Parolefare: Post-prison Supervision and Low-Wage Work

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How might parole operate as a labor market institution, and how might it contribute to the governance of poverty and social marginality? Drawing on a series of correctional, employment, and arrest records for a cohort of parolees in Michigan, we show that parole generally supervises a jobless population, but also oversees a significant number of people who work. We also find evidence that parole, contrary to many expectations, increases the odds of employment. However, we do not find convincing evidence that parolee employment alleviates individual poverty or reduces the odds of recidivism. These results inspire a conceptualization of parolefare*, another poverty regulating regime that successfully motivates worker-citizenship but does little to extend or protect the life chances of the poor.*

Keywords: parole, employment, recidivism, poverty governance

Research illustrating how the carceral state sweeps up those on the margins of the labor market and exacerbates their marginality in the process is more than ample (Apel and Sweeten 2010; Bushway, Stoll, and Weiman 2007; Irwin 2004; Pager 2007; Raphael 2014; Wacquant 2009; Western 2006). Accordingly, many elected officials, academics, and think tankers have of-

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fered a common prescription for reducing criminal justice contacts: put convicted felons to work (City of New York 2017; Finn 1998; Kachnowski 2005; Mead 2007; Western 2008). This recommended treatment is often packaged with other calls for behavioral and contextual interventions (substance abuse programming, cognitive behavioral therapy, and housing assistance), but employment remains a primary concern. It is not surprising, then, that so many programs behind bars and on the street tend to emphasize the promise of wage labor for justice-involved populations (Abrams and Lea 2016; Jonson and Cullen 2015; Miller 2014; Muhlhausen 2018; Seim 2016).

The intuition that hard work can be a good preventative for hard time does indeed find some support in evidence-based evaluations of job-training, transitional employment, work release, and so-called human capital investment programs (Drake, Aos, and Miller 2009; Redcross et al. 2012; Solomon et al. 2004; Uggen 2000). Yet evidence is also compelling that such interventions actually have little to no effect on future recidivism (Bohmert and Duwe 2011; Jacobs 2012; Moses 2012; Turner and Petersilia 1996; Visher, Winterfield, and Coggeshall 2005). Complicating matters even more, simple employment may matter less than quality employment. In general, working in the lower bulb of an employment hour glass—in the secondary labor market where most people who encounter police, courts, jails, prisons, and other correctional institutions tend to be found when they are not jobless or incarcerated—may do little to discourage criminal activity (Crutchfield 2014; see also Schnepel 2018; Uggen 1999).

Although insightful, much of the research that underscores these kinds of discussions neglect a significant sector of American criminal justice: parole. This omission is problematic. For one, the sheer size of parole suggests its significance among people caught in the belly of criminal justice processing. In Michigan, the focus of this study, more than 90 percent of state prisoners are released on parole supervision (Herbert, Morenoff, and Harding 2015).[1] Also, perhaps more than any major institution of criminal justice, parole is officially oriented toward shaping people's labor market participation. Indeed, parole officers usually mandate their subjects to work in the formal economy and apparently mix practices of policing with practices of social work to encourage compliance (Irwin 1970; Petersilia 2003; Werth 2013; West and Seiter 2004). These officers usually couple this frequently articulated, but rarely enforced, employment mandate with claims that labor can promote desistance and that wages can significantly challenge post-prison poverty (Seiter 2002).

To our knowledge, no convincing research suggests whether these claims are valid. Reasons have certainly been offered to suspect that parole may impel labor through surveillance, sanction, and aid (Rakis 2005). However, this is a far cry from what much of the scholarship on prisoner reentry tells us. Not only are formerly imprisoned people a notoriously difficult-to-employ population, but parole officers may actually impede labor by burdening their subjects with drug tests, check-ins, mandatory treatment programs, and other requirements (Patersilia 2003; Solomon et al. 2004; Travis 2005; Werth 2011). Accordingly, parole may paradoxically dampen its double mission of public safety and offender reintegration by building barriers to post-prison employment.

In this article, we turn to a unique dataset of longitudinal parole, employment, and arrest records for a cohort of working-age formerly imprisoned people in Michigan to address two questions. First, does parole affect the odds of employment? Second, does working while under parole supervision reduce the odds of recidivism? We first calculate the unadjusted employment rate and the wages earned for this population over five years. Then we assess the potential labor effect of parole. We deploy a fixed-effects approach to model the odds of

1. Fragmented across fifty-one departments (federal plus state) and often divided administratively by county, parole agencies in the United States collectively supervised an estimated 870,500 formerly imprisoned people at year-end 2015 (Kaeble and Bonczar 2017). So-called discretionary parole decisions may be increasingly replaced by mandatory release dates, but the regime that supervises former convicts and often facilitates their return to the penitentiary, what is typically called parole, remains.

quarterly employment. To check the robustness of our findings, we also model the odds of employment in five relatively common industries for formerly imprisoned people to work in (employment services, manufacturing, food services, construction, and retail). Again using fixed-effects modeling, we consider the consequences of parolee employment by estimating the odds of recidivism as measured by arrest or reincarceration (prison or jail).

Our results complicate what social scientists often assume about parole and parolee labor. Although employment is unquestionably low for parolees, we find that parole supervises a sizable number of formally employed people. We demonstrate that earnings are low for these workers, the median hovering around the federal poverty line. We also find, however, that parole exposure is positively associated with employment. By comparing individuals over time, we demonstrate that discharge from parole significantly lowers the odds of working in the formal economy. This association is not apparently driven by one industry, such as employment services (which includes temp work), but is also evident in manufacturing, food services, and retail industries. This finding directly counters commonly held assumptions that parole hinders employment. Last, in considering the effects of parolee labor, we do not find evidence that working while under parole is associated with decreased recidivism. This finding seems to be consistent with claims that so-called bad jobs are not reasonable deterrents of criminal justice contact and helps disrupt a false crime-employment dichotomy (Crutchfield 2014; Fagan and Freeman 1999; Harding, Morenoff, and Wyse 2019; Ramakers et al. 2017).

These findings inspire a conceptualization of *parolefare*, another poverty-regulating regime that motivates worker-citizenship but does little to extend or protect the life chances of the poor by way of increased employment. Like work-centric welfare programs, parole seems to encourage wage labor, but mostly for poor men instead of poor women. Similarly, parole does not apparently offer a convincing pathway out of poverty through formal employment. Such comparisons nevertheless account for only part of the story. Parole operates less like a workfare institution and more like a penal institution in that it supervises a population still largely outside formal labor. And, unlike some work-first welfare programing, parole does not apparently yield docility by way of mandated labor. We do not find evidence that holding employment while under parole lowers a person's odds of recidivism. This raises serious questions about the functions of employment mandates for parolees and other justice-involved populations.

POOR DISCIPLINE OR DISCIPLINING THE POOR?

We understand parole to be part a splintered and contradictory state that governs, or regulates, relatively poor populations using a mixture of assistance and punishment (Piven and Cloward 1971; Seim 2017; Soss, Fording, and Schram 2011; Wacquant 2009). The technologies of American poverty regulation are numerous and specific, but two complementary strategies highlighted by Loïc Wacquant (2009) seem integral today: a *workfare* regime for processing mostly poor women who are also disproportionally women of color and a *prisonfare* regime for handling their male counterparts. Through workfare, the state reinforces the conditions for employers to exploit the poor by conditioning aid on labor-force participation (Collins and Mayer 2010; Hays 2003; Peck 2001; Soss, Fording, and Schram 2011). Through prisonfare, the state manages poor populations generally and historically excluded from labor, property, and civic life, amplifying their exclusion by containment and isolation (Alexander 2012; Irwin 2004; Western 2006).

We draw on two perspectives, one that preferences a more prisonfare-based explanation and one that preferences a more workfare-based explanation, to develop some hypotheses on parole and labor. These perspectives are mined from two similarly titled books: Jonathan Simon's *Poor Discipline* (1993) and Joe Soss, Richard Fording, and Sanford Schram's *Disciplining the Poor* (2011). According to Simon, the state, through the parole office, regulates an impoverished population largely excluded from labor. For him, contemporary parole officials recognize the futility of labor as a disciplinary force and do little to actually promote post-prison employment. Put another way, parole is

part and parcel of a kind of prisonfare management of urban poverty. However, should parole actually impel more than impede labor, then Soss and colleagues analysis may be more useful. For those authors, the state regulates an impoverished population marginally included into the most exploitive segments of labor by way of the workfare office.

In *Poor Discipline*, Simon (1993) divides parole into three historical periods: disciplinary parole (circa 1890 to 1950), clinical parole (circa 1950 to 1970), and managerial parole (circa 1970 forward). During the first two periods, parole incentivized labor to varying degrees. Under disciplinary parole, the state coerced formerly imprisoned people into a tight labor market, subjecting them to the normalizing, punitive, and controlling conditions of work. Under clinical parole, the state decreased its focus on the employment of formerly imprisoned people. Less able to rely on a tight labor market to discipline their subjects, parole authorities intensified their supervision capacities to control and rehabilitate parolees therapeutically. The end of the clinical era was defined in part by a recognition of the "futility of enforcing a labor requirement on a population increasingly excluded from the labor market" (Simon 1993, 96). Work mandates were often underenforced, though most were preserved on paper. This ushered in the current regime. In partial reaction to mass incarceration and the hardening of urban poverty, so-called managerial parole shifted even further away from labor enforcement. This does not mean that all efforts to promote parolee employment have ceased. It does suggest, however, that parole is now generally oriented toward securing, containing, and isolating "a class excluded from the labor market" (Simon 1993, 259). From this point of view, the flourishing of contemporary employment-based programs for parolees might best be explained as an effort to neutralize and surveil a population of labor market outcasts.

In *Disciplining the Poor*, Soss, Fording, and Schram (2011) offer a comparable account of contemporary poverty governance in America, but they focus less on the penal state and more on its welfare analog. They argue that poverty governance in the United States has taken a "disciplinary turn," a mutation defined by the interaction of two political forces: neoliberalism (strengthening the state in service to the market and reorienting public and quasi-public institutions around market principles) and new paternalism (expanding state surveillance of unruly populations and conditioning public goods and rights on the demonstrable fulfillment of social obligations). When read with *Poor Discipline*, it becomes clear that the "neoliberal paternalism" detailed in *Disciplining the Poor* ascends concurrently with managerial parole in the late twentieth century.

Although these studies have much in common, one difference between *Poor Discipline* and *Disciplining the Poor* is clear. The managerial parole regime detailed in the first book aims to surveil and isolate a population largely omitted from the labor market. The neoliberal paternalistic regime detailed in the second, however, pressures its subjects "into accepting the worst jobs at the worst wages" (Soss, Fording, and Schram 2011, 7). These books inspire competing hypotheses regarding the employment effects of parole. From *Poor Discipline*, we might expect that in addition to supervising a largely jobless population, parole has no, little, or perhaps even a negative labor effect. From *Disciplining the Poor*, we might expect parole, should it operate like workfare, to significantly impel labor.

Our inquiry should not end with just a consideration of employment. As Soss, Fording, and Schram (2011) make clear, successfully promoting poor people's employment does not guarantee an extension of their life chances. In general, the employment incentivized by workfare does not adequately challenge poverty, and this may also be the case for employment incentivized by parole. Although, to be fair, that is not really part of the formal mission of parole or its labor mandate. As noted, these mandates are typically justified as a strategy to challenge future criminal justice contact. Thus, in speculating how incentivized employment may or may not extend the life chances of former prisoners, we should also consider the association between parolee labor and recidivism.

METHODOLOGY

As noted, our study focuses on parole in Michigan. General parole requirements in the state

include an explicit mandate to secure and maintain employment. Various exemptions aside, such as for those who are diagnosed with a disability, enrolled in school, or over the age of sixty-five, most parolees in Michigan are formally required to work. There is little reason to believe that parole officers are reincarcerating their subjects for being unemployed, but they seem to incentivize employment in different ways. As we learned from conversations with agency officials and former prisoners (Harding, Morenoff, and Wyse 2019), Michigan parole officers do not typically require employed parolees to report to them as often as unemployed parolees. Parole officers in the state are also supposedly more likely to allow their subjects with jobs to check in by phone rather than in person. For those who are unemployed and not in school, evidence of time spent searching for work is typically required by parole officers. It remains to be seen, however, whether incentivizing employment this way is effective or if working while under supervision significantly extends a parolee's life chances.

Data

We draw on three primary data sources. First, we examine a longitudinal set of administrative data from the Michigan Department of Corrections (MDOC). These data include all people in Michigan who were paroled from a state correctional facility to Michigan communities in 2003 (n = 11,064). In addition to offering detailed demographic information such as dates of birth and county of parole, these records also detail the dates people started parole and, if applicable, the dates they were discharged from parole or died as well as the dates of certain post-prison events, including reincarceration. Second, we link our sample to data from the Michigan Unemployment Insurance Agency (MUIA) that contain employer-reported quarterly records of wages earned and industries worked according to the North American Industry Classification System. We matched 99.7 percent of the 2003 parolee cohort records to the MUIA data, leaving a cohort size of 11,032.[2] Although the employment data do not include informal work, they provide insight into parolees' participation in formal labor. Third, we also link to post-prison arrest records that local law enforcement agencies report to the Michigan State Police (MSP). These records capture incidents in which an individual was arrested by police (which may or may not lead to a parole revocation).[3]

Using the merged MDOC, MUIA, and MSP files, we constructed a dataset that nests quarterly records within individuals and limits the observation period to quarters between individual-specific prison release dates for the 2003 parole and December 31, 2007. This provided us with an observation period of up to a maximum of twenty calendar quarters per individual. During this period, Michigan experienced a relatively stable monthly state unemployment rate of 7 percent from January 1, 2003, through December 31, 2007 (Bureau of Labor

2. To match parolees with their quarterly employment statuses, all social security numbers (SSN) available in MDOC databases for the 2003 parole cohort were sent to the Michigan Unemployment Insurance Agency and Workforce Development Agency for matching. In some cases, more than one SSN was available for each subject. For thirty-two individuals, MDOC had no SSN, so these individuals have no UI data and are removed from the dataset. Returned UI records were matched with names from MDOC databases, including aliases, to eliminate incorrect SSNs. Approximately 5 percent of the sample had no UI data match their SSN, indicating that they had no formal employment in Michigan between 1997 and 2010. If more than one SSN that MDOC had recorded for the same person matched records in the UI data, project staff selected the best match by comparing employer names listed in the UI records with those listed in the MDOC records (from parole agent reports). This procedure resulted in one-to-one matches of individual records between MDOC and UI records for more than 99 percent of sample members. For less than 1 percent of the sample, a single SSN could not be selected after matching on the parolee's name and the name or names of that person's employer or employers. In such cases, UI data were retained for all SSNs listed in the MDOC records for a given individual, under the assumption that such people worked under multiple SSNs.

3. People placed in custody by a parole agent are not captured by the MSP arrest measure.

Statistics 2018). Because we are interested only in working-age parolees, we eliminated eleven individuals who were paroled as minors, forty-three who were paroled as elderly, and 468 quarterly records in which individuals aged out of the labor market during the observed period (turned sixty-five before the first day of the quarter). We then omitted sixteen individuals who died during the first quarter of their parole as well as three who were discharged during that same time. Next, we dropped 2,684 quarterly records in which individuals were listed as deceased during the observed period. To handle the remaining records with missing data, we dropped thirty-one individuals (less than 0.3 percent) who had no values on one or more of the variables used in our analyses. These procedures left us with an unbalanced panel dataset of 199,503 quarterly records across 10,928 individuals.

We draw on other data as well. We retrieved county-specific monthly records from the Michigan Department of Labor and Economic Growth, calculated average quarterly unemployment rates, and then linked these to individuals based on the initial county of parole. Across the overall dataset, the mean county unemployment rate was 7.4 percent, the minimum at 2.8 percent and the maximum at 22.2 percent (standard deviation = 1.4 percent). Because we do not have detailed information on where individuals lived over time, we are forced to assume that our subjects did not move between counties or out of the state. We also integrated state-level minimum wage data by adding a variable that codes each quarter as either $5.15 (majority of time observed), $6.95 (beginning in the fourth quarter of 2006), or $7.15 (beginning in the third quarter of 2007).[4] Last, we examined aggregate Temporary Assistance for Needy Families (TANF) data on caseload and work participation maintained by the Office of Family Assistance (U.S. Department of Health and Human Services 2018). These data allowed us to compare employment rates among Michigan residents governed by the state's primary workfare program, TANF, with employment rates among parolees.

Variables

Our study uses three types of dependent variables. First, we constructed a dichotomous variable that captures whether an individual earned any money from formal employment in the quarter.[5] Second, we constructed five dichotomous variables that capture whether the individual was employed in each of the five most common industries observed in the data: employment services (which includes jobs assigned through temporary staffing agencies), manufacturing, construction, food services, and retail. Together, these five industries account for just over half of the employment records in the data. Third, we produced three related time-varying measures of post-prison recidivism: whether the individual was arrested in the quarter, reincarcerated in prison or jail in the quarter, or experienced either (or both) of these events.[6]

Our primary independent variable, which we

4. To capture quarter-specific changes, we relied on newspaper reporting of minimum wage law changes under Governor Granholm (Graboski 2006). We confirmed these changes with annual minimum wage rates as reported by the U.S. Department of Labor (2017).

5. We calculated an unadjusted employment rate by dividing the number of individuals who worked in a given quarter by the total number of individuals who are assumed to be "exposed" to the labor market during that period (that is, not reincarcerated, alive, and under sixty-five). Unlike the official employment rate, this method does not exclude from the denominator people who are not actively seeking employment.

6. Although the MDOC data reliably capture post-prison jail spells only for those under active parole supervision, it reliably captures prison spells for both parolees and those who have discharged from parole. To mitigate this potential for bias in jail reincarceration, we primarily compare employed parolees with jobless parolees in the recidivism models. We also assume that risks for both arrest and reincarceration depend on whether a formerly released person is on parole or discharged from parole. Parole violations probably substitute arrests for a significant number of parolees.

call *discharged*, codes each quarterly record as a period of either active parole or postparole discharge.[7] When examining the associations between discharged and our outcomes, we control for a lagged binary indicator capturing whether the individual experienced at least sixty days of the quarter in jail or prison. This ensures that comparisons between parole and discharge are focused on periods when the individual is in the community, without having to drop quarterly records when reincarceration is experienced, which would potentially introduce sample selection bias. Finally, our fixed-effects models also control for electronic monitoring, parole violation, county unemployment, season, and quarter since release. We lag most of our predictors to prevent problems with reverse casually in the fixed-effects modeling.[8] Table 1 provides descriptive statistics for all study variables (for time-invariant characteristics, see table A1).[9]

Analysis

Our goal is to estimate the association between being discharged from parole and the labor market and recidivism outcomes described above net of potential confounders. We deploy a series of hybrid or between-within fixed-effects logit models. Leveraging strengths of both traditional fixed effects and random effects, the hybrid approach decomposes time-variant predictors in a panel dataset into a within-person component and a between-person component (that is, person-specific means) while estimating a random-effects model to account for clustering of periods within individuals (Allison 2009). The intuition is that the person-level means on key variables control for all between-person variation in the predictor, allowing the coefficients on within-person variables to be interpreted as fixed-effects coefficients. This hybrid method is ideal because it does not, like a standard fixed-effects model, eliminate individual cases with zero variation in the outcome over time (for example, individuals who are never employed or who never recidivate). Per Stephen Vaisey and Andrew Miles (2017), we interact our person-level mean variables with our set of indicator variables for time (quarter since release) to relax the common trends (parallel trajectories) assumption of fixed-effects models. We also test the endogenous selection assumption by examining whether lagged outcomes predict discharge from parole and find that assumption is not violated in our data (Vaisey and Miles 2017).[10]

7. For all applicable individuals, the first quarter discharged is the first full quarter after the date of discharge.

8. Because we lag most independent and control variables, we ran analyses with and without the first quarter of parole. In all cases, the results were effectively the same.

9. Although family status and education level are probably not time-invariant in reality, our dataset does not account for changes over time with these variables so we are forced to assume they do not vary within individuals during the quarters observed. Given the relatively short period over which we observe our sample, this assumption does not seem unreasonable. Moreover, we do not control for age as a time-varying factor (except when dropping individuals once they turn sixty-five years of age) because of its high correlation with quarter since release.

10. Paul Allison (2009), typically credited with producing the definitive summary of the hybrid method for social scientists, suggests constructing the between-person component as deviations from the person-specific mean. However, later, in a defense of the hybrid approach for logistic regression, Allison (2014) demonstrates that it is unnecessary to construct the deviations in logistic models as long as the corresponding person mean variables are included. We do not include cluster-level means for spring, summer, or fall season indicators because these are simply control variables. We also do not include person means for the "will discharge" indicators because these are very highly correlated with person mean for lagged discharge. We also follow Allison's (2014) advice to test the linearity assumptions of the hybrid logistic model by examining whether controlling for squared and cubic terms on the person-mean variables changes the fixed-effect estimates. These controls do not change the estimated coefficients in the model, indicating that we can safely exclude them.

Table 1. Descriptive Statistics

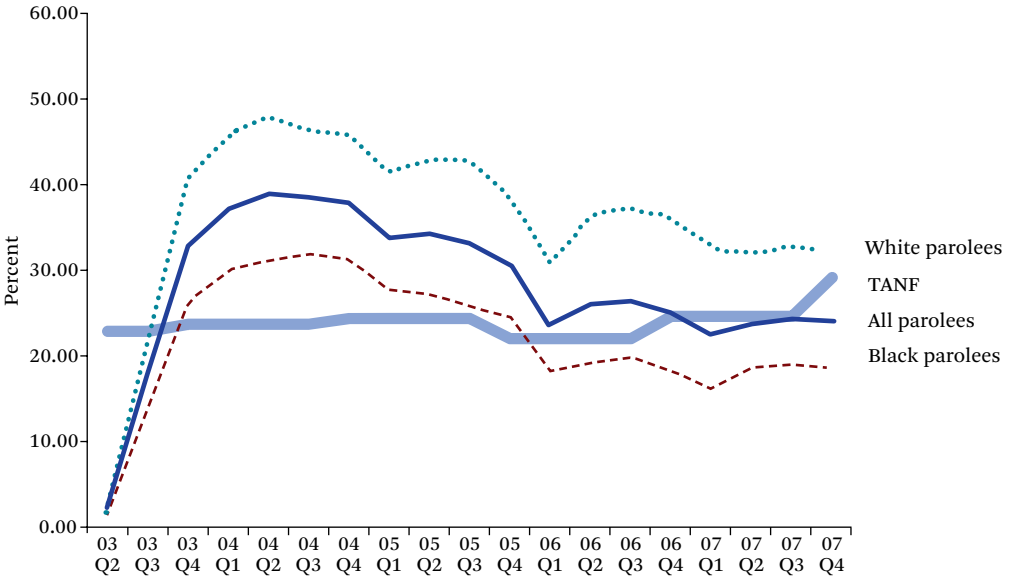
	Individual-Quarters	Individuals Ever Experiencing	Quarters Among Individuals Ever Experiencing
Dependent variables			
Employment models			
Employed	25.00	67.76	36.75
Employed in employment services	5.49	34.09	16.12
Employed in manufacturing	5.46	21.91	24.88
Employed in food industry	3.78	18.44	20.42
Employed in construction	2.57	12.55	20.24
Employed in retail	2.13	11.66	18.35
Recidivism models			
Arrested	8.57	69.20	12.32
Starting new reincarceration spell	8.23	62.88	13.09
Arrested or starting new reincarceration spell	13.11	78.70	16.61
Independent variables			
Employment models			
Discharged from parole, lagged	28.00	58.60	46.85
Recidivism models			
On parole with a job, lagged	14.49	62.17	23.35
On parole without a job, lagged	57.51	100.00	58.02
Discharged with a job, lagged	9.29	33.62	27.10
Discharged without a job, lagged	18.71	51.69	35.49
Controls			
Reincarcerated for sixty-plus days, lagged	18.18	47.12	38.20
Arrested, lagged[a]	8.12	67.87	11.88
On electronic monitoring, lagged	15.21	18.94	79.14
Absconded from parole, lagged	10.96	38.28	28.61
Winter season	22.96	99.71	23.03
Spring season	24.43	99.75	24.46
Summer season	25.67	99.73	25.74
Fall season	26.94	99.65	27.07
$5.15 State minimum wage	73.27	100.00	73.57
$6.95 State minimum wage	16.06	97.79	16.23
$7.15 State minimum wage	10.68	97.51	10.82

Source: Authors' calculations.

Note: All numbers in percentages. N = 10,928 individuals, 199,503 records. Omits individual-quarters in which subjects are deceased or over the age of sixty-five on the first day of the quarter. The first column of statistics covers individual-quarters, the second column covers differences across individuals, and the third column covers differences inside individual cases overtime. For example, 25 percent of the employed variable are coded as employed. However, 67.76 percent of individuals have ever held employment during the observed period. And, of those individuals, 36.75 percent of their quarterly records were coded as employed.

[a] For employment models only.

Figure 1. Parolee Employment Rate by Year-Quarter



Source: Authors' calculations.

Note: Omits individual-quarters in which subjects are deceased, over the age of sixty-five on first day of quarter, or discharged from parole. TANF rate captures cross-sectional unsubsidized employment rates for all adult participants in Michigan's TANF program between fiscal year 2003 and fiscal year 2008.

As with any fixed-effects approach, this hybrid model essentially treats every individual as their own control when estimating within-person effects. Time-invariant factors, whether included in the model or not, are thus controlled by the fixed effects. This means that there is no reason to include predictors that do not vary within individuals in the period observed. This is why we do not include controls for race, gender, criminal history, and other variables typically used to predict employment and recidivism among formerly incarcerated individuals. This approach may be much less vulnerable to omitted-variable bias than cross-sectional analyses or even a traditional random-effects model, but it is potentially vulnerable to unmeasured time-varying confounders. Certainly imperfect, this method offers a powerful test for whether parole affects employment outcomes and whether parolee labor affects the odds of recidivism.

RESULTS

We divide our results into three sections. First, we detail employment trends for parolees. We then consider how parole supervision might affect employment. Last, we examine the relationship between parolee employment and recidivism.

EMPLOYMENT ON PAROLE

Figure 1 shows the proportion of parolees who are employed in the formal labor market between the second quarter of 2003 and the fourth quarter of 2007, including only quarters in which an individual is on parole and not reincarcerated for sixty or more days. This figure shows that parole in Michigan supervises individuals with a generally low employment rate, but a nontrivial portion of the sample experiences some employment. Averaged across these periods, the overall employment rate is 28 percent. Quarterly employment for this cohort begins at 2 percent in the second quarter of 2003, rises to nearly 39 percent in the second quarter of 2004, and then drops to hover around 25 percent until the end of the observation period. This temporal pattern is consistent with prior studies of formal employment among formerly imprisoned individuals (Bushway, Stoll,

and Weiman 2007). African Americans have consistently lower post-prison employment than whites, although the over-time pattern is similar for both groups. Employment peaks in the second quarter of 2004 for whites at just under 50 percent. Among blacks, employment peaks in the third quarter of 2004 at just over 30 percent.

We conclude that Michigan parole manages a mostly but not entirely jobless population. In other words, parole governs a population primarily detached from wage labor. In fact, this indicates even more exclusion than what Simon (1993, 147) reports for his California parolees. He estimates that only 43.3 percent of parolees receive income from work.

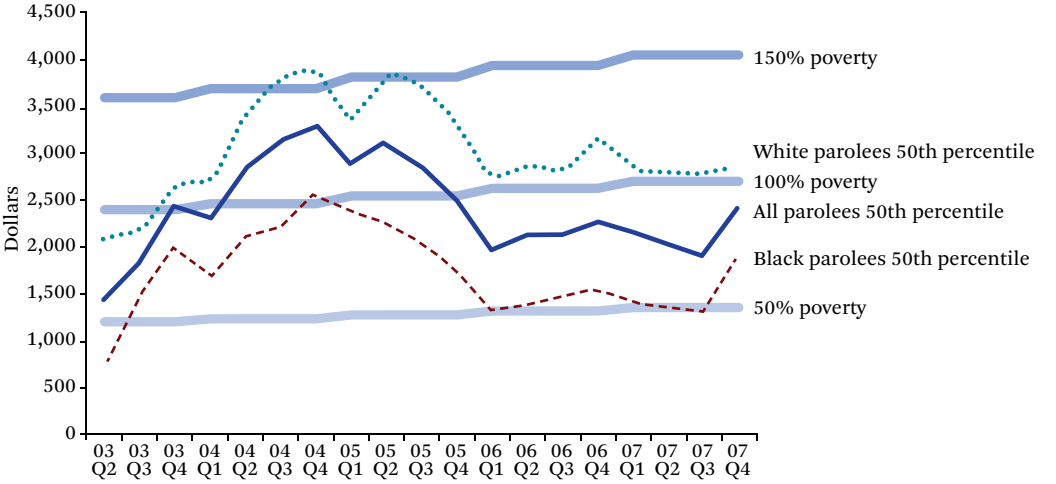
Still, we should not just write off parole as just another penal institution for managing labor market outcasts. Evidence also indicates that parole is a bit like workfare in that it actively supervises a number of formally employed subjects. There is even reason to believe that people are more likely to work while on parole than before they went to prison. A separate publication drawing on the same data estimates that only 14 percent of the individuals in our study were employed in the year before the imprisonment spell that preceded their 2003 parole (Herbert, Morenoff, and Harding 2015).

A useful, albeit imperfect, comparison can also be made with Michigan's adult TANF recipients, a local population under the prototypical American workfare regime. Drawing on data from the U.S. Department of Health and Human Services, we divided the number of adult TANF recipients recorded as holding unsubsidized employment by the total number of adult TANF recipients during the same general period.[11] This provides a reasonable basis for comparing workfare and parole. Like the employment rate in our parolee sample, this does not account for adults who may replace their traditional work mandate with other activities (such as college courses and job search assistance programs). Drawing on these data, figure 1 plots the employment rate among Michigan TANF recipients, showing a generally similar rate of employment between parolees and TANF recipients. The average employment rate of 24 percent for TANF recipients in Michigan is only slightly lower than what is observed for the cohort of parolees over a similar period (28 percent). To be clear, we understand this is bit like comparing apples and oranges. TANF employment requirements can vary between single and two-parent families and the TANF data we draw on are cross-sectional. Moreover, TANF recipients may voluntarily exit the program, effectively stopping the clock on their five-year standard limit for enrollment, and then return later if eligible. Parole obviously does not operate this way. Nevertheless, in terms of the simple share of active subjects who hold formal employment, Michigan's parole and workfare programs are seemingly similar.

Although wage and benefit details are unavailable for the TANF data we retrieved, sociologists often note that the program fails to lift its subjects out of poverty, at least significantly or for long periods (Collins and Mayer 2010; Hays 2003; Soss, Fording, and Schram 2011). The same may be true for parole. This regime supervises a population that frequently, if not primarily, earns an income through the formal labor market. However, as seen in figure 2, which plots quarterly earnings from formal employment by race, these payments are low. The

11. This measure of employment among TANF recipients is a more appropriate comparison for the parole data than the commonly reported "employment participation rate," which can span an array of work activities (such as unpaid job training, employment searching, and select college programming) and typically depends on a minimum of thirty hours of such activities a week (Lower-Basch 2018). Because TANF recipients can combine work activities, we only examine unsubsidized employment when calculating an employment rate. On average, less than 0.08 percent of adult TANF recipients worked a subsidized job (either in a "public" or a "private" position) during the years we analyze. For the denominator, we use total number of adult TANF recipients rather than the total number of work-eligible individuals because we have no way of determining work eligibility among parolees.

Figure 2. Wage Profile of Employed Parolees by Year-Quarter



Source: Authors' calculations.

Note: Omits individual-quarters in which subjects are deceased, over the age of sixty-five on first day of quarter, discharged from parole, or jobless. Unadjusted dollar amounts and year-specific poverty thresholds.

average median earnings for all parolees during the observed period is $2,398 per quarter (about $184 per week). This peaks at $3,273 during the fourth quarter of 2004. Despite the clear racial disparity in earnings, the median earnings pattern over time follows a similar trend for both black and white parolees, the latter often earning about double what the former earn. Plotting the federal poverty line for a single adult, which begins at $2,393 per quarter in 2003 and increases to $2,697 per quarter in 2007, provides useful insight, as do the thresholds for 50 percent and 150 percent poverty. This suggests that black parolees tend to earn wages below the poverty line across most quarters, and that their white counterparts tend to earn below 150 percent of the poverty line. Black or white, the wages earned through employment while under parole are typically low.

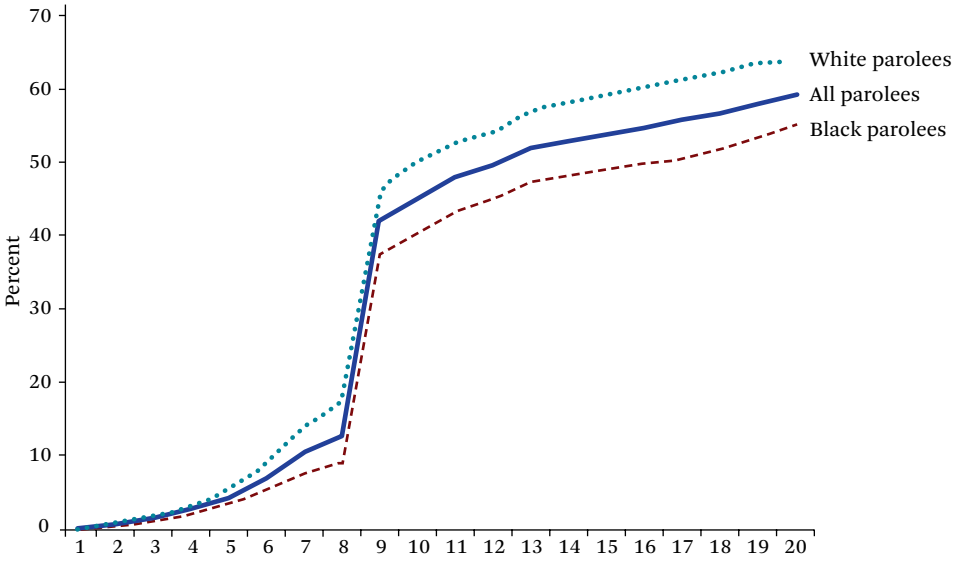
In sum, our examination of employment while on parole shows that this institution manages a mostly jobless population. However, it also supervises people who work at a rate comparable to those governed by the local workfare regime. Moreover, when parolees do work, most earn little. It is unclear whether this is due to low-wage, part-time employment, sporadic work, or some combination of these conditions, but most working parolees garner earnings near the federal poverty level.

The Employment Effects of Parole

We now turn to the association between parole and employment. We use discharged to distinguish periods of parole from periods of nonparole. Should parole discharge status be negatively associated with employment within persons when controlling for other pertinent time-varying factors, then we have evidence that parole increases the odds of employment.

Figure 3 graphs the share of individuals who have discharged from parole over time since release from prison. The proportion of subjects who have discharged from parole gradually increases over time until the ninth quarter, when it jumps dramatically from 12 percent to 42 percent. This dramatic jump occurs after two years on parole, a standard period of parole in Michigan during the period covered by our study. This jump is important for our ability to assess the effect of parole discharge because it indicates that a substantial portion of the variation in discharge status from parole over time is determined by the rules of parole rather than by

Figure 3. Parole Discharge Rate by Quarter Since Start of 2003 Parole



Source: Authors' calculations.

Note: Omits individual-quarters in which subjects are deceased or over the age of sixty-five on first day of quarter.

individual idiosyncrasies that might change over time. Following this dramatic jump, the probability of being discharged from parole increases gradually again through the end of our observation period, ending at about 60 percent of individuals.

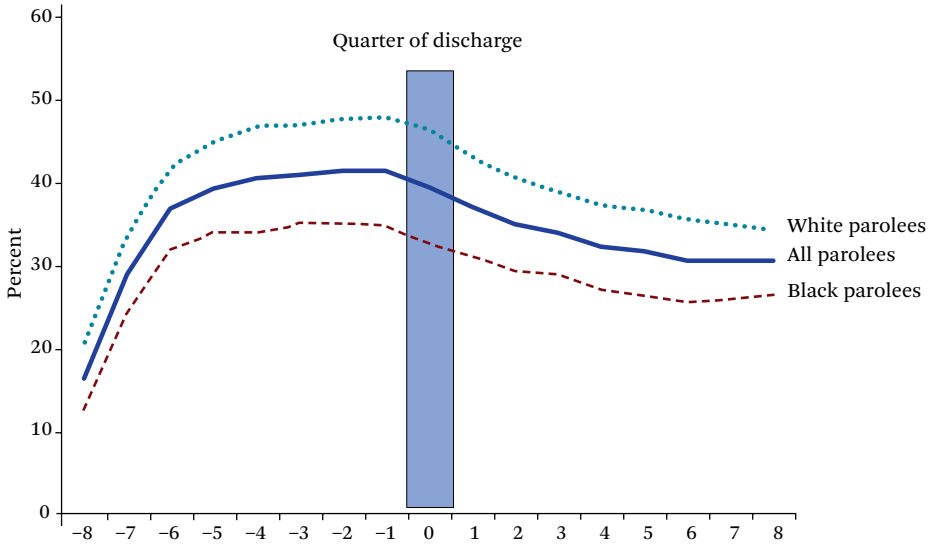
Figure 4 examines the bivariate relationship between parole discharge status and employment by plotting employment rates over time relative to the discharge quarter among all individuals who eventually discharge during our observation period. Quarter zero is the individual's discharge quarter, negative quarters are before discharge, and positive quarters are after discharge. This graph shows a clear pattern of employment relative to the timing of discharge. Employment peaks in the quarters immediately before discharge and declines after that. Parolees experience maximum employment just before their discharge and then experience decreased participation in formal labor after their parole ends. This suggests that parole may increase employment.

To more rigorously examine the relationship between parole and employment, we now turn to the fixed-effects logit models with controls for the time-varying predictors discussed in the methodology section. Table 2 summarizes two fixed-effects models predicting any formal employment in the quarter.[12] These models answer our first research question: how does parole affect employment outcomes among formerly incarcerated individuals?

Model 1 estimates that discharging from parole lowers the odds of employment by more than 35 percent (odds ratio = 0.644). In other words, parole seems to increase employment. The average marginal effect for the lagged discharged variable in this model suggests that when the average parolee discharges, their probability of employment drops by five percentage points. Model 2 adds indicators for pe-

12. Given the difficulties in comparing coefficients across logit models (Mood 2010; Karlson, Holm, and Breen 2012), we caution against comparing coefficients across models. Instead, tables 2 and 3 should be understood as capturing seven indicators of the relationship between parole discharge and employment. For the full versions of the models, see the online appendix, available at: https://www.rsfjournal.org/content/6/1/173/tab-supplemental.

Figure 4. Employment Rates of Discharging Parolees, by Quarterly Distance to Discharge



Source: Authors' calculations.
Note: Limited to individuals who discharged form parole during the observed period. Omits individual-quarters in which subjects are deceased or over the age of sixty-five on first day of quarter. Quarter 0 corresponds to individual-specific date of discharge.

Table 2. Any Employment, Fixed-Effects Logit Models

	Model 1	Model 2
Discharge, lagged	0.644***	0.679***
	(0.02)	(0.03)
Will discharge in one quarter		1.105*
		(0.05)
Will discharge in two quarters		1.122*
		(0.05)
Will discharge in three quarters		1.091
		(0.05)

Source: Authors' calculations.
Note: Odds ratios and standard errors, 10,928 individuals, 199,503 records. All models include controls for reincarceration, arrest, electronic monitoring, absconding, county-level unemployment, seasons, and quarters. Full models reported in the online appendix.
*$p < .05$; **$p < .01$; ***$p < .001$

riods one, two, and three quarters before discharge. The odds ratios for each of these quarter indicators are higher than one and the first two are statistically significant, indicating that employment increases in the quarters leading up to discharge, which is consistent with figure 4.

The models in table 3 replicate the second model in table 2, except that they limit the outcome to quarterly employment in one of the five most common industries in our data. These models show that the patterns documented in table 2 are not driven by employment in particular industries. Instead, we see that parole

Table 3. Any Employment by Industry, Fixed-Effects Logit Models

	Model 1 Employment Services	Model 2 Manu-facturing	Model 3 Food Services	Model 4 Construction	Model 5 Retail
Discharge, lagged	0.647***	0.667***	0.812*	0.853	0.678***
	(0.04)	(0.05)	(0.07)	(0.08)	(0.07)
Will discharge in one quarter	0.956	0.972	1.300**	1.070	0.999
	(0.07)	(0.08)	(0.12)	(0.12)	(0.12)
Will discharge in two quarters	1.002	0.992	1.236*	1.173	0.962
	(0.07)	(0.08)	(0.11)	(0.12)	(0.11)
Will discharge in three quarters	1.058	1.028	1.299**	1.143	0.846
	(0.07)	(0.08)	(0.11)	(0.12)	(0.10)

Source: Authors' calculations.

Note: Odds ratios and standard errors, 10,928 individuals, 199,503 records. All models include controls for reincarceration, arrest, electronic monitoring, absconding, county-level unemployment, seasons, and quarters. Full models reported in the online appendix.

*$p < .05$; **$p < .01$; ***$p < .001$

discharge reduces the probability of employment in all five industries (although the coefficient is not significant in model 4 for construction). Increased employment in the quarters immediately before discharge is most clearly evident for the food services industry (model 3) and does not appear to be occurring in retail.

In sum, although we find evidence that parole governs a mostly jobless population, we also find evidence suggesting that it increases employment among its subjects.

The Recidivism Effects of Parolee Labor

Parolee labor may not be a convincing program for combating poverty, but this is not the official goal of parole in the same way it is for workfare. Representatives from the latter regime often explicitly, although perhaps not very seriously, insist that they aim to lift their subjects out of poverty by promoting personal responsibility. Parole authorities, on the other hand, usually claim they want to promote personal responsibility for other ends, namely, for "offender reintegration" and "public safety." Reading between the lines, this seems to more accurately mean the separation of the offender from penal custody and the promotion not of an individual economic security but of a public security from activities deemed criminal. Parolee employment may yield these ends by reducing recidivism.

In considering whether this is the case in our data, we investigate the relationship between parolee employment and recidivism. Table 4 shows fixed-effects logit models in which the primary predictors of interest are based on a four-category variable that combines discharge status and employment. Its categories are on parole and jobless (the reference category), on parole and employed, discharged and jobless, and discharged and employed. As we did with the independent variable in the employment models, we lag this variable to avoid potential reverse causality in quarters in which arrest or reincarceration precede job loss.

If parolee employment helps prevent future criminal justice contact, we would expect that being on parole and employed (relative to being on parole and jobless) would be associated with lower odds of arrest or reincarceration. This does not seem to be the case, given that being employed while on parole is associated with greater odds of arrest (model 1) and no difference in the odds of reincarceration (model 2). In addition, the large odds ratios for discharged and employed and for discharged and jobless in model 1 indicate that discharge status itself is associated with greater odds of arrest, whereas the odds ratios below one on these

Table 4. Recidivism Outcomes, Fixed-Effects Logit Models

(Reference = On Parole and Jobless)	Model 1 Arrest	Model 2 Reincarceration	Model 3 Arrest or Reincarceration
On parole and employed, lagged	1.095**	1.048	1.069*
	(0.03)	(0.03)	(0.03)
Discharged and employed, lagged	2.111***	0.248***	1.492***
	(0.12)	(0.03)	(0.08)
Discharged and jobless, lagged	2.064***	0.392***	1.529***
	(0.09)	(0.03)	(0.06)

Source: Authors' calculations.
Note: Odds ratios and standard errors, 10,928 individuals, 199,503 records. All models include controls for reincarceration, electronic monitoring, absconding, county-level unemployment, seasons, and quarters. Full models reported in the online appendix.
*$p < .05$; **$p < .01$; ***$p < .001$

variables in model 2 indicate that discharged is associated with lower odds of being placed in jail or prison. We interpret these estimates as consistent with the idea that the greater surveillance of parole supervision enhances the risk of being reincarcerated for parole violations while also substituting parole violations for some arrests by police.[13]

It is worth emphasizing that our results suggest that parole supervision is related to recidivism in complicated ways. Among employed subjects, being on parole seems to lower the odds of arrest (model 1) and increase the odds of reincarceration (model 2). Among those who are not employed, the effects are about the same. We assume that this discrepancy is largely explained by technical violations. Whether that is or is not the case, we find some evidence that parole reduces recidivism. This is also apparent in the third model, which collapses arrest and reincarceration into a single outcome. This finding is not unique (Ostermann 2013, 2015). We are, however, not particularly interested in how parole may or may not affect recidivism. Instead, we are interested in how working while under parole supervision might affect the odds of recidivism. Our findings clearly challenge the assumption that parolees who work in the formal economy are less likely to recidivate than their jobless counterparts. If parole lowers the odds of recidivism (and that is not demonstrated beyond question in this study anyway), we are at least skeptical that employment explains this effect.

In sum, we find evidence that parole's successful promotion of labor does not challenge the odds of post-prison recidivism, at least as indicated by the odds of being arrested or reincarcerated. Just as increasing the employment of welfare recipients does not seem to signifi-

13. To assess whether employment lowers recidivism after parole discharge, we tested the equivalency of the coefficients for discharged and employed and discharged and jobless for each recidivism model. These coefficients are not significantly different from one another in the arrest model ($p = .632$) but are significantly different from one another in the reincarceration model ($p = .001$). These coefficients are also not significantly different from one another in the arrest or reincarceration model ($p = .593$). Together, these tests suggest that being employed after discharge, versus being jobless after discharge, does not reduce the odds of arrest net of other time-varying confounders. In fact, the odds of arrest are positively associated with employment after discharge, albeit slightly. However, when recidivism is measured as either arrest or reincarceration, employment after discharge is associated with reduced odds of recidivism. Thus, to the extent that labor challenges recidivism at all, it seems to only do so after parole discharge (after the odds of any employment drops somewhat dramatically anyway). This may reflect a better average job quality for employment held after discharge.

cantly challenge their economic insecurity, parolee labor does not seem to facilitate offender reintegration or public safety.

DISCUSSION AND CONCLUSION

Our findings motivate a reconceptualization of parole as an institution that blends the poverty regulating strategies of prisonfare and workfare (Wacquant 2009). First, parole seems to regulate labor in a way similar to prisonfare, by controlling a population largely detached from the labor market. When averaged across all observed quarters, only about 28 percent of the 2003 cohort of formerly imprisoned people in Michigan earned wages from formal labor while under parole supervision. This is powerful evidence that supports Simon's (1993) vision of managerial parole as a regime that largely governs people excluded from labor.

Second, parole seems to regulate labor in a way similar to workfare. The comparison is imperfect, but the average unadjusted employment rate for Michigan parolees is not unlike a similar statistic for adult TANF recipients in the same state around the same period. Extending the possibility that parole maintains parallels with workfare as Soss, Fording, and Schram (2011) describe, we predicted that post-prison supervision would impel formerly imprisoned people to work. Our study yielded a novel finding that supports this hypothesis. Fixed-effects models showed that formerly incarcerated individuals have greater odds of employment while under parole than after they discharge from this supervision. This pattern conflicts with common assumptions that parole impedes employment. Additionally, the effect of parole on employment is not limited to just one or two industries.

But though it is similar to both prisonfare and workfare, we argue that parolefare is something distinct. On the one hand, post-prison supervision is the usual sequel to imprisonment and part and parcel of an expansive penal state that handles a mostly impoverished male population. Still, parolefare cannot be written off as yet another prisonfare institution for simply managing labor market outcasts. Parolefare not only supervises a substantial number of workers, it also impels employment.

On the other hand, this does not mean that parolefare is just workfare under a different name or for a different gender. Indeed, these regimes are perhaps permanently separated by two major conditions. First, parolefare, like prisonfare but unlike workfare, operates on official platforms of public safety and offender reintegration. Narratives of work first and poverty alleviation are peripheral (if not nonexistent) for parolefare but essential for workfare. Second, although both parolefare and workfare rely on carrots and sticks, they almost definitely do so in different proportions. Whereas workfare draws in clientele because of the rewards it offers to obedient subjects (for example, cash benefits), one of parole's primary incentive devices is the threat of punishment (that is, reincarceration).

Limitations

Our theorization is ambitious, but our study is not without notable weaknesses. We examined only one cohort in one state more than a decade ago and are therefore reasonably concerned about external validity. And, although fixed-effects modeling controls for time-invariant factors, it assumes all relevant time-varying confounders are controlled in the model. We are potentially exposed to omitted-variable bias, including individual-level changes in residence, family status, supervision level, informal and supplemental income, and health. These shortfalls are not insignificant, but such omitted variables would need to change values at the same time as parole discharge to seriously challenge our conclusions.

Next is the issue of measurement. Our data limit us to a discussion of *formal* employment. Previous scholarship suggests a likely discrepancy between the unemployment insurance covered records (or paystub jobs) we examine and the self-reported employment of young men with histories of criminal justice involvement (Kornfeld and Bloom 1999). However, we assume that, like workfare recipients, the subjects of parole are pressured to demonstrate their participation in so-called legitimate labor. We also assume that parole would not, or at least would be much less likely to, impel informal labor. Unfortunately, we have no way of testing these assumptions.

Similarly, we recognize that life chances are

imperfectly, or at least incompletely, captured by our recidivism models. We assume that arrest and reincarceration tend to sever a former prisoner's life chances more than extend or protect them. Still, the case might be made that we would have been better off examining different life chance outcomes like mortality or morbidity. We do not necessarily disagree. However, we believe recidivism makes the most sense for an examination of parolee employment because it is an official policy focus of this institution and is frequently cited as a justification for the employment mandate. Even so, recidivism is not the only outcome of parolee employment we consider. We also examine quarterly earnings and show that they tend to hover around the federal poverty line, where life chances are typically low.

We also understand the obvious mismatch in our causal theory and our descriptive data. However, without any data accounting for the random assignment of parole discharge (or, better yet, parole admission) we are restricted to the types of analyses we performed. Ultimately, we do not test a parolefare theory, but instead develop and support it. We nevertheless think the parolefare framework we advance—that being the simple claim that parole impels some employment for a population still largely excluded from the labor market but that it does so without considerably extending their life chances—is useful.

This framework certainly faces some alternative explanations, especially for the observed patterns in employment. It is possible that employment participation dropped for our subjects as some sort of nascent effect of a worsening economy. However, we think this is unlikely due to both our controls for county-level unemployment rates and the fact that we limit our dataset to quarters before the Great Recession. Another possibility is that formerly imprisoned people experience a deterioration in hope and motivation that is independent of supervision status. According to this logic, by the time they reach parole discharge, people are so exhausted or defeated that they abandon the labor market, but not because they are excused from supervision. The employment drop after discharge would just be a coincidence. However, we doubt that best explains our findings because such discouragement is unlikely to be experienced by different individuals at the same time. Indeed, neither of these alternative explanations can account for the simple, and somewhat dramatic, drop in average employment at the moment of discharge, which varies significantly by individual subject (figure 4).

Far more concerning is our inability to speak to the mechanisms of parolefare. How, and under what particular conditions, parole impels the kinds of formal employment that do little to extend life chances remains a mystery. We can only speculate. Perhaps parolees feel compelled to work in the formal economy to simply satisfy their supervision conditions but then they ditch these likely mentally and materially underwhelming jobs following discharge. However, if true, that is probably only part of the story. The mere presence of a formal rule is likely insufficient. Parole officers probably play a critical role. They may surveil their unemployed subjects more intensively than their employed subjects, which may motivate some parolees to take any job they can find with clear plans to quit after discharge. Also, in executing their social work duties, parole officers may successfully broker their clientele to third-party services that increase employability (such as job search programs). Parolefare may also be mixed into their policing duties, and these officers may significantly impel labor with the threat of reincarceration. It is also possible the mechanisms are less direct. Maybe in imposing restrictions—such as in where to live, who to interact with, and how to behave—parole promotes an increase in personal stability that somehow makes a formerly imprisoned person more employable. Following this logic, it is possible that a kind of Durkheimian anomie follows a parole discharge. A sense of normlessness may spring from discharge and this might somehow promote joblessness and further instability. It is also possible that mandated employment does not fundamentally alter one's sense of self. There might be particular frustrations associated with more "forced" employment and this might somehow be associated with criminal activity. We simply do not know, but all of these imagined mechanisms are consistent with the broad vision of parolefare we

develop in this article.[14] In the end, we hope that future research explores these possibilities in a deeper analysis of parole operations and the effects of parolee labor.

We also hope that future research moves beyond parole and considers how other forms of community corrections might complicate the proposed framework. The obvious alternative is the much larger institution of probation, for which we do not have data. Probation is typically imposed as an alternative to imprisonment and probation officers usually manage a larger case load than parole officers. We assume that our findings are most generalizable to supervision regimes that include explicit employment mandates, intensive monitoring, and similar sanctions (such as reduced check-in requirements for employed subjects). This assumption, however, should be put to the test. Just as we hope to see more considerations of parolefare outside of Michigan, we hope to also see inquiries into what might be called probationfare. For now, we can only surmise interesting, but understudied, similarities and differences between these regimes of poverty governance.

POLICY IMPLICATIONS

This article fuels an ongoing policy debate regarding the benefits of formerly imprisoned people's employment. We struggle to identify the virtues of parolee labor. It does not appear to provide a pathway out of poverty nor does it convincingly reduce the odds of arrest or reincarceration in our data. We therefore find it difficult to recommend the policies and programs that seek to commodify post-prison labor. We are not alone either. Christy Visher, Laura Winterfield, and Mark Coggeshall (2005), in what arguably stands as the most comprehensive meta-analyses of employment programming for ex-offenders (not just formerly imprisoned people), conclude that such interventions do not affect the likelihoods of recidivism (see also Bohmert and Duwe 2011; Jacobs 2012; Moses 2012; Turner and Petersilia 1996). Despite the reasonable defenses of employment-focused programming post-incarceration (Bushway and Apel 2012; Drake, Aos, and Miller 2009; Redcross et al. 2012; Solomon et al. 2004; Uggen 2000), we add to more pessimistic conclusions.

Our vision of parolefare also has the potential to clarify and critique the policy discourse regarding employment and criminal justice contact. In this regard, we offer a timely contribution. Lawrence Mead (2007), a notorious advocate for mandating welfare mothers to work in the 1990s, has more recently turned his attention to formerly imprisoned men. He insists that the latter suffer from a similar "breakdown in work discipline," and he proposes a plan that first coerces jobless parolees into programs that broker low-wage employment before shoving more incompliant subjects into forced work crews (Mead 2007, 61). Although his proposal rubs against our findings as well as the scholarship showing that participation in the secondary labor market does little to promote criminal desistance (Crutchfield 2014; see also Schnepel 2018; Uggen 1999), such a labor the-parolee strategy seems to appeal to the convenient assumption that employment is the key to successful reentry.

Moreover, a work-first slogan orients many discussions of criminal justice policy, making our framework relevant beyond just a consideration of parole. For example, in New York, the mayor has introduced a Jail to Jobs program that promises transitional job opportunities to everyone leaving city jails (City of New York 2017). The mayor's office claims that such short-term (and presumably low-wage) employment can reduce recidivism by 22 percent. However, the evidence for this claim relies on an evalua-

14. Somewhat related to the issue of mechanism, we are not convinced that parolefare necessitates an instrumentalist vision of parole. Our insistence that parole helps commodify post-prison labor does not mean we are characterizing parole simply as an institution for serving businesses with cheap labor power in the interest of promoting capital accumulation. In fact, we think it is more plausible that parole is not a simple instrument of a ruling class but instead a relatively autonomous institution equipped with distinct logics for motivating worker-citizenship. It seems more likely that parole officers and their managers believe that post-prison labor promotes public safety and offender reintegration. Indeed, this seems to be the common sense opinion reinforced by scholars, elected officials, reentry-focused nonprofit leaders, and other correctional personnel.

tion of an employment-based reentry program that actually produced more complicated conclusions (Redcross et al. 2012). Three years following their engagement in the work crew–focused program, participants were less likely to recidivate but not more likely to hold formal employment, leading the study's authors to admit that the recidivism effects were puzzling (Zweig, Yahner, and Redcross 2010, 14). How else could a jobs-focused program for returning prisoners reduce recidivism if not by increasing their employment? Adding to the confusion, many of the same researchers evaluated another transitional jobs program—though one less likely to also mix in intensive reentry support and coaching—and found no recidivism effects despite also undergoing a rigorous case-control experimental evaluation (Redcross et al. 2010; see also Bushway and Apel 2012).

Our broad vision of parolefare can help us make sense of these empirical patterns and the often-mismatched policy prescriptions that follow. Pipelining formerly incarcerated people into precarious labor does not seem to yield especially promising challenges to recidivism. Still, like the large-scale implementation of workfare programming in the 1990s, an any-job-will-do mantra appeals to conventional values. Such a framework is also easily absorbed into a rationale of reentry that emphasizes personal responsibility (Seim 2016, 452–53; see also Abrams and Lea 2016; Miller 2014). Parolefare, we argue, is part and parcel of this rationality.

To be clear, we do not imply that parolefare would be justified if other researchers found evidence that low-wage work reduces the odds of arrest or reincarceration. In thinking about parole not just as criminal governance but also as poverty governance, we would be more impressed with research showing how post-prison labor can significantly reduce individual poverty for long periods. On a related note, we do not argue that work is automatically bad for formerly incarcerated people. We expect quality employment to be a crucial factor in directing people away from both routine criminal justice contact and material deprivation. Indeed, as seen in another article in this volume, Joe LaBriola (2020) examines the same dataset and finds that formerly incarcerated people who work in select industries with higher wages, longer tenure, and more unionization are less likely to be arrested or reincarcerated. Unfortunately, very few individuals in the dataset find work in such industries, and we have little reason to believe that parole does much to increases the odds of higher-quality employment. In the end, we are simply not convinced that the types of labor typically impelled by parole, and now increasingly by employment-focused reentry programing, constitutes the best course of action if the goal is to significantly extend the life chances of those caught in American criminal justice.

In closing, parole supervises a population generally, but not totally, excluded from labor. We find evidence that it also impels its subjects into formal employment. The real novelty of our study lies in our discovery that when someone discharges from parole his or her odds of employment are lowered. This association is significant across multiple industries. We also challenge assumptions that parolee labor is automatically "good" by questioning its ability to offer a livable income and reduce recidivism. Parolefare, a concept engineered for the findings presented and motivated by extant theory, helps us make sense of these otherwise perplexing results. We believe this concept has implications for a number of academic and policy discussions regarding employment after prison.

Table A1. Baseline Demographic Profile of Working-Age Parolees, 2003 Cohort

Age, race, and gender	
Age at baseline (mean)	35.02
SD	9.41
Min	18.00
Max	64.92
Black (percent)	53.44
White (percent)	44.75
Other race (percent)	1.81
Male (percent)	92.26
Education and employment	
High school dropout or GED recipient (percent)	72.75
High school or more (percent)	26.13
Unknown education (percent)	1.12
Ever employed before 2003 parole (percent)	62.17
Family	
Any dependents (percent)	59.58
Never married (percent)	66.48
Divorced or separated (percent)	20.19
Married (percent)	12.26
Unknown or other marital status (percent)	1.07
Judicial history	
Non-assault convictions (percent)[a]	45.85
Assault conviction (percent)[a]	28.49
Drug convictions (percent)[a]	25.66
Number of pre-2003 prison terms (mean)	1.46
SD	1.94
Min	0
Max	32
Length of prison term leading to 2003 parole (mean years)	2.96
SD	3.20
Min	0.16
Max	31.32

Source: Authors' calculations.
Note: N = 10,928. Omits individual-quarters in which subjects are deceased or over the age of sixty-five on first day of quarter.
[a] Mutually exclusive categories capturing most serious conviction (in terms of maximum sentence under state law) leading to prison sentence ending with 2003 parole.

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