

Labor Laws and Manufacturing Performance in India: How Priors Trump Evidence and Progress Gets Stalled

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ABSTRACT

Strong labor protections for ordinary workers are often portrayed as a ‘luxury developing countries cannot afford’. No study has been more influential in propagating this perversity trope in the context of the Indian economy than the *QJE* article of Besley and Burgess (2004). Their article provides econometric evidence that pro-worker regulation resulted in lower output, employment, investment and productivity in India’s registered manufacturing sector. This paper reviews existing critiques of Besley and Burgess (2004), which highlight conceptual and measurement errors and uncover econometric weaknesses. The paper takes a step beyond these: it reports a failure to replicate Besley and Burgess’ findings and demonstrate the non-robustness of their results. My deconstruction is not only about the econometrics, however. I show that Besley and Burgess’ findings are not just inconsistent with their theoretical priors, but also internally contradictory and empirically implausible, taxing any person’s capacity for belief. The paper, written by two ‘useful economists’, exhibits a gratuitous empiricism in which priors trump evidence. On all counts, it fails the test of being useful to the purpose of ‘evidence-based’ public policy advice.

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“One of my greatest pleasures in writing has come from the thought that perhaps my work might annoy someone of comfortably pretentious position. Then comes the saddening realization that such people rarely read.”

— John Kenneth Galbraith (1981, pp. 30-31).

I. “Laws created to help workers often hurt them”

A most effective rhetorical weapon, as Albert Hirschman (1991) argued in an essay written in response to the triumphant neo-conservatism of the 1980s, is the ‘perversity trope’—the claim that some purposive intervention to improve some feature of the political, social or economic order only serves to worsen the condition one wishes to ameliorate. Examples of this standard trope are by now well known: ‘welfare’ impoverishes people, affirmative action disenfranchises minorities, or antitrust policy just destroys competition. In development economics, the ‘perversity trope’ is perhaps most often invoked to discredit pro-worker labor laws, which are portrayed as ‘luxuries developing countries cannot afford’. The idea is that laws governing wages and working conditions or facilitating collective bargaining must raise labor costs and prices, hence bring damage to business profits and firm investment, and in so doing destroy the exact jobs they were intended to protect. “... laws created to help workers often hurt them,” is how the World Bank (2008, p. 8) sums it up. This particular ‘perversity trope’ has been invoked numerous times in the Indian debate on the impacts of labor market regulation on registered manufacturing performance that has been raging for decades (Bhattacharjea 2006; Schrank 2014; Srivastava 2016; Storm and Capaldo 2018; Karak and Basu 2019). To justify policies of labor market deregulation, successive Indian governments, of varying political colors, have claimed that India’s ‘archaic’ and ‘restrictive’ labor regulation in registered manufacturing hurt industrial performance—the recent anti-labor reforms by the BJP-led government of Narendra Modi are only the latest manifestation of what has been standard policy since at least the mid-1980s.

‘Useful’ economists (*per* John Kenneth Galbraith 1973) have often been more than willing to buttress the ‘perversity thesis’ by providing empirical evidence in its support. No economists have been more useful in propagating the claim that pro-worker labor regulation ends up hurting workers in India than LSE economists Timothy Besley and Robin Burgess. Their 2004 *Quarterly Journal of Economics* article provided seemingly robust and

unmistakable econometric evidence that “pro-worker labor regulation resulted in lower output, employment, investment, and productivity in the formal manufacturing sector. Output in the informal sector increased” (Besley and Burgess 2004, p. 92-93).¹ Ever since its publication in one of the profession’s most prestigious journals, the peer-reviewed article has been grist to the neo-conservative mill. It has had (and is still having) an outsized influence on public policymaking, morphing into the ‘origin’ paper on how well-intended public intervention can backfire. The Government of India referred to it in its *Economic Survey 2006* to justify labor market deregulation and the World Bank highlighted the study approvingly and prominently in its *World Development Report 2005*. The *de jure* measure of labor regulation developed by Besley and Burgess has been widely used in subsequent research, and to date, the article has been cited 346 times in ISI-journals and 1426 times on Google Scholar. And importantly, the paper created a cottage industry of diligent researchers trying to establish evidence of the negative developmental impacts of India’s ‘License Raj’—the system of (central) government controls regulating entry and production in registered manufacturing (e.g. Aghion *et al.* 2008)—to put the final nail in the coffin of the interventionist Nehru-Mahalanobis industrialization strategy and speed up India’s transition to ‘end-of-history’ neoliberalism.

It is true that the Besley and Burgess paper has been substantively critiqued, mostly by Indian economists (Bhattacharya 2006; D’Souza 2010; Karak and Basu 2019), who successfully challenged its methodology and findings. In this paper, I undertake a new and different critique of Besley and Burgess (2004). In doing so, I hope to radically challenge their rhetoric of reaction that one-sidedly highlights the perverse impacts on workers of pro-worker regulation, but completely ignores any of the potential positive impacts of labor regulation.² Section II summarizes the work of Besley and Burgess and section III examines

¹ The historical irony is that Beatrice and Sidney Webb, the founders of the *London School of Economics and Political Science*, where Besley and Burgess work, held the exact opposite view, arguing that “What is most urgently needed ... is an extension of the strong arm of the law on behalf of the oppressed workers in the sweated trades” (Webb and Webb 1902, p. xvii). The Webbs argued in favour of minimum wages, maximum working hours, and considerably strengthening the countervailing power of workers to end employers’ parasitism, and advocated bringing ‘democracy’ to industry through unions and collective bargaining (Kaufman 2013).

² A prominent example of such a positive impact is its ‘technology forcing’ effect, as tougher rules favour the stronger enterprises and lead to the displacement of weaker, less productive, firms (Mayneris *et al.* 2014; Huang *et al.* 2014)—also in India, such regulation is found to be positively associated with innovation, as measured by

extant critiques of their research methodology and assumptions; I report a failure to replicate Besley and Burgess' findings and demonstrate the statistical non-robustness (a.k.a. flimsiness) of the impacts of labor regulation on registered manufacturing performance. However, my own 'deconstruction' of Besley and Burgess (2004), in sections IV and V, is not about the econometrics. My point is that the findings of Besley and Burgess are not just inconsistent with their own (neoclassical) theoretical priors, but also internally contradictory and empirically implausible. On closer look, the paper is a professional embarrassment and provides no basis whatsoever for the 'perversity thesis'. Rather, what it almost perfectly illustrates is how a combination of scientific pretension and a deep desire for respectability can lead to a gratuitous empiricism in which priors trump evidence—to the detriment of socio-economic progress. Section VI concludes by locating the present exercise within the context of the larger political-economy literature on labor laws and economic development.

II. Labor laws and manufacturing performance: Besley and Burgess (2004)

“Evidence suggests,” so writes India’s Ministry of Finance (2006, p. 209) in its *Economic Survey 2006*, “that States, which have enacted more pro-worker regulations, have lost out on industrial production in general.” It is no secret that the *Survey* was referring to the econometric analysis by Besley and Burgess (2004), which exploited cross-state variations in manufacturing performance and in labor regulation during the period 1958-1992 to evaluate the impacts of regulation on output, employment, investment and productivity. To measure the degree of labor market regulation, Besley and Burgess created a new indicator of regulation, based on the state-wise variation in the direction of amendments that different states made over the period of analysis to the Industries Disputes Act (IDA) of 1947.³ They classify the amendments as ‘pro-worker’, ‘neutral’, or ‘pro-employer’, assigning scores of +1, 0 and -1, respectively. The state-wise scores are cumulated over time to obtain a ‘regulatory measure’ for each state in each year; I shall call their measure the BB-index. The index is found to be strongly positively correlated (with a one-year lag) with workdays lost to strikes

patenting activity or number of start-ups in higher-technology industries such as software and bio-pharma (Acharya *et al.* 2010).

³ The amendments mostly concern IDA Sections 5A, 5B, 9 and 11, which relate to individual/collective retrenchments and layoffs, and closures, and changes in job description (Bhattacharjea 2006). The onus of rigidity comes from Section 5B which imposes restrictions on layoffs for all factories with 100 or more employees.

and lockouts per worker; this correlation is taken to suggest that the BB-index signals poor labor relations due to ‘excessive’ union power and conflictual labor-management relations.

Besley and Burgess (2004, p. 92) do not hide their theoretical priors: their main worry is that excessive bargaining power of organized labor will blunt investment incentives and hurt the business climate, which in their view would hinder industrialization and economic development. They rather one-sidedly talk about the “vested interests” of unionized workers (on p. 113 and p. 116), without mentioning similar vested interests of employers, as did their precursors at the LSE (see Webb and Webb 1902). Workers, in their view, use stricter labor laws to extract a greater share of the surplus from existing investments (thus diminishing firms’ incentives to invest in the future), but they do not find it worthy of mention that employers extract surplus value from workers, as is shown by the declining wage share (see Table 3 below)—also in states with stricter labor laws, because labor law enforcement is often weaker in places where the letter of the law is more stringent (Chatterjee and Kanbur 2015; Srivastava 2016; Kanbur and Ronconi 2016). In an underdeveloped section titled “Theoretical Considerations”, Besley and Burgess mention the following ways through which pro-worker regulation ends up hurting manufacturing performance:

1. A first channel is the *relative price effect*: pro-labor legislation will “raise the (fixed or marginal) cost of employing laborers” (Besley & Burgess 2004, p. 101). This is then said to result in either capital-labor substitution and a consequent increase in the capital intensity of production, or it lowers the firm’s (optimal) output, because it increases the marginal cost of production.
2. A second channel is called the *expropriation effect*: pro-worker labor regulation enables workers to extract a greater share of the return on existing (sunk) investments, which discourages future investment. Investment will be lower and hence capital stocks will be lower—reducing growth of output and employment.

These explanations come straight out of Econ101, but more advanced microeconomic analyses show these can be misleading (see Roychowdhury 2014); Basu and Felkey (2008), for instance, demonstrate that higher (efficiency) wages can be associated with lower unemployment even in competitive labor markets and that, absent a minimum wage, the economy may converge to a low-wage and high-unemployment equilibrium. Besley and Burgess furthermore ignore the possibility that pro-worker regulation could promote economic development—for instance, by forcing firms to become more productive (see Storm and Capaldo 2018).

Besley and Burgess use their index as an independent variable in regressions explaining (with a one-year lag) registered manufacturing output, employment, labor productivity (defined as value added per employee) and fixed investment; the regressions include socio-economic and political control variables.⁴ They do this using two different panel datasets. The first—state-wise—panel dataset is based on aggregate manufacturing data for sixteen major Indian states during the period 1958-1992.⁵ The second dataset is an unbalanced panel dataset on registered manufacturing performance at the three-digit industries-level across the same sixteen states during 1980-1997.⁶ The industry-level regressions include industry dummies, but no time dummies. Results from this analysis will be called industry-level findings. The main findings from the state-wise and industry-level analyses are summarized in Table 1. All estimated impacts have negative signs: pro-worker regulation is consistently associated with lower output, lower employment, lower productivity and lower investment (measured as fixed capital per capita) in registered manufacturing. However, rather remarkably, pro-worker labor laws did not significantly affect earnings per employee. “This lines up with the fact that theory does not give any clear-cut predictions for wages,” argue Besley and Burgess (2004, p. 109) somewhat falteringly, adding that the “bottom line is that workers do not appear to be gaining from pro-worker amendments.”

The fact that the signs of the impacts are found to be negative in both the state-wise and the industry-level analysis convinces Besley and Burgess that their results at the aggregate level are not driven by inter-state differences in industrial structure or technical progress. However, it is clear from Table 1 that the magnitude of estimated effects is considerably smaller in the industry-level analysis as compared to the state-wise analysis. Besley and Burgess (2004, p. 118) step over the considerable differences, claiming, somewhat easily, that the magnitude of the coefficients in the two panels are similar. In line with their own reasoning, the observable differences must however be attributed to differences in industrial structure and specialization between Indian states: the impacts on performance of

⁴ Remarkably, the coefficients on most of the control variables are not statistically significant, which means the controls are not meaningful (as they do not control for anything). The political control variables do have significant coefficients, but these variables are not orthogonal to the BB-index; this suggests the regressions suffer from multi-collinearity.

⁵ Without missing observations, they therefore have $n = 35 \text{ years} \times 16 \text{ states} = 560$ observations; however, due to missing data, the true $n = 552$ observations (Besley and Burgess 2004, p. 111).

⁶ This would give a panel consisting of $n = 18 \text{ years} \times 74 \text{ industries} \times 16 \text{ states} = 21323$ observations (Besley and Burgess 2004, p. 119).

labor regulation is arguably stronger in the case of older labor-intensive manufacturing industries (concentrated in states including Maharashtra and West Bengal) as compared to more modern and more capital-intensive industries (concentrated in other states). The state-wise evidence is overstating the negative impacts of labor regulation, in this sense, which means the industry-level findings are the better ones. I use the industry-level findings to construct counterfactual scenarios in Sections IV and V.

Table 1
Labor regulation and output, employment, productivity and investment
in registered manufacturing in India

Impact of cumulative change in labor regulation on the <i>log-value</i> of:	state-wise (1958-1992)	industry-level (1980-1997)
registered manufacturing value added per capita	-0.186 (0.064)	-0.087 (0.022)
registered manufacturing employment	-0.072 (0.042)	-0.060 (0.019)
registered manufacturing value added per employee	-0.127 (0.059)	-0.026 (0.013)
fixed capital stock per capita	-0.120 (0.048)	-0.063 (0.034)
earnings per employee in registered manufacturing	nil	–

Source: Besley and Burgess (2004), Tables III, V and VII. Standard errors are given in parentheses.

The findings appear to be economically significant. To illustrate this, Besley and Burgess (2004, p. 112) highlight two extreme counterfactuals.⁷ The first case is Andhra Pradesh where, *without pro-employer reforms*, manufacturing output would have been 72% of its actual 1990 level, generating 199,000 fewer jobs. The second counterfactual concerns West Bengal where, *without pro-worker reforms*, manufacturing output would have been 24% higher than its 1990 level and employment would have been up by 180,000 extra jobs. These counterfactual findings were picked up by the World Bank and used, in its *World Development Report 2005*, to claim that “amendments to the strict employment regulation in one state (Andhra Pradesh) in the 1980s allowed 1.8 million urban poor to find jobs in manufacturing and services companies in the next decade” (World Bank 2005, p. 150). With

⁷ The counterfactuals are based on point estimates, without any confidence intervals. In setting up the counterfactuals, Besley and Burgess (2004, p. 112, *fn.* 22) ignore the influence of the full set of political and economic controls; however, they do note that including these controls reduces the magnitudes of the reported effects.

remarkable sleight of hand, the World Bank somehow manages to dress up the numbers for Andhra Pradesh by a factor of nine—adding, without proper empirical backing, that the beneficiaries of the job creation were all urban poor.⁸ Besley and Burgess are not responsible for the WDR’s bizarre interpretation of their findings, but, as far as I know, they did not publicly denounce it.

III. Extant critiques of Besley and Burgess (2004)

There is no dearth of critiques of Besley and Burgess (2004), including excellent methodological ones by Bhattacharjea (2006), D’Souza (2010) and Karak and Basu (2019). I summarize these here under two headings: (a) conceptual problems with the BB-index; and (b) problems with the econometrics and the robustness of the findings.

Conceptual problems with the BB-index. The BB-index is a *cumulative score* that is based on the pro-worker or pro-employer nature of a state-specific change in the IDA during 1958-1997. For each of the 16 Indian states, Besley and Burgess coded each pro-worker amendment as +1, each neutral amendment as a zero, and each pro-employer amendment as –1. There were 113 such amendments during the reference period, which collapse to a total of only 43 changes in the state-wise BB-indices; these infrequent 43 changes are spread across 552 ‘state-years’ (as given by the panel size). Using the index, a state can be classified as ‘pro-worker’ (if the BB-index is positive) or ‘pro-employer’ (when the index is negative). Table 2 presents the BB-index for the 16 states. Eight out of 16 states did not amend the IDA during the period of analysis; adding a spoonful of scientific pretension, these states are labeled ‘untreated control states’. In another three states (Gujarat, Madhya Pradesh and Orissa), the IDA was amended only once or twice. The legal action during 1958-1992 occurred in only five states; Andhra Pradesh (which amended the IDA twice in a pro-employer way, but then in 1987 introduced six pro-worker changes and one pro-employer amendment); Maharashtra which introduced four pro-worker amendments; Tamil Nadu which is classified as strongly pro-employer; and West Bengal, the outlier, which, until 1979, introduced only one pro-worker amendment, followed by 16 pro-worker labor law changes during the 1980s.

⁸ Spoiler alert: services companies were not included in the analysis of Besley and Burgess...

Besley and Burgess (2004, p. 98) acknowledge that their scoring system required a number of judgment calls, but they confidently report that they “found surprisingly few cases of uncertainty”. Observers beg to disagree, however, and highlight considerable problems, including a demonstrably inappropriate classification of individual amendments (based on erroneous interpretation of the legal changes and misdating of the amendment)⁹ and the coding of incommensurable changes as either +1 or –1 (Bhattacharjea 2006, pp. 10-15). What is more, the BB index exclusively focuses on the IDA and ignores other existing labor laws, the impacts of which often overwhelm those of the IDA. As a result, the IDA may not be representative (or only partly representative) of the degree of labor regulation. For instance, Bhattacharjea (2006, p. 14) argues that Uttar Pradesh was wrongly classified as ‘neutral’, while it should have been classified as ‘pro-employer’ in view of the pro-employer nature of other labor laws. For anyone with even the slightest familiarity with India, the classification of Kerala, without a large manufacturing sector and known for its social-democratic outlook, as ‘neutral’ and of pro-employer Gujarat and Maharashtra, two of India’s most industrialized states, as ‘pro-worker’ is equally bizarre (D’Souza 2010).

⁹ Bhattacharjea (2006, pp. 10-11) points out errors in the classification of amendments of 1968, 1982 and 1987 for Andhra Pradesh; of amendments of 1982 and 1983 for Madhya Pradesh; for Maharashtra (1981), Orissa (1983) and Rajasthan (1960 & 1984).

Table 2
The BB-index of labor regulation

	mean	st.dev.	cumul.	min.	max.	classification
Andhra Pradesh	-0.171	1.60	3	-2	3	pro-employer '58 -87; pro-worker '87 -92
Assam	0.000	0.00	0	0	0	neutral
Bihar	0.000	0.00	0	0	0	neutral
Gujarat	0.829	0.82	2	0	2	pro-worker
Haryana	0.000	0.00	0	0	0	neutral
Jammu & Kashmir	0.000	0.00	0	0	0	neutral
Karnataka	0.000	0.00	0	0	0	neutral
Kerala	0.000	0.00	0	0	0	neutral
Madhya Pradesh	0.286	0.45	1	0	1	pro-worker
Maharashtra	1.314	1.86	4	0	4	pro-worker
Orissa	0.571	0.92	2	0	2	pro-worker
Punjab	0.000	0.00	0	0	0	neutral
Rajasthan	-3.623	1.33	-3	-5	0	pro-employer
Tamil Nadu	-2.314	0.47	-3	-3	-2	pro-employer
Uttar Pradesh	0.000	0.00	0	0	0	neutral
West Bengal	5.800	7.35	17	0	17	workers' paradise

Source: Besley and Burgess (2019).

What is equally noteworthy is that most pro-worker amendments occurred in the 1980s—this is reflected by the fact that 30 of the 43 changes in the BB-index occur in precisely this decade. But, paradoxically, the 1980s were a period of weakening labor power and increased evasion of labor laws (Bhattacharjea 2006, p. 17; D’Souza 2010; Srivastava 2016). The weakening power of unionized workers is illustrated by the steady post-1980 decline in the wage share, as illustrated in Table 3. In 1980, the share of wages in value added in India’s registered manufacturing sector was 44%; the wage share subsequently declined to 32% in 1992-93 and to 26% in 1998-99—a staggering decline of 18 percentage points in less than twenty years (Jayadev and Narayan 2018). Wages increased much more slowly than labor productivity, as unionization rates declined, social security for ‘protected’ workers was scaled down and earnings inequality rose sharply (Srivastava 2016).

Paradoxically, the labor share in manufacturing value added declined most in states classified as ‘pro-worker’—by 19 percentage points during 1980-81/1998-99. In communist West Bengal, the most pro-worker of all Indian states, the wage share, which was 62% in 1980, declined to 60% in 1985-86 and decreased further to 51% in 1998-99—notwithstanding the fact that this state introduced 16 pro-worker IDA amendments during the 1980s. The wage

shares in the supposedly pro-worker states of Gujarat, Madhya Pradesh and Maharashtra were drastically suppressed between 1980-81 and 1985-86 (and later), and there were truly massive losses in the wage share in the ‘neutral’ states Bihar and Uttar Pradesh. Speaking of the increased bargaining power of unionized workers in the face of such unprecedented declines in the labor share in registered manufacturing appears far-fetched.

All this suggests another limitation of the *de jure* BB-index: non-compliance with the labor rules is pervasive and *de facto* enforcement is weak or absent (Chatterjee and Kanbur 2015; Srivastava 2016). Findings by Botero *et al.* (2004), which are suggestive of negative consequences of labor regulation, have been shown to disappear once the analysis controls for enforcement.¹⁰ Moreover, employers in India’s registered manufacturing massively circumvented the (old and new) labor laws by employing more and more (temporary) contract workers, who are not covered by these laws (D’Souza 2010; Jayadev and Narayan 2018). The share of contract workers in total registered manufacturing employment rose from a negligible level in the early 1970s to about 12% in the mid-1980s and almost 20% by 1998-99 (Srivastava 2016). All this suggests a secular weakening of the bargaining power of workers—which happened despite the IDA amendments.

¹⁰ Kanbur and Ronconi (2016) ran the same regressions as Botero *et al.* (2004), adding a control for ‘enforcement’. The results of Botero *et al.* turned statistically insignificant.

Table 3
The wage share in gross value added:
India's registered manufacturing sector, 1969-70 – 2013-14, percentages

	1969-70	1980-81	1985-86	1992-93	1998-99	2013-14	change between 1980-81 and 1992-93	change between 1980-81 and 1998-99
<i>Pro-employer states:</i>							-12.41	-14.52
Andhra Pradesh	40.09	48.37	42.57	32.48	28.77	33.63	-15.89	-19.60
Rajasthan	39.63	37.62	41.70	30.14	25.79	26.82	-7.48	-11.83
Tamil Nadu	43.06	41.35	39.11	28.95	28.51	35.14	-12.39	-12.84
<i>Neutral states:</i>							-10.03	-16.75
Assam	40.08	32.40	17.00	20.80	19.24	23.08	-11.60	-13.15
Bihar	46.22	51.42	40.54	40.03	31.85	18.94	-11.39	-19.57
Haryana	42.92	30.70	37.02	42.25	32.15	32.74	11.55	1.45
Jammu & Kashmir	86.32	55.67	43.14	28.23	64.54	19.58	-27.44	8.87
Karnataka	35.77	42.02	42.11	31.96	27.41	30.59	-10.05	-14.61
Kerala	32.42	38.23	36.95	35.72	24.65	35.47	-2.50	-13.58
Punjab	39.08	32.35	37.36	31.11	22.74	34.55	-1.24	-9.56
Uttar Pradesh	36.79	53.90	49.60	30.35	25.62	28.56	-23.55	-28.29
<i>Pro-worker states:</i>							-13.67	-19.19
Gujarat	43.48	38.67	34.09	23.50	17.50	17.83	-15.16	-21.17
Madhya Pradesh	46.66	35.44	30.38	27.47	22.59	25.86	-7.97	-12.85
Maharashtra	42.86	41.65	37.36	31.00	26.41	23.36	-10.65	-15.24
Orissa	35.27	46.09	53.48	29.50	26.60	20.09	-16.59	-19.49
West Bengal	60.43	62.39	59.87	53.43	50.81	38.55	-8.96	-11.58
<i>All-India</i>	44.44	44.11	40.17	31.73	26.53	26.61	-12.38	-17.58

Source: Annual Survey of Industries, various issues.

Econometric problems. The regressions by Besley and Burgess have been criticized for omitted-variables bias. True, Besley and Burgess control for a state's development expenditure, installed electricity generation per capita, population and the political color of the ruling government and they do include state and year fixed effects. The coefficients on most of the control variables are not statistically significant, which means the controls are not really meaningful. But Besley and Burgess ignore the influence of relevant variables including (i) the allocation of industrial licenses by the central government of India, which was a major determinant of industrial location across states that was independent of state-level IDA amendments (Bhattacharjea 2006); (ii) the growth (or stagnation) of agriculture, which in turn is a determinant of the growth of the 'home market'; and (iii) other existing labor regulations, the importance of which often overwhelms that of the IDA. Indeed, a recent econometric analysis by Karak and Basu (2019) shows that including additional variables may cause the results to change in qualitative terms. Karak and Basu add a 'manufacturing profitability' variable (a fourth omitted variable) to the regression model and then find that the coefficient on the BB-index is insignificant (using a panel dataset for 1969-2005).

Besley and Burgess (2004, p. 108) report that the inclusion of state-specific time trends in their regressions washes out the negative impact of labor regulation: the coefficient on their index turns statistically insignificant. They interpret this as evidence that their measure of labor regulation is a driver of these state-specific time trends—which smacks of confirmation bias. The industrial stagnation in West Bengal during the years 1965-1980 was closely related to agrarian stagnation in the state and to crippling power cuts, while the manufacturing sectors of 'pro-worker' Gujarat, Maharashtra and West Bengal suffered from the collapse of profitability of the old textile mills—all drivers not directly or at all related to pro-worker IDA reforms.¹¹ A better way to interpret Besley and Burgess' finding that the coefficient on labor regulation is not significant when state-specific time trends are included in the regression is to conclude that any impact of labor laws on manufacturing is overwhelmed by other drivers. For some perspective, Karak and Basu (2019) find that the coefficient on profitability is statistically significant in regressions including state-specific time trends. State-wise divergence in manufacturing performance is driven by differences in profitability, and possibly demand—not by variations in labor law and industrial relations.

¹¹ The decline of the textile industry in Maharashtra was overwhelmingly driven by the 'exit' of capital from lower-profit sunset industries to higher-profit activities, including real estate activities using mill land in the centre of the state's metropolitan cities. I am grateful to Professor C.P. Chandrasekhar for pointing this out.

Besley and Burgess derive particularly strong policy claims from their empirical evidence. They forget, however, to address the critical issue: to what extent their hypothesis testing is adequately ‘powered’ (Ioannidis *et al.* 2017). Low statistical power increases the risk of making a Type II error (*i.e.* failing to reject a false null hypothesis). Adequate statistical power thus means that the probability of *not* making a Type II error is (sufficiently) high.¹² An assessment of the power of Besley and Burgess’ findings (using the estimated coefficients and standard errors in Table 1) reveals that the state-wise findings for employment, labor productivity and fixed capital lack adequate power and that the same is true for the industry-level coefficients on productivity and fixed capital. Five out of eight statistically significant results (at the conventional 5% level of statistical significance) are underpowered—which indicates a failure to reject a false null hypothesis with power $\geq 80\%$. These five impacts are not credible and empirically defensible, as they are “quite likely to be artifacts from chance and bias” (Ioannidis *et al.* 2017, p. F240).¹³

I tried to replicate Besley and Burgess’ findings for registered manufacturing output reported in their Table III, using the panel data made available online by the *Economic Organisation and the Public Policy Programme* (EOPP) of the LSE (Besley and Burgess 2019). Curiously, the on-line dataset did not allow replication, a first reason being that the number of observations was much lower (at 382) than the number of observations reported by Besley and Burgess (namely: 508); the on-line dataset starts in 1960 (not in 1958) and has a large number of missing observations for most states during 1989-1992. Hence, I created an alternative panel dataset using data from Besley and Burgess (2019) in combination with data by Karak and Basu (2019). The unbalanced dataset for the period 1960-1992 I used in the regressions reported in Table 4 includes data for 15 Indian states (all states listed in Table 2, except Assam); due to missing observations and due to the fact that I use the one-year lagged BB-index, the number of observations is 432. The data on the BB-index and state-wise

¹² Besley and Burgess do not explicitly state their null hypothesis H_0 . The title of their article and the manner in which they frame their approach suggests that their H_0 is that the impact of pro-worker regulation on manufacturing performance is negative. The type II error then is a failure to reject a false H_0 . If one adopts the conventional 5% level of statistical significance ($\alpha = .05$) and 80% power level ($1 - \beta = .8$), then for an impact to have adequate statistical power ($\geq 80\%$), its standard error needs to be smaller than the absolute value of the coefficient divided by 2.8 (see Ioannidis *et al.* 2017 for details).

¹³ A survey of 159 meta-analyses that draws upon more than 6,700 empirical studies, concludes that the median statistical power of the 64,076 estimates of economic parameters is 10.5%; this means that half of the studies have approximately 10% or fewer of their estimates with adequate power (Ioannidis *et al.* 2017).

population are from Besley and Burgess (2019). *Annual Survey of Industries* (ASI) data on real manufacturing value added during 1969-2005 are from Karak and Basu (2019); I added ASI data on real manufacturing value added for the years 1960-1968. Table 4 presents the replicated baseline regressions of Besley and Burgess in which (the log-value of) registered manufacturing output is the dependent variable and the one-year lagged labor regulation index is the main independent variable. All regressions include state-specific fixed effects and year fixed effects as well as the log-value of state population (as a control).

In the first column of Table 4, the coefficient on the labor regulation index is -0.051 and it is statistically significant at 0.1 percent. The coefficient is substantially smaller than the coefficient of -0.184 reported by Besley and Burgess (2004), but it is similar to the result of Karak and Basu (2019), who find a coefficient of -0.048 . The coefficient is adequately powered (power $\geq 80\%$). Column (2) addresses the sensitivity of the results to the inclusion of West Bengal. Unlike Besley and Burgess (whose regression includes additional controls), I find that excluding the observations for West Bengal from the regression knocks out the (negative) impact of labor regulation—the coefficient on labor regulation is no longer statistically significant. In this context it is noteworthy that Besley and Burgess do not present an industry-level regression while excluding the observations for West Bengal. In column (3), I add state-specific time trends. This changes the sign of the statistically significant coefficient on labor regulation from negative to positive, a result counter to the finding of Besley and Burgess (2004), as it suggests that the impact of pro-worker labor regulation on registered manufacturing performance is positive, when controlling for state-specific time trends. The coefficient is also adequately powered (power $\geq 80\%$). In column (4), the period of analysis is restricted to the years 1981-1992 (which is when 30 of the 43 IDA amendments occurred); the coefficient on the BB-index is not statistically significant. In column (5), the regression is run for the longer period 1981-2005 and again it is found that, post-1980, labor regulation did not have a statistically significant effect on manufacturing output.

What the replication analysis of Table 4 shows is that the results of Besley and Burgess (2004) are not statistically robust and often not adequately powered, but flimsy and time-specific, and (with my data) hinging on the observations pertaining to the outlier state: West Bengal. It appears that only particular model specifications can yield results similar to those of Besley and Burgess. This is precisely the sort of thing that leads many experts to the view that statistical analyses can be either greatly discounted or completely ignored (Leamer and Leonard 1983). If only this had also happened to the Besley and Burgess' findings.

Table 4
Effect of labor regulation on India's manufacturing output, 1960-1992

<i>Dependent variable:</i> <i>log net value added in registered manufacturing</i> (constant prices)	(1)	(2)	(3)	(4)	(5)
labor regulation (one-year lagged BB-index)	-0.051*** (0.000)	-0.009 (0.686)	0.013** (0.010)	-0.159 (0.521)	-0.022 (0.510)
<i>log population</i>	0.024 (0.266)	0.025 (0.215)	0.062*** (0.001)	1.059 (0.233)	0.427 (0.840)
state fixed effects	Y	Y	Y	Y	Y
year fixed effects	Y	Y	Y	Y	Y
state-specific time trends			Y		
<i>no. of observations</i>	432	402	432	180 (1981-1992)	240 (1981-2005)
<i>no. of states</i> (WB = West Bengal)	15	14 excl. WB	15	15	15
R^2	0.48	0.51	0.06	0.07	0.24

Notes: The results in column (1) are a replication of the findings by Besley and Burgess (2004), column (6), Table III and columns (1) and (2) of Table IV. The signs and statistical significance of the findings are not sensitive to the inclusion of any of the 15 states with the exception of West Bengal (which passed the largest number of pro-worker amendments). Hence, in regression (2), the observations for West Bengal were excluded from the panel regression. All regressions have been estimated with the fixed-effect estimator (following Karak and Basu 2019). *p*-values, based on standard errors clustered by state, appear in parentheses below coefficient estimates. Significance levels are: *** < 0.001; ** < 0.01; * < 0.05.

Source: Author's own estimations based on panel data for 15 Indian states. The data sources are given in the text.

IV. The curse of Econ101¹⁴

The conceptual weaknesses of their approach and the observed non-robustness of the econometric findings of Besley and Burgess offer serious cause for concern. But there is one further—more pressing—reason to discount the analysis and findings of Besley and Burgess: analytically their results just do not make sense. To see this, consider the first theoretical channel through which pro-worker labor regulation is supposed to have reduced both output and employment in India’s registered manufacturing sector. Besley and Burgess label this the ‘relative price effect’ and argue that it works in two textbook-like ways.

Capital-labor substitution. Pro-labor legislation raises the cost of employing laborers, so argue Besley and Burgess (2004, p. 101), and this must result in capital-labor substitution, a consequent increase in capital intensity and a loss of jobs. This is illustrated in panel (A) of Figure 1. The problem is that the econometric findings of Besley and Burgess show something different: capital intensity in India’s registered manufacturing sector did *not increase*, but *decreased* in response to the introduction of pro-worker labor laws. To see this, let me define the log-value of capital intensity (κ) as the difference between the log-value of fixed capital (k) and the log-value of employment (e) in registered manufacturing:

$$(1) \quad \log \kappa = \log k - \log e$$

Differentiating (1) with respect to the BB-index (henceforth denoted by θ), I get using the industry-level findings reported in Table 1:

$$(2) \quad \frac{d \log \kappa}{d\theta} = \frac{d \log k}{d\theta} - \frac{d \log e}{d\theta} = -0.063 + 0.060 = -0.003$$

This *log-difference* implies that the introduction of one (single) pro-worker amendment of the IDA *reduces* capital intensity by 0.7%. This percentage change in capital intensity is calculated in the following manner. We know from (2) that a unitary increase in the BB-index reduces the log-value of κ by -0.003 . Let me define κ_B = capital intensity before the introduction of the pro-worker amendment, and κ_A = capital intensity after the introduction of the amendment. We then have: $\log(\kappa_A) - \log(\kappa_B) = \log(\kappa_A / \kappa_B) = -0.003$; this then means that $(\kappa_A / \kappa_B) = 10^{-0.003} = 0.993$, or capital intensity after the pro-worker reform is 99.3% of

¹⁴ The title is due to Kwak (2017) who argues that using introductory microeconomics to explain real-life phenomena is often more misleading than helpful.

what it was *ex ante*. Accordingly, one pro-worker amendment of IDA is associated with a reduction in κ of 0.7 percentage points. For West Bengal, which has a mean number of IDA-amendments of 5.8 during 1958-1992, this implies capital intensity gets reduced by 4%. Capital intensity in registered manufacturing is, therefore, not higher, but *lower*, in states which introduced pro-worker labor laws.

The corollary of this decline in capital intensity was, of course, a rise in labor intensity. Let me define the log-value of labor intensity (ℓ) as the difference between the log-value of employment (e) and the log-value of output (x):

$$(3) \quad \log \ell = \log e - \log x$$

Differentiating (3) with respect to labor regulation (θ) gives the following result:

$$(4) \quad \frac{d \log \ell}{d\theta} = \frac{d \log e}{d\theta} - \frac{d \log x}{d\theta} = -0.060 + 0.087 = +0.027$$

This log-difference indicates that the introduction of one (single) pro-worker labor law amendment increased labor intensity of manufacturing by more than 6%. For West Bengal, this in turn implies an increase in labor intensity of 37% in its registered manufacturing. This is quite a counterintuitive result. Why would registered manufacturing firms choose to hire *more* workers per unit of output in response to the introduction of pro-worker rules? Clearly, this is a finding in need of an explanation.

We learn in Econ101 that this could occur only in response to a decline in the real wage. Let me labor this point. If we assume that registered industrial firms are maximizing profits and their production processes can be described by a textbook constant-returns-to-scale CES production function, then the log-value of labor intensity (ℓ) can be expressed as a function of the log-value of the real wage (w) and the elasticity of capital-labor substitution (denoted by σ):

$$(5) \quad \log \ell = \text{constant} - \sigma \log w$$

A decline in the real wage leads to the substitution of labor for capital and a higher labor intensity. But amendments in labor law (θ) may affect both ℓ and w and, hence, I write:

$$(6) \quad \frac{d \log \ell}{d\theta} = -\sigma \frac{d \log w}{d\theta}$$

What equation (6) says is that if stronger labor regulation raises the real wage, this will lead to a reduction (not an increase) in labor intensity. Besley and Burgess find no statistically

significant impact of labor regulation on real earnings per employee; hence $\frac{d \log w}{d \vartheta} = 0$ and labor intensity in registered manufacturing should have stayed unchanged. Besley and Burgess, however, find that India's registered manufacturing sector became *more* labor intensive following the introduction of pro-labor regulation. Let me for the sake of illustration assume as that $\sigma = 0.75$, which is in line with econometric evidence for Indian registered manufacturing firms.¹⁵ It then follows from eq. (4) that the real wage should have *declined* following a pro-worker legal amendment:

$$(7) \quad \frac{d \log w}{d \vartheta} = \left(\frac{1}{\sigma}\right) \times -\frac{d \log \ell}{d \vartheta} = 1 \frac{1}{3} \times (-0.027) = -0.036$$

This translates into a real wage decline of 8%, which plainly is absurd and it is also not what Besley and Burgess find (see Table 1). The real wage did not change and hence there was no relative-price induced capital-labor substitution. The 'relative price effect' is irrelevant.

Increase in the marginal cost of production. "Regulation also lowers the firm's optimal output level since it raises the marginal cost of production," argue Besley and Burgess (2004, p. 102). This claim, which similarly comes straight out of Econ101, is illustrated in panel (B) of Figure 1, using the case of West Bengal as the example. The market supply curve is determined by the (upward-sloping) marginal cost curve of firms. The market demand curve is a (downward-sloping) curve. Profit-maximizing firms will produce up to the point where marginal cost equals marginal revenue (which equals price). Pro-worker regulation is associated higher unit labor cost and hence higher marginal costs; this is illustrated by the curve labeled 'actual marginal cost'. Without the pro-worker regulation, unit labor cost and marginal cost would have been lower—as is indicated by the curve labeled 'counterfactual marginal cost curve'. Lower marginal cost would have allowed for a lower price, higher demand and higher output, as the figure illustrates. Besley and Burgess claim that this reflects what happened in West Bengal.

To assess the validity of their claim, we have first to define a few concepts, including marginal cost (*mc*) and nominal unit labor cost (*ulc*). Let me, however, start by defining price (*p*) as being based on marginal cost (*mc*):

$$(8) \quad p = \pi \times mc$$

¹⁵ See Goldar, Pradhan and Sharma (2014). As a 'stylized fact', $0 < \sigma < 1$.

where π = a profit mark-up (which is assumed constant). Marginal cost, in turn, follows from total cost (tc), which is defined as the sum of fixed cost (fc) and variable cost; the latter consist of wages paid to workers ($W \times \ell \times x$) and other variable (intermediate input) cost. Hence we have:

$$(9) \quad tc = fc + W \ell x + p_M \alpha_M x$$

where W = the nominal wage, p_M = the price of intermediate inputs and α_M = the quantity of intermediate inputs per unit of output (which I assume constant). Marginal cost then is:

$$(10) \quad mc = \frac{d tc}{d x} = w \ell + p_M \alpha_M = \frac{w}{\lambda} + p_M \alpha_M = ulc + p_M \alpha_M$$

where $\lambda = (1 / \ell) =$ labor productivity, and $ulc = \frac{W}{\lambda}$. Combining equations (8) and (10), I obtain the following price function:

$$(11) \quad p = \pi \times (ulc + p_M \alpha_M)$$

Expressed in percentage-changes, eq. (11) becomes:

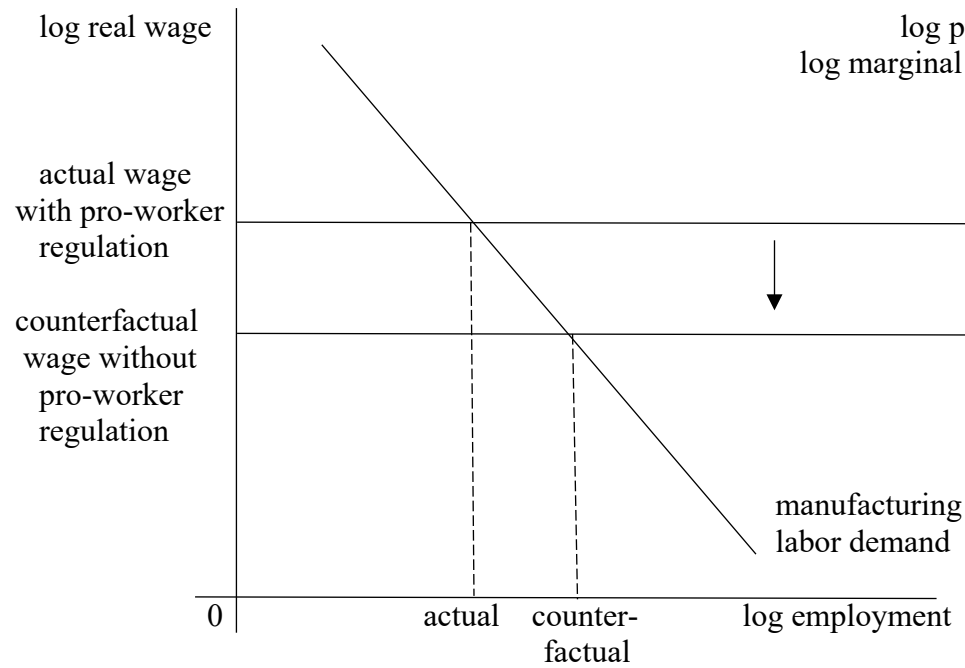
$$(12) \quad \hat{p} = \chi \widehat{ulc} + (1 - \chi) \widehat{p}_M, \quad \text{where } \chi = \frac{ulc}{(ulc + p_M \alpha_M)}$$

Increases in unit labor cost raise marginal cost (as in eq. (10)) and hence do increase the price (if π is constant). But we can also see that the impact of higher unit labor cost on the price depends on its share in the price (χ). To assess the empirical validity of the argument made by Besley and Burgess, I have to rewrite eq. (12) in terms of the growth of *real* unit labor cost, which are defined as $rulc = \frac{w/p}{\lambda} = \frac{w}{\lambda}$, or in growth rates: $\widehat{rulc} = \widehat{w} - \widehat{\lambda}$, where \widehat{w} = the growth of the real wage. Using this definition of the growth of real unit labor cost and subtracting $\chi \hat{p}$ from both sides of eq. (12), I get after rearranging:

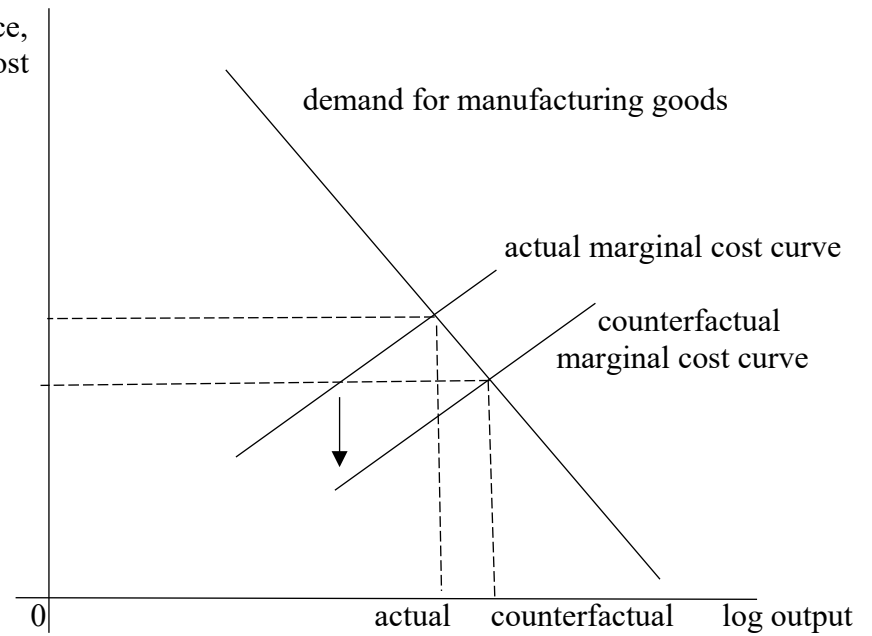
$$(12\#) \quad \hat{p} = \left[\frac{\chi}{1-\chi} \right] \widehat{rulc} + \widehat{p}_M$$

Figure 1

Pro-worker labor regulation and manufacturing output: theoretical considerations for the case of West Bengal



Panel (A): capital-labor substitution



Panel (B): the 'marginal-cost' channel

We can use eq. (12#) to assess the empirical validity of the argument made by Besley and Burgess and focus on the observed impacts of pro-worker reform on *rulc*. From the definition of $rulc = \frac{w}{\lambda}$, it follows that

$$(13) \quad \log rulc = \log w - \log \lambda$$

The introduction of pro-worker regulation (θ) may affect both w and λ and, hence, we get:

$$(14) \quad \frac{d \log rulc}{d\theta} = \frac{d \log w}{d\theta} - \frac{d \log \lambda}{d\theta}$$

Now, according to the findings of Besley and Burgess, earnings per employee did not change due to the introduction of additional labor regulation, *i.e.* $\frac{d \log w}{d\theta} = 0$. But labor productivity (in log-value) did decline, *i.e.* $\frac{d \log \lambda}{d\theta} = -0.026$ (see Table 1); this implies that one pro-worker IDA amendment depressed labor productivity by 5.8 percentage points. Taken together, one pro-worker amendment of the IDA is found to raise *rulc* in India's registered manufacturing sector by 5.8%; for West Bengal (with a mean BB-index of 5.8), *rulc* ought to have increased by about 34%.

But from eq. (12#) we know that unit labor cost is just a fraction $[\chi/(1-\chi)]$ of total cost and price. According to data from the *Annual Surveys of Industries* χ takes a value of 0.13 for registered manufacturing in West Bengal.¹⁶ It follows that a large real unit-labor-cost increase of 34% could have raised the price of manufacturing goods by just 5.2%. This—fairly limited—increase in the manufacturing price cannot plausibly explain the decline in demand for and output of registered manufacturing in West Bengal by 24%, as Besley and Burgess claim. To explain this decline in manufacturing output, one would have to assume a price elasticity of demand for manufacturing goods of -4.5 . But estimates for India (1960-61 to 1980-81) suggest that the price elasticity of demand for manufactured goods takes a value of around -0.33 (Gupta 1988)¹⁷, which would mean that a rise in the manufacturing price of 5.2% could have depressed demand and output only by a mere -1.7% (or one fourteenth of the impact found by Besley and Burgess). The conclusion must be that it is impossible to empirically attribute the non-trivial decline in manufacturing demand and output in pro-worker West Bengal to a cost-push effect of pro-worker reforms.

¹⁶ In West Bengal, *ulc* is 0.12 in 1990-91 (based on *ASI* data). The share of intermediates $\alpha_M p_M = 0.78$ in the same year (based on *ASI* data). This means $\chi = 0.13$.

¹⁷ According to Gupta's estimations, the price elasticities of demand for intermediate goods and consumer goods are lower (in absolute terms), while the same elasticity is higher (in absolute terms) for capital goods. This makes good empirical sense.

Table 5
Unit labor cost in India's registered manufacturing sector

	1969-1970	1985-1986	1998-1999
Andhra Pradesh	0.083	0.091	0.058
Assam	0.096	0.049	0.041
Bihar	0.138	0.102	0.060
Gujarat	0.111	0.067	0.036
Haryana	0.102	0.075	0.062
Jammu & Kashmir	0.232	0.126	0.083
Karnataka	0.129	0.120	0.067
Kerala	0.113	0.093	0.065
Madhya Pradesh	0.116	0.089	0.045
Maharashtra	0.093	0.093	0.059
Orissa	0.141	0.117	0.070
Punjab	0.085	0.067	0.043
Rajasthan	0.109	0.092	0.049
Tamil Nadu	0.117	0.086	0.062
Uttar Pradesh	0.095	0.099	0.058
West Bengal	0.165	0.138	0.114
<i>All-India (average)</i>	0.123	0.092	0.057
standard deviation	0.037	0.023	0.019

Source: Author's estimation based on data from the *Annual Survey of Industries*. *Note:* unit labor cost is defined as 'total emoluments' to 'gross value of output'.

The expropriation effect. The third explanation offered by Besley and Burgess is that pro-worker reforms discourage manufacturing investment, because better protected workers will extract a greater share of the return on these investments. This explanation is also not plausible in light of the econometric findings by Besley and Burgess themselves. For a start, it is found that following the introduction of pro-worker regulation, workers in registered manufacturing saw no increase in earnings—"the bottom line is that workers do not appear to be gaining from pro-worker amendments", conclude Besley and Burgess (2004, p. 109). As Table 3 made clear, the share of wages in registered manufacturing value added declined during the 1980s (and after), despite the introduction of pro-worker labor regulation. Supposedly 'better protected' workers could not and did not extract a greater share of the returns on firms' investment—and thus the expropriation effect did not happen. But there are four additional reasons why the findings of Besley and Burgess disqualify the expropriation effect as an *explanans*.

First, why would firms increase the labor intensity of production, by as much as 37% in the case of West Bengal, if there was a credible threat that workers, made more powerful by pro-worker reforms, would expropriate them in future? This just does not make sense. Second, the expropriation argument fails to acknowledge the considerable difference between the letter of the law and the actual practice. How can workers extract a greater share of the profits when the *de facto* enforcement of the labor laws is weak or even zero (Chatterjee and Kanbur 2015)—or where firms circumvent the law by hiring contract workers (Srivastava 2016)? Third, a closer look at the econometric results suggests that the obtained impacts are empirically implausible. Specifically, Besley and Burgess find that the introduction of one single pro-worker amendment reduces the log-value of fixed capital by 0.063, or as in eq. (2): $\frac{d \log k}{d\theta} = -0.063$. This translates into a non-trivial, and implausibly large, decline in fixed capital of 13½% in response to one pro-worker amendment of the IDA. What it means is that without pro-worker reforms fixed capital in registered manufacturing in West Bengal would have been 90% higher than its actual 1990 level—an outcome that beggars belief.

Finally, Besley and Burgess (2004, p. 112) argue that the negative impact of pro-worker labor regulation on value added per employee (see Table 1) is proof of the expropriation effect, “whereby blunting investment incentives leads to labor regulations being associated with lower productivity ...” Their idea is that pro-worker reforms blunt investment, thereby lowering capital intensity, and thus depressing labor productivity. Their estimates suggest that one pro-worker amendment of IDA did reduce labor productivity by as much as 5.8%. This does not square with their other findings, however.¹⁸ To see this, consider a constant-returns-to-scale CES production function framework, within which labor productivity can be defined as a function of capital intensity:

$$(15) \quad \log \lambda = \text{constant} + \beta \log \kappa$$

This gives:

$$(16) \quad \frac{d \log \lambda}{d\theta} = \beta \frac{d \log \kappa}{d\theta}$$

where β = the share of capital in value added; β takes a value of 0.6 for Indian registered manufacturing (see Table 3). We know from eq. (2) that capital intensity declined in response

¹⁸ Nor with their own reasoning. Their first mechanism—capital-labor substitution—requires capital intensity to go up, whereas capital intensity will go down following the logic of the ‘hold-up’ argument. One cannot take a swim and not get wet, however.

to the introduction of pro-worker regulation, or $\frac{d \log \kappa}{d \theta} = -0.003$. Taken together, this does suggest that one pro-worker labor law amendment did depress labor productivity by $0.6 \times -0.003 = -0.0018$, or by just 0.4%. Clearly, the ‘blunting of investment’ by stricter labor regulation can explain only a tiny fraction of the reduction in labor productivity—leaving more than nine-tenths of the decline (of 5.8%) unexplained. It looks like Besley and Burgess are making an issue when there is no issue. The expropriation story holds no water.

V. The findings of Besley and Burgess are also not economically meaningful

“The results leave little doubt,” state Besley and Burgess (2004, p. 124), “that regulation of labor disputes in India has had quantitatively significant effects.” They illustrate the policy importance of their econometric findings, writing the following:

“The coefficients from the basic specifications in Tables IV and V imply that without their pro-employer reforms, Andhra Pradesh would have registered manufacturing output which was 72 percent of its actual 1990 level and manufacturing employment that was 73 percent of its 1990 level. If West Bengal had not passed any pro-worker amendments, it would have enjoyed a registered manufacturing output that was 24 percent higher than its 1990 level and employment that was 23 percent higher.” (Besley and Burgess 2004, p. 112).

Persuaded by the consistency as well as the economic significance of their findings, Besley and Burgess (2004, p. 124) conclude that it is “apparent that much of reasoning behind labor regulation was wrong-headed and led to outcomes that were antithetical to their original objectives” and that “attempts to redress the balance of power between capital and labor can end up hurting the poor.” The two LSE economists get carried away by the unambiguity of their findings, writing that the “battle cry of labor market regulation is often that pro-worker labor market policies redress the unfavourable balance of power between capital and labor, leading to a progressive effect on income distribution. We find no evidence of this here—indeed the distributional effects appear to have worked against the poor.” This is the rhetoric of reaction in full swing.

I tried to understand and reconstruct the counterfactual analysis for Andhra Pradesh and West Bengal. It proved less than straightforward to reproduce these counterfactuals. The only way I managed to obtain the above-mentioned impacts of labor regulation on registered manufacturing output in Andhra Pradesh and West Bengal was when using (i) the mean value of the *non-cumulative* BB-index for each state; and (ii) the coefficient (of -0.073) of the impact on *total* (*i.e.* registered and unregistered) manufacturing output per capita of a unitary

change in the BB-index (taken from column (5), Table III, Besley and Burgess 2004). This raises two thorny issues. First, because the coefficients were estimated using the *cumulative* BB-index, one should properly use the mean value of this cumulative index—rather than the mean of the non-cumulative index; note that the non-cumulative BB-index has a mean value of -1.86 for Andhra Pradesh and $+1.23$ for West Bengal, while the corresponding means for the cumulative BB-index are -0.171 and $+5.8$. Second, because the counterfactuals concern *registered* manufacturing output, one must use the estimated coefficient for *registered* manufacturing output, which takes a value of -0.184 , given in column (3), Table IV. Using this coefficient value and the means of the *cumulative* BB-index, I find that, without pro-employer reforms, output of registered manufacturing in Andhra Pradesh would have been 93 percent of its actual 1990 level—an effect vastly smaller than the one reported by Besley and Burgess. But the real story is that of West Bengal where, without pro-worker reforms, registered manufacturing output would have been a whopping 1000 percent higher than its actual 1990 level. Bengal could have been the next Asian Tiger bar for those damned pro-worker amendments of IDA which didn't merely blunt manufacturing investment, but actually asphyxiated the industry.

The finding for West Bengal is not a freak outcome, but illustrates the general meaninglessness of the findings obtained by Besley and Burgess. To see this, I have used the industry-level coefficients (summarized in Table 1) to construct counterfactuals for manufacturing output, employment, labor productivity and fixed capital stocks for the three pro-employer states and the five pro-worker states—the results appear in Table 6. In setting up the counterfactuals, I follow Besley and Burgess (2004, p. 112, *fn.* 22) and ignore the impacts of the political and economic controls. It can be seen that Maharashtra's registered manufacturing output would have been 30% higher than what it was in 1990 if this state had refrained from introducing 1.3 pro-worker amendments; manufacturing output in Rajasthan would have been less than half of its 1990 level without the 3.6 pro-employer amendments of IDA. The counterfactuals need no further commenting. These are obviously unrealistic and economically make no sense.

Permit me to hammer home the point. The findings of Besley and Burgess imply that just one single *pro-worker* amendment of IDA during the thirty-three year period of 1958-1992 carried an uncannily high cost to India's registered manufacturing: *without the pro-worker amendment*, output would have been 22% higher, employment 15% higher, labor productivity 7% higher and fixed capital stock 16% higher. These findings tax any reasonable person's capacity for belief. Finally, and just for the record, Table 6 confirms what I argued in

the theoretical section: according to the findings of Besley and Burgess, the introduction of a pro-worker amendment did reduce capital intensity (fixed capital per worker) and increase labor intensity (workers per unit of output). These findings nullify the theoretical explanations offered by Besley and Burgess.

VI. Power and the useful economist¹⁹

The paper of Besley and Burgess (2004) suffers from fatal errors of omission and commission, but I shall spare us the pleasure of extensive recapitulation. What I want to emphasize instead is that this high-profile paper is by no means the only one to fail the replication test and/or break down after closer scrutiny. The problem is endemic in economics, which so often claims to do ‘value-free’ social-science research and provide ‘evidence-based’ public-policy advice. I already referred to the study by Kanbur and Ronconi (2016), who demonstrate that the negative impacts of labor laws on economic performance found by Botero *et al.* (2004), another *Quarterly Journal of Economics* article, turn statistically insignificant after quite sensibly controlling for ‘enforcement’. Likewise, careful replication analyses by Howell, Baker, Glyn and Schmitt (2007), Baccaro and Rei (2007) and Vergeer and Kleinknecht (2012) jointly falsify a dozen high-profile econometric studies, published in peer-reviewed outlets, which all reported negative impacts of pro-worker regulation on unemployment in the OECD economies, including articles published by *The Economic Journal*. The published results were found to be robustly non-robust, with signs (+/–) of impacts changing and their statistical significance invalidated in response to minor amendments in estimation procedures. The fundamental flimsiness of approach and findings of this body of work stands in stark relief to its more than sweeping, and socially costly, public policy recommendations to abolish pro-worker labor regulation. The fact that all these papers came through the peer review process does suggest that the peers suffer from confirmation bias, defining ‘scientific excellence’ as whatever is closest to their own scholarly beliefs and methods (Ioannides *et al.* 2017; D’Ippoliti 2018).²⁰ Worse, deviating from good scientific practice, the top journals generally do not generally deem it necessary to publish failed replications as a corrigendum to the articles they did publish. Priors continue to overwhelm evidence and sound academic practice.

¹⁹ The title is due to Galbraith (1973).

²⁰ Corroborative evidence by D’Ippoliti (2018) shows that citation practices are influenced by membership of a ‘social community’ and ideological proximity.

Table 6
Counterfactual analysis: India's registered manufacturing, 1990,
without either pro-employer or pro-worker amendments

	mean BB-index	Percentage change relative to actual level in 1990:					
		Output	Employment	Labor productivity	Fixed capital	Capital intensity	Labor intensity
<i>without pro-employer amendments</i>							
Andhra Pradesh	-0.171	-3%	-2%	-1%	-2%	0%	1%
Rajasthan	-3.623	-52%	-39%	-20%	-41%	-2%	13%
Tamil Nadu	-2.314	-37%	-27%	-13%	-29%	-2%	10%
<i>without pro-worker amendments</i>							
Gujarat	0.829	18%	12%	5%	13%	1%	-6%
Madhya Pradesh	0.286	6%	4%	2%	4%	0%	-2%
Maharashtra	1.314	30%	20%	8%	21%	1%	-10%
Orissa	0.571	12%	8%	4%	9%	1%	-4%
West Bengal	5.800	220%	123%	42%	132%	9%	-97%

Notes: The counterfactual results are obtained using the coefficients from the industry-level estimations for the period 1980-1997 (summarized in Table 1) and the state-wise means of the cumulative BB-index.

But not all is lost: the perversity thesis—that pro-worker regulation ends up hurting the workers it was designed to protect—is becoming increasingly untenable. Recent meta-analyses of the literature on labor regulation and economic performance by Nataraj *et al.* (2014) and Broecke *et al.* (2017) find that the effects of regulation on growth and employment are generally small or absent, and negligible compared to those on income distribution. Summing up the evidence, Richard Freeman (2010) writes that pro-worker labor regulations “reduce the dispersion of earnings and income inequality” while their “effects on other aggregate outcomes, such as employment and unemployment are inconclusive.” In a World Bank discussion paper reviewing the impacts of labor regulation on economic performance, Betcherman (2014) concludes that these impacts on growth are much smaller than the heat of the debates suggest. The *World Development Report 2013 on Jobs* concurs: the ‘efficiency enhancing’ and ‘efficiency undermining’ effects of labor regulation generally cancel out, and hence most of their effects are redistributive. World Bank research (Kuddo, Robalino and Weber 2015, p. 11) concludes that “although the range of estimates from the literature varies considerably, the emerging trend is that the effects of minimum wages on employment are usually small or insignificant (and in some cases positive).” Even the IMF (2016, p. 115) is changing its view in response to the new evidence, concluding in its *World Economic Outlook* of 2016 that: “The analysis shows that reforms that ease dismissal regulations with respect to regular workers do not have, on average, statistically significant effects on employment and other macroeconomic variables.” These new findings directly challenge Besley and Burgess’ ‘rhetoric of reaction’ and also open up a range of public policy choices to improve distribution, productivity and competitiveness (ILO 2016/17); and they bring us full circle to Sidney and Beatrice Webb (1902) who emphasized the “efficiency and equity gains to be had from regulating and balancing labor markets, democratizing firm governance systems, and giving workers effective voice in the polity...” (Kaufman 2013, p. 788).

There are two final inferences to which this ‘deconstruction’ of Besley and Burgess (2004) compels us. First, the fact that this article was published in a double-blind peer-reviewed top-notch economics journal shows that arrangements by which the rhetoric of reaction is conserved in the modern academy remain formidable. It is by telling “the young and susceptible and the old and the vulnerable” that the outcomes of progressive, emancipatory public policy (such as pro-worker regulation) are antithetical to their interests, that established economics becomes the “invaluable ally of those whose exercise of power depends on an acquiescent public”, as Galbraith (1973, p. 11) put it so forcefully. Economics needs to emancipate itself from those who exercise power—a task that must begin with “the

emancipation of economic belief” (Galbraith 1973, p. 11). Or as Ferguson and Johnson (2018) put it more recently: we have “to ask if something [is] not radically wrong with the structure of the discipline itself that conduced to the maintenance of a narrow belief system by imposing orthodoxies and throwing up barriers to better arguments and dissenting evidence.”

Second, let me close by restating what I would like the reader to take away from this paper, which is that economics has to exercise considerable caution and humility in constructing and interpreting the empirical evidence and using it to back up public policy advice. As the example of Besley and Burgess (2004) illustrates, the social and economic damage caused by wrong-headed policy advice can be substantial. I know of no credible evidence that suggests that pro-worker labor regulation systematically ends up hurting workers and that could possibly justify the waves of labor market deregulation around the globe (Freeman 2010; Storm and Capaldo 2018). If anything, labor regulation is a strong force for the social good and for economic progress, because it acts as ‘beneficial social or regulatory constraints’ (see Streeck 2004), which force capitalists (in Schumpeterian fashion) to innovate so as to benefit from these constraints and thereby improve the economy’s dynamic efficiency (Storm and Capaldo 2018). The evidentiary base in support of the ‘technology-forcing’ impacts of labor rules is building up in an unrelenting manner—and it will eventually force established theory to recognize that laws created to help workers do not hinder economic performance—not in India nor elsewhere. But we know from past experience that this will take a long time. As John Kenneth Galbraith (1971, p. 50) saw so clearly, “[f]aced with the choice between changing one's mind and proving that there is no need to do so, almost everyone gets busy on the proof.”

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