

LABOUR MARKET REGULATION AND INDUSTRIAL PERFORMANCE IN INDIA

A Critical Review of the Empirical Evidence

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ABSTRACT

This paper offers a critique of recent empirical studies on the impact of labour regulation on industrial performance in India. It begins with a review of earlier studies that tried to infer the effects on manufacturing employment of amendments to the Industrial Disputes Act (IDA) in 1976 and 1982 that required government permission for layoffs, retrenchments and closures, and shows that the results are ambiguous. It then criticizes the widely-used index of state-level labour regulation devised by Besley and Burgess (2004), and the econometric methodology they use to establish that excessively pro-worker regulation led to poor performance in Indian manufacturing. Several recent studies that have used their index are also surveyed. Finally, the paper reviews other evidence, pointing in a very different direction, on the actual enforcement of labour laws, labour flexibility, and industrial employment. Throughout, attention is paid to the crucial role of judicial interpretation of the IDA, which has been neglected in this literature.

Keywords: India, industrial relations, employment protection laws, job security regulations, labor flexibility.

JEL Classification: J53, J68, O53

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1. Introduction

Various studies indicate that Indian labour laws are highly protective of labour, and labour markets are relatively inflexible. These laws apply only to the organised sector. Consequently, these laws have restricted labour mobility, have led to capital-intensive methods in the organised sector and adversely affected the sector's long-run demand for labour. Labour being a subject in the concurrent list, State-level labour regulations are also an important determinant of industrial performance. Evidence suggests that States, which have enacted more pro-worker regulations, have lost out on industrial production in general.

-- (Ministry of Finance, 2006, p.209).

The paragraph from the most recent *Economic Survey* quoted above is the latest indication of the government's thinking on the reform of Indian labour laws. It does not identify the "various studies" or the source of the state-level evidence that support its assertions. But to those familiar with the literature, the reference is obviously to the work of Besley and Burgess (2004) and several recent papers that use the "regulatory measure" devised by them to come to conclusions along the same lines.¹ In this paper, I undertake a critique of this body of work.

One of the remarkable features of this literature has been that out of the dozens of labour laws in force, empirical studies have focused almost exclusively on the Industrial Disputes Act (IDA) of 1947. Particular attention has been paid to its Chapter V-B, introduced by an amendment in 1976, which required firms employing 300 or more workers to obtain government permission for layoffs, retrenchments and closures. A further amendment in 1982 (which took effect in 1984) expanded its ambit by reducing the threshold to 100 workers. It is argued that since permission is difficult to obtain, employers are reluctant to hire workers whom they cannot easily get rid of. Job security laws thus protect a tiny minority of workers in the organised sector and prevent the expansion of industrial employment that could benefit the mass of workers

outside. It is also argued that the restriction on retrenchment has adversely affected workplace discipline, while the threshold set at 100 has discouraged factories from expanding to economic scales of production, thereby harming productivity. Several other sections of the IDA allegedly have similar effects, because they increase workers' bargaining strength and thereby raise labour costs either directly through wages or indirectly by inhibiting work reorganization in response to changes in demand and technology.

While these arguments are persuasive, the evidence to back them is less so. There are actually two distinct approaches have been used in the empirical literature to examine the consequences of the IDA for industrial performance. The first, following from the paper by Fallon and Lucas (1993), employs a "before and after" methodology to examine the impact of the 1976 and 1982/84 amendments on employment in manufacturing at the national or industry level. The second, following from Besley and Burgess (2004), exploits variations in IDA amendments made by different states to explain various state-level economic outcomes. Studies using the first approach are discussed in Section 2, where I show that their results are inconclusive, and that the entire literature ignores crucial developments in the political and legal spheres which vitiate many of these findings. In Section 3, I summarize and discuss the work of Besley and Burgess (hereafter BB). I show that the regulatory measure that they construct is flawed, and that the results of their econometric analysis are extremely fragile. Section 4 deals with studies by later authors who draw upon BB to analyze a range of related phenomena, and Section 5 points to evidence of slack enforcement of labour laws and changes in industrial performance that has been ignored by all these authors. Section 6 summarizes and concludes the paper.

The need for this exercise needs to be justified in view of three excellent recent surveys of the literature on the subject by Shyam Sundar (2005), Anant et al (2006) and Sharma (2006); the latter two are also sceptical of BB's findings. They dispute claims that excessively pro-worker

¹ It could be argued that the *Economic Survey* is not an academic research paper, and so it is not bound to identify its sources. But on the subject of total factor productivity growth (p.137), it names the authors of

legislation impaired industrial performance by providing evidence of considerable employment flexibility, slack enforcement of labour laws and weakening of labour's bargaining power. This material is certainly very useful, and I cite it extensively below in Section 5. But I critically examine the BB methodology itself, which has remained unquestioned throughout this debate. I also critically examine some recent literature that does not figure in these three surveys, and unlike them I examine the crucial role of judicial interpretation of the IDA.²

I should state at the outset that the objective of this paper is to examine the quality of academic research on labour regulation and industrial performance in India, and not to argue a case for or against regulation. In fact, the main lessons are in regard to academic standards, not labour standards.

2. Effects of the 1976 and 1982 central amendments

2.1 Econometric Studies

The methodology of all the papers to be reviewed in this section involves estimation of different versions of dynamic labour demand functions for Indian manufacturing industries in the registered (organised) sector. Such functions attempt to capture the costs of hiring, firing and training workers by incorporating adjustment lags in the relationship between employment and labour demand. Employment (N) is typically regressed on its own lagged value and the current and lagged values of demand-related variables such as the level of output and wages. The studies surveyed below use many such variables, multi-period lags, and sophisticated econometric techniques to deal with the problem created by using a lagged dependent variable as a regressor.

no less than nine recent studies, although it does not provide a list of references.

² The three surveys are course far more comprehensive in other respects, covering among other things employment and wage trends in the economy, social security issues, the international evidence on the relationship between job security legislation and employment, and summaries of the Report of the Second National Commission on Labour, international agreements and the legal provisions on job security in different countries. The focus of this paper is much narrower.

But oversimplifying greatly in the interest of exposition, for a single labour demand variable (X) and a one-period lag, for industry i in period t , the regression equation can be represented as:

$$N_{it} = \beta_0 + \beta_1 X + \beta_2 X_{i,t-1} + \beta_3 N_{i,t-1} + \beta_4 J + \beta_5 JN_{i,t-1} + v_{it}$$

A statistically significant positive coefficient β_3 measures the degree of inertia in the adjustment process. The immediate impact on employment of a change in job security laws can be inferred from the coefficient β_4 on a dummy variable J that switches from zero to one in the year the change occurs. The coefficient β_5 on the interaction of J with the lagged employment term shows how the legal change affects the degree of inertia.

The pioneering study by Fallon and Lucas (1993) for the period 1959-82 employed a dummy variable taking the value zero up to 1975-76 and unity thereafter. The coefficients corresponding to β_3 and β_5 for most industries indicated that employment adjustment was sluggish even before 1976, with no significant increase in the degree of inertia after the amendment. Fallon and Lucas, however, highlighted their finding that the amendment did cause a decline in employment (as revealed by the negative coefficient corresponding to β_4) in most industries, averaging 17.5% across industries. This effect was stronger in plants employing more than 300 workers (the threshold at which the amendment applied), those in the private sector, and those with lower union membership.

These results were obtained using data for the census sector of the ASI, which covers plants with at least 100 workers. However, Fallon and Lucas also showed that the sample sector (registered manufacturing firms employing less than 100 workers) displayed no offsetting gains in employment. Nor did it display statistically significant reductions in employment, which would have indicated that the decline in employment in the census sector was being caused by industry-

specific factors that were not being picked up by the other explanatory variables of the model. Most of the estimates were robust to the inclusion of a time trend.

These results did give credence to the growing belief that excessive concern for job security was adversely affecting employment. However, Bhalotra (1998, p.7) pointed out that the 17.5% overall drop in employment that Fallon and Lucas attributed to the 1976 amendment was based on the questionable procedure of averaging across all industry-specific coefficients on the dummy variable, many of which were statistically insignificant at conventional levels. They had found coefficients that were significantly negative in 25 (out of 35) industries – but at a 25% level of significance. At the more conventional 10% and 5% levels of significance, respectively only 14 and 11 industries displayed employment declines in response to the amendment.

In her own estimates of dynamic labour demand functions, Bhalotra used a series (1979-87) that was too short to capture the full effects of the 1982/84 amendment. She did however comment critically on the claim that it was responsible for the phenomenon of “jobless growth” witnessed by the organised Indian manufacturing sector during most of the 1980s. She pointed out (pp.7-8) that the amendment could not have had a major impact in light of evidence she offered on: (a) sluggish adjustment in employment (so the amendment that took effect in 1984 could not have caused the immediate fall in employment that continued until 1988); (b) widespread evasion of labour laws; (c) manufacturing wages being negatively related to regional unemployment during that decade (which would be unlikely if workers faced no effective threat of retrenchment); and (d) employment increasing in factories with between 100 and 1000 workers but falling for those with more than 1000 (whereas the 1982/84 amendment had a threshold at 100). Replicating a World Bank study which had estimated a simpler dynamic labour demand equation for the period 1974-84, she found a decline in employment inertia during her own sample period (1979-87). She related this apparent increase in labour market flexibility in the 1980s, despite the tightening of job security legislation, to the growing trend towards subcontracting output to small firms which were outside the scope of the law, and to easier

recourse to firing by larger firms which occurred despite the provisions of the law (Bhalotra, pp.20-22).

More recent estimates of dynamic labour demand functions for Indian industry by Aggarwal (2002) and Dutta Roy (2004) have covered much longer periods, used different econometric techniques, and have dealt separately with workers and ‘supervisors’ (in ASI data these include employees unconnected with production and possibly also higher-paid workers). Although Aggarwal (2002), who covers the period 1960-98, does not explicitly deal with the issue of job security legislation, his measure of inertia increases in the sub-period 1976-85 but becomes insignificant for 1985-98, when employment responded swiftly to growth in value added. This indicates that the 1976 amendment may have reduced flexibility, but the 1982/84 amendment paradoxically increased it, or that the law had no effect on actual practice in the more recent period.

Dutta Roy (2004), allowing for inter-related factor demands and covering the period 1960-95, does explicitly test for the impact of the IDA amendments. She finds that most industries exhibited considerable rigidity in their employment adjustment even before 1976, and only in the cement industry did it get worse after the amendments (for workers only, not for supervisors). Paradoxically, in four industries, the amendments actually appeared to *increase* flexibility, which Dutta Roy attributes to the growing use of contract and casual labour, and to greater flexibility in hours worked. Her conclusions are worth quoting at some length: “our findings indicate that the imposition of job security regulations can, by no means, be identified as the sole, or even the primary, cause for the observed rigidities in employment adjustment in the Indian manufacturing sector. Major proportion of the industries studied reveal rigidities even in the pre-JSR [job security regulations] period, indicating that these may be attributable to industry-specific characteristics” (p.248).

The average length of time required to complete most of the adjustment of employment to exogenous shocks, as estimated by both Bhalotra (1998) and Dutta Roy (2004), was of the

order of five to six years. It also varied considerably across industries and as between workers and supervisors, which means that the average lag at the aggregate level would be affected by changes in the composition of industries within a state and of employees within industries. It is important to keep this in mind while reviewing some of the studies to be discussed in Sections 3 and 4, which have neglected the dynamics of adjustment.

Theoretically, as both Bhalotra (1998) and Dutta Roy (2004) pointed out, job security regulations should decrease *both* hiring and firing rates, with ambiguous effects on employment. In an earlier paper, Dutta Roy (2002) had examined the impact of the 1976 and 1984 amendments on the rates of accession and separation separately (the change in employment, the focus of all the other papers surveyed here, would be the difference between these two rates). She found that both rates responded positively to planned employment growth³ between 1966 and 1975, but negatively between 1976 and 1983, with the separation rate falling more than the accession rate. Neither rate responded significantly to planned employment growth between 1984 and 1992. These findings indicate that although the IDA amendments did not affect employment very much, they did affect labour turnover and the way in which accessions and separations were used to adjust employment. She concludes that the job security regulations did inhibit labour market flexibility.

2.2 The Political and Legal Environment

The papers surveyed above have searched diligently for the effects of the 1976 and 1982/84 IDA amendments on employment. Unfortunately, all of them have overlooked developments in the political and legal spheres that complicate matters. Evidence of the slackening of enforcement since the 1980s will be reviewed in Section 5 below, but one very obvious political development makes it difficult to treat the 1976 amendment as causing a structural break, which is how it is

³ This is determined in her model by output growth and labour costs, as data limitations did not permit her to estimate dynamic labour demand functions for this exercise.

treated in most of these studies. The amendment was passed during the 1975-77 State of Emergency, during which democratic rights were effectively suspended. It is likely that taking advantage of this situation, employers retrenched labour en masse. Supporting evidence comes from a minor result reported by Dutta Roy (2002, p.152), who found that the only year dummy that emerged as significant in her regression for the separation rate was for 1976, when it was positive, indicating an abnormal increase in separations. This may explain the reduction in employment in 1976-77 that can still be inferred from the Fallon and Lucas results at conventional levels of significance even after Bhalotra's critique. Otherwise, precisely because Chapter V-B was supposed to have made retrenchment and closure more difficult, one would not expect a sudden fall in employment *after* it came into effect in March 1976. It is possible that mass retrenchments were undertaken in anticipation of stricter enforcement in future, but this is difficult to establish empirically. In the longer term, any adverse effect of the 1976 amendment should have shown up as a more sluggish response to changes in labour demand, which Fallon and Lucas did not find, and on which the findings of other studies are mixed.

The literature has also overlooked the fact that all the relevant sections of Chapter V-B – requiring official permission for layoffs (Section 25(M)), retrenchments (25(N)), and closures (25(O)) – were contested in legal battles that raged for the next quarter-century. 25(O) was struck down as unconstitutional by the Supreme Court as early as 1978.⁴ Several High Courts then invalidated 25(M) and (N) on similar grounds. The 1982 amendment, apart from reducing the permission threshold to 100, incorporated several procedural changes in 25(O) so as to satisfy the Supreme Court. It was brought into effect in 1984, almost simultaneously with a separate amendment that modified the other sections along similar lines. The Supreme Court eventually upheld Sections 25(N) and (M), in 1992 and 1994 respectively. After conflicting verdicts by

⁴ *Excel Wear vs Union of India* (1978) 4 SCC 224. In this and subsequent citations, SCC refers to the authoritative compilation *Supreme Court Cases*.

various High Courts, it upheld the amended 25(O) only in 2002.⁵ Thus, 25(O) would have remained inoperative all over the country between 1978 when it was struck down by the Supreme Court and 1984 when the curative amendment took effect, and along with the other two sections it would have been unenforceable in certain states unless the relevant High Courts judgments were stayed while the appeals were pending in the Supreme Court. Even in other states, legal uncertainties may have affected the incentives of employers to apply for permission.

Further, all these amendments involving Chapter V-B were in the direction of reduced flexibility for employers, but the 1984 amendment also changed the definition of ‘retrenchment’ in Section 2(oo) so as to exclude from its purview any termination of service resulting from the non-renewal of a contract or under a stipulation contained in the contract. This would be conducive to *greater* flexibility, because retrenchment requires notice and payment of compensation for establishments covered by Chapter V-A (those employing at least 50 workers), plus official permission for those covered by Chapter V-B.

Thus, the 1976 and 1982/84 amendments cannot be regarded as events which unambiguously increased labour inflexibility, enabling econometricians to employ their usual techniques. This may account for the contradictory findings reviewed above.

3. Effects of state-level amendments: Besley and Burgess

3.1: A Summary of the Paper

Besley and Burgess (2004) exploit variation in the direction of amendments that different states made to the IDA over the period 1947-97. They classify amendments as pro-worker, neutral or pro-employer, assigning scores of +1, 0 and –1 respectively to each state for the relevant year. They then cumulate these scores state-wise over time to obtain what they call a ‘regulatory measure’ for each state in each year; I shall henceforth refer to this as the ‘BB index’. BB use this

⁵ See respectively *Workmen vs Meenakshi Mills* (1992) 3 SCC 336, *Papnasam Labour Union vs Madura Coats Ltd* (1995) 1 SCC 501, and *Orissa Textile and Steel Ltd vs State of Orissa* (2002) 2 SCC 578.

index, along with several control variables,⁶ to explain (with a one-year lag) state-level variables pertaining to the organised manufacturing sector, including output per capita, employment, intensity of labour usage, fixed capital, the number of factories,⁷ and labour productivity (value added per employee), using panel data for 1958-92. BB find that regulation in a pro-worker direction adversely affected all these outcomes for registered manufacturing, but promoted output in unregistered manufacturing, consistent with the view that labour regulation, which covers only the organised sector, diverts activity to the unorganised sector. Their innovative treatment of endogeneity issues enables them to conclude that poor manufacturing performance was a consequence rather than a cause of pro-worker labour regulation.

Using industry dummies, BB find very similar negative effects on industrial performance at the level of three-digit industries within each state for a different time period (1980-97). This enables them to claim that their results at the aggregate level are not being driven by interstate differences in industrial specialization or technological progress. They also find that pro-worker legislation had no effect either on earnings per worker in the organised sector or on aggregate or rural poverty in the state, but it increased urban poverty (measured in each case by the head-count ratio). They attribute this to its adverse effect on organised-sector employment.

If the Besley-Burgess results are to be believed, well-intentioned pro-worker legislation has actually harmed workers by lowering employment and investment in the organised sector while worsening urban poverty, and has had no positive impact on earnings per worker. However,

⁶ The state's development expenditure and installed power generation capacity per capita, population, and ruling political parties, as well as state and year fixed effects. The inclusion of a state's installed power generation capacity to explain the performance of its manufacturing sector ignores a number of serious problems: (a) low plant load factors, which mean that actual generation falls far short of installed capacity; (b) trade in power between states via regional grids, which means availability differs from generation; (c) huge transmission and distribution losses; and (d) power consumption by sectors other than organized manufacturing. Each of these would vary over time and across states. Inclusion of power *consumption* by the relevant industry, on the other hand, would invite accusations of simultaneity. But the omission of any direct indicators of state-level infrastructure or human capital is disturbing.

⁷ They incorrectly and repeatedly use 'number of factories' interchangeably with 'number of firms' (pp.109 n.20; 118; 128).

there are several problems with their analysis. Some have been mentioned in the preceding two end-notes, but there are more serious ones, to which I now turn.

3.2: Problems with the BB Index

BB have provided a synopsis of the state-level amendments, and the scores they assigned, in Appendix 2 of their paper. This refers to a more detailed summary that has been helpfully provided in an Appendix which is accessible from Burgess' website.⁸ A close examination of these appendices shows that both the scoring of individual amendments, and the methodology used to combine the scores, is quite misleading.

The BB scoring system allows a state to be classified as pro-worker or pro-employer on the basis of just one or two amendments at any time in the 50 years of IDA history (1947-97) covered by the study. Thus, Gujarat is designated as pro-worker (a strange characterization, to which I return below) because of a solitary amendment which it passed in 1973, allowing for a penalty of fifty rupees a day on employers for not nominating representatives to firm-level joint management councils. Andhra Pradesh gets a pro-employer tag because of three amendment episodes: one in 1949 which need not be questioned, another in 1968 described in Appendix 2 of BB as one that "Limits strikes and lockouts in designated public utilities", and a third in 1982 that "facilitates settlement of industrial disputes in labour courts". Now, if one refers to the more detailed description of the latter two amendments in BB's web-based Appendix, one gets a completely different picture. The 1968 AP amendment merely included hospitals and dispensaries in the list of public utilities, making it irrelevant for research confined to the manufacturing sector. The case of the amendment that BB claim that Andhra Pradesh made to Sections 11A to 11D of the IDA in 1982 is even stranger: the amendment inserting Section 11-B (there is no 11 C and D) was actually enacted in 1987, and it merely conferred on labour tribunals and labour

⁸ <http://econ.lse.ac.uk/staff/rburgess/wp>

courts the powers of a civil court to enforce their awards – which need not be pro-employer.⁹ BB make the same mistakes in the case of the Madhya Pradesh amendments of 1982 (empowering labour courts) and 1983 (extending closure rules to undertakings engaged in construction activities – which are not included in their study).

Further problems arise in BB’s interpretation of supposedly pro-worker amendments made by Maharashtra (1981), Orissa (1983) and Rajasthan (1984) to Section 25-K of the IDA, which defines the scope of the controversial Chapter V-B. In their Appendix 2, BB summarize these changes as “Extends rules for layoff, retrenchment and closure to smaller firms”. In their unpublished web Appendix, this is spelt out in greater detail as follows for Maharashtra and Rajasthan: “The rules for lay-off, retrenchment and closure may according to the discretion of the state government be applied to industrial establishments of a seasonal character and which employ more than 100 but less than 300 workers. Under the central act these rules only apply to permanent establishments which employ more than 300 workers.” Thus, the pro-worker coding of these amendments seems to rest on their extending the scope of Chapter V-B in two ways: (a) to establishments of a seasonal character, and (b) to establishments employing between 100 and 300 workers.

Unfortunately, the coding is erroneous on both counts. As regards (a), the Maharashtra and Rajasthan amendments *specifically excluded* establishments “of a seasonal character” (Radhakrishnaiah 2003, p.190). BB’s description of the Orissa amendment correctly omits this phrase, as Orissa merely reduced the threshold of applicability to 100 workers, retaining the exclusion of seasonal establishments which was already in the central Act.¹⁰ But this reduction in threshold by all three states had already been initiated by the central government in its 1982 amendment to Section 25-K, which was brought into force in August 1984. This means that BB

⁹ Radhakrishnaiah (2003), p.135. The judicial decisions in the four cases under this section summarized by Radhakrishnaiah appear to be equally divided in favour of workers and employers.

cannot justify their classification of the three state amendments as pro-worker on the basis of (b). Obviously, these states were unwilling to wait for the central amendment to be notified and went ahead with their own. This might perhaps indicate their pro-worker sympathies, but these amendments would at best have had a transient impact, as the central amendment came into effect within a few years (in Rajasthan's case, within a few months), erasing any difference between states on this count. The web appendix of the BB paper shows that the authors used this argument to justify a score of 'zero' (neutral) for amendments to Section 25-R by four states during 1982-84.

Admittedly, disregarding the three state amendments to Section 25-K would not alter the score of +1 assigned by BB to the respective state-years, because in each case at least one other IDA section was simultaneously amended in a pro-worker direction. However, it would certainly leave the overall pro-worker characterization of Orissa resting on just one minor amendment in half a century.

This practice of assigning a score of just +1 or -1 to a year in which a state made more than one amendment is itself problematic, for several reasons. With this procedure, 113 amendments collapse to only 19 episodes of legislative change within the period of the econometric study (1958-97), four of them in West Bengal alone, with the remaining 15 spread across nine other states over 40 years. Changes in the BB index are thus infrequent, and of equal magnitude (either +1 or -1) in the ten states in which some change occurred, regardless of their relative importance or the extent to which they were actually implemented. The limitations of this procedure become apparent if we look at the amendments made in Rajasthan in 1960, summarized in Appendix 2 of Besley and Burgess (2004) along with the scores assigned by them:

¹⁰ Incidentally, the full texts of the amendments also show that BB overlooked the fact that while the Orissa and Rajasthan amendments were applicable to all of Chapter V-B, Maharashtra's was applicable only to Section 25-O, governing closures (Radhakrishnaiah 2003, p.190).

Exact criteria for being union member defined [-1]; Defines employers in firms sub-contracted to industry as employers for industrial disputes purposes [1]; Defines who is allowed to be involved in bargaining process on behalf of unions [-1]; Gives definition of what a union is in an industrial dispute [-1]; Definition of worker for industrial disputes purposes extends to those subcontracted with an industry [1].

Of the five amendments in this package, the second and fifth are but different sides of the same coin. The interpretation of the third is erroneous: it merely required the state government to appoint a Registrar of Unions and local Assistant Registrars, with no indication that these officials were supposed to represent unions. And these disparate and incommensurable changes, pulling in different directions, are lumped together and classified as ‘pro-employer’, with a summary score of -1 for the year 1960. But the same score is assigned to Andhra Pradesh for 1968, in which it enacted the minor amendment described above. Can we really regard these reform episodes as equivalent? In turn, Andhra Pradesh made eight changes to the IDA in 1987. BB give six of them a score of +1 and two -1, but the state gets a score of +1 for the year.

A different problem arises when several amendments operating in the same direction are passed in the same year. In 1983, for example, Orissa passed two pro-worker amendments: the one discussed above, extending rules for layoff, retrenchment, and closure to smaller firms, and another allowing workers to appeal against a decision to close down a firm. As they were passed in the same year, they raise Orissa’s score by +1. On the other hand, Maharashtra passed what BB describe as the same two amendments in different years (1981 and 1983),¹¹ so its score is incremented by +2 under the cumulation rule. Why should a state that spreads the same reforms over a number of years be regarded as more pro-worker than one that enacts them in the same year?

¹¹ Two other pro-worker amendments were also passed in 1981, but as in the case of Orissa, they still raised Maharashtra’s score by only 1 in that year.

This entire range of problems – inappropriate classification of individual amendments, summary coding of incommensurable changes as either +1 or –1, and misleading cumulation over time – is manifest in the case of Andhra Pradesh. The state’s irrelevant amendment of 1968 and the miscoded and misdated amendment of ‘1982’ (described above) are wrongly given scores of – 1 each, which are then cumulated with the 1949 amendment. The three together thus easily outweigh the far more extensive set of mainly pro-worker reforms in 1987, which are collectively assigned a score of +1. Consequently, Andhra Pradesh’s aggregate score is negative, giving it ‘pro-employer’ credentials, which as we shall see below are used by both BB and the World Bank to commend its policies.

Finally, the BB index ignores the existence of many other labour laws. There are at least 45 central Acts alone dealing with labour, and under the Indian Constitution, states can not only make their own amendments to many of these central Acts, but also pass their own laws on matters that fall under the subjects enumerated in the Concurrent List.¹² Their amendments to the IDA alone, therefore, may not be representative. And Section 10 of the Contract Labour (Regulation and Abolition) Act (1970) gives wide discretion to state governments in permitting firms to employ contract labour, which may be used to escape the obligations of the IDA. A full study of the inter-state variation in these laws is far beyond the scope of this paper.

There are, however, at least three state laws¹³ that overlap considerably with the IDA, and take precedence over it. Section 31 of the Industrial Disputes (Amendment and Miscellaneous Provisions) Act of 1956 provided that the IDA would not override any state laws on industrial disputes that were already in force. Further, according to Article 254 of the Constitution, if any

¹² Debroy (2005, pp.39-41) lists 45 central Acts, 16 associated rules, and four other Acts that deal at least partly with labour. He also gives a highly entertaining selection of excerpts from these laws to show how utterly absurd and unenforceable some of them are. Anant et al (2006, p.242) mention 47 central and 200 state labour laws.

¹³ The Bombay Industrial Relations Act, 1946 (applicable to the successor states of Gujarat and Maharashtra), the U.P. Industrial Disputes Act, 1947, and the Madhya Pradesh Industrial Relations Act, 1960. Other state laws whose coverage overlaps with that of the IDA include the Maharashtra Recognition of Trade Unions and Prevention of Unfair Labour Practices Act (1971), as well as the Shops and Establishments Acts in various states.

provision of a state law is “repugnant to” a central law on a matter falling within the Concurrent List, the state law will be void to the extent of the repugnancy. But the state law will prevail if it has been enacted later and has received the assent of the President of India. These considerations have proved decisive in several cases. Uttar Pradesh, for example, amended its own 1947 IDA in 1983 to insert Section 6-V, setting the threshold for permission for layoffs, retrenchment and closures at 300. As the Supreme Court noted while upholding the applicability of the U.P. over the central Act, this must have been done in the full knowledge of the 1982 central amendment of the IDA which reduced the threshold to 100.¹⁴ In another case, involving retrenchment, the Supreme Court upheld the applicability of certain definitions in the U.P. Act, which had originally been identical to those in the central Act, but had *not* been amended in line with a pro-worker amendment to the latter in 1964.¹⁵ These cases go to the heart of BB’s methodology: they found that U.P. had made no amendments to the central IDA over the entire 35 year period of their study, and therefore classified it as a ‘control’ state. But having its own IDA, U.P. did not need to amend the central act. On the basis of its legislative record, the state should be classified as pro-employer.

3.3 Econometrics: Questionable specifications

The many flaws pointed out above are indicative of a certain casualness in the construction of the index. They do not, however, undermine the econometric results. On the contrary, measurement errors in an explanatory variable bias the estimated coefficients towards zero, so the results are actually strengthened. But there are other problems that cannot be so easily evaded. First, BB’s use of the same set of control variables (listed in n.6 above) in regressions that seek to explain

¹⁴ *Engineering Kamgar Union vs Electro Steel Castings*, (2004) 6 SCC 36; re-affirmed in *Amrendra Kumar Pandey vs Triveni Sheet Glass Works*, (2005) 7 SCC 307.

¹⁵ *Kamala Nehru Memorial Hospital vs Vinod Kumar*, (2006) 1 SCC 498.

outcomes as disparate as output, employment, wages, entry and poverty is disquieting. Not surprisingly, the coefficients on most of the controls are statistically insignificant.¹⁶

Testing for the robustness of estimates with different combinations of *relevant* conditioning variables can demolish deeply-held convictions, as has been seen in the case of the relationship between liberal trade policies and growth (Levine and Renelt, 1992). Industrial activity can be potentially influenced by several other state-level variables: a recent paper by Sanyal and Menon (2005) (discussed below) uses a dozen, while the recent *India Labour Report* (TeamLease Services, 2006) constructs a ‘labour ecosystem index’ using no less than 27 state-level variables covering infrastructure, governance, human capital availability, and various measures of industrial disputes and dispute settlement. True, not all these variables are available from 1958, which is what BB required for their longer panel, but some of them are available for the period covered by their shorter 1980-97 industry-level panel. And at least for poverty the statistical correlates are well-known and available in series going back to 1960.¹⁷

Besley and Burgess also ignore the allocation of industrial licences by the central government, which was a significant determinant of industrial location for most of their sample period. Curiously, they observe that “There have been no formal amendments to [the Industries (Development and Regulation) Act] at the state level. We therefore have a situation where industries in different states of India are subject to a common set of industrial policies except in the area of industrial relations. Entry regulation, via licensing and other instruments, for example is completely controlled by the central government” (pp.95-97). But this assertion entirely misses the point, which is that the central allocation of licences influenced the regional distribution of

¹⁶ Moreover, although BB claim that their results are robust to imposing different lags on the regulatory measure, they have not extended this analysis to any of the control variables. At least in respect of the impact on employment, several studies surveyed in Section 2 above have shown that adjustment to exogenous changes was sluggish and varied over time for most of their sample period.

¹⁷ See Datt and Ravallion (2002) and the earlier work cited by them.

industrial activity independently of state-level regulations, and was in turn influenced by political considerations.¹⁸

There are thus far too many potentially relevant omitted variables for comfort in the BB regressions, which could seriously bias the estimated coefficients on the index if they are correlated with it. For example, the spate of pro-worker IDA amendments in West Bengal in the 1980s coincided with crippling power cuts and an adversarial relationship with the central government which controlled industrial licences. Pro-employer amendments in Tamil Nadu in the same decade coincided with the rapid expansion of technical education which fed the growth of industry. And the collapse of the textile mills, which caused a major fall in registered manufacturing employment in Gujarat, Maharashtra and West Bengal in the 1980s, was not entirely attributable to labour market conditions.

BB themselves candidly report that inclusion of state-specific time trends in the regression equations invariably makes the coefficient on their regulatory measure insignificant. This is true of their results for industrial performance at both state and industry levels, as well as in those for poverty.¹⁹ State-level IDA amendments, as we saw above, were very infrequent, so it is disturbing that their effect is so comprehensively knocked out by the inclusion of time trends. BB claim that “labour regulation therefore appears to be driving differences in these trends”, and that the underlying influence could be poor labour relations (“union power and labour/management hostility” as they describe it on pp.108 and 125). They in fact show that their index is strongly positively correlated (with a one-year lag) with workdays lost to strikes and lockouts per worker over the period 1958-92 – and this finding, unlike all their other results, is robust to inclusion of state-specific time trends (pp.99-100). But the basis of this relationship is unclear,

¹⁸ See Bhagwati and Desai (1970, pp.267-69) and Biswas and Marjit (2002). Of course, state-level conditions would influence industrialists’ desire to apply for licences, but these could be granted or denied based on extraneous considerations, which would thereby determine actual state outcomes.

¹⁹ Besley and Burgess (2004), pp. 108, 112 n.21, 117 n.29, 119 n.32, 121, 125. Their Table IV, on pp.106-7, which reports results for state-level manufacturing performance, shows that the estimated coefficients on several other explanatory variables also exhibit considerable instability when the time trend is included.

and in any case, I shall show in Section 5 below, the relationship between IDA amendments and industrial unrest has comprehensively broken down since the late 1980s.

This leads us to a more general point. BB turn a blind eye to evidence of the weakening of labour's bargaining power and evasion of labour laws since the 1980s, despite the 'pro-worker' legal environment. I mention some of the more recent literature in Section 5 below, but there were already several studies available at the time that BB were writing their paper, for example Bhalotra (1998) and Deshpande (2001). In fact, it is remarkable that BB's list of references contains no study on India's labour markets more recent than Fallon and Lucas (1993).

3.4 Questionable Counterfactuals

Besley and Burgess use their regression results to make bold counterfactual statements on how much more manufacturing output and employment, and how much less poverty, West Bengal would have had if it had not passed any pro-worker amendment, and how much worse Andhra Pradesh would have fared on these counts without pro-employer amendments (pp.112, 121). These counterfactuals are based on point estimates, without any confidence intervals. In light of the problems discussed above, these estimates must be taken with a large pinch of salt.

The World Bank's *World Development Report 2005* (hereafter *WDR*) puts its own entirely unwarranted gloss on these calculations. Citing Besley and Burgess (2004), it claims 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 service companies in the next decade" (p.150). This statement is misleading on several counts. First, we saw above that for Andhra Pradesh in the 1980s, BB listed a supposedly pro-employer amendment in 1982 that was both inconsequential and actually passed in 1987, and a clutch of other amendments in 1987 that they designated as *pro-worker*. Second, the BB paper certainly does not establish that 1.8 million

They do not report results with a time trend in their tables on industry-level performance or poverty, so one cannot really judge how serious this problem of non-robustness is.

urban poor found jobs in manufacturing and service companies in the 1990s. The nearest statement that one can find is the following: “According to our estimates, there would have been around 640 thousand more urban poor in Andhra Pradesh in 1990” had it not amended the IDA (Besley and Burgess, 2004, p.121). As we saw above, BB estimated *separate* regressions for manufacturing employment and urban poverty. The *WDR* somehow manages to conflate the two, extend the results for 1990 into the ensuing decade, nearly triple the number of beneficiaries, and employ some of them in “service companies”, which BB did not cover at all! In any case, extrapolation of the BB results into the 1990s should be subject to the ‘Lucas critique’: parameters estimated under one policy regime may not be valid for a very different one. The *WDR*’s bizarre interpretation of BB’s already fragile estimates should give rise to considerable scepticism – to put it mildly – about its praise for Andhra Pradesh.

3.5 Semantics

At various places in their study, BB use language that betrays a lack of academic impartiality. They refer to an “expropriation effect”, whereby workers can extract a part of the return to capital (p.102); they do not admit the reverse possibility. They also refer to the “vested interests” of workers (pp.113, 116), but not of employers. The evidence to be discussed in Section 5 below shows that at least since the 1980s, it is the power of employers that has been steadily advancing, presumably because of their “vested interests”, and workers who have lost their jobs have often been deprived of their statutory benefits – which surely meets any reasonable definition of ‘expropriation’.

4. Other papers that use the Besley-Burgess index

4.1: Cross-section Studies

Besley and Burgess (2004) only reported the value of their index up to 1992 (there were in fact no further state amendments after 1989). A problem thus arises for some later studies that have tried

to use their index to explain more recent developments. The problem is especially acute for those that use firm-level data. Such data are available only from 1989 onwards, so the BB index exhibits no variation within states over time that these authors can exploit, as BB did, to explain outcomes during the 1990s. Instead of using the annual values of the index, therefore, they have used it, with or without modification, for cross-section analysis. After critically reviewing the individual studies, I shall point out some limitations of this approach.

Hasan, Mitra and Ramaswamy (2003) modify the BB classification of states in three respects. First, they collapse the three categories into two, designating states that BB had classified as 'pro-employer' as those having flexible labour markets, and the rest (those deemed to be 'pro-worker' or 'control' states by BB) as having inflexible labour markets. But they also take cognizance of a strange feature of the classification that stands out for anyone who is reasonably familiar with India. As mentioned above, on the basis of IDA amendments, BB classify Gujarat and Maharashtra as pro-worker; they also designate Kerala as pro-employer. Drawing attention to a World Bank report on the investment climate in various states that provides evidence of actual implementation of labour legislation as indicated by the degree of over-manning and the frequency of inspections, Hasan et al reverse the classification of these three states for their own econometric analysis. Finally, Madhya Pradesh, which BB had classified as pro-employer, is classified by Hasan et al as having inflexible labour markets, because pro-employer amendments in 1982 were offset by a pro-worker amendment the following year.

Using this modified classification, and allowing for lagged adjustment of employment, Hasan et al find that trade liberalization increased the own-price elasticities of demand for labour in Indian manufacturing, comparing the post-reform period 1992-97 to 1980-91. As expected, these elasticities are higher and also more sensitive to trade reforms in states with more flexible labour markets. This indicates that employment responds more vigorously to liberalization where labour markets are flexible. On the other hand, Hasan et al also find that post-reform volatility in

productivity and output results in larger wage and employment volatility, which is a theoretical consequence of higher labour-demand elasticities, as predicted by Rodrik (1997). They also report (pp.31 n.49; 33 n.52) estimations using the original Besley-Burgess classification (collapsed into their two categories). This destroys the result that labour demand becomes more elastic in response to liberalization in states with flexible labour markets, showing how sensitive it is to the classificatory scheme.

Using the original BB classification, Topalova (2004) finds that firms' total factor productivity (TFP) responded favourably to trade liberalization (as measured by lagged industry-specific tariff rates, 1989-2001), but there was no difference as between firms located in the three categories of states. (Recall that BB had found a positive impact of pro-employer reforms on *labour* productivity, not TFP, and for an earlier period.)

In a recent study of the determinants of the location of large private investment projects across states, Sanyal and Menon (2005) use the BB tabulation to compute the *share* of pro-worker amendments from 1949 to 1990 in each state, and also single out two specific types of amendments – the right to strike and provision of severance pay – as separate explanatory dummy variables. In addition, they use direct measures of labour conflict (the number of strikes and lockouts, and the union density) for each state, alternatively as continuous variables and as dummies for states with above-median observations. They also employ a much wider array of state-level controls. Two estimation methodologies are applied: first, panel data models for 1994-99, with the number and value of projects in a state as alternative dependent variables; second, a conditional logit model for 1998-99 that estimates the statistical probability of a project locating in a state. The share of pro-worker amendments (and in an alternative specification, the severance pay dummy) and the lockout variables emerge as significant deterrents to project location, as does urban inequality (perhaps an indicator of social tensions).

The Sanyal-Menon paper suffers from several limitations. First, while using the share of pro-worker amendments may have a certain logic, it has the unfortunate consequence that states

that made only pro-employer amendments get the same score of zero as those that made no amendments at all. Second, since the authors do not report their coding of the individual amendments, it is not clear which ones they classify as granting workers the right to strike or severance pay. These rights were recognised in the central IDA; state amendments would at best have modified them. Third, the concluding summary (p.848) claims that the results from the logit model shows that a higher incidence of strikes discourages investment, but the relevant tables (5-7) do not report any coefficient for this variable; it appears only in Table 4 which reports the results of the panel regressions, where it is insignificant. And finally, according to their summary statistics reported in Table A2, U.P. which recorded no amendments in the BB tabulation is shown as having a 100 per cent share of pro-worker amendments, and along with Andhra Pradesh and Bihar it is shown as having absolutely no unionised industrial workers in 1998-99!

All the papers reviewed above analyze the period after 1989 (in the case of Sanyal and Menon, the late 1990s), whereas the BB classification is based on IDA amendments up to 1989. They are therefore analyzing performance in the 1990s with reference to a classification of states based on their labour laws as they evolved before 1990. In several cases, as I showed in Section 3.2 above, this classification is based on a single inconsequential amendment at any time in the preceding 40 years. And as Section 5 will show, the de facto regulatory regime has changed quite substantially since the 1980s, even without any de jure changes in the IDA.

4.2 Studies Using Annual Values of the Index

Two very recent studies use annual values of the BB index so as to exploit variation within states over time, and also improve upon the original BB paper in some ways. These studies do, however, give rise to disquiet on other grounds.

As noted above, interstate distribution of industrial licences is an omitted variable in Besley and Burgess (2004). In a recent paper Aghion et al (2006), with Burgess as one of the co-

authors, analyze the impact of industrial delicensing at the national level on performance at the three-digit state-industry level. They capture delicensing with a dummy variable whose value for each industry switches from zero to one from the year in which it was delicensed. Their results (for the period 1980-97) show that delicensing by itself has a significant effect on the number of factories, but not on output or employment. But interesting results emerge when the delicensing dummy is interacted with the BB index: output responds positively to delicensing in pro-employer states, but negatively in pro-worker states, explaining the weak overall effect on average. These results are robust to the inclusion of state and industry time trends and political controls. Further results show that employment, entry (measured by the number of factories) and fixed capital investment are higher in pro-employer states and also respond more favourably to delicensing than in pro-worker states; employment actually falls in the latter.

Apart from these estimations with yearly observations of the BB index, Aghion et al obtain similar results in other regressions taking its value for each state in the year preceding each of two delicensing ‘waves’ (1985-90 and 1991-97), and others in which states are divided into three or five groups based on its average level over the entire period. But this suggests that the results, like those of the cross-section studies reviewed in the preceding subsection, are being driven by time-invariant differences between the states. Nine episodes of state amendments occurred in the years 1980-84 (many of them of questionable significance, as I showed in Section 3.2), four in the next five years, and none in the 1990s. Since delicensing began only in 1985, the inter-state differences in the IDA were largely inherited from the pre-reform period.²⁰

Aghion et al also investigate the consequences of trade liberalization, for which they employ a measure of tariff protection for each industry in each year. This too is shown to have had only weak average effects on industry-level output across all states because of offsetting

²⁰ Consistent with this are some interesting results reported by Rodrik and Subramanian (2005, p.222-23). Using state-level data for 1960-2000, they regress annual growth rates of per capita state domestic product and non-agricultural output on the average BB index for each decade, with and without other controls. They find that only in the 1980s did the index have a significant (negative) impact on growth.

expansions and contractions. But interacting it with the BB index shows that tariff reduction led to significant output expansion in pro-employer states relative to the others.

The authors strengthen the credibility of their results by performing ‘falsification exercises’. These involve Monte Carlo simulations with ‘delicensing’ taking place in a randomly generated year for each industry, repeated 1000 times. The interaction term involving the BB index and the actual delicensing variable outperformed its counterpart involving the randomly-generated one. Another exercise involves hypothetically assuming that delicensing in an industry occurred three years before it actually did. Again, the coefficient on the interaction term involving the real delicensing variable remains statistically significant, while the spurious one turns out to be insignificant.

While these falsification exercises with the delicensing indicator are exemplary, one wishes that Aghion et al had done the same with the BB index itself, which is central to their results. In light of the evidence that the differential response to delicensing seems to be largely attributable to inherited inter-state differences in this indicator, such an exercise would have shown whether amendments had any effect within states during the sample period. Further, Aghion et al do not use any controls related to economic conditions in each state (even the ones of doubtful relevance that Besley and Burgess used, listed in n.6 above). They do use state-industry fixed effects, but these would pick up only time-invariant influences. The question of robustness, therefore, still remains open. The other criticisms advanced above in respect of the accuracy of the BB index itself, and the absence of dynamics, also remain relevant.

Even more recently, Ahsan and Pagés (2006) have recoded the state-level IDA amendments tabulated by BB along two specific dimensions: their effects on the ability to initiate or sustain industrial disputes (D), and on the ease of labour adjustment by employers (A). They further distinguish amendments extending Chapter V-B to smaller firms. The BB aggregation and cumulation methods are used to construct separate indices for each of these, and it turns out that the BB index is highly correlated with the cumulated D index, but not with the other two. Each of

the Ahsan-Pagés indices is then used along with control variables (state budget deficits, population and development expenditure) to explain trends in the same range of performance variables as the original BB paper, again at both state as well as state-industry level.

The results of the Ahsan-Pagés paper are qualitatively similar to BB's, with the interesting additional insight that amendments that increased the costs of settling disputes along the D dimension had a more harmful impact than those that reduced labour flexibility along the A dimension – but the amendments extending the scope of Chapter V-B had extremely adverse effects. The authors also recognize the growing dissonance between de facto and de jure regulation by including the share of contract labour in each state as an additional explanatory variable. Since this is available only since 1985, the exercise is carried out for 1985-97. It shows that increasing use of contract labour ameliorated the adverse impact of regulations on output, but not on employment.

Since Ahsan and Pagés use the BB summary of amendments as well as their aggregation and cumulation methodologies, they carry over the errors of the original study. This is particularly serious in the case of state amendments affecting the coverage of Chapter V-B, since there were only five such amendments in all, three of which I have shown to be miscoded because they would at best have had a transient effect relative to other states, given that the same change in Section 25-K came into effect nationwide shortly afterwards. In the case of the BB paper, I acknowledged that by itself this miscoding would not alter the BB index because these three state amendments formed part of larger packages which might justify the pro-worker scores assigned to the respective state-years. But for Ahsan and Pagés, who single out amendments to Chapter V-B for a separate index, the matter is more serious, because under the cumulation rule these amendments *permanently* elevate the state's score on this count. As a separate issue, it is also difficult to understand how they have coded several other amendments affecting procedural matters, union recognition etc. (described in the preceding section) along the 'D' dimension.

These scores must be regarded as extremely subjective. Robustness of the estimates also remains a concern.

Even if, despite all these reservations, we take the results of the entire body of research discussed in this section at face value, deregulation of the labour market may be a mixed blessing. It may promote investment, output, employment and labour productivity in organised manufacturing, but not wages or total factor productivity. Consequently, it will worsen the distribution of value added between capital and labour. The Hasan et al paper shows that it will also increase instability in employment and earnings.

5. Other evidence

Can the actual functioning of labour markets be inferred only from changes in the *de jure* legal framework, and that too from the amendments of just one out of some 45 central laws that affect industrial relations? There have been no further state-level amendments of the IDA since 1989, at least until 2002. There have also been no further central amendments after 1984, except for a minor one in 1996 that designated the central government as the appropriate authority for the air transport industry and two public sector undertakings. If one were to adhere strictly to the BB methodology, one would conclude that there has been *no* change in the regulatory environment affecting the labour market since 1989, and that their classification of states remains valid. I now present a slew of evidence that the reality is very different.

Nagaraj (2004) and Anant et al (2006, pp.252-53) provide compelling evidence of a steady decline since the early 1980s in the number of strikes, the number of workers involved, and the number of person-days lost (although there seems to have been some increase in the latter in recent years). Figure 5.10 in Anant et al (p.253) shows quite clearly that the number of person-days lost to strikes has remained below the number lost to lockouts since the mid-1980s. Thus, BB's categorical statement, couched in the present tense, that "there *are* twice as many workdays

lost to strikes than to lockouts” (p.99 n.9, emphasis added) is simply wrong. The link between pro-worker amendments and labour unrest, which plays an important role in their paper, seems to have been broken.

Several recent studies have shown that during the 1990s, real wages in organised manufacturing were almost stagnant on average, and actually declined in many industries; the growth rate of product wages decelerated; and both wage series lagged far behind labour productivity, whose growth accelerated. Consequently, the wage share of value added declined.²¹ The increase in labour demand elasticities revealed by the Hasan et al study (2003), summarized above, also indicates a substantial weakening of labour’s bargaining power and greater flexibility even in the states classified as having inflexible labour laws.

Anant et al (2006, p.246), Debroy (2005, p.60), Hazra (2005, pp.153-54) and Sharma (2006) describe several ways in which the apparently pro-labour clauses of the IDA are of little help to workers. Employers can dispute whether the worker was actually a worker as defined in the Act, and whether s/he was in continuous service for the 240 days required to benefit from its protective clauses. There are long delays in adjudication, for which many workers do not have the staying power. The ‘competent authority’ can be manipulated to delay the grant of permission for retrenchments and closures, since after sixty days it is deemed to have been granted. And only nominal fines are levied on employers who do not abide by the law. Increasing recourse to contract and casual labour, which are not covered by the IDA, is further weakening its protection. Employees are often coerced into accepting ‘voluntary’ retirement schemes. Shyam Sundar (2006) summarizes a monograph that studies over 200 collective bargaining agreements signed during 1991-2001, most of which contained clauses giving management considerable flexibility in setting work norms, wages, and employment. According to a World Bank-CII survey, although Indian employers find labour regulations far more of an obstacle to growth than employers in

²¹ Balakrishnan and Babu (2003), Nagaraj (2004), Banerjee (2005), Ghose (2005), Goldar and Banga (2005). Sen and Dasgupta (2006) show that these trends have continued upto 2002-03.

most other countries, firms employing more than 100 workers reported corruption, tax rates and tax administration as more serious constraints, and power problems as equally serious; for smaller firms, policy uncertainty and the cost of finance were also ranked ahead of labour regulations (Ahsan and Pagés, 2006, Figures 1 and 2).

Apart from these ground-level realities, changing judicial interpretations of the IDA further complicate the measurement of its effect on industrial performance. India follows a common-law system, and apart from deciding on the constitutional validity of a law (as discussed in Section 2.2. above), the higher judiciary can bring about substantial changes in the way it is enforced, with no change in the statute itself. In several recent judgments, the Supreme Court has quite consciously initiated a change of course in its interpretation of the IDA, stating that its earlier verdicts were excessively pro-worker, and that a more ‘balanced’ approach is required in an era of market-oriented reforms.²² The full consequences of this new thinking are still unfolding.

As for the inter-state variation in enforcement, according to one study, in 2001 “the largest number of workers affected through closures was in UP, Gujarat and Kerala ... through retrenchments was in Gujarat, Manipur and Goa ... [and] through layoffs was in Pondicherry, Kerala and UP.... We have subjective notions of which are reform-minded states and which are not. But if you look at the names of States just mentioned, permissions seem to have been granted in a variety of states. Are we unnecessarily excited about something that is not much of an issue?” (Debroy, 2005, pp.59-60). It is of course possible that despite these rankings, Kerala may still be ‘pro-worker’, in that its employers are forced to seek permission for what is done covertly in other states, or that permission is actually denied for applications affecting a much larger number of workers, but this only reinforces my point that a state’s labour policy cannot be directly

²² *State of U.P. vs Jai Bir Singh*, (2005) 5 SCC 1; *Allahabad Jal Sansthan vs Daya Shankar Rai*, (2005) 5 SCC 124; *U.P. State Brassware Corporation vs Udai Narain Pandey*, (2006) 1 SCC 479; and *Hombe Gowda Educational Trust vs State of Karnataka*, (2006) 1 SCC 430, which cites several other recent

inferred from its labour laws. Debroy goes so far as to state that the low score that India gets on international rankings of labour flexibility “is probably largely perceptual rather than real”.

Some additional comment is required on the *de facto* situation in states that Besley and Burgess, and the authors who follow them, designate as pro-worker. West Bengal of course gets the highest pro-worker score on the BB index, and one would expect that it would have a high incidence of strikes. In fact, since the 1960s it has been the state with the highest incidence of lockouts. Although the reason offered by employers for lockouts was usually “indiscipline and violence”, the reasons reported by conciliation officers were usually financial difficulties and management’s intention to reduce employment. Thus, lockouts were often “closures in disguise” (Shyam Sundar, 2004). A study cited by Anant et al (2006, p.251) found that in West Bengal most lockouts ended with workers ultimately agreeing to the downsizing of the workforce, without interference by the government. Even in the early 1980s, after a spate of pro-worker IDA amendments, a contemporary observer (Roy, 1984) noted that the incidence of strikes had fallen sharply, while that of lockouts had increased, so that the latter had come to dominate the former, unlike the situation then prevailing at the national level. (As I pointed out above, the national situation was also reversed a few years later.)

In Maharashtra, even as supposedly ‘pro-worker’ amendments to the IDA were being passed in the early 1980s, the textile workers’ strike was brutally suppressed, and following the mass retrenchment of employees who tried to form a union in Reliance Textile Industries, the state government refused, despite repeated High Court orders, to refer the matter to a labour court as required under Section 12(5) of the IDA (Singh, 1986).

As for Gujarat, research based on fieldwork enables us see how misleading a classification of states on the basis of their labour laws can be. Streefkerk (2001), after revisiting in 1998 the area of south Gujarat where he had undertaken fieldwork in 1974, noticed greater casualisation,

rulings. A legal scholar who has critically commented on some of these judgments has expressed concern about the Court’s “overzealous process” of ensuring workplace discipline: see Kaul (2006).

feminisation, and the use of contract labour in industry. Interestingly he sees these as a continuation of long-term trends that were legitimised by the reforms of the 1990s, not caused by them. In both 1974 and 1998, factory inspectors were indifferent to their responsibilities, and routinely paid off by employers. “The retreat of [the] Indian state does not apply to most of the industrial workers in Valsad region.... For *many workers the state did not, and still does not exist*” (Streefkerk, 2001, p.2404, emphasis added). Another scholar who has spent most of his professional life closely observing the labour scenario in Gujarat has documented the inability of the thousands of workers who lost their jobs in the collapse of Ahmedabad’s textile factories in the 1980s and 1990s to receive their statutory benefits (Breman, 2004, especially pp.160-69).

Thus, the evidence regarding actual labour market conditions is very different from that suggested by the BB index. Interestingly, the recent *India Labour Report* (TeamLease Services, 2006), using a different coding of state-level amendments to the IDA along with several direct indicators of enforcement and industrial disputes, ranks the ‘labour law ecosystem’ in Maharashtra, Karnataka, Punjab and Gujarat, in that order, as being the most conducive to employment generation. In the BB study, these states were respectively categorized as pro-worker, pro-employer, neutral and pro-worker.

Our scepticism about the BB approach is only strengthened if we now turn to evidence of recent trends in employment. In a large-scale study covering 1300 firms, Deshpande et al (2004) looked at labour flexibility and employment in nine industries in ten states over the period 1991-98.²³ They found that most firms had increased employment during this period, but more than a quarter had decreased it. Although firms employing less than 100 workers increased their employment more than larger firms, possibly indicating a threshold effect of Chapter V-B, the difference was insignificant for manual workers. On the other hand, as compared to these smaller firms, a larger proportion of the bigger firms reported a decrease in employment, and this

²³ The results are summarized by Deshpande (2001), Shyam Sundar (2005) and Sharma (2006).

proportion increased with the size of firm. The inference was that these firms were not being deterred from hiring and firing, despite the IDA provisions remaining undiluted.

Nagaraj (2004, p.3388) argues that “Although the labour laws remained the same, their enforcement was diluted or government ignored their evasion by employers. In effect, it was reform by stealth”. He shows that “between 1995-96 and 2000-01, about 1.1 million workers, or 15 per cent of workers in the organised manufacturing sector lost their jobs. These losses have been widespread across major states and industry groups” (p.3390). This fall in employment was especially steep after 1998, and occurred despite a secular decline in the wage/rental ratio. At the level of individual states, Bagchi and Das (2005, pp.953-54) show that there was a significant *increase* in employment in organised manufacturing after the mid-1990s in both Gujarat and West Bengal. This occurred without any *de jure* liberalization of their supposedly ‘pro-worker’ labour laws.

6. Concluding Remarks

This paper has surveyed two distinct analytical approaches to the question of what effects changes in the Industrial Disputes Act have had on manufacturing performance in India. The first approach, which tries to trace the impact of the central amendments of 1976 and 1982/84 on employment, gives conflicting results. I pointed out, however, that the State of Emergency under which the earlier amendment was implemented, the prolonged litigation that followed both amendments, and the other 1984 amendment that excluded contractual employment from the retrenchment rules, ensured that these two dates cannot be seen as decisive structural breaks.

The second approach, which exploits variation in state-level amendments to the IDA, was shown to be based on a flawed index of regulation. It is not at all clear what exactly the Besley-Burgess ‘regulatory measure’ actually measures: it seems to capture, for the early 1980s, the inter-state variation in some amalgam of labour regulation, industrial relations, and the investment climate. While we have learned to appreciate from Banerjee and Iyer (2005) that

institutions can have an effect on economic performance long after they have been modified or discarded, the specific transmission mechanism through which BB envisage pro-worker legislation influencing manufacturing performance – industrial unrest – does not seem to be operating in recent years. Several papers in this genre also suffer from various other methodological shortcomings, particularly their inadequate tests for robustness.

All the studies reviewed in this paper have heroically attempted to quantify changes in ‘black-letter’ labour law, but I have tried to show that this can be misleading, because the impact of laws passed by the legislature is mediated by both the executive and the judiciary. To construct an index of the degree to which the workers’ interests are enhanced by the IDA alone, one would need to look at the variation between states and over time in the proportion of disputes referred by state governments for adjudication; of pro-worker decisions by state labour courts, tribunals, and High Courts; and of applications for permission for layoff, retrenchment, closure, and prohibition of casual labour that were denied. Clearly, this is a Herculean task.

A few qualifications are also necessary. I have picked several holes in studies that try to establish a case for labour market reform. But this does not mean that labour flexibility is unimportant and reforms unnecessary. Indian labour laws do need to be reformed, if only to rationalise them, eliminate inconsistencies, and make compliance less onerous, as suggested by the three surveys cited in Section 1 above. Debroy (2005, p.58) argues that the real problem lies in the multiplicity of labour laws, often with conflicting definitions of the same terms, and the inspector raj that it creates. He argues that “by equating labour market reforms with the ‘hire and fire’ of IDA, we therefore do a disservice to the cause of labour market flexibility”.

At the same time, it must be acknowledged that downsizing and closure of firms are a fact of life in a market economy. But deliberate non-enforcement of labour laws (or “reform by stealth”, to use Nagaraj’s phrase) without instituting adequate social protection mechanisms or retraining facilities is hardly the way to deal with the problem. A pattern of trade liberalisation that deflects the costs of adjustment from the powerful to the powerless has made things worse

(Bhattacharjea, 2005). And although there are several *theoretical* (even common-sense) arguments in favour of greater labour flexibility, there are also some in favour of restrictions on flexibility (on grounds of economic efficiency, not just concern for workers).²⁴ This paper, however, has been confined to the *empirical* evidence on the relationship between labour regulation and industrial performance, and has shown that it is very fragile. In that sense, I have been more concerned with academic standards than labour standards.

I began this paper with an excerpt from the government's *Economic Survey*, and it is appropriate to end with it. The paragraph that I quoted should not be read in isolation, for its context is important. It follows a candid summary of the very disturbing rise in unemployment rates between 1993-94 and 2004, revealed by the 60th Round National Sample Survey. This rise has occurred in both urban and rural areas, but especially the latter, where unemployment rates on the basis of daily or weekly status have risen by more than fifty per cent (Ministry of Finance, 2006, p.209). The fact that the *Survey* then proceeds immediately to complain about the inflexibility of labour markets in the organised sector (in the paragraph quoted at the beginning of this paper), and then in the very next paragraph to an admiring account of China's drastic labour market reforms, strongly suggests that its authors see similar reforms as the panacea for India's growing employment crisis.²⁵ It may come as a surprise to them that China's score is distinctly *worse* than India's in the CII-World Bank survey of employers' perceptions of the degree to which labour laws are regarded as an obstacle to growth (Ahsan and Pagés, 2006, Figure 2). But quite apart from questions about the reality, replicability and desirability of the Chinese model, and the evidence summarized in Section 5 above of growing flexibility in India despite supposedly restrictive labour laws, it needs to be pointed out that less than eight million workers out of the labour force of more than 400 million are employed in organised manufacturing, with

²⁴ The neoclassical literature is based on efficiency-enhancing risk sharing and commitment in labour contracts. Some of these arguments are summarized in Shyam Sundar (2005); see also D'Souza (2005).

an even smaller proportion in units covered by Chapter V-B. Even if labour flexibility results in a dramatic increase in employment in this sector, it is hardly going to make a dent in the national unemployment situation.

²⁵ The recent draft Approach Paper for the Twelfth Plan makes the same linkage in its repeated calls for labour market flexibility (which it tries to distinguish, not very successfully, from ‘hire and fire’): see Planning Commission (2006), pp.25, 62, 79.

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