MONOPSONY IN LABOR MARKETS: A META-ANALYSIS

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When jobs offered by different employers are not perfect substitutes, employers gain wage-setting power; the extent of this power can be captured by the elasticity of labor supply to the firm. The authors collect 1,320 estimates of this parameter from 53 studies. Findings show a prominent discrepancy between estimates of direct elasticity of labor supply to changes in wage (smaller) and the estimates converted from inverse elasticities (larger), suggesting that labor market institutions may rein in a substantial amount of firm wage-setting power. This gap remains after they control for 22 additional variables and use Bayesian Model Averaging and LASSO to address model uncertainty; however, it is less pronounced for studies employing an identification strategy. Furthermore, the authors find strong evidence that implies the literature on direct estimates is prone to selective reporting: Negative estimates of the elasticity of labor supply to the firm tend to be discarded, leading to upward bias in the mean reported estimate. Additionally, they point out several socioeconomic factors that seem to affect the degree of monopsony power.

Economic intuition tells us that when employers cut wages, workers respond by cutting their labor supply or by leaving their employer in pursuit of better options. This simple argument omits a number of important considerations, such as non-compete agreements, geographic isolation, moving costs, or non-monetary worker preferences. When workers are reluctant to explore outside options, firms possess wage-setting power (or

monopsony power), the extent of which depends on the elasticity of labor supply that the firm faces (η).

The degree of firms' wage-setting power is important: Recent studies point out that significant monopsony power can explain many empirical puzzles, such as bunching in wages (Dube, Manning, and Naidu 2018) or wage dispersion across firms (Card, Cardoso, Heining, and Kline 2018). The extent of labor market monopsony also has profound implications for how labor market policies affect workers and firms. Most notably, minimum wage increases are less likely to cause job loss in markets with a higher degree of monopsony. For regulators, it is important to identify conditions under which firms possess monopsony power. In a complementary article in this issue, Manning (2020) provides a thorough review of theoretical considerations of monopsony models and the current state of the literature. He also explores implications for various labor market phenomena that can be explained by a monopsonistic model of the labor market.

Estimates of η reported in the empirical literature vary widely. There have been attempts to isolate some factors that drive this variation. Manning (2003) applied different empirical methods to the same labor market data and compared results; Ransom and Oaxaca (2010) examined differences in pay of male and female workers within a single firm. Such within-study comparisons can shed light on the importance of some features of methodology and data, but they fall short of explaining the overall variation in estimates reported by the literature. Estimates may vary on account of differences in the "true" value of η across data sets that feature different demographics, occupations, or geographical regions. They may also vary with the estimation strategies that researchers employ, study quality, or preferences of the profession, which give some estimated values a higher probability of being reported.

To disentangle the sources of variation in the estimates of η, we conduct the first meta-analysis of the literature, using 1,320 estimates reported in 53 studies. First, we investigate whether certain results have higher likelihood of being reported—in other words, whether publication bias occurs in the monopsony literature. Second, we model the variation in estimates using meta-regressions that include 23 measures of the studies' methodology, data, and publication characteristics. Finally, we provide estimates of the average η conditional on studies employing best practices. In doing so, we offer a measure of how far, on average, labor markets deviate from perfectly competitive behavior.

Estimating the Elasticity of Labor Supply to the Firm

During the 20th century, discussion of monopsony power in the labor literature largely focused on the pure monopsony model in which a single firm comprised the entirety of demand for labor in a market (e.g., in a company

town).[1] In consequence, relatively little attention was paid to the more general case of imperfect competition, in which several competing firms exercise wage-setting power. Yet the foundation for this thinking about imperfect competition was laid more than 85 years ago. Robinson (1933) described three specific reasons why the perfectly competitive model of the labor market may fail, even when many firms are in the market competing for labor. She argued that a firm may end up facing an upward-sloping labor supply curve because of geographical isolation, workers' non-monetary preferences, or workers' ignorance of opportunities existing at other firms. Such labor markets, in which a firm faces upward-sloping supply despite the presence of many competitors, are termed monopsonistic (or oligopsonistic).

Manning's (2003) book *Monopsony in Motion* inspired a conceptual shift in the literature by applying the Burdett and Mortensen (1998) model to formally model monopsonistic labor markets, in which firm wage-setting power stems from search frictions. He also provided a straightforward estimation framework, paving the way for a new empirical literature on monopsony. More recent work has also begun to revisit causes of market power, such as legal restrictions to mobility (Naidu 2010; Naidu and Yuchtman 2013; Balasubramanian et al. 2018; Krueger and Ashenfelter 2018), differentiated jobs (Card, Cardoso, et al. 2018), moving costs (Ransom 2019), and labor market concentration (Brummund 2011; Webber 2015; Azar, Marinescu, and Steinbaum 2017; Benmelech, Bergman, and Kim 2018; Rinz 2019). Broadly, these causes can be categorized into factors related to concentration (oligopsonistic markets) and differentiated jobs or frictions to mobility (monopsonistically competitive markets). Berger, Herkenhoff, and Mongey (2019) discussed welfare and practical implications of these market structures, and Naidu and Posner (2019) and Gibbons, Greenman, Norlander, and Sorensen (forthcoming) decomposed market power into oligopsonistic and monopsonistically competitive components.

The relationship between wage-setting power and η arises when a firm chooses labor input L to maximize profit Π while facing an upward-sloping labor supply curve:

$$(1) \qquad \Pi = \max[p \times f(L) - w(L) \times L]$$

Here, p is the price, $f(.)$ is the production function, and $w(L)$ is the firm's wage, which depends on the number of workers hired. The solution,

$$(2) \qquad w(L) = MRP_L \frac{\eta}{1 + \eta},$$

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[1]Manning (2003) demonstrated this by examining labor economics textbooks. Other fields adopted models of imperfect competition, despite the presence of many firms in the market, on account of factors such as differentiated products (e.g., Krugman 1980; Berry, Levinsohn, and Pakes 1995; Melitz 2003).

links the firm's wage to two factors: the marginal revenue product of labor (MRP_L), and $\eta \equiv \frac{\partial L}{\partial w}\frac{w}{L}$, the elasticity of labor supply to an individual firm with respect to the wage. If supply is perfectly elastic (and $\eta = \infty$), then the last worker hired is paid her worth to the firm: Assuming firms can exercise all of their potential monopsony power, Equation (2) implies $w = MRP_L$. If $\eta = 9$, the worker is paid 90% of her worth to the firm, and half her worth if $\eta = 1$. It is, however, unclear whether firms can exercise all of their monopsony power, as factors such as minimum wages, union contracts, social norms, worker responses to perceptions of fairness (see Dube, Giuliano, and Leonard 2019), or optimization costs (Dube et al. 2018; Manning 2020) may also affect wage. Nevertheless, this simple model provides important insight into how monopsony power affects wages, and η provides insight into the degree of firms' wage-setting power.

Researchers have employed several approaches to estimate η. One approach involves a direct regression of the number of workers employed at a given firm on their wages:

$$(3) \qquad \ln(L_i) = \eta \cdot \ln(w_i) + \xi_i$$

where L_i is labor employed by the firm, and w_i denotes wages paid. Studies employing this method typically produce estimates of $\hat{\eta}$ that do not exceed 2, implying workers are paid less than two-thirds of their value to the firm, assuming firms can fully exercise their monopsony power (e.g., Bodah, Burkett, and Lardaro 2003; Falch 2010; Staiger, Spetz, and Phibbs 2010; and others).

A related approach also using the stock of workers employed by a firm reverses the left- and right-hand sides of the regression in Equation (3):

$$(4) \qquad \ln(w_i) = \chi \cdot \ln(L_i) + \zeta_i$$

where χ is the inverse elasticity of labor supply. Both of the above techniques would fit into what Manning (2020) terms "wage elasticity of the labor supply curve to employers." Though one might expect to find that $\hat{\eta} = \frac{1}{\chi}$, the most common estimates of χ lie below $1/2$, with few exceeding this mark (e.g., see Fakhfakh and FitzRoy 2006; Sulis 2011; Matsudaira 2014). As pointed out by Manning (2003), this inconsistency suggests possible structural differences between the two estimation methods.

As an alternative to these stock-based approaches, Manning (2003) proposed a turnover-based approach to estimate monopsony power, motivated by the idea that perfect competition in labor markets fails because of search frictions, which Manning (2020) now refers to as "modern monopsony." This framework builds on a simplified Burdett and Mortensen (1998) search model, in which firms face search costs, and frictions inhibit worker mobility between jobs. Workers separate from jobs paying lower wages; the job separation rate is a function of the wage. Card and Krueger (1995) pointed out that η can be characterized by how the wage affects worker

inflows (through recruitment) and how it affects worker outflows (through separations). Specifically:

$$(5) \qquad\qquad \eta = \eta_R - \eta_s,$$

where η_s is the elasticity of separations and η_R is the elasticity of recruitment. Reliable data to competently estimate both η_s and η_R is rare. A practical solution was suggested by Manning (2003): In a steady state, the elasticities of separation and recruitment should be linked through $\eta_s = -\eta_R$. From this, two additional estimation strategies arise:

$$(6) \qquad\qquad \eta = -2\eta_s$$

$$(7) \qquad\qquad \eta = 2\eta_R$$

Estimating the recruitment elasticity requires information on how many qualified applicants a position received. This kind of data is hard to come by, so few papers have estimated η_R. Using high-quality administrative data on Norwegian teachers, a field experiment in Mexico, and field data from Amazon Turk, Bó, Finan, and Rossi (2013), Falch (2017), and Dube, Jacobs, Naidu, and Suri (2020), respectively, estimated the elasticity of recruitment. Estimating the separation elasticity requires data on employee wages and job tenure. This approach is more common than the elasticity of recruitment; it was adopted by Ransom and Sims (2010), Booth and Katic (2011), and Depew and Sørensen (2013). Estimates obtained using recruitment elasticities appear to be slightly lower than those produced using separation elasticities. An important research question is whether the assumption of $\eta_s = -\eta_R$ is in fact justified.

Finally, some researchers employed techniques that impose more structure, for example, Fleisher and Wang (2004), Ogloblin and Brock (2005), Naidu, Nyarko, and Wang (2016), and Dobbelaere and Mairesse (2013). Some of this work generally fits into the mold of what Manning (2020) refers to as "new classical monopsony."[2]

Another important question is whether, and to what extent, estimates of η are biased due to potential endogeneity in the relationship between wages and employment. An identification strategy is therefore crucial. Studies estimating η via the regression model in Equation (3) can use firm-specific shocks to wage to identify supply. This approach was taken by Falch (2010), who used wage premiums paid to teachers in schools facing teacher shortages. Studies that estimate χ with the regression model in Equation (4) require labor demand shifters for identification. Matsudaira (2014) exploited increases in demand for nurses resulting from changes in staffing regulations. Studies using the separation approach can instrument for

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[2]Manning (2020) describes two additional approaches. "Measuring variations in monopsony power" typically does not estimate η but instead explores correlations between concentration and wages. "Thoroughly modern monopsony" is a new framework he proposes; no papers in our study clearly fit these molds. His article also includes several papers published after the end of our data collection.

worker wages to purge unobserved individual heterogeneity. Ransom and Sims (2010) used wages based on union contracts as an instrument. For the estimation of η_R, Bó et al. (2013) ran a field experiment to generate exogenous variation in wages.

Data and Sample Characteristics

We employed Google Scholar to search for relevant studies. We selected search parameters based on two criteria: 1) the search returns papers related to monopsony, and 2) it returns papers that *estimate* parameters of monopsony power. To verify the comprehensiveness of our search, we also studied references of the papers returned from our Google search and prior surveys for other matches. We adopted two inclusion criteria. First, the study needed to present estimates allowing for computing η. We therefore eliminated papers that examine the relationship between measures of labor market concentration and wages, papers estimating the firm-size wage effect (unless the effect was claimed by the authors to be an estimate of η), and papers that report estimates of the elasticity of labor supply to an entire labor market, rather than to an individual firm. Our second inclusion criterion was that the study needed to report sufficient information to compute a standard error. We found 53 such studies, providing 1,320 estimates. Publication years spanned 1977 through 2019. To investigate how estimates of η are affected by various aspects of study design, we also collected information on 23 features related to the data, methodology, and publication characteristics. These variables are summarized in Online Appendix Table A1 and are discussed below in the section titled Why Do Estimates of Supply Elasticity Vary? Further details on our search and data construction are available in Appendix F; see Appendix G.1 for a discussion on standard errors.

Estimates of η vary depending on specifications used by researchers. We term estimates "direct" if η can be arrived at through a linear transformation of the authors' estimates (e.g., studies that estimate η through η_s).[3] These estimates make up 1,140 of our 1,320 estimates. They are depicted in Figure 1(a), with the median estimate approximately 1.4, which implies that assuming firms can use all of their monopsony power, workers are paid 58% of their marginal product—strong evidence for monopsony. This distribution is bell-shaped but highly skewed with many estimates clustering below the median.

Figure 1(b) displays the distribution of 180 estimates of the inverse of η (parameter χ in the model in 4). The median inverse elasticity is approximately 0.07, corresponding to a supply elasticity around 14 and a wage markdown of only 7%. This inconsistency between direct and inverse estimates may suggest structural differences between the two approaches.

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[3]Note that this notation is different from the terminology of Manning (2003), who used the term "direct regression" to exclusively refer to "stock"-based regressions of the wage on the stock of labor (see the model in Equation (4)).

Figure 1. Estimates of Supply Elasticity: "Direct" vs. "Inverse"
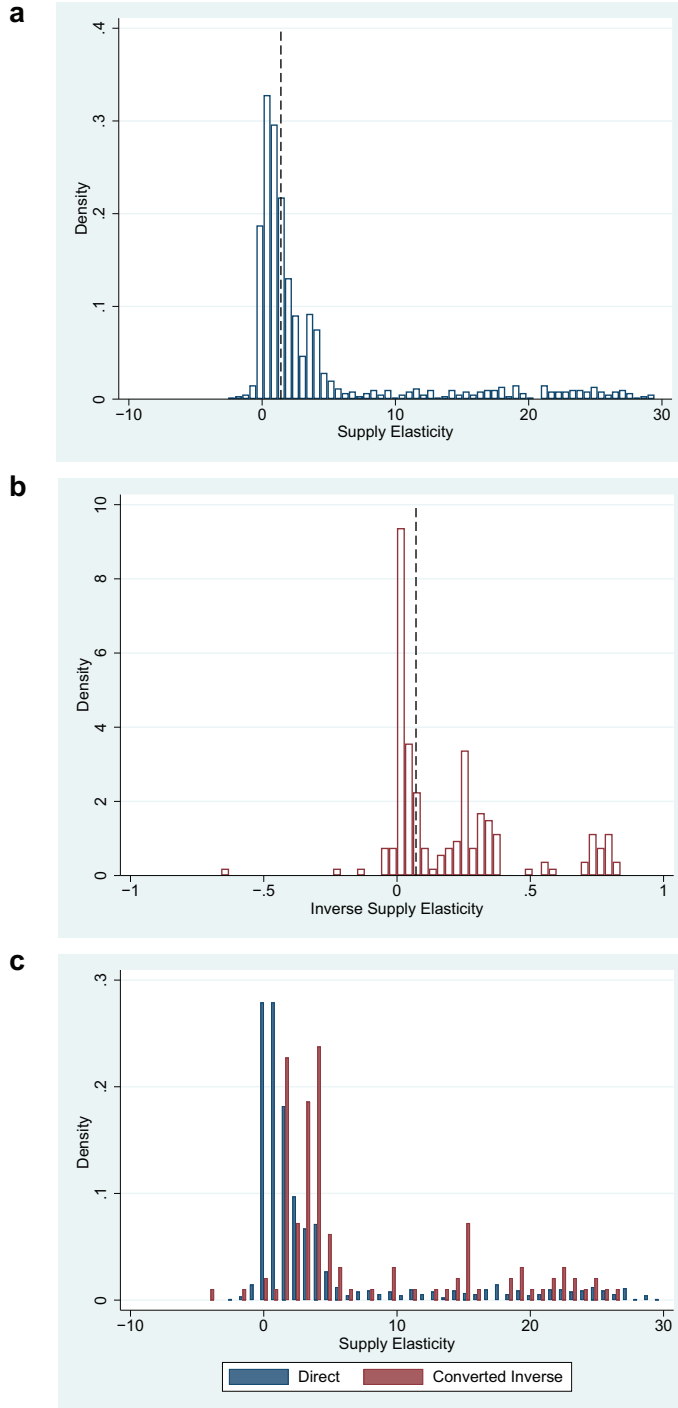
Notes: The figure displays the distribution of estimates of the elasticity. Figure 1(a) shows estimates of η that were obtained via the "direct" methods; that is, methods that allow for a calculation of η via a linear transformation (i.e., from model (3), using separation or recruitment rates, or performing a structural estimation). Figure 1(b) shows estimates of χ obtained from regression (4). We then convert these estimates to the elasticity of labor supply to the firm using $\eta = \frac{1}{\chi}$ and plot the pooled data set in Figure 1(c) (here, we show only estimates between –10 and 30).

Table 1. Supply Elasticity Estimates by Data and Methods

	Unweighted				Weighted				
	Mean	Median	5%	95%	Mean	Median	5%	95%	N
All	10.58	1.68	−0.15	31.32	7.07	1.69	−0.27	19.96	1,320
Europe	6.96	1.49	0.24	19.49	10.42	2.10	0.34	21.98	347
Other advanced	5.93	1.73	−0.26	25.92	2.33	1.59	−0.39	16.65	837
Developing	48.48	2.15	−0.30	275.48	19.74	1.25	−0.35	126.42	136
Nurses	0.95	1.38	−4.38	4.10	−2.65	0.77	−27.36	3.79	78
Teachers	3.08	2.95	1.04	5.44	5.07	3.65	1.06	17.06	102
Inverse	47.39	5.24	−6.10	232.99	29.65	4.50	−27.17	165.84	180
Direct	4.77	1.41	−0.14	24.69	2.55	1.47	−0.05	8.56	1,140
Separations	5.85	1.73	−0.24	25.87	3.05	1.74	0.21	16.21	868
Recruitments	2.06	2.53	−0.03	4.73	1.43	0.77	−0.03	4.07	92
L on W	0.86	0.96	0.05	1.63	0.75	0.84	0.05	1.51	67
Structural	1.05	0.33	0.13	5.54	1.98	0.38	−0.35	8.56	113
Top journal	12.24	11.34	0.18	30.78	4.51	1.92	0.18	19.18	343

Notes: 5% and 95% denote corresponding percentiles. "Weighted" refers to summary statistics based upon weighting by inverse number of estimates reported in study, giving each study equal weight. L on W indicates regression with labor stock being left-hand-side variable and wage being right-hand-side variable.

Alternatively, papers estimating χ could, by chance, study less monopsonistic markets or use techniques that yield larger estimates.

Figure 1(c) combines all estimates. Again, we note striking differences that suggest these two sets of results come from different distributions. Table 1 reports sample statistics for the full sample, as well as the subsamples of estimates obtained through direct and inverse methods. The mean estimate of η is 10.58, whereas the median is much lower—only 1.68. We observe a similar pattern when weighting estimates by the inverse number of estimates per study, giving equal weight to each study (regardless of how many estimates it reports). See Appendix G.2 for more on weighting. In Table 1, sample means for direct estimates are lower (4.77), and the means for inverse estimates are higher (47.39), and very different from the median of 5.24.

Elasticity estimates vary across other dimensions as well. First, the mean and median for estimates coming from developing countries are larger than those for Europe and other advanced economies. However, when the estimates converted from inverse elasticities are excluded, the mean elasticity estimate for developing countries drops from 48.48 to only 1.14, suggesting that structural differences between inverse and direct estimations are in fact important. We also observe that estimates obtained on European data are somewhat higher than those from other advanced economies—although, as in the case of developing countries, when conditioning on estimates obtained using direct methods, the difference becomes much more modest.

We also observe differences across occupations. Much of the literature, and 180 of our estimates, focuses exclusively on nurses and teachers because of employer concentration in these markets. Our sample statistics support the notion that these markets are less competitive than the markets for other occupations. Overall, variation is low in direct estimates, though the skewing of their distribution in Figure 1(a) suggests potential under-reporting of negative results. We investigate this in the next section.

Before proceeding with the estimations, we must improve comparability between inverse and non-inverse estimates. All estimates of η obtained through direct methods lie between -3 and 41. Some estimates of the inverse elasticity $\hat{\chi}$ lie very close to zero—they become enormous when converted to $\hat{\eta}$. To ensure that we are working with comparable data, we cut outliers by 2.5% from each tail. This cut leaves a sample of 1,254 estimates, among which 136 are converted from the inverse elasticity. Appendix Table A1 compares sample statistics of our variables for the full sample and the trimmed subsample, showing no notable differences. In the next two sections we will focus on this subsample, although we also report results for alternative outlier treatments. For details, see Appendix G.3.

Publication Bias

Estimates of η based on direct methods cluster close to zero, implying that the underlying parameter is also close to zero. When estimated on random data using standard techniques, a model with a small, positive, underlying parameter would sometimes yield estimates that lie quite far from the true value on account of large standard errors. Some estimates would be large and positive, while others, given the small true value, would be negative. If all estimates of η are reported, averaging across results should nevertheless yield a mean close to the underlying true parameter. If, however, negative estimates are under-reported, the mean of this truncated distribution would be biased. Here, we will investigate whether this literature is prone to such "selective reporting."[4] Selective reporting was documented, for example, in literatures on returns to schooling (Ashenfelter et al. 1999), the employment effects of the minimum wage (Card and Krueger 1995; Doucouliagos and Stanley 2009), the effects of currency unions on trade (Rose and Stanley 2005; Havranek 2010), and the degree of excess sensitivity in consumption to predictable changes in income (Havranek and Sokolova 2020).

Positive values of η, however large, can easily be interpreted by researchers, but the same cannot be said of the negative values: They imply a downward-sloping supply curve. It is possible that researchers who obtain

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[4]"Selective reporting" is a better, more general description than "publication bias," as observed under-reporting of results may not be related to the publication process. Nevertheless, the literature has converged on the "publication bias" term (e.g., Card and Krueger 1995; Ashenfelter, Harmon, and Oosterbeek 1999; Stanley 2001; Efendic, Pugh, and Adnett 2011; Rusnak, Havranek, and Horvath 2013; Havranek 2015); we also use it for consistency.

Figure 2. Funnel Plot of "Direct" Elasticity Estimates
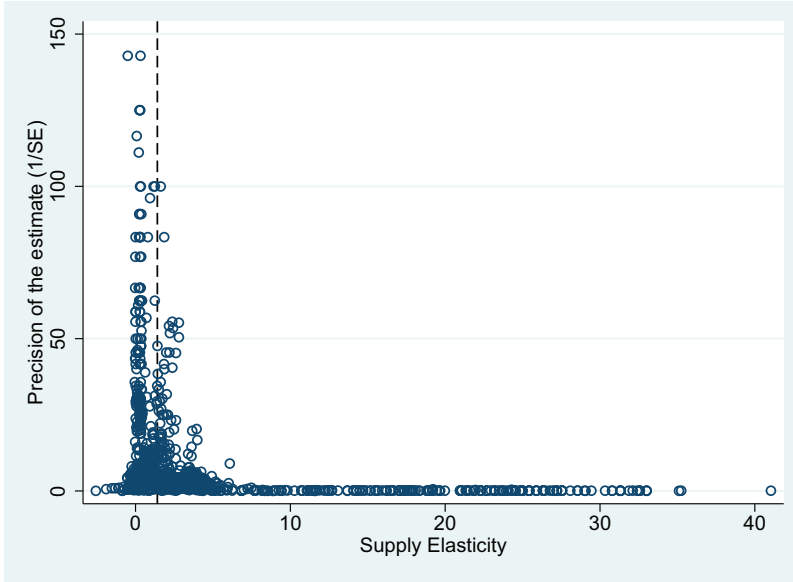
Notes: The figure plots estimates that were obtained via the "direct" method, i.e., estimates shown in Figure 1(a). In the absence of "selection for the right sign," the funnel would exhibit symmetry around the most precise results.

negative results would see them as an indication of their model being wrong and would therefore engage in further specification searches. These patterns, albeit unintentional, would lead to a mean reported estimate that exaggerates the "true" underlying effect.

Figure 2 presents a funnel plot of estimates reported by studies of the direct elasticity. The values of estimates obtained are plotted against their precision. The most precise estimates cluster close to zero; this seems to imply that the underlying true elasticity parameter is small. In the absence of selection for the "right sign," the funnel should appear symmetrical, with less precise estimates being distributed around the "true" effect (see Egger, Smith, Scheider, and Minder 1997). The funnel on Figure 2 is skewed: The right tail is much more prominent compared to the left tail, which suggests publication bias in the form of selection for a positive sign.

To investigate publication bias, we conduct the funnel asymmetry test used by Card and Krueger (1995) and others. Common estimation methods rely on the assumption that the ratio of the estimate to its standard error is t-distributed. Under this assumption (or assuming any other symmetrical distribution), the estimate and the standard error should be uncorrelated. Therefore, in a regression of the estimate on its standard error, the coefficient λ on the standard error should be zero:

$$(8) \qquad \hat{\eta}_{ij} = \eta_0 + \lambda \cdot SE\left(\hat{\eta}_{ij}\right) + u_{ij}$$

Table 2. Testing for Publication Bias

	OLS	FE	BE	Study
Panel A: All estimates				
SE	1.443	0.400	1.258	0.562
	(0.000)	(0.001)	(0.000)	(0.072)
	[0.000]	[0.000]	[0.000]	[0.000]
Constant	1.733	4.009	2.175	1.837
	(0.004)	(0.000)	(0.055)	(0.000)
	[0.000]		[0.000]	[0.000]
Studies	46	46	46	46
Observations	1,118	1,118	46	1,118
Panel B: Published estimates only				
SE	1.800	0.491	2.125	1.832
	(0.000)	(0.000)	(0.000)	(0.000)
	[0.000]	[0.000]	[0.000]	[0.000]
Constant	1.322	4.231	1.272	1.083
	(0.004)	(0.000)	(0.016)	(0.000)
	[0.000]		[0.000]	[0.000]
Studies	38	38	38	38
Observations	995	995	38	995

Notes: Estimation results from Equation (8). Standard errors (SE) clustered at study level; p values shown in parentheses. Wild bootstrapped p values in square brackets, implemented via the Stata's boottest command (Roodman 2018) with Rademacher weights and 9,999 replications. Package does not allow computation of bootstrapped p value for constant term in fixed-effects specification. BE, study-level between effects; FE, study-level fixed effects; OLS, ordinary least squares; "Study" uses inverse of number of estimates reported in study as weight.

where $\hat{\eta}_{ij}$ is the i-th estimate from the j-th study, $SE(\hat{\eta}_{ij})$ is its standard error, and u_{ij} is the disturbance term. Systematic under-reporting of negative estimates would thus result in a positive λ in Equation (8)—see Stanley (2005) for a detailed discussion. The coefficient λ can thus be viewed as measuring publication bias severity.[5]

We estimate Equation (8) and report results in panel A of Table 2, clustering standard errors at the study level. As our number of clusters is relatively small (46), we additionally compute wild bootstrapped clustered p values, as recommended by Cameron, Gelbach, and Miller (2008). The first column of Table 2 shows the results of ordinary least squares (OLS) estimation of Equation (8). The coefficient λ is large, positive, and significant. In the second column, we control for study-level fixed effects, accounting for unobserved study-level characteristics. Again, the estimate of λ is positive and significant, albeit smaller in magnitude. The third column uses only variation between studies and again finds evidence for publication bias, despite

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[5]The estimate of η_0 may approximate the true parameter, but this approximation is unbiased only when publication bias is proportional to the standard error (see Stanley 2008).

a much smaller sample size. We also weight our data by the inverse number of estimates per study, in order to give each study equal weight, and report results in the fourth column. Again, we find evidence for publication bias. Finally, in panel B of Table 2, we exclude estimates from unpublished working papers. In this subsample of published estimates, estimates of λ are significant and of larger magnitude, indicating that selective reporting is a prominent issue for this literature that is not alleviated by the journal refereeing process.

An alternate approach to detecting publication bias, developed by Andrews and Kasy (2019), is to explicitly model the process governing selectivity and estimate relative probabilities of different results being reported that may depend on sign and significance. We discuss this technique in Appendix B and apply it to our direct estimates. We normalize to 1 the reporting probability of positive results significant at the 5% level and estimate the relative reporting probabilities for negative significant, negative insignificant, and positive insignificant results. The findings, reported in Appendix Table B1, show that results with negative Z-scores are dramatically under-reported. Positive results significant at 5% are approximately nine times more likely to be reported than positive but insignificant results. These magnitudes increase when we focus on the subsample of published estimates.[6]

The methods discussed above crucially differ in the assumptions about the data-generating process for elasticity estimates. One way to think about the studies in our sample is to consider them to be estimating one true underlying elasticity; because studies use data with varying levels of noise, the reported estimates would consist of the true elasticity plus noise (presumed symmetrical). Under this assumption and absent publication bias, the observed distribution of estimates should be symmetrical; our analysis of the funnel plot, the OLS, the between effects (BE), and Study specifications in Table 2 all rely on this assumption. There are, however, reasons why elasticity η may vary with the study context (see the following section); this would imply there may not be one true elasticity but rather different true elasticities that vary across studies. Under this assumption, two factors contribute to variation in estimates: the variation in the underlying true parameter and the symmetrical noise in the data. Our fixed-effect estimation (FE in Table 2) allows for this possibility. The Andrews and Kasy (2019) estimation we perform allows for it as well—however, this exercise required making specific assumptions about the distribution of the true effect, and as Andrews and Kasy (2019) noted, the results may be sensitive to the functional form adopted.[7] In the next section we use an alternative modeling

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[6]We report robustness to treatment of outliers with this method in Appendix Table D3 and Table D4.

[7]We follow Andrews and Kasy (2019) and assume the true effect to be *t*-distributed; an argument could be made for using an asymmetric distribution for the true effect that precludes negative values.

assumption: We permit the underlying true effects to vary across studies with a set of observables and explicitly model this variation.

A related caveat pertaining to both the funnel asymmetry and the Andrews and Kasy (2019) approaches is that they rely on an assumption of independence between the estimates produced by latent studies and their standard errors. But there may be aspects of study design that influence both the estimate and the standard error. For example, estimates converted from inverse elasticities in Figure 1 are systematically larger than direct estimates, and so are their standard errors. Indeed, when we perform publication bias tests of Table 2 on the full sample without excluding the inverses, the evidence for publication bias becomes stronger due to this spurious correlation. For this reason, we thus limit our focus to studies of the more homogenous direct estimates.[8]

Other study characteristics could still create similar biases within the subsample of direct estimates. One solution would be to use an instrument for the standard error that is uncorrelated with other aspects of study design. Prior literature has employed the number of observations used to produce the estimate (Havranek and Sokolova 2020). Unfortunately, in our data the number of observations performs poorly in predicting the standard error.[9] We nevertheless report the results in Appendix Tables D1 and D2.

An alternative solution to this endogeneity problem is to control for a broad set of study characteristics that influence the estimation results. In the next section, we build a model that includes 23 such regressors. We find strong evidence for publication bias in this context as well.

Why Do Estimates of Supply Elasticity Vary?

Explanatory Variables

So far, we have noted a few methodological aspects that are likely to have systematic effects on the estimates of η. Most important, the estimates are much higher for studies that measure the inverse supply elasticity, and lower for those employing direct methods. We now move to analyze other aspects of study design that could affect the estimates using all available features that are important and that vary sufficiently across studies. Our goal is to understand the effects that the researcher's data and method choices have on their inference about firms' monopsony power. To this end, we construct a set of 23 explanatory variables that measure the most crucial features of the studies, such as data and study quality, and the most common decisions that researchers make, such as model specification and estimation technique. We group these variables into five categories and discuss

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[8]We also explored modeling publication bias for inverse estimates, finding much less economically significant evidence of publication bias. We did not pursue this further on account of very small sample size and the fact that these estimates and standard errors were obtained via a nonlinear transformation.

[9]The first-stage coefficient is not significant at conventional levels and fails weak instrument tests.

them below. We also present a full list of predictors, their definitions, and summary statistics in Appendix Table A1.

Data Characteristics

Monopsony power may have changed over the years; we therefore control for the midpoint of the data used to produce an estimate. We include the logarithm of the number of observations used to obtain each estimate, as larger data sets may produce more precise results. Employer discrimination or household expectations may cause women to face more search constraints than men face, resulting in fewer job options and thus a lower elasticity of labor supply to the firm. The literature pays considerable attention to whether wage markdowns differ by worker gender. In our sample, 15 papers directly examine gender differences in η whereas others report the female share in the sample they use. We capture this information in a regressor *female share*. For studies that do not report estimates or sample composition for males and females separately, we set *female share* = 0.5. We further explore our treatment of gender in more depth in Appendix G.5.

Country and Occupation

Institutions vary across countries; monopsony power may as well. To our knowledge, no cross-country studies of monopsony power exist—we are the first to gather systematic evidence on this topic with our data spanning 16 countries, and papers on gender alone covering 8 (Australia, Canada, Russia, Norway, Brazil, Italy, Germany, United States). We group country data into three categories: *Developing* (6 countries), *Europe* (7 countries), and *Other Advanced* (United States, Canada, and Australia), which is the reference group (63% of our data). Studies examining the labor market for nurses or teachers comprise 14% of estimates in our sample; we construct indicators for these two variables.

Method and Identification

As discussed above, estimates obtained from inverse supply elasticities appear distinct from those obtained by other methods (that we term "direct"). The former is a "stock-based" estimation approach that uses correlation between the wage and the number of workers employed by the firm (see Equation (4) in the Data and Sample Characteristics section). Manning (2003: 83) argued estimates obtained this way may be biased from unobserved labor supply shocks, "making the slope of the supply curve seem less positive than it really is." Unobserved worker quality, rent sharing, and compensating wage differentials may also create an upward bias in these estimates, a conclusion that our sample statistics support. A firm-specific labor demand shifter could reduce the bias, and a subset of studies in our sample uses this approach. We thus create regressors for identified

and unidentified estimates converted from inverse elasticities. Manning (2003) argued that in stock-based regression (Equation (3)) a bias in the opposite direction may arise. Again, firm-specific shocks (this time to wages) would provide clean identification. In our sample, all estimates gathered through this method are obtained with an identification strategy.

Unlike stock-based methods, turnover-based methodologies using separation or recruitment rates employ individual-level data subjecting them to less simultaneity. Nevertheless, threats to identification still exist. For example, workers with unobservable characteristics who increase their productivity may receive both higher wages and more outside job offers, resulting in higher separation rates. We distinguish between separation-based and recruitment-based estimates obtained with and without an identification strategy. Finally, we control for estimates obtained in models that impose additional structural assumptions (e.g., models with production), with and without an identification strategy.

Estimation Technique

A researcher may use a variety of econometric models to obtain estimates of η. For example, Depew and Sørensen (2013) used a linear probability model, Ransom and Oaxaca (2010) used a probit, and recent studies such as Hirsch, Jahn, and Schnabel (2018) employed survival analysis. Accordingly, we construct indicator variables for a probit or logit and for a hazard model.

Publication Characteristics

Ashenfelter et al. (1999) showed that failing to control for publication bias in the context of meta-regression can result in exaggerated effects attributed to estimation methods. We therefore include an interaction between the standard error of the estimate and an indicator variable for estimates obtained through direct methods to capture publication bias, as discussed earlier.

As supply elasticity estimates may vary with unobserved features of paper quality, we control for publication characteristics. Our sample contains 343 estimates published in a top-five general interest journal or in the *Journal of Labor Economics*; we include a dummy variable collecting such estimates.[10] For the unpublished working papers, we distinguish between those posted with either the National Bureau of Economic Research or IZA Institute of Labor Economics, and all others. We also control for the number of citations per year since the paper first appeared on Google Scholar. This variable could potentially capture some additional aspects of study quality. Alternatively, a relationship between the estimate and citations could

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[10]We also considered including the impact factors of the journals but were forced to exclude this variable because of multicollinearity. For context, when comparing the impact factors for journals for studies included in our analysis, we find that the top journals have impact factors around five times higher than other journals (approximately 2.5 vs. 0.4; Impact = Recursive Impact Factors from https://ideas.repec.org/top/top.journals.recurse.html).

indicate that the profession tends to favor certain results over others, providing additional evidence of publication bias.[11]

We also record each paper's publication year (as in Koetse, de Groot, and Florax 2009; Egger and Lassmann 2012; Valickova, Havranek, and Horvath 2015; or Havranek, Rusnak, and Sokolova 2017).[12] This variable may capture advances in methodologies and empirical practices occurring as the field developed (e.g., better instruments). Alternatively, the focus of journals and researchers may have shifted over time, resulting in altered preferences for monopsony estimates and changing the direction of publication bias, which this explanatory variable may help detect.

Estimation and Results

We consider the following regression model:

$$(9) \qquad \hat{\eta}_{ij} = \alpha_o + \sum_{l=1}^{23} \beta_l X_{l,ij} + u_{ij},$$

where $\hat{\eta}_{ij}$ is estimate i of the supply elasticity reported in study j, $X_{l,ij}$ are measures of study design and quality discussed above and summarized in Appendix Table A1, and u_{ij} is the disturbance term. The model in Equation (9) captures key features of the process governing how researchers obtain estimates of η.

Our dependent variable in Equation (9) is an *estimate* of the true parameter. This model implies that u_{ij} incorporates a sampling error, which may depend on the number of observations and complexity of empirical design that obtained the estimates. The meta-analysis literature often addresses sampling error by using precision weights, which would be efficient if the sampling error were the only component of u_{ij}. However, u_{ij} may also account for the residual heterogeneity in monopsony power. Thus, u_{ij} likely reflects both the sampling error and the variation in η. Lewis and Linzer (2005) and Solon, Haider, and Wooldridge (2015) argued that when the latter is relatively important, unweighted OLS may perform better than the Weighted Least Squares (WLS) approach. Card, Kluve, and Weber (2018) pointed out that studies with larger samples do not always provide more precision: More data in large-scale studies can be offset by their use of more complex techniques. In our sample, the correlation between the number of observations and precision is –0.0342.

For our application, two concerns echo the discussion above. First, as noted earlier, estimates obtained with more complex methods (such as an identification strategy) may be less precise—not because these estimates are

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[11]This variable is common in the meta-analyses literature, see Havranek (2015), Havranek, Horvath, and Zeynalov (2016), Card, Kluve, et al. (2018), Havranek and Sokolova (2020).

[12]Because we have both published and unpublished studies in our sample, we count as "publication year" the year the paper first appeared on Google Scholar—for consistency across the two groups.

inferior but because of the overall complexity of the research design. Precision weights would thus assign lower weight to estimates produced with more sophisticated techniques. Second, unobserved heterogeneity in the true parameter is quite likely in our application as different firms cultivate unique work environments, affecting workers' responsiveness to wage changes. Accordingly, we follow Card, Kluve, et al. (2018) by using an unweighted specification for our baseline. While we do not claim that our methodology explains all variation in the true η, we argue that we employ a second-best option that, nevertheless, allows us to explore variation in estimates reported by the existing literature—and that body of literature is what makes up our sample.

We begin our analysis by pooling both identified and unidentified estimates; we then repeat it for the subsample of identified estimates. In both cases, we check robustness to weighting by the inverse of the number of estimates the associated study reports. This method allows us to determine if baseline results are driven by studies reporting larger numbers of estimates, following Havranek and Irsova (2017) and Gunby, Jin, and Reed (2017). We do not use precision weights in the analysis of the pooled data set for reasons discussed above, but we employ them for the subsample of identified estimates only.

Table 3 presents estimation results for our pooled identified and unidentified sample. We note that the positive association between these estimates and their standard errors discussed earlier remains intact even after controlling for various aspects of study design. This correlation is consistent with our previous conclusion about selective reporting in this literature. We also find that top journals publish higher estimates of η—at least according to the unweighted specification that detects an economically meaningful difference of approximately 3.55. We do not find statistically significant differences between working papers and peer-reviewed publications, or the outlet in which a working paper is published.

Estimates converted from inverse elasticities, both identified and unidentified, are larger by at least 11.27 as compared to the unidentified separations-based reference group. The difference between identified and unidentified estimates based on separation elasticities in general is not statistically significant, and neither is the difference between inverse elasticities obtained with and without an identification strategy. When comparing the magnitudes of the coefficients, however, we note that the gap between estimates constructed using separations and those converted from inverse elasticities becomes smaller once an identification strategy is in place.

This finding is consistent with the argument in Manning (2003) that estimates converted from inverse elasticities may be upward biased from unobserved supply shocks. However, the gap does not disappear entirely, in line with Tucker (2017), who applied the two methods to the same data set and argued that the endogeneity bias alone did not explain the gap between these two estimated elasticities. Indeed, there may be fundamental

Table 3. Why Do Estimates of Supply Elasticity Vary?

Response variable	OLS, unweighted				OLS, study weights			
	Coef.	*SE*	*p value*	*p value (wild)*	*Coef.*	*SE*	*p value*	*p value (wild)*
Data characteristics								
SE non-inverse	0.984	0.307	0.001	0.003	0.730	0.297	0.014	0.092
No obs (log)	0.332	0.258	0.199	0.413	0.305	0.236	0.197	0.343
Midyear of data	−0.027	0.017	0.126	0.318	−0.033	0.025	0.187	0.371
Female share	−2.365	1.788	0.186	0.452	−2.220	1.551	0.153	0.284
F-test (group 1)	14.477		0.006		7.541		0.110	
Country and Occupation								
Developing	2.440	3.074	0.427	0.593	4.992	4.264	0.242	0.494
Europe	0.594	1.141	0.603	0.710	2.057	1.186	0.083	0.144
Nurses	−8.252	5.449	0.130	0.248	−0.871	2.632	0.741	0.803
Teachers	−3.651	2.071	0.078	0.122	−0.598	1.584	0.706	0.702
F-test (group 2)	3.330		0.504		3.106		0.540	
Method and Identification								
Separations, id.	3.481	3.350	0.299	0.462	3.893	2.270	0.086	0.182
Inverse, id.	15.677	6.097	0.010	0.074	11.274	3.467	0.001	0.029
Inverse, not id.	17.542	3.695	0.000	0.008	12.301	3.972	0.002	0.012
Recruitment, id.	3.267	1.719	0.057	0.071	−1.231	1.913	0.520	0.584
Recruitment, not id.	0.209	2.217	0.925	0.944	−3.834	2.424	0.114	0.336
L on W regression, id.	3.280	2.641	0.214	0.156	0.970	2.047	0.636	0.640
Structural & other, id.	−8.536	5.263	0.105	0.158	−8.687	4.658	0.062	0.273
Structural & other, not id.	1.973	1.772	0.265	0.461	−3.605	2.956	0.223	0.299
F-test (group 3)	104.482		0.000		40.324		0.000	
Estimation technique								
Hazard	−0.936	1.890	0.620	0.742	−1.400	1.622	0.388	0.508
Probit, logit, other	−1.283	1.661	0.440	0.625	1.310	1.262	0.299	0.375
F-test (group 4)	0.601		0.741		3.292		0.193	
Publication characteristics								
Top journal	3.551	1.617	0.028	0.058	1.309	1.072	0.222	0.271
Citations	2.357	1.690	0.163	0.329	1.557	1.274	0.222	0.418
Pub. year (Google)	0.147	0.128	0.252	0.354	0.068	0.085	0.424	0.525
NBER or IZA	−1.841	2.099	0.380	0.538	1.189	2.510	0.636	0.735
WP other	−0.199	2.735	0.942	0.955	−0.121	2.728	0.965	0.973
F-test (group 5)	13.802		0.017		5.822		0.324	
Constant	−5.132	3.835	0.181	0.302	−1.711	2.721	0.529	0.651
N	1,254				1,254			

Notes: A detailed description of variables is available in Online Appendix Table A1. As in Table 2, we include only estimates from our trimming discussed in the Data and Sample Characteristics section. We report results obtained under alternative outlier treatments in Online Appendix E. Coef., coefficient; id., estimates obtained with an identification strategy; OLS, ordinary least squares; SE, standard errors.

differences in what these methods measure. The inverse elasticity may measure how much market power firms have when hiring new workers, whereas the separation-based approach reveals the market power firms possess over incumbent workers. Tucker (2017) theorized that market power increases after hiring, as workers develop firm-specific human capital and as job-specific amenities are revealed.

For studies using structural models, estimates depend on the presence of an identification strategy with identified estimates markedly lower than separations-based estimates, and unidentified estimates showing no systematic difference. Recruitment-based identified estimates are close in value to the separation-based identified estimates in the unweighted specification. This result disappears though once we weight data by the inverse number of estimates reported, suggesting that the observed similarity is due to a few studies reporting large numbers of estimates (as opposed to many studies reporting several estimates per study). Finally, we see no statistically significant difference between unidentified separations-based estimates and the stock-based direct estimates from the labor supply on wage approach.

The coefficient estimate on female share is consistently negative, suggesting greater monopsony power over women. Yet, none of these coefficients are statistically significant at conventional levels, which is possibly attributable to measurement error or a lack of precise demographic information in most studies. We explore this further in Appendix G.5, where we find statistical significance when we restrict our sample to estimates that were determined separately for men and women. The results on geography and occupation are generally insignificant, both individually and jointly, though the coefficients for nurses and teachers are consistently negative. The estimates related to nonlinear estimation techniques (hazard, probit, and logit models) are also not significant.[13] There is also no clear evidence of a trend in η over time: Studies published more recently report higher estimates, but studies using newer data report lower elasticity estimates, consistent with monopsony power increasing over time—though neither effect is significant.

The results of Table 3 illustrate the importance of having an identification strategy. We repeat the exercise of Table 3 in Table 4 using a subset of 549 identified elasticity estimates. Here, we also include a specification with precision weights. We again find strong support for selective reporting: In line with evidence from both Table 2 and Table 3, we observe a positive correlation between the identified direct estimates and their standard errors, again pointing toward publication bias in this literature. As before, we document a discrepancy between estimates based on separation and those from inverse elasticities, although the difference is smaller (from 6.80–8.97 as opposed to 11.27–17.54 in the pooled sample). Identification thus helps reconcile estimates obtained via these two approaches.

The same cannot be said about estimates obtained using other techniques. First, we see that estimates based on recruitment elasticities are larger than those based on separations. As discussed in the introductory section, researchers who estimate η based on separations or recruitments must assume steady-state equivalence between the two rates. Results in Table 4

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[13]We further explore how institutions across countries may affect η in subsection Appendix C.3, generally finding insufficient variation to make any conclusions.

Table 4. Why Do Estimates of Supply Elasticity Vary? Identified Estimates Only

Response variable	OLS, unweighted				OLS, study weights				OLS, precision weights			
	Coef.	SE	p value	p value (wild)	Coef.	SE	p value	p value (wild)	Coef.	SE	p value	p value (wild)
Data characteristics												
SE non-inverse	0.969	0.259	0.000	0.095	1.008	0.464	0.03	0.256	1.688	0.148	0.000	0.011
No obs (log)	0.749	0.598	0.211	0.376	0.399	0.692	0.564	0.719	0.195	0.184	0.289	0.516
Midyear of data	−1.391	0.323	0.000	0.033	−0.755	0.357	0.035	0.277	−0.532	0.201	0.008	0.001
Female share	−18.668	11.684	0.11	0.444	−9.913	7.198	0.168	0.324	−6.399	4.196	0.127	0.264
F-test (group 1):	92.177	–	0.000	–	11.337	–	0.023	–	591.887	–	0.000	–
Country and Occupation												
Developing	4.234	7.276	0.561	0.728	8.179	7.178	0.255	0.56	−0.561	0.937	0.549	0.808
Europe	−7.884	4.828	0.102	0.238	−7.138	4.869	0.143	0.381	−7.701	2.542	0.002	0.028
Nurses	−15.155	6.736	0.024	0.089	−14.692	9.894	0.138	0.367	−10.36	4.252	0.015	0.069
Teachers	−7.523	2.486	0.002	0.015	−3.121	2.559	0.223	0.301	−2.632	1.322	0.047	0.096
F-test (group 2):	57.719	–	0.000	–	4.229	–	0.376	–	9.944	–	0.041	–
Method amd identification												
Inverse	8.967	5.47	0.101	0.133	6.804	6.363	0.285	0.409	8.25	1.433	0.000	0.000
Recruitment	6.344	2.979	0.033	0.048	−0.567	3.923	0.885	0.923	5.125	1.891	0.007	0.026
L on W regression	13.636	5.777	0.018	0.108	10.932	8.213	0.183	0.454	10.403	3.551	0.003	0.012
Structural & other	−10.326	3.2	0.001	0.031	−11.185	5.159	0.03	0.305	−1.387	1.234	0.261	0.532
F-test (group 3):	34.819	–	0.000	–	15.793	–	0.003	–	34.603	–	0.000	–
Estimation technique												
Probit, logit, other	−4.375	4.856	0.368	0.424	−6.217	7.568	0.411	0.663	−1.842	1.753	0.293	0.39
F-test (group 4):	0.811	–	0.368	–	0.675	–	0.411	–	1.104	–	0.293	–

(continued)

Table 4. Continued

Response variable	OLS, unweighted				OLS, study weights				OLS, precision weights			
	Coef.	SE	p value	p value (wild)	Coef.	SE	p value	p value (wild)	Coef.	SE	p value	p value (wild)
Publication characteristics												
Top journal	−3.366	5.198	0.517	0.611	−7.926	5.633	0.159	0.459	−4.23	1.616	0.009	0.066
Citations	6.038	1.419	0.000	0.027	5.011	2.532	0.048	0.39	1.628	1.082	0.132	0.241
Pub. year (google)	1.004	0.418	0.016	0.068	0.45	0.498	0.366	0.48	0.476	0.168	0.005	0.025
NBER or IZA	3.594	6.806	0.597	0.677	0.674	5.6	0.904	0.926	−4.979	1.149	0.000	0.004
WP other	−0.866	6.832	0.899	0.912	−4.368	6.816	0.522	0.661	−8.573	1.909	0.000	0.007
F-test (group 5):	26.334	–	0.000	–	6.774	–	0.238	–	45.722	–	0.000	–
Constant	86.515	24.47	0.000	0.074	56.081	29.471	0.057	0.412	33.33	14.888	0.025	0.071
N	549	–	–	–	549	–	–	–	549	–	–	–

Notes: Sample restricted to identified estimates. Left presents unweighted OLS estimation; the middle panel reports inverse of the number of estimates per study as weights results; right reports results using precision weights. Detailed description of all variables is available in Appendix Table A1. As in Table 2 and Table 3, we include only estimates from our trimming discussed in the Data and Sample Characteristics section. We report results obtained under alternative outlier treatments in Online Appendix E. Coef, coefficient; L on W indicates regression with labor stock being left-hand-side variable and wage being right-hand-side variable; OLS, ordinary least squares; SE, standard errors.

seem to imply that this steady-state assumption may not always hold—with the exception of the specification weighted by inverse of estimates reported (study weights). Table 4 also indicates that studies directly regressing employment on wage produce higher estimates, as opposed to studies using structural models. Similar to Table 3, here we find weak evidence linking higher female shares to higher estimated monopsony power. Signs are consistently negative, though marginally significant. In terms of occupation, studies focusing exclusively on the markets for nurses or teachers again produce lower estimates, suggesting that these markets tend to be more monopsonistic. In Appendix G.5, we show evidence that these predominantly female occupations capture part of the effect of gender on η. Finally, we see that once we condition on identified estimates, top journals no longer appear to publish higher estimates of η. We do not find statistically significant differences between working papers and peer-reviewed publications, or between the outlets in which a working paper is published.

We also test the robustness of these results with two alternative estimation approaches, Bayesian Model Averaging and Least Absolute Shrinkage and Selection Operator (LASSO) (Tibshirani 1996). In subsection Appendix G.4, we provide a detailed discussion of results obtained using these models; see also tables in Appendix C.

Best-Practice Estimates

For the final estimates to be useful to the reader, we use our meta-regression results to construct estimates associated with best practices in the literature. To do this, we substitute high parameter values for variables that we believe reflect best practice, apply low parameter values for those that do not, and use sample means for all other variables. For example, we correct for publication bias by substituting zero for the standard error on non-inverse estimates, the value of the 90th percentile for the *number of observations, publication year, mid-year of data*, and *number of citations*. We also set the value of *top journal* to 1.

In the top panel of Table 5, we present best-practice estimates from a separations-based strategy, by far the most common strategy employed in studies we examine. We obtain a relatively small estimate of 7.1. Under perfect competition, where the elasticity is infinite, the last worker hired would be paid the full amount of their marginal revenue product. Here, the point estimate implies that firms can pay the last worker hired approximately 12% less than their marginal revenue product. We obtain very similar estimates using LASSO.

When we consider results based on gender, the best-practice estimates reveal that, consistent with evidence discussed above, monopsony wage markdowns are larger for women: The estimated markdown is 10.7% for males and 14.3% for females, and 32.1% for males and 38.5% for females when focusing on only estimates of η for which the female share was not

imputed.[14] Overall, our results suggest a 4% to 9.4% monopsony-driven gender wage gap when focusing on all estimates. In Appendix G.5, we examine other treatments of gender that account for occupational effects as well as measurement error. The bottom panel of Table 5 compares best-practice estimates obtained using separation and inverse elasticities, with and without an identification strategy in place. The inverse-based estimates are larger than separations-based estimates. However, the gap between the two estimates narrows when an identification strategy is in place, suggesting that endogeneity is a significant concern for this literature. The largest upper bound estimate here, 50.09, suggests that firms have the power to mark down wages by approximately 2%.

Overall, the best-practice estimates provide strong evidence of firms possessing monopsony power. We explore further the effect of identification by repeating the exercise in Table 5 using the subset of identified estimates only and report the results in Appendix Table C7. There, point estimates are lower than those in Table 5, providing further evidence of monopsony power. However, the results are obtained using a smaller sample (576 instead of 1,254 observations), have wide confidence intervals, and are much more sensitive to the definition of best practice. We therefore prefer estimates in Table 5.

This evidence of firm monopsony power can reconcile some empirical puzzles in the labor literature. For example, in two meta-analyses, Card and Krueger (1995) and Doucouliagos and Stanley (2009) showed that increases in the minimum wage do not depress employment. This finding goes against the logic of the competitive labor market framework but can be explained under a monopsonistic framework. Manning (2003: 345–47) used a general equilibrium monopsony model to generate responses in employment to changes in minimum wages. In his example, positive or negligibly small responses are generated under elasticities of 3.3 and 5, not too far from the results we report in Table 5. Dube et al. (2018) argued that firm wage-setting power explains bunching in wages at round numbers: the lower η, the less costly the "wrong" wage is for turnover costs. For example, elasticities of 1 and 5 would be associated with firms forsaking 1% or 10% of profits due to bunching, respectively. Card, Cardoso, et al. (2018) provided micro-foundations for the static monopsony model, assuming heterogeneity in worker preferences across work environments. This approach differentiates jobs on a dimension other than wage, giving wage-setting power to firms. The authors show that, under the assumption of a supply elasticity of 4 (and markdown of 20%), this model can explain observed dispersion of wages and their link to firm productivity. Our results are broadly consistent with the elasticities needed to generate the above results.

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[14]We should note that not all explanatory variables in our baseline specification were estimable in the subsamples, so the absolute estimates of the male and female elasticities may not be directly comparable to those reported elsewhere in the article. The relative differences between men and women and their implied markdowns, however, should be informative.

Table 5. Best-Practice Estimates

Group	Point estimate	95% interval	95% interval (wild)	Implied markdown
Separations: Model				
Linear model	7.133	[1.75; 12.51]	[−0.88; 15.07]	12.3
LASSO	7.177	[2.37; 11.99]	[0.41; 13.78]	12.2
Separations: Gender				
Women	5.971	[1.09; 10.86]	[−0.90; 13.13]	14.3
Men	8.336	[1.98; 14.70]	[−1.19; 17.52]	10.7
Separations: Gender (Non-Imputed)				
Women	1.599	[−1.78; 4.97]	[−2.90; 12.32]	38.5
Men	2.118	[−1.25; 5.48]	[−2.26; 12.50]	32.1
Separations vs. Inverse				
Separations: Not identified	6.429	[1.00; 11.85]	[−1.39; 14.29]	13.5
Separations: Identified	9.910	[2.08; 17.74]	[−0.89; 19.17]	9.2
Inverse: Not identified	24.674	[19.33; 30.02]	[14.61; 31.24]	3.9
Inverse: Identified	22.810	[8.29; 37.33]	[1.58; 50.09]	4.2

Notes: The table presents fitted "best-practice" estimates using alternative models and data. Estimates in rows 1–2 are obtained using models reported in Table 3 and the post-LASSO results of Appendix Table C3. The rest of the results are obtained using the linear model. We report both the standard 95% confidence interval calculated for errors clustered at the study level, and the 95% confidence interval calculated with wild bootstrap clusters. Estimates of the markdown are obtained using Equation (2). LASSO, Least Absolute Shrinkage and Selection Operator.

Discussion and Conclusion

Imperfect competition among employers can lead to workers being paid less than their worth to the firm. Recently, academic research on such labor market structures has made its way into policy debate. At the end of the Obama administration, the US Council of Economic Advisers issued a policy brief on monopsonistic labor markets and potential policy remedies (Council of Economic Advisors 2016). A *New York Times* op-ed by Krueger and Posner (2018b) put before a wide audience three of their anti-monopsony policy recommendations: increased federal scrutiny of mergers, state-level bans of non-competes affecting low-wage workers, and a ban of no-poach agreements between franchisees under the same parent company (Krueger and Posner 2018a). In late 2017, Senators Cory Booker and Elizabeth Warren wrote an open letter to then Attorney General Jeff Sessions, urging enforcement of recent Department of Justice guidance that no-poach agreements are illegal (Booker 2017; Warren and Booker 2017). In October 2019, the US House of Representatives held a subcommittee hearing on competition in labor markets.[15] This newfound interest from policymakers calls for a detailed investigation of the existing quantitative evidence for monopsonistic labor markets.

Here, we synthesize empirical evidence on η, a parameter that captures the extent of firms' wage-setting power. We show that features pertaining to

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[15]See https://docs.house.gov/meetings/JU/JU05/20191029/110152/HHRG-116-JU05-20191029-SD001 .pdf.

methodology, data, publication quality, and researcher's implicit preference combine to explain the observed variation in estimates. We also provide quantitative predictions of what supply elasticity estimates should be for different estimation techniques, conditional on employing best research practices. Our results suggest that, overall, the literature provides strong evidence for monopsonistic competition and implies sizable markdowns in wages.

As Manning (2020) notes, "Though sizeable numbers of economists cling to the view that labor markets are close to perfectly competitive, an emerging problem is perhaps the opposite, namely that the amount of monopsony power estimated in many studies is so high as to raise questions about how it can be reconciled with observed levels of profits." Our best-practice estimates demonstrate that properly adjusting for several factors yields much more plausible results than many of the smaller elasticity estimates imply. The separations-based approach yields elasticity estimates between 6.4 (not-identified) and 9.9 (identified), and the stock-based inverse approach produces estimates above 20. These estimates produce smaller elasticities for women than for men. Best-practice estimates based on studies explicitly examining gender imply an approximate 9% monopsony-based gender wage gap, consistent with a monopsonistic explanation of a large share of the 8% unexplained gender wage gap shown by Blau and Kahn (2017) to exist in US labor markets in 2010.

Two caveats are in order. First, we do not claim to explain the systematic variation in the "true" supply elasticity parameter. Instead, our empirical exercise approximates the data-generating process for supply elasticity *estimates*, conditional on the existing literature. Some of the variation that we report is likely driven by differences in the underlying parameter value (e.g., estimates for different countries), whereas other variation may arise purely due to choices made by researchers (e.g., estimation technique or selective reporting). Second, our results provide evidence on η and the implied degree of firms' wage-setting power, but not necessarily whether the firms are able to exercise this power. Given this concern, our results regarding implied salary markdowns from separations can be viewed as a prediction of what these markdowns would be, assuming that firms fully exploit the power they have over workers. Alternatively, our results showing less wage-setting power from inverse estimates could indicate that employers are not able to exploit the full extent of the monopsony power implied by a simple wage-setting model. This scenario is consistent with labor market institutions partially reining in employers' wage-setting power.

Our analysis also suggests avenues for future research. Heterogeneity in results by method suggests that future researchers may want to follow the "thoroughly modern monopsony" approach proposed by Manning (2020). Our results on gender also illustrate how the monopsonistic framework can help understand between-group wage gaps. With the notable exception of Gerard, Lagos, Severnini, and Card (2018), empirical research in the

monopsonistic framework has focused little to no attention on racial wage gaps. This omission from the literature exists in spite of evidence from detailed administrative data showing that marginalized racial and ethnic groups often remain stuck at the bottom of an ever-widening income distribution (Akee, Jones, and Porter 2019). Hoover, Compton, and Giedeman (2015) showed how increases in the Black/White wage gap stem from institutional changes that, from a monopsonistic perspective, could be viewed as increasing labor market power. Dettling et al. (2017) documented large Black/White wealth gaps, while Stelzner and Bahn (2020) linked wealth inequality to potentially differential monopsony power over racial groups. Future work on race in the framework of monopsonistic labor markets explored here should help economists better understand structural racial inequality.

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