Labor Economics
Market Power: Monopsony and Unions

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Naidu, Nyarko and Wang (2016)   **JPE**
Monopsony Power in Migrant Labor Markets: Evidence from the United Arab Emirates

Migrant labor markets often have restrictions in competition often by tying a worker's visa to a particular employer. These restrictions have been criticized for restraining competition, lowering migrant wages, facilitating labor rights violations, and increasing monopsony power.

This paper uses a policy change in the migrant labor market in the United Arab Emirates (UAE) to estimate the wage and labor supply effects of increased labor market competition. In the UAE, a sponsorship system existed that tied a worker to one employer for the duration of their multiyear contract. At the end of the contract, if the worker wanted to stay in the UAE then they could either renew the contract with the existing employer or transfer to a new firm. However, for the transfer to occur they needed their current firm to provide a No-Objection Certificate (NOC). In January 2011, the UAE government reformed the system to allow transitions to new firms without approval from the previous employer (this could only happen at the end of the contract). This provides a good setting to study how such a reform would affect the labor market.

The authors have data on monthly payroll from January 2009 to October 2012. This covers almost 430,000 unique individuals working in over 20,000 firms. This data covers approximately 10-15 percent of the total migrant workforce in the UAE (note that UAE has an 89% migrant share of population). They can also match this to data on workers' labor contracts. This is important because it allows them to see the start and end dates of the contract. They need this because the reform affects what workers can do once their contracts expire.
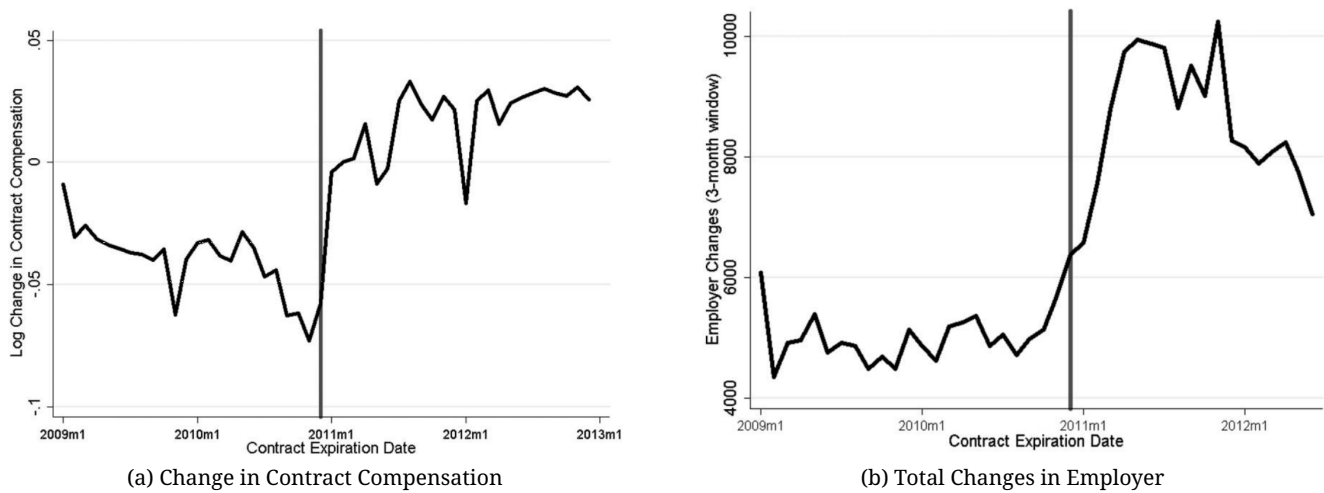
So what effect did the reform have? The initial summary statistics indicate that both compensation and mobility increased (Figure 1). In (a), we see that the change in compensation (between the new and recently expired contract) is much higher for those whose contracts ended after the reform. In (b), we see that workers whose contracts expired after the reform are much more likely to have changed employers.

To causally estimate the effect of the reform, the authors effectively use a difference-in-differences strategy. We want to compare the people whose contracts expired just after the reform (the treated group) to those whose contracts expired just before the reform (the control group). You might be concerned that after the reform induced different types of migrants to work in the country. This probably true, so the authors focus on workers whose contract expire in 2010 or 2011. These people would have initially signed their contracts in 2007-2008, which was well before the reform was even known to occur. Given this, we would not expect the two groups to be different along observable (and unobservable) dimensions.

For each person, they look at the 3 months prior to their contract's expiration, the month of expiration, and then the 3 months following that (i.e. 7 months in total). The regression they run for a person i in year-month t whose initial contract expires at time x is:

$$y_{ixt} = \sum_{k=-3}^{3} \alpha_k \left(D_{x+k} \times Pre_x \right) + \sum_{k=-3}^{3} \beta_k \left(D_{x+k} \times Post_x \right) + \delta_i + \phi_t + \varepsilon_{it}$$

Figure 1: Changes by Contract Expiration Date

| (a) Change in Contract Compensation | (b) Total Changes in Employer |

Source: Naidu, Nyarko and Wang (2016), Figures 1,3

where Pre_x is a dummy for x being before January 2011 and $Post_x$ is a dummy for it being after. D_{x+k} is an indicator for whether time t is the equal to month k in each person's 7-month window (e.g. if a contract expires in March 2011, then $D_{x+2} = 1$ when t is May 2011). The α_k's tell us the effect on the outcome variable for people whose contract expire before the reform, and β_k's tell us the effect for those whose contract expire after the reform. Therefore, the difference $\beta_k - \alpha_k$ will tell us causal effect of this policy.

The authors look at various outcomes such as earnings, exiting that UAE, and employer transitions. Figure 2 graphically shows the coefficient estimates and we can see that (as expected) earnings increase after contract expiration and workers are much less likely to leave the country after their contract expires. The results are consistent with the idea that this reform reducing monopsony power of firms and moved towards a more competitive labor market.

Figure 2: Coefficient Estimates

| (a) Impact on Log Earnings | (b) Impact on Country Exits |
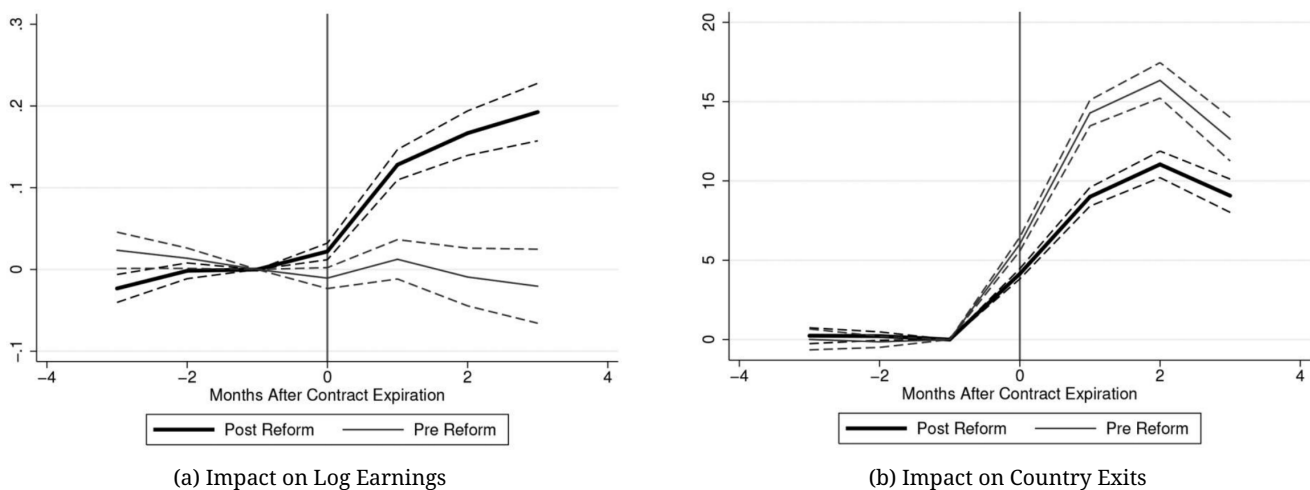
Source: Naidu, Nyarko and Wang (2016), Figures 4,6

DiNardo and Lee (2004) – Union Elections

DiNardo and Lee (2004)   **QJE**
Economic Impacts of New Unionization on Private Sector Employers: 1984-2001

This paper aims to study the effect of unions. Unions tend to be established in firms that are more profitable, so comparing firms that have unionized workers to ones that do not suffers from selection bias. One could also argue that the selection bias goes the other way: it may be easier to establish unions at poorly managed and less successful firms. So it's not clear which way the bias goes, but it's still an important question to figure out the causal effect of becoming unionized.

The ideal experiment would be to randomly assign unions to each firm. This cannot happen, but we can use a feature of the union recognition process to get close to this ideal experiment. Most new unionization occurs as a result of an election among workers. If the majority of workers vote in favor of the union, then the firm is legally obligated to bargain "in good faith" with the new union. It is likely to be the case the the union vote share (what percentage of people voted in favor of the union) is correlated with lots of different firm characteristics. However, if we focus on *close* elections (i.e. where the vote union vote share was close to 50%) that we can use a regression discontinuity (RD) design. Places were the union vote share was 51% should be fairly similar to places were the vote share was 49%. However, the ones above 50% received the union while the one below did not. This discontinuity can be used to study the effect of unionization.

The authors make an interesting point of the effect that they are measuring. This is captured in Figure 3. Imagine there are three different worlds, but hold the union vote share fixed in all cases. In the first world, unions are not allowed. So, no matter the vote share is, there will not be a union. So, no matter the vote share, the bargaining power of workers will be the same. This is represented by the line $B_0(V)$. In the second world, unions are allowed but the election has not been had yet. Now, the theoretical vote share can impact the the bargaining power - not through the establishment of a union (since the election hasn't happened) but through the *threat* of having an election. Its possible even that the threat increases with the (expected) union share, which means that bargaining power would increase with vote share. This gives us line $B_M(V)$. The third world is the real world: unions are allowed and an election is held. This now gives us two cases: the union loses ($B_N(V)$) or the union wins ($B_U(V)$). If the union loses, the bargaining power of workers would increase further (think of this as showing that the threat of an election was indeed credible and so the firm has to respond by trying to avoid another unionization attempt). Finally, if the union wins then they get an *additional* bargaining power boost from having the union itself (the "direct impact"). Using their setting, it is clear then that the RD is trying to measure this direct impact of unionization, since this is all we observe in the world (comparing those who just lose to those who just win).

The authors look at data from 1984-2001. They are able to observe union election results, as well as data on employment, wages, productivity, as well as whether the firm is active (survival).

First, one concern might be that there are not many close elections. Or, similarly, that close elections are quite weird and so studying them makes it hard to generalize. Figure 4 shows the distribution of union vote shares. We see that extreme cases are actually relatively rare, and that close elections are actually relatively common (though it is interesting to see that the most common outcome is less than 50%).

It may seem obvious, but to check that the RD is valid, we should see that union recognition only occurs in places with vote share larger than 50%. Figure 5 shows that it is indeed the case (i.e. that we have a "first stage" result). For this, they look at elections from 1984-1995. Then, they track firms and see whether they have eventually have a union recognized in another (subsequent) election between 1984-1999. This shows

Figure 3: Theoretical Impact of Bargaining Power and Vote Share
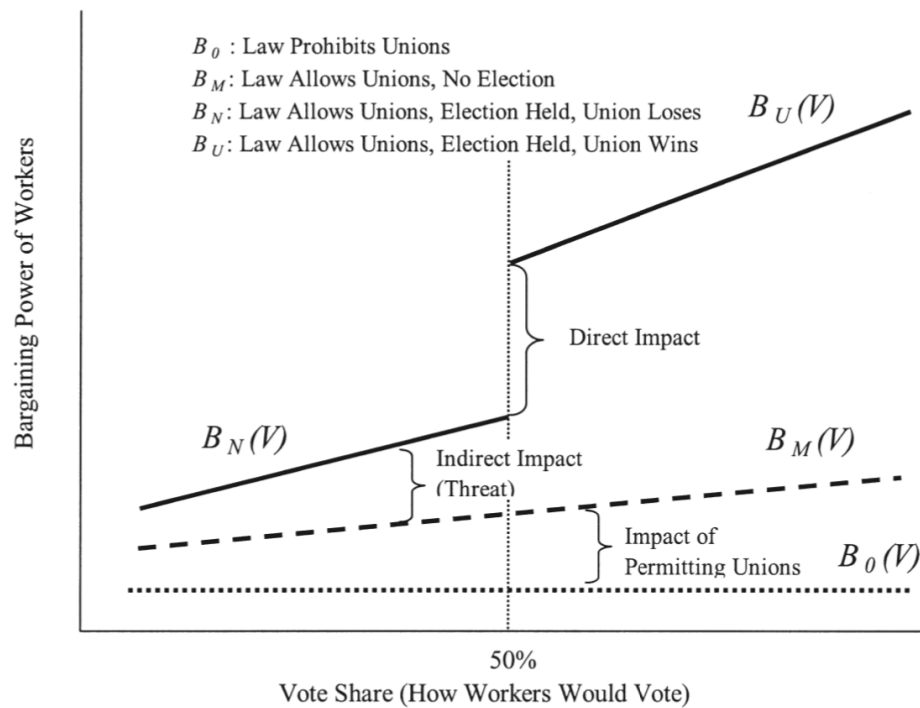
Source: *DiNardo and Lee (2004), Figure 1*

that if you lose the first election, then it's fairly likely you lose the second one too (however, note that it is more likely for places that lose close elections to eventually get a union). Additionally, this also shows us that we do not have to worry about unions eventually being decertified, as this seems to be a fairly rare event.

Another thing the authors can do is study the indirect "threat" of unionization. This is the difference $B_N(N) - B_M(V)$. We can't observe this using the RD, so they need another strategy. For this, they use an "event-study design". This just means tracking some unit (in this, a firm) over time and looking at what happens before and after a key event (in this, a union election). This sounds like one half of the story in a diff-in-diff, where we know it is important to look at the difference between treatment and control before and event a policy change. The identification assumption in an event study is that the timing of the key event is relatively random. So we can normalize the times for all the units, i.e. let $t = 0$ be the time of the key event, $t = -1$ be the period before, $t = 1$ be the period after and so on. Then we can pool them all together and look at how things change, on average, before and after. For this, they look at firms who have an election but where the union loses the election.

Figure 4: Distribution of Union Vote Shares
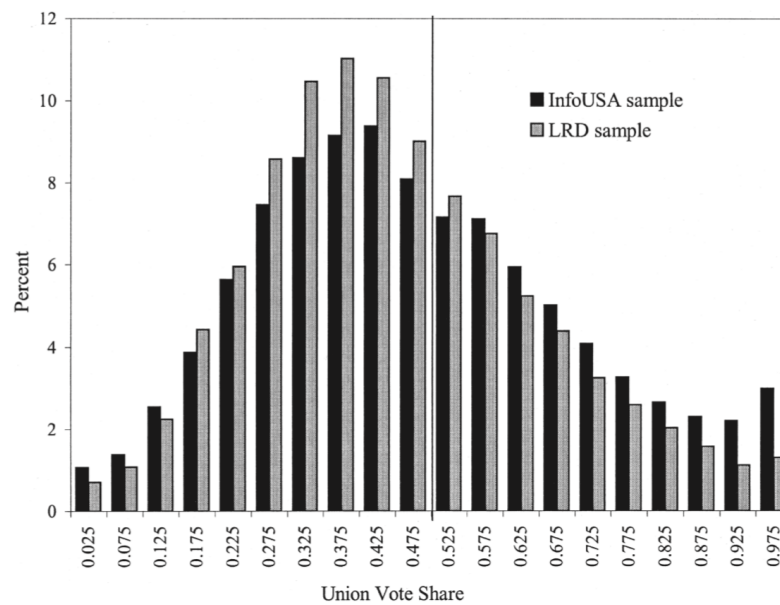
Source: *DiNardo and Lee (2004), Figure 2*

Figure 5: Union Status by Vote Share
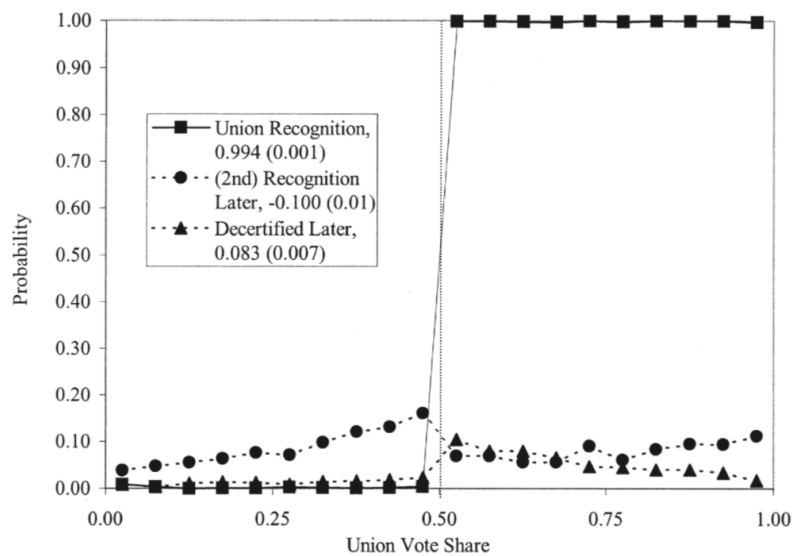
Source: *DiNardo and Lee (2004), Figure 3*

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DiNardo, John, and David S Lee. 2004. "Economic Impacts of New Unionization on Private Sector Employers: 1984-2001." *The Quarterly Journal of Economics*, 119(4): 1383–1441.

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