Understanding Criminal Record Penalties in the Labor Market

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Abstract

This paper studies the earnings and employment penalties associated with criminal records using linked data on criminal justice interactions, state laws on formal labor market restrictions, and wages and employment. We estimate two-way fixed effects models that decompose earnings into a worker's portable earnings potential and firm pay premia, both of which are allowed to shift after a worker acquires a record. We find that sorting into lower-paying firms explains a small share of the record penalty. Legal record restrictions are not concentrated in higher-paying jobs, meaning that sorting away from them contributes little to the earnings gap. Conditional on earnings potential, however, workers with a record are substantially less likely to be employed. The results suggest that criminal record penalties operate primarily by reducing employment and lowering earnings potential at every firm rather than increasing sorting into lower-paying jobs, although the bulk of the gaps can be attributed to differences that existed prior to acquiring a record.

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Workers with a criminal record have significantly lower rates of employment and earnings than workers without a record (Western, 2002). These gaps reflect barriers workers with records face in the labor market. Simply having a record, for example, legally precludes work in some occupations.[1] Even if not specifically prohibited from hiring them, employers may be reluctant to take on workers with a criminal history due to concerns such as perceived liability risk (Holzer, Raphael and Stoll, 2006; Cullen, Dobbie and Hoffman, 2023). Correspondence studies have consistently shown sizable callback penalties for disclosing a criminal record on job applications, even in entry-level positions paying near the minimum wage in retail and fast food (Pager, 2003; Pager, Bonikowski and Western, 2009; Uggen et al., 2014; Agan and Starr, 2018).

Although there is clear evidence that such labor market barriers exist, how they shape outcomes for workers with records is less certain. Workers with a record frequently face a host of other challenges beyond their criminal history, including limited formal education and prior labor market experience. Roughly 65% of the incarcerated population did not finish 12th grade, for example (Harlow, 2003).[2] These disadvantages may push workers with records into lower paying jobs regardless of the landscape of legal and de-facto labor market restrictions they face. In addition, whether restrictions disproportionately affect opportunities in relatively high-paying jobs (e.g., as nurses) compared to lower-paying ones (e.g., as a fast food crewmember) remains an open question.

This paper studies the sources of labor market disadvantage for workers with criminal records. Our analysis uses a newly constructed dataset linking together state administrative criminal justice records, information on legal employment constraints for workers with records, and earnings and employment data at the Census Bureau. The combined data allows us to reliably measure adult criminal justice interactions for more than eight million workers, identify the job categories where they face the most stringent restrictions on employment opportunities, and track employment across both firms and occupations. We use this information to measure how labor market sorting contributes to criminal record wage penalties and its connection to formal labor market constraints.

Our analysis makes several contributions. First, we isolate the role of between-firm sorting

[1]For example, individuals with felony drug, weapons, property or other convictions are barred from serving as a TSA agent (49 U.S.C. §44936); convictions for "any criminal offense involving dishonesty or a breach" can be disqualifying for positions at FDIC-insured institutions (12 U.S.C. §1829); drug and other convictions can also be disqualifying at any federally funded facility serving children under 18 (34 U.S.C. §20351). State-specific laws specify many other exclusions, including in licensed occupations (The National Conference of State Legislatures, 2024).

[2]Approximately 18% of the overall population 18 and older did not complete 12th grade at the time this statistic was computed.

in explaining the sizable wage gap between workers with and without a record, which stands at roughly 24% among those employed full time in our sample. To do so, we estimate variations on two-way fixed effect models of earnings pioneered by Abowd, Kramarz and Margolis (1999) (hereinafter AKM). Our models allow log earnings to depend on a person-specific effect and a firm effect, both of which are interacted with criminal record status. This simple framework allows log earnings to decline after a conviction through sorting into lower-paying of jobs (e.g., more low-paid retail work), declines in pay premia at the same firm (e.g., due to discrimination), and drops in person effects (e.g., due to productivity declines). This framework also provides a simple way to decompose log earnings gaps between groups, building on the analyses in Card, Cardoso and Kline (2016), Gerard et al. (2021), and Beauregard et al. (2025), among others.

The estimates show that firms play an important role in explaining wage dispersion for each group. Firm effects account for a substantial share of wage variation for workers with a record (21.5%)—if anything, a larger share than for workers without one—indicating that firm-level heterogeneity is an important component of wage determination for this population. We also find no evidence of meaningful dispersion in within-firm gaps in pay premia between workers with and without a record once we correct for sampling error, suggesting that any pay penalty workers with a record face at a given firm is constant across firms.

Although firms are important for earnings dispersion within groups, they explain a small share of wage differences between groups. Workers with a record are disproportionately employed at firms with low paid non-record coworkers, but these coworkers tend to earn lower wages regardless of where they work. As a result, sorting across the firm pay premia distribution explains just 0.027 log points, or 11%, of the total gap. Similar findings emerge when we re-weight the non-record sample to match the record sample's demographic, geography, industry, or occupation distribution, implying that sorting effects are a persistent but relatively small component of gaps even among similar workers in similar sectors and places. In fact, between-industry sorting contributes approximately zero to average gaps because, although workers with a record are less likely to be in high-paying industries like finance or professional services, they are more likely to be employed in relatively high-paying sectors such as manufacturing and construction.

Rather than sorting, the majority of the gap reflects differences in workers' earnings potential prior to acquiring a record. Pre-record person effects explain 66% of gaps, consistent with the large differences in formal education levels between workers with and without a record. The remaining 23% of gaps reflects declines in earnings potential within firms post-conviction. This drop appears to vary little across firms, indicating that workers with a record tend to

earn less after conviction regardless of where they work. This suggests that if discrimination accounts for why workers with a record earn less than their peers in the same firm, the level of discrimination varies little across firms.

Our second contribution is to use data on legal employment constraints for workers with records to explain this finding. These data come from the National Inventory of Collateral Consequences of Conviction and report restrictions on employment for workers with criminal records codified in state laws and regulations. These restrictions range from requirements to run background checks (e.g., for applicants seeking a license to operate a cemetery, Fla. Stat. §497.263) to outright prohibitions (e.g., for individuals with a prior felony on working as firefighters, O.C.G.A. §25-4-8). We merge the roughly 30,000 restrictions in this database to SOC occupation codes, allowing us to connect them to employment information in the Census and study how restrictions affect jobs across the pay distribution.

The results show that workers without a record tend to earn substantially more in occupation-state combinations without legal restrictions. Gaps are largest between workers in jobs with a mandatory restriction applying to prior felonies vs. not, where they stand at roughly 5 log points. Paralleling our findings from our AKM analysis, however, more than 100% of this difference can be attributed to workers rather than firms. For the same worker, unrestricted jobs tend to pay *less* than restricted jobs. Thus, although workers with a record are substantially less likely to work in an occupation-state with a restriction, this sorting contributes little to pay disparities because restrictions are not strongly targeted on pay.

A lack of sorting on pay does not imply a lack of sorting on other important job dimensions, however. Our third contribution is to characterize other ways that firms that employ workers with records tend to differ from those that do not. Workers with a record tend to work in firms with significantly higher quit rates and lower propensities to hire workers from competitors. As a result, revealed preference measures of their overall desirability such as those proposed by Sorkin (2018) suggest they are significantly worse overall. Among non-record workers, legal employment restrictions are also negatively correlated with these measures. These findings imply that although workers with a record may not sorting into lower-paying jobs to a large degree, they may sort into jobs offering significantly worse amenities.

Our final contribution is to show that in addition to shaping which jobs workers with a record may obtain, restrictions also simply explain whether workers with a record work at all. Using sampling error bias-corrected regressions of employment rates onto estimated person effects, we find that differences in pre-record person effects explain 30% of employment

rate gaps between workers with vs. without a felony record and 42% of the smaller gaps between workers with and without any criminal record. Accounting for maximal drops in productivity post conviction, they can explain at most 37% and 49% of employment gaps for each record type, respectively. The size of the residual gap unexplained by person effects compared to sorting effects for earnings suggests that the challenges of navigating the labor market with a record primarily impact whether a worker finds a job at all instead of its overall level of pay.

This paper contributes to a large literature on the connections between the labor market and criminal justice system. The labor market disadvantage of individuals with prior criminal justice contact has been documented extensively (e.g., Western, 2002). Studies that attempt to isolate the role of criminal charges and convictions tend to point to large declines in employment and earnings around the conviction event (Mueller-Smith and Schnepel, 2021; Rose, 2021; Agan et al., 2024*a*). Several analyses have also studied criminal justice interactions further upstream, such as arrest (Grogger, 1995) and pretrial detention (Dobbie, Goldin and Yang, 2018). A parallel literature shows that disclosing a criminal record substantially harms job application prospects (e.g., Pager, 2003; Agan and Starr, 2018) and studies employer motivations for disqualifying workers with criminal histories (Holzer, Raphael and Stoll, 2006; Cullen, Dobbie and Hoffman, 2023). Our analysis builds on this literature by studying the sources of criminal record penalties in the labor market and quantifying the contribution of employer sorting, reductions in earnings potential, and pre-record gaps in person effects.

This paper also connects more broadly to work on the overall impacts of criminal justice interactions, where evidence increasingly suggests that convictions (instead of sentencing punishments) prompt the most material changes (Humphries et al., 2024; Kamat, Norris and Pecenco, 2024). Holding conviction status fixed, for example, evidence suggests incarceration may reduce recidivism but has limited impacts on earnings and employment (Rose and Shem-Tov, 2021; Jordan, Karger and Neal, 2023; Garin et al., 2025). At the same time, other work suggests that undoing any harms of a criminal record may be challenging (Agan et al., 2024*a*,*b*), consistent with persistent skills being the most important factor for outcomes. Our results suggest that criminal records reduce log wages primarily by lowering persistent human capital, as captured by person effects.

We also contribute to a growing literature using two-way worker firm fixed effect models to study earnings inequality within and between groups. These analyses have studied gender gaps in Portugal (Card, Cardoso and Kline, 2016), Brazil (Morchio and Moser, 2024), and Denmark (Gallen, Lesner and Vejlin, 2019), race gaps in Brazil (Gerard et al., 2021) and the

U.S. (Card, Rothstein and Yi, 2024), immigrant-native wage gaps in Israel (Arellano-Bover and San, 2024), and the union pay-premia in the U.S. (Beauregard et al., 2025). We build on these prior analyses and show how studying a time-varying worker characteristic also allows for new approaches to decomposing between-group earnings gaps, which the prior literature has primarily approached by setting within-firm pay gaps to zero in a low-surplus sector such as restaurants.

2 Sample construction and summary statistics

This section begins by describing the administrative criminal justice data, earnings records, and survey data used in this analysis. We then describe the measurement of a criminal record and sample construction. Lastly, we discuss summary statistics for workers with a criminal record and compare them to those of workers without any recorded criminal justice contact.

2.1 Data sources

Administrative criminal justice data. We use criminal justice data from two sources. The first is statewide court and corrections records from the Criminal Justice Administrative Records System (CJARS) (Finlay, Mueller-Smith and Papp, 2022). CJARS is a joint project of the University of Michigan and the U.S. Census Bureau to integrate and standardize criminal justice records from a variety of state and local sources. The second data source is statewide court and correction data we collected by sending individual requests to various jurisdictions across the U.S. All criminal justice data includes information on the date of offense, charges, and case disposition; the nature of the disposition itself (e.g., conviction vs. acquittal); and the dates of incarceration spells in state prisons. Both CJARS records and the data we collected were linked to an anonymous person code known as a Protected Identity Key (PIK) by Census staff on the basis of name, date of birth, and address information. PIKs can then be used to link individuals across Census data products.

Longitudinal Employer Household Dynamics (LEHD). The LEHD data consists of quarterly unemployment insurance earnings records shared with the Census by all fifty states and the District of Columbia (D.C.), covering 96% of private sector jobs (Abowd et al., 2009) and all state and local government workers. Federal employees, self-employed workers, members of the military, and some agricultural work are excluded. Projects must seek approval from individual states to access their LEHD data. Twenty-one states and D.C.,

accounting for about 45% of the total U.S. population, approved our request.[3] We also have access to a separate file that indicates whether an individual had earnings in *any* state, including those that did not approve the study, allowing us to construct an indicator for having any LEHD earnings nationally. LEHD staff have also created a companion file that provides age, sex, race, and education information for LEHD workers based on links to the Decennial Censuses, American Community Survey, and Social Security Administration records, with values imputed where necessary.

Firms in the LEHD data are identified by state employer identification numbers, which typically reflect the entity reporting UI taxes to state authorities and may comprise multiple establishments. LEHD data contain a separate quarterly earnings record for each worker-firm pair. We transform this data into a worker-level panel in each state by keeping the top-paying employer in each quarter as well as the sum of earnings from all employers and the number of employers. We inflate all earnings information to 2020 real dollars using the Consumer Price Index (CPI). The vintage of LEHD data we use covers employment from the 1990s through 2022, with exact start dates depending on the state. The records also contain information on several firm characteristics, such as North American Industry Classification System (NAICS) codes.

American Community Survey (ACS). We use ACS responses from 2001 to 2021 to construct several additional variables used in some of our analyses, such as occupations. Individuals are linked between ACS and LEHD data based on the date of ACS response and PIKs. When merging occupations, for example, we keep any ACS response within eight quarters of each quarterly LEHD observation.

National Inventory of Collateral Consequences of Conviction (NICCC). The NICCC is a database of restrictions on individuals with prior criminal convictions imposed by the statutes and regulations of all fifty states, the federal system, and the District of Columbia. It was begun by the American Bar Association in 2012 and today includes tens of thousands of restrictions with structured attributes in a searchable database. We scraped all consequences related to "Employment and volunteering" and "Occupational and professional license and certification" for all 50 states. Each restriction includes a description, a citation to the relevant statute or regulation, the offense type it applies to (e.g., any conviction or any felony), the level of discretion allowed (e.g., is the consequence automatic / mandatory), and the duration (e.g., time since conviction that the consequence applies). We

[3]These states are Arizona, Arkansas, California, Colorado, Delaware, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Montana, Nebraska, Nevada, New Mexico, North Dakota, Ohio, Oklahoma, Tennessee, Texas, and Wyoming, as well as the District of Columbia.

then used large language models to map each restriction to detailed SOC codes, allowing each restriction to apply to several occupations or none. The matching process uses text embedding to select a relatively large set of candidate SOC matches for each restriction and then calls OpenAI's GPT-5-mini to select final matches.

2.2 Sample construction

Our sample is based on data from seven states where we both have access to LEHD records and where statewide court records cover a sufficiently long period to accurately measure criminal histories. These states are Arizona, Arkansas, Iowa, Maryland, North Dakota, Oklahoma, and Texas. In each state, we include quarters where workers are aged 22 to 62 (inclusive) and exclude individuals born too early to measure criminal histories from age 18 on. We also excluded quarters after the criminal justice data ends in each state to ensure we cover charges and convictions over the full sample period. Because men make up the bulk of individuals in our criminal justice data, we drop women entirely. Finally, we drop all earnings records after 2019 to avoid the influence of the COVID-19 pandemic.

Table B.1 provides an overview of the cohort and period restrictions used in each state. Workers with any LEHD earnings in one of these seven states over the period covered and born after the required year will be included in the sample.[4] We then build a panel of quarterly earnings for each worker over the relevant sample window. Although each worker in the sample was originally observed in at least one of the seven sample states, we include earnings data from all 22 states where we have access as well as national employment indicators to capture any activity after migration.

We then attach criminal histories to each worker in the sample. We record the earliest felony conviction as the first quarter where the worker has a felony conviction disposed or is admitted to incarceration in the union of our criminal justice records and the CJARS datasets. We also consider a broader notion of criminal record status defined as having a prior felony charge (regardless of conviction) or a conviction for any lower-level crime excluding traffic and public order offenses. These two definitions of record status serve as our primary categorizations of interest. We refer to the former as the "any felony" sample and the latter as the "any record" sample. A worker is considered to have a criminal record of the relevant type in and after the earliest relevant quarter.

A key concern is whether our measures of criminal history accurately capture all charges and

[4] A small share of workers appear in multiple states. In these cases, we ensure they meet the sample restrictions in each of the states where they appear.

convictions for workers in our sample. We view the most likely type of error as cases where workers are incorrectly coded as having no record when they have one but failed to match to our data or acquired it in a jurisdiction where we do not have coverage. We mitigate this concern by using *all* criminal justice data in CJARS and our records to measure criminal histories, not just those from the seven states that form the basis of the analysis sample. This means prior convictions in states like North Carolina, Florida, and many other jurisdictions will still be captured in our data.[5]

Nevertheless, some mismeasurement is inevitable. To the extent that some workers we classify as having no record in fact have one, we will likely understate the gaps between workers with and without a record. As we show below, however, the gaps we measure are enormous, suggesting that this issue is not too extreme. Out-of-state prior records are also relatively rare in our data. Approximately 12% of the workers with a felony record in our sample obtained it outside the seven core sample states. About 14% of workers classified as having a felony record in our sample would not be if we used only criminal justice data from the state where they were first recorded as having any earnings.

2.3 Summary statistics

Table 1 presents summary statistics for all workers in our sample, those with a felony conviction vs. not ("felony" vs. "no felony"), and those without any type of record vs. not ("any record" vs. "no record"). The overall sample is 29 years old on average, 78% white, and is roughly split between having at least some college vs. none. About 8.2 million unique workers are present in the data, with employment at roughly a million distinct firms. About 15% of workers are recorded as having some criminal record and 6% as having a prior felony conviction. These means are lower than some prior work estimating the population prevalence of criminal records.[6] The difference likely stems from our focus on convictions instead of lower-level interactions such as arrests, which are less well measured in our data, our exclusion of traffic and public order offenses, as well as the requirement that all individuals in our sample have at least some interaction with the formal labor market that results in positive LEHD earnings.

Workers with a record tend to be less likely to be white and have lower levels of formal education, with differences more extreme in the felony vs. no felony sample. Workers with a record of any type also have significantly lower earnings and employment rates. On average,

[5]The CJARS documentation provides a complete description of their data coverage.

[6]Brame et al. (2012)'s analysis of the National Longitudinal Survey of Youth finds a self-reported arrest rate between the ages of 18 and 23 of 30% when non-response is treated as missing at random.

they earn $2,750 less per quarter. Log earnings, which condition on working, are 38 log points lower. Workers with a felony conviction face even worse labor market outcomes relative to non-felons, earning $3,734 less per quarter and, conditional on employment, receiving wages that are 58 log points lower. A similar pattern appears in employment outcomes, with workers who have a felony record experiencing the lowest employment levels. These patterns underscore the substantial disparities associated with having a criminal record, with felony convictions linked to the most severe labor market penalties.

What drives these disparities in economic outcomes? One potential factor is the employment barriers imposed by a criminal record, particularly for those with felony convictions. Workers with records are more likely to be employed at firms where co-workers earn significantly lower wages, suggesting that these barriers push them into lower-paying jobs. Notably, differences in co-worker pay account for most of the earnings gap, indicating that differences in firm sorting may play a critical role in explaining wage disparities between workers with and without a record. Another contributing factor is differences in worker skills and preferences that were present before the worker acquired a criminal record. For instance, workers without records are twice as likely to hold a college degree, reflecting substantial gaps in skills and labor market productivity. While these descriptive statistics suggest that both pre-existing and post-conviction factors contribute to earnings disparities, their relative impact remains difficult to quantify. We next turn to an econometric model designed to help quantify the role of these different channels.

3 Econometric model

In this section, we propose a statistical model for quantifying the role of firms, pre-record characteristics, and changes in earnings potential post-record. Our analysis builds on two-way fixed effects models introduced by Abowd, Kramarz and Margolis (1999). Specifically, we model mean log quarterly wages, w_{it}, for workers without a record at period t ($G_{it} = 1$) as:

$$E[w_{it}|i,t,G_{it}=0] = \alpha_i + \psi_{j(i,t)} + \beta_0' X_{it} + \epsilon_{it} \tag{1}$$

and for workers with a record in period t as:

$$E[w_{it}|i,t,G_{it}=1] = \underbrace{\alpha_i + \tilde{\alpha}_i}_{pers\ FE} + \underbrace{\psi_{j(i,t)} + \chi_{j(i,t)}}_{firm\ FE} + \underbrace{\beta_0' X_{it} + \beta_1' X_{it}}_{exp.\ profile} \tag{2}$$

Combining Equations 1 and 2 the model can be written as:

$$\overbrace{w_{it}}^{\substack{Log\\wages}} = \overbrace{\alpha_i}^{\substack{Person\\effects}} + \overbrace{\psi_{j(i,t)}}^{\substack{Firm\\effects}} + \underbrace{G_{it}[\overbrace{\tilde{\alpha}_i}^{\substack{\Delta Person\\effects}} + \overbrace{\chi_{j(i,t)}}^{\substack{\Delta Firm\\effects}}]}_{\substack{\Delta \text{ in pay after}\\ a \text{ criminal record}}} + \overbrace{\beta_0' X_{it} + G_{it}\beta_1' X_{it}}^{\substack{exp.\ profile\\by\ record\ status}} + \epsilon_{it} \tag{3}$$

This model amounts to the canonical AKM specification but fully interacted with record status. Holding the worker's firm fixed, log earnings can decline after acquiring a record because their person effect changes ($\tilde{\alpha}_i$), gaps in firm effects emerge (χ_j), or the influence of covariates shifts (β_1). Earnings may also be lower for workers with a record due to lower pre-record person effects (α_i), capturing differences in earnings potential that precede record acquisition altogether. Finally, workers with a record may tend to be employed at firms with different pay premia (ψ_j) both before and after they first acquire a record.

One interpretation of this model is as a purely statistical decomposition of earnings patterns for each group. A growing body of literature demonstrates that AKM models provides a good approximation to conditional means (see Card, Heining and Kline, 2013; Card et al., 2018; Kline, 2024), including recent work in the U.S. using Social Security Administration earnings records, tax records, and LEHD data from the Census Bureau (Song et al., 2019; Bonhomme et al., 2023; Card, Rothstein and Yi, 2023*a*,*b*). We show that the standard specification tests perform well in our setting too.

Interpreting firm effects as the *causal* effects of firms on earnings requires assumptions on the mobility processes that identify them. Assuming "exogenous mobility" (Card, Heining and Kline, 2013) rules out between-firm mobility correlated with unobserved determinants of earnings, which includes both shocks to workers' productivity as well as sorting based on match effects. Kline (2024) relates the assumptions required for a causal interpretation of firm effects to those used in difference-in-differences analyses. In the AKM context, a causal interpretation requires both parallel trends—so that outcomes in the absence of a move would have remained stable—and no selection on treatment effects—so that the wage changes of movers are a good guide to potential wage changes for the average worker. Whether these assumptions are satisfied remains contested (see, e.g., Borovičková and Shimer, 2024).

Other work has provided a model-based interpretation of Equation 3 (e.g., Lamadon, Mogstad and Setzler, 2022; Morchio and Moser, 2024). If employers set wages using standard Lerner markdown rules, for example, then log wages admit the representation in Equation 3 whenever markdowns are constant within firm and marginal product is log additive in

worker components (constant and time varying), firm productivity, and any residual shocks to either (see, e.g., the model in Card et al., 2018). Identification of the parameters in such a wage equation would then depend on assumptions about the relevant unobservables. We do not take a stand on the model generating Equation 3 in this paper and primarily interpret it statistically, although we occasionally will refer to person effects and firm effects as related to workers' and firms' productivities, respectively.

The model requires several normalizations. First, the mean of the firm effects ψ_j is not identified. How one normalizes these parameters is inconsequential for the analyses we consider, but for interpretability we follow recent work and set their mean to zero in the restaurant sector (NAICS 722), an industry commonly considered to be low-surplus (Card, Rothstein and Yi, 2023*a*,*b*, 2024).[7] Second, because the mean firm effect is not identified for either record status group, the mean gap in firm effects χ_j is also not identified. The normalization of these parameters matters since it determines the relative role of declines in person effects post conviction vs. the emergence of within-firm differences in pay premia. We follow the prior literature in considering normalizations based on an assumption of zero within-firm gaps in the restaurant industry (Card, Cardoso and Kline, 2016; Gerard et al., 2021; Arellano-Bover and San, 2024; Morchio and Moser, 2024),[8] as well as an alternative that provides bounds on changes in mean person effects leveraging the fact that record status is time-varying.

A third issue is that although both α_i and $\tilde{\alpha}_i$ are identified (up to normalization) for all workers observed with both $G_{it} = 0$ and $G_{it} = 1$, some workers acquire a record at an early age and are never observed with $G_{it} = 0$. For workers with $\min_t G_{it} = 1$, only $\alpha_i + \tilde{\alpha}_i$ is identified. This means that the overall mean of α_i given that $G_{it} = 1$ is also not identified. To solve this issue, we assume that $\Delta = E[\tilde{\alpha}_i | \min_t G_{it} = 0, G_{it} = 1] = E[\tilde{\alpha}_i | G_{it} = 1]$. Because Δ can be estimated directly along with $E[\alpha_i + \tilde{\alpha}_i | G_{it} = 1]$ using their sample analogues, we can back out $E[\alpha_i | G_{it} = 1]$ as $E[\alpha_i + \tilde{\alpha}_i | G_{it} = 1] - \Delta$.[9] While this procedure requires no

[7] Card, Rothstein and Yi (2023*b*) write: "This is a large sector, with 2.7% of national employment; is nontradeable, not geographically concentrated, and competitive; and does not require highly specialized skills. It is also typically one of the lowest-paying industries. This makes it a useful benchmark for other sectors." In our sample, the average firm effect is lowest in the accommodation / food service sector overall.

[8] Morchio and Moser (2024) derive their normalization from a theoretical model, which implies that firms exhibiting a zero within-firm gender pay gap must satisfy three conditions: (i) they rank near the bottom of the utility distribution for both genders; (ii) they treat men and women as almost perfect substitutes in production; and (iii) they offer little to no amenity value to the workers they employ. To meet these conditions, Morchio and Moser (2024) focuses on the smallest firms in the restaurant and fast-food sector.

[9] Computing these quantities requires estimating the model under a given normalization, but so long as the same one is used to both estimate the mean of $\tilde{\alpha}_i$ in the sample observed with both $G_{it} = 0$ and $G_{it} = 1$ and $E[\alpha_i + \tilde{\alpha}_i | G_{it} = 1]$ in the full population it is irrelevant because it affects both objects equally. We estimate the model in the full population and then compute the mean of $\tilde{\alpha}_i$ in the sub-sample observed both

selection bias due to conditioning on $\min_t G_{it} = 0$, we find that $E[\alpha_i + \tilde{\alpha}_i | G_{it} = 1]$ differs from $E[\alpha_i + \tilde{\alpha}_i | \min_t G_{it} = 0, G_{it} = 1]$ by only 0.008 log points when G_{it} is defined by having any prior felony and 0.016 log points when G_{it} is defined as having any record, suggesting any selection is minimal.

Finally, when estimating this model we follow the standard approaches for accounting for the fact that LEHD contains no information on hours (Sorkin, 2018; Card, Rothstein and Yi, 2023*a*,*b*). Specifically, we include only quarters with earnings of at least \$3,800 (about full-time earnings at the federal minimum wage) from a single employer and drop the first and last quarter of each worker-firm spell. We also require a connected set for both record status groups. We use the union of the largest connected sets for both groups, dropping the small share of firms in the connected set for workers with a record but not without. Thus, although ψ_j is identified for every firm in the estimation sample, χ_j is only identified for every firm where at least one worker with a record has some employment.

4 Explaining earnings dispersion

We begin by using estimates of the parameters in Equation 3 to decompose sources of pay dispersion for workers with and without a criminal record. This decomposition is based on the idea that due to the additive structure of the model, earnings variances for each group ($G_{it} = 0$ vs. $G_{it} = 1$) can be written as:

$$Var_0(w_{it}) = Var_0(\alpha_i) + Var_0(\psi_{j(i,t)}) + 2Cov_0(\alpha_i, \psi_{j(i,t)}) + \eta_0 \tag{4}$$

$$\begin{aligned} Var_1(w_{it}) = {} & Var_1(\alpha_i + \tilde{\alpha}_i) + Var_1(\psi_{j(i,t)}) + Var_1(\chi_{j(i,t)}) + 2Cov_1(\alpha_i + \tilde{\alpha}_i, \psi_{j(i,t)}) \\ & + 2Cov_1(\alpha_i + \tilde{\alpha}_i, \chi_{j(i,t)}) + 2Cov_1(\psi_{j(i,t)}, \chi_{j(i,t)}) + \eta_1 \end{aligned} \tag{5}$$

where $Var_g(X)$ and $Cov_g(X, Y)$ are shorthand for $Var(X|G_{it} = g)$ and $Cov(X, Y|G_{it} = g)$, respectively, and the residual terms η_g capture dispersion due to covariates ($\beta_0' X_{it}$ and $\beta_1' X_{it}$) as well as unexplained earnings variation (ϵ_{it}).[10] These summaries of the joint distribution of firm effects, person effects, and within-firm gaps provide a high-level overview of the relative quantitative importance of elements of Equation 3 for pay dispersion within each group. Since this decomposition is focused on within-group earnings dispersion only, it is unaffected by any normalization choices. Because separately estimating α_i and $\tilde{\alpha}_i$ is possible only for workers observed both pre- and post-conviction, we focus in the record sample on

pre- and post-conviction using the same estimates, ensuring the normalization does not affect the results.

[10] Covariates in our setting capture the shape of experience profiles, which as noted earlier are normalized to be flat at age 40. Earnings levels at age 40 are reflected in person effects.

the role of the "total" person effect $\alpha_i + \tilde{\alpha}_i$, which is identified for all workers ever observed with $G_{it} = 1$. The role of firms (ψ_j) and within-firm gaps (χ_j) in the record samples is of particular interest in our setting since these terms describe how important *where* a worker with a record is employed is for their relative earnings.

4.1 Person effects, firm effects, and within-firm gaps

Table 2 reports estimates of the variance and covariance of person and firm effects and within-firm gaps for workers both with and without a record and the resulting variance decompositions. Recent work has emphasized the importance of correcting for bias due to sampling error in these estimates (e.g., Kline, Saggio and Sølvsten, 2020; Bonhomme et al., 2023). Without correction, for example, variance estimates are biased upwards and covariance estimates can be biased downwards, a phenomenon sometimes referred to as "limited mobility" bias. In Table 2, we present both plug-in estimates and bias-corrected estimates constructed under a homoscedasticity assumption (Andrews et al., 2008).[11]

Panel A presents estimates treating G_{it} as an indicator for any felony record (Columns 1 and 2) and as an indicator for any criminal record (Columns 3 and 4). In the no-record sample, the bias corrected variance of firm effects (ψ_j) is approximately 5 log points, implying that moving one standard deviation in the firm effects distribution represents an approximately 22% change in earnings. These firm effects explain about 18% of the total variance of log wages, indicating a substantive role for firms in accounting for earnings dispersion. Person effects (α_i) are roughly twice as variable as firm effects, explaining 40% of the variance of log earnings for both workers without a felony and workers without any record. Finally, we find strong positive covariance between firm and person effects. Dividing the covariance by the square root of the product of the relevant bias-corrected variances yields a correlation coefficient of about 0.26. This pattern of higher earning workers sorting to higher earning firms explains a further 14% of earnings dispersion among workers without a record.

Our estimates for workers without a record are comparable to those found in other studies studying the overall population. For example, our uncorrected estimate of the covariance of worker and firm effects is 0.014, the same as in Table A1 of Card, Rothstein and Yi (2023*a*), who also use the LEHD. Moreover, our uncorrected estimate of 21% for the share of the variance explained by firm effects is similar to prior uncorrected estimates of the importance of firms that range between 15% to 25% (Card et al., 2018; Bonhomme et al., 2023; Kline,

[11] Bonhomme et al. (2023) showed that the homoscedastic correction usually yields similar results to heteroscedasticity-robust alternatives (Kline, Saggio and Sølvsten, 2020) and has lower variability in Monte-Carlo simulations.

forthcoming).

Panel B of Table 2 presents estimates for workers with a felony record and those with any record. These findings show similar patterns to those in Panel A, with some key differences. First, firm effects ψ_j show a similar level of overall variance (5 log points), indicating that not all workers with a criminal record are concentrated in disproportionately low paying firms. Because earnings dispersion is lower overall in the record samples, however, as a share of total variance firms explain slightly *more* of earnings dispersion for workers with vs. without a record.

Second, plug-in estimates of total person effects ($\alpha_i + \tilde{\alpha}_i$) indicate comparable variability to workers without a record, but our bias corrections have substantially more bite in this case. This occurs because, as we demonstrate further below, workers with a record tend to be employed much less frequently than workers without one, so their person effects are estimated less precisely and as a result are more over-dispersed. After bias correction, we find that person effects explain 26-33% of earnings dispersion in the record populations. A smaller role for person effects is consistent with lower average skill in this population. Differences in earnings due to accumulated schooling, for example, would impact a worker's person effects.

Third, although the marginal distributions of person and firm effects differ in some aspects between the two samples, they show a similar positive correlation. If anything, person and firm effects are more strongly correlated among workers with a record. This pattern of assortative matching explains about 13-14% of earnings dispersion. Thus, although there is significantly less dispersion overall in total person effects among workers with a record, relatively higher earning workers with a felony record or any record remain more likely to work at higher paying firms.

The final panel of Table 2 presents estimates of the dispersion of within-firm gaps in pay (χ_j). Before correcting for bias due to sampling error, these pay gaps account for approximately 12-18% of the variance in log earnings. Meaningful dispersion in pay gaps could indicate discrimination against workers with a criminal history in some firms. However, after correcting for over dispersion due to sampling error, the variance of within-firm pay gaps is effectively zero. In fact, the point estimate is slightly negative at -0.008 for workers with a felony record and -0.001 for workers with any record.[12] Because χ_j is only defined at firms where workers with a record are *ever* employed, this estimate pertains only to firms that are willing to hire at least one worker with a record. Nevertheless, the data are consistent with

[12]We also find a small positive covariance between gaps and firm effects and gaps and person effects after bias-correction, although given the low variance in gaps these estimates likely reflect noise as well.

a world where any within-firm pay differences between workers with and without a record are common across all firms that employ workers with records.

These findings speak to a large literature that, following Card, Cardoso and Kline (2016), has used AKM models to study wage gaps between groups (Gerard et al., 2021; Gallen, Lesner and Vejlin, 2019; Dostie et al., 2023; Arellano-Bover and San, 2024). A common measure of differential pay is the "rent-sharing" coefficient, defined as $1 + \frac{Cov(\chi_j, \psi_j)}{Var(\psi_j)}$. This coefficient can be interpreted as the projection of firm effects for workers with a record $(\psi_j + \chi_j)$ onto firm effects for workers without one (ψ_j). This statistic is typically motivated by a model where firm effects capture surplus sharing between firms and workers, but one group has lower bargaining power and therefore potentially captures a lower share (e.g., Card, Cardoso and Kline, 2016; Gerard et al., 2021). Before correcting for sampling error, we find rent-sharing coefficients of 0.62 and 0.87 for workers with a felony record and with any record, respectively. However, after correcting for sampling error, we find coefficients of 1.21 and 1.03, which are consistent with no compression in firm effects for workers with vs. without a record.

4.2 Validating AKM

Several papers have attempted to stress test how well the additive specification Equation 3 captures earnings patterns across workers and within the same worker over time. These tests have shown that in a variety of countries and contexts the model provides a good approximation to average log earnings across the firm and person effect distribution and for movers between firms. We replicate several key exercises supporting the additive structure of firm effects in our samples here. The results of our mover's analysis below, as well as the evidence from prior work (e.g., Card et al., 2018; Bonhomme et al., 2023), are consistent with the additive structure of the model and limited sorting on match effects.[13]

Figure A.3 presents event study plots of worker wages before and after transitions across firms with varying levels of co-worker pay, following the approach proposed by Card, Heining and Kline (2013). The plots show no evidence of pre-trends prior to the transition. Workers moving to firms with more highly paid coworkers experience wage gains, while workers moving to firms with lower paid coworkers experience wage losses. The post-move wage changes are symmetric: workers moving to firms with higher or lower co-worker pay experience wage gains and losses of similar absolute magnitude.

[13] Borovičková and Shimer (2024), however, provide an example of a data generating process under which these tests do not reveal the presence of worker-firm match effects in earnings.

Figure A.4 reinforces this symmetry by plotting changes in wages for movers up the firm distribution against those for movers down the distribution. Consistent with the model's additive structure, the dots are clustered around a line with a slope of -1, indicating moves up vs. down offer equal and opposite changes in wages. The OLS estimate of the slope, which does not correct for attenuation bias due to sampling error in the right-hand side variable, is -.96.

5 Explaining earnings gaps

In addition to explaining pay dispersion within groups, parameter estimates from Equation 3 can be used to assess sources of earnings disparities *between* groups. Specifically, the additive structure of our model implies that differences in average log wages can be expressed as:

$$\begin{aligned}\underbrace{E[w_{it}|G_{it}=0]-E[w_{it}|G_{it}=1]}_{\textit{raw gap}} &= \underbrace{E[\alpha_i|G_{it}=0]-E[\alpha_i|G_{it}=1]}_{\textit{person effects (pre-record)}} \\ &+ \underbrace{E\left[\psi_j|G_{it}=0\right]-E\left[\psi_j|G_{it}=1\right]}_{\textit{between firm sorting}} \\ &- \underbrace{\underbrace{E[\tilde{\alpha}_i|G_{it}=1]}_{\textit{change in person effects}} - \underbrace{E\left[\chi_j|G_{it}=1\right]}_{\textit{change in firm effects}}}_{\textit{change in ``earnings potential'' due to record}} \\ &+ \underbrace{\beta_0' E[X_{it}|G_{it}=0]-(\beta_0+\beta_1)'E[X_{it}|G_{it}=1]}_{\textit{covariates}}\end{aligned} \tag{6}$$

This expression allows us to decompose pay gaps into components attributable to several sources. The first component is differences in earnings potential that existed prior to record acquisition, as captured by mean pre-record person effects α_i. The next component captures differential sorting across firms with varying pay premia ψ_j. The next component reflects changes in person effects post record and mean within-firm pay gaps, the combination of which we dub changes in "earnings potential." Covariates explain the remainder; in what follows, we subtract off this term to focus on what explains earnings differences after accounting for experience profiles.

As noted in Section 3, a key challenge is that although the change in earnings potential is identified, the relative contribution of changes in person effects vs. mean within-firm gaps is not.[14] This normalization issue exclusively affects the third line of the decomposition.

[14] One can arbitrarily shift all changes in person effects by $-k$ while simultaneously adjusting pay gaps by

The contributions of pre-record person effects and between firm sorting are invariant to this normalization issue (as well as the overall normalization of firm effects ψ_j). We therefore begin our discussion with these components before discussing how to interpret changes in earnings potential.

5.1 Overall gaps

Figure 1 presents the overall results of the decomposition in Equation 6 for workers with a felony record (Panel A) and any record (Panel B). The log earnings gap between workers without a prior felony conviction vs. with one, net of covariates, is roughly 24 log points.[15] Person effects, which capture gaps in earnings potential that existed *prior* to record acquisition, explain the majority of this gap (66%). As discussed in Section 3, this estimate is constructed by using the sub-sample of workers with a record observed both before and after their first conviction to estimate the average change in person effects pre vs. post record. We then subtract this estimate from the mean of total person effects in the full record population, or $E[\alpha_i + \tilde{\alpha}_i | G_{it}] = 1$, to produce an estimate of $E[\alpha_i | G_{it} = 1]$.[16] Panel B shows that the gap between workers with any record vs. not stands at 17 log points, with person effects accounting for an even larger share of the total (77%) in this case.

Differences in firm premia, on the other hand, explain a comparatively small portion of gaps. They account for roughly 11% of the total earnings difference between workers with vs. without a prior felony conviction. Comparing workers with any record vs. not, we find a slightly smaller role for firm effects. As share of the total, firm effects explain 9% of the gap in this population. These estimates suggest some role for firms in explaining the labor market penalty of a criminal record, although smaller than what some previous analyses have found for the role of firms in explaining race or gender gaps. Gerard et al. (2021), for example, find that firm effects explain 18-20% of racial wage gaps in Brazil, while Card, Cardoso and Kline (2016) find they explain about 14-20% of gender gaps in Portugal. However, recently Card, Rothstein and Yi (2024) find that firm effects explain none of the racial gaps in the U.S.

Some of these sorting effects may come from differential access to particular industries with

$+k$, leaving fitted values unchanged.

[15] These are the gaps among full-time workers in "sandwich" quarters with earnings above the federal minimum wage, and so naturally smaller than the overall gaps in Table 1. We decompose gaps in the propensity to work full time later in Section 6.

[16] As noted earlier, because the choice of normalization for within-firm pay gaps impacts our estimate of the change in person effects pre vs. post conviction and the average total person effect equally, it does not affect our estimate of $E[\alpha_i | G_{it} = 1]$.

varying levels of average pay premia (Card, Rothstein and Yi, 2023*a*). Figure 2 illustrates the role of industry by plotting average firm effects (left y-axis) and the share of employment for each group (right y-axis) by 2-digit NAICS code. Industries are sorted on the x-axis based on their average firm effects from highest (mining, oil, and gas) to lowest (accommodation and food services).[17] Panel A shows that workers with a felony are disproportionately employed in manufacturing, construction, administrative and support services, and accommodation and food services. As is clear from the figure, some of these industries turn out to be relatively high paying (e.g., construction), while others are not (food service). As a result, the *between* industry component of sorting is relatively unimportant, explaining just 11% of the total sorting effect. The bulk of sorting therefore occurs within 2-digit industry.

Panel B of Figure 2 reports similar estimates for workers with any criminal record vs. no criminal record. Here, industry differences play a larger role in explaining sorting across firms and account for 51% of the gap in firm effects. The difference between Panels A and B mainly stems from manufacturing—workers with any record are less likely to be employed in this typically high-paying industry than workers with a felony record specifically. Of course, some pay differences across industries likely reflect compensating differentials (Sorkin, 2018; Humlum, Rasmussen and Rose, 2025). To the extent that construction jobs are high paying but difficult and physically taxing, for example, between-industry gaps in wages may understate between-industry gaps in utility.

The last bar in Figure 1 reports the portion of the log earnings gap between workers with vs. without a record not explained by either pre-record person effects or sorting across firms. This residual change in earnings potential reflects reductions in earnings at all firm and can be attributed in our model to either a drop in average person effects or the emergence of an average within-firm pay disparity. For both the felony and any record contrasts, the combination of these two factors is non-negligible, explaining roughly 23% of the total disparity in the former case and 15% in later. Relative to sorting effects, which capture how representation *across* firms contributes to gaps, changes in earnings potential, which capture common gaps *within* firms, are thus roughly twice as important.

The results under the common normalization of pay gaps to be mean zero in the restaurant sector (Card, Cardoso and Kline, 2016; Gerard et al., 2021; Arellano-Bover and San, 2024) indicates that all the drop in potential earnings is related to lower person effects post-record. As a result, gaps are explained by either sorting or post-record person effects, with the vast majority (89%) accounted for by the latter. This normalization choice implies that,

[17]Figure A.5 compares average firm effects by industry to those from Card, Rothstein and Yi (2023*a*), which use LEHD records from all states. Our estimates align closely with theirs.

on average, workers with a record receive a small within-firm *bonus* of roughly 1 log point. While the restaurant sector is typically viewed as low-surplus and competitive, assuming zero gaps in this industry also rules out some plausible mechanisms by which a criminal record may reduce earnings. If workers with a record are disproportionately less likely to work in front-of-house jobs with access to the best tips, for example, within-firm gaps may emerge even among restaurants. Indeed, Agan and Starr (2018) finds large effects of disclosing a record on employer contact rates in a correspondence study of fast food and big-box chains, suggesting that record status matters to employers even in this sector.

Looking across these results, we reach several conclusions. First, it is clear that pre-record person effects are responsible for the majority of earnings gaps. This finding underscores the fact that individuals who interact with the justice system tend to have low levels of formal schooling and face a host of other challenges, many of which predate interaction with the justice system altogether. Second, sorting appears to contribute meaningfully to gaps, but is roughly half as important as sources of pay differentials within firms. Third, there is likely some role for within-firm pay gaps in explaining pay differences between workers with and without a record. In a world where differences in person effects explain all of sorting (linearly), pre-record differences in person effects are sufficient to account for all of the observed sorting effects. Further declines in person effects would require *over* explaining the gap in firm premia due to sorting.

5.2 Conditional gaps

Comparing workers with a record to all workers without one may overstate gaps because of differences in education, age, and other factors. Table 3 presents decomposition exercises that account for various sets of observable characteristics. The methodology is identical to that in the previous sub-section, except that the non-record sample is re-weighted to match the observable characteristics of the record sample. We use characteristics that can be coded as a discrete set of indicators for membership in a particular covariate cell, so that the re-weighting procedure amounts to dropping cells where no workers have a record and weighting the non-record group by the relative share of workers with a record in each cell.[18] Panel A reproduces the unweighted, baseline gaps from Table 1 to facilitate comparisons.

Panel B adjusts for demographics by re-weighting based on the interaction of year of birth and four levels of educational attainment.[19] Consistent with workers with a record having

[18] Because records are relatively rare, effectively zero cells have zero workers without a record and need to be dropped. In other words, covariate overlap is not an issue.

[19] These levels are the same used in estimates of Equation 3: less than high school, high school, some

less overall education, re-weighted log earnings gaps are smaller than in the baseline decomposition by about 2.3 log points for the felony contrast and 2.2 log points for the any record contrast. The reduction mainly comes from a smaller gap in pre-record person effects, which stands at 14 log points and accounts for 64% of the total for the felony vs. no felony comparison, and 11 log points (75%) for the any record comparison. Sorting effects are also slightly smaller in both cases, although they account for a similar share of the overall gap. The change in earnings potential is invariant to re-weighting the non-record sample because it involves expectations over the population with $G_{it} = 1$ only (i.e., it is already weighted to match the record group's observables). Because re-weighting reduces the overall gap, however, this final term explains a slightly larger share of the conditional disparity in Panel B.

Panel C re-weights based on cells defined by year of birth, education, two-digit industry, and city and state. Although one impact of having a record may be to influence where a worker lives and the industry in which they are employed, Panel B shows that substantial gaps in earnings remain within these cells—about 17 log points for the felony comparison and 11 log points for the any record comparison. Pre-record person effect gaps shrink substantially after accounting for these observables, falling by roughly a third. Roughly two thirds of the gap in person effects between workers with vs. without a record thus reflects gaps within industry and place. Sorting effects, however, remain similar in magnitude to the overall sample, standing at 2.2 log points for the felony sample and 1.6 log points for the any record sample. As in Panel B, changes in earnings potential are not affected by re-weighting and so explain the same amount of log earnings differences as before.

Finally, Panel D presents the same exercise as Panel C but replacing industry with occupation-by-year codes from the ACS.[20] These results tell a similar story to those in Panel C for the comparison between workers with and without a felony. For the comparison between workers with any record vs. not, however, sorting effects become quite a bit smaller.

Taking these estimates together suggests several conclusions. First, industry and occupation are important for the raw earnings gap between workers with and without a record. Accounting for these factors reduces overall disparities by 25% or more. Second, the contribution of sorting remains important even within narrowly defined covariate cells, often

college, and bachelors or higher.

[20]We add these codes by attaching any ACS response within eight quarters of the focal quarter in the LEHD. We use the raw ACS occupation codes interacted with year because ACS codes change over time. Within-firm gaps change slightly relative to Panels A-C because the sample here contains only workers matched the ACS.

explaining a similar fraction of gaps as in the unconditional decomposition. This suggests that if acquiring a criminal record shifts access to high paying jobs, the effect is roughly as strong among similar workers in similar types of jobs as between them. Finally, changes in earnings potential tend to increase in relative importance when conditioning on workers observables, but only because these observables absorb other potential driver of gaps while leaving the role of earnings potential unchanged.

5.3 Sorting and legal restrictions

Why does sorting not explain a larger share of record penalties? We next use the NICCC data to help unpack this finding. Table 4 contains the key results. This table compares the earnings of workers in occupation-state combinations with legal restrictions captured in the NICCC vs. workers in occupation-state combinations without legal restrictions. To focus on how restricted vs. unrestricted jobs differ in their level of pay as opposed to their propensity to discriminate or other factors, the analysis here includes only workers without a prior felony. We examine three different types of restrictions: any restriction (column 1), any restriction that is mandatory / automatic (column 2), and any restriction that is mandatory / automatic and specifically targets prior felonies (column 3).

The results show that workers in unrestricted jobs tend to earn more: roughly 2.6 log points for jobs with any restriction vs. no restriction, and nearly 5 log points for jobs with mandatory felony restrictions vs. no mandatory felony restriction. As indicated by the last two rows of the table, the more severe restrictions affect a smaller share of employment, but workers with prior felonies are also significantly less likely to hold these jobs. The row labeled person effects shows that more than 100% of the gap between workers in restricted vs. unrestricted jobs is attributable to the portable components of pay. If anything, jobs with restrictions tend to be lower paying. The average firm effect among jobs with a mandatory felony restriction is 4.2 log points lower than the average in jobs without such a restriction.

These results imply that although record restrictions seem to concentrate on high paying jobs, what makes these jobs high paying is the people who hold them, not the jobs themselves. As a result, even if workers with records tend to disproportionately sort into jobs without restrictions, that sorting may in fact tend to decrease earnings gaps.

5.4 Sorting on other firm characteristics

Although sorting may explain a small share of wage gaps, workers with records may tend to be employed at firms that differ in other important ways. Table 5 explores two measures of

firm desirability considered in the prior literature: a poaching rate (the share of new hires that come from other employers vs. non-employment) and the quit rate (the fraction of workers that separate each quarter). Overall, workers with a felony record tend to work at firms with lower poaching rates and higher quit rates. The difference in the latter metric is 5.6 percentage points, or 33% higher than the non-felony average. The final rows of the table show that the majority of these gaps persist within 2-digit NAICS industries.

6 Explaining employment disparities

The preceding analyses focused on explaining dispersion and between-group gaps in log earnings among workers employed full time. As demonstrated in Table 1, however, workers with a criminal record have substantially lower overall employment rates. For example, individuals with a felony record are 11 percentage points less likely to be employed and 18 percentage points less likely to hold a job over a full quarter that pays at least the federal minimum wage. While some of these gaps may reflect the labor market barriers faced by workers with a criminal record, they may also stem from differences in underlying skills and productivity that pre-date contact with the criminal justice system. This section uses estimates from Equation 3 to explore the role of these two factors.

Figure 3 presents the key exercise. This graph is a binned scatter plot of average employment rates against person effects for workers with vs. without a felony record, with each bin representing a decile of the within-group person effect distribution. Individuals observed both before and after acquiring a record are treated as two different observations. The outcome is the average employment rate in "sandwich" quarters with sufficient earnings to qualify for inclusion in the sample used to estimate our primary specification Equation 3. The graph presents two versions of person effects for workers with a record. The first, dubbed "pre-record" effects, is an estimate of α_i constructed using the same strategy as in Section 5. The second, dubbed "post-record" effects, uses $\alpha_i + \tilde{\alpha}_i$ as workers with a record's person effect assuming that mean within-firm gaps χ_j are zero. The former can be thought of as the level of earnings potential workers with a record had prior to acquiring their record, while the latter can be thought of as earnings potential post-record (with differences stemming from either within-firm gaps or drops in person effects).

The results show that average employment rates are increasing in estimated person effects. The relationship appears fairly linear except in the right tail of the person effect distribution, where differences do not translate into differences in employment rates. Because person effects are noisily estimated, slope attenuation in the tails may simply reflect measurement

error. Indeed, if the most extreme person effect observations reflect pure noise, one would expect a flatter slope in the tails. The lines on the figures present OLS slopes that are not corrected for attenuation bias due to sampling error. After correcting using the estimates from Table 2, the relationship is even stronger for both groups: employment rates increase with person effects at adjusted slopes of approximately 0.22 for the non-felony group and 0.61 for the felony group—roughly double the uncorrected slopes of 0.19 and 0.32, respectively.[21]

Key differences emerge between the felony record and non-felony population. First, at every level of estimated person effects, employment rates are higher for the non-felony group than the felony group. These gaps are quite substantial. At the median person effect for the record sample, for example, gaps in sandwich employment stand at 7-10 percentage points. While the size of this gap depends somewhat on the normalization chosen for the person effects, it is also clear under either polar case large conditional employment gaps remain. Indeed, mean post-record person effects for the felony group would need to be roughly 40 log points higher—implying 40% average within-firm gaps in pay—to equalize conditional mean employment rates.

Second, bias-corrected slopes tend to be larger for the felony record sample than the non-felony sample. This difference, which is unaffected by the normalization of mean within-firm pay gaps, appears to emerge because employment is more strongly increasing in person effects in the left tail of the distribution, where more mass is concentrated for the felony sample. This difference could reflect either a universal pattern about the returns to skill for employment or the fact that a felony conviction erects stronger employment barriers for relatively low- vs. high-skill workers. Indeed, Figure A.6 shows that person effects are strongly negatively correlated with incarceration rates, implying that workers with low person effects may be making more frequent and potentially disruptive transitions in and out of prison.[22]

To summarize the share of employment gaps explained by person effects, we use bias-corrected regressions of employment rates on person effects to conduct a simple Oaxaca-Blinder decomposition. This decomposition is based off a record status-group specific linear regression of average employment rates onto person effects such that if $G_i = g$ indicates an individual with record status g and $\bar{e}$ is their average employment rate, $\bar{e}_i = \gamma_g + \beta_g(\alpha_i + g\tilde{\alpha}_i) + \eta_i$.

[21] To correct for attenuation bias, we multiply the naive slope estimate from a regression of mean employment rates on mean person effects in each decile by the ratio of the unadjusted to bias-corrected estimates of the variances of person effects. This estimate is constructed separately for the record and non-record groups.

[22] Distinguishing between overall nonlinearities and record-specific returns to person effects for employment would require estimating the non-linear relationship between employment and person effects, which is difficult without access to additional instruments to correct for measurement error.

We bias-correct the OLS slope estimate for each group (and therefore the constants γ_g as well) using the signal-to-noise ratio for person effects from Table 2. The resulting decomposition is:

$$
\begin{aligned}
E[\bar{e}_i|G_i=0] - E[\bar{e}_i|G_i=1] = & \underbrace{\beta_0 \left(E[\alpha_i|G_i=0] - E[\alpha_i|G_i=1]\right)}_{\textit{pre-record person effects}} \\
& - \underbrace{\beta_0 E[\tilde{\alpha}_i|G_{it}=1]}_{\textit{changes in person effects}} \\
& + \underbrace{E[\alpha_i + \tilde{\alpha}_i|G_{it}=1](\beta_0 - \beta_1)}_{\textit{returns}} + \underbrace{\gamma_0 - \gamma_1}_{\textit{gap in constants}}
\end{aligned} \tag{7}
$$

The first term reflects the share of employment gaps attributable to differences in pre-record person effects. The second term captures the effect of changes in person effects post-record, which as noted above, we compute assuming zero within-firm gaps in for this exercise. The final terms reflect components unexplained by differences in person effects.

The results presented as text on Figure 3 show that pre-record person effects explain a meaningful portion of the employment gap: roughly 30%. Accounting for changes in earnings potential post record increases the share explained to 37%. However, the majority of the gap remains unexplained in both case, suggesting that individuals with a record continue to face significant barriers to employment.

Panel B of Figure 3 presents the same analysis as Panel A for workers with any criminal record vs. those with no record. The employment gap between these groups is roughly half the size of the gap in Panel A. Bias-corrected slopes are steeper for the any record sample than the non-record sample, although not as steep as the slope for the felony record sample in Panel A. Differences in person effects account for a larger share of the total employment gap in Panel B vs. Panel A. Still, substantial residual gaps—more than half of the total—remain.

7 Conclusions

Why do workers with a criminal record earn so little compared to similar workers without one? One plausible explanation is that having a record limits the availability of high-paying jobs. The labor market restrictions of a record are codified into law at the state or federal level, as well as in occupational licensing requirements. Other restrictions operate implicitly through employer discrimination motivated by concerns about legal liability, worker produc-

tivity, or other factors. Because many workers with a record face severe disadvantage from an early age, however, it is unclear how the jobs available to workers with a record differ from those they would have been able to access without one.

This paper studied this question using merged administrative earnings and employment and criminal history data. We find little evidence that job quality, as measured by firm pay premia estimated in two-way fixed effect models, explains earnings gaps between workers with vs. without a record. This finding holds in the cross-section comparing all workers with and without a record, as well as in a difference-in-differences framework that examines changes in firm premia after the first conviction relative to matched controls. Instead, the amount workers would earn at *any* firm declines sharply post-conviction. And conditional on earnings potential, workers with a record tend to be employed much less frequently.

These results imply that the labor market penalty of a criminal record primarily arises through whether a worker finds a job at all and how much any firm would pay them, rather than where specifically they work. One interpretation is that criminal record restrictions are not tightly linked to job quality as measured by firm pay premia. Indeed, prior correspondence studies show that employers put significant weight on job applicants' criminal histories even in low value-added sectors such as fast food. While the industries that tend to employ workers with vs. without a record are meaningfully different, workers with records are also over-represented in some high premia industries such as construction. It is possible that after acquiring a record, workers predominately find work in jobs that pay roughly the same amount but have other unattractive amenities (e.g., outdoor physical labor).

The results also suggests that policies targeting overall employment and work in the types of jobs likely to hire workers with a record are more likely to decrease the record vs. non-record gap than policies seeking to reduce barriers in more prestigious and selective jobs these workers would be unlikely to access even without a record. To the extent that the post-conviction decline in earnings potential at every firm reflects decreases in productivity, the results point to a role for policy in mitigating skill depreciation around a criminal justice interaction. On the other hand, if these declines instead reflect pay discrimination or common patterns of within-firm job sorting, the results suggest that challenges in internal labor markets and pay policies, rather than barriers in overall access to the firm, are a key determinant of record penalties.

Tables and figures

Table 1: Summary statistics

	Full sample (1)	Felony (2)	No felony (3)	Any record (4)	No record (5)
Criminal justice interaction					
Any felony conviction	0.06	1.00	-	0.40	-
Any criminal conviction	0.15	1.00	0.09	1.00	-
Share of quarters with any incar.	0.01	0.10	0.00	0.04	0.00
Demographics					
Age	29.01	29.71	28.97	29.44	28.94
White	0.78	0.71	0.78	0.75	0.79
Black	0.13	0.23	0.12	0.19	0.11
Asian	0.04	0.01	0.05	0.01	0.05
Multiple races	0.02	0.02	0.02	0.02	0.02
Education					
Less than HS	0.19	0.25	0.19	0.24	0.19
Only HS	0.31	0.35	0.30	0.34	0.30
Some college	0.31	0.29	0.31	0.30	0.31
College or more	0.19	0.10	0.20	0.12	0.21
Labor market outcomes					
Total quarterly earnings	10,550	7,046	10,780	8,210	10,960
Log earnings	8.88	8.33	8.91	8.55	8.93
Share of quarters with any earnings	0.73	0.63	0.74	0.69	0.74
Share of quarters with earnings > $3800	0.61	0.44	0.62	0.53	0.63
Share of sandwich quarters	0.52	0.35	0.53	0.43	0.54
Average earnings of co-workers	10,450	8,088	10,600	8,869	10,730
Average quarters in sample	42.45	43.26	42.40	42.78	42.39
Number of workers ('000)	8226	646	7774	1360	7262
Number of firms ('000)	1097	349	1078	522	1046
Number of person-quarters ('000)	139000	8297	131000	20500	118000

Notes: This table presents summary statistics for the full sample (column 1), workers with a felony conviction vs. not (columns 2 and 3), and workers with any recorded criminal record vs. not (columns 4 and 5). All statistics are person-weighted. Demographic and education data are taken from the LEHD individual characteristics files. Labor market data refers to earnings and employment outcomes recorded in all states where this project has LEHD access. "Sandwich" quarters refers to quarters with earnings above $3,800 and with positive earnings from the same employer in the preceding and subsequent quarter. Average quarters in sample refers to the average total number of quarters over which each worker was observed between ages 22 and 62 in years where we have LEHD coverage (see table B.1 for details). All dollar values are inflated to 2020 equivalents using the CPI.

Table 2: Distributions of estimated person effects, firm effects, and within-firm gaps

	Felony record sample		Any record sample	
	Estimate (1)	Share of variance (in %) (2)	Estimate (3)	Share of variance (in %) (4)
A. No record				
$Var(w_{it})$	0.284	100.0	0.285	100.0
$Var(\psi_{j(i,t)})$	0.059	20.9	0.059	20.8
bias-corrected	0.052	18.4	0.052	18.1
$Var(\alpha_i)$	0.134	47.3	0.136	47.8
bias-corrected	0.115	40.4	0.116	40.8
$Cov(\psi_{j(i,t)}, \alpha_i)$	0.014	10.0	0.014	9.8
bias-corrected	0.020	13.8	0.020	13.8
B. Has record				
$Var(w_{it})$	0.233	100.0	0.249	100.0
$Var(\psi_{j(i,t)})$	0.058	24.9	0.060	24.0
bias-corrected	0.050	21.5	0.051	20.3
$Var(\alpha_i + \tilde{\alpha}_i)$	0.115	49.2	0.115	46.3
bias-corrected	0.061	26.0	0.081	32.6
$Cov(\psi_{j(i,t)}, \alpha_i + \tilde{\alpha}_i)$	0.016	13.9	0.017	13.3
bias-corrected	0.016	13.9	0.017	13.3
C. Within-firm gaps				
$Var(\chi_{j(i,t)})$	0.041	17.7	0.029	11.6
bias-corrected	-0.008	-3.5	-0.001	-0.5
$Cov(\psi_{j(i,t)}, \chi_{j(i,t)})$	-0.022	-18.8	-0.008	-6.1
bias-corrected	0.011	9.0	0.002	1.4
$Cov(\alpha_i + \tilde{\alpha}_i, \chi_{j(i,t)})$	-0.022	-18.8	-0.012	-9.5
bias-corrected	0.011	9.0	0.004	3.2

Notes: This table presents estimates of the variances and covariances of log earnings and firm and person effects from estimates of the model in Equation 3. The model is estimated separately treating G_{it} as an indicator for have a prior felony conviction (columns 1 and 2) and treating it as an indicator for having any criminal record (columns 3 and 4). All estimates are worker-quarter weighted. For variance estimates, the share of variance is defined as the variance of effects divided by the variance of log wages (e.g., $Var(\psi_{j(i,t)})/Var(w_{it})$). The share of the variance of the covariance terms is defined as twice the covariance divided by the variance of log wages (e.g., $2Cov(\psi_{j(i,t)}, \alpha_i)/Var(w_{it})$).

Table 3: Conditional mean log earnings decompositions

	No felony vs. felony		No record vs. record	
	Estimate (1)	% of gap (2)	Estimate (3)	% of gap (4)
A. Baseline gaps				
Log earnings	0.241	100%	0.168	100%
Person effects (pre-record)	0.159	66%	0.129	77%
Firm effects	0.027	11%	0.015	9%
Change in earnings potential	0.054	23%	0.025	15%
B. Demographics re-weighted				
Log earnings	0.218	100%	0.146	100%
Person effects (pre-record)	0.138	64%	0.109	75%
Firm effects	0.025	12%	0.012	8%
Change in earnings potential	0.054	25%	0.025	17%
C. Industry, geography, demographics re-weighted				
Log earnings	0.169	100%	0.107	100%
Person effects (pre-record)	0.093	55%	0.067	62%
Firm effects	0.022	13%	0.016	15%
Change in earnings potential	0.054	32%	0.025	23%
D. Occupation, geography, demographics re-weighted				
Log earnings	0.158	100%	0.091	100%
Person effects (pre-record)	0.087	55%	0.067	74%
Firm effects	0.022	14%	0.006	7%
Change in earnings potential	0.052	33%	0.021	23%

Notes: This table presents decompositions of the log earnings gap between workers without a felony conviction vs. with one (columns 1 and 2) and between workers with no record vs. any record (columns 3 and 4) after accounting for observable characteristics. The estimates are constructed in the same way as in Table 1, except the non-record group is re-weighted to mirror the record group's distribution of observables. Panel A reproduces the baseline gaps. Panel B re-weights based on the interaction of education and year of birth. Panel C adds the interaction of geography (Core-Based Statistical Area of employer) and two-digit industry, while Panel D uses occupation instead of industry. Occupation codes are added by matching the sample to proximate ACS responses (i.e., a response to the ACS within eight quarters of the relevant quarter in the LEHD). The sample in Panel D conditions on having a valid ACS match, and so is subsample of the full sample.

Table 4: Earnings gaps between workers in restricted vs. unrestricted occupations

	Any restriction (1)	Any mandatory restriction (2)	Any mandatory felony restriction (3)
Gaps among workers without a criminal record			
Log earnings			
Level	0.02647	0.04848	0.04925
(% of gap)	100%	100%	100%
Person effects			
Level	0.03041	0.0682	0.07384
(% of gap)	115%	141%	150%
Sorting			
Level	-0.01932	-0.0395	-0.0422
(% of gap)	-73%	-81%	-86%
Employment share in restricted job			
No felony record	0.2069	0.1263	0.0772
Felony record	0.2063	0.0895	0.0451

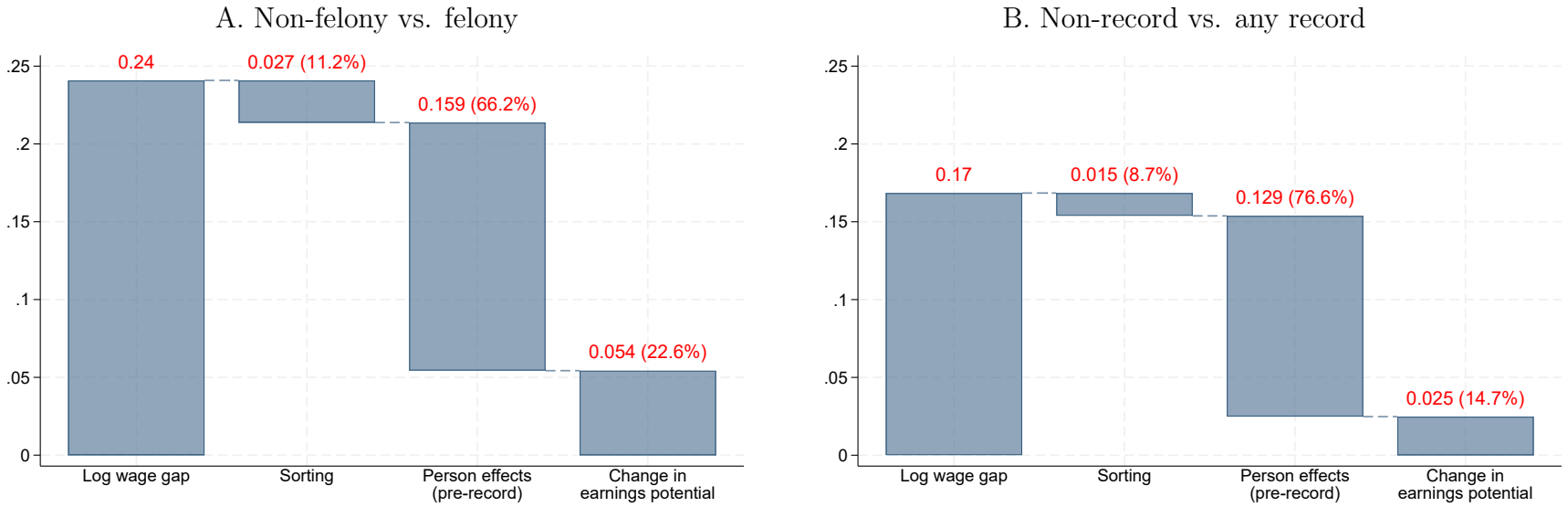
Notes: This table presents decompositions of the log earnings gap between workers in jobs with a legal record constraint and those in jobs without a constraint after accounting for observable characteristics. Column 1 classifies a job as restricted if the occupation-state combination appears in the NICCC data at all. Column 2 uses only mandatory restrictions. And column 3 uses only mandatory restrictions that apply to prior felonies. Only workers without a felony record are included in the decomposition. The sample of non-record workers is the same as Panel D 3, which adds occupation codes by matching workers to proximate ACS responses (i.e., a response to the ACS within eight quarters of the relevant quarter in the LEHD). Because only non-record workers are included, person effects and firm effects are the sole source of earnings gaps beyond covariates. The final two rows report the share of quarterly full-time employment in restricted jobs for the non-felony sample and the felony record sample.

Table 5: Quit and poaching rate gaps between workers with and without felony records

	Share of hires EE (1)	Quarterly quit rate (2)
No felony mean	0.820	0.165
Felony mean	0.807	0.221
Difference	-0.013	0.056
Between NAICS-2	-0.004	0.014
Share	31%	26%
Within NAICS-2	-0.009	0.042
Share	69%	74%

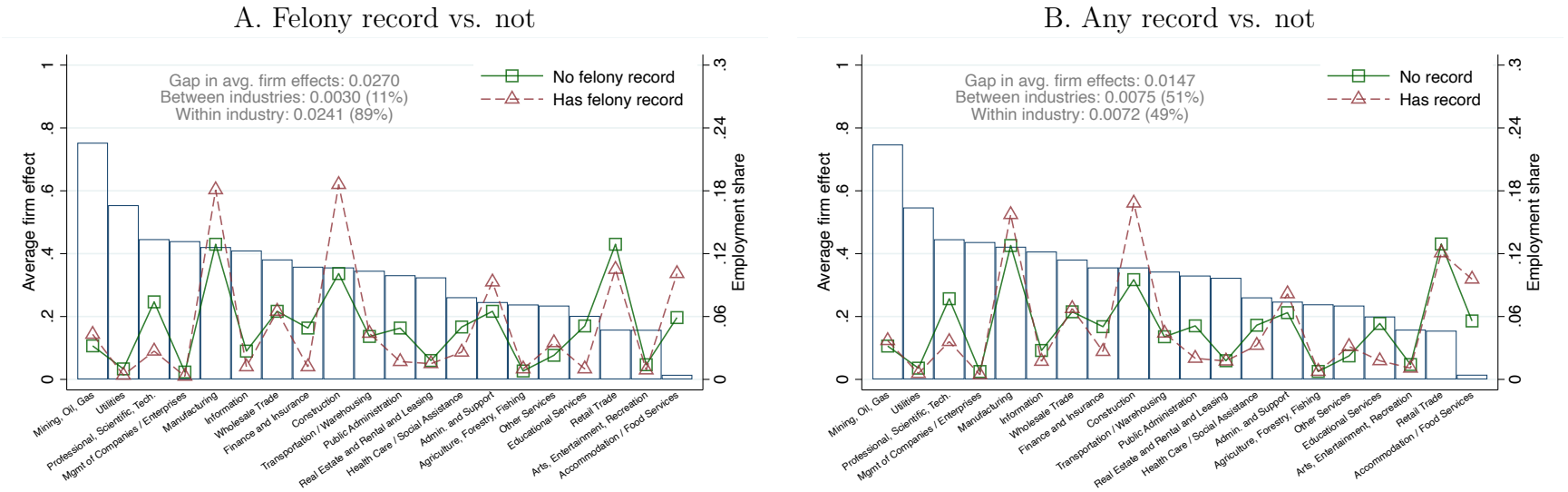
Notes: This table presents differences in average firm-level poaching rates (share of hires from other employers vs. non-employment) and quarterly quit rates for workers with a felony record vs. without. The first two rows present the means for each group. The third row presents the difference. The final rows decompose the difference into the component that persists within 2-digit NAICS industry vs. between industry.

Figure 1: Mean log earnings decompositions



Notes: This figure presents decompositions of the log earnings gap between workers without a felony conviction vs. with one (Panel A) and between workers with no record vs. any record (Panel B). Each decomposition is constructed using parameter estimates from Equation 3 applied to each type of record vs. no record comparison separately. Log earnings gaps are net of the estimated effects of covariates. Person effects refers to the first component in Equation 6 and captures differences in average person effects pre-record. This component is estimated by subtracting the average change in person effects pre- vs. post-record among workers where it is observed (i.e., $E[\tilde{\alpha}_i | \min_t G_{it} = 0, G_{it} = 1]$) from the average total person effect in the full record population (i.e., $E[\alpha_i + \tilde{\alpha}_i | G_{it} = 1]$). Firm effects refers to the sorting component in Equation 6, which captures differences in the employment share of workers across firm wage premia ψ_j. The change in earnings potential is the residual, which comprises either drops in person effects post record or increases in within-firm average pay gaps.

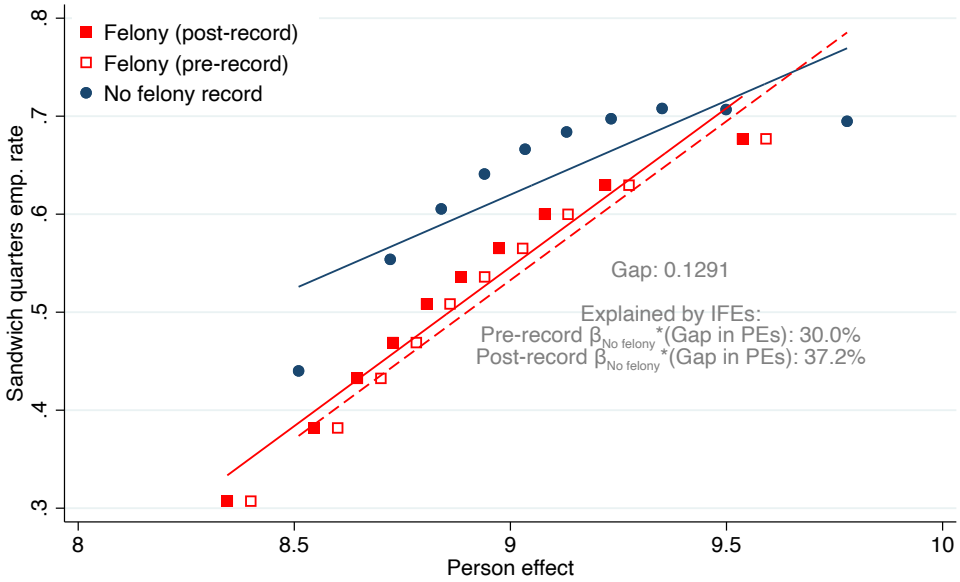
Figure 2: Industry wage premia and sorting patterns



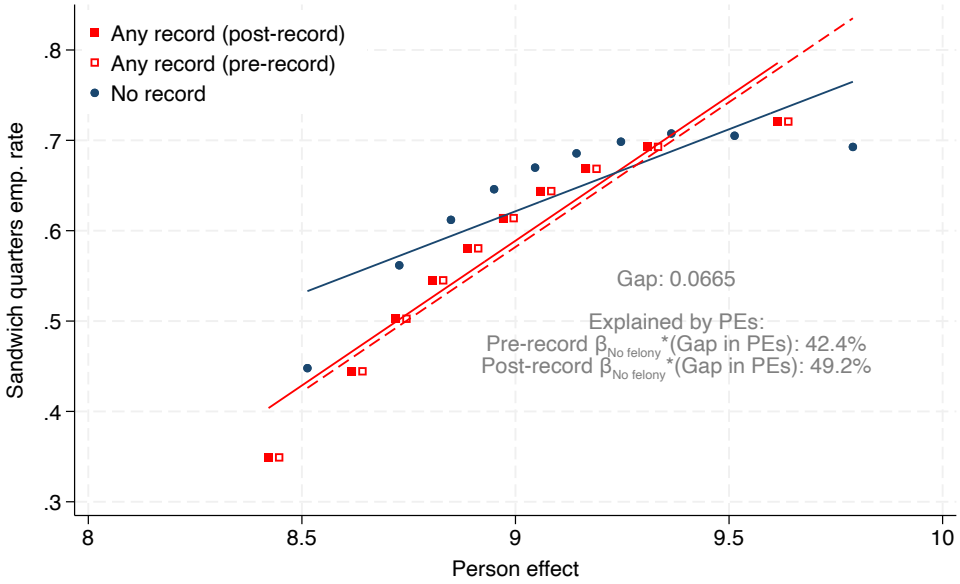
Notes: This figure presents industry average firm effects (ψ_j) from estimates of Equation 3. Industries are sorted from largest (left) to smallest (right), with their average firm effect (worker-quarter weighted using the non-record sample in each panel) on the left y-axis. The right y-axis reports the share of worker-quarter observations in each industry by group. Panel A reports results for workers with a felony record vs. workers without a felony record, while Panel B shows reports results for workers with any criminal record vs. those without a record.

Figure 3: The link between employment and person effects by criminal record status

A. Felony record



B. Any record



Notes: This figure plots average worker-level employment rates in "sandwich" quarters against estimated person effects by record status. A sandwich quarter is a quarter with earnings > \$3,800 and positive earnings from the same employer in the preceding and subsequent quarters. Panel A shows results for the felony record vs. non-felony record samples, while Panel B reports results for the any record comparison. Each dot reflects means after grouping workers into deciles based on their estimated person effects within each record group. For the record samples, the hollow dots capture pre-record person effects α_i computed using the strategy discussed in Section 3. The solid dots capture post-record person effects $\alpha_i + \tilde{\alpha}_i$ assuming that average within-firm pay gaps are zero. The lines are OLS slopes fitted to the reported means. The text reports the overall gap in employment rates and the share explained by the gap in person effects using the bias-corrected slope of employment on person effects for the non-record sample. The correction is computed by dividing the OLS slope by the signal-to-noise ratio of person effects from Table 2.

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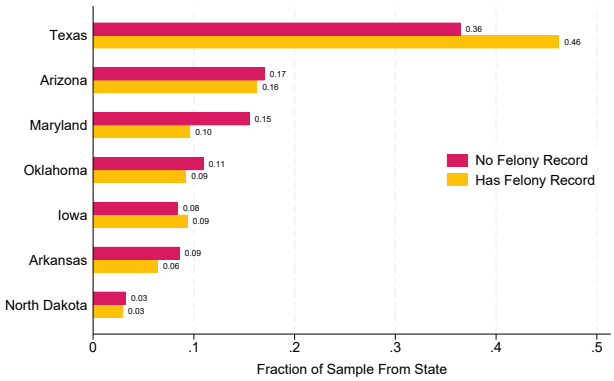
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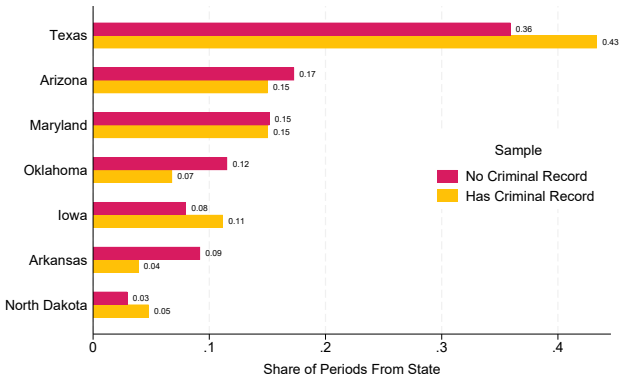
A Appendix figures

Figure A.1: Distribution across states by record status

A. Felony vs. no felony

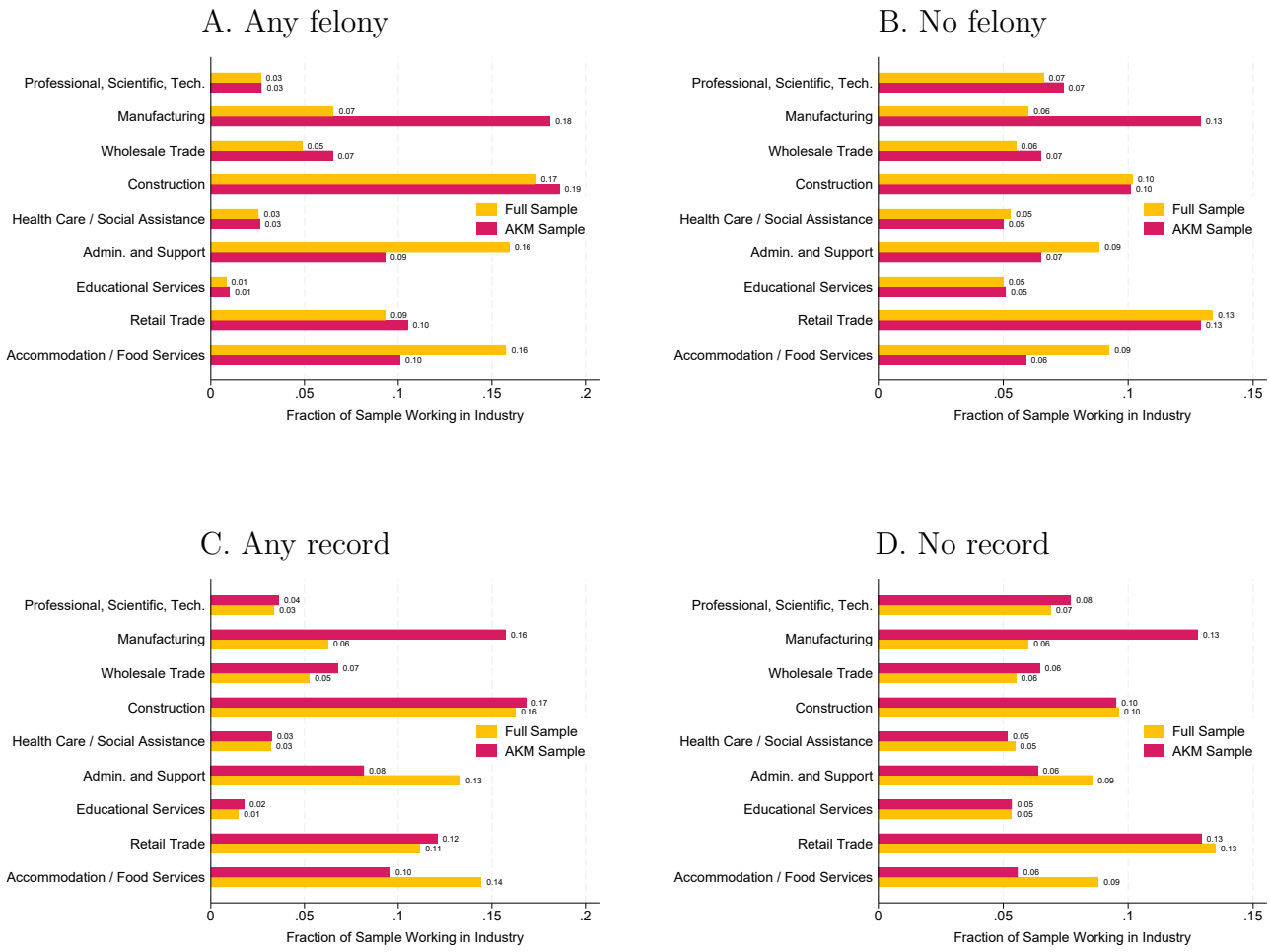


B. Any record vs. no record



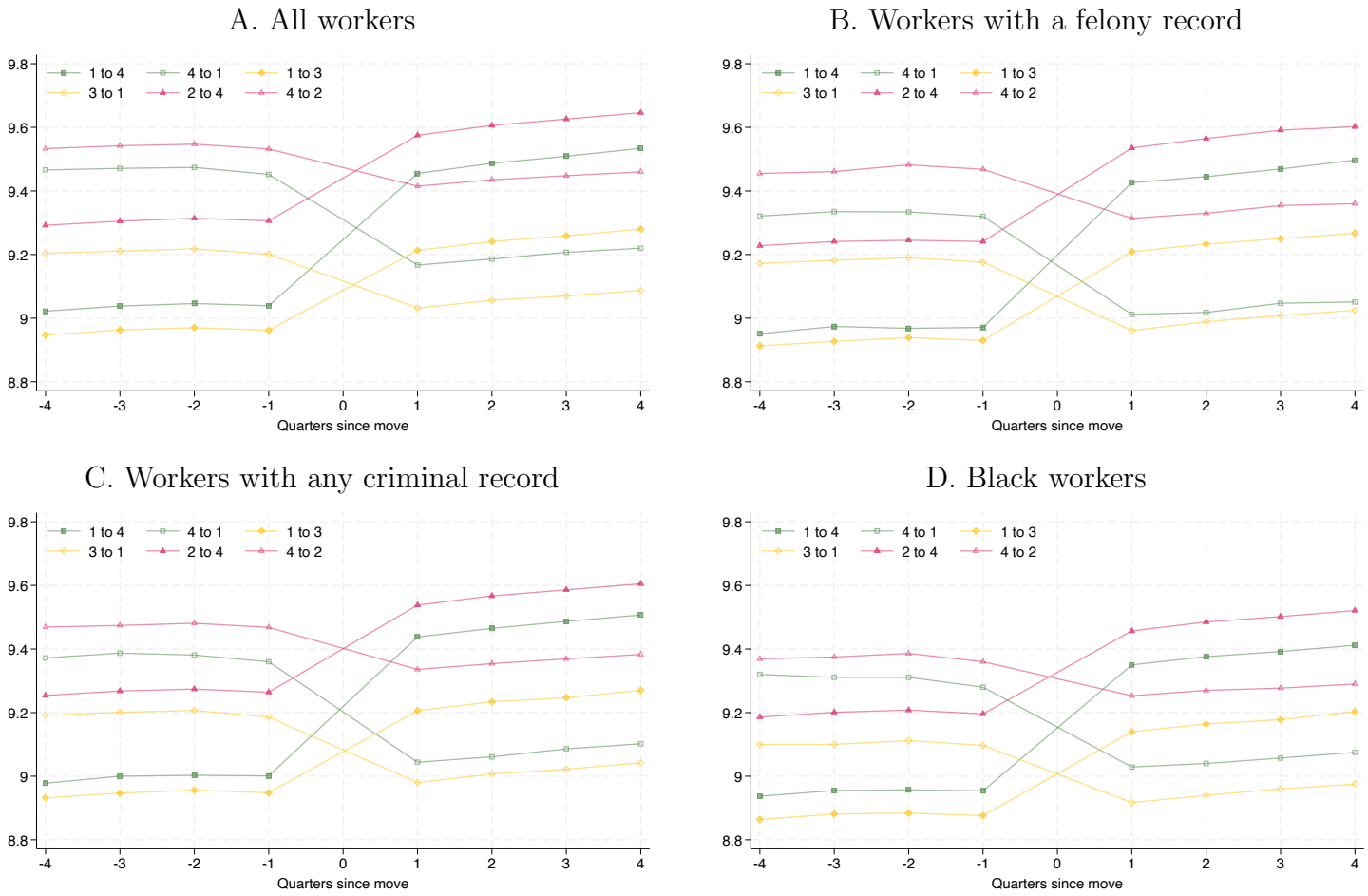
Notes: This figure presents the distribution of observations across states in by record status.

Figure A.2: Comparison of industry composition in full and AKM samples by record status



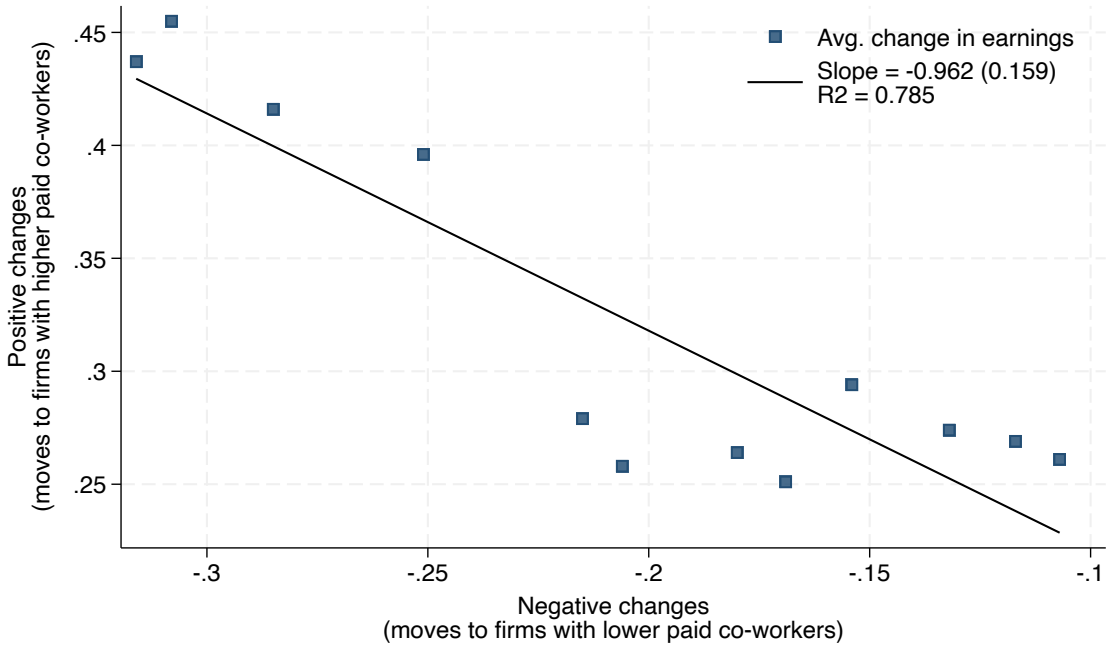
Notes: This figure presents the industry distribution in the full sample and in the sample used to estimate the AKM models by record status.

Figure A.3: Event studies around a worker's transition across firms



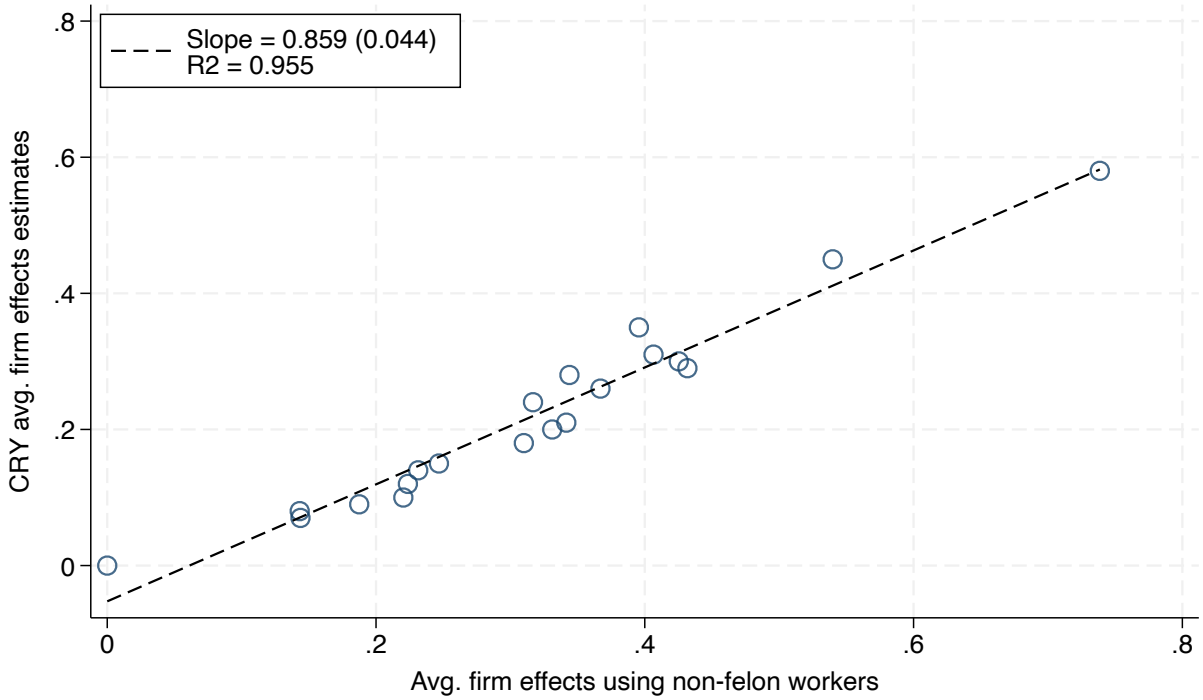
Notes: The figure plots the average log earnings of movers between firms classified by quartile of average co-worker log earnings. Quarter $t = -1$ refers to the last full quarter of employment (i.e., "sandwich" quarter) at the origin firm, while quarter $t = +1$ is the first sandwich quarter at the destination firm. We allow for up to four quarters of non-employment between these two employment spells. As is common in the literature (e.g., Card, Rothstein and Yi, 2023*b*), the movers sample is a subset of the full sample but restricted to workers employed at the same firm for at least four consecutive quarters before and after the move.

Figure A.4: Symmetry in moves between higher and lower paying firms



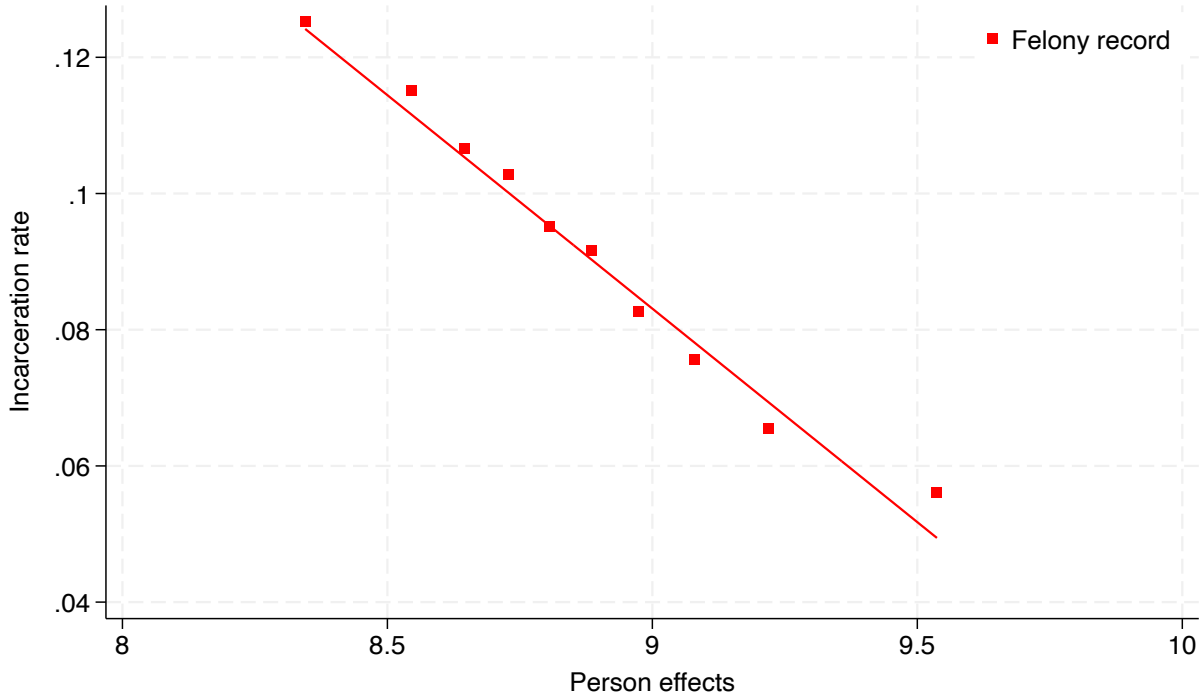
Notes: The figure plots the change in average log earnings at the time of the move (i.e., between quarters $t = -1$ and $t = 1$ in Figure A.3) of workers who move between firms, classified by the quartile of average co-worker log earnings at both the origin and destination firms. The figure includes estimates from all the groups (i.e., Panels A to D of Figure A.3). This figure visualizes the estimates in Figure A.3 differently to illustrate the symmetry in changes in earnings between switches upward to higher-paying firms and downwards to firms with lower average co-worker pay. Perfect symmetry implies all dots should fall on a line with a slope of -1, up to sampling error.

Figure A.5: Comparison of industry average firm effects to estimates from Card, Rothstein and Yi (2023*a*)



Notes: This figure compares our estimates of average firm effects for workers without a felony records by two-digit NAICS codes to estimates from Card, Rothstein and Yi (2023*a*), who estimate similar models using the full sample of LEHD states. All dollar values are inflated to 2020 equivalents using the CPI.

Figure A.6: The relationship between incarceration rates and person effects



Notes: This figure shows the relationship between estimated person effects and the share of quarters with any incarceration among workers with a felony record. The x-axis shows the average person effect in each decile of the person effects distribution. Person effects are computed assuming zero average within-firm pay gaps. The y-axis shows probability of having any incarceration in a given quarter.

B Appendix tables

Table B.1: Sample description

State	Source of CJ data	Min. birth year	LEHD start year	LEHD end year
Arizona	Both	1978	2005	2019
Arkansas	Author collected	1974	2005	2018
Iowa	Author collected	1979	2005	2019
Maryland	CJARS	1974	2005	2017
North Dakota	CJARS	1977	2005	2019
Oklahoma	Both	1977	2005	2019
Texas	Both	1976	2005	2013

Notes: This table summarizes the cohorts and periods restrictions used for the seven LEHD approving states used to construct the analysis sample. Workers observed working over the period listed in one of these states and born after the listed minimum birth year are included in the sample. Workers observed in multiple states are required to satisfy the most conservative birth year restriction.