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Explaining the Decline in the US Employment-to-Population Ratio: A Review of the Evidence†

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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 2018. Population aging has had a large effect on the overall employment rate over this period, but within-age-group declines in employment among young- and prime-age adults also have played a central role. Among the factors with effects that we can quantify based on existing evidence, labor demand factors, in particular increased import competition from China 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 state minimum wages and in the share of individuals with prison records also have contributed modestly to the decline in the aggregate employment rate. In addition to the factors whose effects we roughly quantify, we identify a set of potentially important factors about which the evidence does not yet allow us to draw clear conclusions. These include the challenges associated with arranging child care, improvements in leisure technology, changing social norms, increased use of opioids, the growth in occupational licensing, and declining labor market fluidity. 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. (*JEL* E24, J64)

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1. *Introduction*

For several decades now, the employment rate among prime-age US 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 had been rising since the late 1960s, but beginning about two decades ago stagnated and then fell. Although the Great Recession exacerbated these worrisome developments, 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 2018, 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 2018.[2] One note of caution concerning these estimates is that the different factors we discuss are in fact unlikely to be separable. None of the various factors we will consider operates in isolation and all of the estimates are context specific. For example, if the outside option of disability insurance benefits had not existed, the number of workers displaced by trade who dropped out of the labor force likely would have been lower. Alternatively, if the labor market for low-wage workers had been stronger over the period we examine, then the elasticity of work with respect to disability insurance benefits might well have been smaller. This important caveat 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.

2. *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 US labor market. Tables 1A, 1B, and 1C display simple tabulations for the overall, male, and female population ages sixteen and older, showing annual average employment-to-population ratios and population shares by age

[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.

[2] This approach is very different from the approach taken by some 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 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.

TABLE 1A
Changes in Employment-to-Population Ratios and Population Shares: Total, by Age and Education, 1999–2018

	E/P_{1999}	E/P_{2018}	$\Delta E/P_{99\text{–}18}$	s_{1999}	s_{2018}	$\Delta s_{99\text{–}18}$
Age 16–24	0.590	0.505	−0.085	0.164	0.147	−0.016
Age 25–34	0.813	0.792	−0.020	0.183	0.173	−0.010
Age 35–44	0.823	0.804	−0.019	0.215	0.157	−0.057
Age 45–54	0.805	0.785	−0.020	0.171	0.160	−0.011
Age 55–64	0.577	0.631	0.054	0.111	0.163	0.052
Age 65+	0.119	0.189	0.070	0.156	0.199	0.043
Age 16–24						
Not in school	0.726	0.705	−0.021	0.085	0.069	−0.016
In school	0.443	0.329	−0.114	0.079	0.079	0.000
Age 25–34						
Less than HS	0.650	0.606	−0.044	0.022	0.013	−0.009
HS	0.797	0.738	−0.059	0.056	0.045	−0.011
Some college	0.835	0.796	−0.039	0.051	0.048	−0.003
College	0.875	0.863	−0.013	0.053	0.067	0.014
Age 35–44						
Less than HS	0.663	0.657	−0.005	0.025	0.015	−0.009
HS	0.814	0.752	−0.062	0.072	0.039	−0.033
Some college	0.845	0.811	−0.034	0.059	0.040	−0.019
College	0.879	0.868	−0.012	0.059	0.063	0.004
Age 45–54						
Less than HS	0.595	0.617	0.022	0.020	0.016	−0.004
HS	0.771	0.734	−0.037	0.053	0.044	−0.009
Some college	0.833	0.792	−0.041	0.047	0.042	−0.005
College	0.892	0.864	−0.028	0.052	0.058	0.006
Age 55–64						
Less than HS	0.408	0.453	0.045	0.021	0.016	−0.004
HS	0.554	0.581	0.027	0.040	0.051	0.010
Some college	0.620	0.629	0.009	0.024	0.044	0.020
College	0.710	0.737	0.027	0.026	0.052	0.027
Age 65+						
Less than HS	0.071	0.100	0.029	0.049	0.026	−0.023
HS	0.113	0.147	0.034	0.055	0.064	0.009
Some college	0.144	0.198	0.055	0.028	0.049	0.021
College	0.204	0.265	0.061	0.024	0.060	0.036
TOTAL	0.643	0.604	−0.038	1.000	1.000	0.000

Notes: Authors' calculations using monthly Current Population Survey (CPS) data downloaded from IPUMS-CPS. Sample restricted to individuals 16 and older. Data weighted using CPS composite weights.

TABLE 1B
Changes in Employment-to-Population Ratios and Population Shares: Men, by Age and Education, 1999–2018

	E/P_{1999}	E/P_{2018}	$\Delta E/P_{99\text{–}18}$	s_{1999}	s_{2018}	$\Delta s_{99\text{–}18}$
Age 16–24	0.610	0.506	−0.104	0.171	0.153	−0.018
Age 25–34	0.899	0.857	−0.042	0.186	0.178	−0.008
Age 35–44	0.902	0.883	−0.018	0.220	0.160	−0.060
Age 45–54	0.865	0.845	−0.020	0.174	0.162	−0.012
Age 55–64	0.661	0.691	0.030	0.110	0.163	0.052
Age 65+	0.164	0.233	0.068	0.138	0.184	0.046
Age 16–24						
Not in school	0.778	0.727	−0.051	0.090	0.075	−0.015
In school	0.424	0.295	−0.129	0.081	0.079	−0.003
Age 25–34						
Less than HS	0.815	0.748	−0.067	0.024	0.015	−0.009
HS	0.892	0.823	−0.069	0.060	0.053	−0.008
Some college	0.913	0.857	−0.056	0.049	0.048	−0.001
College	0.933	0.910	−0.023	0.053	0.063	0.010
Age 35–44						
Less than HS	0.778	0.805	0.027	0.027	0.017	−0.010
HS	0.891	0.838	−0.053	0.076	0.044	−0.031
Some college	0.918	0.885	−0.033	0.056	0.039	−0.017
College	0.954	0.939	−0.015	0.062	0.059	−0.003
Age 45–54						
Less than HS	0.700	0.717	0.017	0.020	0.017	−0.003
HS	0.840	0.802	−0.038	0.049	0.048	−0.001
Some college	0.876	0.847	−0.029	0.047	0.040	−0.007
College	0.934	0.921	−0.013	0.057	0.056	−0.001
Age 55–64						
Less than HS	0.514	0.555	0.041	0.020	0.017	−0.004
HS	0.640	0.644	0.004	0.036	0.052	0.016
Some college	0.681	0.676	−0.005	0.023	0.041	0.018
College	0.767	0.794	0.027	0.031	0.052	0.022
Age 65+						
Less than HS	0.104	0.139	0.035	0.043	0.023	−0.020
HS	0.153	0.186	0.033	0.041	0.053	0.011
Some college	0.183	0.238	0.055	0.025	0.044	0.020
College	0.252	0.301	0.049	0.029	0.064	0.035
TOTAL	0.716	0.663	−0.053	1.000	1.000	0.000

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

TABLE 1C
Changes in Employment-to-Population Ratios and Population Shares: Women, by Age and Education, 1999–2018

	E/P_{1999}	E/P_{2018}	$\Delta E/P_{99\text{–}18}$	s_{1999}	s_{2018}	$\Delta s_{99\text{–}18}$
Age 16–24	0.570	0.503	–0.066	0.157	0.142	–0.015
Age 25–34	0.730	0.728	–0.002	0.180	0.168	–0.012
Age 35–44	0.746	0.727	–0.019	0.210	0.155	–0.055
Age 45–54	0.748	0.727	–0.020	0.169	0.158	–0.011
Age 55–64	0.501	0.575	0.074	0.112	0.164	0.053
Age 65+	0.087	0.154	0.068	0.173	0.213	0.039
Age 16–24						
Not in school	0.672	0.680	0.008	0.081	0.063	–0.017
In school	0.461	0.361	–0.100	0.076	0.078	0.002
Age 25–34						
Less than HS	0.470	0.432	–0.038	0.020	0.011	–0.009
HS	0.697	0.628	–0.069	0.053	0.038	–0.015
Some college	0.767	0.738	–0.029	0.052	0.048	–0.005
College	0.824	0.823	–0.001	0.054	0.071	0.017
Age 35–44						
Less than HS	0.538	0.484	–0.054	0.023	0.014	–0.009
HS	0.736	0.646	–0.090	0.069	0.034	–0.035
Some college	0.784	0.745	–0.039	0.062	0.041	–0.021
College	0.803	0.808	0.005	0.056	0.067	0.011
Age 45–54						
Less than HS	0.494	0.505	0.011	0.019	0.014	–0.005
HS	0.716	0.659	–0.057	0.057	0.040	–0.016
Some college	0.793	0.744	–0.048	0.046	0.043	–0.003
College	0.844	0.814	–0.031	0.047	0.060	0.013
Age 55–64						
Less than HS	0.312	0.349	0.037	0.021	0.016	–0.005
HS	0.491	0.518	0.027	0.045	0.049	0.005
Some college	0.566	0.589	0.024	0.025	0.047	0.022
College	0.633	0.684	0.051	0.021	0.052	0.031
Age 65+						
Less than HS	0.047	0.071	0.023	0.055	0.029	–0.026
HS	0.091	0.121	0.030	0.067	0.074	0.006
Some college	0.115	0.168	0.053	0.031	0.054	0.023
College	0.137	0.227	0.089	0.020	0.057	0.037
TOTAL	0.574	0.549	–0.025	1.000	1.000	0.000

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

and education. The reported numbers are based on monthly Current Population Survey (CPS) data for 1999 (the year at the end of the long 1990s expansion just before the dot-com recession of the early 2000s) and 2018 (nine years into the post–Great Recession economic recovery).[3] Although the employment rate for men ages twenty-five to fifty-four began to fall as early as 1970, the employment rate for women ages twenty-five and fifty-four rose through the 1990s, as did the overall employment rates for men ages sixteen and over and women sixteen and over. It is the declines in employment rates starting in the early 2000s that we seek to understand.

Over the period from 1999 to 2018, the overall annual employment-to-population ratio fell from 64.3 percent to 60.4 percent, a decline of 3.8 percentage points.[4] Employment rates fell for both sexes, though the decline was steeper for men (5.3 percentage points) than for women (2.5 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 a particular starting year or ending year.[5] Had we been conducting our examination a few years earlier, however, the cumulative decline requiring an explanation 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 prerecession 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 twenty-five-to-fifty-four-year-old age band dropped by about 2 percentage points between 1999 and 2018. Men ages twenty-five-to-thirty-four experienced a substantial decline (4.2 percentage points), whereas the employment rate for women in that age range was little changed. Among those ages thirty-five to forty-four and those ages forty-five to fifty-four, men and women experienced similar declines.

Among sixteen-to-twenty-four-year-olds, the overall employment rate fell by 8.5 percentage points between 1999 and 2018, from 59.0 percent in 1999 to 50.5 percent in 2018. The employment rate for young men fell by 10.4 percentage points and that for young women by 6.6 percentage points. The decline for teenagers and young adults enrolled in school (11.4 percentage points) has been much larger than the decline for those in the same age range who are not enrolled in school (2.1 percentage points).

In contrast to the declines within the prime-age and young groups between 1999 and 2018, there was an *increase* in the employment-to-population ratio of 5.4 percentage points for those ages fifty-five to sixty-four, from 57.7 percent to 63.1 percent. This was largely attributable to increasing employment among women; the corresponding employment rate for men changed much less. The overall employment rate among those ages sixty-five and older rose even more—from 11.9 percent to 18.9 percent, an increase of 7.0 percentage points—with similar increases recorded for both men and women.

Despite the rise in employment at older ages, those ages fifty-five to sixty-four and, especially, those sixty-five 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 ages fifty-five and older increased substantially

[3] The Current Population Survey microdata used to produce the numbers in table 1 and table 2 were downloaded from the IPUMS website. See Flood et al. (2018).

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

[5] The published Current Population Survey series used to produce figure 1 can be downloaded from the Bureau of Labor Statistics website using the agency's Series Report tool available at https://data.bls.gov/cgi-bin/srgate. See Bureau of Labor Statistics (1965–2018).

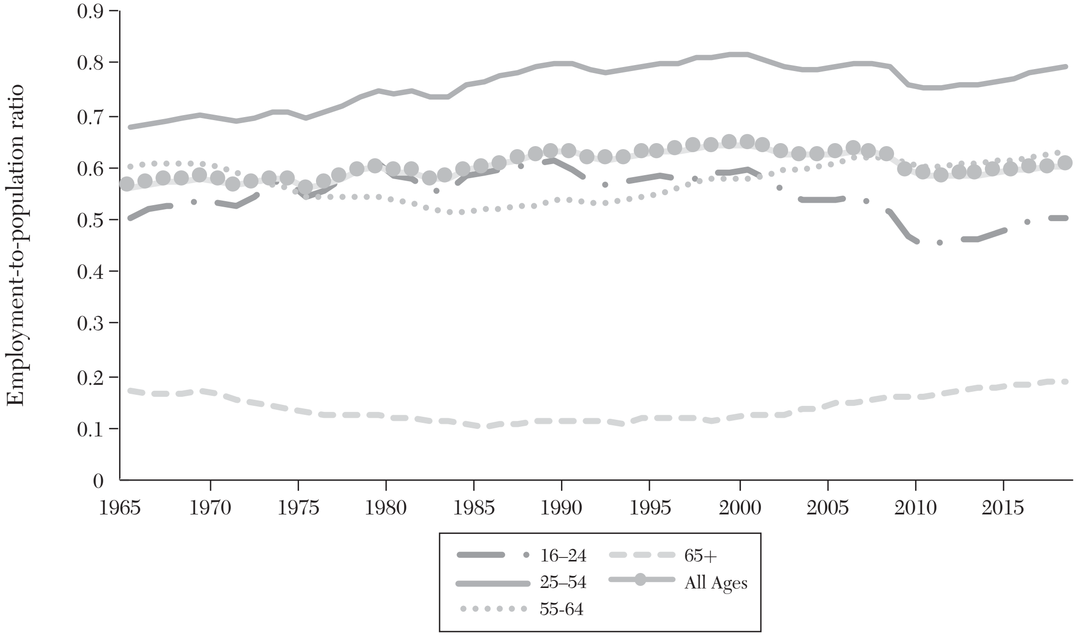

Figure 1. Employment-to-Population Ratio, by Age, 1965–2018

Source: Authors' calculations using published Bureau of Labor Statistics seasonally unadjusted employment and population level series. See Bureau of Labor Statistics (1965–2018).

between 1999 and 2018. Taken together, these facts imply that population aging has contributed to the reduction in the overall employment-to-population ratio.

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:

$$(2)\quad \Delta(E/P)_{t_0,t_1} = \sum_i s_{i,t_0}\Delta(E/P)_{i,t_0,t_1} + \sum_i \left[(E/P)_{i,t_0} - (E/P)_{t_0}\right]\Delta s_{i,t_0,t_1} + \sum_i \Delta s_{i,t_0,t_1}\Delta(E/P)_{i,t_0,t_1}.$$

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

TABLE 2A
Shares of Overall Employment-to-Population Ratio Changes Attributable to Within-Group Employment Changes and Changes in Population Composition, 1999–2018

	Overall	Male	Female
Contribution of $s_i \times \Delta E/P_i$			
16–24	41.1%	37.9%	46.2%
25–54	28.3%	27.9%	29.7%
55–64	–17.2%	–7.5%	–34.9%
65+	–24.3%	–15.6%	–38.6%
Contribution of $E/P_i \times \Delta s_i$			
16–24	–8.5%	–6.6%	–3.8%
25–54	22.0%	27.8%	52.9%
55–64	22.7%	8.1%	20.7%
65+	62.5%	45.8%	69.7%
Contribution of $\Delta E/P_i \times \Delta s_i$			
16–24	–5.1%	–4.7%	–5.8%
25–54	–4.0%	–2.8%	–5.9%
55–64	–9.4%	–4.4%	–18.5%
65+	–8.0%	–5.8%	–11.8%
Total	100.0%	100.0%	100.0%

Notes: 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.

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 2018 using data disaggregated into 26 age–sex groups for the overall column and 13 age groups for the male and female columns.[6] A common narrative regarding the recent decline in the employment-to-population ratio is that it has been driven by the aging of the population. 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 indeed have produced a decline in the overall employment-to-population ratio of roughly the magnitude actually observed.

Because the net change in the overall employment-to-population ratio reflects both negative and positive influences, however, this does *not* mean that factors other than population aging have been unimportant. In fact, as shown by the numbers in the first two rows of table 2A, the within-group

[6] 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.

employment rate declines among young and prime-age adults have had a sizable effect on the overall employment-to-population ratio. Had the distribution of the population across age–sex groups stayed the same as in 1999, within-group declines in employment rates among those in the sixteen-to-fifty-four-year age range between 1999 and 2018 would have caused the overall employment-to-population ratio to fall by 69.4 percent of the net observed overall decline (or about 2.7 percentage points).

Partially offsetting these large negative effects are two factors that worked to raise the overall employment rate between 1999 and 2018. First, increases in employment rates among those ages fifty-five and older raised the overall employment rate by 41.5 percent of the net overall decline (about 1.6 percentage points). Second, shifts in population away from groups with falling employment rates and toward groups with rising employment rates—the interaction effects captured by the third set of terms in equation (2)—raised the overall employment rate by 26.5 percent of the net overall decline (about 1.0 percentage point). The effects of rising employment rates among those ages fifty-five and older are shown in the third and fourth rows of table 2A; the interaction effects are shown in the table's bottom panel.

The text table below summarizes all of these various effects on the overall employment-to-population ratio as captured by the table 2A estimates. Population aging and falling within-group employment rates among those ages sixteen to fifty-four together are responsible for a 6.5 percentage point decline in the employment rate. Rising employment rates among adults ages fifty-five and older, together with the interaction effects attributable to population share increases among groups whose employment rates have been rising, have partially offset this decline.

	Percentage point effect
Changes in population shares	3.8 pp decline
Employment declines among those age 16–54	2.7 pp decline
Employment increases among those age 55 plus	1.6 pp increase
Interaction terms	1.0 pp increase
Total	3.8 pp decline

Table 2B reports the results of a decomposition similar to that displayed in table 2A, but for groups that are further disaggregated by educational status in addition to age and (if applicable) sex. Absent other changes, the declines we observe in employment among in-school sixteen-to-twenty-four-year-olds would have produced a decline in the overall employment rate equal to 25.8 percent of the observed net decline (1.0 percentage point); declines in employment among out-of-school sixteen-to-twenty-four-year-olds have played a smaller role. Declines in employment among twenty-five-to-fifty-four-year-olds who are high school graduates or have some college together would have produced a decline in the overall employment rate equal to 40.5 percent of the observed net decline (1.5 percentage points); declines among high school dropouts and college graduates in this age group have been less important. In other words, changes in employment rates within just three groups—in-school sixteent-to-twenty-four-year-olds plus those ages twenty-five to fifty-four 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 (about 2.5 percentage points). Similar 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 ages fifty-five

TABLE 2B
Shares of Overall Employment-to-Population Ratio Changes Attributable to Within-Group Employment Changes and Changes in Population Composition, 1999–2018

	Overall	Male	Female
Contribution of $s_i \times \Delta E/P_i$			
Age 16–24			
Not in school	7.5%	11.0%	1.1%
In school	25.8%	21.3%	34.0%
Age 25–54			
Less than HS	1.7%	0.9%	7.1%
HS	25.3%	18.6%	51.4%
Some college	15.2%	11.0%	24.2%
College	7.3%	5.2%	4.6%
Age 55–64			
Less than HS	−2.1%	−1.2%	−2.8%
HS	−2.9%	−0.4%	−5.0%
Some college	−1.4%	−0.4%	−3.7%
College	−2.8%	−2.3%	−5.5%
Age 65+			
Less than HS	−3.5%	−2.8%	−4.5%
HS	−5.2%	−2.5%	−9.4%
Some college	−3.2%	−2.1%	−5.0%
College	−3.1%	−2.6%	−5.3%
Contribution of $E/P_i \times \Delta s_i$			
Age 16–24			
Not in school	−1.3%	0.2%	4.7%
In school	−2.0%	−2.7%	−2.4%
Age 25–54			
Less than HS	−4.1%	2.8%	−6.5%
HS	12.6%	13.3%	39.4%
Some college	9.1%	9.1%	23.7%
College	−10.1%	−2.2%	−39.9%
Age 55–64			
Less than HS	−3.9%	−1.7%	−5.6%
HS	4.6%	2.7%	1.8%
Some college	7.3%	2.5%	4.9%
College	3.1%	−0.3%	−2.7%
Age 65+			
Less than HS	−39.4%	−22.9%	−55.1%
HS	14.0%	12.0%	13.3%
Some college	30.3%	19.0%	38.8%
College	46.7%	30.4%	59.6%

(Continued)

TABLE 2B
Shares of Overall Employment-to-Population Ratio Changes Attributable to Within-Group Employment Changes and Changes in Population Composition, 1999–2018 (*Continued*)

	Overall	Male	Female
Contribution of $\Delta E/P_i \times \Delta s_i$			
Age 16–24			
Not in school	−2.0%	−2.3%	−1.0%
In school	−0.5%	−1.0%	−0.2%
Age 25–54			
Less than HS	−1.0%	−0.6%	−3.1%
HS	−8.3%	−4.2%	−20.8%
Some college	−2.5%	−1.5%	−4.6%
College	1.0%	0.3%	1.3%
Age 55–64			
Less than HS	0.5%	0.3%	0.8%
HS	−0.8%	−0.3%	−0.6%
Some college	−1.8%	−0.5%	−4.8%
College	−3.6%	−2.1%	−9.5%
Age 65+			
Less than HS	1.6%	1.3%	2.0%
HS	−0.7%	−0.7%	−0.5%
Some college	−2.6%	−1.8%	−4.0%
College	−4.9%	−3.1%	−10.6%
Total	100.0%	100.0%	100.0%

Notes: 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.

and older boost overall employment in the table 2B decompositions, but the effect is 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 ages fifty-five and older shown in table 2A can be tied to rising education levels at these older ages. 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 toward cells in which the employment-to-population ratio has risen.

We again have summarized all of these various effects on the overall employment to population ratio, this time as captured by the table 2B estimates, in a text table (see below). Changes in the composition of the population and falling within-group employment rates among those ages sixteen to fifty-four together are responsible for a 5.7 percentage point decline in the employment rate. As before, this has been partially offset by rising employment rates among those ages fifty-five and older together with the interaction effects attributable to population share increases among groups whose employment rates have been rising.

	Percentage point effect
Changes in population shares	2.6 pp decline
Employment declines among those ages 16–54	3.2 pp decline
16–24-year-olds in school	1.0 pp decline
25–54-year-olds with high school or some college	1.5 pp decline
Employment increases among those age 55 plus	0.9 pp increase
Interaction terms	1.0 pp increase
Total	3.8 pp decline

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

1. In a decomposition by age and sex, decreases in within-age-group employment rates among those ages sixteen to fifty-four can account for 69.4 percent of the net overall decline in the employment-to-population ratio between 1999 and 2018, or approximately a 2.7 percentage point drop.

2. Declines in employment among school enrollees account for the majority of the contribution of those ages sixteen to twenty-four to the overall employment rate decline.

3. Declines in employment rates for those with a high school degree and some college account for the largest shares of the contribution of those ages twenty-five to fifty-four 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.

4. Increases in employment rates among those ages fifty-five 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. While our analysis will focus on the role of within-group employment rate changes, the changing composition of the population has been an important driver of the overall employment-to-population ratio. Accounting only for changes in the population's age–sex mix 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 goal for 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 2018 period. Although employment rates have been rising for those ages fifty-five and older, 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, leading to that growth being smaller than it otherwise might have been.

3. *Factors behind the Trends*

We turn next to a review of available evidence on the factors that might have contributed to falling employment rates. These declines could have been driven by shifts in labor demand, shifts in labor supply, or 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 import competition 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 from improvements in the options available to nonworkers, increases in the costs of participating in the labor force that deter some people from seeking employment, or changing attitudes toward work. Supply-side explanations for low or falling US employment rates that postulate increases in the attractiveness of the options available to nonworkers 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. 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. For young adult and prime-age men, changes in social norms such that not working has become more acceptable also could have played a role. 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. Much of the remainder 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.

An additional possible factor is that, in the years following the Great Recession, employment rates could have been affected by negative hysteresis. This possibility is explored by Yagan (2019). He estimates that an area exposed to a one percentage point larger unemployment shock in 2007–09 had an employment rate that was 0.3 percentage points lower six years later in 2015. This is an interesting finding, but it is unclear whether it reflects a sustained response to the initial unemployment shock (what some might term true hysteresis) or the persistence of whatever factor caused the initial decline in employment. Yagan grapples in his paper with trying to identify the mechanisms behind his results and concludes that persistently low local labor demand, combined with mobility frictions that keep local residents of hard-hit areas from moving to other areas, is a leading candidate. We note the possibility of hysteresis subsequent to the Great Recession as another contributor to falling employment, but absent good evidence that would allow us to assess its importance, do not have more to say on the subject.

For each of the potential explanatory factors we consider, our goal is to assess whether the available evidence supports a causal relationship between it and employment rates and, if so, whether the factor has changed over the 1999–2018 period in such a way as to have contributed to falling employment. Some of the same factors also could have been important for understanding the evolution of employment during earlier periods. Even for prime-age men, whose employment has been falling for decades, however, the factors that mattered from the 1970s through the 1990s could differ from the factors that have mattered subsequently. We have not attempted an in-depth exploration of the drivers of employment trends in earlier eras, though doing so might be a useful extension of the present analysis. Instead, we have focused on identifying the factors that might explain the fall in employment rates since 1999 documented in the previous section of the paper.

3.1 *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. The outstanding question is what might have caused these adverse shifts in labor demand, especially for less-educated workers.

Two labor demand factors that have received extensive attention in the literature are import competition 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.

3.1.1 *Increased Import Competition from China*

One of the major economic questions of recent years has been the extent to which the increase in imported goods from China has negatively affected American workers, specifically, those working in the manufacturing sector. US manufacturing employment declined from about 17.3 million in 1999 to about 12.7 million in 2018, a loss of about 4.6 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 effects of 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 over the period 2007 to 2011 is attributable to losses in manufacturing. Charles, Hurst, and Schwartz (2019) point out that the spatial concentration of manufacturing activity is one reason why shocks to manufacturing might have especially large aggregate labor market effects.

A number of recent papers have linked the decline in manufacturing sector employment to increased import competition from China. Growth in Chinese imports led to a reduction in demand for domestic manufacturing workers who might have otherwise produced these goods. Given the large representation of less-educated prime-age men in the US manufacturing sector, some of this research was motivated by an interest in understanding the decline in the wages of less-educated men. For the purposes of this review, we focus primarily on the employment effects documented in the literature.

In an analysis that looks at the period from 1990 through 2007, Autor, Dorn, and Hanson (2013) find 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 US 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 US 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 US manufacturing employment after 2000 to the change in US 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 US 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 normal trade relations (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 2001, so exposure to the policy change is not about a change in tariff rates per se, but rather about a reduction in the threat of higher tariffs.[7]

[7] 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 US manufacturing employment. In brief, they argue, the removal of this uncertainty did three things: (1) it increased the incentive for US 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 US market; and (3) it increased the attractiveness of investments in capital- or skill-intensive technologies at home that are more consistent with the US comparative advantage. Using US 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 US importers. Using US microdata, they confirm that PNTR is associated with a relative increase in the number of pairs of US 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 US data, the authors document that the associated decline in US manufacturing is heightened by input–output linkages and shifts toward less labor-intensive production (per mechanism (3)).

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 US 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, however, there may be broader employment effects that could either amplify or offset the direct effects. Contraction of US 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). It also could affect the industries that purchase manufactured goods (downstream effects). The upstream effects on suppliers to US manufacturing are unambiguously negative, but the downstream impact on manufacturing customers will depend on how those firms interact with the imports from China. Work by Acemoglu et al. (2016) described below makes an attempt to measure broader effects, but it is harder to identify the causal impact of increased imports from China on aggregate US employment, as opposed to employment in the specific industries or localities that are directly affected and, accordingly, we view the aggregate employment estimates more cautiously.

Building on some of the research described above, Acemoglu et al. (2016) quantify how much of the reduction in manufacturing employment between 1999 and 2011 is attributable to rising import competition from China and also attempt to 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 behind reductions in US manufacturing employment and that, after accounting for input–output linkages, it also had a negative effect on overall job growth. As already indicated, this latter conclusion is necessarily more tentative than the finding of a causal reduction in manufacturing employment.

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 instrumental variables (IV) estimation strategy, instrumenting for industry exposure with industry exposure to Chinese import competition in eight other high-income countries.[8] Their results imply that greater Chinese import penetration accounts for approximately 10 percent of the decline in US manufacturing employment after 1999. The authors then consider employment losses associated with a contraction of US manufacturing through both upstream and downstream industry effects. Using data from the 1992 US 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 provides a general equilibrium treatment of potential employment 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

[8] The suitability of this instrument requires that country-specific import demand shocks are uncorrelated across high-income economies and that US imports from China do not lead to higher levels of exports from China to other countries, such as through an economy of scale effect.

(CZ) level, they estimate no discernible effect of import exposure in a CZ on employment in nonexposed 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.[9]

New work underway by Bloom et al. (2019) builds on this literature with an investigation of firm and job dynamics in response to the China shock, looking at variation across sectors and regions. This new work offers a more nuanced view of the overall economic impacts of the China shock on US economic activity. These authors use administrative microdata on US establishments from the Census Bureau's Longitudinal Business Database (LBD). They apply the same estimation strategy as Autor, Dorn, and Hanson (2013), exploiting regional variation in exposure to Chinese import competition as instrumented for by Chinese exports to other countries. They find that increased imports from China had heterogeneous effects across regions and firms. Manufacturing firms in high human-capital areas, such as the US coastal areas, restructured their domestic activity toward service work, including to research and marketing functions. This led to a decrease in manufacturing jobs and an increase in nonmanufacturing jobs in these areas. Manufacturing workers were harmed, but manufacturing firms in these areas were generally fine. In contrast, manufacturing firms in lower human-capital areas, such as the Midwest, experienced a loss of manufacturing jobs but did not add jobs in other sectors. This harmed manufacturing workers and surviving manufacturing firms in these areas. The earlier papers described above estimate an average employment effect of the China shock and thus do not reveal these interesting patterns in firm dynamics.

The effects reported by Bloom et al. (2019) give more nuance to the economic dynamics, but do not contradict the conclusions of earlier work on the employment consequences of growing imports from China, driven by a reduction in employment and wages for lower-wage workers. Specifically, Bloom et al. show that the decrease in manufacturing jobs in low human-capital areas was not offset by an increase in nonmanufacturing jobs, so that there was a net effect on employment. Bloom et al. (2019) do find, however, that the impacts of imports from China have weakened over time. They find sizable net negative employment effects from 2000 to 2007, but consistent with firm and labor market adaptation, no net effects over the period from 2007 to 2015. For this reason, in trying to calculate the overall effect of increased Chinese imports on aggregate employment, it seems most appropriate to use the effects estimated by Acemoglu et al. (2016) but not to extrapolate those effects upwards in line with the subsequent growth in the volume of Chinese imports.

In addition to the already-noted difficulties of translating a local-area estimate of job losses due to growth in Chinese imports into an aggregate estimate, it also is important to remember that the employment effects of interest have been generated within a specific context and in interaction

[9] 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 (2016) local-area-based empirical analysis.

with other features of the existing economic landscape. If the China shock had occurred against a backdrop of more robust growth in another sector that employed workers similar to those displaced from manufacturing, for example, the resulting job losses likely would have been smaller. Charles, Hurst, and Schwartz (2019) report some evidence that the decline in manufacturing demand has been associated with increased take-up of disability benefits. They also report that, compared to earlier periods, workers in recent decades are less likely to move across regions in response to a local manufacturing shock. These findings suggest that if disability insurance were harder to access and/or workers had been more mobile, the impact of demand shocks to manufacturing employment, such as the China import shock, might have translated into less of a reduction in aggregate employment.

With all of these cautions in mind, we attempt to quantify the contribution of increased import competition from China to the decline in employment over the 1999 to 2018 period. The Acemoglu et al. (2016) analysis covered the period from 1999 through 2011. If the growth in the volume of imports from China since 2011 had affected employment in the same way as the earlier growth, extrapolation of the Acemoglu et al. (2016) estimates would imply that more than 3.10 million workers had been displaced by 2018. Given the Bloom et al. (2019) findings that the negative employment response was limited to an earlier period, however, this extrapolation does not seem appropriate. We thus take the 2.37 million jobs number reported by Acemoglu et al. (2016) and use that as our estimate of jobs lost due to increased imports from China. Adding these jobs to the 2018 employment count would raise the employment-to-population ratio by 0.92 percentage points. As with all of our estimates of how different factors have affected the employment to population ratio, these numbers should be interpreted as a rough gauge, not a precise or definitive count.

3.1.2 *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 CZ level as a function of CZ exposure to trade competition from China (measured and instrumented for in the same way as in Autor, Dorn, and Hanson 2013) 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 Autor, Dorn, and Hanson (2015) findings 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 nonemployment. Furthermore, the associated employment losses are much larger for noncollege-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 points among college-educated workers and by 1.21 percentage points among noncollege 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 twenty-fifth to the seventy-fifth percentile of exposure to routine tasks, 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 US 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 to replace jobs. For example, a widely-cited paper by Frey and Osborne (2013) estimates that over the coming decades, 47 percent of US workers are at risk of having their jobs automated. Brynjolfsson and McAfee (2011) argue that these sorts of numbers wildly overstate the likely actual impact of automation on employment. They believe that automation will have important effects on the kinds of work that people do in the future, but find it implausible that the long-run effects of automation will be to leave a large fraction of the population without work. Taking a more empirical perspective, Acemoglu and Restrepo (2017) point to two specific factors that will affect the equilibrium impact of automation on aggregate employment rates. First, the relative costs of automation versus labor will determine the extent to which firms choose to automate. Second, the equilibrium labor market impacts of automation will depend on adjustments in other sectors. Their empirical analysis moves beyond the existing research to provide an estimate of the net effect of industrial robots on US 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 the local labor market. Local labor market exposure to robots is measured for this purpose 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 the 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 Robotics (IFR), which provides counts of the stock of robots by industry, country, and year for 50 countries starting in 1993. The data show that, between 1993 and 2007, the stock of robots in the United States 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 US 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 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 IV 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, exposure to 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 CZ by 0.37 percentage points, 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 points, or 5.6 workers per new robot. The authors caution that this is a rough gauge, and note that under more conservative assumptions, the reduction in employment could be as low as 0.18 percentage points.

Based on the data used by Acemoglu and Restrepo (2017), in 1999, there were 79,959 robots installed in the United States. By 2018, the estimated stock had grown to 279,683 robots, an increase of 199,724 robots.[10] Acemoglu and Restrepo's preferred estimate is that each robot displaces about 5.6 workers. We use this number to generate an approximate estimate of the decline in employment attributable to the growing penetration of industrial robots between 1999 and 2018. In so doing, we emphasize the tentative nature of the estimate, owing among other things to the difficulty of identifying causal impacts on aggregate employment (as opposed to highly localized employment) and to the fact that the data on robot adoption are relatively crude. With the appropriate caveats in mind, our estimate is that robot adoption between 1999 and 2018 reduced employment by about 1.1 million jobs. Adding this estimated count of robot-displaced workers to the 2018 workforce would raise the employment-to-population ratio by 0.43 percentage points.

3.2 *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 from improvements in the options available to nonworkers, increases in the costs of entering the labor force that lead fewer people to seek employment, or changes in preferences. One potential explanation involving improvements to the options afforded to nonworkers 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 Participation (SIPP) indicating that in 2013, 63.0 percent of households with nonworking prime-age men received means-tested assistance from programs including Medicaid, Temporary Assistance for Needy Families (TANF), SNAP, or the Women, Infants and Children 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.

Lack of support for working parents is another potentially important supply-side influence on employment rates. While insufficient support 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 in order for this to explain falling employment rates. Other explanations focus on changes in the attractiveness of leisure activities or changes in social norms that may have affected preferences for work, together with the possibility that increasing rates of opioid addiction have made substantial numbers of people less able to work. 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 seems inconsistent with this as an explanation for the overall decline in employment rates.

[10] The authors thank Pascual Restrepo for sharing the robot data used in Acemoglu and Restrepo (2017) with us. We updated their data series (which runs through 2014) to 2018 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).

3.2.1 *Federal Disability Insurance Programs*

Disability insurance benefits provide an alternative source of income for some qualifying individuals who might have a high disutility of work (or an especially high utility of leisure) and are on the margin of working or collecting a disability insurance benefit. 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 of the role that SSDI has played in driving down employment rates.[11] In fact, the SSDI caseload peaked in 2014; SSDI applications have been steadily declining since 2011, which observers attribute at least in part to the continuing cyclical recovery. Two other large federal disability insurance programs, the federal Supplemental Security Income (SSI) Program and the Veterans Affairs disability compensation (VADC) program, also have grown during the long time period we consider.

Eberstadt (2016) emphasizes the increased reliance on disability payments from these programs among working-age men (the focus of his book) in recent decades. He reports tabulations from the SIPP showing that in 2013, 6.3 percent of men ages twenty-five to fifty-four reported receiving any disability benefits, as compared to 4.2 percent in 1985. Among men ages twenty-five to fifty-four 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.[12] Given that there is a tendency for household survey respondents to underreport participation in welfare and social insurance programs, all of these numbers may be underestimates.

The SSDI program is administered by the US 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 ten 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 2007–09 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 2018 for every five-year age category from ages forty to forty-four through age fifty-five to fifty-nine, as well as for those between ages sixty and the applicable full retirement age. For

[11] 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.

[12] Krueger (2017) reports that among 571 not-in-the-labor-force men ages twenty-five to fifty-four who participated in an online survey, 50.5 percent reported receiving some type of disability payment.

example, the share of individuals ages forty to forty-four on the SSDI program increased from 2.5 percent to 2.6 percent over the 1999 to 2018 period; the share for those ages fifty to fifty-four increased from 5.1 percent to 5.9 percent; and the share for those from ages sixty to the full retirement age increased from 8.9 percent to 11.7 percent.

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 ordinary least squares (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.[13] 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 SSDI 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.[14] Another recent paper

[13] 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.

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

using an entirely different approach finds dis-employment effects of SSDI benefits of a very similar magnitude. Gelber, Moore, and Strand (2017) exploit discontinuous changes in the SSDI benefit formula and a regression kink design to estimate the effect of payment size on earnings among beneficiaries. Using administrative data on all new SSDI beneficiaries from 2001 to 2007, they find that an increase in SSDI payments of $100 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.

In terms of an extensive margin response, the Gelber, Moore, and Strand (2017) analysis implies that $1,000 in SSDI benefits corresponds 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.[15]

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.[16] These data indicate that the SSDI caseload grew by 3.66 million recipients between 1999 and 2018, from 4.88 million to 8.54 million recipients, with most of the growth in beneficiary counts occurring at older ages. We would like to know how much of the growth in caseload has occurred as a result of increasing within-age-group receipt rates, rather than as a result simply of population growth and aging. To that end, within each age bin for which published data are available, we compare the actual caseload change to the hypothetical change that would have occurred had the SSDI receipt rate in that age bin remained constant.[17] Summing over age groups, we estimate that there were 1.32 million more people on SSDI at the end of 2018 than we would have expected had age-group-specific receipt rates not changed; this represents about 15 percent of the caseload.

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 255,818 more workers.[18] Adding those workers to the

[15] Gelber, Moore, and Strand (2017) 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). The latter 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.

[16] The SSA caseload data were downloaded from https://www.ssa.gov/policy/docs/statcomps/supplement/2019/ (accessed on June 15, 2019).

[17] The age bins are 18–29, 30–39, 40–44, 45–49, 50–54, 55–59, and 60 to the full retirement age. Receipt rates among those 18–29 and 30–39 are very low in both 1999 and 2018, but fell slightly over that period, meaning that the excess caseload for these age groups is actually negative.

[18] The Council of Economic Advisers (2016) reports on a similar back-of-the-envelope calculation of how much of the reduction in the labor force participation rate among prime-age men between 1967 and 2014 can be explained

2018 workforce would increase the employment-to-population ratio by 0.09 percentage points.[19]

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.[20] 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 3.69 million in 1999, representing about 2.1 percent of the non-elderly adult population, to 4.71 million in 2018, representing 2.3 percent of the non-elderly adult population, down slightly from a few years earlier.[21] Increases in the number of SSI recipients have been attributed to both demographic and policy factors (Duggan, Kearney, and Rennane 2016). We are aware of no direct evidence that allows us to quantify the extent to which the modest increase in program participation between 1999 and 2018 has pulled people who otherwise would have been working out of the workforce. As the program is structured, SSI recipients are people who did not have a sufficient attachment to the labor market to qualify for SSDI or, if they did qualify, had very low earnings and thus very low SSDI benefits. This leads us to believe that any such employment effects cannot be large, but growth in SSI participation could perhaps have contributed some very small amount to the decline in employment among the non-elderly. We note that some SSI participants (the so-called "dual eligibles") also participate in SSDI, so the estimated effect of SSDI on employment might include some combined effect with SSI.

A third federal disability insurance program is the disability compensation program administered by the US Department of Veterans Affairs (VADC). This program pays benefits to individuals with medical conditions resulting from US military service. In contrast to SSDI benefits, VADC benefits are based solely on a determination of the severity of the impairment a veteran has suffered. Benefits are paid for life and generally are not subject to being reduced if a veteran is working. After 2001, the VADC program 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 1995–99 and 2010–14, the relative labor force participation rate of veterans (as compared to demographically similar nonveterans) fell by five percentage points. They note that over this time, the share of veterans receiving VADC grew by 9 percentage points, from 9 to 18 percent, and average real benefits grew substantially. Assuming that the increase in VADC participation and benefit amounts is entirely responsible for the decline in relative labor force participation, they tentatively estimate that 55 percent of new VADC 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 VADC benefit recipients. These authors

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 had they been denied. The authors apply this estimate to nonworking prime-age male SSDI beneficiaries and find that adding these men to the 2014 workforce would have reduced the 7.5 percentage point decline in labor force participation among prime-age men between 1967 and 2014 by about 0.5 percentage points.

[19] This estimate is consistent with the conclusion of Charles, Hurst, and Schwartz (2019) that increased enrollment in SSDI/SSI could explain at most 15 to 20 percent of the decline in employment during the 2000s.

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

[21] Data on the number of SSI recipients by age group are available at https://www.ssa.gov/policy/docs/statcomps/supplement/2019/index.html (accessed on June 15, 2019).

exploit the 2001 Agent Orange policy change that expanded VADC 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 VADC benefits subsequently dropped out of the labor force.[22] Because the disability compensation benefit amount is not dependent on work status but only on service-related health conditions, 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 2018 had VADC benefit receipt not increased. The Department of Veterans Affairs reports 4.7 million VADC benefit recipients in 2018, as compared to around 2.3 million in 1999.[23] 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 2018 caseload by applying 1999 age-category specific program population shares to the 2018 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 ages thirty-five to fifty-four (since this overlaps with the ages of their analysis sample). We expect the elasticity of work to program participation to be 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 (ages thirty-four and under and ages fifty-five to seventy-four) and zero for those seventy-five and older. This leads us to estimate a loss of 182,619 workers over this period, which is an admittedly very rough calculation, but nonetheless useful as a ballpark estimate. Adding these workers to the 2018 employed population would raise the employment-to-population ratio by 0.07 percentage points.

Our summary read of the evidence is that the rise in participation in disability insurance programs has made a notable, albeit secondary, contribution to the decline in employment over this period. The existing literature has produced credible causal estimates of the effect of the SSDI and VADC program on labor supply. We use those estimates to gauge how much higher the employment-to-population ratio would have been in 2018 without the growth in these programs, coming up with a combined estimate of 0.17 percentage points. This does not account for any independent effect of growth in participation in the SSI program, but because the growth in the number of people receiving SSI has been modest and SSI-only 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.

3.2.2 *Supplemental Nutrition Assistance Program*

SNAP, formerly the "food stamp" program, provides vouchers for food purchases to eligible individuals and families. In 2018, the program provided benefits to 39.7 million Americans (adults and children), at a cost of $64.9 billion. Unlike most US

[22] 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 fifty-five to sixty-four who were granted access to Veterans Affairs health insurance in the mid-1990s.

[23] The 2018 Benefits Report from the Department of Veterans Affairs is available here: https://www.benefits.va.gov/REPORTS/abr/docs/2018-abr.pdf, accessed on July 29, 2020. The 1999 Benefits Report is available here: https://www.benefits.va.gov/REPORTS/abr/fy99-abr.pdf, accessed on June 16, 2019.

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 more limited access to benefits. The vouchers can be used to purchase most foods at grocery stores or other authorized retailers. Average monthly benefits in 2018 amounted to $253 per household or $126 per person per month, which translates to benefits worth about $1.40 per meal.[24]

Given the relatively low level of income support the program provides, it seems unlikely that the provision of these food vouchers has caused a substantial number of people to choose nonwork over work. That said, it is worth considering whether the existence of the program might raise the reservation wage, and hence reduce the labor supply, of potential workers. We begin by discussing the labor supply incentives inherent in this transfer program. We then consider whether there were notable expansions in the generosity of the program during the 1999 to 2018 period that might have contributed to declining employment rates and highlight the most rigorous available 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 available. The standard SNAP eligibility rules specify 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). States also may designate households eligible for certain other means-tested programs as automatically eligible for SNAP benefits. Households' eligibility for benefits must be recertified every 6 to 24 months. The benefit amount is highest for households with zero income and falls 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 than the disincentives in those other programs.

Most able-bodied adults, whether or not they have children, are subject to an additional requirement that they must be working or looking for work in order to qualify for SNAP benefits. The work requirements for prime-age (eighteen to forty-nine years old) able-bodied adults without dependents, referred to by the US government as ABAWDs, are especially restrictive. Except during periods of high unemployment, 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.

The number of SNAP program beneficiaries expanded considerably during the Great Recession and, as of 2018, had not yet fallen back to prerecession levels. The reasons for this are not entirely clear. The American Recovery and Reinvestment Act of 2009 (ARRA) suspended the normal three-month time limit imposed on nonworking ABAWDs through September 2010. By statute, during periods of high unemployment, states may request a waiver from the ABAWD rule. From 2011 through 2014, more than 40 states had statewide waivers of this rule in place.

[24] Center on Budget and Policy Priorities (2019) provides a description of the SNAP program; program participation statistics are available from the Food and Nutrition Service at https://fns-prod.azureedge.net/sites/default/files/resource-files/SNAPsummary-7.pdf, accessed on July 29, 2020.

As economic conditions have improved, all but a handful of these statewide waivers have lapsed, though some states have retained waivers limited to economically depressed areas within their jurisdictions. The increase in the number of ABAWDs on the rolls, however, can account for only a portion of the increase in the number of beneficiaries.[25] The 2009 ARRA legislation increased the monthly SNAP benefit amount by 13.6 percent, but that provision expired in 2013.

The only research we know of that studies the relationship between SNAP expansions in recent years and labor supply is a paper by East (2018) that focuses on relaxed restrictions to program eligibility for immigrants in the post–welfare reform era. Her analysis suggests that single immigrant women reduce their employment when they gain SNAP eligibility and married immigrant 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 do find a statistically significant reduction in hours and, in some specifications, also employment among female heads of household.

Did the SNAP expansions during and following the Great Recession lead to lower rates of employment or longer spells of unemployment? 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 generally the main determinants of caseloads (see review by Hoynes and Schanzenbach 2016). That said, the increasing numbers of people receiving SNAP benefits during and following the Great Recession could have contributed, at least to some extent, to employment not recovering as rapidly as it otherwise might have. Although the literature does not suggest that the program has sizable dis-employment effects and it seems implausible that the SNAP program was an important driver of the exits from employment observed before or during the recession, the expanded availability of SNAP benefits could have allowed some individuals to remain out of work longer than might otherwise have been the case.

3.2.3 *Expanded Access to Publicly Provided or Subsidized Health Insurance*

The 2010 Affordable Care Act (ACA) expanded access to health insurance in a number of different ways. Effective in the fall of 2010, employer-provided health plans offering dependent coverage were required to extend that coverage to employees' young adult children through age twenty-six. Beginning in 2014, in states that chose to participate, Medicaid coverage was extended to include low-income childless individuals. At the same time, income-based subsidies for individuals to purchase health insurance began to be offered on newly created exchanges. Starting in 2015, employers with more than 100 employees have been required to offer health insurance to their full-time employees or pay a fine; in 2016, that requirement was extended to employers with 50–99 employees. The timing of these changes is such that the ACA cannot account for the longer-term secular decline in employment rates. Still, it is worth considering what is known about the possible effects of these changes on employment, and especially about the relationship between access to public health insurance and labor supply, in order to gauge whether the ACA might have contributed to a slower recovery of employment rates following the Great Recession than otherwise would have been observed. Looking to the future, we also

[25] The most recent currently available data on the characteristics of benefit recipients can be found at https://fns-prod.azureedge.net/sites/default/files/resource-files/Characteristics2017.pdf, accessed on June 16, 2019.

would like to know how the maintenance (or reversal) of the ACA provisions might affect employment rates going forward.

There are multiple channels through which the ACA could have lowered employment. First, the fact that many 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 individuals who are eligible for subsidies, the phaseout of those subsidies as income increases should make additional work hours less attractive. Third, by raising the consumption level associated with nonwork, the expansion of Medicaid to childless individuals should lead to lower levels of employment. Fourth, the increase in labor costs associated with the employer penalty for not offering employer-provided health insurance could have negatively affected employers' demand for workers, though the extent to which this is so will depend on whether and to what extent increases in health insurance costs can be offset by wage reductions.[26]

Research on earlier expansions in access to public health insurance suggests that the labor supply effects of such expansions may vary depending on the specific context within which they occur.[27] Garthwaite, Gross, and Notowidigdo (2014) 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 estimate 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 allow them to reject employment declines of more than 4.4 percentage points.

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 (2014) study is that Tennessee's program covered relatively higher income individuals, a group that is more likely to be able to find jobs with health insurance benefits. The lower estimated effects for Wisconsin and Oregon may be due to 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.

In a paper that analyzes the effects of the 2006 Massachusetts health care reform, Kolstad and Kowalski (2016) provide evidence relevant to assessing the potential labor market impact of the ACA's employer mandate. They find that implementation of the employer mandate in Massachusetts led

[26] In a 2014 report based on a simulation model, the Congressional Budget Office predicted that, on net, the various provisions of the ACA would reduce the total number of hours worked by about 1.5 percent to 2.0 percent during the period 2017 to 2024, driven almost entirely by a reduction in labor supply (Congressional Budget Office 2014a).

[27] There is 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).

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.[28]

Several recent studies have looked directly at the employment effects of various ACA provisions. Heim, Lurie, and Simon (2018) use a data set of US tax records spanning 2008–13 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. Leung and Mas (2018) 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. A recent paper by Duggan, Goda, and Jackson (2019) exploits 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 employment effects of these ACA provisions were close to zero.

As already stated, implementation of the ACA is sufficiently recent that it cannot explain the fall in employment rates that has been underway since 1999. Our summary read is that, although there were reasons to fear that implementation of the ACA in recent years could have had a negative effect on employment rates, there is little evidence of such effects in practice.

3.2.4 *Earned Income Tax Credit*

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 25 million tax filing units received the EITC in 2018, with the value of claimed credits totaling $63 billion. The EITC was introduced into the federal income tax code in 1975 and became permanent in 1978. Widely viewed as an effective means of incentivizing labor force participation and reducing poverty, the program has been expanded several times since 1990, most dramatically in 1993 and 1996 and also again in 2001 and 2009.[29] As described 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 eighteen. For a nonworker whose household is in the part of the EITC schedule along which additional earnings raise the amount of the credit received, the EITC creates an unambiguous incentive to enter employment. For someone whose

[28] Dillender, Heinrich, and Houseman (2016a, 2016b) also provide evidence relevant to the effects of the employer mandate, specifically the effect that it may have had on the prevalence of part-time work. Dillender, Heinrich, and Houseman (2016a) examine the effects of the Massachusetts health care reform, finding that it increased the prevalence of part-time work among lower-wage workers. Dillender, Heinrich, and Houseman (2016b) compare the post-ACA experience of other states to that of Hawaii, which has had a more stringent employer mandate for decades, and provides evidence that the employer mandate led to an increase in involuntary part-time employment. These papers do not address the effects of employer mandates on overall employment or hours.

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

household is already receiving the maximum credit or is in the range where the credit is being phased out, however, the EITC may make going to work less attractive. Because the changes to the EITC since 1999 should have worked to encourage labor force participation rather than the reverse, however, we do not believe the EITC is a candidate to explain falling employment rates.

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 in 2018 was $3,461 for eligible tax filers with one child; $5,716 for those with two children; and $6,431 for those with three or more qualifying children. The income cutoffs at which the EITC falls to zero for single filers were $40,320 for one-child families; $45,802 for two-child families; and $49,194 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 with an employed partner. In 2018, the income cutoffs were about $5,700 higher for married filers with children than for single filers with the same number of children. In 2018, the maximum credit available to childless single filers was just $519 and no credit was available for those in this group with $15,270 or more 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 US tax code treats married couples as a single tax unit, and the EITC credit phases out as combined household earnings increase. For EITC households already receiving the maximum credit or in the phase-out range, the EITC can be expected to make it less attractive for a nonworking spouse to enter the labor market. Eissa and Hoynes (2004) find that EITC expansions between 1984 and 1996 reduced married women's labor force participation by more than a full percentage point. 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. Absent other changes this might have been expected to increase spousal labor supply relative to the earlier period.

If one were to net out potential EITC-induced increases in employment over the 1999 to 2018 period—owing to the 2001 and 2009 changes in the program—then the overall decline in employment to be explained might be slightly larger than the net decline 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 been a significant driver of overall employment rates, and certainly not of employment rate declines, during this period.

3.2.5 *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 childcare 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, childcare, preschool, and elder care. The childcare costs borne by American families can be significant. Ziliak (2014) reports that, as of 2012, the costs of full-day center-based childcare represented from a quarter to a third of the average annual earnings of single mothers of young children, depending on the state, though non-center can be significantly less expensive. In order for a lack of support for working parents to help with explaining observed declines in US employment rates, however, something must have changed—either family policy must have become less accommodating or the difficulties faced by working parents must have grown.

Standard labor supply models imply that higher childcare costs should be associated with lower parental labor force participation rates. For a single parent or a married parent whose spouse is already employed, the cost of childcare 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 childcare costs to be associated with lower employment rates for women with children. Anderson and Levine (1999) report that the employment decisions of lower-skill workers are especially sensitive to childcare costs.

An important limitation of the early studies was the lack of an exogenous source of variation in childcare costs. Some more recent research has used the introduction of universal kindergarten and, later, prekindergarten 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; it is less clear that this is the case for public prekindergarten programs.

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 from birth to age five 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, Gruber, and Milligan 2008; Lefebvre and Merrigan 2008; Haeck, Lefebvre, and Merrigan 2015). An important feature of the Quebec reform was its universal 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 three-year-olds in Spain. In contrast, policy reforms in Norway (Havnes and Mogstad 2011) and Sweden (Lundin, Mörk, and Öckert 2008) that lowered the cost of childcare in a context where there was already a significant amount of publicly provided care had very limited incremental effects on mothers' employment.

All of the preceding relate to mothers' labor supply, but with falling fertility, the share of women who are mothers has been shrinking. In 1999, about 18.6 percent of women ages sixteen to fifty-four lived with an own child under the age of five; by 2018, that share had fallen to about 16.6 percent. Whatever the cause, the fact that fewer women have young children at home could perhaps have modestly ameliorated any negative effects that a lack of support for families has had on the overall employment rate.

Public spending on childcare and childcare subsidies in the United States is very low relative to the level of support provided in other countries. That said, back-of-the-envelope calculations based on data compiled by the OECD suggest that per-child public spending on childcare and early childhood education in the United States has risen, not fallen, over the period we are studying.[30] Furthermore, much of this spending has targeted children from lower-income households. This fact does not support the notion that low or falling levels of public support for childcare expenses have driven the decline in employment over this period.

A related piece of evidence comes from an assessment of the price of childcare over recent decades conducted by Herbst (2015). He uses data from a number of sources, including household survey data from the SIPP and establishment level data that he uses to estimate childcare costs. He finds that low-income families were not spending more on childcare in 2011 than they were in 1990. He further fails to find evidence that the cost of providing childcare has increased. Though not dispositive, his analysis of various sources of data argues against the idea that childcare services have become more expensive over this time period.

A somewhat different hypothesis is that just-in-time scheduling practices have created new childcare problems for working parents (Boushey and Ansel 2016). Workers with unpredictable schedules are apt to find it considerably more difficult to coordinate childcare and, if they do not have a regular childcare arrangement, may not qualify for available childcare 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 could have contributed to declining employment rates.

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, the relevant question for our purposes is again whether these entitlements have become less generous over time. The modest changes that have occurred in fact would appear to have been in the opposite direction. Since 1993, the Family and Medical Leave Act 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 preexisting temporary disability insurance program. New Jersey introduced a similar program in 2009, also providing up to six weeks of benefits; a program in Rhode Island providing up to four weeks of paid benefits took effect in 2014; and New York enacted a paid leave program providing for up to

[30] We base this statement on data on public spending reported in the OECD Family Database for 1999 and 2016 (the latest year available). The data are available at http://www.oecd.org/els/family/database.htm (accessed on June 19, 2019). A caveat is the OECD's acknowledgement that the data for countries with federal systems, such as the United States, may not be complete, due to the difficulty of obtaining information regarding state and local spending.

eight weeks of benefits that took effect at the beginning of 2018. The District of Columbia, Washington, and Massachusetts also have enacted laws to create paid leave programs, though none of these had yet taken effect as of the end of 2018.[31]

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 (2017) 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 California and New Jersey 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 trend decline in participation.

To sum up, the available evidence shows clearly that the cost of childcare can be an important impediment to mothers' employment. There is no strong evidence, however, that childcare costs have risen over time in such a way as to have contributed significantly to falling employment rates. Indeed, the available data suggest that, among the less educated and lower-income families for whom childcare expenditures might pose the greatest barrier to employment, costs per hour of care were little higher in 2011 than in 1990, though more current data would of course be welcome. To the extent that workers' schedules have become less predictable, however, arranging for childcare may have become more difficult and this could have contributed to falling employment. While paid leave for new parents may be desirable for other reasons, there is little evidence that its absence has had much effect on employment rates. Further, because the lack of paid parental leave is a long-standing feature of the US labor market, it logically cannot be responsible for falling labor market participation.

3.2.6 *Labor/Leisure Choice and Social Norms*

The decision to work reflects not only the monetary trade-offs associated with working, but also preferences for work versus other activities. If leisure has become relatively more attractive, either because leisure technology has improved or because there is less of a stigma attached to not working, then for a given wage, people will supply less labor. Some observers, such as Aguiar et al. (2017) claim that leisure has become more attractive to young men because of improvements in video game technology. Others, including Eberstadt (2016), argue that men are now more willing to sit out of work, essentially because of changing social norms. In a previous era, these observers argue, men of working age would have been ashamed not to be working, and their family members would not have been willing to support them if they did not work. Today, that is not necessarily

[31] Details on the state leave programs mentioned in the text can be found at https://www.nationalpartnership.org/our-work/resources/economic-justice/paid-leave/state-paid-family-leave-laws.pdf, accessed on July 29, 2020.

the case. In other words, preferences and social expectations could have changed in such a way that, facing similar circumstances, individuals today may be less likely to choose to work than would have been the case in the past. It is very hard to disentangle these competing explanations.

Aguiar et al. (2017) point to a specific factor that has potentially made leisure time more attractive to young men—improved video gaming technology. Time use data reveal that young men are filling their nonwork 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 period, men between the ages of twenty-one and thirty 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. Nonemployed 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. The authors' provocative conclusion is that 23 to 46 percent of the decline in the market hours of men ages twenty-one to thirty between 2004 and 2015 could be explained by innovations in video gaming technology. 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.

A somewhat different explanation is that the driving factor behind increased time on video games is 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 other relatives. If today's video gaming technology had been available during the 1990s, would young men then have worked less then than they did? Perhaps, but this is far from clear. In the story told by Aguiar et al. (2017), 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. We cannot readily rule out this cohort-based explanation.

As a factual matter, as noted by Aguiar et al. (2017), there has been a significant increase in the share of young adults who are living with a parent or relative other than a spouse. Citing data from the American Community Survey, Aguiar et al. (2017) note that 67 percent of non-employed young men ages twenty-one to thirty lived with a parent or close relative in 2012–15, as compared to 46 percent in 2000–2003. Aguiar and his coauthors interpret this as suggesting 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. Living with their parents is one way these men are able to support themselves if they choose not to work or to work less. Interestingly, although young women do not seem to have been as attracted to video gaming as young men and their employment rates have not fallen as sharply, they also became significantly more likely to live with a parent or close relative between 2000–2003 and 2012–15, suggesting that support from parents may be playing a safety net role for them as well.

One observation that is consistent with a change in social norms and preferences for work among prime-age men is an increase in temporary spells of nonwork among men who have a long-term attachment to the labor force. Coglianese (2018) documents a rise between 1984 and 2011 in "in-and-outs," who he defines as men who take short (less than two years at a time), infrequent breaks out of the labor force in between jobs. He shows that this phenomenon is distinct from a more permanent exit from the labor force, with different consequences and causes. In particular, he provides evidence suggesting that the rise of in-and-outs has occurred across all industries and does not result from a decline in labor demand for prime-age men, nor from the availability of disability insurance, but is more consistent with a change in the desired amount of labor supply among prime-age men in the United States.

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, or if it has become more socially acceptable for a married man to be supported by his wife, married men today 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, one would expect an increase in women's wages to lead to relatively more labor supply from wives and relatively less from husbands. As pointed out by the Council of Economic Advisers (CEA) in a recent report (2016), however, the raw data do not suggest that this is what is going on. 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). We cannot rule out the possibility that nonworking married men today are simply more comfortable relying on the earnings of their wives than in the past. On balance, however, there is not good evidence for thinking that spousal employment is a key factor behind falling employment rates.

3.2.7 *Opioid Use*

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 bases this conclusion on an analysis of county-level data from the Centers for Disease Control and Prevention (CDC) on the per capita volume of opioid prescriptions for 2015, combined with Current Population Survey (CPS) data on labor force participation for 1999–2001 and

2014–16. He estimates an OLS regression of individual-level male labor force participation on 2015 county-level per-capita opioid prescriptions, a dummy variable for 2014–16, the interaction of those two terms and other controls. Under the strong assumption that county-level opioid prescription rates are exogenous to county-level labor market trends, this regression yields a causal estimate of the effect of opioid prescriptions. Making that assumption, the results imply 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 labor force participation among prime-age men over this period.

Aliprantis, Fee, and Schweitzer (2018) fit models similar to those of Krueger (2017) using the same county-level CDC data on opioid prescriptions together with American Community Survey (ACS) data on labor market outcomes. Their analysis makes use of annual observations covering the period from 2007 to 2016; they regress dummy variables for current period labor force, employment, and unemployment status on the lagged county-level opioid prescription rate and other controls. Using ACS data rather than CPS data allows them to include more disaggregated geographic areas and more periods in their analysis, but their results are generally consistent with those of Krueger (2017).

An important question, however, is whether the causal arrow that underlies the correlations captured in the analyses just described in fact runs from opioid prescribing to employment rates or in the opposite direction. Harris et al. (2017) attempt to overcome this identification challenge and isolate exogenous variation in opioid supply by using the concentration of high-prescribing physicians in the county as an instrument for per-capita opioid prescriptions. They analyze the link between county-level opioid use (driven by the supply of physicians willing to write prescriptions) and employment using data from ten states covering the period from 2010 through 2015. In their baseline models, high-prescribing physicians are defined as the share of doctors in the county in the top 5 percent nationally of opioid prescriptions written or top 1 percent nationally of opioid doses prescribed, both based on Medicare Part D data, but they obtain similar results when high-prescribing doctors are defined based on prescriptions written for controlled non-opioid drugs. Their findings support Krueger's assertion; they conclude that increased opioid prescribing causes employment rates to fall.

Currie, Jin, and Schnell (2018) carry out an analysis similar to that of Harris et al. (2017) using national data for the 2006–14 period, with county-level prescription information purchased from QuintilesIMS and employment obtained from the Census Bureau's Quarterly Workforce Indicators program. They look separately at employment for men and women ages eighteen to forty-four and ages forty-five to sixty-four. Their models use the prescribing rate in the county for adults ages sixty-five and older to instrument the prescribing rate for younger adults. In contrast to Harris et al. (2017), Currie, Jin, and Schnell (2018) find a small positive effect of opioid prescribing on women's employment and no effect on men's employment.

Charles et al. (2019) provide some evidence pointing to a causal effect of weak labor market conditions driving increased drug use. Their paper shows that a decline in a state's share of employment in manufacturing between 2000–2016 (predicted using a shift-share instrument) is associated with an increase in per capita opioid prescriptions. Opioid use rose the most in places that experienced the largest exogenous adverse shocks to manufacturing.

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 magnitudes 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.

3.2.8 *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 (2017), net immigration contributed an average of 0.48 percentage points to annual population growth between 1990 and 2000. After 2000, the pace of immigration dropped off somewhat, but it continued to add roughly 0.3 percentage points 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. As discussed in the thorough review of the immigration literature offered by Blau and Mackie (2017), 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. In addition, 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 the wages and employment of natives 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 on natives 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 1970–80 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 sixteen-and-seventeen-year-olds, but has little effect on the hours of older natives.

In contrast to these studies estimating sizable effects for some groups, 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."

Although the literature has focused primarily on the effects of immigration on native workers, for the purpose of understanding how immigration might have affected the overall employment-to-population ratio, it also is relevant to ask how the employment rate among immigrants compares to the employment rate among natives. Immigration tends to occur during a person's economically active years rather than later in life. All else the same, this will tend to make the employment-to-population ratio higher among new immigrants than among the native population, since the flow of new immigrants includes relatively few older people. As of 2018, the overall employment rate among all foreign-born individuals sixteen and older was 63.4 percent, slightly higher than the native rate of 59.8 percent. Foreign-born men were more likely than native men to be employed (75.5 percent versus 64.5 percent) and foreign-born women were somewhat less likely than native women to be employed (52.1 percent versus 55.5 percent).[32] All else the same, given the composition of the immigrant population, the rising share of the ages-sixteen-and-older population that is foreign born may have had a slight upward effect on the overall employment-to-population ratio, but we are not aware of research that has addressed exactly this question.

Our reading of the available evidence is that, broadly consistent with the conclusion reached by Blau and Mackie (2017), 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. As a group, immigrants are somewhat more likely than natives to be employed. 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.

3.3 *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 and occupational licensing requirements. There has been a great deal of research on the employment effects of minimum wages; the empirical basis for drawing conclusions about the employment effects of occupational licensing is thinner.

3.3.1 *Minimum Wages*

The subject of how minimum wages affect employment has long been contentious. In a perfectly competitive labor market, a minimum wage that exceeds the market-clearing wage can be expected to reduce employment, but the size of the effect will depend on the elasticity of labor demand. In a monopsonistic labor market in which firms' marginal cost of labor may exceed the wage they are paying, however, a minimum wage that raises wage rates, at least up to some level, need not reduce employment.

For many years, the standard reference on the topic of how the minimum wage affects employment 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 1–3 percent reduction in teen employment, with little effect on

[32] Statistics on the native and foreign-born labor force reported by the Bureau of Labor Statistics were accessed at https://www.bls.gov/news.release/pdf/forbrn.pdf on June 16, 2019.

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, b; 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 eight states and the District of Columbia as of the beginning of 1988, by 2018 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 28 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 had increased to the changes in states or counties deemed to be similar where no such increase had 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 (2014) and Powell (2016), have found significant negative employment impacts.

One difference across this set of studies lies with 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 (2019) examine the impact of the increase in the federal minimum wage in July 2009 to $7.25 per hour on the subsequent employment of workers who had been earning less than $7.50 per hour in 2008. They use data from the 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. Their baseline estimate is that the 2009 increases in the federal minimum wage reduced employment in the affected group by 6.6 percentage points or about 9 percent, which translates to a potential effect on the overall employment-to-population ratio of about half a 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 baseline 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. (2019) 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 is a range of estimates and no consensus in the literature.

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 we do not attempt to do so.

Because turnover rates are high among minimum wage workers, most existing research has assumed that adjustments to an increase in the minimum wage occur 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. To the extent that existing estimates look at minimum wage effects realized over relatively short periods of time, they may understate the long-run effects of higher minimums.

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. Brummund and Strain (2020) use county-level data for the period from 1990 to 2016 to compare the effects of minimum wage increases in cases where the minimum wage is and is not indexed to inflation. They find substantially larger employment elasticities in response to an increase in the case of an indexed minimum wage. Further, we would add, 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.

To estimate the potential impact of minimum wage increases between 1999 and 2018, we first need to know how the average real minimum wage changed over this period. Data from the Department of Labor on statutory state and federal minimum wages are not available for 1999; we base our estimates on the information reported for 1998. According to our calculations, the effective real minimum wage fell by 3.2 percent from 1998 to 2007 and then rose by 13.5 percent between 2007 and 2018, for a net increase of 9.9 percent over the entire 1998 to 2018 period.[33]

[33] Our estimate of the effective change in the minimum wage was constructed by weighting the percentage increase in the real minimum wage in each of the 50 states and the

To set an upper bound on the potential dis-employment effect of this 9.9 percent increase in the effective minimum wage, we take the estimated employment elasticity for teenagers of –0.44 from Powell (2016), an estimate that is relatively high compared to those reported in most other recent papers. Powell does 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 Congressional Budget Office (2014b) assessment of the minimum wage literature and arbitrarily assume that the employment elasticity for affected adults with respect to an increase in the minimum would be a third the size of the elasticity for affected teens. Teens are about four times as likely as adults to have wages at or below the minimum wage, leading us to assume an effect for all adults of about one-twelfth the size of the effect for all teens. Under these assumptions, the estimated effect of minimum wage increases since 1999 on the 2018 employment-to-population ratio is roughly 0.3 to 0.4 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 roughly a 0.10 percentage point reduction in the employment-to-population ratio between 1999 and 2018. We hasten to add, however, that there is a considerable error band around this estimate.

District of Columbia over each of the periods in question by the jurisdiction's 2000 population share. Information on state minimum wages is available at https://www.dol.gov/agencies/whd/state/minimum-wage/history (accessed on June 17, 2019); 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. Similar results were obtained using 2010 rather than 2000 state population shares to carry out the calculations.

3.3.2 *Rise in Occupational Licensing*

Another possible explanation for falling employment rates is the notable increase in the share of workers in occupations for which a state or local government license is required to work. By one estimate, this share has risen 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 occupations, such as physicians, dentists, teachers, and electricians, in which there is an obvious rationale for requiring some demonstration of the qualifications of those performing the work. They also include 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).

The literature offers two different perspectives on the role of occupational licensing. One perspective emphasizes the barriers that occupational licensing creates to entry into affected occupations. In this view, occupational licensing should raise wages but reduce employment in covered occupations. A second perspective emphasizes the role that licensing may play in increasing consumer confidence and thereby potentially increasing the demand for the services of those in affected occupations. These two competing perspectives share the prediction that licensing should raise occupational wages, but differ in their predictions about the effects of licensing on occupational employment. Further, even if it is the case that licensing reduces employment in the affected occupations, the effects on aggregate employment are less straightforward. A decline in employment in the licensed sector should increase the supply of labor to the non-licensed sector. If wages in the non-licensed sector fall as a result, those jobs may become less attractive and some people who otherwise would have worked may decide to leave the labor force. 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.[34]

Numerous studies have concluded that occupational licensing requirements raise wages in the licensed occupations. In an analysis using American Community Survey data for the 2000s, Thornton and Timmons (2013) find, in models that include state fixed effects, that the introduction of licensing requirements for massage therapists raised their wages by about 12 percent. Most state licensing requirements for nurses were introduced between 1940 and 1980. Looking at Census data for this period, Law and Marks (2017) find that the introduction of these licensing requirements was associated with an increase in nurses' wages of 5 to 10 percent. Gittleman, Klee, and Kleiner (2018) 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 of about 5 percent.[35] Using data from a telephone survey they commissioned Westat to conduct, Kleiner and Krueger (2013) find that state-level licensing is associated with an average occupational wage premium on the order of 15 percent, roughly in line with the wage premium associated with union membership.

Evidence on the employment effects of occupational licensing is more mixed. Thornton and Timmons (2013) find that licensing requirements for massage therapists reduced their share of employment, though we note that the data from the Occupational Employment Statistics survey they analyze exclude self-employed massage therapists. Using decennial Census data, Law and Marks (2017) find no significant effect of licensing requirements for nurses in a state on the share of labor force participants who report a nursing occupation. Kleiner (2006) presents evidence that within-occupation employment growth is substantially slower in states with full licensing requirements. In a recent study, Blair and Chung (2019) use the cross-state county-boundary pairs that they are able to observe in Current Population Survey data for 2015 to study the effects of licensing requirements on the share of employment in affected occupations. Their estimates imply that a licensing requirement reduces the share of people employed in an occupation by 17 to 19 percent. Although the estimates in some of these studies are large,

[34] Stories similar to those told about the potential effects occupational licensing also have been told about the potential effects of unionization. On the one hand, the fact that unionization leads to a union wage premium could mean that fewer people end up working in the union sector than would be efficient, leading to crowding in the nonunion sector. On the other hand, unionization may be associated with higher productivity that justifies union workers' higher wages (see, e.g., Freeman and Medoff 1984). In recent decades, as the prevalence of occupational licensing has risen, the share of the US workforce who are union members has fallen, 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). To the extent that the growth in occupational licensing has affected the labor market mainly by reducing the supply of workers to licensed occupations and pushing more people toward unlicensed occupations, the decline in unionization occurring over the same period might be expected to have had offsetting effects. More specifically, the decline in unionization could have reduced the extent to which above-market union wages push workers out of that sector and crowd them into the nonunion sector.

[35] 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 they estimate for white men (8 percent).

having a lower share of employment in licensed occupations does not necessarily imply that overall employment is lower.

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 any requirements set in the new state to continue working in the occupation. Peterson, Pandya, and Leblang (2014) exploit changes in state-level residency training requirements for immigrant physicians over the years between 1973 and 2010. They find that states imposing more stringent requirements receive fewer immigrant physicians, consistent with the prediction that occupational licensing restricts employment-based migration. One might expect reciprocity agreements under which states recognize licenses issued in other states to reduce the barriers to inter-state mobility created by licensing requirements. In one recent study, however, DePasquale and Stange (2016) find no increase in geographic mobility or employment among nurses after the reciprocal arrangements associated with the Nursing Compact were introduced. Johnson and Kleiner (2017) make use of a novel strategy to distinguish the effects of licensing from the effects of other possible differences between licensed and unlicensed occupations, such as the importance of having an established client base, on interstate mobility rates. They estimate the coefficient on licensing in two equations, one with whether the respondent made an interstate move as the dependent variable and the second with whether the respondent made a within-state move to a nonadjacent area as the dependent variable.[36] The difference between these coefficients is their estimate of the effect of licensing on interstate mobility. In Johnson and Kleiner's models, estimated using American Community Survey data for the period 2005 through 2015, licensed occupations are the 22 occupations for which all states impose licensing requirements. They find that licensing requirements that differ across states (e.g., those for lawyers, insurance agents, elementary school teachers, electricians, and barbers) lead to significantly lower rates of inter-state mobility, but licensing requirements that are essentially national (e.g. those for social workers, veterinarians, nurses, doctors, dentists, optometrists, and a variety of other health professionals) have no such effect.

Given the dramatic increase in occupational licensing over recent decades and a plausible rationale for believing this could have led to net reductions in employment, it seems possible that occupational licensing has contributed to the decline in the employment-to-population ratio over the 1999 to 2018 period. One channel for such an effect might be that licensing has made 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.

3.3.3 *Other Institutional Frictions*

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 reductions in labor market fluidity more generally. Davis and

[36] The areas used by Johnson and Kleiner (2017) are what are referred to as Public Use Microdata Areas of Migration, corresponding to 100,000 or more residents and defined separately within each state.

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 (2014) argue, it implies longer jobless spells that could lead to a loss of human capital and counterproductive 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, and Notowidigdo 2013; Ghayad 2013; and Eriksson and Rooth 2014, though in a study focused on college-educated women, Farber, Silverman, and Von Wachter 2016 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.

3.3.4 *Skill Mismatch between Workers and Jobs*

Another argument is that structural mismatch between the skills possessed by available workers and the requirements of available jobs has prevented employers from hiring as many people as they would like, leading to a lower level of employment. The fact that unemployed workers coexist with vacant jobs sometimes is cited in support of this argument. During the recent Great Recession, for example, statements to this effect were made by politicians from both parties seeking to explain 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. As documented by Abraham (2015), the Beveridge curve was stable between 2000 and 2009. During the years following the onset of the Great Recession, however, higher vacancy rates were associated with given unemployment rates, leading some to conclude that mismatch between available workers and vacant jobs must have worsened. Absent direct evidence of growing mismatch, however, this may have been 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 from the Current Population Survey on the jobs most recently held by the unemployed 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.

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. 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. Davis, Faberman, and Haltiwanger (2013) provide some evidence that the outward postrecession shift in the Beveridge curve might have been related to declining employer recruitment intensity. Whatever the explanation, by 2018, unemployment and vacancies were back along the prerecession Beveridge curve, consistent with the apparent outward shift in the aftermath of the Great Recession having been a temporary cyclical phenomenon rather than the result of any longer-term increase in mismatch.

More fundamentally, thinking about mismatch simply in terms of the apparent fit between unemployed individuals and vacant jobs may be misguided (Abraham, Haltiwanger, and Rendell 2020). For one thing, the unemployed are not the only people who are potentially available to fill vacant jobs. People currently out of the labor force are an important potential source of labor supply; perhaps surprisingly, in a typical month a larger number of jobs are filled by people who had been out of the labor force the previous month than are filled by people who had been unemployed (see, e.g., Hornstein, Kudlyak, and Lange 2014). Employers also fill a significant share of their vacant positions by recruiting people who are currently employed elsewhere (see, e.g., Hall and Schulhofer-Wohl 2018). Given the variety of options available to employers for filling their jobs, simply comparing the industry or occupational distribution of available positions with the industry or occupational distribution of the last job held by currently unemployed individuals could give a very misleading picture of the extent of skill mismatch in the labor market. Further, employers are likely to have some discretion about how to organize work at their firms, and thus some discretion about the types of vacancies they will seek to fill.

All of this is not to say that skill mismatch plays no role in the labor market, but only that there is a lack of direct evidence about its importance and, more important, no compelling reason to believe that it has worsened over time. Our reading of the limited available evidence is that growing skill mismatch is unlikely to have contributed notably to the observed decline in employment rates, but further research on this topic would be welcome.

3.3.5 *Spatial Mismatch and Reduced Geographic Mobility*

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. (2018) offers causal evidence that distance from available jobs 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 clear whether the findings can be generalized. More importantly, we do not know that (travel time) distance from possible jobs has increased for less-educated workers.

Declining rates of geographic mobility are another possible explanation for falling employment rates. 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 postwar period. Their tabulations using data from the US Census show 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 geographic 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 1980–2010, 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 US 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 US 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.

Autor (2019) casts further doubt on the role that declining mobility has played in driving down employment among adults without a college degree. He focuses mostly on wages, but his work likely has implications for employment as well. He shows that the urban wage premium—the relatively higher wage that a worker of a given level of skill would earn in a metro versus nonmetro area—that historically existed for all workers has disappeared for noncollege educated workers. Whereas other authors have posited that the decline in geographic mobility of noncollege workers into high-wage cities has contributed to their weak employment and wage outcomes, Autor (2019) shows that there has been a disappearance of the middle-skill jobs in metro areas that once benefited noncollege workers relative to their nonmetro counterparts. He proposes that "the slowing inflow of non-college workers into urban labor markets may reflect less a failure of arbitrage than a fall in the economic allure that these labor markets once held for less-skilled workers." This does not imply that out-of-work individuals might not be able to increase their employment by moving, but Autor (2019) raises doubts about the claim that reduced mobility necessarily means that individuals—noncollege educated individuals in particular—aren't taking advantage of employment opportunities that exist elsewhere.

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, make 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.

3.3.6 *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 US 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 thirty-to-thirty-four-year-old African American man 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).[37]

Because standard labor market statistics derived from the Current Population Survey are based on the noninstitutionalized 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 ages twenty to thirty-nine 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). For black men in the same age range, however, it lowers the employment-to-population ratio 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

[37] We focus here on incarceration and its effects on subsequent employment outcomes, but note that the number of people with felony convictions who do not serve prison time also has risen (Shannon et al. 2017). Less is known about this population and their subsequent experiences than about those who are incarcerated.

rates is that labor market skills could deteriorate while a person is in prison, although 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 2018, Doleac and Hansen 2020).

The most credible estimates that we know of on the causal impact of incarceration on later employment come from Mueller-Smith (2015), who uses original data from Harris County, Texas. His data set 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.[38] 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. The largest effects are for those 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. Among those in that group who served at least two years, there is a statistically significant 39 percentage point reduction in the likelihood of employment two years after release; among those who served one to two years, there is a statistically significant 24 percentage point reduction. These are very large effects. The estimated effects for a six-month prison term or for those with low or no 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 (2015) estimates of the causal impact of having served time on employment. Ideally we would have data on the stock of US adults who have been incarcerated, but this information does not exist in any public data set. 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 ages eighteen and sixty-four as of 2014; we use 6.5 million, the midpoint of this range, in our calculations.[39] Note that this estimate does

[38] An earlier study by Kling (2006) used random assignment to judges to isolate a causal effect of longer incarceration time. His study uses state prison records from Florida (1993–2002) and California (1987–97), so he is limited to looking at length of incarceration rather than the extensive margin of any incarceration. In contrast to what Mueller-Smith (2015) finds with regard to length of time served, Kling (2006) does not find evidence that a longer incarceration sentence is associated with worse outcomes; in fact, he finds that in the short term, one to two years post-release, a longer sentence term is associated with an increase in employment and earnings.

[39] We would expect the number of previously-incarcerated individuals in the noninstitutionalized civilian population to have been larger in 2018 than in 2014, but have no way to estimate how much larger and therefore simply use the Bucknor and Barber estimates in our calculations.

not include people who served time in jail rather than prison.[40]

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 (Bureau of Labor Statistics 2019), when sample members were roughly ages thirty to thirty-four. Based on the NLSY97 data, we estimate that about 9.1 percent of these young adults who were ages thirty to thirty-four 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 (4.1 percent of the population) and that the remainder of those with spells of less than a year together with those reporting longer spells had been in prison (5.0 percent of the population). Among the 5.0 percent we assume had been in prison, approximately 42 percent had been confined for two years or more, approximately 24 percent had been confined for one to two years, and approximately 34 percent had been confined for less than a year.

Based loosely on observed trends, we assume that 60 percent of the formerly incarcerated population estimated by Bucknor and Barber (2016) served time as a result of the policy-induced rise in incarceration rates since the 1990s.[41] This yields an additional 1.6 million working-age individuals with a prior prison term of two years or longer and 0.9 million with a prior prison term of one to two years. Using the numbers on the distribution of preconviction earnings obtained by Mueller-Smith (2015), 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. We then apply his estimates of the reduction in the probability of employment associated with a prison term—39 percentage points for those with significant prior earnings and two years or more in prison; 24 percentage points for those with significant prior earnings and one to two years in prison; 11 percentage points for those with low prior earnings and two years or more in prison; and 9 percentage points for those with low prior earnings and one to two years in prison. Based on these calculations, we estimate that in the absence of the rise in incarceration, there would have been about 307,066 more employed workers in 2018. Note that this calculation assumes no incarceration-related employment losses among those ages sixty-five and older. Adding these extra workers to the workforce would have increased the employment-to-population ratio by about 0.12 percentage points.

Given the number of assumptions 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

[40] 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 US 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.

[41] Among the NLSY79 cohort born between 1957 and 1965, 7.2 percent report having been jailed before the age of thirty-four; 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.

TABLE 3
Factors Contributing to Decline in Employment-Population Ratio, 1999–2018

Factors	Estimated reduction in EPOP (percentage points)
Major contributing factors	
Import competition from China	0.92
Adoption of industrial robots	0.43
Other contributing factors	
Increased receipt of disability benefits (SSDI, VADC)	0.17
Higher minimum wages	0.10
Increased rate of incarceration	0.12
Insignificant factors	
SNAP expansions	~0
Public health insurance expansions	~0
More generous EITC	~0
Increased difficulties due to lack of family leave	~0
Expanded immigration	~0
Indeterminate given state of evidence	
Increased difficulties due to lack of child care	unclear
Changes in leisure options	unclear
Changes in social norms	unclear
Increased use of opioids	unclear
Rise in occupational licensing	unclear
Increases in institutional frictions and/or mismatch	unclear
TOTAL NET EPOP DECLINE (percentage points)	3.8

Note: EPOP stands for employment-to-population ratio.

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.

4. *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 overall employment-to-population ratio over the 1999 to 2018 period. Table 3 lists the factors that we have considered as potential drivers of this decline, 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 the identified factors; in other cases, there is too little available evidence for us to draw quantitative conclusions. As reported in table 1A, the employment-to-population ratio for the population sixteen and over fell by 3.8 percentage points between 1999 and 2018. This number is useful as a way to scale the percentage point reductions attributed to the various factors, but 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, among the factors whose effects we are able to quantify, labor demand factors are the most important drivers of the secular decline in employment over the 1999 to 2018 period. In this category, the effects of increased imports from China are single largest contributor to the decline in employment, potentially accounting for an estimated 0.92 percentage point decline in the employment-to-population ratio. The next largest contributor we are able to quantify is the growing penetration of robots into the labor market. Based on the evidence reviewed, we attribute a decline in the employment-to-population ratio of 0.43 percentage point to this factor.

We judge labor supply factors as a group to have been less important drivers of the decline in employment. Our rough estimate is that the growth in SSDI caseloads over the 1999 to 2018 period led the employment-to-population ratio to be 0.09 percentage points lower than it otherwise would have been. We also conclude that the Veterans Affairs Disability Compensation program likely has contributed to a reduction in the employment to population ratio, on the order of perhaps an additional 0.07 percentage points. Taken together, the estimated effects of the two disability programs sum to perhaps 0.17 percentage points. We do not attribute any of the reduction in aggregate employment to other social safety net programs, such as the SNAP program, the expansion of public health insurance or health insurance subsidies, or the EITC program.

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 childcare 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 accessibility of childcare, especially for low-wage workers with unpredictable schedules, and further research on the role of family policy broadly construed as an influence on parents' employment decisions would be welcome.

We do not attempt to assign a magnitude to the possible contribution of improved leisure technology, in particular video 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 caused by opioid supply rather than endogenous demand for drug use as still being rather speculative. This is another issue that warrants further research.

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.

Turning to the potential effects of labor market frictions, increases in the real value of state minimum wages also may have had an impact on the employment to population ratio, accounting for perhaps an additional 0.10 percentage point decline over the period of interest. Another factor that may have played a role is the rise in incarceration 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 0.12 percentage points.

Although there is growing evidence that occupational licensing affects entry into covered occupations, the literature has little to say about its effects on the level of aggregate employment. We have seen no compelling evidence that institutional frictions have been important drivers of falling employment, but given the decline in worker mobility and the open question about the reasons for that decline, we view this as another topic on which the literature has not yet produced a definitive answer.

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 parameters 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. We are more confident about our qualitative conclusions concerning whether a factor's impact is relatively large or relatively small.

Throughout the paper, we have attempted to highlight open questions and identify areas in which more research is needed. There are many outstanding questions and much to explore.

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