from 1999 to 2011. They characterize this as a conservative lower bound estimate, since their local-area-based analysis does not capture some components of the industry interlinkage effects and national-level aggregate demand effects.[7]

[6] The suitability of this instrument requires that country-specific import demand shocks are uncorrelated across high-income economies and that U.S. imports from China do not lead to higher levels of exports from China to other countries, such as through an economy of scale effect.
[7] There is a potential offsetting effect that could lead to this estimate overstating aggregate job loss. Lower prices for consumer goods that are subject to import competition could increase the amount that consumers spend on other domestically-produced goods and services (because of a positive income effect), thereby raising employment levels in the industries that produce them. A general equilibrium effect along these lines would not be captured by Acemoglu et al.'s local-area-based empirical analysis.

Over the 1999 to 2011 period considered by Acemoglu et al. (2016), Chinese imports valued in 2007 US dollars (using the PCE as the deflator) increased by 270 percent. If we look instead at the period 1999 to 2016, Chinese imports valued in 2007 US dollars increased by 302 percent, implying that the increase over the longer period has been about 12 percent larger. So, a rough estimate of the number of workers displaced by increasing Chinese imports would be 1.12*2.37 million, or about 2.65 million workers. Adding those workers to the 2016 workforce would increase the employment-to-population ratio by 1.04 percentage points.

Technology

There has been widespread academic and public interest in the question of how technology, including computerization and robots, has affected and will continue to affect employment. One can readily find a wide range of viewpoints in the public discourse, ranging from alarmist predictions of massive unemployment caused by robots to sanguine predictions about net new job creation. The academic evidence about the role of technology on net employment rates, as opposed to the impact of technological advances on wages and inequality, is actually somewhat thin and suggests modest negative employment effects, at least to date.

Autor, Dorn, and Hanson (2015) consider the extent to which trade pressures and technological advancements have worked in tandem, looking at local-level exposure to trade competition and local-level susceptibility to computerization side by side from 1980 to 2007. Like a number of the papers described above, they use local area data to estimate the effect of exposure to employment threats – in this case trade and computerization – on local labor market outcomes. They estimate employment outcomes at the commuting-zone (CZ) level as a function of CZ exposure to trade competition from China (measured and instrumented for in the same way as in Acemoglu et al. 2016) and CZ exposure to computerization, as measured by industry specialization in routine-task-intensive production and clerical occupations. The authors demonstrate that the effects of exposure to competition from trade and technology can be separately identified because the two are largely uncorrelated at the local level.

The findings of the Autor, Dorn and Hanson (2015) analysis reveal distinct employment effects of exposure to trade and technology competition. Trade competition leads to sharp declines in local manufacturing employment, resulting in net increases in local area unemployment and non-employment. Furthermore, the associated employment losses are much larger for non-college educated workers. During the period from 1980 to 2007, a $1,000 increase in per-worker import exposure is estimated to have reduced the employment rate by 0.53 percentage point among college-educated workers and by 1.21 percentage points among non-college workers.

In contrast, CZ exposure to routine task specialization is associated with no overall change in employment rates. A more detailed look at employment effects by gender reveals that, although the data do not show a statistically significant negative effect of commuting-zone exposure to routine task replacement on the aggregate employment rate, there is a significant negative effect on the employment rate of women. Moving from a commuting zone at the 25th to 75th percentile of routinization exposure, the more exposed commuting zone would see a relative decline in the female employment-to-population ratio of 1.8 percentage points per decade.

As outlined by Autor, Dorn and Hanson (2015), results from a task-based analysis help to explain the divergent aggregate employment effects found for trade versus technology exposure. The task-based analysis reveals that exposure to trade competition has negative effects across all occupations. In contrast, exposure to competition from computing technology affects only routine-task-intensive occupations, and employment losses in those occupations tend to be offset by employment gains in abstract and manual-task-intensive occupations.

In a more recent paper, Acemoglou and Restrepo (2017) attempt to quantify the impact of industrial robots on U.S. employment and wages between 1990 and 2007. Industrial robots are defined as being “automatically controlled, reprogrammable, and multipurpose.” They are fully autonomous machines that do not need a human operator (the way a coffee machine does, for example) and they can be programmed to perform several manual tasks (unlike an elevator, for example). Note that industrial robots constitute a different technological threat to employment than computerization, which is the focus of the Autor, Dorn, and Hanson (2015) paper described immediately above.

Previous research on the employment effects of automation typically has emphasized the *potential* for automation. For example, Frey and Osborne (2013) estimate that over the next two decades, 47 percent of US workers are at risk of having their jobs automated, and a recent World Bank report suggests that 57 percent of OECD jobs could be automated over the next two decades (World Bank 2016). As emphasized by Acemoglu and Restrepo, however, such estimates do not speak to the equilibrium impact of automation on aggregate employment rates. First, the extent to which firms would choose to automate will depend on the relative costs of automation versus labor. Second, the equilibrium labor market impacts will depend on the adjustments in other sectors. Their empirical analysis moves beyond the existing research to provide an empirical estimate of the net effect of industrial robots on U.S. employment.

Acemoglu and Restrepo’s (2017) empirical analysis is motivated by a conceptual task-based model in which robots and workers compete in the performance of a range of tasks, the share of tasks performed by robots varies across industries and there is trade between labor markets specializing in different industries. The simple model developed in the paper reveals that a greater penetration of robots into an economy affects wages and employment negatively through a displacement effect, but also positively through a productivity effect. The authors demonstrate that, in this class of models, the local labor market effects of robots can be estimated by regressing the local area change in employment and wages on the exposure to robots in each local labor market, where the local labor market exposure to robots is measured by the sum over industries of the fraction of workers in that local labor market in an industry times the national penetration of robots into that industry.

The local labor market approach taken in this paper is similar to the approach taken in the previously described trade and technology papers by Autor and/or Acemoglu and their coauthors. The data on robot penetration come from the International Federation of Robots (IFR), which provides counts of the stock of robots by industry, country, and year for 50 countries from 1993 to 2014. The data show that, between 1993 and 2007, the stock of robots in the United State and Western Europe increased fourfold, amounting to one new industrial robot for every thousand workers in the United States and 1.6 new industrial robots for every thousand workers in Western Europe. The authors use data from the 1970 and 1990 U.S. Censuses to calculate baseline industry employment shares for 722 CZs. Labor market outcomes are constructed from the 1970, 1990, and 2000 Censuses and the 2007 American Community Survey.

The critical source of identifying variation underlying the empirical analysis is the variation across CZs in the baseline distribution of sectoral employment across industries, which makes a local area more or less exposed to robots given the uneven adoption of robots across industries in subsequent decades. For the resulting estimate to reflect a causal relationship between robot exposure and labor market outcomes, it must be the case that the adoption of robots in a given industry is not related to other economic trends in CZs that specialize in that industry. To surmount this threat to causal identification, the authors implement an instrumental variables approach using the industry-level adoption of robots in a set of advanced countries to instrument for the national-level industry adoption of robots in the United States. In addition, the regression analyses control for a host of potential CZ-level confounding factors, including trade exposure, the decline of routine jobs, offshoring, the adoption of other types of information technology capital, and the total capital stock. Interestingly, the adoption of robots at the CZ level is not highly correlated with these other variables.

The analysis yields the following key estimate: Assuming no trade between CZs, each additional robot per thousand workers between 1993 and 2007 reduced the employment-to-population ratio in a commuting zone by 0.37 percentage point, as compared to a CZ with no exposure to robots. The authors view this estimate as “large but not implausible,” noting that it implies a reduction of 6.2 workers for each new robot, which they say is consistent with case study evidence on the relative productivity of robots. The authors also offer an adjusted estimate that allows for trade between CZs. To make this adjustment, the authors have to rely on assumptions about the elasticity of substitution between goods produced in different CZs, on the amount of cost savings from robots, and on the elasticity of labor supply. Based on parameter values supported by existing studies, the adjusted estimates are somewhat less negative, though still sizable, implying that one more robot per thousand workers reduces the aggregate employment-to-population ratio by about 0.34 percentage point, or 5.6 workers per new robot.

In 2007, the last year that Acemoglu and Restrepo (2017) consider, there was an estimated stock of 160,632 robots installed in the United States. Their estimate is that each robot displaces about 5.6 workers, implying a deficit of 899,529 workers in 2007. In 2016, there was an estimated stock of 250,475 robots; applying their estimate of 5.6 displaced workers per robot translates into

1,402,660 displaced workers.[8] Adding these workers to the 2016 workforce would raise the employment-to-population ratio by 0.55 percentage points.

B. Labor Supply Factors

Another important class of explanations for the decline in the employment-to-population ratio posits inward shifts in the labor supply curve, resulting either from improvements in the options available to non-workers or from increases in the costs of entering the labor force that lead fewer people to seek employment. One potential explanation involving improvements to the options afforded to non-workers is increases in the availability and/or generosity of safety net assistance, be it through federal disability insurance, or expansions in the SNAP food assistance program, or the expansion of publicly provided or subsidized health insurance. Eberstadt (2016), for instance, cites data from the Survey of Income and Program Participating indicating that in 2013, 63 percent of households with non-working prime-age men received means-tested assistance from programs including Medicaid, Temporary Assistance for Needy Families (TANF), SNAP, or the Women, Infants and Children (WIC) food assistance program. He further observes that this reflects a marked jump from 43.6 percent of such households receiving similar means-tested assistance in 1985. Other “outside option” supply-side explanations for falling employment rates include increases in the number of men who have a working spouse and the growth of inexpensive entertainment options such as video games that make staying home more attractive relative to working.

Lack of support for working parents is another potentially important supply-side influence on employment rates. While insufficient supports may deter some parents from entering the labor force, the difficulty of combining work with caring for children would need to have risen over time for this to explain falling employment rates. A final supply-side story sometimes told about falling employment rates is that immigrants have crowded out certain groups of domestic workers, though the available evidence again seems inconsistent with this as an explanation for the overall decline in employment rates.

Federal Disability Insurance Programs

The rise in Social Security Disability Insurance (SSDI) receipt among working-age adults in recent decades coincides with a period of falling employment rates, naturally raising the question

---

[8] The authors thank Pascual Restrepo for sharing the robot data used in Acemoglu and Restrepo (2014) with us. We updated their data series (which runs through 2014) to 2016 using information from more recent IFR reports. The IFR collects data on new robot installations and then calculates the stock of robots by taking last year's stock plus new installations minus installations from 12 years ago (assuming that robots remain in service for 12 years).

of the role that SSDI has played in driving down employment rates.[9] But SSDI is not the only large federal disability insurance program that has seen substantial participation growth in recent decades. Both the federal Supplemental Security Income (SSI) Program and the Veterans Affairs Disability Compensation (VADC) program also have grown during this time.

Eberstadt (2016) emphasizes that the increased reliance on disability payments among working age men (the focus of his book) extends beyond just SSDI. He reports tabulations from the Survey of Income and Program Participation (SIPP) showing that in 2013, 6.3 percent of men age 25-54 reported receiving any disability benefits, as compared to 4.2 percent in 1985. Among men age 25-54 not in the labor force, those shares were 56.5 percent and 38.3 percent. In other words, between 1985 and 2013, there was an 18 percentage point increase in disability benefit receipt among prime-age men out of the labor force.[10] Given that there is a tendency for household survey respondents to under-report participation in welfare and social insurance programs, all of these numbers may be underestimates.

The SSDI program is administered by the U.S. Social Security Administration (SSA). Program eligibility is restricted to individuals who have worked in a job covered by Social Security in at least five of the 10 most recent years. To be eligible, an individual also must have a medically determinable physical or mental impairment that is expected to result in death or to last at least a year that limits his or her ability to engage in "substantial gainful activity" (i.e., more than a very modest amount of labor market work).

The share of working-age adults receiving SSDI benefits rose from 2.2 percent in the late 1970s to 3.6 percent in the years preceding the 2007-2009 recession to 4.6 percent in 2013 (Liebman 2015). In addition to the increase in the size of the caseload, there has been a change in the composition of SSDI recipients over the past few decades, with more recipients now qualifying for benefits with hard-to-verify impairments and with the program playing an increasingly important role in providing income for less educated workers negatively impacted by economic factors (Liebman 2015). Disaggregating by age group, calculations based on the numbers of SSDI recipients released by SSA show that the share of the population on the program increased between 1999 and 2016 for every five-year age category from age 30-34 through age 55-59, as well as for those between age 60 and the applicable full retirement age. For example, the share of individuals age 35 to 39 on the SSDI program increased from 1.8 percent to 2.1 percent over the 1999 to 2016 period; the share for those age 45 to 49 increased from 3.6 percent to 4.2 percent; and the share for those age 55 to 59 increased from 7.7 percent to 9.9 percent.

---

[9] Looking at an earlier period, Autor and Duggan (2003) document that, from the 1970s through the 1990s, the combination of declining labor market demand for less educated workers, increased SSDI benefit replacement rates, and expanded SSDI program eligibility criteria led to falling employment rates and SSDI caseload growth.

[10] Krueger (2017) reports that among 571 not-in-the-labor-force men age 25-54 who participated in an online survey, 50.5 percent reported receiving some type of disability payment.

Rigorous research provides robust evidence that the availability of benefits under the SSDI program has caused individuals who are at the margin of SSDI eligibility to work at lower rates than would have been the case had those benefits not been available. The seminal work of Bound (1989) used denied applicants to approximate the counterfactual employment rates of accepted applicants. Using an OLS approach, he estimates that receipt of a SSDI award reduced the likelihood of work by 34 percentage points. Von Wachter, Song, and Manchester (2011) apply Bound's approach to observational data from the 1980s, 1990s, and 2000s and find a larger impact on labor force participation, which they attribute to more recent cohorts of SSDI beneficiaries having higher work potential, owing to the fact that they are younger and more likely to have nonterminal qualifying conditions.

Maestas, Mullen and Strand (2013) use administrative data to match SSDI applications to disability examiners and exploit variation in examiners' allowance rates as an instrument for benefit receipt. This is an advance over previous papers that exploited differences in award receipt without an exogenous determining factor. The IV approach of Maestas, Mullen and Strand (2013) yields the finding that, among the nearly 23 percent of applicants on the margin of program entry (meaning that their award determination depends on the leniency of the examiner), subsequent employment would have been 28 percentage points higher two years after initial award had they not received benefits.[11] The estimated effect ranges from no effect for applicants with the most severe conditions to 50 percentage points for applicants with the least severe conditions.

A similar finding emerges from the work of French and Song (2014), who use variation in the propensity of administrative law judges (ALJs) in the second stage of the appeals process to estimate the labor supply effect of SSDI receipt. They find that the employment rate of applicants granted benefits at this stage would have been 26 percentage points higher three years after a decision had they not been granted DI benefits. An earlier paper by Chen and van der Klaauw (2008) applied regression-discontinuity methods to linked SIPP and administrative data to estimate the impact of SSDI award receipt on subsequent labor supply. They find that the labor force participation rate of marginal SSDI beneficiaries whose conditions were right around the cutoff level for qualification would have been about 20 percentage points higher had they been denied benefits.

The consistent finding that emerges from these papers reporting well-identified estimates is that a sizable subset of SSDI beneficiaries would have worked in the years immediately following their initial SSDI application had they not been awarded benefits.[12] Another recent paper using

---

[11] To put this estimated effect into perspective, in terms of unadjusted differences, among applicants who are allowed benefits either initially or on appeal, four years after the decision, only 10 percent are working and earning more than $1,000 a year. Among those initially denied who did not appeal, four years after the decision about 50 percent are working and earning more than $1,000 a year; the rate is about 35 percent for those denied both initially and after an appeals process.

[12] This notion is also supported by the work of Moore (2015), who studies the experience of SSDI recipients who were removed from the rolls following the 1996 reform that eliminated drug and alcohol

an entirely different approach finds dis-employment effects of DI benefits of a very similar magnitude. Gelber, Moore, and Strand (2017) exploit discontinuous changes in the DI benefit formula and a regression kink design to estimate the effect of payment size on earnings among beneficiaries. Using administrative data on all new DI beneficiaries from 2001 to 2007, they find that an increase in DI payments of $100 dollars causes an average decrease in beneficiaries' earnings of $22, consistent with a large negative income effect of unearned benefits on labor supply. They emphasize that the confirmation of a labor-reducing income effect, as opposed to a distortionary substitution effect, is important to thinking about how access to increased benefits over time might have led to reduced employment more broadly.[13]

In terms of an extensive margin response, the Gelber, Moore and Strand (2017) analysis implies that $1,000 in SSDI benefits correspond to a 1.29 percentage point reduction in employment. They use the fact that, on average, SSDI beneficiaries receive combined annual cash and medical benefits of $20,950 ($13,750 in cash benefits plus $7,200 in medical benefits), to translate the Maestas, Mullen and Strand (2013) and French and Song (2014) estimates into comparable elasticities. By their calculations, the estimates in those papers imply that $1,000 in SSDI benefits reduces the probability of employment by 1.22 or 1.11 percentage points respectively. If these estimates can be applied more broadly, they imply that the increase in real SSDI benefit amounts during the period 1999 to 2016 would have led to sizable reductions in employment among the relevant population.

To gauge how much of the reduction in employment-to-population ratios can be explained by expanded SSDI access during the period under review, we conduct a back-of-the-envelope calculation using age-specific caseload data from SSA.[14] These data indicate that the SSDI caseload grew by 3.93 million recipients between 1999 and 2016, from 4.88 million to 8.81 million recipients, with almost all of that increase coming from increases in the number of recipients above age 45. We would like to know how much of this growth in caseload has occurred as a result of increasing receipt rates by age group, rather than as a result simply of population growth and aging. To that end, within each five-year age bin, we compare the actual caseload increase to the hypothetical increase that would have occurred had the SSDI receipt rate in that age bin remained constant. Summing over age groups, we estimate that there were 1.64 million more people on SSDI at the end of 2016 than we would have expected had age-specific receipt rates not changed (this is 19 percent of the caseload).

---

addiction as a qualifying condition. His work finds that approximately 22 percent of terminated beneficiaries started working at levels above the substantial gainful activity thresholds used by SSA to judge eligibility.

[13] They also confirm that the earnings reductions they estimate are statistically no different than the crowd-out estimates of Maestas, Mullen, and Strand (2013) and French and Song (2014), who find that SSDI receipt causes annual earnings losses (including both intensive and extensive margin effects) of $3,781 and $4,059, respectively, corresponding to earnings crowd-out of 18 and 19 cents per dollar of SSDI benefits, respectively.

[14] SSA caseload data are available at https://www.ssa.gov/policy/docs/statcomps/di_asr/index.html .

To benchmark the effect this growth might have had on the number of people employed, we apply the age-specific employment elasticities from Table 6 of Maestas, Mullen and Strand (2013) to the excess caseload within each five-year age bin. This calculation suggests that, without the growth in SSDI caseloads in excess of the growth expected based simply on population growth and aging, there would have been 360,869 more workers.[15] Adding those workers to the 2016 workforce would increase the employment-to-population ratio by 0.14 percentage points.

We turn next to the federal Supplemental Security Income (SSI) program. The SSI program provides cash income to low-income elderly individuals, as well as to disabled children and disabled non-elderly adults with limited earnings histories.[16] The program is administered by the SSA and eligibility is determined by an identical set of medical eligibility criteria as are used for SSDI. The number of non-elderly adults receiving SSI benefits rose from 3.65 million in 1998 to 4.94 million in 2013, reaching 2.5 percent of the non-elderly adult population. This is attributable to both demographic and policy factors (Duggan, Kearney, and Rennane 2016). We are award of no direct evidence that allows us to quantify the extent to which this increase in program participation has pulled people who otherwise would have been working out of the workforce, but growth in SSI participation could have contributed some modest amount to the decline in employment among the non-elderly.

A third federal disability insurance program is the Disability Compensation (DC) program administered by the U.S. Department of Veterans Affairs. This program pays benefits to individuals with medical conditions resulting from U.S. military service. In contrast to SSDI benefits, DC benefits are based solely on a determination of the severity of the impairment a veteran has suffered. Benefits are paid for life and are not reduced if a veteran is working. Since 2001, the DC program has experienced rapid growth, due in part to liberalization of the medical eligibility criteria (Duggan, Rosenheck and Singleton 2010). Coile, Duggan and Guo (2015) estimate that between 2000 and 2013, the relative labor force participation rate of veterans (as compared to demographically similar non-veterans) fell by five percentage points. They note that over this time, the share of veterans receiving DC grew by 9 percentage points, from 9 to 18 percent, after having been generally stable for decades. Assuming that the increase in DC participation and benefit amounts is entirely responsible for the decline in relative labor force participation, they tentatively estimate that 55 percent of new DC recipients would be working in the absence of the program. Autor et al. (2016) also estimate a sizable, albeit much smaller, causal reduction in labor force participation associated with DC benefit recipient. These authors

[15] The Council of Economic Advisers (2016) does a similar back of the envelope calculation to estimate how much of the reduction in the labor force participation rate among prime-age men between 1967 and 2014 can be explained by increased receipt of SSDI benefits,. They take the estimate from French and Song (2014) that employment among accepted marginal applicants would have been 26 percentage points higher if they had been denied. The authors apply this estimate to non-working prime-age male SSDI beneficiaries and find that adding these men to the workforce implies that the labor force participation rate would have fallen by 0.5 percentage points less, out of a decline of 7.5 percentage points between 1967 and 2014.

[16] Duggan, Kearney, and Rennane (2016) includes a thorough review of the SSI program.

exploit the 2001 Agent Orange policy change that expanded DC eligibility for Vietnam War veterans who had served "in theater," but not for Vietnam War veterans who did not serve "in theater." They estimate that 18 percent of veterans who became eligible for the program and received DC benefits subsequently dropped out of the labor force. [17] Because the DC benefit amount is not dependent on work status but only on service-related health condition, this estimated effect is a pure income effect.

Using the causal estimate from Autor et al. (2016), we carry out a back-of-the-envelope calculation of the additional number of veteran workers there would have been in 2016 had DC benefit receipt not increased. The Department of Veterans Affairs reports 4.36 million DC benefit recipients in 2016, as compared to around 2.3 million in 1999.[18] We make use of program caseload numbers by broad age category to approximate the number of "excess" VADC recipients over this period. To do this, we calculate a projected 2016 caseload by applying 1999 age-category specific program population shares to the 2016 population and define the additional recipients to be the number of "excess" program participants; the implicit assumption is that this growth is due to policy changes over this time. We then apply the 18 percent estimate from Autor et al. (2016) to the excess caseload age 35 to 54 (since this overlaps with the ages of their analysis sample). We expect that the elasticity of work to program participation is smaller outside this age range, and hence make the somewhat arbitrary assumption that the employment effect is half as large for those in adjacent age categories (age 34 and under and age 55 to 74) and is zero for those 75 and older. This leads us to estimate a loss of 145,990 workers over this period, which is an admittedly very rough calculation, but nonetheless useful as a ballpark estimate. Adding these workers to the 2016 employed population would raise the employment-to-population ratio by 0.06 percentage points.

Our summary read of the evidence is that the rise in participation in disability insurance programs has had a notable contribution to the decline in employment over this period. The existing literature has produced credible causal estimates of the effect of the SSDI and Veterans Affairs DC program on labor supply. We use those estimates to gauge how much higher the employment-to-population ratio would have been in 2016 without the growth in these programs, coming up with a combined estimate of 0.20 percentage points. We note that this does not account for any effect of growth in participation in the SSI program, though because SSI recipients are people who by definition had a weaker prior attachment to the labor force than those receiving SSDI, we do not expect any such effect to have been large.

---

[17] The authors report that the implied labor supply elasticity is comparable to that found by Boyle and Lahey (2010) in their study of the labor supply of older nondisabled veterans ages 55 to 64 who were granted access to VA health insurance in the mid-1990s.

[18] Veterans Affairs Veterans 2016 Benefits Report available here: https://www.benefits.va.gov/REPORTS/abr/ABR-All_Sections_FY16_06292017.pdf; the 1999 Benefits Report is available here: https://www.benefits.va.gov/REPORTS/abr/fy99-abr.pdf

Supplemental Nutrition Assistance Program (SNAP)

The Supplemental Nutrition Assistance Program (SNAP), formerly the “food stamp” program, provides vouchers for food purchases to eligible individuals and families. In 2014, the program provided benefits to nearly 1 in 7 Americans, at a cost of $74.2 billion. Unlike most U.S. transfer programs, SNAP eligibility is not restricted to a particular group of people (such as the aged or disabled), though as discussed further below, prime-age adults without dependents who are not working or in a training program have limited access to benefits. The vouchers can be used to purchase most foods at grocery stores or other authorized retailers. Average monthly benefits in 2014 amounted to $257 per household, or $125 per person per month, which translates to benefits worth about $4.11 per person per day.[19]

Given the fairly low level of income support that the program provides, it seems unlikely that the provision of these food vouchers would lead a substantial number of people to choose non-work over work. That said, it is worth considering whether the existence of the program appears to raise the reservation wage, and hence reduce the labor supply, of potential workers. We briefly discuss three factors. First, we outline the labor supply incentives inherent in this transfer program. Second, we consider whether there were notable expansions in the generosity of the program during the 1999 to 2016 period that might have contributed to declining employment rates. Third, we highlight the most rigorous empirical evidence about the program’s effects on labor supply.

SNAP is designed as a classic income transfer program. Standard labor supply theory implies that an eligible individual would choose less work and more leisure in the presence of the SNAP program than if no such income support were availability. Eligibility for SNAP requires that a household’s gross monthly income not exceed 130 percent of the federal poverty line, that net income after deductions not exceed the poverty line, and that countable household assets be less than $2,250 (higher for households with an elderly or disabled member). Households must be recertified every 6 to 24 months for eligibility. The benefit amount is highest for households with zero income and is reduced as household income rises. The statutory benefit reduction rate is 0.30, meaning that a household loses $30 in benefits for every additional $100 in income. Note that this is lower than the benefit reduction rate in other transfer programs such as TANF and SSI, making the labor supply disincentives inherent in the SNAP benefit formula weaker.

The SNAP program rules are quite restrictive for non-working prime-age (18 to 49 years old) able-bodied adults without dependents, referred to by the U.S. government as ABAWDs. Most prime-age ABAWDs are restricted to three months of benefits within a three-year period if they are not working or in a training program at least 20 hours per week. This feature of the program is essentially tantamount to a work requirement for childless adults, meaning that it is very unlikely to explain the decline in participation for younger men in particular.

[19] These statistics come from Hoynes and Schanzenbach (2016), which includes a thorough review of program features, theoretical considerations, and empirical evidence of program effects.

By statute, the time limits imposed on non-working ABAWDs are relaxed during periods when unemployment in a state is high, as was the case in many states during the Great Recession. In addition to that automatic eligibility extension, as part of the stimulus plan in the American Recovery and Reinvestment Act of 2009 (ARRA), the monthly SNAP benefit amount was increased by 13.6 percent. That provision expired in 2013. Did these expansions lead to lower rates of employment or longer spells of unemployment during the years of the Great Recession? While it is difficult to say definitively, a key lesson from studies of SNAP caseloads is that macroeconomic conditions, as opposed to program parameters, are the key determinants of caseloads (see review by Hoynes and Schanzenbach 2016). Furthermore, those expansions were temporary and cannot be responsible for any part of the longer term secular decline in employment.

The only paper we know of that studies the relationship between SNAP expansions in recent years and labor supply is a working paper by East (2016) that focuses on relaxed restrictions to program eligibility for immigrants in the post-welfare-reform era. Her analysis suggests that single immigrant women reduce employment when they gain SNAP eligibility and married men reduce their hours of work. Hoynes and Schanzenbach (2012) exploit the county variation in the rollout of the food stamp program in the 1960s and early 1970s to investigate how labor supply responded to access to program benefits. They find no evidence of a reduction in employment or hours in the full sample, but they do find a statistically significant reduction in hours and, in some specifications, also employment among female heads of household.

In summary, the availability of food vouchers through the SNAP program is equivalent to an income transfer and, as is typical of income transfer programs, creates labor supply disincentives. Empirical evidence corroborates the predictions of standard labor supply theory and finds modest labor supply reductions among the populations most likely to be eligible for and receive program benefits. Given that childless non-disabled adults are subject to strict time limits, it is likely that any effects of the program on employment rates among this population are extremely small, if not entirely negligible.

Expanded Access to Publicly-Provided or Subsidized Health Insurance

The 2010 Affordable Care Act (ACA) expanded public health insurance access to segments of the population not previously covered by the Medicaid program, in particular, to low-income childless individuals. The ACA also offered income-based subsidies for individuals to purchase health insurance on newly created exchanges. The timing of this policy enactment is such that ACA expansions cannot account for the longer-term secular decline in employment rates before 2010. Still, it is worth considering what is known about the relationship between access to public health insurance and labor supply in order to gauge whether these expansions might be contributing to a slow recovery of employment rates and the extent to which the maintenance (or reversal) of these expansions might affect employment rates going forward.[20]

[20] In a 2014 report based on a simulation model, the Congressional Budget Office (CBO) predicted that, on net, the various provisions of the ACA would reduce the total number of hours worked by about 1.5

There are multiple channels through which the ACA provisions that expanded health insurance coverage might lead to lower levels of employment. First, the fact that individuals now can obtain health insurance outside of an employment arrangement at a lower price than previously should make employment relatively less attractive. Second, for eligible individuals, the phase-out of subsidies as income increases should make additional work hours less attractive. Third, the expansion of Medicaid to childless individuals would lead to lower levels of employment in a standard labor supply model, as the consumption level associated with non-work is increased. Fourth, the ACA can be expected to negatively affect employers' demand for workers by increasing labor costs through the employer penalty for not offering employer-provided health insurance.[21]

The estimated labor supply effects of public health insurance expansions appear to vary depending on the specific context within which they occurred.[22] Garthwaite, Gross, and Notowidigdo (2013) find a large labor supply response to the large 2005 disenrollment in Tennessee's public insurance program. They estimate that coverage among childless adults fell by 7.3 percentage points and that this decline led to a 4.6 percentage point increase in employment, implying that nearly two-thirds of childless adults who lost coverage entered employment. Dague, DeLeire, and Leininger (2017) find a smaller, but still notable, labor supply response to the 2009 enrollment freeze in Wisconsin's public health insurance program. They find that program coverage leads to an employment reduction of between 2 and 10 percentage points. Baicker et al. (2014) find smaller effects in the context of the Oregon Medicaid Health Insurance experiment in 2008 that extended program coverage to a randomly selected group of eligible individuals not previously covered by health insurance. Their point estimate of the local average treatment effect is a decrease in employment of 1.6 percentage points, or 3 percent; their confidence intervals imply that employment declines of more than 4.4 percentage points can be rejected.

---

percent to 2.0 percent during the period 2017 to 2024, driven almost entirely by a reduction in labor supply (CBO 2014b).

[21] The ACA requires that employers with at least 50 full-time-equivalent employees offer affordable health insurance to employees working 30 or more hours per week, or pay a fine. To investigate the labor market effects of this mandate, Dillender, Heinrich, and Houseman (2016) use CPS data and a difference-in-difference strategy to compare the post-ACA experience of other states to that of Hawaii, which has had a more stringent employer mandate for decades. Their findings suggest that the ACA has led to an increase in involuntary part-time employment. Kolstad and Kowalski (2016) consider the labor market effects of the 2006 Massachusetts health care reform. They find that implementation of the employer mandate led to a reduction in wages paid to covered workers, but only a small reduction in labor hours, which is consistent with a high valuation of the mandated benefit on the part of workers and a corresponding outward shift in the curve relating labor supply to the wage rate.

[22] There are a set of papers that examined the effect of Medicaid expansions during the 1980s and 1990s on the labor supply of single mothers, the group that was targeted by those earlier expansions. These generally find no discernible labor supply responses, see for example, Meyer and Rosenbaum (2001). This literature is summarized in Buchmueller, Ham, and Shore-Sheppard (2016).

There are various potential explanations for the differences in findings across these studies. One possible reason for the especially large estimates in the Garthwaite, Gross and Notowidigdo (2013) study is that Tennessee's program covered relatively higher income individuals, who are more likely to be able to find jobs with health insurance benefits. The lower estimated effects for Wisconsin and Oregon may be explained by the policy changes having taken place during a period when labor markets were weaker, which might have affected individuals' ability to adjust to changes in health insurance access by changing their employment status.

Three recent studies investigate the employment effects of various components of the Affordable Care Act. Leung and Mas (2016) investigate whether states that expanded Medicaid as part of the ACA experienced differential trends in employment among childless adults as compared to states that did not adopt Medicaid expansions. They find that, although an expansion policy increased Medicaid coverage by 3.0 percentage points among childless adults, there was no statistically discernible change in their employment rate associated with the policy change. Heim, Lurie, and Simon (forthcoming) use a data set of U.S. tax records spanning 2008-2013 to study how the ACA provision requiring employers to allow young adults to remain on their parents' health insurance plans has affected labor market-related outcomes. They find no evidence of changes in labor market outcomes for young adults in response to this provision. A recent paper by Duggan, Goda, and Jackson (2017) documents heterogeneous labor market responses to the ACA based on baseline population characteristics. They exploit variation across geographic areas in the potential impact of the ACA based on preexisting population shares of uninsured individuals within income groups that would have been affected by the Medicaid expansions (i.e., lower income individuals) and separately the federal subsidies for private health insurance (i.e., middle income individuals). They find that the aggregate labor supply effects of the ACA were close to zero, but that there are offsetting effects for different segments of the population. In particular, their analysis finds that labor force participation fell significantly in areas with a high share of previously uninsured individuals who gained eligibility for private insurance subsidies (i.e., middle-income individuals), but increased in areas with a high share of previously uninsured individuals whose earnings were too low for them to qualify for private insurance subsidies (i.e., lower income individuals who increased earnings in order to qualify).

Our summary read of the evidence is that expanded access to Medicaid and government subsidies for health insurance purchases may have had a modest negative effect on employment rates in recent years. But, given the timing of the ACA expansions and the modest estimated effects in the literature, expanded access to health insurance probably has not been a primary driver of the secular decline in employment between 1999 and 2016.

<u>Earned Income Tax Credit</u>

The Earned Income Tax Credit (EITC) is a refundable tax credit for low-income tax filers with positive annual earnings. According to the IRS, more than 27 million tax filing units received the EITC in 2016, with the value of claimed credits totaling $67 billion. The EITC was first introduced as part of the federal income tax code in 1975 and made permanent in 1978. Because the program is widely recognized as incentivizing labor force participation and effectively reducing poverty, it has been expanded a number of times since 1990, most dramatically in 1993 and 1996 and also notably in 2001 and 2009.[23] As we describe below, the EITC offers only minimal benefits to childless tax filers, so any effect of changes in the EITC on observed employment rates over recent decades would have been concentrated on workers with qualifying children under age 18.

The amount of the EITC credit depends on annual earnings and number of children in the household. There is a phase-in range of income, over which the credit subsidizes earnings at a rate of up to 45 percent (for those with more than two children), followed by a plateau range of income where the family receives the maximum credit, followed by a phase-out range where the amount of the credit is reduced down to zero. The maximum credit amount currently ranges from $506 for childless tax filers to $6,269 for eligible tax filers with three or more qualifying children. The income cutoffs at which the EITC falls to zero for single filers are $39,296 for one-child families; $44,648 for two-child families; and $47,955 for families with more than two children. Legislation in 2001 introduced a separate schedule for married filers with a longer phase out range. That legislation also increased the maximum EITC credit amount available for workers with at least three children. The income eligibility thresholds for married filers were expanded again in 2009 to reduce the negative incentives for work among spouses. Those income cutoffs are now about $5,400 higher than for single filers. Childless single workers can qualify for a small credit (currently $510) if they have less than $14,880 in annual earnings.

The empirical literature on the labor supply effects of the EITC yields a consensus finding that EITC expansions during the 1990s increased the labor force participation rates of single mothers with children (e.g., Eissa and Liebman 1996; Meyer and Rosenbaum 2001). This implies that, all else equal, expansions in the EITC should have *increased* the employment rates of low-wage single mothers. In contrast, for married couples with two earners, the EITC has ambiguous effects. This is because the U.S. tax code treats married couples as a single tax unit, and the EITC credit phases out as combined household earnings increase. For households where adding a second earner would lead to a reduction in the EITC payment (for example, by placing the household's combined income in the credit phase-out range), there is a disincentive for spousal employment. Eissa and Hoynes (2004) find that EITC expansions between 1984 and 1996 led to a decline of more than a full percentage point in married women's labor force participation. The changes in the EITC in 2001 and in 2009 lessened the negative disincentive for spousal employment by extending the phase-out range of income. All else equal, those changes might have increased spousal labor supply relative to the earlier period.

If one were to net out potential EITC-induced increases in employment over the 1999 to 2016 period – owing to the 2001 and 2009 changes in the program – then the declines in employment to be explained would be somewhat larger than the net declines actually observed. We do not attempt a calculation of the potential aggregate magnitude of the effects of EITC changes over this period. Such a calculation would be highly speculative and the EITC is unlikely to have

[23] For a comprehensive history of the program and review of institutional features, we refer the reader to Nichols and Rothstein (2015).

been a significant driver of overall employment rates, and certainly not of employment rate declines, during this period.

Spousal Employment

Another labor supply factor to consider, at least with reference to married men, is spousal employment. If men today are more likely to be married to working women than in previous decades, they might choose to supply relatively less labor. In a standard labor supply model, an increase in one spouse's income could have a negative income effect on the amount of labor supplied by the other spouse. In addition, if spousal labor supply is substitutable, rather than complementary, an increase in women's wages would lead to relatively more labor supply from wives and less from husbands. As pointed out by the Council of Economic Advisers (CEA) in a recent report (2016), however, the raw data do not support this hypothesis. As the CEA report documents, the share of prime-age men out of the labor force who have a working spouse actually fell somewhat between the late 1990s and 2015, and the share who do is relatively small (only about 20 percent in 2015). This is thus unlikely to be a key factor behind falling labor force participation rates.

Video Games, Opioids, and Social Norms

In addition to inward shifts in the labor supply curve that could have been caused by income effects associated with transfer income or spousal earnings, it has been proposed that an increase in the value of leisure time owing to improved video gaming technology also could have increased the relative attractiveness of non-work (Aguiar et al. 2017), as could the increased availability of opioid drugs (Krueger 2017). There is also the possibility of changing social norms that have made it more socially acceptable for young men to be out of work and financially supported to various degrees by their parents or girlfriends (Eberstadt 2016).

Time use data reveal that young men are filling their non-work hours by consuming more leisure, in particular, that they are spending more time in recreational computing and gaming. Aguiar et al. (2017) document that between 2004-07 and 2012-15, the drop in market hours for young men was matched by a roughly equivalent increase in leisure hours. The picture these authors paint using data from the American Time Use Survey is a bleak one. They report that, over this time period, men between the ages of 21 and 30 increased their recorded leisure time by about 2.5 hours per week, and that roughly three quarters of that (1.9 hours) was spent in recreational computing time, including video gaming. Non-employed young men in the later period are spending 5.9 hours per week on video gaming.

The authors try to establish a causal link between improved video gaming technology and a reduction in hours worked among young men. Lacking exogenous variation in the supply of improved gaming technology, either across time or place, they instead develop a method based on Engle Curve estimation from which they infer innovations to leisure technology over time.

They then estimate a system of leisure demand equations and use structural modeling assumptions about labor and leisure elasticity parameters to estimate the role that improved leisure technology could have played in reducing labor supply. They authors' provocative conclusion is that 23 to 46 percent of the decline in the market hours of men aged 21 to 30 between 2000 and 2015 could be explained by innovations in video gaming technology.

This line of research highlights the important issue of how young men today are responding to adverse labor demand shocks. Aguiar et al. (2017) explicitly tackle the question of whether there have been changes to the latent labor force attachment of young men, so that they respond more negatively to adverse demand shocks, as compared to previous generations or other workers. They suggest that video gaming technology is a notable and important change that is affecting the labor supply decisions of today's young men. The paper is intriguing, and the mechanism and direction of the effect warrant consideration, but the point estimates reported unavoidably rest on a good many unverifiable modeling assumptions.

Another way to pose the question about the role of gaming technology is as follows: If today's video gaming technology had been available during the 1990s, would young men then have worked less than they did? Perhaps, but this is far from clear. In the story told by Aguiar et al., important motivating facts are that the drop in labor demand experienced by young men (as captured by wages) has been similar to that for older men, but young men's employment has fallen by more. They speculate that the large amount of time young men spend playing video games is an important part of the explanation for their falling employment rate. But these facts also could be viewed as posing a challenge to their story: Video games are available to all men, so why are they not affecting the behavior of both young men and older men? One possible explanation is that, for younger men, the perceived stigma of being out of work playing on a computer or console in their parents' or other relative's home is lower than for their older peers. In other words, the explanation may lie with the young men themselves, rather than with the availability of video gaming technology. This would be a cohort-based explanation that cannot readily be ruled out.

There is an auxiliary observation in Aguiar et al (2017) about living with other family members that deserves additional attention. They note that young men today live with their parents at substantially higher rates than in the past; 67 percent of non-employed young men age 21 to 30 lived with a parent or close relative in 2015, as compared to 46 percent in 2000. This suggests that parents may be playing a safety net role for young men, not unlike the role of the Social Security Disability Insurance program for older men.

Another provocative hypothesis is that the increase in opioid prescriptions is in part responsible for the decrease in labor force participation rates among prime-age men. Krueger (2017) observes that labor force participation has fallen more in areas where relatively more opioid pain medication has been prescribed. He draws this conclusion by combining county-level data on the volume of opioid prescriptions per capita in 2015 from the Centers for Disease Control and Prevention to Current Population Survey data on labor force participation in 1999-2001 and

2014-2016.[24] Our read of the evidence is that, although it seems clear that the problems of depressed labor force participation and opioid use are interrelated, the arrows of causality run in both directions and there is not yet rigorous evidence to quantify the magnitude of the relevant effects. It is quite plausible that some people who have gotten an opioid prescription have become addicted and consequentially stopped working, as is suggested by Krueger (2017). It is also quite plausible that weak labor market prospects, and a corresponding sense of economic despair, has led some people to opioid use (see Case and Deaton 2017). It remains an open empirical question as to how much each has driven the other.

We place the complex inter-related issues of changing social norms, enhanced value of leisure time, pain and opioid use, and interactions thereof in a "residual" category of explanations that warrant further research attention.

Family-Friendly Policies: Childcare and Paid Parental Leave

One observation frequently made in discussions of labor force participation is that the United States lacks the public support for child care and paid parental leave that is common in much of the rest of the developed world. For instance, Kleven (2014) points out that despite very high tax rates on workers, Scandinavian countries boast higher employment rates than the United States or United Kingdom, both of which impose much lower tax rates on workers. He speculates that this is because Scandinavian countries effectively subsidize labor supply by lowering the prices of goods that are "complementary" to working, namely, child care, preschool, and elder care. A natural question, then, is whether the lack of supportive policies – or "complementary" policies in the language of Kleven (2014) – can help to explain the stagnation and subsequent decline in U.S. women's employment-to-population ratio.

Standard labor supply models imply that higher child care costs should be associated with lower labor force participation rates. For a single parent or a married parent whose spouse is already employed, the cost of child care is almost certain to be an important factor in the decision about whether to work; this seems especially to have been the case for mothers, though changing gender roles may lead to it becoming more of a factor for fathers over time. The early empirical literature on this topic dates from the 1980s (e.g., Blau and Robins 1988, Ribar 1992, Connelly 1992, Kimmel 1998, Anderson and Levine 1999, Connelly and Kimmel 2003) and consistently found higher child care costs to be associated with lower employment rates for women with

[24] Krueger (2017) estimates an OLS regression of individual-level male labor force participation on county-level opioid prescriptions per capita in 2015, a dummy variable for 2014-2016, and the interaction of those two terms. The resulting point estimates imply that labor force participation fell by more over this 15 year period in places with a higher rate of opioid prescriptions per capita. Under the extremely strong assumption that year 2015 county-level opioid prescription rates are exogenous to county-level labor market trends, so that this regression yields a causal estimate of the effect of opioid prescriptions, he infers that the increase in opioid prescriptions – which grew by a factor of 3.5 nationwide between 1999 and 2015 – could account for 20 percent of the observed decline in male labor force participation over this period.

children. Anderson and Levine (1999) report that the employment decisions of lower-skill workers are especially sensitive to child care costs.

An important limitation of the early studies was the lack of an exogenous source of variation in child care costs. Some more recent research has used the introduction of universal kindergarten and, later, pre-kindergarten to investigate the effect of care that is essentially free during school hours for eligible children on mothers' employment (Cascio 2009, Fitzpatrick 2010, Cascio and Schanzenbach 2013). In a related study, Gelbach (2002) used information on children's quarter of birth to examine the effect on mothers' employment rates of having a child who had reached the age cutoff for kindergarten attendance. These studies suggest that public kindergarten programs lead to significant increases in mothers' employment; the findings with regard to the effects of public pre-kindergarten programs are less clear cut.

Additional evidence on the effects of publicly provided childcare comes from the province of Quebec in Canada, where a comprehensive reform adopted in 1997 called for regulated childcare spaces to be provided to all children age 0 to 5 at a price of $5 per day. Studies of that reform conclude that it had significant and long-lasting effects on mothers' labor force participation (Baker, Gruer and Milligan 2008, Haeck, Lefebvre and Merrigan 2015). An important feature of the Quebec reform was its comprehensive nature; once fully implemented, it made very low-cost childcare available for all children in the province. Nollenberger and Rodriguez-Planas (2015) find similarly positive effects on mothers' employment associated with the introduction of universal preschool for 3 year olds in Spain. In contrast, policy reforms in Norway (Havnes and Mogstad 2011) and Sweden (Lundin, Mork and Ockert 2008) that lowered the cost of child care in a context where there was already a significant amount of publicly provided care had very limited incremental effects on mothers' employment.

Public spending on child care and child care subsidies in the United States is very low relative to the level of support provided in other countries. The higher levels of such spending elsewhere in the world may help to explain why female employment rates in many developed countries are now higher than the rate in the United States. Figures cited by Blau and Kahn (2013), however, show that U.S. public spending on child care as a share of GDP actually increased between 1990 and 2010, meaning that changes in U.S. child care policies have been in the wrong direction to explain the recent decline in the female employment-to-population ratio.

A somewhat different hypothesis is that the challenges associated with arranging and paying for child care also may have grown. Ziliak (2014) reports that, as of 2012, the costs of full-day center-based child care represented from a quarter to a third of the average annual earnings of single mothers of young children, depending on the state. To the extent that child care costs have risen and the earnings of lower-wage workers have not kept pace, child care costs could have become a more important barrier to employment. Boushey and Ansel (2016) point to employers' just-in-time scheduling practices as another potentially important factor. Workers with unpredictable schedules are apt to find it considerably more difficult to arrange for child care and, if they do not have a regular child care arrangement, may not qualify for available child care subsidies. Data on the prevalence of just-in-time scheduling practices are scarce, but anecdotal evidence suggests they may have become more common. If so, this also could have contributed to declining employment rates. Unfortunately, we are not aware of evidence that would allow us to quantify the influence of changes in the cost or availability of child care on employment rates. This is an important area for future research.

Lack of paid leave for new parents is another factor sometimes cited as a barrier to employment in the United States. While the United States lacks the generous entitlements to paid parental leave that are common in many other developed countries, these entitlements have not become less generous over time; indeed, the modest changes that have occurred have been in the opposite direction. Since 1993, the Family and Medical Leave Act (FMLA) has required employers with 50 or more workers to offer job-protected but unpaid family or medical leave of up to 12 weeks to qualifying employees. In 2004, California introduced a program that provides an entitlement to up to six weeks of paid parental leave through its pre-existing temporary disability insurance program. New Jersey introduced a similar program in 2009, also providing up to six weeks of benefits, and a program in Rhode Island providing up to four weeks of paid benefits took effect in 2013. New York and the District of Columbia recently passed laws to introduce paid leave programs providing for up to 8 weeks of benefits, though neither has yet been implemented.[25]

Thinking about paid leave is somewhat complicated by the fact that the effect of introducing or extending an entitlement to paid parental leave on employment rates could be either positive or negative. On the one hand, the availability of paid parental leave may encourage women who do not yet have children to work and, by preserving the relationship with her employer, also may ease a woman's transition back to work following the birth of a child. On the other hand, paid parental leave may encourage some women who otherwise would have returned to work more quickly to remain at home for a longer period of time and discourage some employers from hiring women of child-bearing age.

Rossin-Slater (2016) provides a careful review of the empirical evidence pertaining to the effects of paid parental leave entitlements. Her assessment is that paid leaves of up to a year in length may have modest positive effects on women's medium- and long-run employment, though she also concludes that longer periods of paid leave do not raise subsequent employment rates and can have negative impacts on wages. If anything, then, the introduction of modest paid leave entitlements in two large states during the 2000s could perhaps have had a (small) positive effect on female participation, an effect that would go in the wrong direction to have any part in explaining the recent participation declines.

Our summary read of the evidence is that child care costs can be an important impediment to mothers' employment. We have no hard evidence that this problem has worsened over time, but the role of child care costs as an influence on employment rates merits further study. While paid

---

[25] Details on the state leave programs mentioned in the text can be found at http://www.nationalpartnership.org/research-library/work-family/paid-leave/state-paid-family-leave-laws.pdf

leave for new parents may be desirable for other reasons, there is little evidence that its absence has much effect on employment rates. Further, because the lack of paid parental leave is a long-standing feature of the U.S. labor market, it logically cannot be responsible for falling labor market participation.

Immigration

A final labor-supply-related factor sometimes mentioned in connection with the decline in the employment-to-population ratio, especially for younger and less-skilled native workers, is increased immigration. According to estimates produced by the Census Bureau cited in Blau and Mackie (2016), net immigration contributed an average of 0.48 percentage point to annual population growth between 1990 and 2000; since 2000, the pace of immigration has dropped off somewhat, but it continued to add roughly 0.3 percentage point to annual population growth, accounting for roughly 30 to 40 percent of total population growth, depending on the year.

The idea that immigrants take jobs away from native workers undoubtedly has popular appeal, but in its simplest form it rests on a fallacy – the mistaken notion that there are a fixed number of jobs in the economy, so that more employed immigrants must mean fewer employed natives. In reality, as discussed in the thorough review of the immigration literature offered by Blau and Mackie (2016), the real world is complex and there are many channels through which immigration may affect the employment of native workers.

In a model with a single type of labor, an upward-sloping labor supply curve and a fixed stock of capital, immigration can be modeled as an outward shift in the aggregate labor supply curve that causes native wages and employment to fall. If immigrants and native workers specialize in different tasks, however, they may be complements rather than substitutes (Peri and Sparber 2009, Ottaviano and Peri 2012). In that case, immigration could raise the marginal productivity and potentially the employment of native workers. Immigrant workers also are consumers, and their spending may increase the demand for labor. Further, investment may increase in response to the higher marginal product of capital associated with an influx of immigrants. Depending on how the capital stock evolves, in the long run the economy could simply be larger, with no permanent adverse effect on the wages and employment of native workers. Further, highly-skilled immigrants such as scientists and engineers may create positive externalities through innovation and resulting increases in productivity (Hunt and Gauthier-Loiselle 2010, Kerr and Lincoln 2010). This too could lead to positive effects of immigration on native employment. All of this implies that the effect of immigration on native employment is very much an empirical question.

One frequently-used approach to identifying the effects of immigration on wages and employment takes advantage of differences across areas in the number of immigrants. Because stronger economic conditions can be expected both to attract more immigrants and to raise the native employment rate, any simple cross-area correlation between the number of immigrants and employment rates for native workers could be misleading. A common approach to addressing this problem is to construct an instrument for the number of immigrants in a locality by applying growth factors based on national changes in the number of immigrants of a particular nationality to the number of immigrants of the same nationality who were living in the local area in an earlier base period. The rationale for this instrument is that immigrants tend to settle in areas where others of the same nationality already live. A concern about the spatial methodology is that outflows of domestic workers could offset the effects of immigration, so that cross-area comparisons understate immigration's effects. Borjas (2006) identifies this as an important consideration, but other studies such as Card and DiNardo (2000), Card (2001) and Peri (2007) conclude that outflows of natives have little effect on estimates of the effects of immigration based on cross-area data.

Another common approach to estimating the effects of immigration is to categorize workers based on their skills or qualifications, and then to use variation in immigration by skill level to estimate the effects of immigration on wages and employment. A challenge in these studies is how to group workers by skill level in the data; immigrants with a given level of education, for example, may not be viewed by employers as good substitutes for natives with the same level of education (Peri 2007). An additional concern is that immigrant flows may be endogenous with respect to the demand for different types of labor. Further, estimates produced by this type of study encompass the direct effects of immigration but not the indirect effects (e.g., increases in wages of a group attributable to increases in immigration in another part of the skill distribution).

We do not attempt a comprehensive review of the voluminous literature on the contentious topic of how immigration has affected native workers, but summarize a small number of selected studies chosen to illustrate the range of reported estimates using different approaches. At the high end of the wage effects obtained in studies using a spatial approach, Altonji and Card (1991) found that, over the 1970-1980 period, a 1 percentage point increase in the immigrant share in an area was associated with a 1.2 percent decrease in the wages of less-skilled natives, but no detectable change in their employment-to-population ratio. Using national data on male workers disaggregated by level of education and experience, Borjas (2003) also found large effects of immigration on wages. Over the period from 1980 to 2000, immigration raised the supply of male labor by about 10 percent; he estimates that this increase caused a decline of approximately 9 percent in the wages of native male high school dropouts and a decline in male wages overall of about 3 percent. Smith (2012) estimates that a 10 percent increase in the number of low-skilled immigrants causes roughly a 3 percent long-run decrease in the annual hours worked by 16- and 17-year olds, but has little effect on the hours of older natives.

In contrast to these studies estimating sizeable effects, a number of studies that rest on cross-area data in which workers are disaggregated by occupation rather than by education, including Card (2001) and Orrenius and Zavodny (2007), find much smaller effects of immigration on the wages of less-skilled natives. Ottaviano and Peri (2012) conclude that, in recent decades, immigration had a small *positive* effects on the wages of native workers, including those with less than a high school degree. Similarly, Basso and Peri (2015) conclude that "the net growth of immigrant labor has a zero to positive correlation with changes in native wages and native employment, in aggregate and by skill group."

Our reading of the available evidence is that, broadly consistent with the conclusion reached by Blau and Mackie (2016), immigration has little overall effect on native wages or employment, especially in the long run. There is considerable variation in the findings across studies and more evidence to suggest that immigration could be responsible for significant wage declines – and perhaps employment declines – among groups who are more substitutable with immigrants, such as younger and less-skilled native workers. The weight of the evidence in the literature leads us to be skeptical that immigration has been an important factor in the observed overall decline in the employment-to-population ratio.

C. Labor Market Institutions and Frictions

Beyond the factors that have shifted labor demand and labor supply, some have suggested that institutional constraints and growing labor market frictions increasingly could be hindering the matching of people to jobs, leading to employment levels that are lower than they otherwise would have been. Institutional constraints that could prevent wages from falling to market-clearing levels and thereby dampen employment include minimum wages, union-negotiated collective bargaining agreements and occupational licensing requirements. There has been a great deal of research on the employment effects of minimum wages; the theoretical effects of (falling) unionization and (increasing) occupational licensing on employment are less clear cut and the empirical basis for drawing conclusions about them is considerably weaker.

Minimum Wages

The subject of how minimum wages affect employment has long been contentious. For many years, the standard reference on the topic was the review by Brown, Gilroy and Kohen (1982). Based primarily on aggregate time series evidence, their summary conclusion was that a 10 percent increase in the minimum wage could be expected to cause a 1 to 3 percent reduction in teen employment, with little effect on employment among adults. The 1990s saw a renewal of interest in the minimum wage, with a series of studies analyzing state-level responses to minimum wage changes (e.g., Card 1992a, Card 1992b, Katz and Krueger 1992, Neumark and Wascher 1992, 2000; and Card and Krueger 2000).

The debate launched by these studies has spawned a sprawling new industry of minimum wage research that has been facilitated by subsequent changes in the minimum wage landscape. Whereas the Federal minimum wage was binding in all but 8 states and the District of Columbia as of the beginning of 1988, by 2016 there were 29 states plus the District of Columbia that had minimum wages above the Federal minimum, with a difference of $1.00 per hour or more in 20 of these jurisdictions. Many recent minimum wage studies have exploited the ongoing changes in state minimum wages, by comparing changes in employment rates in states—or in counties within states—where the state minimum wage has increased to the changes in states or counties deemed to be similar where no such increase occurred. Some of these studies, such as Dube, Lester and Reich (2010), Allegretto, Dube and Reich (2011), Allegretto et al. (2013) and Dube and Zipperer (2015), have found no detectable adverse employment effects due to minimum wage increases of the magnitudes observed in the data. Others, such as Neumark, Salas and Wascher (2013) and Powell (2016), have found significant negative employment impacts.

One difference across this set of studies lies with the how the set of states or counties used for making comparisons is constructed. In the literature that uses counties as the unit of observation, the most common approach has been to use counties that are in geographic proximity—so-called county border pairs—whereas other studies have used a more formal synthetic control or similar methodology. Within the set of studies based on the synthetic control approach, there are also differences in how the matching is accomplished. Another difference across the studies lies with how underlying trends that might have affected employment in a particular county are taken into account, for example, through a linear time trend versus some more flexible specification. The findings of the different studies appear to be quite sensitive to these choices and there is no consensus about the right approach to take.

An emerging literature has used individual-level data to focus on workers with wages in the interval most likely to be affected by increases in the minimum wage. Clemens and Wither (2014) examine the impact of the increase in the Federal minimum wage to $7.25 per hour in July 2009 on the subsequent employment of workers who had been earning less than $7.50 per hour in 2008. They use data from the Survey of Income and Program Participation (SIPP) to compare the changes in employment for this group in states where the increase in the Federal minimum was binding versus states where it was not, and, in the states where the increase was binding, to compare the changes in employment for those initially earning less than $7.50 per hour to the changes for people earning slightly higher wages. Their baseline estimate is that the 2009 increase in the Federal minimum wage reduced employment in the affected group by 6.3 percent, which translates to a potential effect on the overall employment-to-population ratio between 2006 and 2012 of as much as 0.7 percentage point. Because minimum wage workers commonly cycle into and out of the labor force and Clemens and Wither look only at people who were employed in 2008, however, their analysis seems likely to provide an incomplete picture of the effects of the 2009 increase in the federal minimum.

Jardim et al. (2017) study the effect of the 2015 and 2016 increases in the Seattle minimum wage, using repeated cross sections based on unemployment insurance wage records to track the changes in employment in different wage intervals in Seattle as compared to other nearby jurisdictions. They find little effect of the 2015 increase in the Seattle minimum to $11 per hour but a significantly larger effect of the 2016 increase to $13 per hour. A limitation of this study is that multi-establishment firms are excluded from the study sample. Finally, Cengiz et al. (2017) study the effects of state minimum wage increases over the period from 1979 through 2016 using a bunching approach. They estimate that, when minimum wage increases occur, declines in employment in the interval just below the new higher minimum are approximately offset by increases in employment in the next higher wage interval, implying no net effect on employment for minimum wage increases of the magnitude observed in the data. Again, there are a range of estimates and no consensus in the literature.

Because turnover rates are high among minimum wage workers, most existing research has assumed that adjustments to an increase in the minimum wage will be realized relatively quickly. Sorkin (2015) argues that, in a putty-clay model in which permanently higher minimum wages lead firms to choose more capital-intensive technologies, the long-run effects of a permanent increase in the minimum could be substantially larger than the short-run effects estimated in most studies. Similar, Meer and West (2016) argue that a permanent increase in the minimum wage is likely to affect employment primarily by reducing future job growth, as firms that build new production capacity choose more capital-intensive technologies. Most past minimum wage increases have been specified in nominal terms and firms would have known that the real value of the new minimum would erode over time with inflation, moderating the incentive to invest in labor-saving technology. To the extent that state minimum wages increasingly are indexed to inflation, however, this could change in the future. Brummond and Strain (2016) use county-level data for the period from 1990 to 2012 to compare the effects of minimum wage increases in cases where the minimum wage is indexed to inflation and cases where it is not. They find substantially larger employment elasticities in response to an increase in the minimum wage in the presence of indexation. Further, the size of any future minimum wage increases is likely to matter. Even if past minimum wage increases have had little effect on employment, this would not necessarily be the case for larger increases in the future.

As an aside, it should be noted that all of the estimates we have cited account only for the direct effects of higher minimum wages on employment. If there are indirect effects on employment resulting from increased aggregate demand associated with increased purchasing power among low-income consumers, any negative impacts reported in existing studies could overstate the true employment effect of minimum wage increases. We are not aware of estimates that would allow us to credibly quantify any such aggregate demand effect and do not further consider that possibility.

To estimate the potential impact of minimum wage increases between 1998 and 2016, we first need to know how the average real minimum wage changed over this period. For that calculation, we use data from the Department of Labor on statutory state and federal minimum wages and state population shares. According to our calculations, the effective real minimum wage fell by 1.6 percent from 1998 to 2007 and then rose by 10.8 percent between 2007 and 2016, for a net increase of 9.0 percent over the entire 1998 to 2016 period.[26]

To set an upper bound on the potential dis-employment effect of this 9.0 percent increase in the effective minimum wage, we take the estimated employment elasticity for teenagers of 0.3 from

---

[26] This estimate was constructed by weighting the increase in the real minimum wage in each of the 50 states and the District of Columbia over each of the periods in question by the jurisdiction's 2000 population share. Information on state minimum wages was obtained from the Department of Labor at https://www.dol.gov/whd/state/stateMinWageHis.htm; data are reported only for selected years prior to 2000. The calculation used the Federal minimum wage where it was binding and the state minimum wage in cases where it exceeded the Federal minimum wage. Where the applicable state minimum varied (e.g., by size of firm), the calculation used the highest reported rate. The nominal minimum was converted to a real minimum using the Consumer Price Index as a deflator.

Neumark, Salas, and Wascher (2013), an estimate that is high relative to those reported in other recent papers. Neumark, Salas and Wascher do not report an estimated employment elasticity for adults. For the purposes of a back-of-the-envelope calculation, we follow the convention adopted by the CBO (2014a) assessment of the minimum wage literature and arbitrarily assume that the adult elasticity would be a third the size of the teen elasticity. Under these assumptions, the effect of minimum wage increases since 1999 on the 2016 employment-to-population ratio is 0.6 percentage points. Putting one-third weight on this estimate and two-thirds weight on the zero employment effect more commonly found in the recent literature, we speculate that minimum wage increases may have accounted for a 0.2 percentage point reduction in the employment-to-population ratio between 1999 and 2016, while hastening to add that there is a considerable error band around this estimate.

Decline in Unionization

Whereas minimum wages apply to all or nearly all of the jobs in the labor market, union collective bargaining agreements set a floor only on the wages paid by unionized employers. In a world in which the supply of labor is fixed, a standard two-sector analysis implies that setting an above-market wage in the union sector will increase the supply of labor to the non-union sector, depressing non-union wages. If the supply of labor to the nonunion sector is elastic, however, overall employment could fall as some workers in the nonunion sector choose to leave the labor force. In recent decades, of course, the share of the U.S. workforce who are union members has fallen dramatically, from more than 35 percent in the 1950s to just over 10 percent in 2016 (Hirsch and Macpherson 2003, Mayer 2004, Dunn and Walker 2016). If anything, to the extent that there is less displacement from the union sector and any resulting downward pressure on nonunion wages has eased, one might think that the decline in unionization should have boosted overall employment, leaving more of the decline in the employment-to-population ratio to be explained by other factors.

Rise in Occupational Licensing

In contrast to the falling share of workers who are unionized, the share of workers in occupations for which a state or local government license is required to work has risen considerably, by one estimate from just 5 percent of workers in the late 1950s to nearly 30 percent of workers today (Kleiner and Krueger 2013). Occupations subject to licensing requirements in one or more states include not only physicians, dentists, teachers, and electricians – jobs in which there is an obvious rationale for requiring some demonstration of the qualifications of those performing the work – but also a large number of occupations in which the rationale for licensing is considerably less obvious, such as auctioneers, florists, locksmiths, ballroom dance instructors, hair braiders, manicurists, interior designers, and upholsterers (Kleiner 2015).

Occupational licensing laws make entry into a regulated occupation more expensive, limiting the number of people choosing the occupation. This can be expected to increase the supply of labor to the non-licensed sector and lower wages there, causing work in the non-licensed sector to be less attractive. As a result, some people who otherwise would have worked may decide to leave the labor force. Using data from a telephone survey they commissioned Westat to conduct, Kleiner and Krueger (2013) find that state level licensing leads to an average occupational wage premium on the order of 15 percent, roughly in line with the wage premium associated with union membership. Gittleman, Klee, and Kleiner (2015) analyze data from a module included on the 2008 SIPP and, after controlling for a large number of other observable characteristics, find that holding a state-issued occupational license is associated with a wage premium closer to 5 percent.[27] Kleiner (2006) presents evidence that within-occupation employment growth is slower in states with full licensing requirements. Similar to the effect of an increase in the number of people seeking work in the nonunion sector following the introduction of an above-market union wage, the magnitude of any resulting change in overall employment will depend on the elasticities of labor demand and labor supply in the lower paid non-licensed sector.

The increasing prevalence of occupational licensing also could have dampened employment by making workers less geographically mobile. Because licensing occurs at the state level, workers in licensed occupations who move across state lines typically must meet a new set of state requirements to continue working in the occupation. One recent paper by Peterson, Pandya, and Leblang (2014) exploits changes in residency training requirements for immigrant physicians within states over the years between 1973 and 2010. They find that states that impose more stringent requirements receive fewer immigrant physicians, consistent with the prediction that occupational licensing restricts employment-based migration. In contrast, another recent paper by DePasquale and Stange (2016) finds no increase in geographic mobility or employment among nurses after the reciprocal arrangements associated with the Nursing Compact were introduced.

Given the dramatic increase in occupational licensing over recent decades and the theoretical rationale for believing this might have led to net reductions in employment, it is plausible that occupational licensing contributed to the decline in the employment-to-population ratio over this period. It could have done so in part by making it more difficult for workers who lost their job due to other factors, such as trade or technology, to start their own business or enter a new occupation. At this stage of the literature, however, we find it difficult to draw any strong conclusion about the labor market effects of the growth in occupational licensing and flag this as an area warranting additional research.

<u>Other Institutional Frictions</u>

The concerns about occupational licensing and its potential effects on labor market mobility are among a set of concerns raised by Davis and Haltiwanger (2014) about institutional frictions and

[27] Also using data from the 2008 SIPP module and controlling for a variety of observable characteristics, Blair and Chung (2017) estimate occupational licensing premiums for black men, white women and black women(13 percent, 14 percent and 16 percent, respectively) that are considerably larger than the premium for white men (8 percent).

reductions in labor market fluidity more generally. Davis and Haltiwanger define labor market fluidity in terms of the rate of job entry and job exit and show that, by this definition, fluidity has fallen considerably in recent decades, a finding corroborated by Molloy et al. (2016) using somewhat different data. Although reduced fluidity may have beneficial effects – in particular, by reducing the rate at which workers enter unemployment – there are channels through which it could lead to lower employment rates. On the worker side, Davis and Haltiwanger argue, it implies longer jobless spells that could lead to a loss of human capital and counter-productive increases in the psychic costs of job seeking. Further, these effects could interact with employer hiring behavior that disadvantages those with longer jobless spells (see, for example, Kroft, Lange, Notowidigdo (2013), Ghayad (2013), and Eriksson and Rooth (2014), though in a study focused on college-educated women, Farber, Silverman and von Wachter (2015) find no evidence of lower employer callback rates for those with longer jobless spells).

Any negative effects of reduced fluidity are likely to inflict disproportionate harm on workers who are more marginal or possess more limited skills. Davis and Haltiwanger present some evidence based on annual state-level panel data that lower fluidity may indeed be linked to lower employment rates, but without a better understanding of the causes of declining fluidity and the channels through which these factors might affect employment, we are not comfortable drawing any strong conclusions. For the moment, we identify this as another interesting area for future research.

Skill Mismatch between Workers and Jobs

The co-existence of available workers and vacant jobs sometimes is taken to indicate that there is a structural mismatch between the skills possessed by available workers and the requirements of available jobs. During the recent Great Recession, for example, statements to this effect commonly were made by politicians from both parties as an explanation for why unemployment was so high (Abraham 2015). In any dynamic labor market, however, there will always be both unemployment and vacancies resulting simply from normal turnover. When a job vacancy is created, whether through attrition or an employer's desire to increase the number of people employed, filling it unavoidably takes some time. The question of interest for our purposes is whether the process of matching available workers to vacant jobs has become less efficient over time, reducing the steady state level of employment.

In a simple model in which unemployed workers are seeking to match with vacant jobs, frictions in the matching process will produce an outward shift in the downward sloping curve that traces out the relationship between unemployment and vacancies, sometimes referred to as the Beveridge curve.[28] As documented by Abraham (2015), the Beveridge curve was stable between 2000 and 2009. During the years following the onset of the Great Recession, the vacancy rates

[28] Hall and Schulhofer-Wohl (2015) make the important point that employed people also may engage in job search and that a full assessment of what has happened to the matching function needs to take that into account.

associated with given unemployment rates were higher, leading some to conclude that mismatch between available workers and vacant jobs must have worsened. Absent direct evidence of growing mismatch, however, this could be the wrong conclusion to draw. Şahin et al. (2014) use Job Openings and Labor Turnover Survey and Help Wanted Online data on job openings together with data on the unemployed from the Current Population Survey to look for evidence of possible changes in industry, occupational and geographic mismatch. Their industry analysis covers the period from 2001 to 2012; the occupational and geographic analyses cover the period from 2005 to 2012. They conclude that increased occupational and industry mismatch could have contributed to the increase in unemployment during and immediately after the Great Recession, but that any such increase in mismatch was short-lived.[29]

Other possible explanations for the apparent outward shift in the Beveridge curve include unemployed workers searching less hard for work or employers recruiting less intensively to fill their jobs than in the past. In either of these cases, the outward shift would be better interpreted as the result of an underlying change in labor supply or labor demand behavior, rather than as an indication of mismatch. Some evidence consistent with the explanation that the outward post-recession shift in the Beveridge curve was related to declining employer recruitment intensity is provided by Davis, Faberman, and Haltiwanger (2013). We would add that it remains an open question whether the post-2009 outward shift is cyclical or secular. Temporarily elevated levels of vacancies relative to unemployment have been observed during past business cycle episodes and the outward shift in the Beveridge curve in recent years may be another example of the same phenomenon.

<u>Spatial mismatch and reduced geographic mobility</u>

A related explanation for the relatively low rates of employment among low-wage workers is “spatial mismatch,” which posits that residential distance from job locations keeps workers out of jobs. Much of the support for this notion comes from cross-sectional evidence, which is potentially confounded by individual and neighborhood effects. A recent paper by Andersson et al. (forthcoming) offers causal evidence that distance from job availability leads to longer job search duration among low-income workers with strong labor force attachment. The authors use longitudinal, matched employer-employee administrative data integrated with data on worker and neighborhood characteristics from the 2000 Census, combined with comprehensive transportation network data for nine large Great Lakes metropolitan areas. Among workers displaced by a mass layoff, those with longer commuting times to potential new job sites experience significantly longer spells of joblessness. While this is valuable information, it is not

[29] One caveat concerning the Sahin et al. (2014) findings with respect to occupational mismatch specifically is that some types of jobs may be more likely than others to be advertised online; a second is that individuals may be qualified to do a number of different things, but all that is observed about the type of work they are qualified to perform is the occupation of their most recent job. That said, this study provides the best direct evidence we have about labor market mismatch and it does not find that mismatch along any dimension has been worsening secularly.

clear how generalizable the findings are and, more importantly, we do not know that (time) distance from possible jobs has increased for less-educated workers.

A related issue is the extent to which declining rates of geographic mobility have led to lower rates of employment. Molloy, Smith, and Wozniak (2011) document that internal migration rates have trended steadily downward over the past 25 years and are now lower than at any previous time in the post-war period. Using data from the U.S. Census, they tabulate that in 1980, 9.9 percent of the population had moved across state lines in the past five years; that rate was 9.6 in 1990 and 8.9 in 2000. Other measures reveal a similar downward trend. Davis and Haltiwanger (2014) also document declines in mobility. If workers have become less willing to move in search of better economic opportunities, this could have caused an increase in geographic mismatch. Ganong and Shoag (2017) present evidence suggesting that over the period 1980 and 2010, stringent land use regulations have led to income gains being capitalized into higher house prices, and that this in turn has led to reduced rates of directed migration. They claim that this phenomenon has been a significant factor contributing to the decline of income convergence across regions.

An important recent paper by Dao, Furceri, and Loungani (2017) examines the migration response to regional labor shocks, building on the seminar work of Blanchard and Katz (1992). The paper documents the cyclical and trend behavior of U.S. labor mobility from 1977 to 2015 using state and MSA level labor market data from the Bureau of Labor Statistics (BLS) and population and migration data from the U.S. Census. A key finding of the paper is that rates of out-migration from areas experiencing economic downturns has decreased over this nearly 30 year period. The paper also shows that interstate migration in response to regional asymmetries in job opportunities actually increases in recessions, which implies that the finding of reduced out-migration in response to negative shocks is more of a long-term structural phenomenon then a feature of the Great Recession.

While declining mobility may indeed have contributed to declining employment rates, Kaplan and Schulhofer-Wohl (2017) suggest that this need not be the case. First, they argue that the returns to occupations have become less geographically specific than in the past. Second, they suggest that advances in information technology and declines in travel costs have made it easier to learn about faraway places before moving there, so that there are fewer migrants who move, discover they are unhappy in their new location, and return home. If their story is right, declines in gross migration rates do not translate directly into workers being allocated less efficiently across areas.

We conclude that the role of declining geographic mobility in driving down rates of employment is an important open question. Although we are not aware of direct evidence to suggest that geographic mismatch has grown in recent decades, the facts about declining geographic mobility, in particular the finding of a muted response to negative economic shocks, makes it plausible that employment-to-population ratios might be higher if rates of directed migration were higher. This is another topic that merits further investigation.

Incarceration

A final important trend that warrants attention is the dramatic increase in incarceration during the past three decades. The incarceration rate, defined as the number of inmates per 100,000 U.S. residents, increased from 220 in 1980 to 756 in 2008, before falling slightly to 710 in 2012 (Kearney et al. 2014). This increase is especially relevant for the demographic groups that are most likely to face incarceration, namely young minority males. For instance, Western and Wildeman (2009) estimate that, in 2005, a 30 to 34 year old African American men without a high school degree would have had nearly a 70 percent chance of having been imprisoned at some point in his life thus far.

Academic research suggests that increases in crime cannot explain the growth in the incarceration rate since the 1980s. Rather, that growth appears to be attributable to changes in policy, such as sentencing guidelines and mandatory sentencing laws for drug-related offenses that have increased both the likelihood of going to prison and sentence lengths (Raphael and Stoll 2013).

Because standard labor market statistics derived from the Current Population Survey are based on the non-institutionalized population and exclude those who are incarcerated, they understate the extent to which young men have become detached from the labor market. Doleac (2016) reexamines employment statistics in light of this fact. She compares the official employment-to-population ratios for black and white men aged 20–39 with adjusted versions that include the incarcerated in the denominator. As she explains, taking the incarcerated into account has only a minimal effect on the employment-to-population ratio for white men in this age range (for example, reducing it from around 81 percent to 80 percent in 2014), but lowers the employment-to-population ratio for black men in this age range by almost 4 percentage points in recent years (for example, from around 66 percent to 62 percent in 2014.)

Individuals who are incarcerated not only are unable to work during the period when they are in prison, but having been incarcerated may have a negative effect on their employment prospects after release. One channel through which incarceration could negatively impact subsequent employment rates is that labor market skills could deteriorate while a person is in prison, though in some cases well designed rehabilitation programs might actually enhance inmates' labor market skills. A second potentially important channel is that employers may discriminate against those with criminal records or prison time. This is the motivation for recent "ban the box" initiatives, though some preliminary evidence suggests that such policies could lead to statistical discrimination that lowers hiring rates for young minority men (Agan and Starr 2016; Doleac and Hansen 2016).

The most credible estimates that we know of on the causal impact of incarceration on employment come from Mueller-Smith (2015), who uses original data from Harris County, Texas. His dataset consists of criminal court records – over 2.6 million records accounting for 1.1 million unique defendants – linked to administrative data for state prisons and county jails and state unemployment insurance wage records. His empirical analysis takes advantage of the random assignment of criminal defendants to courtrooms staffed by judges and prosecutors with different propensities of sending a defendant to prison. He finds that among those with significant previous earnings, a prison term – driven by exogenous courtroom assignment – causes subsequent employment rates to be lower. The estimated labor market impacts grow with previous earnings and with time spent in prison. Specifically, among individuals whose annual earnings over the three years prior to going to prison averaged over $17,050 (the federal poverty threshold at the time of observation for a family of four), there is a statistically significant 39 percentage point reduction in the likelihood of employment two years post release if the person served at least two years in jail and a statistically significant 24 percentage point reduction if the person served at least one year. The estimated effects for a 6 month prison term or for those with no or low earnings prior to a conviction are smaller and generally not statistically different from zero.

To gauge how much of the decline in the aggregate employment rate might be attributable to increases in incarceration rates, we make a very rough calculation based on Mueller-Smith's estimates of the causal impact of having served time on employment. Ideally we would have data on the stock of U.S. adults who have been incarcerated, but this information does not exist in any public dataset. Instead, we use estimates of the number of former prisoners developed by Bucknor and Barber (2016). Their estimate rests on data from the Bureau of Justice Statistics on the number of people of different ages released from prison in each year from 1968 through 2014. After adjustments to account for recidivism and mortality, these counts can be cumulated to produce an estimate of the stock of former prisoners. Bucknor and Barber (2016) estimate that there were 6.1 million to 6.9 million former prisoners between age 18 and 64 as of 2014; we use 6.5 million, the midpoint of this range, in our calculations. Note that this estimate does not include people who served time in jail rather than prison.[30]

To apply the Mueller-Smith (2015) impact estimates, we also need an estimate of the fraction of these individuals who had been in prison two years or more, one to two years and less than one year. We base our estimates of these fractions on data for the 1997 National Longitudinal Survey of Youth (NLSY) from 2014, when sample members were between the ages of 30 to 34. Based on the NLSY97 data, we estimate that about 9.1 percent of adults who were age 30-34 in 2014 had spent some time in jail or prison. We assume that those reporting one-month spells in confinement and half of those reporting spells of less than a year had been in jail rather than in prison (2.1 percent of the population) and that those reporting longer spells had been in prison (7.0 percent of the population). Among the 7.0 percent we assume had been in prison,

[30] Bucknor and Barber (2016) adopt the methodology used by Schmitt and Warner (2010), who show that their estimate of the size of the ex-prisoner population for 2008 is similar to that obtained by other independent researchers. The estimates in these two papers are also broadly consistent with those reported by Shannon et al (2017) using similar life table methods. Shannon et al (2017) estimate that, in 2010, there were 4.9 million U.S. adults who had been formerly in prison or on parole and predict continuing increases in the number of former prisoners due to the release over time of those who are currently incarcerated.

approximately 43 percent had been confined for two years or more and approximately 27 percent had been confined for one to two years.

Based loosely on observed trends, we assume that 60 percent of the formerly incarcerated population estimated by Bucknor and Barger (2016) served time as a result of the policy-induced rise in incarceration rates since the 1990s.[31] This yields an additional 1.7 million working age individuals with a prior prison term of two years or longer and 1.0 million with a prior prison term of one to two years. Using the numbers on the distribution of pre-conviction earnings obtained by Mueller-Smith, we further assume that 18 percent of these individuals would have had significant earnings and 58 percent would have had some lower level of earnings prior to serving their prison term. Applying his estimates of the reduction in the probability of employment associated with a prison term (39 percentage points for those with significant earnings and two years or more in prison, 24 percentage points for those with significant earnings and one year in prison, 11 percentage points for those with low earnings and two years or more in prison, and 9 percentage points for those with low earnings and one year in prison), we estimate that in the absence of the rise in incarceration, there would have been about 324,000 more employed workers in 2016. Note that this calculation assumes no incarceration-related employment losses among those age 65 and older. Adding these extra workers to the workforce would have increased the employment-to-population ratio by about 0.13 percentage points.

Given how many assumptions are required to make this calculation, we do not take our estimate too literally as a specific magnitude, but it does give us a sense for the likely ranking of incarceration as a contributor to falling employment. The role of incarceration, and criminal convictions more generally, in driving down rates of employment, especially among young minority males, is an issue that warrants further research and policy attention.

IV. CONCLUDING OBSERVATIONS

We conclude our review of the evidence with an attempt to rank the various factors we have considered by their likely contribution to the decline in the aggregate employment rate over the 1999 to 2016 period. Table 3 lists the factors that we have considered as potential drivers of the decline in the employment-to-population ratio for the population, including labor demand factors, labor supply factors, institutional factors and labor market frictions. Where possible, we have entered our best estimate of the effects of identified factors on the overall employment-to-population ratio; in other cases, there is too little available evidence for us to draw quantitative conclusions. As reported in Table 1A, the overall employment-to-population ratio for the population 16 and over fell by 4.5 percentage points between 1999 and 2016. This number is

[31] Among the NLSY79 cohort born between 1957 and 1965, 7.2 percent report having been jailed before the age of 34; the corresponding number for the NLSY97 cohort born between 1980 and 1984 is 17.4 percent, 2.5 times as large. Data on time in confinement are not available in the earlier survey, but we assume as a rough approximation that the percent in each of the time-served categories increased in the same proportion as the overall percent with any jail or prison time.

useful as a way to scale the percentage point reductions attributed to the various factors, but we remind the reader that, as discussed at length above, it is a net figure that reflects both positive and negative influences on the overall employment rate over the period we study.

Our review of the evidence leads us to conclude that labor demand factors are the primary drivers of the secular decline in employment over the 1999 to 2016 period. In this category, the effects of trade seem to be the single largest contributing factor to the decline in employment, potentially accounting for a 1.04 percentage point decline in the employment-to-population ratio. The next largest contributor appears to be the penetration of robots into the labor market. Based on the evidence reviewed, we attribute a decline in the employment-to-population ratio of 0.55 percentage points to this factor.

Labor supply factors as a group have been less important in driving the decline in employment, though still not inconsequential. Our rough estimate is that the growth in SSDI caseloads over the 1999 to 2016 period led the employment-to-population ratio to be 0.14 percentage points lower than it otherwise would have been. The Veteran Affairs Disability Compensation program also has contributed to a reduction in employment rates, on the order of an additional 0.06 percentage point reduction in the employment-to-population ratio. Increases in the real value of the minimum wage may have had a non-negligible impact on employment rates among prime-age adults, accounting for perhaps an additional 0.20 percentage point decline in the employment-to-population ratio over this period. Another supply side factor has been the rise in incarceration over this period and the resulting growth in the number of individuals with prison records. Our best guess is that this factor has contributed to a decline in the EPOP on the order of 0.13 percentage points.

We do not attempt to assign a magnitude to the possible contribution of improved leisure technology, in particular gaming technology, but call attention to the provocative hypothesis that has been advanced about its possible effects on young men's participation. This is an issue deserving additional attention, along with the consumption enhancing (and labor reducing) role that (endogenously) changing social norms and the increased likelihood of living with parents and other family members could be playing for young men. The rise in opioid use among prime-age individuals is another factor that has been associated with decreased employment rates, but we view the evidence on how much of the associated reduction in employment is causally driven by opioid supply rather than endogenous demand for drug use as still being rather speculative. This is another issue that warrants further research.

The difficulties that working parents face in reconciling their parental and work responsibilities also undoubtedly are a factor in individual labor supply decisions, but lack of public support for affordable child care or paid family leave in the United States cannot explain the secular *decline* in employment, as there have been no substantial changes in these policies. It is possible, however, that other forces have reduced the affordability or accessibility of child care, especially for low wage workers, and further research on that topic would be welcome. We do not attribute any of the reduction in aggregate employment to increases in the number of immigrants. The available evidence suggests that immigration may have had a modest effect on teen employment, but there is no consistent indication that it has affected either the overall employment rate or the employment of subgroups within the prime-age adult population.

Institutional factors and labor market frictions also appear to have been relatively unimportant as drivers of employment compared to the labor demand factors we have identified, but given the decline in worker mobility and the open question about the reasons for that decline, we view this as a topic on which the literature has not yet produced a definitive answer. The literature on the role of occupational licensing in leading to lower rates of employment or employment growth is in our view similarly inconclusive to date.

Even where we have entered an estimate of the size of a factor's effect on aggregate employment, our numbers are necessarily speculative. An important consideration is that, as described above, many of the estimates in the literature from which we draw are identified based on some type of local variation in exposure to a policy or condition. Some of the authors of the papers we cite have incorporated econometric adjustments in an attempt to make aggregate statements based on a parameter estimated using local data. Where that is not the case, we have attempted to be careful in interpreting the available findings. Still, we acknowledge the uncertainty around the available estimates and urge caution in putting too much emphasis on the specific percentage point numbers.

A second note of caution concerns our implicit assumption that the different factors we have discussed are separable. The various factors we have considered do not operate in isolation—all of the estimates are context specific. For example, if the outside option of disability insurance benefits did not exist, the number of workers displaced by trade who dropped out of the labor force likely would have been lower. Alternatively, if the labor market had been stronger over this period, then the elasticity of work with respect to disability insurance benefits likely would have been smaller.

These caveats notwithstanding, our evidence-driven ranking of factors and the relative magnitudes assigned to them should be useful for guiding discussions about the main drivers of the reduction in the aggregate employment-to-population ratio and consequently the likely efficacy of alternative policy approaches to increasing employment rates going forward.

REFERENCES

Aaronson, Stephanie, Tomaz Cajner, Bruce Fallick, Felix Galbis-Reig, Christopher Smith, and William Wascher. 2014. “Labor Force Participation: Recent Developments and Future Prospects,” *Brookings Papers on Economic Activity*, 45(2): 197-255.

Aaronson, Daniel, Jonathan Davis, and Luojia Hu. 2012. “Explaining the Decline in the U.S. Labor Force Participation Rate,” *Chicago Fed Letter*, Federal Reserve Bank of Chicago, No. 296.

Abraham, Katharine G. 2015. “Is Skill Mismatch Impeding U.S. Economic Recovery?” *ILR Review* 68(2): 291-313.

Acemoglu, Daron, David Autor, David Dorn, Gordon H. Hanson, and Brendan Price. 2016. “Import Competition and the Great US Employment Sag of the 2000s,” *Journal of Labor Economics* 34(S1): S141-S198.

Acemoglu, Daron and Pascual Restrepo. 2017. “Robots and Jobs: Evidence from US Labor Markets,” NBER Working Paper No. 23285, Cambridge, MA.

Agan, Amanda and Sonja Starr. 2016. “Ban the Box, Criminal Records, and Statistical Discrimination: A Field Experiment,” Working Paper No. 16-012, Michigan Law, University of Michigan, Ann Arbor.

Aguiar, Mark, Mark Bils, Kerwin Charles, and Erik Hurst. 2017. “Leisure, Luxuries and the Labor Supply of Young Men,” NBER working paper No. 23552, Cambridge, MA.

Allegretto, Sylvia, Arindrajit Dube, and Michael Reich. 2011. “Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data,” *Industrial Relations* 50(2): 205-240.

Allegretto, Sylvia, Arindrajit Dube, Michael Reich, and Ben Zipperer. 2013. "Credible Research Designs for Minimum Wage Studies,” Institute for Research on Labor and Employment, Working Paper No. 148-13, University of California, Berkeley.

Altonji, Joseph G. and David Card. 1991. “The Effects of Immigration on the Labor Market Outcomes of Less-Skilled Natives,” in J. Abowd and R. Freeman, eds., *Immigration, Trade, and the Labor Market*, Chicago: University of Chicago Press, 201-234.

Anderson, Patricia M. and Philip B. Levine. 1999. “Child Care and Mothers’ Employment Decisions,” NBER Working Paper No. 7058, Cambridge, MA.

Andersson, Fredrik, John C. Haltiwanger, Mark J. Kutzbach, Henry O. Pollakowski, and Daniel H. Weinberg. Forthcoming. “Job Displacement and the Duration of Joblessness: The Role of Spatial Mismatch,” *Review of Economics and Statistics*.

Autor, David H., David Dorn, and Gordon H. Hanson. 2013. “The China Syndrome: Local Labor Market Effects of Import Competition in the United States,” *American Economic Review* 103: 2121-68.

Autor, David H., David Dorn, and Gordon H. Hanson. 2015. “Untangling Trade and Technology: Evidence from Local Labor Markets,” *The Economic Journal* 125(584): 621-646.

Autor, David, David Dorn, Gordon Hanson, and Jae Song. 2014. “Trade Adjustment: Worker Level Evidence,” *Quarterly Journal of Economics* 129(4): 1799-1860.

Autor, David H. and Mark G. Duggan. 2003. “The Rise in the Disability Rolls and the Decline in Unemployment,” *Quarterly Journal of Economics* 118 (1): 157–206.

Autor, David, Mark Duggan, Kyle Greenberg, and David S. Lyle. 2016. “The Impact of Disability Benefits on Labor Supply: Evidence from the VA’s Disability Compensation Program,” *American Economic Journal: Applied Economics* 8(3): 31-68.

Baicker, Katherine, Amy Finkelstein, Jae Song, and Sarah Taubman. 2014. "The Impact of Medicaid on Labor Market Activity and Program Participation: Evidence from the Oregon Health Insurance Experiment,” *American Economic Review* 104(5): 322-28.

Baker, Miccl, Jonathan Gruer, and Kevin Milligan. 2008. “Universal Child Care, Maternal Labor Supply, and Family Well-Being,” *Journal of Political Economy* 116(4): 709–745.

Basso, Gaetano, and Giovanni Peri. 2015. “The Association between Immigration and Labor Market Outcomes in the United States,” *Research Briefs in Economic Policy*, No. 43, Cato Institute, Washington, DC.

Bernard, Andrew, J. Bradford Jensen, and Peter Schott. 2006. “Survival of the Best Fit: Exposure to Low-Wage Countries and the (Uneven) Growth of U.S. Manufacturing Plants,” *Journal of International Economics* 68(1): 219-237.

Blair, Peter Q. and Bobby W. Chung. 2017. “Occupational Licensing Reduces Racial and Gender Wage Gaps: Evidence from the Survey of Income and Program Participation,” Clemson University, unpublished working paper.

Blanchard, Olivier Jean and Lawrence F Katz. 1992. “Regional Evolutions,” *Brookings Papers on Economic Activity*, 23(1): 1-76.

Blau, David and Philip K. Robins. 1988. "Child-Care Costs and Family Labor Supply," *The Review of Economics and Statistics* 70(3): 374-381.

Blau, Francine and Lawrence Kahn. 2013. "Female Labor Supply: Why is the US Falling Behind?" IZA Discussion Paper No. 7140, Bonn, Germany..

Blau, Francine and Christopher Mackie. 2016. *The Economic and Fiscal Consequences of Immigration.* The National Academies Press: Washington, D.C.

Borjas, George J. 2003. "The Labor Demand Curve is Downward sloping: Reexamining the Impact of Immigration on the Labor Market," *Quarterly Journal of Economics* 118(4): 1335-1374.

Borjas, George J. 2006. "Native Internal Migration and the Labor Market Impact of Immigration," *Journal of Human Resources* 41(2): 221-258.

Bound, John, 1989. "The Health and Earnings of Rejected Disability Insurance Applicants," *American Economic Review* 79(3): 482-503.

Boushey, Heather and Bridget Ansel. 2016. "Working by the Hour," Washington Center for Equitable Growth. Washington, DC.

Boyle, Melissa A. and Joanna N. Lahey. 2010. "Health Insurance and the Labor Supply Decisions of Older Workers: Evidence from a U.S. Department of Veterans Affairs Expansion," *Journal of Public Economics* 94: 467–478.

Brown, Charles, Curtis Gilroy, and Andrew Kohen. 1982. "The Effect of the Minimum Wage on Employment and Unemployment," *Journal of Economic Literature* 20(2): 487-528.

Brummond, Peter and Michael Strain. 2016. "Minimum Wage Heterogeneity: Inflation-Indexing and Local Wages," University of Alabama, unpublished working paper.

Buchmueller, Thomas, John C. Ham, and Lara D. Shore-Sheppard. 2016. "The Medicaid Program," in R. Moffitt, ed., *Economics of Means-Tested Transfer Programs in the United States*,Chicago: University of Chicago Press, 21–136.

Bucknor, Cherrie and Alan Barber. 2016. "The Price We Pay: Economic Costs of Barriers to Employment for Former Prisoners and People Convicted of Felonies," Center for Economic and Policy Research, Washington D.C.

Card, David. 1992a. "Using Regional Variation in Wages To Measure the Effects of the Federal Minimum Wage," *Industrial and Labor Relations Review* 46(1): 22-37.

Card, David. 1992b. "Do Minimum Wages Reduce Employment? A Case Study of California, 1987-89," *Industrial and Labor Relations Review* 46(1): 38-54.

Card, David. 2001. "Immigrant Inflows, Native Outflows, and the Local Labor Market Impacts of Higher Immigration," *Journal of Labor Economics*, 19(1): 2-64.

Card, David and Alan Krueger. 2000. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Reply," *American Economic Review* 90(5): 1397-1420.

Card, David and John DiNardo. 2000. "Do Immigrant Inflows Lead to Native Outflows?," *American Economic Review* 90(2): 360-367.

Cascio, Elizabeth. 2009. "Maternal Labor Supply and the Introduction of Kindergartens into American Public Schools," *The Journal of Human Resources* 44(1): 140-170

Cascio, Elizabeth, and Diane Whitmore Schanzenbach. 2013. "The Impacts of Expanding Access to High-Quality Preschool Education," *Brookings Papers on Economic Activity,* 44(2): 127-192

Case, Ann and Angus Deaton. 2017. "Mortality and Morbidity in the 21st Century," *Brookings Papers on Economic Activity*: 397-443.

Cenzig, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2017. "The Effect of Minimum Wages on the Total Number of Jobs: Evidence from the United States Using a Bunching Estimator," University of Massachusetts mimeo.

Charles, K.K., E. Hurst and M.J. Notowidigdo. 2016. "The Masking of Declining Manufacturing Employment by the Housing Bubble," *Journal of Economic Perspectives* 30(2): 179-200.

Chen, Susan and Wilbert van der Klaauw. 2008. "The Effect of Disability Insurance on Labor Supply of Older Individuals in the 1990s," *Journal of Econometrics* 142(2): 757-784.

Clemens, Jeffrey and Michael Wither. 2014. "The Minimum Wage and the Great Recession: Evidence of Effects on the Employment and Income Trajectories of Low-Skilled Workers," NBER Working Paper No. 20724, Cambridge, MA.

Coile, Courtney, Mark Duggan, and Audrey Guo. 2015. "Veterans' Labor Force Participation: What Role Does the VA's Disability Compensation Program Play?" *American Economic Review* May 105(5): 131-36.

Congressional Budget Office. 2014a. *The Effects of a Minimum-Wage Increase on Employment and Family Income*, February, Congressional Budget Office. Washington, D.C.

Congressional Budget Office. 2014b. *The Budget and Economic Outlook: 2014 to 2024*, February, Congressional Budget Office. Washington, D.C.

Connelly, Rachel. 1992. “The Effect of Child Care Costs on Married Women’s Labor Force Participation,” *The Review of Economics and Statistics* 74(1): 83-90

Connelly, Rachel and Jean Kimmel. 2003. “The Effect of Child Care Costs on the Employment and Welfare Recipiency of Single Mothers,” *Southern Economic Journal* 69(3): 498-519

Council of Economic Advisers. 2016. “The Long-Term Decline in Prime-Age Male Labor Force Participation,” Washington, DC, June.

Dague, Laura, Thomas DeLeire, and Lindsey Leininger. 2017. "The Effect of Public Insurance Coverage for Childless Adults on Labor Supply,” *American Economic Journal: Economic Policy*, 9(2): 124-154.

Dao, Mai, Davide Furceri, and Prakash Loungani. 2017. “Regional Labor Market Adjustment in the United States: Trend and Cycle,” *Review of Economics and Statistics* 99(2): 243-257.

Davis, Steven and John Haltiwanger. 2014. “Labor Market Fluidity and Economic Performance,” NBER Working Paper No. 20749, Cambridge, MA.

Davis, Steven, Jason Faberman, and John Haltiwanger. 2013. "The Establishment-Level Behavior of Vacancies and Hiring,” *The Quarterly Journal of Economics* 128(2): 581-622.

DePasquale, Christina, and Kevin Stange. 2016. “Labor Supply Effects of Occupational Regulation: Evidence from the Nurse Licensure Compact,” NBER Working Paper No. 22344, Cambridge, MA.

Dillender, Marcus O., Carolyn J. Heinrich, and Susan N. Houseman. 2016. "Health Insurance Reform and Part-time Work: Evidence from Massachusetts,” *Labour Economics* 43: 151-158.

Doleac, Jennifer. 2016. Increasing Employment for Individuals with Criminal Records. A Hamilton Project Policy Memo. The Brookings Institution, Washington, D.C.

Doleac, Jennifer L. and Benjamin Hansen. 2016. “Does ‘Ban the Box’ Help or Hurt Low-Skilled Workers? Statistical Discrimination and Employment outcomes for those With and Without Criminal Records,” NBER Working Paper No. 22469, Cambridge, MA.

Dube, Arindrajit, T. William Lester, and Michael Reich. 2010. "Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties," *Review of Economics and Statistics* 92(4): 945–964.

Dube, Arindrajit and Ben Zipperer. 2015. "Pooling Multiple Case Studies Using Synthetic Controls: An Application to Minimum Wage Policies," IZA Discussion Paper No. 8944, Bonn, Germany.

Duggan, Mark, Robert Rosenheck, and Perry Singleton. 2010. "Federal Policy and the Rise in Disability Enrollment: Evidence for the VA's Disability Compensation Program" *Journal of Law and Economics* 53(2): 379-398.

Duggan, Mark, Melissa S. Kearney, and Stephanie Rennane. 2016. "The Supplemental Security Income Program" in R. Moffitt, ed., *Economics of Means-Tested Transfer Programs in the United States*, Chicago: University of Chicago Press, 1-58.

Duggan, Mark, Gopi Shah Goda, and Emilie Jackson. 2017. "The Effects of the Affordable Care Act on Health Insurance Coverage and Labor Market Outcomes," NBER Working Paper No. 23607, Cambridge, MA.

Dunn, Megan and James Walker. 2016. "Union Membership in the United States," *Spotlight on Statistics*, Bureau of Labor Statistics, Washington, DC.

East, Chloe. 2016. "The Labor Supply Response to Food Stamp Access," University of Colorado Denver, unpublished working paper.

Eberstadt, Nicholas. 2016. *Men without Work*. West Conshohocken, PA: Templeton Press.

Eissa, Nada and Jeffrey Liebman. 1996. "Labor Supply Response to the Earned Income Tax Credit," *The Quarterly Journal of Economics* 111(2): 605-637.

Eissa, Nada and Hilary Hoynes. 2004. "Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit," *Journal of Public Economics* 88(9-10): 1931-1958.

Eriksson, Stefan and Dan-Olof Rooth. 2014. "Do Employers Use Unemployment as a Sorting Criterion When Hiring? Evidence from a Field Experiment," *American Economic Review* 104(3): 1014-39.

Farber, Henry S., Dan Silverman, and Till von Wachter. 2015. "Factors Determining Callbacks to Job Applications by the Unemployed: An Audit Study," NBER Working Paper No. 21689, Cambridge, MA.

Fitzpatrick, Marie D. 2010. “Preschoolers Enrolled and Mothers at Work? The Effects of Universal Prekindergarten,” *Journal of Labor Economics* 28(1): 51-85

French, Eric and Jae Song. 2014. "The Effect of Disability Insurance Receipt on Labor Supply," *American Economic Journal: Economic Policy* 6(2): 291-337.

Frey, Carl B. and Michael A. Osborne. 2013. “The Future of Employment: How Susceptible are Jobs to Computerization?,” Oxford Martin School, unpublished working paper.

Ganong, Peter and Daniel Shoag. 2017. “Why Has Regional Income Convergence in the U.S. Declined?” *Journal of Urban Economics* 102: 76-90.

Garthwaite, Craig, Tal Gross, and Matthew J. Notowidigdo. 2013. “Public Health Insurance, Labor Supply, and Employment Lock,” *Quarterly Journal of Economics* 129(2): 653-696.

Gelbach, Jonah B. 2002. "Public Schooling for Young Children and Maternal Labor Supply," *American Economic Review* 92(1): 307-322.

Gelber, Alexander, Timothy Moore, and Alexander Strand. 2017. “The Effect of Disability Insurance Payments on Beneficiaries' Earnings,” *American Economic Journal: Economic Policy* 9(3): 229-61.

Ghayad, Rand. 2013. “The Jobless Trap,” Northeastern University, unpublished working paper.

Gittleman, Maury, Mark A. Klee and Morris M. Kleiner. 2015. “Analyzing the Labor Market Outcomes of Occupational Licensing,” NBER Working Paper No. 20961, Cambridge, MA.

Haeck, Catherine, Pierre Lefebvre, and Philip Merrigan. 2015 “Canadian Evidence on Ten Years of Universal Preschool Policies: The Good and the Bad,” *Labour Economics* 36: 137-157.

Hall, Robert E. and Sam Schulhofer-Wohl. 2015. “Measuring Job-Finding Rates and Matching Efficiency with Heterogeneous Job-Seekers,” Working Paper No. 721, Federal Reserve Bank of Minneapolis.

Havnes, Tarjei and Magne Mogstad. 2011. "No Child Left Behind: Subsidized Child Care and Children's Long-Run Outcomes," *American Economic Journal: Economic Policy* 3(2): 97-129.

Heim, Brad, Ithai Lurie and Kosali Simon. Forthcoming. “Did the Affordable Care Act Young Adult Provision Affect Labor Market Outcomes? Analysis Using Tax Data” *Industrial and Labor Relations Review.*

Hirsch, Barry and David Macpherson. 2003. “Union Membership and Coverage Database from the Current Population Survey: Note,” *ILR Review* 56(2): 349-354.

Hoynes, Hilary W. and Diane W. Schanzenbach. 2012. “Work Incentives and the Food Stamp Program,” *Journal of Public Economics* 96(1): 151-162.

Hoynes, Hilary W. and Diane W. Schanzenbach. 2016. “U.S. Food and Nutrition Programs,” in R. Moffitt, ed., *Economics of Means-Tested Transfer Programs in the United States*, Chicago: University of Chicago Press, 219–301.

Hunt, Jennifer and Marjolaine Gauthier-Loiselle, 2010. "How Much Does Immigration Boost Innovation?" *American Economic Journal: Macroeconomics*, 2(2): 31-56.

Jardim, Ekaterina, Mark C. Long, Robert Plotnick, Emma van Inwegen, Jacob Vigdor, and Hilary Wething. 2017. “Minimum Wage Increases, Wages, and Low-wage Employment: Evidence from Seattle,” NBER Working Paper No. 23532, Cambridge, MA.

Juhn, Chinhui and Simon Potter. 2006. “Changes in Labor Force Participation in the United States,” *Journal of Economic Perspectives* 20(3): 27-46.

Kaplan, Greg and Sam Schulhofer-Wohl. 2017. “Understanding the Long-Run Decline in Interstate Migration,” *International Economic Review* 58(1): 57-94.

Katz, Lawrence F. and Alan B Krueger. 1992. "The Effect of the Minimum Wage on the Fast Food Industry,” *Industrial and Labor Relations Review* 46(1): 6-21.

Kearney, Melissa, Ben Harris, Elisa Jacome, and Lucy Parker. 2014. “Ten Economic Facts about Crime and Incarceration in the United States,” The Hamilton Project. Washington, D.C.

Kerr, William R., and William F. Lincoln. 2010. “The Supply Side of Innovation: H1-B Visa Reforms and U.S. Ethnic Invention,” *Journal of Labor Economics* 28(3): 473-508.

Kimmel, Jean. 1998. “Child Care Costs as a Barrier to Employment for Single and Married Mothers,” *Review of Economics and Statistics* 80(2): 287-299.

Kleiner, Morris M. 2006. *Licensing Occupations: Ensuring Quality or Restricting Competition?*, Kalamazoo, MI: W. E. Upjohn Institute for Employment Research.

Kleiner, Morris. 2015. “Reforming Occupational Licensing Policies,” The Hamilton Project. Washington, D.C.

Kleiner, Morris M., and Alan B. Krueger. 2013. “Analyzing the Extent and Influence of Occupational Licensing on the Labor Market,” *Journal of Labor Economics* 31(S1): 173-202.

Kleven, Henrik J. 2014. “How Can Scandinavians Tax So Much?” *Journal of Economic Perspectives* 28:4: 77-98.

Kolstad, Jonathan and Amanda Kowalski. 2016. "Mandate-Based Health Reform and the Labor Market: Evidence from the Massachusetts Reform,” *Journal of Health Economics* 47: 81-106.

Kroft, Kory, Fabian Lange, and Matthew J. Notowidigdo, 2013. “Duration Dependence and Labor Market Conditions: Evidence from a Field Experiment,” *Quarterly Journal of Economics* 128(3): 1123-1167.

Krueger, Alan B. 2017. “Where Have All the Workers Gone? An Inquiry into the Decline of the U.S. Labor Force Participation Rate,” *Brookings Papers on Economic Activity*.

Leung, Pauline, and Alexandre Mas. 2016. “Employment Effects of the ACA Medicaid Expansions,” NBER Working Paper No. 22540, Cambridge, MA.

Liebman, Jeffrey B. 2015. "Understanding the Increase in Disability Insurance Benefit Receipt in the United States,” *Journal of Economic Perspectives* 29(2): 123-50.

Lundin, Daniela, Eva Mork, and Bjorn Ockert. 2008 “How Far Can Reduced Childcare Prices Push Female Labour Supply?” *Labour Economics* 15(4): 647-659.

Maestas, Nicole, Kathleen J. Mullen, and Alexander Strand. 2013. "Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt,” *American Economic Review* 103(5): 1797-1829.

Mayer, Gerald. 2004. “Union Membership Trends in the United States,” August, Washington D.C., Congressional Research Service.

Meyer, Bruce D. and Dan T. Rosenbaum. 2001. “Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers,” *Quarterly Journal of Economics* 116(3): 1063-1114.

Meer, Jonathan and Jeremy West. 2016. “Effects of Minimum Wage on Employment Dynamics,” NBER Working Paper No. 19262, Cambridge, MA.

Moffitt, Robert A. 2012. “The Reversal of the Employment-to-population Ratio in the 2000s: Facts and Explanations,” *Brookings Papers on Economic Activity*, 43(2), 201-264.

Molloy, Raven, Christopher L. Smith, and Abigail Wozniak. 2011. “Internal Migration in the United States,” *Journal of Economic Perspectives* 25(3): 173–196.

Molloy, Raven, Christopher L. Smith, Riccardo Trezzi, and Abigail Wozniak. 2016. "Understanding Declining Fluidity in the U.S. Labor Market," *Brookings Papers on Economic Activity*, 47(1): 183-259.

Moore, Timothy J. 2015 "The Employment Effects of Terminating Disability Benefits," *Journal of Public Economics* 124: 30–43.

Mueller-Smith, Michael. 2015. "The Criminal and Labor Market Impacts of Incarceration," University of Michigan, unpublished working paper.

Neumark, David and William Wascher. 1992. "Employment Effects of Minimum and Subminimum Wages: Panel Data on State Minimum Wage Laws," *ILR Review* 46(1): 55-81.

Neumark, David and William Wascher. 2000. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Comment," *American Economic Review* 90(5): 1362-1396.

Neumark, David, J.M. Ian Salas, and William Wascher. 2013. "Revisiting the Minimum Wage-Employment Debate: Throwing Out the Baby with the Bathwater?" NBER Working Paper No. 18681, Cambridge, MA.

Nichols, Austin and Jesse Rothstein. 2015. "The Earned Income Tax Credit," in R. Moffitt, ed., *Economics of Means-Tested Transfer Programs in the United States*, Chicago, IL: University of Chicago Press, 137-218.

Nollenberger, Natalia and Núria Rodríguez-Planas. 2015. "Full-time Universal Childcare in a Context of Low Maternal Employment: Quasi-experimental Evidence from Spain," *Labour Economics* 36: 124-136.

Orrenius, Pia M. and Madeline Zavodny. 2007. "Does Immigration Affect Wages? A Look at Occupation-level Evidence," *Labour Economics* 14(5): 757-773.

Ottaviano, Gianmarco and Giovanni Peri. 2012. "Rethinking the Effect of Immigration on Wages," *Journal of the European Economic Association* 10(1): 152-197.

Peri, Giovanni. 2007. "Immigrants' Complementarities and Native Wages: Evidence from California," NBER Working Paper No. 12956. Cambridge: MA.

Peri, Giovanni and Chad Sparber. 2009. "Task Specialization, Immigration, and Wages," *American Economic Journal: Applied Economics*, 1:3: 135-169.

Peterson, Brenton D., Sonal S. Pandya, and David Leblang. 2014. "Doctors With Borders: Occupational Licensing as an Implicit Barrier to High Skill Migration," *Public Choice* 160(1-2): 45-63.

Pierce, Justin R and Peter K. Schott. 2016. "The Surprisingly Swift Decline of US Manufacturing Employment," *American Economic Review* 106(7): 1632-62.

Powell, David. 2016. "Synthetic Control Estimation Beyond Case Studies: Does the Minimum Wage Reduce Employment?" Rand Working Paper No. 1142, RAND Corporation, Santa Monica, CA.

Raphael, Steven and Michael Stoll. 2013. *Why Are So Many Americans in Prison?*, New York: Russell Sage Foundation.

Ribar, David. 1992. "Child Care and the Labor Supply of Married Women: Reduced Form Evidence" *The Journal of Human Resources* 27(1): 134-165

Rossin-Slater, Maya. 2016. "Maternity and Family Leave Policy," NBER Working Paper No. 23069, Cambridge, MA.

Şahin, Ayşegül, Joseph Song, Giorgio Topa and Giovanni L. Violante. 2014. "Mismatch Unemployment," *American Economic Review* 104(11): 3529-64.

Schmitt, John and Kris Warner. 2010. "Ex-offenders and the Labor Market," Center for Economic and Policy Research, Washington D.C.

Shannon, Sarah, Christopher Uggen, Jason Schnittker, Melissa Thompson, Sara Wakefield, and Michael Massoglia. 2017. "The Growth, Scope, and Spatial Distribution of People with Felony Records in the United States, 1948-2010." *Demography* 54(5): 1795-1818.

Sorkin, Isaac. 2015. "Are There Long-Run Effects of the Minimum Wage?" *Review of Economic Dynamics* 18(2): 306-333.

Smith, Christopher L. 2012. "The Impact of Low-Skilled Immigration on the Youth Labor Market," *Journal of Labor Economics* 30(1): 55-89.

von Wachter, Till, Jae Song, and Joyce Manchester. 2011. "Trends in Employment and Earnings of Allowed and Rejected Applicants to the Social Security Disability Insurance Program," *American Economic Review* 101(7): 3308-29.

Western, Bruce and Christopher Wildeman. 2009. "The Black Family and Mass Incarceration," *Annals of the American Academy of Political and Social Sciences* 621: 221-42.

World Bank. 2016. *Digital Dividends*. World Development Report 2016. A World Bank Group Flagship Report, Washington, DC.

Ziliak, James P. 2014. “Supporting Low-Income Workers through Refundable Child-Care Credits,” The Hamilton Project. Washington, DC.

Table 1A: Changes in Employment-Population Ratios and Population Shares, Total, by Age and Education, 1999-2016

	E/P_{1999}	E/P_{2016}	$\Delta E/P_{99\text{-}16}$	s_{1999}	s_{2016}	$\Delta s_{99\text{-}16}$
Age 16-24	0.590	0.494	-0.096	0.164	0.152	-0.012
Age 25-34	0.813	0.774	-0.038	0.183	0.172	-0.011
Age 35-44	0.823	0.793	-0.030	0.215	0.157	-0.058
Age 45-54	0.805	0.772	-0.033	0.171	0.167	-0.004
Age 55-64	0.577	0.618	0.041	0.111	0.163	0.052
Age 65+	0.119	0.186	0.066	0.156	0.190	0.033
Age 16-24						
Not In School	0.726	0.680	-0.046	0.085	0.072	-0.013
In School	0.443	0.327	-0.116	0.079	0.080	0.001
Age 25-34						
Less than HS	0.650	0.598	-0.052	0.022	0.014	-0.008
HS	0.797	0.714	-0.083	0.056	0.044	-0.012
Some College	0.835	0.780	-0.055	0.051	0.049	-0.002
College	0.875	0.853	-0.023	0.053	0.064	0.010
Age 35-44						
Less than HS	0.663	0.642	-0.021	0.025	0.016	-0.008
HS	0.814	0.740	-0.073	0.072	0.040	-0.032
Some College	0.845	0.805	-0.040	0.059	0.041	-0.018
College	0.879	0.861	-0.019	0.059	0.060	0.001
Age 45-54						
Less than HS	0.595	0.587	-0.008	0.020	0.017	-0.002
HS	0.771	0.723	-0.048	0.053	0.048	-0.005
Some College	0.833	0.779	-0.054	0.047	0.045	-0.002
College	0.892	0.863	-0.029	0.052	0.057	0.005
Age 55-64						
Less than HS	0.408	0.421	0.013	0.021	0.017	-0.004
HS	0.554	0.570	0.016	0.040	0.050	0.010
Some College	0.620	0.625	0.006	0.024	0.046	0.021
College	0.710	0.723	0.013	0.026	0.050	0.025
Age 65+						
Less than HS	0.071	0.094	0.023	0.049	0.027	-0.022
HS	0.113	0.142	0.028	0.055	0.062	0.007
Some College	0.144	0.198	0.054	0.028	0.045	0.017
College	0.204	0.272	0.068	0.024	0.055	0.031
TOTAL	**0.643**	**0.597**	**-0.045**	1.000	1.000	0.000

Notes for Tables 1A-1C: Authors' calculations using monthly CPS data downloaded from IPUMS-CPS. Sample restricted to individuals 16 and older. Data weighted using CPS composite weights.

Table 1B: Changes in Employment-Population Ratios and Population Shares, Men, by Age and Education, 1999-2016

	E/P_{1999}	E/P_{2016}	$\Delta E/P_{99\text{-}16}$	s_{1999}	s_{2016}	$\Delta s_{99\text{-}16}$
Age 16-24	0.610	0.501	-0.110	0.171	0.158	-0.013
Age 25-34	0.899	0.843	-0.056	0.186	0.176	-0.010
Age 35-44	0.902	0.873	-0.028	0.220	0.159	-0.061
Age 45-54	0.865	0.834	-0.031	0.174	0.169	-0.005
Age 55-64	0.661	0.675	0.014	0.110	0.162	0.052
Age 65+	0.164	0.231	0.067	0.138	0.175	0.037
Age 16-24						
Not In School	0.778	0.706	-0.072	0.090	0.077	-0.012
In School	0.424	0.304	-0.121	0.081	0.081	-0.001
Age 25-34						
Less than HS	0.815	0.753	-0.063	0.024	0.016	-0.008
HS	0.892	0.796	-0.096	0.060	0.052	-0.009
Some College	0.913	0.849	-0.065	0.049	0.048	-0.001
College	0.933	0.904	-0.029	0.053	0.060	0.007
Age 35-44						
Less than HS	0.778	0.779	0.001	0.027	0.018	-0.009
HS	0.891	0.824	-0.066	0.076	0.045	-0.030
Some College	0.918	0.879	-0.038	0.056	0.040	-0.016
College	0.954	0.939	-0.015	0.062	0.056	-0.006
Age 45-54						
Less than HS	0.700	0.696	-0.004	0.020	0.019	-0.001
HS	0.840	0.790	-0.050	0.049	0.052	0.003
Some College	0.876	0.836	-0.039	0.047	0.042	-0.006
College	0.934	0.919	-0.015	0.057	0.056	-0.001
Age 55-64						
Less than HS	0.514	0.510	-0.005	0.020	0.018	-0.003
HS	0.640	0.629	-0.010	0.036	0.051	0.015
Some College	0.681	0.674	-0.007	0.023	0.043	0.019
College	0.767	0.779	0.012	0.031	0.051	0.020
Age 65+						
Less than HS	0.104	0.129	0.025	0.043	0.024	-0.019
HS	0.153	0.179	0.026	0.041	0.051	0.009
Some College	0.183	0.231	0.048	0.025	0.040	0.016
College	0.252	0.316	0.064	0.029	0.060	0.030
TOTAL	**0.716**	**0.658**	**-0.059**	1.000	1.000	0.000

Table 1C: Changes in Employment-Population Ratios and Population Shares, Women, by Age and Education, 1999-2016

	E/P_{1999}	E/P_{2016}	$\Delta E/P_{99\text{-}16}$	s_{1999}	s_{2016}	$\Delta s_{99\text{-}16}$
Age 16-24	0.570	0.488	-0.082	0.157	0.145	-0.011
Age 25-34	0.730	0.707	-0.023	0.180	0.168	-0.012
Age 35-44	0.746	0.715	-0.031	0.210	0.155	-0.055
Age 45-54	0.748	0.712	-0.035	0.169	0.165	-0.004
Age 55-64	0.501	0.565	0.064	0.112	0.164	0.052
Age 65+	0.087	0.149	0.063	0.173	0.203	0.030
Age 16-24						
Not In School	0.672	0.651	-0.021	0.081	0.066	-0.014
In School	0.461	0.350	-0.111	0.076	0.079	0.003
Age 25-34						
Less than HS	0.470	0.409	-0.061	0.020	0.013	-0.008
HS	0.697	0.609	-0.089	0.053	0.038	-0.015
Some College	0.767	0.718	-0.049	0.052	0.050	-0.002
College	0.824	0.810	-0.014	0.054	0.067	0.013
Age 35-44						
Less than HS	0.538	0.486	-0.052	0.023	0.015	-0.008
HS	0.736	0.638	-0.098	0.069	0.035	-0.034
Some College	0.784	0.738	-0.046	0.062	0.042	-0.020
College	0.803	0.796	-0.007	0.056	0.063	0.007
Age 45-54						
Less than HS	0.494	0.467	-0.028	0.019	0.016	-0.003
HS	0.716	0.649	-0.067	0.057	0.044	-0.013
Some College	0.793	0.732	-0.061	0.046	0.048	0.001
College	0.844	0.811	-0.033	0.047	0.058	0.011
Age 55-64						
Less than HS	0.312	0.327	0.014	0.021	0.016	-0.005
HS	0.491	0.514	0.023	0.045	0.050	0.005
Some College	0.566	0.585	0.019	0.025	0.048	0.023
College	0.633	0.670	0.037	0.021	0.050	0.029
Age 65+						
Less than HS	0.047	0.067	0.019	0.055	0.030	-0.025
HS	0.091	0.117	0.027	0.067	0.073	0.006
Some College	0.115	0.173	0.058	0.031	0.050	0.019
College	0.137	0.222	0.085	0.020	0.050	0.031
TOTAL	**0.574**	**0.541**	**-0.033**	1.000	1.000	0.000

Table 2A: Share of overall EPOP changes attributable to changes in population composition and within-group employment changes

	Overall	Male	Female
Contribution of $s_i*\Delta E/P_i$			
16-24	39.3%	36.4%	43.4%
25-54	41.5%	36.6%	49.2%
55-64	-10.9%	-3.4%	-22.5%
65+	-18.7%	-13.1%	-26.1%
Contribution of $E/P_i*\Delta s_i$			
16-24	-6.7%	-5.7%	-3.5%
25-54	17.7%	24.2%	37.0%
55-64	18.0%	6.5%	14.1%
65+	39.9%	32.1%	38.3%
Contribution of $\Delta E/P_i* \Delta s_i$			
16-24	-3.9%	-3.5%	-4.4%
25-54	-5.2%	-3.8%	-7.0%
55-64	-5.8%	-2.1%	-11.4%
65+	-5.2%	-4.1%	-6.9%
TOTAL	100.00%	100.00%	100.00%

Notes for Tables 2A-2B: Authors calculations using monthly CPS data downloaded from IPUMS-CPS. Sample restricted to individuals 16 and older. Data weighted using CPS composite weights. Numbers calculated using detailed age categories (16-19, 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59, 60-64, 65-69, 70-74, and 75+ years) and then aggregated to the broader age groupings shown.

Table 2B: Share of overall EPOP changes attributable to changes in population composition and within-group employment changes

Contribution of s_i*$\Delta E/P_i$	Overall	Male	Female
Age 16-24			
Not in School	11.0%	13.2%	7.7%
In School	22.6%	18.5%	28.9%
Age 25-54			
Less than HS	4.0%	2.5%	8.8%
HS	27.3%	22.1%	45.1%
Some College	16.9%	12.0%	24.6%
College	8.4%	5.5%	7.9%
Age 55-64			
Less than HS	-0.2%	0.4%	-0.5%
HS	-1.3%	0.7%	-2.8%
Some College	-0.9%	-0.1%	-2.3%
College	-1.6%	-1.3%	-3.4%
Age 65+			
Less than HS	-2.2%	-1.8%	-2.7%
HS	-3.7%	-1.8%	-6.0%
Some College	-2.4%	-1.5%	-3.8%
College	-2.7%	-2.7%	-3.5%
Contribution of E/P_i*Δs_i			
Age 16-24			
Not in School	-1.1%	-0.1%	2.8%
In School	-1.3%	-1.7%	-2.1%
Age 25-54			
Less than HS	-2.6%	2.2%	-4.0%
HS	10.2%	11.3%	28.2%
Some College	6.4%	7.6%	13.2%
College	-5.9%	0.2%	-23.5%
Age 55-64			
Less than HS	-3.2%	-1.4%	-4.5%
HS	3.3%	1.9%	1.0%
Some College	6.1%	2.0%	3.2%
College	2.5%	-0.2%	-1.9%

-

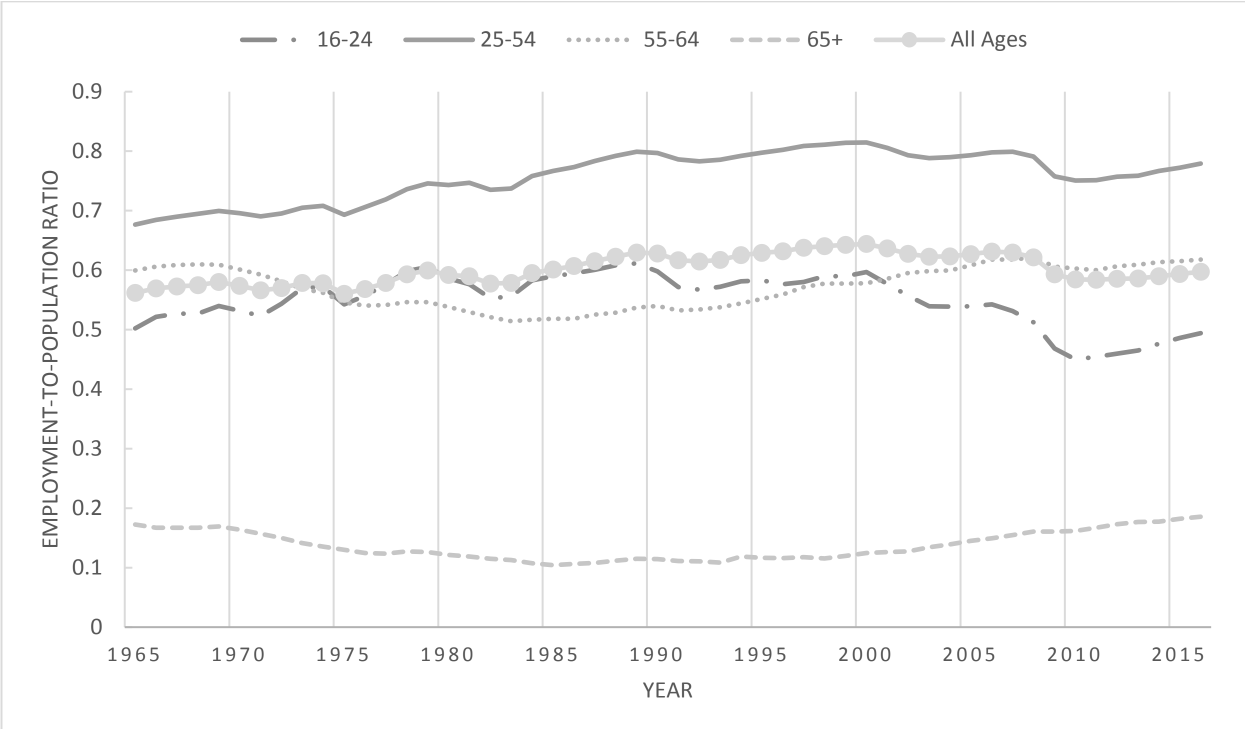
Table 2B: Share of overall EPOP changes attributable to changes in population composition and within-group employment changes (contd)

	Overall	Male	Female
Contribution of $E/P_i{*}\Delta s_i$ (continued)			
Age 65+			
Less than HS	-19.4%	-39.8%	-31.5%
HS	8.8%	8.9%	10.0%
Some College	13.6%	23.7%	20.6%
College	23.3%	36.8%	32.9%
Contribution of $\Delta E/Pi{*}\ \Delta si$			
Age 16-24			
Not in School	-2.3%	-2.1%	-2.3%
In School	-0.5%	0.3%	-0.1%
Age 25-54			
Less than HS	-0.8%	-2.9%	-1.3%
HS	-4.5%	-16.8%	-8.1%
Some College	-1.5%	-3.2%	-2.2%
College	0.2%	1.7%	0.9%
Age 55-64			
Less than HS	0.1%	0.2%	0.1%
HS	0.3%	-0.2%	-0.3%
Some College	-0.2%	-3.1%	-1.2%
College	-1.3%	-5.3%	-2.0%
Age 65+			
Less than HS	0.7%	1.1%	0.9%
HS	-0.4%	-0.4%	-0.4%
Some College	-1.0%	-2.9%	-1.7%
College	-3.0%	-6.1%	-3.8%
TOTAL	100.00%	100.00%	100.00%

Table 3: Factors Contributing to the Decline in Employment-to-Population Ratio from 1999-2016

Factors	Estimated reduction in EPOP (percentage point)
Major contributing factors	
Expanded trade with China	1.04
Adoption of industrial robots	0.55
Significant contributing factors	
Increased receipt of disability benefits (SSDI, VADC)	(0.14+0.06=) 0.20
Higher minimum wages	0.20
Increased rate of incarceration	0.13
Insignificant factors	
SNAP expansions	~0
Public health insurance expansions	~0
More generous EITC	~0
Increased rates of spousal employment	~0
Increased difficulties due to lack of family leave	~0
Expanded immigration	~0
Decline in unionization	~0
Indeterminate given state of evidence	
Changes in leisure options /social norms (including video games and opioids)	unclear
Increased difficulties due to lack of child care	unclear
Rise in occupational licensing	unclear
Increases in institutional frictions and/or mismatch	unclear
TOTAL NET EPOP DECLINE (percentage points)	4.5

Figure 1: Employment-to-Population Ratio, by Age, 1965-2016



Source: Bureau of Labor Statistics.