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Identifying Sorting in Practice†

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We propose a novel methodology to uncover the sorting pattern in labor markets. We identify the strength of sorting solely from a ranking of firms by profits. To discern the sign of sorting, we build a noisy ranking of workers from wage data. Our test for the sign of sorting is consistent even with noisy worker rankings. We apply our approach to a panel dataset that combines social security earnings records with detailed financial data for firms in the Veneto region of Italy. We find robust evidence of positive sorting. The correlation between worker and firm types is about 52 percent. (*JEL* J24, J31, J41, J62, L25)

What is the pattern of sorting of heterogeneous workers into heterogeneous firms? Do better workers typically work in better firms? In some labor markets, like academia, anecdotal evidence supports this idea. However, discerning the pattern of sorting in labor markets remains elusive. A direct analysis of sorting requires knowledge of the underlying types of both firms and workers, which is hard to obtain. In this paper, we propose a new strategy to identify the strength and sign of sorting.

Uncovering the actual patterns of assortative matching is key for the analysis of the labor market. In the presence of sorting, shocks and policies that affect firms do not necessarily affect workers evenly. For example, recessions and trade liberalization push low-profits firms out of the market (see Caballero and Hammour 1994 and Melitz 2003). Under positive assortative matching, low-skill workers are disproportionately affected by the resulting displacements. Moreover, understanding sorting patterns is central in the growing empirical literature studying the role of firms in the labor market (see Card et al. 2018). For instance, Card, Heining, and Kline (2013) show that sorting plays an important role as a source of wage inequality. Several papers that focus on the determinants of wages need to account for nonrandom

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sorting of workers to firms. See Carlsson, Messina, and Skans (2016) for the effect of productivity on wages, Macis and Schivardi (2016) for the effect of exports, Breda (2015) for the effect of collective bargaining, and Battisti (2013) for coworker peer effects. Analogously, see Bender et al. (2016) for the importance of the subject when studying the determinants of cross-firm variation in measured productivity. Furthermore, whenever sorting is driven by complementarity in production, the strength of sorting conveys information on the magnitude of the complementarity.

Ideally, one would observe worker and firm types to measure the sorting pattern. What makes one firm better than another? Firms are heterogeneous in several dimensions. The firm type combines a number of features related to technology, demand, and market structure (Syverson 2011). Firms differ in managerial talent and practices (e.g., Bloom and Van Reenen 2007), organizational form (e.g., Garicano and Heaton 2010), working environments and human resources practices (e.g., Ichniowski and Shaw 2003), market power and technology spillovers (e.g., Bloom, Schankerman, and Van Reenen 2013), sunk costs (e.g., Collard-Wexler 2013), and span of control (e.g., Eeckhout and Kircher 2012), among other dimensions. It is hard to find empirical counterparts for each of these characteristics. Moreover, even if they could be measured, aggregating them is far from straightforward.

Although different in several dimensions, firms share an objective function: they maximize the expected value of their payoffs. This maximized value aggregates the features that make firms heterogeneous into a natural one-dimensional ranking. Better firms have higher expected payoffs. A firm's maximized value of expected payoffs is unobservable, but has a natural empirical counterpart: profits.

To estimate the pattern of sorting, we use firm-level data on profits to rank firms and, when needed, worker-level data on wages to construct a noisy ranking of workers. We exploit a unique panel dataset that combines social security earnings records and labor market histories for individual workers in the Veneto region of Italy with detailed financial data for their employers.

Our first contribution is to provide a methodology to measure the strength of sorting, *without* the need to rank workers, and without using wages. We define the strength of sorting by the *correlation ratio* η, which measures the variance of firm types that can be explained by worker types. Intuitively, the more intensively workers sort into firms, the smaller the variance of partners' types for a given worker, compared to the unconditional variance of firm types. To estimate the strength of sorting, we use observations of the same worker matched to different firms. We find that worker types explain around 27 percent of the variance in employer types in our dataset. This corresponds to a correlation ratio of 52 percent.

Our second contribution is to provide a simple test for the sign of sorting. The strength of sorting provides a measure of the association between firm and worker types, but remains silent about its direction. If worker types were observed, the sign of the correlation between worker and firm types would reveal the direction of sorting. We use wages to rank workers by their types, but acknowledge that wages only provide a noisy ranking of workers. The potential nonclassical nature of the noise represents a major threat to obtaining a consistent test. We use independent draws from a worker's distribution of employers to guarantee that the noise in wages is uncorrelated with the worker and firm types. We thus obtain an attenuated estimate of the correlation between worker and firm types. The attenuation of the point estimate is not detrimental, as we only need to establish its sign. Therefore, we can consistently estimate the sign of sorting despite the noise in the ranking of workers. We find evidence of positive sorting in our dataset.

We present a large set of robustness checks for our results. First, we challenge our baseline ranking of firms. Our data contains the entire list of balance sheet and profits and loss account entries that each firm in the sample is compelled to disclose publicly, in accordance with existing regulations. We take advantage of this data and construct several profit measures. Our results are highly robust to the diverse profit definitions and normalizations. Second, we propose a method to also exploit job-to-job transitions without unemployment spells. Third, we account for endogenous mobility. In our baseline specification we only use workers who transit unemployment and consider match destruction as exogenous. The sample of workers transiting unemployment and the sample of firms firing workers are potentially selected. We take into account both sources of selection and show that our results do not differ significantly.

Our profit-based approach to measuring sorting is motivated by two intuitive considerations. First, economists are generally comfortable with the idea that, in most economic environments, a *better* firm should ultimately be more profitable. Any characteristic that makes a firm *better* should increase its value, and profits can be used to proxy for it. Second, the information needed to measure profits is publicly available. More importantly, the available information is sufficiently detailed to allow for the construction of different measures of a firm's profitability, thereby offering alternative empirical counterparts to the theoretical ranking of firms.

While simple and intuitive, there are caveats with our proposed approach. From an empirical perspective, other datasets may contain less detailed information, making it harder to compute an accurate empirical counterpart to the unobserved notion of maximized intertemporal profits. The increasing availability of matched employer-employee data that can be linked to rich firm-level financial information is expected to significantly relax this limitation in the future. From a theoretical standpoint, while profits naturally aggregate a firm's heterogeneity along several dimensions, it is possible to think about environments where profits do not necessarily rank firms by, say, a firm's latent productivity. Then, without a particular model of the labor market, one cannot state, in general, that our methodology measures sorting on latent underlying productivity. In Section VB, we present a simple model to illustrate the "cost of mismatch." In this simple environment, we show that the value of a firm is increasing in its latent productivity. Thus, by proxying the value of the firm by average profits, our method detects sorting by unobservables in this environment. More generally, our approach can be seen as a method to document sorting when firms are ranked by profits. As such, our methodology adds to the existing tools in the empirical literature studying sorting of heterogeneous workers into heterogeneous firms.

Related Literature.—The theoretical literature on sorting focuses mainly on how complementarity in production determines the allocation of workers to firms. In his seminal paper, Becker (1973) studies a frictionless economy and shows that positive sorting arises if and only if the production function is supermodular. Shimer and Smith (2000) extend Becker's model to account for search frictions, and show that stronger complementarities in the production function are required to guarantee positive sorting. Atakan (2006) explicitly models search costs and provides sufficient conditions that restore Becker's classical result. In Eeckhout and Kircher (2010) root-supermodularity is necessary and sufficient for positive sorting. Bartolucci and Monzón (2014) allow for bilateral on-the-match search and show that positive sorting can arise even without complementarity in production.

In their pioneering work, Abowd, Kramarz, and Margolis (1999) quantify the relative importance of worker versus firm components in the determination of wages. They find that differences in firm fixed effects account for a sizable share of the overall wage variation, firm-size differentials, and industry wage differentials. A striking result in initial applications of their methodology is that the correlation between worker and firm fixed effects is small, or even negative, and often statistically insignificant. More recent papers find instead a positive and significant correlation between the two sets of fixed effects for a number of countries (e.g., Card, Heining, and Kline 2013; Maré and Hyslop 2006; Skans, Edin, and Holmlund 2009; and Bagger, Sørensen, and Vejlin 2013). We report on the performance of Abowd, Kramarz and Margolis's exercise in our data (see Section IVA). We obtain a small, negative, but statistically significant, correlation between the firm fixed effect and the worker fixed effect. The correlation from Abowd, Kramarz, and Margolis's (1999) methodology is informative on the extent to which high-wage workers sort into high-paying firms. Whenever worker and firm fixed effects are increasing in their unobservable productive characteristics, this correlation is also informative about sorting by latent productivity.

We provide a complementary methodology to those typically used to study sorting. Unlike the methods based on Abowd, Kramarz, and Margolis (1999), our approach does not hinge on estimated worker and firm fixed effects. This is relevant because of several reasons. First, it is reasonable to expect that better workers are paid more, on average. However, workers may differ in their valuation of various nonwage amenities, and, hence, equally productive workers may earn different average wages. Second, it is not necessarily true that better firms pay higher wages. Gautier and Teulings (2006), Eeckhout and Kircher (2011), Lopes de Melo (2018), and Bagger and Lentz (2016) argue that whenever wages are non-monotone in the firm type (or there are amenities), firm fixed effects estimated from wage equations do not necessarily reflect the firms' underlying productive types.[1] If so, Abowd, Kramarz, and Margolis's (1999) correlation may be uninformative of the pattern of sorting by types. Third, worker and firm fixed effects are likely to be noisily estimated even in large administrative datasets.[2] Most workers are matched to only a

[1] Efficiency wage models provide another reason why firm fixed effects may not reflect underlying productivity. Due to heterogeneity in monitoring technology, profit maximizing firms may have different wages policies. Alternatively, in line with the compensating differential hypothesis, equally productive firms may optimally offer different combinations of wage policies and firm-wide amenities.

[2] Profits in our data are not directly contaminated by estimation errors, as they are drawn from each firm's certified balance sheets and income statements. Of course, other sources of measurement errors, e.g., due to variation in accounting practices, may still potentially be a concern. In our paper we show that many alternative measures of profits provide a robust picture of the extent of sorting in our labor market.

few employees. This makes the estimation of worker fixed effects noisy. Firm fixed effects are identified by the wage changes of those leaving or joining any given firm. Even in large samples, many firms may not experience enough worker turnover for the firm fixed effect to be measured reliably. The problem is likely to be more serious in countries characterized by a majority of small and medium-sized firms.

Our paper contributes to a growing literature aiming at measuring sorting.[3] Eeckhout and Kircher (2011) and Lopes de Melo (2018) propose methods to measure the strength of sorting, but remain silent on its sign. Eeckhout and Kircher (2011) suggest using information on the range of accepted wages of a given worker. Lopes de Melo (2018) relies on the correlation between a worker fixed effect estimated from a wage equation and the average fixed effects of her coworkers. Mendes, van den Berg, and Lindeboom (2010) identify both the strength and sign of sorting, under the assumption that a worker's type can be identified from a set of observable characteristics. Bonhomme, Lamadon, and Manresa (2016) extend Abowd, Kramarz, and Margolis's strategy by discretizing the unobserved heterogeneity. Hagedorn, Law, and Manovskii (2017) and Bagger and Lentz (2016) estimate fully specified structural search and matching models to recover the sign and strength of sorting.

We describe our dataset in Section I. Section II presents our methodology. We discuss how to use our dataset to build a global ranking of firms and noisy rankings of workers. We then define the strength and sign of sorting, and introduce our methods for uncovering the sorting pattern. Section III presents our results. Section IV provides a discussion and robustness checks. We compare our results to those from methodologies based on Abowd, Kramarz, and Margolis (1999). We then illustrate how our estimate of the sorting pattern can inform on the cost of mismatch due to frictions. Section V concludes.

I. Data

We build our dataset by combining information from two different sources: individual labor market histories and earnings records from the Veneto Workers History (VWH), and firm financial data from Bureau van Dijk's "AIDA."[4] The VWH contains information on private sector employees in the Veneto region of Italy over the period from 1975 to 2001, obtained from administrative records of the Italian Social Security System (see Tattara and Valentini 2007). It is a typical matched employer-employee database, where individual workers can be followed over time and across different employers. The available information allows for the computation of accurate daily wages for each worker, and how these evolve over time and across each worker's different employers. The VWH is especially suited to our application since it contains not only the universe of incorporated businesses in the region but also information on every single employee working in these firms.

[3] We focus on one-dimensional sorting. For multidimensional sorting see Dupuy and Galichon (2014) and Lindenlaub (2017).

[4] Card, Devicienti, and Maida (2014) use this dataset to investigate the extent of rent-sharing and hold-up in firms' investment decisions.

The financial information contained in the AIDA database are derived from standardized reports that firms are required to file annually with the Chamber of Commerce. The data are available from 1995 onward for firms with annual sales above 500,000 Euros. In principle, all (nonfinancial) incorporated firms with annual sales above this threshold are included in the database. We use tax code identifiers to match job-year observations for employees in the VWH to employer information in AIDA for the period from 1995 to 2001 (see Appendix B for details).

The AIDA data contain *all* entries of the standardized balance sheet and of the profit and loss accounts (including sales, value added, total wage bill, the book value of capital—broken down into a number of subcategories, the total number of employees, and the firm's national tax number). As described in detail in the AQ2 Appendix, the AIDA data allow us to compute various measures of firm profits, including economic profits (which deduct an estimate of the opportunity cost of capital), gross operating surplus (GOS) (which does not deduct estimated capital costs), as well as accounting net profits (AP) as reported annually in the final item AQ3 of a firm's profit and loss accounts. We use these measures of profits both in levels and normalized by the number of workers employed by the firm. As an alternative normalization, we also consider the return on equity (ROE).

We make a series of exclusions to arrive at the sample of transitions that we use for estimating the strength and sign of sorting. First, we consider only those workers who—within the 1995–2001 period—ever switched from a firm in the dataset to another firm in the dataset, with or without an intervening spell of unemployment. Second, we eliminate apprentices and part-time employees. There are around 168,000 job switchers in the sample (who represent 20 percent of the original sample), moving between almost 12,000 firms, for a total of 228,590 transitions. Of those, 178,219 are transitions mediated by at least one month of nonemployment.

Although this sample can already be used to estimate the strength of sorting, we focus on a more restricted sample of movers to test for the sign of sorting. Our test of the sign of sorting requires information on workers' wages. Hence, to minimize measurement error in wages, we further restrict the sample to workers with a minimum of labor market attachment, and discard observations with unreasonably low or high wages (see the Appendix for details). We further restrict the estimation sample to firms with at least two movers, in order to allow for firm fixed effects in some of our regressions testing for the sign of sorting. This leaves around 97,000 job switchers, moving between 9,000 firms, for a total of 156,213 transitions. Of these, 120,426 are transitions mediated by at least one month of nonemployment. In the Appendix, we report that, despite the drop in the number of observations in the more restricted sample, the characteristics of workers and firms are similar to those in the larger sample of transitions. We perform our analysis in the smaller sample with clean wages.[5]

[5] In our online Appendix, we show that the estimates of the strength of sorting are virtually unaffected when we use the larger transition sample.

II. Methodology

The labor market features two-sided heterogeneity: there are better and worse firms, and also workers of worse and better quality. We are interested in uncovering how worker and firm heterogeneity are associated in the labor market. In order to do so, we rank each side of the market by a one-dimensional index: $p \in [0,1]$ ranks the firms from worst to best, and $\varepsilon \in [0,1]$ does the same for workers. We want to learn about $\ell(\varepsilon, p)$, which denotes the joint distribution of worker and firm types in the economy. The main challenge for uncovering the sorting pattern is that the sources of heterogeneity (and their corresponding rankings) are, in principle, unobserved. We tackle this challenge with our rich dataset and a novel identification strategy.

A. *Ranking Firms*

We rank firms by their profits. The partnership model (one firm matched to only one worker) with productivity as the only source of heterogeneity between firms is canonical in the theory of sorting. However, firms are heterogeneous in several dimensions. We take advantage of the common objective function of firms to rank them. Profits aggregate the different dimensions of firm heterogeneity into a natural one-dimensional ranking.

In markets with frictions, firms accept less than ideal workers. For a given firm, some matches are more profitable than others. Therefore, match-level profits by themselves lead to a noisy ranking of firms. Luckily, firms are matched to a large number of workers (the dataset we use has a mean firm size of 58 and only includes firms with at least 10 workers), and we observe profits at the firm-level. Thus, we can consistently estimate firm's expected profits, which integrates out unobserved match-specific heterogeneity.

Idiosyncratic shocks to firms may generate noise in our ranking of firms. However, the within-firm variation in profits accounts for only 7.63 percent of the total variation in profits in our sample. We average profits in the longitudinal dimension and each firm is observed, on average, for 5.35 years. Therefore, noise in the measurement of profits has a negligible effect on our results.[6]

We construct a ranking $p \in [0,1]$ that orders firms by their average profits. This provides an observable global ranking of firms that aggregates several dimensions of heterogeneity. We report our results using many alternative definitions of profits, and obtain very similar results.

[6]The within-firm variance of average profits is of order $1/T_j$ of the variance of the idiosyncratic shocks, where T_j is the number of periods that firm j is observed in. For example, for a firm observed in 5 periods, idiosyncratic shocks account for slightly more than 1 percent of the variance of average profits. We replicate results only including firms observed for more than T periods, with $T \in \{1, \ldots, 5\}$. Results do not change significantly. In an earlier version of our paper we show that the use of current-year profits, instead of longitudinally averaged ones, has little impact on the results (see Bartolucci and Devicienti 2013 for details).

B. *Ranking Workers*

Mean wages have been proposed and used to rank workers (see, for example, Eeckhout and Kircher 2011). The rationale is analogous to that of our ranking of firms. A better worker should have a better performance in the labor market. If workers only cared about wages, mean wages would provide a valid ranking of workers according to their types.

Most employees, however, are only matched to a few employers. In our sample, workers have 1.3 employers, on average, along the 7-year duration of our panel. The limited number of draws from the distribution of jobs for each worker makes estimates of worker specific parameters inconsistent. But also, in datasets, where the complete worker history is observable, the small number of partners for each worker along the life-cycle is an impediment to produce precise estimates of their mean wages.[7] Furthermore, when using data on the entire life-cycle of a worker, the assumption of time-invariant quality becomes more controversial.

We exploit information contained in wages to rank workers. However, because of the aforementioned reasons, we can only construct noisy rankings of workers.

C. *The Strength of Sorting*

We define the strength of sorting as the variance of firm types that can be explained by worker types. The variance of partners $\mathrm{var}\big[p\,|\,\varepsilon\big]$ for a given worker of type ε is the variance of firms unexplained by ε. Therefore, a standard variance decomposition, $\sigma_p^2 = \mathrm{var}\big[E\big[p\,|\,\varepsilon\big]\big] + E\big[\mathrm{var}\big[p\,|\,\varepsilon\big]\big]$, provides a sensible measure of the strength of sorting. The smaller the variance of partners' types for a given worker type relative to the unconditional variance of firm types, the more intensively workers sort into firms.

DEFINITION 1 (Strength of Sorting): *The strength of sorting is characterized by the correlation ratio* $\eta = \sqrt{\mathrm{var}\big[E\big[p\,|\,\varepsilon\big]\big]/\sigma_p^2}$.

The strength of sorting is commonly associated with the correlation coefficient ρ between firm and worker types (e.g., Bagger and Lentz 2016). When the expected employer type $E\big[p\,|\,\varepsilon\big]$ is a linear function of worker type, ρ^2 and η^2 coincide.[8] In Becker's symmetric model without frictions [1973], workers match only with firms of their same type (in case of positive assortative matching). Thus, $E\big[p\,|\,\varepsilon\big]$ is trivially linear in worker type. However, even stylized models of the labor market with frictions (e.g., Shimer and Smith 2000 or Atakan 2006) do not imply that $E\big[p\,|\,\varepsilon\big]$ is linear in ε. We focus on the correlation ratio because it does not impose linearity on $E\big[p\,|\,\varepsilon\big]$.

[7] The average number of employers in a 30-year career is slightly lower than 6 in Italy.

[8] The correlation ratio is by definition asymmetric and weakly greater than the correlation coefficient (which is symmetric). ρ^2 reveals the proportion of the variance of firm types explained by the best *linear* prediction from the worker types. See Kruskal (1958) for a discussion on the relationship between different measures of association.

We estimate the strength of sorting η through information contained in observations of the same worker employed by different firms.[9] Let i denote a worker and j an employer, so ε_i is worker i's type and p_{ij} is the type of the firm j where i is employed. Each draw from the distribution of i's employers can be expressed as $p_{ij} = \phi_i + v_{ij}$, with $\phi_i \equiv E[p \mid \varepsilon_i]$ and v_{ij} linearly independent of ε_i. The variance of p can be decomposed into two components: $\mathrm{var}[p_{ij}] = \sigma_p^2 = \sigma_\phi^2 + \sigma_v^2$.

Therefore, we can use standard panel data techniques to separate out permanent from transitory components in the variation of p_{ij} (see, for example, Arellano 2003). Take two random draws p_{ij} and p_{ih} from $\ell(\varepsilon_i, p)$ for each worker i in a representative sample of workers. Then,

$$E(p_{ij}p_{ih}) - E(p_{ij})\,E(p_{ih}) \;=\; \mathrm{cov}(p_{ij}, p_{ih}) = \sigma_\phi^2.$$

There is a large number of workers in our dataset, but each has only a few partners. Thus, we can obtain precise estimates of σ_ϕ^2 and σ_p^2, but not of the individual realization of ϕ_i. The correlation coefficient between p_{ij} and p_{ih} is equal to the square of the correlation ratio:

$$(1) \qquad \rho(p_{ij}, p_{ih}) \;=\; \frac{\mathrm{cov}(p_{ij}, p_{ih})}{\mathrm{var}[p_{ij}]} \;=\; \frac{\sigma_\phi^2}{\sigma_p^2} \;=\; \eta^2.$$

We use transitions mediated by an unemployment spell to obtain employer draws. Take worker i, matched to an employer of type p_{ij} after the unemployment spell. Let the employer type before unemployment be denoted by p_{ij}^{PREV}. Since the transition is mediated by unemployment, p_{ij} and p_{ij}^{PREV} are independent conditional on worker type ε_i. In standard models used to study sorting (e.g., Shimer and Smith 2000 or Atakan 2006), job destruction is exogenous and there is not on-the-job search. Then, p_{ij} and p_{ij}^{PREV} are random draws from the distribution $\ell(\varepsilon_i, p)$ of employers for a worker of type ε_i.[10] Therefore, we estimate the correlation ratio η through $\rho(p_{ij}^{PREV}, p_{ij})$.

D. *The Sign of Sorting*

The strength of sorting η provides a measure of the association between firm type p and worker type ε. However, η cannot reveal the sign of sorting. Is there in fact positive assortative matching? If so, is it observed for the whole support of firms?

[9] Given that we use observations from the same worker in different periods, we take the joint distribution $\ell(\varepsilon, p)$ to be in steady state during our time frame. For the purpose of our methodology, we do not need to specify a particular matching process. The period covered in our analysis is one of relative stability of the Italian labor market. See the Appendix for a discussion on the institutional background.

[10] With on-the-job search, two employers are still conditionally independent if there is an interim unemployment spell. However, the distribution of partners potentially depends not only on the worker type but also on the number of transitions after an unemployment spell (that is how much time the worker has climbed her ladder of employers). Moreover, job destruction may depend on firm and worker types. In Section IVC, we discuss how to address these concerns and show that our results are robust to them.

From an empirical perspective, the sign of sorting has typically been associated to the sign of the correlation coefficient between firm and worker types. Following our definition of the strength of sorting, we focus on the expected employer type.

DEFINITION 2 (Sign of Sorting): *There is positive sorting if $E[p \mid \varepsilon]$ is strictly increasing in ε. Negative sorting is defined analogously.*

Theoretical work has focused on several different concepts to define positive sorting. The definition of sorting in Becker's frictionless model [1973] requires all mass to be concentrated in the 45° line. If true, there is also positive sorting given our definition. In Shimer and Smith (2000), sorting is defined in terms of acceptance sets (from unemployment). There is (strict) positive sorting whenever acceptance sets are convex and feature (strictly) increasing bounds. Since there is no on-the-job search in Shimer and Smith (2000), acceptance sets from unemployment are the only determinants of the steady-state distribution. If there is sorting in Shimer and Smith's model, given their definition, there is sorting given our definition. Chade (2006) (with imperfect information about types) and Lentz (2010) (with on-the-job search) define sorting in terms of stochastic dominance. Of course, first-order stochastic dominance implies sorting given our definition.

Our methodology to estimate the sign of sorting relies on information from wages. Employer types p_{ij} are directly observable for each match. Were worker types ε_i also directly observable, one could easily answer whether $E[p \mid \varepsilon]$ increases with ε. One could run a regression of the following form:

$$p_{ij} = \alpha \varepsilon_i + v_{ij}, \tag{2}$$

and by recovering α from (2), learn about the sign of $\partial E[p \mid \varepsilon] / \partial \varepsilon$. Unfortunately, worker types are not directly observed.

We build on the intuitive idea (as in Abowd, Kramarz, and Margolis 1999 and Eeckhout and Kircher 2011) that the better the worker, the better her labor market performance: expected wages $E[w \mid \varepsilon]$ should increase with worker type ε. Through observations of wages we obtain information on $E[w \mid \varepsilon]$ and so we learn indirectly about ε.

The monotonicity condition $\partial E[w \mid \varepsilon] / \partial \varepsilon > 0$ would identify the sign of sorting if expected wages $E[w \mid \varepsilon]$ were observed. A regression of the form

$$p_{ij} = \hat{\gamma} E[w \mid \varepsilon_i] + \hat{v}_{ij} \tag{3}$$

would reveal the sign of α in equation (2), since $\text{sign}(\hat{\gamma}) = \text{sign}(\alpha)$. Expected wages $E[w \mid \varepsilon_i]$ are not observed, but could in principle be estimated from data by the average wage of the worker *i*. The wage agent *i* receives from her *j* employer can always be expressed as

$$w_{ij} = E[w \mid \varepsilon_i] + u_{ij}, \tag{4}$$

where u_{ij} is linearly independent of ε_i. Therefore worker i's average wage is

$$\frac{1}{T_i}\sum_{T_i} w_{ij} \;=\; E\big[w \,|\, \varepsilon_i\big] + \frac{1}{T_i}\sum_{T_i} u_{ij},$$

where T_i is the number of employers of worker i. With a large number T_i of draws from equation (4), $\lim_{T_i \to \infty} \sum_{T_i} w_{ij}/T_i = E\big[w \,|\, \varepsilon_i\big]$. Unfortunately, workers have an average of 1.3 jobs along the 7 years of our panel. Moreover, both p_{ij} and w_{ij} are draws associated with the same firm j, so potentially $\mathrm{cov}\big(\hat{v}_{ij}, u_{ij}\big) \neq 0$. For example, if better firms pay higher wages, then shocks $\hat{v}_{ij}$ and u_{ij} in equations (3) and (4) are (positively) correlated. Thus, with T_i small, the difference $\sum_{T_i} u_{ij}/T_i$ between average observed wages and expected wages is correlated with p_{ij}. As a result, the variation in wages not coming from worker types does not behave as classical measurement error since it is endogenous in (3).

Equations (3) and (4) provide a framework to identify the sign of sorting. Take two *independent* draws p_{ih} and p_{ij} from the distribution $\ell(\varepsilon_i, p)$ of employers for a worker of type ε_i. The wage w_{ij} is correlated with the employer type p_{ih} only through the worker type; that is $\mathrm{cov}\big(\hat{v}_{ih}, u_{ij}\big) = 0$. Then, w_{ij} is a noisy measure of $E\big[w \,|\, \varepsilon_i\big]$ and we can treat u_{ij} as classical measurement error to identify the sign of sorting.

We exploit transitions mediated by unemployment to obtain conditionally independent draws of wages and partner types. Take worker i, who after an unemployment spell obtains wage w_{ij} in an employer of quality p_{ij}. Before being in unemployment, she was working for an employer of quality p_{ij}^{PREV}, with wage w_{ij}^{PREV}. Since the transition was mediated by unemployment, shocks $\hat{v}_{ij}^{PREV}$ and u_{ij}^{PREV} are independent of shocks $\hat{v}_{ij}$ and u_{ij}, conditional on worker type ε_i. Moreover, previous jobs are draws from the distribution of employers for each worker.[11]

The availability of transitions mediated by unemployment allows us to test directly whether better workers (higher w_{ij}) typically have better employers (higher p_{ij}^{PREV}). The simplest way to test whether this occurs (on average) is to run

$$p_{ij}^{PREV} = \gamma w_{ij} + \tilde{v}_{ij}. \tag{5}$$

Equation (5) is our simplest specification. We present additional specifications to deal with remaining threats to identification when we discuss our results for the sign of sorting.

III. Results

A. *The Strength of Sorting*

We use 120,426 transitions mediated by an interim unemployment spell to estimate the correlation ratio η through $\rho(p_{ij}^{PREV}, p_{ij})$. Figure 1 presents the empirical

[11] For now we keep two simplifying assumptions. First, we assume that match destruction is exogenous. Second, we consider wages after an unemployment spell to be random draws from the distribution of wages. When workers search on the job, this may not be the case. We relax these assumptions in Section IVC.

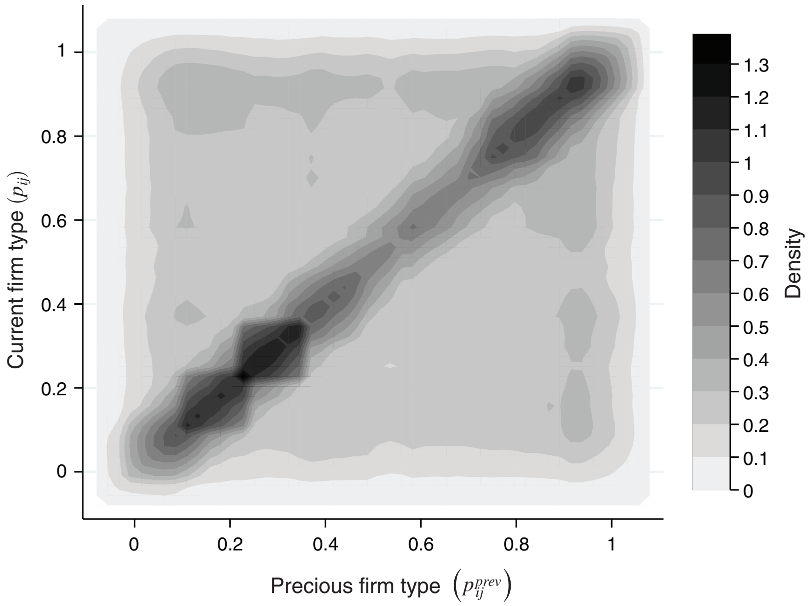

Figure 1. Empirical Distribution of Transitions Mediated by Unemployment

Notes: This figure shows the empirical distribution of worker transitions mediated by an interim unemployment spell. Firms are ranked by economic profits per worker. p_{ij}^{PREV} denotes the previous firm type, while p_{ij} denotes the current firm type. Bivariate kernel density estimate. Kernel type is Epanechnikov.

Table 1—Estimates of the Strength of Sorting η

Economic profits		GOS		AP		
Per worker	Total	Per worker	Total	Per worker	Total	ROE
0.540	0.518	0.540	0.527	0.521	0.531	0.542

Notes: This table presents the estimates of the correlation ratio η for various measures of firm profits. η is estimated from all worker transitions mediated by an interim unemployment spell. All shown estimates are statistically significant: p-values < 0.001.

distribution of all transitions. Previous firm type p_{ij}^{PREV} is on the x -axis while current firm type p_{ij} is on the y-axis. It is noteworthy that most current partner types are in the neighborhood of the previous firm type. The quality of the previous employer explains a significant fraction of the variation in current employer type.

We report the estimates of $\eta = \sqrt{\rho(p_{ij}^{PREV}, p_{ij})}$ for different measures of profits in Table 1. The estimated value for the correlation coefficient η ranges between 0.51 and 0.54. Worker types explain more than a fourth of the variance in employer types. Different measures of profits lead to very close estimates of the strength of sorting.

This methodology can also be used to estimate the strength of sorting for specific subgroups in the population. Table A2 in the Appendix reports the estimates of the strength of sorting η by gender, blue or white collar, and different age groups. The estimates are in line with those in Table 1. We also report estimates of η for two main

TABLE 2—ESTIMATES OF γ (*sign of sorting*)

Economic profits		GOS		AP		
Per worker	Total	Per worker	Total	Per worker	Total	ROE
Panel A. Estimates from equation (5)						
0.098	0.096	0.075	0.082	0.044	0.049	0.058
(0.022)	(0.031)	(0.028)	(0.024)	(0.018)	(0.017)	(0.019)
Panel B. Estimates from equation (5) with controls						
0.143	0.141	0.118	0.129	0.085	0.106	0.091
(0.029)	(0.027)	(0.027)	(0.026)	(0.020)	(0.022)	(0.021)
Panel C. Estimates from equation (5) with firm fixed effects						
0.025	0.031	0.019	0.023	0.001	0.001	0.004
(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)
Panel D. Estimates from equation (5) with firm fixed effects and controls						
0.025	0.036	0.022	0.031	0.010	0.016	0.014
(0.004)	(0.003)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)
Panel E. Estimates from equation (6) with firm fixed effects and controls						
0.020	0.016	0.016	0.014	0.012	0.009	0.012
(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)
Panel F. Average (γ_j) estimated from equation (7) at the firm level with controls						
0.033	0.048	0.033	0.044	0.020	0.028	0.014
(0.009)	(0.010)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)

Notes: This table reports estimates of the sign of sorting from a regression of a worker's previous firm type p_{ij} on the worker's current wage w_{ij} (γ in equation (5)). Controls include worker's age, age squared, tenure, tenure squared, time dummies, and indicators for females, foreign-born workers, blue collar, white collar, and managerial occupations. Firm fixed effects and time fixed effects are included in panels C, D, and E. Standard errors clustered by firm are in parentheses.

sectors of activity (manufacturing and service), which might differ along a number of dimensions, including the extent of product market competition. The estimates are nevertheless quite similar.

B. *The Sign of Sorting*

Panel A of Table 2 reports estimates of γ from equation (5) for different measures of profits. The sign is positive and significant for all measures of profits. Of course, since wages w_{ij} are a noisy measure of worker types ε_i, then $\gamma \neq \hat{\gamma}$. We argue how γ provides an attenuated estimate of $\hat{\gamma}$ (i.e., $|\gamma| \leq |\hat{\gamma}|$) and therefore consistently informs us about the sign of sorting.

Two additional conditions are required to guarantee attenuation. First, the noise in the ranking of workers must be exogenous: $\mathrm{cov}\left(\hat{v}_{ij}^{PREV}, u_{ij}\right) = 0$. Second, equation (2) must be linear and the noise in the measure of worker type in equation (4) must be additively separable. We focus on the second condition in Section IIIC. There, we present nonparametric results that impose less structure on the relationship between p, ε, and measurement error. In the remainder of this section we challenge the exogeneity assumption. First, we control for heterogeneity in workers not associated with the worker type that may lead to $\mathrm{cov}\left(\hat{v}_{ij}^{PREV}, u_{ij}\right) \neq 0$. Next, we also acknowledge that wages may rank workers only locally, for example, when firms differ in the non-pecuniary benefits (amenities) they offer. We rank workers locally (within the firm). Finally, we allow the sign of sorting to vary with the firm type. We find robust evidence of positive sorting under all these specifications.

Wages are determined by several factors that go beyond worker types. Female workers, migrants, and workers with lower tenure receive lower wages on average. Similarly, wages in different occupations vary, even for the same worker type. These sources of variation, which enter into u_{ij}, may also be associated to the (previous) firm type. For example, if female workers suffer segregation and wage discrimination, then firms of high quality hire fewer female workers and female workers make less money, leading to $\mathrm{cov}\left(\hat{v}_{ij}^{PREV}, u_{ij}\right) > 0$.

We include observable characteristics x_{ij} of both the worker and her job in equation (5) to prevent contamination from other sources of worker heterogeneity. Controls x_{ij} include workers' age, age squared, tenure, tenure squared, time dummies, and indicators for female, foreign-born, blue collar, white collar, and managerial occupations. Panel B of Table 2 reports results with controls. The estimates of γ are positive and significant for all measures of profits.

As discussed previously, $E\left[w \mid \varepsilon\right]$ does not necessarily order workers by their types. Workers care about wages but also about other characteristics of the job. Firms can differ in terms of their compensation packages: some may pay high wages with a low level of amenities, while others pay low wages with a high level of amenities. This source of firm heterogeneity is potentially correlated with the firm type and therefore may affect workers of different types unevenly. Moreover, some workers may compensate lower wages with lower unemployment risk.[12]

AQ4

We rank workers locally using within-firm variation in wages. By only comparing coworkers, we first partial out between-firm heterogeneity in compensating differentials. Local rankings rely on expected wages being increasing in worker type only *within* the firm.[13] Panel C of Table 2 reports the results including firm fixed effects in equation (5). Estimated γ are positive and in most of the cases significant. Within-firm variation in wages is not only driven by the worker type. However, the sign of γ is still informative about the sign of sorting if there is attenuation. In this case, attenuation requires exogeneity of the within-firm variation of wages not driven by the worker type. In Panel D of Table 2, we present estimates of γ including firm fixed effects and controlling for worker and job observable characteristics.[14] The estimates of γ are also positive and significant for all the measures of profits.[15]

[12] In our online Appendix, we present estimates of γ controlling for individual specific unemployment risk.

[13] In models like Shimer and Smith (2000), within-firm wages order workers according to their types (see (Lopes de Melo, 2018). In models with on-the-job search, renegotiation, and endogenous search intensity as Lentz (2010), wages are not necessarily monotone in the worker type within the firm (see Bagger and Lentz 2016). In our online Appendix, we use a subsample of transitions where wages can be used to rank coworkers in Lentz's (2010) environment. We also find evidence of positive assortative matching.

[14] In our online Appendix, we present results using finer occupation categories. We use detailed information on workers' position in the contractual "job ladder" (*livelli di inquadramento*) to build a precise classification of jobs within the firm. The results including these more refined within-firm occupational controls strongly corroborate the existence of positive sorting.

[15] Merlino, Parrotta, and Pozzoli (forthcoming) use our tests with Danish data to compare the sorting pattern across genders.

We also study whether better workers *go* to better firms. We include observable characteristics x_{ij} of both the worker and her job and run

$$p_{ij} \;=\; \gamma w_{ij}^{PREV} + x_{ij}'\beta + \tilde{v}_{ij}. \tag{6}$$

The estimates are positive and significant for all the measures of profits (see Panel E of Table 2).[16]

In our last set of results in Table 2, we allow for heterogeneity in γ between firms of different quality p. In some labor markets positive sorting may occur for some range of quality p, and negative sorting for a different range. Our results from equation (5) with firm fixed effects provide an estimate of the *average* effect of workers' local rankings on the expected ranking of the employer only if heterogeneity in γ is i.i.d. We test for γ at the firm-level by estimating

$$p_{ij}^{PREV} \;=\; \zeta_j + \gamma_j w_{ij} + x_{ij}'\beta_j + \tilde{v}_{ij}, \tag{7}$$

where ζ_j, γ_j, and β_j are firm-specific parameters, and x_{ij} includes controls as before. Panel F of Table 2 reports the sample average of the estimated γ_j at the firm level. The average γ is also positive, and significant in most cases.

Finally, we report local estimates of γ. For any given firm j, the number of workers who land in j after an unemployment spell is typically small, which makes the estimate of γ for each firm unreliable. We therefore allow γ to vary smoothly with firm type p. These results are valid under the assumption that firms of similar types have a similar γ. We find that better workers come from better firms for the whole support of new firms (see Appendix D for details).

C. *The Sign of Sorting: Nonparametric Results*

Our test for the sign of sorting builds on a simple intuition: if there is positive sorting, a random draw from the distribution of employers of a better worker should be better, on average, than a random draw from the distribution of employers of a worse worker. We can test directly if this pairwise association exists in the data. We use the ranking of firms and the noisy ranking of workers to perform a Kendall test of association (τ).

DEFINITION 3 (Kendall's τ): *Take any two rankings a, b. The Kendall rank correlation coefficient $\tau(a,b)$ between these rankings is given by*

$$\tau(a,b) = \frac{\sum_{n=1}^{N}\sum_{m<n} \mathbb{1}\{(a_n - a_m)(b_n - b_m) > 0\} - \mathbb{1}\{(a_n - a_m)(b_n - b_m) < 0\}}{\frac{1}{2}N(N-1)}.$$

[16] We also run a regression like the one from equation (6), but with wages on the left-hand side and p_{ij}^{PREV} on the right-hand side. We find similar results (see our online Appendix). We thank an anonymous referee for this suggestion.

We present results using Kendall's τ for two reasons. First, Kendall's τ provides a nonparametric measure of the association between two variables. Second, we can consistently estimate the sign of the association using a Kendall's τ under mild conditions in the specification of the noise in the ranking of workers.

As discussed before, there may be variation in wages not explained by worker types, even within the firm. On-the-job search and renegotiation (as in Postel-Vinay and Robin 2002; and Cahuc, Postel-Vinay, and Robin 2006), measurement error, or match effects may generate this. We relax the specification of wages as follows. Assume that wages (or a monotone increasing transformation ψ_1 of wages) are given by

$$\psi_1(w_{ij}) = \psi_2(\varepsilon_i) + u_{ij}, \tag{8}$$

where ψ_2 is strictly increasing and u_{ij} is an i.i.d. shock. Equation (8) is satisfied in the case of classical measurement error, or i.i.d. match effects in any monotone transformation of wages, such as the standard assumption of classical error in log-wages. Moreover, if we only compare coworkers, equation (8) holds if wages are renegotiated as in Postel-Vinay and Robin (2002) or as in Cahuc, Postel-Vinay, and Robin (2006).[17]

Similarly, assume that employers are drawn from

$$\psi_3(p_{ij}) = \alpha\,\psi_4(\varepsilon_i) + v_{ij}, \tag{9}$$

where $\psi_3(\cdot)$ and $\psi_4(\cdot)$ are strictly increasing functions and ν_{ij} is i.i.d. The sign of $\tau(p,\varepsilon)$ is determined by the sign of α. Unfortunately, since ε is unobserved, we cannot recover $\tau(p,\varepsilon)$ directly. However, we can learn about on the sign of $\tau(p,\varepsilon)$ estimating $\tau(p,w)$. With noisy rankings of workers, the Kendall's correlation between worker and firm types is attenuated. Attenuation increases the probability of accepting the null of no sorting when the true correlation is different from zero. The higher the informational content about worker types conveyed by wages, the higher the power of our test on the sign of sorting. Moreover, our result extends to the case of rank correlation: the (Spearman) rank correlation ρ is always larger than the Kendall coefficient τ. Thus, $\tau(p,w)$ provides a consistent test for the sign of sorting. Lemma 1 presents this formally.

AQ5

LEMMA 1: *Assume that workers' wages and employers are drawn from* (8) *and* (9). *Then*,

$$(i) \qquad \operatorname{sign}\big(\tau(p,w)\big) = \operatorname{sign}\big(\tau(p,\varepsilon)\big) = \operatorname{sign}(\alpha) \textit{ and}$$

$$(ii) \qquad \big|\tau(p,w)\big| \le \big|\tau(p,\varepsilon)\big| \le \big|\rho(p,\varepsilon)\big|.$$

See Appendix E for the proof.

[17] See equation (3) in Cahuc, Postel-Vinay, and Robin (2006) and equation (5) in Postel-Vinay and Robin (2002). In Postel-Vinay and Robin (2002), ψ_1 is a log transformation.

Table 3—Estimates of Kendall's Coefficient τ of Association between Previous Firm and Wages

Sample	Observations	Total profits (1)	Profit per worker (2)
All workers	119,772	0.087	0.076
Blue-collar male	66,899	0.067	0.061
White-collar male	20,201	0.071	0.069
Blue-collar female	20,207	0.178	0.146
White-collar female	12,415	0.128	0.083
Firm-by-firm average	119,772	0.018	0.022

Notes: This table shows nonparametric estimates of the association between a worker's previous firm type p_{ij} and the worker's current wage in the current firm (Kendall's Coefficient τ). All estimates are statistically significant: p-values < 0.001. In column 1, firms are ranked in terms of total economic profits. In column 2, firms are ranked in terms of economic profits per worker. Each row represents a sample where τ is estimated.

Table 3 presents estimates of Kendall's $\tau(p, w)$. We first present results constructing all possible pairs in the data, and ordering workers in terms of wages. The type p denotes the ranking of the *previous* employer. Second, we control nonparametrically for observable characteristics by comparing workers in the same group in terms of gender and occupation. Third, we only compare coworkers measuring the association at the firm level. In this last case we report the sample average of firm-specific Kendall's correlations. Results using aggregated profits and profit per worker are reported in columns 1 and 2, respectively. We find that the association is positive and significant in all cases.

IV. Discussion and Robustness Checks

A. *Methodologies Based on Abowd, Kramarz, and Margolis (1999)*

Next, we apply the methodology in Abowd, Kramarz, and Margolis (1999) to our data. We estimate a standard Mincer-type wage equation that includes worker and firm fixed effects, which we denote by θ_i and ξ_j, respectively (see our online Appendix for details). We use the estimated fixed effects to compute the correlations reported in Table 4. As in some other replications of the classical result from Abowd, Kramarz, and Margolis, we find a small negative correlation (-0.02) between the worker fixed effects and the firm fixed effects. This correlation is statistically significant in our dataset.[18]

We also compute the between and within-firm variance decomposition of Abowd, Kramarz, and Margolis's worker fixed effect. This is closely related to the test proposed in Lopes de Melo (2018). Lopes de Melo (2018) measures the strength of sorting through the correlation between a worker's fixed effect θ_i and the average of his coworkers fixed effects $\tilde{\theta}_{j(i,t)}$. We perform this test in our data, both in levels (obtaining a value of 0.377) and in rankings (0.431). Then, around 40 percent of the variance of the estimated ranking of workers is explained by the firm type. This finding is in line with the evidence reported by Lopes de Melo (2018) for a number of countries. This strategy is an interesting complement to ours, since our proposed

[18] As we use the whole population of workers and firms in this labor market, it is unlikely that these results are driven by limited inter-firm mobility bias of the sorts pointed out by Andrews et al. (2008 and 2012).

Table 4—Abowd, Kramarz, and Margolis's (1999) Fixed Effects and Profits. Correlation Coefficients

	Levels	Rank	Sample
$\rho(\theta_i, \xi_j)$	−0.022		Movers and Stayers
$\rho(\theta_i, \tilde{\theta}_{j(i,t)})$	0.377	0.431	Movers and Stayers
$\rho(\xi_j, p_j)$	0.103	0.198	Movers and Stayers
$\rho(\xi_{ij}^{PREV}, \xi_{ij})$		0.432	Movers

Notes: This table shows pair-wise correlation coefficients between worker fixed effects θ_i, the average of his co-workers fixed effects $\tilde{\theta}_{j(i,t)}$, firm fixed effects ξ_j, and firm's profits p_j. Worker and firm fixed effects are estimated from Abowd, Kramarz, and Margolis's-style wage regressions. All estimated correlations $p(\cdot,\cdot)$ are statistically significant: p-values < 0.001. Profits are measured in terms of economic profit per worker. The "Movers and Stayers" sample is the Abowd, Kramarz, and Margolis's post-estimation sample obtained from the VWH–AIDA matched data (the "complete sample" in Table A1). The "Movers" sample is the one used for our estimates of sorting (column 3 in Table A1).

measure of the strength of sorting is not symmetric. We measure the proportion of the variance of firm types explained by worker type. However, without further assumptions (for example: $E(p|\varepsilon)$ linear in ε) our strategy is silent on the variance of worker types explained by firm-types.[19]

Firm fixed effects ξ_j from an Abowd, Kramarz, and Margolis (1999) style wage regression can be used to study whether workers sort into firms with similar wage policies. In Table 4, we show results from a variance decomposition of ξ_j. We find that 43.2 percent of the variance of firm fixed effects is explained by worker types, which is higher than the estimates from Table 1 obtained when using profits. There are various reasons why workers may move across firms with similar wage policies. For instance, sectoral collective bargaining may significantly reduce the room for wage flexibility at the firm level.[20] Moreover, worker mobility may mostly occur within sector (or even industrial districts in the case of Italy), owing to workers' specific skills.[21] The extent to which the correlation between the current and previous employer wage premiums are revealing of the strength of sorting by types is however unclear. As shown in Table 4, the correlation between the firm fixed effect ξ_j and our ranking of firms based on profits (average profits per worker in Table 4) is relatively small, at around 0.2.[22] Empirical research should provide further evidence on the way profits and firm wage premiums are connected.

In spite of finding strong evidence of sorting with our methodology, we find an essentially zero correlation between worker and firm fixed effects in our data. This may result from firm fixed effects not reflecting underlying firm types in our dataset.

[19] In our data, however, worker fixed effects from a wage regression are noisy measures of the workers' mean wages due to the small number of jobs for each worker. Whenever the noise can be assumed to be of the classical form, the correlation between a worker's fixed effects and his coworkers' fixed effects can be seen as a lower bound of the true correlation.

[20] For example, unlike German firms, Italian firms cannot resort to so-called "opting-out" clauses to deviate from the wage dispositions bargained industry-wide.

[21] Card, Heining, and Kline (2013) show that among German workers who change jobs, the majority move into jobs in the same decile of the firm-effect distribution as the job they left. Schmutte (2015) presents a similar result using US data.

[22] This correlation is lower than what emerges from Card, Cardoso, and Kline (2016), which partly reflects differences between our Veneto and their Portuguese sample. For example, the average firm size is 58 employees in the Veneto data, compared to over 500 employees in their data.

The relatively low correlation between profits and firm fixed effects points in that direction.[23] On a related note, the sampling errors in the estimated worker and firm fixed effects are in general negatively correlated. This generates a mechanical downward bias in the estimates of the correlation between worker and firm fixed effects (see for example Andrews et al. 2008; and Card, Cardoso, and Kline 2016). Overall, our findings suggest that, among the various methods considered, the correlation between the worker and firm fixed effects is the one more likely to be biased towards zero and less likely to detect the "true" amount of sorting.[24]

Moreover, the finding that the correlation between a worker's fixed effect and the average of his coworkers fixed effects is close to our measure of the strength of sorting lends support to the view that workers fixed effects are a comparatively better measure of the worker type than a firm's fixed effects are of the firm type. In other words, workers' types are relatively well identified from wage data, at least in our dataset. While this is line with the theoretical results from Lopes de Melo (2018), further research would be needed to assess whether this is the case more generally.

B. *The Cost of Mismatch due to Frictions*

A complete characterization of the data generating process, that is, a fully specified economic model, allows us to map our estimated measure of sorting to primitives of the economy. Assortative matching in the labor market is normally understood as evidence of complementarity in production between workers and firms. Large productive complementarity between firms and workers types implies large productivity differential between different allocations of workers to firms.

Together with our estimate of η, a frictional labor market model with two-sided heterogeneity can be used to learn about the size of the cross derivative of the production function. Furthermore, a parameterized version of the model allows us to measure the cost of frictions in terms of lost output.

To obtain a sense of the economic importance of our results on sorting in terms of the cost of mismatch, we estimate the parameters of a model as the ones described in Atakan (2006) and Eeckhout and Kircher (2011). The economy is fully characterized by its destruction rate, the cost of search, and two parameters that measure the degree of complementarity in the production function and the total factor productivity. We estimate the model by the method of moments.[25] Setting the primitives to their estimated values, we compare the output from an equilibrium with frictions to the counterfactual one without frictions where firms and workers are optimally allocated. We find that turning off frictions leads to a 21 percent increase in total output of the economy. This number is slightly larger than the equivalent measure obtained by Hagedorn, Law, and Manovskii (2017) for Germany. We also compute

[23] The particularly low correlation in our data may also result from the small size of most Italian firms. See also the related findings by Barth et al. (2016). Using US data, they argue that only a relatively small portion (around 25 percent) of the variance in the firm fixed effects can be explained by the estimated rent-sharing elasticity and the between-firm variance of value added per worker.

[24] The recent work by Kline, Saggio, and Sølvsten (2018) points in a similar direction. They propose a new approach to the unbiased estimation of variance components models under heteroskedasticity which can be applied to two-way fixed effect models.

[25] See the Online Appendix for details on the model, estimation technique and estimates.

an equilibrium without frictions, where agents are assigned randomly. The output in this case is 9.4 percent lower than the optimal assignment case.[26] This illustrative exercise suggests that, despite the relatively large correlation ratio obtained from the data, the potential gains from reallocation are likely to be more modest.

C. *Robustness Checks*

Our identification strategy relies on the availability of random draws from the distribution of partners and wages. In what follows, we present several potential challenges to this strategy. We succinctly discuss how to address them and provide evidence that shows that our results are robust.[27]

First, we discuss the effect of on-the-job search. If workers search on the job, partners' types and wages after an unemployment spell are not necessarily random draws from steady state. We take advantage of the longitudinal dimension of our dataset and present evidence suggesting that this concern does not drive our results. Intuitively, workers may be less selective from unemployment if they can continue to search while on the job. Over time workers change jobs, and eventually the effect of the unemployment spell fades away. We compute the correlation $\eta_t^2 \equiv \operatorname{cov}\left(p_i^{PREV}, p_{i,t}\right)/\sigma_p^2$, where $p_{i,t}$ is the type of the employer t periods *after the beginning of the unemployment spell*. We find that during the first year, η_t^2 increases as t grows. After approximately a year η_t^2 becomes stable around the values from Section IIC. We follow an analogous approach to identify the sign of sorting, and also find a positive sign.

Second, we take into account that information spillovers from former coworkers may generate correlation between employer types, even conditioning by worker type. Workers may find new jobs exploiting networks of former fellow workers (see Cingano and Rosolia 2012). Then, employer types before and after an unemployment spell may be correlated, even conditioning on worker type. We challenge our assumption of conditional independence allowing for a serially correlated transitory component in the variation of employer types. In order to identify this modified model, we need workers for whom we observe three partners. We use information on individuals with at least two transitions. Our results suggest that the serial correlation in the transitory component in partner's types is statically significant, but is rather weak. Accounting for this, we find an estimated strength of sorting slightly larger than those presented in Table 1.

Third, we allow for job destruction to be associated with firm type. The assumption of exogenous job destruction is common in models describing the labor market. With exogenous job destruction, firm types before unemployment are random draws from the distribution of partners. However, some firms may be more likely to layoff workers than others. Firms that are more likely to layoff workers appear more often as previous employers in a sample of workers who transit unemployment.

[26] Bagger and Lentz (2016) report similar values for the gains from sorting in a different framework and with different data.

[27] We present a more thorough discussion of our approach to deal with these challenges in our online Appendix. All robustness results can be found there.

We calculate the monthly firm-specific destruction rate as the fraction of employees observed in unemployment in the following month. We find a clear pattern between destruction rates and firm type. Firms of worse types are more likely to layoff workers than those of higher types. We estimate the strength and sign of sorting weighting each observation by the inverse of the destruction rate corresponding to the previous employer. The estimated strength of sorting η is similar to that reported in Section IIC. The sign of sorting γ is positive and significant in all specifications.

Finally, some workers may be more likely to be fired than others. Workers who are laid off are potentially different from those who do not transit unemployment. Our results are consistent for the group of workers who transit unemployment. However, that group is a nonrandom sample of workers. Their sorting pattern may be different from that of other workers. To account for this concern, we consider firms that layoff their complete workforce (firm closures). In this case, all workers are forced to leave the firm, irrespective of their characteristics.[28] In our data it is possible to identify 710 firms that closed their business during the 1995–2001 time period, involving 15,255 workers. We obtain estimates of the sign and strength of sorting for this subsample. Despite this dramatic reduction in sample size, the results are once again indicative of positive sorting, with a similar strength to that of our baseline analysis.

V. Conclusion

We present a new methodology to identify both the strength and the sign of sorting in the labor market. Our methodology exploits information not only from workers' mobility and wages, but also from firms' profits. We apply our approach to a panel dataset that combines social security earnings records for workers in the Veneto region of Italy with detailed financial data for firms.

We rank firms by their profits. Previous literature has focused on using information on wages alone to try to identify sorting. Profits have two main advantages with respect to wages. First, firms aim to maximize profits, whereas workers also care about job characteristics other than wages. Non-pecuniary compensation then makes wages a noisy measure of a worker's underlying quality. Second, firms are matched to a large number of workers, whereas workers only have a few employers in their work history. As a result, firms' profits integrate out match-specific noise, but workers' wages do not.

In our first contribution, we propose a methodology to measure the strength of sorting that does not require using wages. We characterize the strength of sorting by the correlation ratio, which measures the fraction of the variance in firm types explained by worker types. We exploit information contained in transitions and with standard panel data techniques separate the within worker variation of partners from the between worker variation. We find that worker types explain about 27 percent of the total variance, which corresponds to a correlation ratio of about 52 percent.

In our second contribution, we present a test for the sign of sorting. We use information contained in wages to rank workers. However, variation in wages is driven

[28] Cingano and Rosolia (2012) use a similar strategy to identify the strength of information spillovers on workers' unemployment duration.

by several factors other than worker types. Thus, wages are a noisy measure of the type of the worker. Using both parametric and nonparametric methods, we show how to consistently estimate the sign of sorting in spite of this noise. We find robust evidence of positive sorting.

Our methodology relies on data that is either available for several countries and regions, or can be constructed from available sources. Although beyond the scope of this paper, these characteristics make our tests feasible for the comparison of different labor markets. Moreover, our test for the strength of sorting can be particularly useful in datasets with detailed information on worker transitions and firm financial information, but lacking information on wages, perhaps owing to confidentiality reasons or other legal restrictions. More generally, our method is useful in situations where wages only provide a weak signal of a worker's underlying productivity, e.g., in labor markets dominated by sectoral collective bargaining or automatic seniority-related wage growth.

Appendix

A. *Institutional Background*

The period covered by our analysis is one of relative stability in the institutional setting of the Italian labor market. Major policy interventions aimed at liberalizing fixed-term contracts were introduced only at the end of 2001 and in 2003, beyond our sample period.

Wage setting in Italy is governed by a "two-level" bargaining system. This system, introduced in 1993, replaced an earlier system that included local and sectoral agreements and a national indexation formula.[29] Sectoral agreements (generally negotiated every two years) establish contractual minimum wages for different occupation classes (typically seven or eight sector-specific classes), that are automatically extended to all employees in the sector. Unions can also negotiate firm-specific contracts that provide wage premiums over the sectoral minimums. During the mid-1990s such firm-level agreements covered about 40 percent of private sector employees nationwide (see ISTAT 2000). In addition, individual employees receive premiums and bonuses that add to the minimum contractual wage for their job. In our estimation sample nearly all employees earn at least some premium: the fifth percentile of the percentage premium is 2.5 percent, while the median is 24 percent. The combination of sector and occupation minimum wages with individual-level wage premiums makes within-firm wage variability quantitatively significant. Lazear and Shaw (2009) report that within-firm wage variability in Italy represents about two-thirds of total wage variability, in line with other countries described in their study.

During the mid-1990s Italy's employment protection legislation's index (EPL) was around the median of OECD countries, at a similar level to those of France and Germany (see Banca D'Italia 2003). Moreover, the estimated job and worker

[29] See Casadio (2004) and Dell'Aringa and Lucifora (1994). The Netherlands, Spain, and Portugal have similar two-level systems.

turnover were in line with, if not higher than, similar European countries (see Contini and Trivellato 2005).

The amount of labor turnover in this period is higher than expected by the content of the Italian labor code. A first reason for this lies in the peculiarity of the Italian industrial structure, characterized by a vast majority of small and very small firms. Firms with more than 15 employees could only fire individual workers with just cause: workers dismissed without a justifiable reason had the right to reinstatement. Because of the high diffusion of small firms, this rule did not apply to about 35 percent of employees. A second reason is that, even when the rule was applicable, it was commonly bypassed either legally by extrajudiciary settlements with severance payments or by unlawful practices.[30] Moreover, collective layoffs became available to firms employing more than 15 employees starting from 1991.[31] Finally, labor jurists highlight that the "law in the books" and the "law in action" may differ. This might be particularly relevant in the case of Italy, with its high number of laws and bylaws, sometimes patently contradictory, and amenable to dubious interpretations. Overall, the Italian labor market is characterized by enough wage flexibility and worker mobility to make our empirical strategy viable.

B. *Description of the Data and Profits*

The Veneto Workers History (VWH) includes register-based information for any job that lasts at least one day.[32] On the employee side, the VWH includes total earnings during the calendar year for each job, the number of days worked during the year, the code of the appropriate collective national contract and level within that contract, and the worker's gender, age, region (or country) of birth, and seniority with the firm. On the employer side the VWH includes industry (classified by 5-digit ATECO 91), the dates of "birth" and closure of the firm (if applicable), the firm's location, and the firm's national tax number (*codice fiscale*).

The VWH is matched to AIDA using the firm's fiscal code, available in both datasets.[33] We carry out additional checks of business names (*ragione sociale*) and firm locations (firm addresses) in the two data sources to minimize false matches. The match rate is relatively high: for about 95 percent of the AIDA firms we find a matching firm in the VWH (see Card, Devicienti, and Maida 2014 for additional details).

Balance-sheet data is less accurate for small firms. For this reason we discard observations at firms with fewer than 10 employees. We report the characteristics of our initial sample in column 1 of Table A1. Over the 1995–2001 period, the matched dataset contains about 840,000 individuals aged 16–64, observed in about 1 million job spells (about 3 million job $\times$ year observations) at over 23,000 firms.[34]

[30] For example, forced quits that would go unreported to the judiciary for fear of losing job options offered within the same industrial district.

[31] Under collective layoffs, a firm can dismiss five or more employees within six months.

[32] The Veneto region has a population of about 4.6 million, approximately 8 percent of Italy's total population.

[33] Only a tiny fraction of firms in AIDA are publicly traded. We exclude these firms and those with consolidated balance sheets (i.e., holding companies).

[34] Firms in the sample represent about 10 percent of the total universe of firms contained in the VWH, covering around 42 percent of the Veneto employees. The vast majority of the unmatched firms are non-incorporated,

Table A1—Descriptive Statistics: VWH–AIDA

	Complete sample (1)	Job-changer sample (2)	Job-changer sample (3)
Number of jobs × year obs	3,073,672		
Number of individuals	837,904	168,280	97,455
Number of firms	23,448	11,907	9,228
Number of jobs	1,057,901		
Number of transitions		228,590	156,213
…of which through at least one month of unemployment		178,219	120,426
Worker characteristics			
Mean age	35.25	32.1	31.9
Proportion of female	0.29	0.25	0.28
Proportion of white collar	0.30	0.27	0.26
Proportion of manager	0.01	0.007	0.005
Mean tenure[a]	102.82	33.68	36.48
Mean daily wage	69.35	66.03	59
Mean daily log wage	4.12	4.03	4.02
Mean interim unemployment[a]		8.29	7.90
Median interim unemployment[a]		3	3
Firm characteristics			
Mean firm size	58	62.21	69.4
Mean profit[b]	503.12	587.48	781.4
Mean profit per worker[b]	11.75	12.04	11.49
Mean GOS[b]	656.15	765.22	1,035.76
Mean GOS per worker[b]	15.03	15.34	14.75
Mean AP[b]	184.24	202.21	261.3
Mean AP per worker[b]	4.38	4.73	2.76
Mean return over equity	0.07	0.07	0.07

Notes: This table shows descriptive statistics for the matched VWH–AIDA sample. Column 1 refers to the complete sample, column 2 to the job-changer sample, and column 3 to the job-changer sample with "clean wages" (primary analysis sample; see text).

[a] In months.

[b] 1,000s Euros (in 2000 prices).

Twenty-nine percent of workers in the sample are female, 30 percent are white collar, and a tiny minority, about 1 percent, are managers. The mean age is 35, mean tenure is 103 months, and the mean daily wage is 69 Euros. The mean firm size is 58 employees.

The bottom panel of Table A1 reports the mean values of several measures of profits regularly used in the literature. Most of our results are based on economic profits, given by $\Pi_{jt} = Y_{jt} - M_{jt} - w_{jt}L_{jt} - r_t K_{jt}$, where Y_{jt} denotes total sales of firm j in year t, M_{jt} stands for materials, and $w_{jt}L_{jt}$ are firm labor costs, all as reported in the firm's profit and loss account. To deduct capital costs, we compute K_{jt} as the sum of tangible fixed assets (land and buildings, plant and machinery, industrial and commercial equipment) plus immaterial fixed assets (intellectual property,

small family business (*società di persone*) that are not required by existing regulations to maintain balance sheets books, and are therefore outside the AIDA reference population. The average firm size for the matched sample of incorporated businesses is significantly larger than the size of non-incorporated businesses. Mean daily wages for the matched sample are also higher than in the entire VWH, while the fractions of female and younger workers are lower. See Card, Devicienti, and Maida (2014) for further details.

R&D, goodwill). Following Card, Devicienti, and Maida (2014), we assume that $r_t = 10\%$.[35]

Through the paper, we report results based on gross operating surplus (GOS), which does not deduct estimated capital costs. We also consider accounting net profits (AP), obtained from GOS after deducting taxes and debt service, and adding any financial income and extraordinary revenues (e.g., income deriving from subsidiaries or other equity investment owned by the firm). These measures of profits are either used in levels or normalized by the number of workers employed by the firm. As an alternative normalization, we also consider the return on equity (ROE), defined by the ratio of AP to shareholder's funds.

We average each profit definition longitudinally, i.e., over all years in which a firm is observed in our sample. Empirically, this is helpful in minimizing the impact of short-term fluctuations and measurement error. Theoretically, longitudinal profits are closer to the notion of intertemporal profits that, as discussed earlier, summarize the diverse dimensions of firm heterogeneity into a natural one-dimensional ranking. As shown by Table A1, the average economic profit is about 500,000 euros (in 2000 prices), and profit per worker is 12,000 euros. The corresponding figures for the GOS are slightly higher, and lower for AP. On average, firms have a ROE of about 7 percent.

As shown in column 2 of Table A1, there are around 168,000 job switchers in the data. As expected, job changers are on average younger than the overall sample (their mean age is 32 years), have lower tenure (less than 3 years), and earn comparatively less than the rest of the population (66 Euros daily). The percentage of female workers, white collar workers, and managers are also smaller in the job changer sample than in the overall sample of column 1. The table also reports the number of months that have elapsed between the separation from the former employer and the association with the new one. The median duration of this interim unemployment is only 3 months. However, the mean unemployment duration is 8.3 months, which is consistent with a large fraction of workers with long-term unemployment (ISTAT 2000). We use the sample reported in column 2 to estimate the strength of sorting.

As discussed in Section I, we use a more restricted sample of movers for testing the sign of sorting. This sample is shown in column 3. Specifically, to minimize measurement error in wages we aim at excluding observations that may reflect things other than jobs (reimbursements, for example). We discard observations if the job duration is lower than 26 days. Similarly, we discard observations with unreasonably low wages. We take the "minimum wage" for the lowest category in each sector-wide "national contract" (see Appendix A). We in turn calculate the lowest of all those "minimum wages." Any wage lower than that is discarded (this roughly

[35] The literature on capital investment in Italy suggests that during the mid-to-late 1990s a reasonable estimate of the user cost of capital (r_t) is in the range of 8 percent to 12 percent. See Elston and Rondi (2006), Arachi and Biagi (2005), and Franzosi (1999). Capital is measured as the book value of past investments in the AIDA data. Recomputing a firm's capital based on the perpetual inventory method (as in Card, Devicienti, and Maida 2014) does not modify our results.

Table A2—Estimates of the Strength of Sorting η by Subgroups

	Economic profits		GOS		AP		
Group	Per worker	Total	Per worker	Total	Per worker	Total	ROE
AQ6 All workers	0.540	0.518	0.540	0.527	0.521	0.531	0.542
Men	0.514	0.490	0.515	0.495	0.500	0.498	0.509
Female	0.601	0.579	0.600	0.597	0.575	0.604	0.617
White collar	0.507	0.460	0.498	0.476	0.488	0.483	0.501
Blue collar	0.549	0.535	0.555	0.544	0.534	0.549	0.555
Aged 20–35	0.523	0.495	0.523	0.507	0.505	0.509	0.510
Aged 35–50	0.510	0.496	0.506	0.500	0.483	0.509	0.541
Aged 50–65	0.674	0.674	0.686	0.681	0.661	0.680	0.680
Manufacturing	0.478	0.570	0.497	0.560	0.498	0.563	0.519
Service	0.557	0.589	0.560	0.591	0.540	0.579	0.540

Notes: This table shows the estimates of the strength of sorting η for the indicated groups. η is estimated from worker transitions to the current firm type p_{ij} from the previous firm type p_{ij}^{PREV}. All transitions are mediated by an interim unemployment spell. Reported estimated are all statistically significant: p-values < 0.001.

corresponds to the bottom 1 percent of the wage distribution).[36] We also eliminate unusually high wages by dropping wages higher than the ninety-ninth percentile of the overall wage distribution. Finally, our main estimates of the sign of sorting are based on regressions that include firm fixed effects. This further restricts the estimation sample to firms with at least two movers.

C. *Strength of Sorting by Subgroups*

Table A2 reports the estimates of the strength of sorting η for different measures of profits and for different subgroups of workers. Estimated values are similar across subgroups, with higher values for the oldest workers (those aged 50 to 65) and for women.

D. *Local Estimates for γ (Sign of Sorting)*

Figure A1 presents a Kernel nonparametric regression of the estimated γ_j from equation (7) on the firm type. As seen from Figure 1, the sets of workers accepted by firms of different types are intertwined. Then, there is positive sorting in the economy.

E. *Proof of Lemma 1*

First, we can express $\tau(p,\varepsilon)$ as follows:

$$\text{(A1)} \quad \tau(p,\varepsilon) = 2\int_0^1\int_0^{\varepsilon_n}\left[\Pr\left(p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\right) - \Pr\left(p_n < p_m \,|\, \varepsilon_n, \varepsilon_m\right)\right] d\varepsilon_m d\varepsilon_n$$

$$= 2\int_0^1\int_0^{\varepsilon_n}\left[2\Pr\left(p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\right) - 1\right] d\varepsilon_m d\varepsilon_n.$$

[36] Information about contractual minimum wages (inclusive of any cost-of-living allowance and other special allowances) was obtained from records of the sector-wide national contracts.

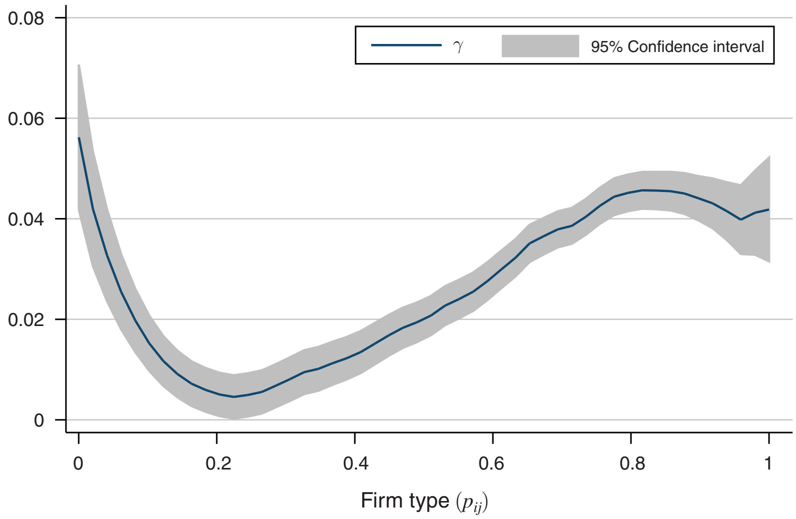

FIGURE A1. LOCAL ESTIMATES OF Γ (*sign of sorting*)

Notes: This figure presents local estimates of the sign of sorting [γ_j in equation (7)]. Kernel nonparametric regression. Kernel type is Epanechnikov. Firms are ordered by economic profits per worker.

And similarly,

$$\begin{aligned}\tau(p,w) &= 2\int_0^1\int_0^{\varepsilon_n}\Big[\Pr\big(w_n > w_m, p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\big) + \Pr\big(w_n < w_m, p_n < p_m \,|\, \varepsilon_n, \varepsilon_m\big) \\ &\qquad - \Pr\big(w_n > w_m, p_n < p_m \,|\, \varepsilon_n, \varepsilon_m\big) - \Pr\big(w_n < w_m, p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\big)\Big]\, d\varepsilon_m d\varepsilon_n \\ &= 2\int_0^1\int_0^{\varepsilon_n}\Big[\Pr\big(w_n > w_m \,|\, \varepsilon_n, \varepsilon_m\big)\big[\Pr\big(p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\big) - \Pr\big(p_n < p_m \,|\, \varepsilon_n, \varepsilon_m\big)\big] \\ &\qquad - \Pr\big(w_n < w_m \,|\, \varepsilon_n, \varepsilon_m\big)\big[\Pr\big(p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\big) - \Pr\big(p_n < p_m \,|\, \varepsilon_n, \varepsilon_m\big)\big]\Big]\, d\varepsilon_m d\varepsilon_n.\end{aligned}$$

Then,

$$\text{(A2)} \qquad \tau(p,w) = 2\int_0^1\int_0^{\varepsilon_n}\big[2\Pr\big(w_n > w_m \,|\, \varepsilon_n, \varepsilon_m\big) - 1\big]\big[2\Pr\big(p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\big) - 1\big]\, d\varepsilon_m d\varepsilon_n.$$

Next, recall that ν_i are i.i.d. Then, for all $\varepsilon_n > \varepsilon_m$, $\Pr\big(p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\big) \geq 1/2 \Leftrightarrow \alpha \geq 0$. As a result, $\text{sign}\big(\tau(p,\varepsilon)\big) = \text{sign}(\alpha)$. For all $\varepsilon_n > \varepsilon_m$, $\Pr\big(w_n > w_m \,|\, \varepsilon_n, \varepsilon_m\big) > 1/2$. Then, in (A2), $2\Pr\big(w_n > w_m \,|\, \varepsilon_n, \varepsilon_m\big) - 1 > 0$. So $\text{sign}\big(\tau(p,w)\big) = \text{sign}(\alpha)$. Next,

$$\begin{aligned}\big|\tau(p,w)\big| &\leq 2\int_0^1\int_0^{\varepsilon_n}\big|2\Pr\big(w_n > w_m \,|\, \varepsilon_n, \varepsilon_m\big) - 1\big|\big|2\Pr\big(p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\big) - 1\big|\, d\varepsilon_m d\varepsilon_n \\ &\leq 2\int_0^1\int_0^{\varepsilon_n}\big|2\Pr\big(p_n > p_m \,|\, \varepsilon_n, \varepsilon_m\big) - 1\big|\, d\varepsilon_m d\varepsilon_n = \big|\tau(p,\varepsilon)\big|.\end{aligned}$$

Next, let Y and X be random variables. Say Y is *left tail decreasing* in X if $\Pr\big(Y \leq y \,|\, X \leq x\big)$ is a nonincreasing function of x for all y. Similarly, Y is *right tail increasing* in X if $\Pr\big(Y > y \,|\, X > x\big)$ is a nondecreasing function of x for all y. Take random variables p and ε as expressed in equation (9). Take $\alpha > 0$. Then,

$$\Pr\big(P \leq p \,|\, \tilde{\varepsilon} \leq \varepsilon\big) = \Pr\big(\alpha\psi_p(\tilde{\varepsilon}) + \nu_i \leq p \,|\, \tilde{\varepsilon} \leq \varepsilon\big) = \Pr\big(\nu_i \leq p - \alpha\psi_p(\tilde{\varepsilon}) \,|\, \tilde{\varepsilon} \leq \varepsilon\big)$$

$$= \Big[\int_0^{\varepsilon} \Pr\big(\nu_i \leq p - \alpha\psi_p(\tilde{\varepsilon})\big)\, d\tilde{\varepsilon}\Big]\Big(\int_0^{\varepsilon} d\tilde{\varepsilon}\Big)^{-1} = \Big[\int_0^{\varepsilon} \Pr\big(\nu_i \leq p - \alpha\psi_p(\tilde{\varepsilon})\big)\, d\tilde{\varepsilon}\Big]\varepsilon^{-1}.$$

Then,

$$\frac{\partial \Pr\big(P \leq p \mid \tilde{\varepsilon} \leq \varepsilon\big)}{\partial \varepsilon} = \Pr\big(\nu_i \leq p - \alpha\psi_p(\varepsilon)\big)\,\varepsilon^{-1} + \Big[\int_0^{\varepsilon} \Pr\big(\nu_i \leq p - \alpha\psi_p(\tilde{\varepsilon})\big)\, d\tilde{\varepsilon}\Big](-1)\,\varepsilon^{-2}$$

$$= \varepsilon^{-1}\Big[\Pr\big(\nu_i \leq p - \alpha\psi_p(\varepsilon)\big) - \Big[\int_0^{\varepsilon} \Pr\big(\nu_i \leq p - \alpha\psi_p(\tilde{\varepsilon})\big)\, d\tilde{\varepsilon}\Big]\varepsilon^{-1}\Big] < 0,$$

since $\Pr\big(\nu_i \leq p - \alpha\psi_p(\varepsilon)\big)$ is decreasing in ε. Similarly,

$$\Pr\big(P > p \,|\, \tilde{\varepsilon} > \varepsilon\big) = \Pr\big(\alpha\psi_p(\tilde{\varepsilon}) + \nu_i > p \,|\, \tilde{\varepsilon} > \varepsilon\big)$$

$$= \Big[\int_{\varepsilon}^{1} \Pr\big(\nu_i > p - \alpha\psi_p(\tilde{\varepsilon})\big)\, d\tilde{\varepsilon}\Big](1-\varepsilon)^{-1}.$$

Then,

$$\frac{\partial \Pr\big(P > p \,|\, \tilde{\varepsilon} > \varepsilon\big)}{\partial \varepsilon} = -\Big[\Pr\big(\nu_i > p - \alpha\psi_p(\varepsilon)\big)\Big](1-\varepsilon)^{-1}$$

$$+ \Big[\int_{\varepsilon}^{1} \Pr\big(\nu_i > p - \alpha\psi_p(\tilde{\varepsilon})\big) d\tilde{\varepsilon}\Big](-1)(1-\varepsilon)^{-2}(-1)$$

$$= (1-\varepsilon)^{-1}\Bigg[-\Pr\big(\nu_i > p - \alpha\psi_p(\varepsilon)\big) + \frac{\int_{\varepsilon}^{1} \Pr\big(\nu_i > p - \alpha\psi_p(\tilde{\varepsilon})\big)\, d\tilde{\varepsilon}}{1-\varepsilon}\Bigg] > 0,$$

since $\Pr\big(\nu_i > p - \alpha\psi_p(\varepsilon)\big)$ is increasing in ε.

Finally, take the case $\alpha > 0$. Then, the random variable p is both left tail increasing and right tail increasing in ε. Then, by Proposition 2.3. in Capéraà and Genest (1993), $\rho(p, \varepsilon) \geq \tau(p, \varepsilon) \geq 0$. The case $\alpha \leq 0$ can be shown similarly. ∎

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1.	Is Arizona, the University of Arizona? Is Berkely, the University of California, Berkeley? And what is Bergen?	
2.	I changed to "the Appendix" from Appendix B because you have one appendix.	
3.	Do you mean in both levels rather than both in levels?	
4.	Is "being increasing" correct? Should it be "being increased"?	
5.	Is something missing after "we can learn about..."?	
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