How Much Should We Trust Estimates of Firm Effects and Worker Sorting?

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Many studies use matched employer-employee data to estimate a statistical model of earnings determination with worker and firm fixed effects. Estimates based on this model have produced influential yet controversial conclusions. The objective of this paper is to assess the sensitivity of these conclusions to the biases that arise

because of limited mobility of workers across firms. We use employer-employee data from the United States and several European countries while taking advantage of both fixed effects and random effects methods for bias correction. We find that limited mobility bias is severe and that bias correction is important.

I. Introduction

Abowd, Kramarz, and Margolis (1999) proposed a statistical model that uses employer-employee data to quantify the contributions of workers and firms to earnings inequality. In the Abowd-Kramarz-Margolis (AKM) model, log earnings are expressed as a sum of worker effects, firm effects, covariates, and idiosyncratic error terms. Abowd, Kramarz, and Margolis (1999) showed how to estimate worker and firm effects using a fixed effects (FE) estimator. The resulting estimates can then be used to decompose the variance of log earnings into the contributions of worker heterogeneity, firm heterogeneity, and sorting of high-wage workers to high-paying firms.

Over the past two decades, the AKM model and FE estimator have been frequently used to analyze earnings inequality in many developed countries.[1] This work has produced several influential yet controversial conclusions, summarized in the review article by Card et al. (2018). One key conclusion is that firm-specific wage settings are important for earnings inequality. Card et al. (2018, S15) concludes, "This literature also finds that firms play an important role in wage determination, with a typical finding that about 20% of the variance of wages is attributable to stable firm wage effects." Another key conclusion is that the correlation between firm and

[1] See, among many others, Gruetter and Lalive (2009), Mendes, van den Berg, and Lindeboom (2010), Card, Heining, and Kline (2013), Goldschmidt and Schmieder (2017), Card, Cardoso, and Kline (2016), Sorkin (2018), and Song et al. (2019). The AKM approach has also been widely used in contexts other than firms and workers, including teachers and students (e.g., Rockoff 2004), hospitals and patients (e.g., Finkelstein, Gentzkow, and Williams 2016), and banks and firms (e.g., Amiti and Weinstein 2018).

worker effects is often small and sometimes negative, indicating little if any sorting of high-wage workers to high-paying firms. At the same time, evidence from Germany (Card, Heining, and Kline 2013) and the United States (Song et al. 2019) indicates that worker sorting has been increasing over time, driving much of the rise in earnings inequality in these countries.

These empirical findings have been important, not only for quantifying the sources of earnings inequality but also for how economists model the labor market. For example, if firm effects are a key source of inequality, then it is natural to ask why similar workers are paid differently. Indeed, the evidence of significant firm effects was instrumental in the development of labor market models with frictions (Mortensen 2003). Furthermore, if better workers do not sort to more productive firms, then one might question the empirical importance of production complementarities for the matching of workers and firms (Shimer and Smith 2000; Eeckhout and Kircher 2011).

Motivated by the importance of the findings from Abowd, Kramarz, and Margolis (1999), we ask the following question: how much should we trust the FE estimates of firm effects and worker sorting? We focus on the problem of estimation, taking as given the AKM model. In particular, we assume that mobility is conditionally exogenous given worker and firm effects, and we rule out the presence of dynamics and worker-firm complementarities. Other work has examined and relaxed these assumptions (e.g., Abowd, McKinney, and Schmutte 2019; Bonhomme, Lamadon, and Manresa 2019).[2] Our goal is to assess the sensitivity of the FE estimator to the incidental parameter problem that arises in the AKM model, often referred to as "limited mobility bias."

Limited mobility bias is due to the large number of firm-specific parameters that are solely identified from workers who move across firms. Abowd et al. (2004) and Andrews et al. (2008, 2012) highlighted this problem, and the simulations reported in Andrews et al. (2008) suggest that the bias can be substantial. If firms are weakly connected to one another because of limited mobility of workers across firms, FE estimates of the contribution of firm effects to wage inequality are biased upward, while FE estimates of the contribution of the sorting of workers to firms are biased downward.

Even though researchers have been aware of limited mobility bias for nearly two decades, very few papers have used methods for bias correction. None of the empirical papers in the survey by Card et al. (2018) correct for limited mobility bias.[3] There could be a variety of reasons for this. As Card

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[2] In addition, recent work has studied worker sorting with two-sided heterogeneity using different approaches (e.g., Borovickova and Shimer 2017; Hagedorn, Law, and Manovskii 2017; Lentz, Piyapromdee, and Robin 2017; Bagger and Lentz 2019).

[3] Several papers published since Card et al. (2018) did not use bias correction (see, e.g., Sorkin 2018; Song et al. 2019; Gerard et al. 2021). Notable exceptions include Kline, Saggio, and Sølvsten (2020) and Lachowska et al. (forthcoming), who apply fixed effects methods for bias correction to data from two regions of Italy and one

et al. (2018) point out, bias correction necessarily involves making potentially restrictive assumptions about the model. In addition, exact computation of fixed effects corrections could be costly and possibly prohibitive in large data sets. As a result, there is no consensus yet about the magnitude of the biases and how they might alter conclusions about labor markets and inequality.

To investigate the importance of limited mobility bias, we use a variety of data sets and methods. Empirically, we study matched employer-employee data from Austria, Italy, Norway, Sweden, and the United States. These countries have different wage structures and labor market institutions. By comparing the results across countries, we shed light on whether our findings are specific to the United States or common across several Western economies that could potentially differ in the importance of firm-specific wage setting and the patterns of worker mobility across firms.

Methodologically, we take advantage of the availability of econometric techniques for bias correction. We implement fixed effects methods for bias correction, originally proposed by Andrews et al. (2008) and developed further by Kline, Saggio, and Sølvsten (2020). In addition, we propose a correlated random effects (CRE) method that builds on Woodcock (2008) and Bonhomme, Lamadon, and Manresa (2019). There are advantages and disadvantages to both the random effects and the fixed effects methods for bias correction. By comparing the results across the methods, we learn whether the conclusions about limited mobility bias are sensitive or robust to the alternative approaches to bias correction. To improve researcher accessibility to bias correction methods, we have released a comprehensive, user-friendly software package for implementing all of the bias correction methods shown in the paper at https://github.com/tlamadon/pytwoway. It is written in Python but includes a Stata extension so that Stata users can perform the bias corrections as well.

Our analyses deliver several important conclusions for empirical work using the AKM model. First, we show in simulations based on real data that limited mobility bias is empirically important and that existing methods for bias correction perform well even as mobility becomes very limited. Second, in all of the countries we consider, we find that limited mobility bias is a major empirical issue for studies using FE to document firm effects and worker sorting. Once bias is accounted for, firm effects dispersion matters less for earnings inequality, and worker sorting becomes always positive and typically strong. Third, alternative methods for bias correction based on different assumptions tend to produce broadly similar results. This is

US state (Washington), as well as Bonhomme, Lamadon, and Manresa (2019) and Lamadon, Mogstad, and Setzler (2022), who have estimated linear and nonlinear models with discrete firm heterogeneity using data from Sweden and the United States. Here we develop a correlated random effects estimator and apply both fixed effects and random effects methods to data from a wide range of countries.

reassuring, as bias correction necessarily involves making restrictive assumptions about the model and/or limiting the set of firms under consideration.

To preview our estimates and put our results into perspective, figure 1 compares our bias-corrected estimates to existing FE estimates of the contribution from firm effects and worker sorting to wage or earnings inequality. We report FE estimates from previous studies in white bars. Then, for each of the five countries of study, we report estimates based on our CRE method using the firm grouping of Bonhomme, Lamadon, and Manresa (2019) in black and estimates based on the heteroskedastic fixed effects (FE-HE) method of Kline, Saggio, and Sølvsten (2020) in gray.[4] In figure 1*A*, we focus on the contribution of firm effects. The interquartile range of estimates of the variance of firm effects in previous studies is 15% to 25%, while the range of our bias-corrected estimates is 5% to 13% using CRE and 6% to 16% using FE-HE. In figure 1*B*, we shift attention to the contribution of sorting. The interquartile range of estimates of the contribution of sorting in previous studies is −2% to 18%, while the range of our bias-corrected estimates is 10% to 20% using CRE and 5% to 13% using FE-HE.

II. Data

For each country, we now discuss the data sources that we use before reporting specific sample selection rules resulting from data structure and variable availability. Next, we describe the procedure we use to harmonize the sample and variable definitions across countries.

A. Data Sources

1. United States

The US data are constructed by linking US Treasury Department business tax filings with worker-level filings. Our sample spans 2001–15, and our main results focus on 2010–15. We express all monetary variables in 2015 dollars, adjusting for inflation using the consumer price index. Earnings data are based on taxable remuneration for labor services reported on form W-2 for direct employees. Earnings include wages and salaries, bonuses, tips, exercised stock options, and other sources of income deemed taxable by the Internal Revenue Service. These forms are filed by the firm on behalf of the worker and provide the firm-worker link. We exclude workers who are employed in the public or nonprofit sector by requiring that their

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[4] See table F1 (tables F1–F3 are available online) for a list of the 18 studies and 37 FE estimates used in this comparison. These estimates are directly comparable to those based on CRE, as both FE and CRE use the largest set of firms that are connected through at least one mover. We also report the results using FE-HE, which restricts attention to a subset of firms that remain connected after any given mover is removed from the sample (a leave-one-out connected component).

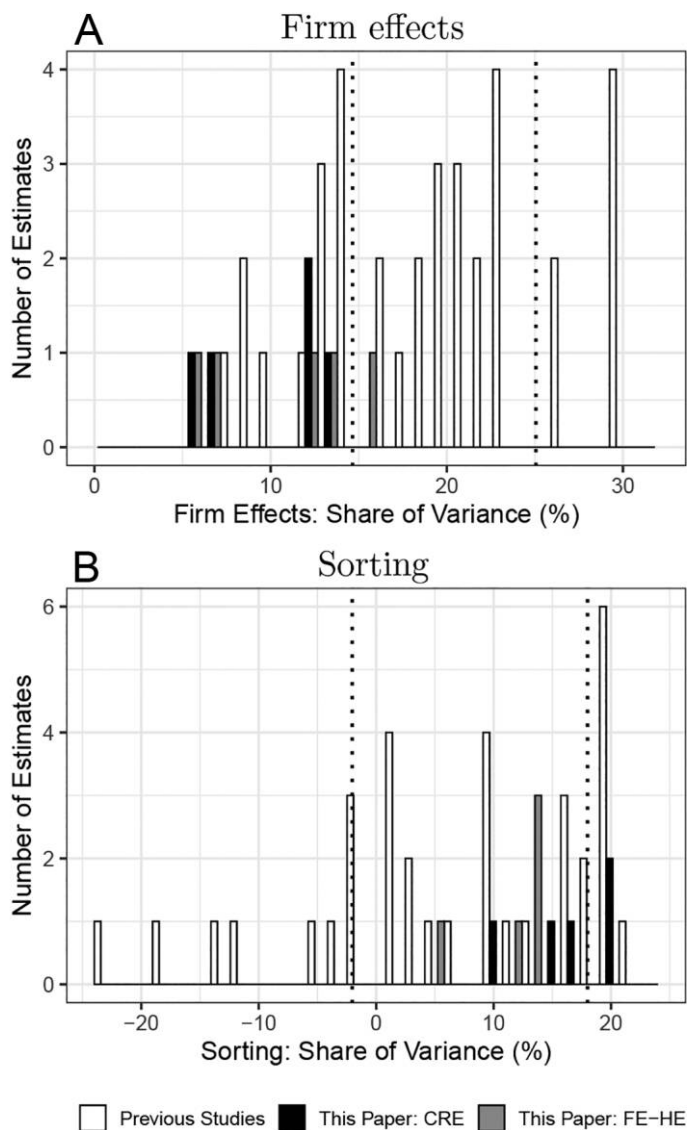Fig. 1.—Comparison to existing studies. FE estimates from previous studies are in white bars. CRE bias-corrected estimates from this paper based on the grouping of Bonhomme, Lamadon, and Manresa (2019) are in black. FE-HE bias-corrected estimates from this paper using the method of Kline, Saggio, and Sølvsten (2020) are in gray. The vertical dotted lines indicate the interquartile range of estimates in previous studies.

employers file tax form 1120 (C corporations), 1120S (S corporations), or 1065 (partnerships). In the US data, we do not observe any information about the duration of the spell within the year.[5] To construct a sample comparable to previous studies in other countries and in the United States, we apply a full-time equivalence earnings threshold, as we describe in detail in the next subsection.

2. European Countries

Each European country allows for the construction of a matched employer-employee data set with information on total annual earnings paid to each worker by each employer. This measure of earnings includes both direct wage payments and other sources of labor income. All data sources include information on the worker's age and gender. Countries differ in the level of detail regarding the duration of the employment spells as well as the calendar years over which data are available. In each country, we focus on 6-year panels in the main analysis and provide results from 3-year panels for comparison, and we adjust all monetary variables for inflation.

Austria.—The Austrian data, called the Arbeitsmaktdatenbank (AMDB), are co-constructed by the Austrian Labor Market Service and the Federal Ministry for Social Affairs, Health, Care and Consumer Protection using worker-level social security records. Our sample spans the years 2010–15. A similar vintage of these data have been used by Borovickova and Shimer (2017). For each job, the data include information on start and end dates as well as total annual earnings. Given this information, we construct the daily average wage as our main outcome of interest.

Italy.—The Italian data, known as the Veneto Worker Histories, were developed by the Economics Department at Università Ca' Foscari Venezia under the supervision of Giuseppe Tattara. These data are constructed by tracking all workers in the provinces Treviso and Vicenza even if they move to other provinces in Italy. Our sample spans 1996–2001. These data have been used, for instance, by Kline, Saggio, and Sølvsten (2020). For each job, it includes information on the number of days worked in the year and annual earnings. Given this information, we construct the daily average wage as our main outcome of interest.

Norway.—The Norwegian data come from the State Register of Employers and Employees, which covers the universe of workers and firms.

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[5] For this reason, our main analysis allows for workers to change employers during the year. However, we observe start and end dates of employment spells for the European countries and can use these dates to restrict the sample to full-year employees. Figure F1 (figs. F1–F16 are available online) presents results for the sample of workers that are employed by a single firm for the entire calendar year in each European country. The bias-corrected estimates are similar when using this alternate sample definition.

Our sample spans 2009–14. For each job, the data include information on start and end dates, annual earnings, and contracted hours. We construct the daily average wage as our main outcome of interest. Because the Norwegian data also provide hours worked per day, we construct the average hourly wage as a secondary outcome.

Sweden.—The Swedish data we use build on the sample from Friedrich et al. (2019), and we focus on 2000–2005. The employee-employer link is built from the Register-Based Labor Market Statistics (RAMS), with access provided by the Institute for Evaluation of Labour Market and Education Policy (IFAU). The data cover the universe of workers and firms, but the sample available to us is limited to employment spells of at least 2 months. The sample contains information about yearly earnings, employer identifiers and month of start and end of each spell. Given this information, we construct average monthly earnings as our main outcome of interest.

B. Sample Harmonization and Construction

To harmonize the data across countries, we apply five steps. First, as is common in the literature, whenever a worker is employed by multiple employers in the same year, we focus on the employer associated with the greatest annual earnings. Second, we restrict attention to workers employed in the private sector. Third, we restrict attention to workers who are between 25 and 60 years of age. Fourth, we adjust for differences in age and time by regressing the outcome measure on calendar year indicators and an age profile. We follow Card et al. (2018) in specifying the age profile as a third-order polynomial that is flat at age 40.

Last, we restrict attention to full-time equivalent (FTE) workers. Since we do not observe hours worked in US data or a formal measure of full-time employment, we follow Lamadon, Mogstad, and Setzler (2022) in defining a worker as FTE if annual earnings exceed $15,000, which is approximately the annualized minimum wage and corresponds to 32.5% of the national average. To harmonize the sample selection across countries, we similarly restrict the European samples to workers with annual earnings above 32.5% of the national average.[6] In section VI.B, we assess the sensitivity of the Norwegian estimates to using annual earnings (as in the United States), daily wages (as in Italy, Sweden, and Austria), and hourly wages as the outcome variable.

Given these harmonized samples, we prepare them for estimation by collapsing the annual observations over each 6-year panel into employment spells. Since we do not want to make assumptions about serial correlation

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[6] In fig. F2, we consider a range of FTE thresholds from $3,750 (about 25% of the annualized minimum wage) to $15,000 (about 100% of the annualized minimum wage). As shown in this figure, our findings about limited mobility bias are robust to the choice of FTE threshold.

within employment spells, we only use the mean log earnings within a spell, which is sufficient to construct our estimators of interest. This approach allows for partial-year employment when constructing spells. In figures F3 (for the United States) and F1 (for the European countries), we apply sample restrictions meant to capture only full-year employment in these spells, finding that the conclusions are unchanged. For workers who move across employers, we further reshape the spell data into an event study format that compares the spell-level log earnings or wage measures before and after a job change. A worker that does not move across employers has only one observation. This structure effectively reduces the data to the information needed for the identification of firm effects and sorting. See section A of the appendix (available online) for additional details.

C. Descriptive Statistics

We next present descriptive information about sample sizes, distributions of moves, and earnings or wage inequality. Table 1 provides descriptive statistics for the five countries we study. It characterizes the full population (first column under each country), the connected set (second column), and the leave-one-out set (third column). The bias correction methods recover variance components on these two sets, as we explain in the next two sections. These sets are constructed by computing the largest set of firms that are connected by at least one mover (connected set), and the largest set of firms that remain connected after any given mover is removed from the sample (leave-one-out set). The rows report information on the number of firms and workers, the distribution of the number of moves per firm, and certain moments of the distribution of log earnings or wages.

In table 1 and in our main analysis, a mover is defined as a worker that is employed by at least two different firms during the sample period. In figure F3, we consider a stricter mover definition in which a worker must be employed for at least three consecutive years at the first firm and at least three consecutive years at the second firm, only measuring earnings during intermediate years within these 3-year spells. This does not materially alter our conclusions. In section VI.B, we further discuss the impact of the definition of job movers on the results.

Table 1 highlights several key features of the data. First, we see that at least 93% of workers belong to the connected set in each country, and at least 87% belong to the leave-one-out set. By contrast, less than half of all firms belong to the connected set, and far fewer belong to the leave-one-out set. This indicates that within each country a large share of firms are very small, account for little of overall employment, and are not connected to other firms by movers. In section V, we further discuss the differences between the connected and leave-one-out sets.

Second, while each country has a large number of moves for the median firm, a substantial share of firms have a small number of moves. For example,

Table 1
Sample Characteristics

	Austria			Italy			Norway			Sweden			United States		
	FS	CS	LOOS	FS	CS	LOOS	FS	CS	LOOS	FS	CS	LOOS	FS	CS	LOOS
Set:															
Baseline years	2010–15			1996–2001			2009–14			2000–2005			2010–15		
Full set	✓	✗	✗	✓	✗	✗	✓	✗	✗	✓	✗	✗	✓	✗	✗
Connected set	✗	✓	✗	✗	✓	✗	✗	✓	✗	✗	✓	✗	✗	✓	✗
Leave-one-out set	✗	✗	✓	✗	✗	✓	✗	✗	✓	✗	✗	✓	✗	✗	✓
Sample counts (1,000):															
Unique firms	446	206	140	198	92	61	233	114	78	136	63	52	7,565	2,568	1,689
Share of full set (%)	100	46	31	100	47	31	100	49	34	100	46	38	100	34	22
Unique workers	3,582	3,396	3,240	1,188	1,111	1,034	1,379	1,286	1,199	1,979	1,921	1,850	59,621	55,464	52,484
Share of full set (%)	100	95	90	100	94	87	100	93	87	100	97	93	100	93	88
Distribution of moves:															
Moves per firm	2	5	8	2	4	6	2	5	7	4	10	11	2	6	8
Worker-weighted quantiles:															
10th quantile	4	4	5	3	3	4	3	3	4	4	5	6	3	4	5
50th quantile	52	51	56	22	22	25	26	26	29	77	77	82	56	58	67
90th quantile	605	605	629	313	311	326	397	399	420	2,354	2,352	2,484	4,214	4,304	4,676
Log earnings distribution:															
Variance	.195	.187	.182	.169	.167	.168	.241	.239	.236	.164	.164	.164	.413	.414	.416
Between-firm share (%)	43	46	44	46	46	45	47	47	46	31	32	31	40	40	39

NOTE.—This table displays descriptive statistics on the baseline panel data from the United States and four European countries. For each country, it provides information on the full set (FS), the connected set (CS), and the leave-one-out set (LOOS).

in the United States the majority of firms have at least 58 moves in the connected set and 67 moves in the leave-one-out set. However, 10% of firms have only four moves in the connected set and five moves in the leave-one-out set.

Third, while earnings or wage inequality varies substantially across countries, the between-firm share of variance tends to be more similar, ranging from 30% in Sweden to 45% in Austria and Italy. The between-firm component captures differences across firms in mean log earnings or wages. Thus, it may reflect firm effects or systematic heterogeneity in the workers that firms hire. To disentangle these two components, the AKM model takes advantage of workers moving across firms, as formalized in section III.

Before describing the AKM model and estimator, in figure F4 we present an event study of the earnings changes experienced by workers moving between different types of firms in the US sample. Following Card, Heining, and Kline (2013) and Card et al. (2018), we define firm groups on the basis of the average pay of coworkers. As in previous studies, we find that workers who move to firms with more highly paid coworkers experience earnings raises, while those who move in the opposite direction experience earnings decreases of similar magnitude, and that the gains and losses for movers in opposite directions between any two groups of firms seem fairly symmetric. By comparison, earnings do not change materially when workers move between firms with similarly paid coworkers. In addition, the earnings profiles of the various groups are all relatively stable in the years before and after a job move. This lends some support to the mobility assumption in the AKM model that workers do not select their firms on the basis of idiosyncratic earnings growth.

III. AKM Estimator and Limited Mobility Bias

In this section, we first describe the AKM estimator of Abowd, Kramarz, and Margolis (1999), and we then provide initial evidence on the presence of bias in the United States and Sweden.

A. Model, Estimator, and Biases

The AKM model is

$$Y_{it} = X_{it}'\beta + \alpha_i + \psi_{j(i,t)} + \varepsilon_{it}, \tag{1}$$

where Y_{it} are the log earnings of worker i in period t, X_{it} are exogenous covariates such as age or calendar time, α_i is the unobserved worker effect, $j(i,t)$ is the firm where i works at t, $\psi_{j(i,t)}$ is the unobserved firm effect, and ε_{it} is an idiosyncratic error term. We denote as N the number of workers, J the number of firms, and T the number of time periods. Following Abowd, Kramarz, and Margolis (1999), we assume that the following mean independence condition holds:

$$\mathbb{E}(\varepsilon_{it}|X_{11}, \ldots, X_{NT}, j(1,1), \ldots, j(N,T), \alpha_1, \ldots, \alpha_N, \psi_1, \ldots, \psi_J) = 0. \quad (2)$$

Throughout this paper, we assume that equation (2) holds in model (1), so the AKM model is correctly specified. This assumption allows for unrestricted dependence of job mobility on firm and worker effects. For instance, high-wage workers may be more likely to move to higher-paying firms than low-wage workers. However, assuming that shocks ε_{it} are mean independent of past and future firm indicators rules out endogenous mobility with respect to shocks and state dependence, which are important in dynamic models with wage posting or sequential bargaining. In addition, model (1) and equation (2) imply that the conditional mean of log earnings is additive in worker and firm effects. Additivity rules out interactions between worker effects α_i and firm effects $\psi_{j(i,t)}$ that may be economically relevant (e.g., Abowd, McKinney, and Schmutte 2019; Bonhomme, Lamadon, and Manresa 2019).

In this model, we focus on the contributions of firm effects and sorting in the following variance decomposition:

$$\text{Var}(Y_{it} - X'_{it}\beta) = \underbrace{\text{Var}(\alpha_i)}_{\text{worker effects}} + \underbrace{\text{Var}(\psi_{j(i,t)})}_{\text{firm effects}} + \underbrace{2\,\text{Cov}(\alpha_i, \psi_{j(i,t)})}_{\text{sorting}} + \underbrace{\text{Var}(\varepsilon_{it})}_{\text{residual}}. \quad (3)$$

We now describe the AKM estimator of the firm effects and sorting components in this decomposition.

The AKM (or FE) estimator treats $\alpha = (\alpha_1, \ldots, \alpha_N)'$ and $\psi = (\psi_1, \ldots, \psi_J)'$ as parameter vectors. It is convenient to write model (1) and equation (2) in vector form as

$$Y = X\beta + A\gamma + \varepsilon, \quad \mathbb{E}(\varepsilon|X, A, \gamma) = 0, \quad (4)$$

where Y and ε are $NT \times 1$, X is a matrix with NT rows, and A is a matrix with NT rows and $N + J$ columns.[7] The vector $\gamma = (\alpha', \psi')'$ includes worker and firm effects, and the matrix $A = [A_W \, A_F]$ depends on worker and firm indicators.

The slope parameter β can be estimated using ordinary least squares after partialing out worker and firm indicators.[8] For simplicity, in the presentation we treat β as known and redefine $Y_{it} - X'_{it}\beta$ as the outcome variable. That is, we work with the model

$$Y = A\gamma + \varepsilon, \quad \mathbb{E}(\varepsilon|A, \gamma) = 0. \quad (5)$$

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[7] Note that the conditioning on α and ψ in eq. (4) is not necessary here, since we are treating them as deterministic parameters. In random effects methods below we treat α and ψ as random.

[8] Formally, denote as $A^\dagger$ the Moore-Penrose inverse of A and as $M_A = I - AA^\dagger$ the residual "hat" projection matrix. The FE estimator of β is $\hat{\beta} = (X'M_A X)^{-1}(X'M_A Y)$. When $A'A$ is nonsingular, $M_A = I - A(A'A)^{-1}A'$; however, M_A remains well defined under singularity.

We start by assuming that $A'A$ is nonsingular. This requires working within a connected component of the firm-worker graph (Abowd, Creecy, and Kramarz 2002) and imposing one normalization on γ, for example, one of the firm effects being equal to zero. With some abuse of notation, we still denote as A the resulting selection of rows and columns of the A matrix, and we redefine N, J, and T appropriately. Then, the FE estimator of worker and firm effects is the least squares estimator

$$\hat{\gamma} = (A'A)^{-1}A'Y.$$

As in other studies using the AKM model, we are interested in the variance components in equation (3), such as the variance of firm effects $\mathrm{Var}(\psi_{j(i,t)})$ and the covariance between worker and firm effects $\mathrm{Cov}(\alpha_i, \psi_{j(i,t)})$. Variance components can be written as quadratic forms in γ; that is, $V_Q = \gamma'Q\gamma$ for some matrix Q. Note that Q typically depends on A, although we leave the dependence implicit in the notation. The FE estimator of V_Q is then

$$\hat{V}_Q^{\mathrm{FE}} = \hat{\gamma}'Q\hat{\gamma}.$$

To see that $\hat{V}_Q^{\mathrm{FE}}$ is biased, note that

$$\mathbb{E}[\hat{V}_Q^{\mathrm{FE}}] = V_Q + \underbrace{\mathbb{E}[\varepsilon'A(A'A)^{-1}Q(A'A)^{-1}A'\varepsilon]}_{=\mathrm{Bias}_Q}, \qquad (6)$$

where the expectations are conditional on A and γ. The expected FE estimator $\mathbb{E}[\hat{V}_Q^{\mathrm{FE}}]$ differs from the true variance component V_Q in general, due to the presence of the bias term Bias_Q. Note that the bias is due to V_Q being quadratic in γ. In contrast, the FE estimates $\hat{\gamma}$ of the level of worker and firm effects are unbiased under equation (4).

As explained by Andrews et al. (2008), the bias intuitively arises from an insufficient number of job movers in the firm. As a result of limited mobility bias, the FE variance of firm effects tends to be overstated. In turn, the covariance between worker and firm effects tends to be negatively biased, since worker effects and firm effects enter model (1) additively. Jochmans and Weidner (2019) show that the magnitude of the bias is inversely related to the degree of connectivity of the firm-worker graph. A limiting case is when the graph is disconnected (i.e., when $A'A$ is singular). Within a connected component, the bias can still be large when connectivity is weak. An implication of their analysis is that the structure of the bias is complex, since it depends on the (large) matrix A of worker and firm indicators. Hence, the magnitude of the bias is ultimately an empirical question.

B. Empirical Illustration of Limited Mobility Bias

To get a sense of the scope for limited mobility bias, an informal approach is to apply the FE estimator of Abowd, Kramarz, and Margolis (1999) to
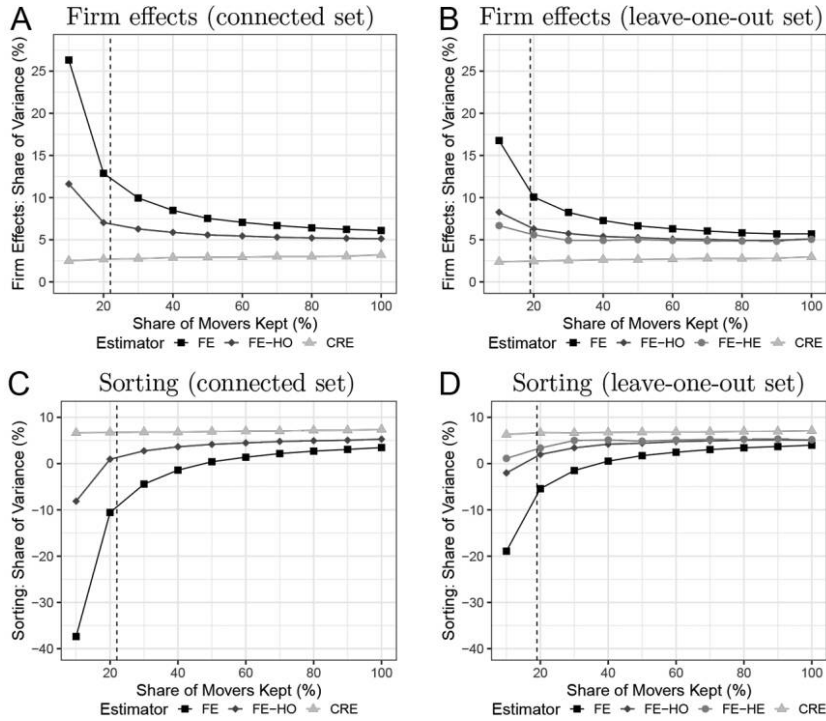
FIG. 2.—Evidence on limited mobility bias in Sweden. In this figure, we consider the subset of firms in Sweden with at least 15 movers. We randomly remove movers within each firm and reestimate the variance of firm effects and covariance between firm and worker effects using the various estimators. For each estimator, we repeat this procedure 20 times and then average the estimates across repetitions. The procedure allows us to keep the connected or leave-one-out set of firms the same and examine the bias that results from having fewer movers available in estimation. The vertical dashed line approximates the point at which movers per firm in this sample matches movers per firm in the full sample. A color version of this figure is available online.

alternative samples of workers and firms that are comparable except for the number of movers per firm. Figure 2 present the results from such an analysis for Sweden using a subsampling strategy inspired by Andrews et al. (2008, 2012).[9]

In figure 2, we randomly remove movers from firms while keeping the connected set of firms fixed in order to understand how the FE estimator responds to reduced worker mobility. To do so, we begin by considering the set of firms in Sweden with a relatively large number of movers, that is, at least 15 movers per firm over a 6-year period. Next, we remove movers

[9] See fig. F5 for a similar analysis for the United States.

randomly within each firm on the basis of a prespecified sampling probability, resulting in various subsamples in which mobility is more limited. Then we restrict each of these subsamples to the set of firms that belongs to the connected set when imposing the smallest sampling probability (which is 10% in practice). This ensures that the set of firms is kept fixed as we compare across samples.[10] Last, we apply the FE estimator to each simulated subsample. For completeness, we repeat this exercise for the leave-one-out connected set of firms.[11] For each simulated subsample, we also report bias-corrected estimates both for the CRE and the fixed effects methods. We discuss these bias-corrected estimates in section IV.

Figure 2*A* provides estimates of the contribution of firm effects to earnings inequality (i.e., $\mathrm{Var}(\psi_{j(i,t)})/\mathrm{Var}(Y_{it})$) for the connected set. Focusing on the FE estimates in the black line, we find that the variance of firm effects declines monotonically as the number of movers per firm increases. Consistent with limited mobility bias, the fewer the number of movers per firm, the larger the variance of firm effects. For the same set of firms, the estimated variance of firm effects is about twice as large (13%) if we keep only 20% of the movers within each firm (on average, four movers per firm) compared with the estimate of 6% we obtain if we keep all of the movers per firm (15 at a minimum and, on average, 45 movers per firm). By way of comparison, there are around 10 movers per firm in the full estimation sample, which roughly corresponds to the number of movers per firm when randomly keeping 22% of movers in the sample with originally 15 or more movers per firm, as indicated by a dashed vertical line. Figure 2*B* repeats this analysis for the leave-one-out set. The results are similar for the leave-one-out set, although FE is subject to less limited mobility bias, reflecting that the leave-one-out set has more movers per firm.

Figure 2*C* provides estimates of the contribution of worker sorting to earnings inequality (i.e., $2\mathrm{Cov}(\alpha_i, \psi_{j(i,t)})/\mathrm{Var}(Y_{it})$) for the connected set. Focusing again on the FE estimates in the black line, we find that the covariance between worker and firm effects increases monotonically as the number of movers per firm increases. For the same set of firms, the FE estimate of the contribution of worker sorting to earnings inequality is about 3% when we keep all movers per firm. However, if we keep only 20% of the movers within each firm, the FE estimates turn negative and large in magnitude. Figure 2*D* repeats this analysis for the leave-one-out set. The results are again broadly similar for the leave-one-out set.

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[10] In our working paper (Bonhomme et al. 2020), we do not impose this restriction, instead allowing the set of firms included in the connected set to become smaller as movers are removed, finding similar results.
[11] The FE-HE method for bias correction of Kline, Saggio, and Sølvsten (2020) recovers estimates of the variance components on the leave-one-out connected set.

IV. Bias Correction: Methods and Illustration

In this section, we describe the fixed effects and random effects methods we use for bias correction and illustrate the methods empirically.

A. Methods

Fixed effects.—Andrews et al. (2008) note that the bias in equation (6) can be written as

$$\text{Bias}_Q = \text{Trace}\big(A(A'A)^{-1}Q(A'A)^{-1}A'\Omega(A)\big),$$

where $\Omega(A) = \text{Var}(\varepsilon|A)$ is the covariance matrix of errors. Andrews et al. (2008) propose an estimator of the bias in the homoskedastic case, under the assumption that $\Omega(A) = \sigma^2 I$, for I, the identity matrix. Specifically, they construct

$$\widehat{\text{Bias}}_Q^{\text{FE-HO}} = \hat{\sigma}^2 \text{Trace}\big((A'A)^{-1}Q\big),$$

using an unbiased estimator of the variance.[12] Under homoskedastic, independent observations, $\widehat{\text{Bias}}_Q^{\text{FE-HO}}$ is unbiased for Bias_Q, so a bias-corrected estimator of V_Q is

$$\hat{V}_Q^{\text{FE-HO}} = \hat{V}_Q^{\text{FE}} - \hat{\sigma}^2 \text{Trace}\big((A'A)^{-1}Q\big).$$

In a recent contribution, Kline, Saggio, and Sølvsten (2020) propose a heteroskedastic generalization. Under the assumption that $\Omega(A)$ is diagonal, they estimate its diagonal elements, using the jackknife, as

$$\hat{\sigma}_{it}^2 = Y_{it}\big(Y_{it} - \hat{\alpha}_i^{-(i,t)} - \hat{\psi}_{j(i,t)}^{-(i,t)}\big),$$

where $\hat{\alpha}_i^{-(i,t)}$ and $\hat{\psi}_{j(i,t)}^{-(i,t)}$ are FE estimates on a subsample where observation (i, t) has been taken out. In particular, computing the estimator requires focusing on a leave-one-out set that remains connected when any (i, t) observation has been taken out. Hence, for this method the estimand changes relative to FE. Letting $\hat{\Omega}(A)$ be the diagonal matrix with diagonal elements $\hat{\sigma}_{it}^2$, the following estimator is unbiased under heteroskedastic, independent observations:

$$\hat{V}_Q^{\text{FE-HE}} = \hat{V}_Q^{\text{FE}} - \text{Trace}\big(A(A'A)^{-1}Q(A'A)^{-1}A'\hat{\Omega}(A)\big).$$

Kline, Saggio, and Sølvsten (2020) provide conditions under which $\hat{V}_Q^{\text{FE-HE}}$ is consistent, and they derive its limiting distribution.

When implementing these methods, we collapse observations at the spell level. This ensures that the above-described estimators are unbiased in

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[12] The estimator $\hat{\sigma}^2 = (NT - N - J)^{-1}Y'(I - A(A'A)^{-1}A')Y$ is unbiased for σ^2 when observations are independent and homoskedastic.

the presence of serial correlation within spell under homoskedasticity and heteroskedasticity, respectively.[13] However, in practice, exact computation of $\hat{V}_Q^{\text{FE-HO}}$ and $\hat{V}_Q^{\text{FE-HE}}$ requires computing the trace of a large matrix inverse, which is prohibitive in most samples we use. For this reason, our empirical implementations rely on approximation methods (Gaure 2014; Kline, Saggio, and Sølvsten 2020); see section VI and section B.1 of the appendix.

Random effects.—Random effects methods are popular in many panel data applications, yet they are rarely used in matched employer-employee settings. Here, we introduce a CRE estimator for variance components. Compared with fixed effects estimators, the CRE estimator requires modeling the means and covariances of worker and firm effects. However, CRE depends on a smaller number of parameters. This parsimony is helpful for computational tractability and to obtain more precise estimates.

Our starting point is the random effects specification in Woodcock (2008). Woodcock postulates that the conditional distribution of worker and firm effects $\gamma = (\alpha', \psi')'$ given worker and firm indicators A has mean $\mu(A)$ and variance $\Sigma(A)$.[14] In his specification, neither μ nor Σ depend on A, and Σ is diagonal. Woodcock uses this model as a prior for the worker and firm effects and computes posterior estimates. We relax this specification in two ways. First, we allow $\Sigma(A)$ to be nondiagonal. Second, we allow $\mu(A)$ and $\Sigma(A)$ to depend on A. It is important to observe that assuming that α and ψ are independent of A would be restrictive. For example, this would require mobility across firms not to depend on worker or firm effects.

To build a flexible specification, we allow $\mu(A)$ and $\Sigma(A)$ to depend on A by using the grouping strategy of Bonhomme, Lamadon, and Manresa (2019). Specifically, we cluster firms into K groups on the basis of their empirical earnings distributions. We use the k-means clustering algorithm for the grouping and use $K = 10$ in our baseline specification. Given this grouping, we allow the means and variances of worker and firm effects to depend on the groups but not on the worker and firm identities within these groups. Similarly, we allow the covariances in $\Sigma(A)$ to depend on the groups (or pairs of groups) while imposing some homogeneity assumptions so as to limit the number of parameters; see section B.2 of the appendix for a detailed description. The CRE model still has many fewer parameters than the AKM fixed effects model.

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[13] As pointed out by Kline, Saggio, and Sølvsten (2020), when $T = 2$, FE-HE estimators of firm effects and sorting components are also robust to the presence of serial correlation between spells. In the empirical analyses, we focus on 6-year panels and collapse earnings observations at the spell level (e.g., a stayer spell is collapsed into a single observation). See sec. B of the appendix for details. Without this collapsing approach, when using more than 2 periods of data, the FE-HE method is generally not robust to the presence of serial correlation within spells.

[14] The model in Woodcock (2008) also accounts for covariates, which we abstract from in the presentation.

We estimate the CRE parameters by minimum distance based on mean restrictions and cross-worker covariance restrictions that are linear in parameters, so implementation is straightforward. We describe the moment restrictions and provide details on the estimation strategy in section B.2 of the appendix. We report CRE estimates of the variance components,

$$\hat{V}_Q^{\text{CRE}} = \hat{\mu}(A)'Q\hat{\mu}(A) + \text{Trace}(\hat{\Sigma}(A)Q). \tag{7}$$

When the firm groups are defined in terms of observable categories such as industry or commuting zone, consistency of CRE follows from standard conditions for minimum distance. In addition, efficiency could be achieved using optimal weights. In our implementation, we tailor the groups to the data and construct them based on earnings using the k-means algorithm. In single-agent panel data models, Bonhomme, Lamadon, and Manresa (2022) provide conditions for consistency of k-means clustering and estimators based on the estimated clusters under continuous heterogeneity. Consistency requires K to tend to infinity with the sample size. We provide an analogous consistency argument for the AKM model in section C of the appendix. In the main analysis, we report results based on $K = 10$ groups. We document robustness with respect to this choice for a range of K.[15]

B. Empirical Illustration of Bias Correction

In figure 2, we illustrate empirically FE-HO, the homoskedastic fixed effects bias correction method of Andrews et al. (2008), as well as our CRE method based on the firm grouping of Bonhomme, Lamadon, and Manresa (2019) and compare them to FE in the Swedish data.[16] As described in section III, this figure considers the subsample of firms with at least 15 movers. Next, we remove movers randomly within firms before applying the FE, FE-HO, and CRE estimators to each random subsample, keeping the connected set of firms the same. We repeat this exercise for the leave-one-out set of firms, which allows us to also compare results to the FE-HE bias correction method of Kline, Saggio, and Sølvsten (2020).

Consider again figure 2A but now focusing on the lines for CRE and FE-HO. The FE estimator and the bias-corrected estimators are similar when including all movers per firm but become more dissimilar when there are

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[15] In addition, in some specifications we report posterior estimates in the spirit of empirical Bayes shrinkage. Interpreting our CRE model as a prior on the worker and firm effects and under additional Gaussianity assumptions, we compute posterior estimates of the variance of firm effects. This provides a useful check, since under correct specification CRE and posterior estimates should be similar; see sec. B.2 of the appendix for the formula for the posterior estimator of V_Q.

[16] See fig. F5 for a similar analysis for the United States.

fewer movers per firm. In contrast with FE, the two bias-corrected estimates remain nearly identical as the number of movers per firms declines, suggesting that the bias-corrected estimators are robust to the number of movers per firm. At the same time, the CRE estimates tend to be smaller than the FE-HO estimates. In figure 2B we repeat this analysis for the leave-one-out set, which allows us to include the FE-HE bias correction. In this case also, all three bias correction methods behave similarly, and in sharp contrast with FE, these estimators seem approximately insensitive to limited mobility bias.

Turning to figure 2C, focusing on the lines for CRE and FE-HO, we see that CRE and FE-HO bias-corrected estimates of the contribution of worker sorting to earnings inequality are also quite similar to each other, and the estimates do not vary much with the sample. In particular, bias-corrected estimates are always positive, while FE estimates in samples with few movers are negative. In figure 2D we repeat this analysis for the leave-one-out set, finding that the three bias-corrected estimators, now including FE-HE, behave quite similarly, albeit with some quantitative differences.

In the next sections, we report results based on both the fixed effects and the random effects methods for bias correction. The rationale for using a variety of methods is that they rely on different modeling strategies. While FE-HO and FE-HE involve a very large number of worker and firm fixed effects, CRE depends on a smaller number of parameters and therefore can be more precise. To illustrate this, figure 3 presents the range (whiskers) and the interquantile range (solid bar) of the estimates from the random draws of Swedish data. Whereas figure 2 presents the mean across random draws of the data, figure 3 presents the variability across these random draws.

The findings from figure 3 suggest that the CRE estimates of firm effects and worker sorting are less variable than those produced by the FE-HO and FE-HE estimates across the random draws of the data. For example, figure 3A considers the variability in the estimates of the contribution of firm effects to wage inequality in the connected set, finding much lower variation for the CRE than the FE and FE-HO estimators when a small share of movers is kept.

V. Empirical Findings

We now present results on firm effects and sorting for Austria, Italy, Norway, Sweden, and the United States. As described in section II, the sample selection and variable definitions are harmonized, to the extent possible, across countries. We compare firm effect and sorting estimates across bias correction methods and samples. Given that some studies have used relatively short panels with no more than 3 years (e.g., Kline, Saggio, and Sølvsten 2020) while others have used longer panels with at least 6 years (e.g., Song et al. 2019), we present results for both 3-year and 6-year panels.
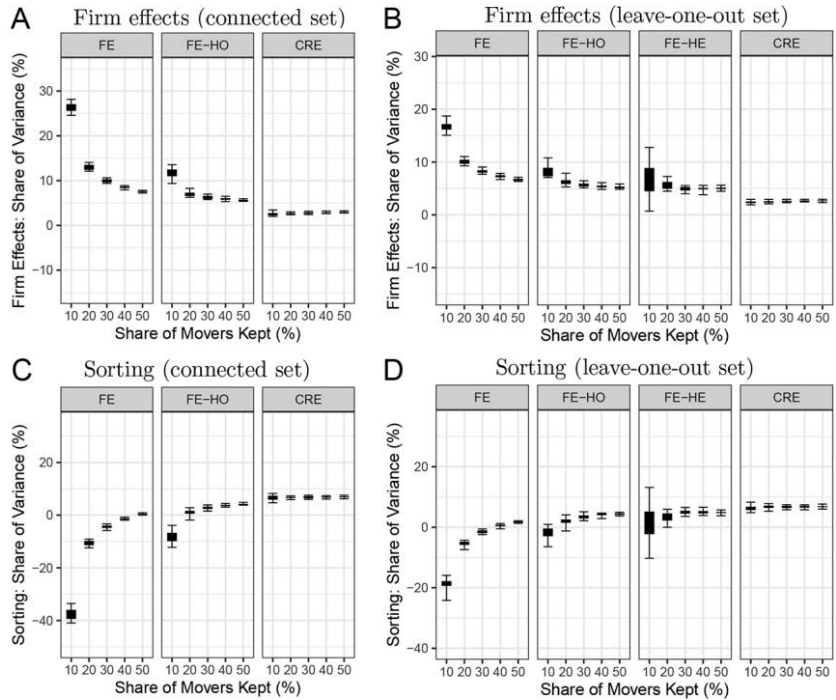
FIG. 3.—Evidence on variability of the estimators in Sweden. In this figure, we consider the subset of firms in Sweden with at least 15 movers. We randomly remove movers within each firm and reestimate the variance of firm effects and covariance between firm and worker effects using the various estimators. For each estimator, we repeat this procedure 20 times and report the overall range (whiskers) and inter-quartile range (solid bar) of estimates across these repetitions. The procedure allows us to keep the connected or leave-one-out set of firms the same and examine the variability in the estimators when there are fewer movers available in estimation.

A. Limited Mobility Bias Leads to Large Upward Bias in Firm Effects

Figure 4 presents the main results for the connected set in the various countries. Figure 4*A* focuses on estimates of the share of earnings inequality due to firm effects for the 6-year panel.

In the United States, the FE estimator suggests that 12% of all earnings variation is due to firm effects.[17] When applying a bias-corrected estimator,

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[17] Song et al. (2019) and Sorkin (2018) use the FE estimator to estimate firm effects in the United States. Our finding that 12% of all earnings variation is due to firm effects falls between the estimates of Song et al. (2019) and Sorkin (2018) of 14% and 9%, respectively. In sec. E of the appendix, we explore the sources of these discrepancies, showing that they differ in part due to the choice of minimum
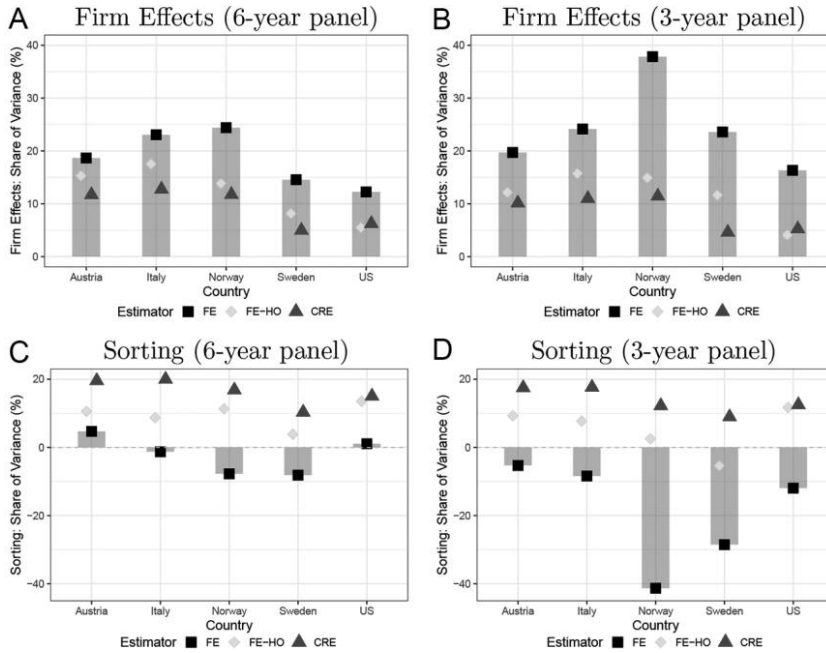
FIG. 4.—Firm effects and sorting across countries. In this figure, we provide FE, FE-HO, and CRE estimates of the contribution to earnings or wage inequality of firm effects (*A*, *B*) and the sorting of workers to firms (*C*, *D*) in Austria, Italy, Norway, Sweden, and the United States. We consider the connected set of firms within each country for 6-year panels (*A*, *C*) and 3-year panels (*B*, *D*). A color version of this figure is available online.

this number falls to 5% when using the FE-HO approach and 6% when using our CRE approach. These estimates suggest that limited mobility bias accounts for at least half of the FE estimates of the contribution of firm effects to wage inequality.

For the European countries in figure 4*A*, the FE estimator suggests that 23%–24% of earnings variance is due to firm effects in Italy and Norway, whereas 15%–18% is due to firm effects in Austria and Sweden. When using the FE-HO bias correction, we find a range of reductions in the estimates from about one-fifth (Austria and Italy) to about one-half (Norway and Sweden) relative to FE. The bias correction becomes stronger when using CRE, with estimates across countries in the 5%–13% range, implying reductions in the estimates ranging from about one-half to about two-thirds relative to FE.

earnings threshold used to define full-time equivalence and in part due to differences in minimum firm size thresholds.

Figure 4*B* repeats these estimates for the shorter 3-year panel. The FE estimator suggests an even greater role for firm effects when the panel is shorter. In the United States, the FE estimator suggests that 16% of all earnings variation is due to firm effects, compared with 12% for the 6-year panel. The FE estimates are also larger in each of the European countries when considering a shorter panel, with firm effects explaining at least 20% of variance in each country with an upper estimate of about 38% (Norway). However, the CRE estimates remain in the 5%–13% range for the United States and each European country, suggesting that the FE estimator is much more biased in shorter panels with fewer movers per firm.

In sum, we conclude there is substantial upward bias in the FE estimator of firm effects in each country, FE is more biased in shorter panels, and the share of earnings variance due to firm effects is substantially smaller compared with what the FE estimator suggests. These conclusions hold true for both bias correction methods.

B. Limited Mobility Bias Leads to Large Downward Bias in Sorting

Figure 4*C* provides the main results on the contribution to inequality of the sorting of workers to firms in the various countries. When using FE, we find a negative estimate of the share of earnings variation due to sorting in all but the United States and one European country. FE estimates range from −8% (Norway and Sweden) to 5% (Austria). However, when using either the FE-HO or CRE bias correction, all of the sorting estimates become positive. In the United States, the FE-HO estimator finds a sorting contribution of 13%, while the CRE estimate is 15%. In the European countries, FE-HO finds estimates of the sorting contribution ranging from 4% (Sweden) to 11% (Austria and Norway), while CRE finds estimates ranging from 10% (Sweden) to 20% (Austria and Italy).

Figure 4*D* repeats this analysis for the shorter 3-year panels. FE suggests a negative contribution of sorting in each country, while CRE finds nearly the same estimates as in the longer 6-year panel, reflecting that limited mobility bias is more severe in shorter panels. Comparing the contribution to inequality of firm effects (fig. 4*A*) to that of sorting (fig. 4*C*), the FE estimates suggest that firm effects explain a larger share of inequality than sorting. However, once one corrects for bias using the CRE estimator, it becomes evident that sorting is more important than firm effects.

When translating the estimates of sorting into correlations, it is important to observe that estimating the correlation between worker and firm effects requires estimating the variance of worker effects, and stronger assumptions would be needed to recover the variance of worker effects (e.g., one could assume a particular dependence structure within and between job spells). However, as long as the covariance is positive, it is easy to compute the following lower bound on the correlation:

$$\text{Corr}(\alpha_i, \psi_{j(i,t)}) \geq \frac{\text{Cov}(\alpha_i, \psi_{j(i,t)})}{\sqrt{\text{Var}(\psi_{j(i,t)})}\sqrt{\text{Var}(Y_{it}) - \text{Var}(\psi_{j(i,t)}) - 2\text{Cov}(\alpha_i, \psi_{j(i,t)})}} .$$
(8)

Using this lower bound, the above results for the United States translate into correlations between worker and firm effects of 0.32 when using FE-HO and 0.34 when using CRE. By contrast, FE suggests only a correlation of 0.02. In the European countries, the CRE estimator of sorting translates into a lower bound on the correlation between worker and firm effects (given by eq. [8]) ranging from 0.24 (Sweden) to 0.34 (Austria and Italy).

Overall, we conclude that the FE estimator for sorting is downward biased and typically of the wrong sign, the biases are more severe in shorter panels, and the bias-corrected share of earnings variance due to sorting tends to be substantial and of similar or larger magnitude compared with the share due to firm effects. These conclusions hold true for both fixed effects and random effects bias correction methods.

C. Comparison between the Connected Set and the Leave-One-Out Set

To apply the FE-HE bias correction method (and compare it to the other bias correction methods), it is necessary to focus on the leave-one-out connected set of firms. In table 1, we saw that most workers from the connected set are also included in the leave-one-out set. However, around half of all firms in the connected set are excluded from the leave-one-out set. A natural concern is that the leave-one-out set differs from the connected set in the composition of workers, moves, and firms. As shown in table 1, larger firms are overrepresented in the leave-one-out connected set.

In figure 5, we consider the leave-one-out set. We plot the CRE estimator on the x-axis and various alternate estimators on the y-axis, so that the 45-degree line represents equality between CRE and the alternate estimators. The FE estimator is denoted by squares, FE-HE by circles, and FE-HO by diamonds. The triangles denote posterior CRE estimates, which we discuss in section VI. In figure 5A, we provide estimates of the share of earnings variance due to firm effects for the longer 6-year panel. We see that the FE estimator is much higher than CRE in each country. By way of comparison, FE-HO and FE-HE line up well along the 45-degree line for some countries, while the estimators are somewhat larger than CRE in other countries. We repeat this analysis for the 3-year panel in figure 5B, finding a similar pattern, but the FE estimates are even further from the 45-degree line.

In figure 5C and 5D, we provide estimates of the share of earnings variance due to the sorting of workers to firms using the 6-year and 3-year panels, respectively. We see that the FE estimates are far below the CRE
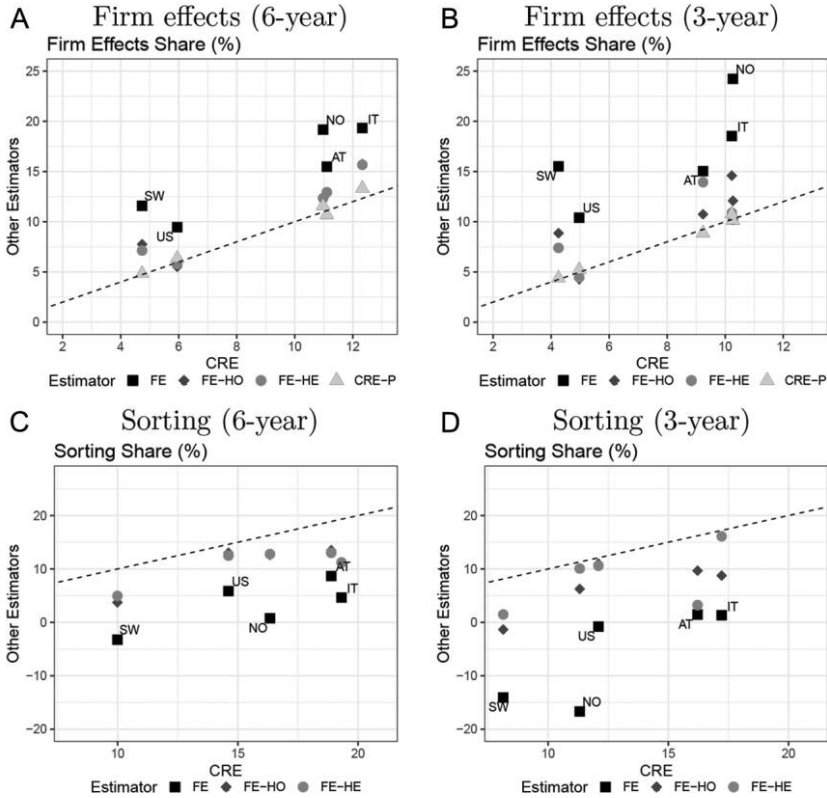
Fig. 5.—Leave-one-out set: various countries. In this figure, we provide FE, FE-HO, FE-HE, and CRE estimates of the contribution to earnings or wage inequality of firm effects (A, B) and the sorting of workers to firms (C, D) in Austria, Italy, Norway, Sweden, and the United States. We consider the leave-one-out set of firms within each country for 6-year panels (A, C) and 3-year panels (B, D). CRE estimates are displayed on the x-axis, and the dashed 45-degree line represents equality between CRE and the alternate estimators. The posterior CRE estimator (CRE-P) for firm effects is also displayed (A, B). A color version of this figure is available online.

estimates, while FE-HO and FE-HE produce estimates that are close to the CRE estimates across the various countries, albeit somewhat lower.[18]

VI. Practical Considerations for Bias Correction

In our last set of results, we turn to some practical considerations for bias correction, including an assessment of situations in which limited mobility bias is likely to be a problem and an examination of some potential

[18] In sec. E of the appendix, we relate our sample and findings to those in Kline, Saggio, and Sølvsten (2020) on Italian data. In addition, in fig. F14 we show FE

implementation issues. We briefly summarize the insights here and refer to our working paper (Bonhomme et al. 2020) for further details.

A. When Is Limited Mobility Bias (Un)likely to Be an Important Problem?

It is difficult to know ex ante whether limited mobility bias is likely to be empirically important, as it depends on a large matrix of worker and firm indicators. Nevertheless, there are reasons to suspect that limited mobility bias is a more important problem in some settings than others.

Two-year panels.—One might conjecture that limited mobility bias is more likely to be severe if one uses very short panels, since a longer time period helps to observe more workers moving across firms. However, a shorter time period may also have advantages, such as making the assumption of time-invariant worker effects more plausible. Furthermore, very short panels can be particularly useful to study the evolution of firm effects and sorting over time, as in the study of Washington state by Lachowska et al. (forthcoming).

In figure 4, we compared 6-year panels to 3-year panels for each country, finding that the FE estimate was more biased in the 3-year panels while the CRE results were nearly identical. In figure F6, we investigate further the performance of the estimators in short panels by splitting our baseline sample from the United States during 2010–15 into each 2-year time interval and applying our estimators to these five short panels. We find that the FE estimator becomes much more biased, with the share of variance due to firm effects rising from 12% in the 6-year panel to more than 20% in the 2-year panels and the share of variance due to sorting falling from 1% in the 6-year panel to below −20% in the 2-year panels. Reassuringly, the bias-corrected estimates do not materially change when shortening the panel.

Small firms.—One possible strategy to reduce limited mobility bias is to restrict firm size. Large firms tend to have more movers and are therefore better connected. For example, Song et al. (2019) and Bassier, Dube, and Naidu (2021) restrict to firms with at least 20 workers, and Sorkin (2018) restricts to firms with at least 15 workers. In figure F7 we explore this possibility in our US sample by restricting the sample to firms with at least 10, 20, 30, 40, or 50 workers. This corresponds to an increase in the number of movers per firm from about five (baseline) to about 45 (minimum 50 workers per firm).

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and bias-corrected estimates for the 20 smallest US states. The results show that bias-corrected estimates are very similar across methods and that the FE estimates of firm effects (worker sorting) are severely upward (downward) biased compared with their bias-corrected counterparts.

As expected, we find that the bias in the FE estimates diminishes as the minimum firm size rises.[19] However, it is necessary to exclude a large share of workers and firms to reduce limited mobility bias. For the share of variance due to firm effects, there is little remaining bias when minimum firm size is 30. For the share of variance due to sorting, there is nonnegligible bias even when minimum firm size is 50.

When interpreting results, it is important to observe that such restrictions change the population of study. Indeed, only two in three workers, one in three moves, and one in 20 firms remain in the sample when the minimum firm size is 50 workers. Economic theory suggests that the distribution of firm effects in larger firms is likely to differ systemically from the distribution in smaller firms.[20] Thus, changing the population of study to minimize limited mobility bias introduces another form of bias, namely, sample selection bias. In section D of the appendix, we characterize analytically and numerically the bias introduced by approximating the variance of firm effects in the population using estimates for a selected subsample. We find that modest sample restrictions based on firm size can lead to substantial bias in the estimated variance of firm effects even if there is no limited mobility bias.

Changes over time.—One situation in which one may be worried about limited mobility bias is when studying changes over time in the wage distribution. In Germany, Card, Heining, and Kline (2013) find that a rise in the variance of firm effects as well as increased sorting over time have contributed substantially to recent increases in wage inequality. In the United States, Song et al. (2019) find that the contribution of the variance of firm effects to earnings inequality has declined over time, while increased sorting over time has contributed substantially to earnings inequality. However, these studies rely on FE estimation and do not perform formal bias correction. One may be concerned that changes in the bias of the FE estimator over time explain the findings for changes in the role of sorting over time.

We now investigate changes over time in the contribution of firm effects and sorting to earnings inequality in the United States. We compare our baseline estimates from the final years in our sample window, 2010–15, to the estimates we obtain for 2001–6. The results are presented in figure F8. The main insight from this figure is that bias correction is important for

[19] Note that while biases tend to be smaller for larger firms in our US sample, there is no theoretical guarantee this will happen in other samples, since the structure of the bias depends on the network of workers and firms in complex ways (Jochmans and Weidner 2019).

[20] For example, with imperfect competition in the labor market, larger firms need to bid up wages to hire the additional workers, and as a result these firms may have larger firm effects on average (see, e.g., Kroft et al. 2021; Lamadon, Mogstad, and Setzler 2022).

obtaining reliable estimates of the contribution of firm effects and sorting to earnings inequality in a given time period but not for capturing how their contribution to inequality changes across time periods. The reason is that limited mobility bias, while sizable, does not change materially over time in our US sample. This conclusion is consistent with the conjecture by Card, Heining, and Kline (2013) that limited mobility bias may be less important for studying inequality over time due to limited mobility bias being similar in different time periods.

B. Possible Implementation Issues for Bias Correction

Mover definition.—In the European countries, our data include start and end dates of employment spells, so we know the year in which a move occurs. However, we do not observe start and end dates in the United States. To harmonize the mover definition across countries, in the analysis above we defined a change in primary employer across years as a move and measured earnings across all years during which the firm was the primary employer. As a check on the importance of this mover definition, we consider a stricter mover definition for the United States in which a worker must be employed for at least three consecutive years at the first firm and at least three consecutive years at the second firm, only measuring earnings during intermediate years in these multiyear spells. Figure F9 provides a diagram to help visualize the difference in these mover definitions and the timing of earnings measurement.

Imposing the strict mover definition in the US sample substantially decreases the number of movers during our sample period. Only one in 60 moves satisfies this particular "3-year/3-year" structure of FTE employment spells during 2010–15. Figure F3 compares the estimates obtained under the baseline and strict definitions of movers. The FE estimate of the contribution of firm effects to earnings variation rises from 12% to 17% (the bias-corrected estimates are both around 5%), and the FE estimate of the contribution of sorting to earnings variation decreases from about 1% to about −17% (the bias-corrected estimates are both around 14%). Yet the CRE estimates are nearly identical under the two definitions, despite the substantial change in sample composition.

Annual earnings, daily wages, and hourly wages.—In many employer-employee data sets, one does not observe hourly wages but instead observes annual earnings or average earnings over an employment spell. When applying the FE estimation, one must then take a stand on the proper measure of wages or earnings. The data from Norway are an exception, as we have accurate measures of days and hours worked in this data set.

In figure F10, we compare results on annual earnings, daily wages, and hourly wages for the same set of workers in the Norwegian data. We provide the comparison for the 6-year and 3-year panels. The FE estimate of the contribution of firm effects rises substantially when using a higher-frequency

measure. In the 6-year (3-year) panel, it rises from about 19% (30%) for annual earnings to about 31% (48%) for hourly wages. The three bias correction methods yield similar results across outcome measures. In the 6-year (3-year) panel, the CRE estimate of the contribution of firm effects rises from about 9% (8%) for annual earnings to about 13% (12%) for hourly wages. These estimates imply that FE is more biased when using higher-frequency outcome measures, and the bias-corrected estimate of the contribution of firm effects to inequality remains economically modest and somewhat greater for higher-frequency measures. A similar pattern is observed for the estimates of sorting, where FE suggests much stronger negative sorting when using hourly wages, but CRE finds substantial positive sorting with similar point estimates across outcome measures.

FE-HO and FE-HE exact versus approximate estimators.—Because of the large sample size in the United States, we cannot compute the FE-HO and FE-HE estimators exactly, and the estimates are computed using an approximate method following Gaure (2014) and Kline, Saggio, and Sølvsten (2020). A natural worry is that the approximation may perform poorly. To investigate this possibility, we apply the estimators to 20 small US states where we can feasibly compute the exact and approximate solutions. In figure F11, we plot exact versus approximate FE-HO estimators in the connected set (fig. F11*a*) as well as exact versus approximate FE-HE estimators in the leave-one-out set (fig. F11*b*). The results show that exact and approximate solutions are close to each other, suggesting that at least in these samples the numerical approximation works well.

CRE number of clusters and posterior estimators.—In our baseline CRE estimation, we cluster firms into 10 groups. One may worry that 10 groups is too restrictive. Figure F12 compares CRE estimates by number of groups in our US sample. We find that as we increase the number of groups from 10 to 50, the estimates remain nearly identical for the earnings variation due to firm effects and sorting.

Above we reported CRE estimates of variance components based on equation (7). We can also compute posterior estimates using the CRE specification as a Bayesian prior. Such estimates enjoy robustness properties when the CRE model is misspecified (Bonhomme and Weidner 2022). In figure 5*A* and 5*B*, we compare the posterior CRE (CRE-P) estimator to our other estimators for the variance of firm effects. We find that CRE-P is almost identical to CRE for both the 6-year and 3-year panels. As shown in figure F14, the same holds true when we compare CRE and CRE-P separately for the 20 small US states. This is to be expected if the CRE model is correctly specified. Last, in figure F13 we report posterior estimates for a random effects specification that does not condition on firm groups. We estimate the firm effects variance to be less than half the CRE estimate. This suggests that accounting for the firm groups in the random effects specification is important.

VII. Broad Lessons for Empirical Work Using the AKM Model

Over the past two decades, a large body of work has used the AKM model and FE estimator to analyze earnings inequality in many developed countries. The results from these studies have been important, not only for quantifying the sources of earnings inequality but also for how economists model the labor market. In this paper, we assessed the sensitivity of FE estimates to the incidental parameter problem that arises in the AKM model, often referred to as "limited mobility bias." Researchers have long been aware of the problem of limited mobility bias. Despite this awareness and the availability of bias correction methods, relatively few studies correct for bias.

In our analyses, we use employer-employee data from the United States and several European countries while taking advantage of both fixed effects and random effects methods for bias correction. Our analyses deliver several important conclusions for empirical work using the AKM model. First, we show in simulations based on real data that limited mobility bias can be empirically important and existing methods for bias correction perform well even as mobility becomes very limited. Given their good performance, there is no need to resort to informal strategies based on sample restrictions (see, e.g., Sorkin 2018; Song et al. 2019; Bassier, Dube, and Naidu 2021), which may introduce sample selection bias. One should instead implement theoretically justified bias correction methods in empirical studies based on the AKM model.

Second, we find in Austria, Italy, Norway, Sweden, and the United States that limited mobility bias is a major empirical issue for studies using FE to document firm effects and worker sorting. Once bias is accounted for, firm effects dispersion matters much less for earnings inequality and worker sorting becomes always positive and typically strong. Thus, we argue that it is important for empirical work using FE to perform bias correction of the estimates, especially when working with short panels.

Third, alternative methods for bias correction based on different assumptions and different cuts of the data (e.g., varying the number of periods or imposing a minimum firm size) tend to produce broadly similar results to one another. This is reassuring, as bias correction necessarily involves making restrictive assumptions about the model or limiting the set of firms under consideration. Furthermore, we find that the bias correction methods are fairly robust to several possible specification and computational issues related to numerical approximation or discretization.

It is important to observe, however, that these conclusions rely on correctly specifying the model of earnings and the processes of worker and firm heterogeneity. There are several reasons why the AKM model may be misspecified; for example, both the assumptions that earnings are log additive and that worker and firm heterogeneity are constant over time may be

violated. To address these concerns, one possibility is to develop methods for bias correction that are robust to misspecification. Another possibility is to enrich the model by, for example, incorporating worker-firm interactions and dynamic processes of worker and firm productivity. In this spirit, Bonhomme, Lamadon, and Manresa (2019) estimate worker-firm interactions while allowing for state dependence and endogenous mobility in Sweden, while Lamadon, Mogstad, and Setzler (2022) allow for worker-firm interactions and dynamic productivity processes of workers and firms in their study of the US labor market.

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