

Graduate Labor Economics

Notes to Accompany Lecture 8: Unions

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This lecture is different. On the surface, we'll be talking about [DiNardo and Lee \(2004\)](#)—which exploits close union elections to identify the causal effect of new union formation on business survival, employment, wages, and productivity—as well as a successor paper by [Lee and Mas \(2012\)](#) that examines stock-market responses to union elections to gauge their effects on the present value of expected profits. But rather than focusing on the substance, we will primarily use these papers as a vehicle for talking about “reading between the lines”. In particular, we'll conduct a close reading of the introduction to DiNardo and Lee, which is a great example of effective economic writing (as well as a classic paper on unions).

A good introduction accomplishes three goals. First, it states, motivates, and frames the research question. Second, it provides a self-contained summary of the analysis to come, so that the busy reader can get the gist without reading the paper in depth, and so the unhurried reader can survey the forest before inspecting the trees. Third, it draws up an implicit contract with the reader, a series of claims to be established and a preview of the evidence to be presented.

Just as the introduction is the writer's opportunity to frame and state her case, it gives the reader an opportunity to (i) situate the paper in the context of prior literature or previously known facts; (ii) digest the main ideas; and (iii) think about how she will evaluate the argument's validity and importance. I typically read a paper's introduction slowly and carefully, forming initial impressions of its strengths and weaknesses, before turning to the body of the text.

Now let's proceed through DiNardo and Lee, paragraph by paragraph.

Paragraph 1: commanding the null hypothesis.

It is widely understood that unions impose costs on employers: the most important way is by raising members' wages. They can also impose other costs on employers—by limiting discretion in hiring and firing, for example, and altering the structure of pay across skill groups. These constraints can lead employers to reduce employment, output, or most dramatically, to cease operation altogether. Indeed, these effects are often directly acknowledged by employers and employees alike. During union organizing drives, for example, firms routinely threaten to close a plant if the union drive is successful (Bronfenbrenner 1994), and employees seem to take these threats seriously: the risk of plant closure is cited as the leading cause of union withdrawal from organizing attempts.

This is a clever way to begin. DiNardo and Lee (hereafter, “DL”) are about to present a suite of zeroes: newly formed unions—at least those formed in the aftermath of close elections—do *not*

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appear to raise wages, nor do they have discernible impacts on firm survival, employment, or output. The stronger readers' priors that unions affect these outcomes, the more surprising and interesting are the results to follow. DL are thus deliberately reminding their audience of the conventional wisdom they are about to dispel.¹

This paragraph also lays a foundation for the empirical analysis by enumerating several outcomes of interest. It's no coincidence that these outcomes—wages, employment, output, and survival—are precisely those DL's data will allow them to analyze. These are not the *only* outcomes worth examining: one might also ask whether unionization lowers stock prices or affects technology adoption. DL aren't going to examine such outcomes in this paper, and they wisely omit them from the first paragraph (as including them might raise false expectations about what's coming).

Paragraph 2: a question of magnitudes.

Are the costs of unionization to employers large or small? Today, in the United States, arguments can be made for either case. On the one hand, conventional estimates suggest that there still exists a sizable union wage premium: demographically similar union workers are paid 15 percent or more than their non-union counterparts. To the extent that employers are sensitive to the price of labor, this may lead to large reductions in employment. On the other hand, there is a broad consensus that in the past three decades, union power in the United States has been on the decline. There has been a decrease in union membership and new organizing activity, high levels of managerial opposition, and increased use of permanent replacement workers. During the 1980s, prominent unions were accepting wage cuts, facing the pressures of the opening of international competition.

Three observations. First, note how simply and directly DL state their research question: *Are the costs of unionization to employers large or small?* It comes early in the paper, and it's impossible to miss. Second, by focusing on whether the costs of unionization are “large or small”, DL are implicitly ruling out the possibility that unions are beneficial to employers.² Third, DL offer compelling stories for why one might expect either a small effect or a large one. There is real uncertainty here, which helps motivate the need for credible causal estimates of the magnitude of unions' effects.

Paragraph 3: tricky challenges that the authors are poised to solve.

At least two important challenges hinder credible measurement of the causal impacts of unionization on employers. One limiting factor is the absence of large, representative data sets that track establishments over time that also provide information on union status. A second important concern is the fact that unionization is nonrandom. Depending on the correlation between factors associated with unionization and those associated with employment, output, and productivity, the observed correlation between union status and employer outcomes may overstate or understate the true effects of unions. Two competing phenomena may induce opposite selectivity biases. On the one hand, unions may tend to organize at highly successful enterprises that are more likely to survive and

¹There's an echo here of Jane Austen's *Pride and Prejudice*, which famously opens: “It is a truth universally acknowledged that a single man in possession of a good fortune must be in want of a wife”—a “truth” it soon discredits.

²This is a reasonable premise given DL's institutional setting: although unionization could conceivably increase some employers' profits (e.g., by raising productivity), the *close union elections* that identify DL's causal estimates are often fiercely resisted by employers, so that revealed preference suggests employers do not gain from them.

grow. On the other, a union organizing drive may be more likely to succeed when a firm is poorly managed, or has faced recent difficulties.

Once again, this paragraph serves multiple functions. First, it throws down a gauntlet: DL are making it clear that they're after "credible measurement of the causal impacts of unionization on employers". They're inviting readers to apply a stringent rubric: whether DL ultimately deliver "credible" estimates of "causal" effects. Second, this paragraph educates the reader about two big empirical challenges to meeting this bar: steep data requirements and a tricky selection problem. Third, again invoking the notion of an implicit contract, DL are signaling that they'll be drawing on "large, representative data" and that they'll be able to surmount the selection problem. The harder the problem, the more impressive the solution, and DL are making sure that readers appreciate the value-added offered by this paper. Fourth, DL point out that the selection bias is hard to sign ex ante, so that naïve OLS regression is unlikely to provide even an informative bound. This further amplifies the need for a well-identified approach.

Paragraph 4: the method in a nutshell.

In this paper we present quasi-experimental evidence on the causal effect of unionization on employer business failures/dislocations, employment, output, productivity, and wages, using two large databases representative of US establishments at risk of being unionized. Our analysis is based on the fact that most new unionization occurs as a result of a secret ballot election. By law, if a majority of workers vote in favor of the union, the law requires the management to bargain "in good faith" with the recognized union. This process creates a natural set of comparisons between establishments that faced elections where the union barely won (say, by one vote) and those that faced elections where the union barely lost (by one vote). As in other regression-discontinuity designs, the comparison between near winners and near losers potentially eliminates any confounding selection and omitted variable biases, and allows us to devise credible and transparent estimates of the effect of unions on employer outcomes.

Scientific writing is both expository and persuasive. Here DL describe their method simply, clearly, and accurately, but they're also selling it: "quasi-experimental" evidence, "causal" estimates, "large" and "representative" data, a "natural" identifying logic that yields "credible" and "transparent" results.

Paragraph 5: the data in a nutshell.

We report several findings from analyzing data that span the 1984–2001 period, and combine information on elections from the National Labor Relations Board (NLRB), on contract expirations from the Federal Mediation and Conciliation Service (FMCS), on subsequent business survival, employment, and output from a commercial database based on telephone listings (InfoUSA), as well as on employment, wages, output, and productivity in the manufacturing sector from the US Census Bureau's Longitudinal Research Database (LRD).

Introductions need to be lean and mean, and that means sticking to broad strokes. DL give us the briefest description of each dataset, just enough to get a general sense of what's at their disposal. Naming the data providers makes it clear they're drawing on (presumably high-quality) data from authoritative sources, and it conveys a wealth of information to any readers who've used any of these datasets before.

Paragraph 6: there’s a first-stage impact on union status.

We first document that the outcome of an NLRB election has a substantial, binding impact on the collective bargaining process, even among close elections. Where they barely win the election, unions are able to maintain their legal recognition over long time horizons; where they barely lose, there is little evidence of subsequent attempts to organize the workplace. Furthermore, unions who barely win have as good a chance of securing a collective bargaining agreement with the employer as those who win the elections by wide margins. And, as expected, unions who barely lose an election have little chance of ever signing such an agreement. These facts show that—statistically speaking—employers face a minimal risk of ever entering collective bargaining negotiations after a union loses a closely contested election.

As a rule, null results are hard to sell, in part because it’s not always clear if the treated units were “really” treated. In the present context, a possible reason why the outcome of a close union election might not matter is that close results are not actually binding, either because marginal victors promptly lose recognition or because marginal losers keep trying to unionize and ultimately gain recognition. By showing that the outcome of a NLRB election has a strong “first stage” impact on union status, DL rule out this (comparatively uninteresting) explanation, leaving us to ponder the deeper result that becoming (and staying) unionized has little impact on observed outcomes.

Paragraph 7: tight zeroes.

This legally mandated shift in the bargaining position of the workers, however, does not lead to significant impacts on a number of employer outcomes. First, union effects on business survival are small—on the order of $-.01$ to $-.02$ on a mean survival rate of $.40$ over an average of eight years. Second, point estimates of the union impacts on employment, output, and productivity, are statistically insignificant; in the manufacturing sector, they range between -3 and 3 percent for production hours, between -4 and 4 percent for output, and between -2 and 0 percent for output per worker, over one- to fifteen-year horizons.

A common mistake is for authors to discuss their results only in terms of *statistical* significance, with little or no discussion of *economic* significance. DL focus on confidence intervals, making it clear what kinds of effect sizes they can rule out. Emphasizing confidence intervals rather than point estimates makes a lot of sense when reporting a null result: a tightly estimated zero effect is informative, whereas a noisy zero is not.

Paragraph 8: a preferred interpretation of the results.

One interpretation of these results is that the true employment effects are moderately sized, but cannot be detected by our research design, due to sampling variability in our estimates. An alternative interpretation is that the effects are truly small (e.g., -2 or -3 percent). We favor the latter interpretation for the following reason: our estimates of union wage impacts are small—centered around zero—with enough precision to rule out a 2 percent wage increase for up to seven years after the election. This implies—provided that wage and employment outcomes remain on the employer’s labor demand schedule—that the impacts on employment are likely to be small, even assuming relatively large labor demand elasticities.

A paper should have a point of view. Of course, the language authors use in making interpretive claims should be carefully aligned with the evidentiary basis underlying those claims: one should acknowledge important caveats and avoid casting suggestive evidence as definitive. But one can also be *too* cautious. When the preponderance of the evidence points in a particular direction, authors who refrain from advancing their preferred interpretation deprive their readers of their own considered judgment about what’s going on. It’s much easier to read and to learn from a paper that tries to tell a story—even if the reader is ultimately not persuaded of the author’s view—than it is to read a paper that shies away from interpreting what it has found. In this paragraph, DL strike a nice balance between presenting readers with a menu of possible takeaways and advancing a particular interpretation of the facts.

Paragraph 9: no stone unturned.

We also explore whether the small wage effects are an artifact of union “threat effects”—whereby employers raise wages to avoid the threat of future unionization. We do so by complementing our regression-discontinuity analysis with an “event-study” analysis that assesses whether wages rise in response to an election, even if the union eventually loses. Point estimates are small (between 0 and 2 percent) and statistically insignificant, ruling out a 3 percent “union threat” effect, three years after the election.

Not much to say here. DL conduct a thorough analysis, and this auxiliary exercise certainly warrants mention in the introduction, but it’s not the main result and DL don’t devote a lot of real estate to it here.

Paragraph 10: brevity is the soul of wit.

Based on the evidence, we conclude that—at least in recent decades in the United States—the legal mandate that requires the employer to bargain with a certified union has had little economic impact on employers.

This is a wonderful paragraph: brief and pointed even as it slips in caveats about external validity.

Paragraph 11: text and subtext.

The small wage effects that we estimate may appear to be at odds with an enormous literature that has documented substantial union wage premiums. The differing results, however, may be explained by some important differences—other than in research design—in the nature of the data used. First, the modern union wage premium literature typically examines individual-level household survey data, rather than establishment-level data as we do here. Freeman and Kleiner (1990) argue that the latter is more appropriate for directly addressing the direct impacts of a workplace becoming unionized. Indeed, other establishment-level analyses find small or statistically undetectable wage effects (Freeman and Kleiner 1990; LaLonde, Marschke, and Troske 1996). Second, the data contain information on recent unionization (within the past twenty years), while most worker-level data sets possess little information on when the union was formed; estimates derived from those data naturally cannot isolate wage impacts that result from unionization that occurred in recent decades. As noted in Freeman and Kleiner, existing wage differences between union and nonunion workers today average the effects of unions of previous periods and the effects of unionization that occurs today.

The results in this paper are provocative and ran counter to many people’s priors, so DL take pains to reconcile their findings with the existing literature. There is diplomatic skill in this paragraph: DL offer several possible reconciliations and avoid overtly criticizing the methodologies used in previous work, but they also work in a subtly effective reminder of their clean quasi-experiment (“other than in research design”, set within em dashes that raise its visibility).

Paragraph 12: standing on the shoulders of giants.

Our results may also appear to be at odds with the standard “textbook” treatment of the neoclassical theory of union impacts, which emphasizes the notion of a union as an effective “monopoly” on labor services. There is, however, an older tradition in economics that argues—on a purely theoretical level—that most trade unions are unsuccessful monopolies. Indeed, in his essay, “The Impact of the Union,” Friedman (1950) argued that the ability of unions to raise wage rates at that time was somewhat exaggerated, because most unions could not overcome market forces that would tend to keep wages aligned with competitive rates. In a published exchange with Paul Samuelson, Friedman explains his reasoning: “I think if [UAW leader Walter] Reuther were to disregard [pressures to moderate wage demands] and if he were to seek—and for the moment let us suppose he is temporarily successful—very radically raised wages, and if that had the effect of grossly reducing employment within the automobile industry you would find opposition building up that would break the union down. Knowing that in advance and being as smart as you and I, he would avoid such action.” (Brackets in original.)

To further shore up their rhetorical position, DL find support for their contrarian view in the early literature on what unions do, appealing to a venerable authority (Milton Friedman) to rationalize their findings.

Paragraph 13: the good ol’ roadmap.

The paper is organized as follows. Section II provides some background on the union recognition process and the industrial relations climate in the United States in recent decades. Section III describes different notions of the causal impact of unionization, the regression-discontinuity design for estimating direct impacts of unionization, as well as the identification strategy for assessing indirect, “union threat” effects. We describe the various data sets in Section IV, present the results in Section V, and discuss the findings in relation to the existing literature in Section VI. Section VII concludes.

Economic papers have established conventions, one of which is that an introduction typically ends with a plain-vanilla roadmap paragraph. As you begin to write your own papers, stick to the conventional paper structure unless you have good reason to deviate. These conventions exist for a reason, and since your readers will be expecting you to follow them, going off-script is noticeable and potentially distracting.

References

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