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A (time) series of unfortunate events: structural change, globalization, and the rise of occupational injuries

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Abstract: There is a dearth of evidence on the evolution of occupational health in the developing world and on the extent to which it has been influenced by (1) the pattern of structural transformation in these economies and (2) integration with global markets. In this study, I compile a rich database on workplace injuries in India covering a five-decade period. I use these data to examine trends in the rate of occupational injuries and show that the rate of occupational injuries began trending sharply upwards starting in the 1990s. This phenomenon does not appear to reflect sectoral shifts in employment arising from structural transformation but rather increases in injury rates in non-agricultural sector. By combining disaggregated industry-level injury data with quasi-experimental variation in exposure to key policy reforms, I find that the uptick in injury rates can at least partially be explained as a result of changes in the policy landscape in the 1990s that exposed Indian manufacturing to international markets.

Key words: structural change, occupational health, workplace safety, globalization, India

JEL classification: J28, J81, O14, O10

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Note: tables and figures at the end of the paper

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1 Introduction

On any given day, an estimated 5,000 people across Asia die as a result of work-related accidents and diseases, and a further 0.73 million suffer non-fatal occupational injuries (Hämäläinen et al. 2017).[1] An effective policy response to this public health crisis rests on an understanding of the complex of factors that shape the phenomenon of unsafe work, including the regulatory environment and changes in economic conditions. Among the latter, two are arguably particularly relevant to the development experience of a number of countries in the last few decades and are the focus of this paper: structural transformation, and increased exposure to global market forces.

Structural change, or the shift in the inter-sectoral pattern of employment and production, was identified by Kuznets (1966) as one of the central features of economic growth, and has since been studied extensively (e.g., Chenery and Syrquin 1975; Duarte and Restuccia 2010; Herrendorf et al. 2014; McMillan et al. 2014). Economic studies of structural transformation typically focus on its impacts on productivity and living standards. But structural change also has a potentially profound impact on another aspect of worker well-being: occupational health. As employment shifts from traditional to modern occupations, workers become exposed to newer kinds of (and in some cases, substantially greater levels of) occupational health risk, most obviously in the relatively mechanized manufacturing sector,[2,3] but also in hazardous sectors such as construction. At the same time, structural change is also usually concomitant with the adoption of productivity-enhancing innovations in the agricultural sector such as farm machinery and pesticides, which exposes workers remaining in the agricultural sector to new work hazards.[4,5] Understanding how this process is playing out in developing countries is of obvious policy and academic interest, especially when one considers the historically atypical structural transformation exhibited by developing countries like India and Nigeria (Amirapu and Subramaniam 2015; Rodrik 2016; Sen 2019), in which flows of employment out of agriculture are bypassing the manufacturing sector.

The second notable change in the economic environment in developing countries is that their economies have steadily become more closely integrated into global markets. The impact of globalization of production on occupational health in the developing world has been a prominent concern among scholars and international labour-rights advocates (see, for example, Frumkin 1999; Loewenson 2001; Brown 2002; Hämäläinen 2009; ILO 2008). The concern here is two-fold. First, offshoring of production by developed countries is shifting the locus of hazardous work to countries with weaker labour regulations (see, for example, Loomis 2015 for a wide-ranging discussion). Second, pressures to maintain competitiveness in international markets could weaken the incentives of firms to invest in workplace

[1] When viewed in perspective, therefore, prominent workplace accidents such as the Rana Plaza fire in Bangladesh in 2013 that resulted in the loss of 1134 lives, and the Tianjin warehouse explosion in China in 2015 that killed 173, are dwarfed by the scale of the day-to-day toll of work in these countries.

[2] As Bowers (1930) (excerpted in Pursell 2001) observed, 'The very nature of modern production demands the sacrifice of hands, legs, eyes, arms and even lives of workers. There are punch presses which smash fingers; metal shears which cut off hands; calendering machines which tear off arms; giant rollers which smash bodies; falls which break legs; cave-ins which wrench backs; flying dust which blinds eyes. These are the concomitants of production.'

[3] In this context, Blattman and Dercon's (2018) experiment of transplanting traditional sector workers into factory jobs is interesting, *inter alia*, for the light it sheds on occupational health as one of the defining differences (from the workers' perspective) between the two kinds of occupations.

[4] See, for example, Yanggen et al. (2003) on the health effects of incorrect pesticide use in Ecuador, and Tiwari et al. (2002) on accidents due to mechanization in rural India.

[5] The co-evolution of occupational health with structural change can be considered an aspect of a broader 'epidemiological transition', in which '...pandemics of infection are gradually displaced by degenerative and man-made diseases as the chief form of morbidity and primary cause of death' (Omran 1971).

safety. This hypothesis is both supported by a well-established negative relationship between competitive pressures and behaviours that impact on workplace safety (e.g. Rasmussen 1997),[6] as well as by empirical evidence that both exposure to import competition as well as participation in export markets result in increased rates of occupational injury (Hummels et al. 2016; McManus and Schaur 2016).[7] A related conjecture is that exposure to (and subsequent adoption of) unfamiliar modes and technologies of production can engender new workplace hazards. The corresponding effects of these factors on occupational health in the developing world, however, remain unknown and are potentially severe, given the lack of awareness about and enforcement of workplace safety regulations.

Despite the long-standing policy interest around these questions, there is a serious dearth of analysis of long-term trends in occupational health in developing countries and their associated determinants.[8] This evidence gap is in large part due to a lack of fine-grained and reliable data covering a long time span. This study aims to fill the gap by constructing and analysing historical trends in occupational injury rates in India. To do so, I first digitize data on workplace fatalities from three distinct sources of administrative data: (1) mandatory employer filings on injuries and fatalities, (2) police reports on fatal accidents in the workplace, and (3) records on compensation paid for workplace injuries. The first two sources of data pertain to accidents in the manufacturing sector, and the third pertains to a broader set of non-agricultural sectors.

A well-known limitation of these kinds of data, however, is that the published national data series (such as those compiled in the ILOSTAT cross-country database on occupational injury) mask a significant level of non-compliance with statutory reporting requirements at the level of the reporting entities (e.g. employers in the first instance, but also local authorities that collate information that is then reported up to central data repositories). Because the degree of non-reporting is not routinely specified alongside the aggregate data, the resulting time series are virtually impossible to interpret reliably. By instead utilizing the full breadth of the data provided by the official government agencies, I am able to (i) examine injury rates at the far more disaggregated level of industries and states, (ii) establish the extent and nature of non-reporting, and (iii) implement procedures to correct for the biases resulting from non-reporting.

A related limitation of injury data derived from official reports is that both employers and employees may have their own incentives to under-report accidents, and the pattern of misreporting may plausibly be influenced by the very economic phenomena that one is attempting to study (see for instance Boone and van Ours (2006) for a demonstration that reporting of workplace accidents is influenced by the business cycle, undermining the validity of studies that attempt to establish the causal effect of the latter). A major advantage of my study is that I am able to supplement the data sources based

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[6] Relatedly, the literature on organizational behaviour and safety has shown that investing in workplace safety lowers the odds of firm survival (Pagell et al. 2020).

[7] See, however, Tanaka (2020) for evidence that an increase in access to export markets improved compliance with health and safety regulations among apparel firms in Myanmar.

[8] The existing literature on occupational health and workplace safety is large and multidisciplinary (encompassing a diverse set of fields including law, engineering, applied psychology, organizational behaviour, epidemiology, and political science) and, as a result, methodologically and thematically diffuse, and it is not possible to provide a comprehensive survey here. Broadly speaking, however, one can study workplace safety in terms of five causative factors: (1) human psychology, and how it affects workers' behaviours, (2) organizational behaviour and management practices, (3) technology and its interaction with human behaviour, (4) labour law, encompassing occupational safety and health regulations, injury compensation mechanisms, as well as general labour market institutions, and (5) general economic climate. The majority of studies on the effect of the economic climate has been focused on developed countries, and consequently tend to concentrate on business cycles (e.g., Kossoris 1938; Robinson 1988; Nichols 1989; Fabiano et al. 1995; Boone and van Ours 2006; Davies et al. 2009; Asfaw et al. 2011). Barring some cross-country studies (which I dicuss later), I am not aware of any comparable research for developing countries.

on official reports with data from a national household survey on disability, which provides information on workplace accidents in each of a set of broad sectors of the economy (including agriculture, mining, and manufacturing). Given the nature and household setting of the survey, the elicited information on workplace injuries is not subject to biases arising from strategic under-reporting. A further innovation of my study is that I am able to utilize survey information on the date of each accident to construct retrospective time series on occupational injuries covering all sectors of the economy. Last, but not least, I provide a careful discussion of the statistical biases that can arise when constructing retrospective injury rates in this way, and propose and implement a simple procedure to correct for these biases.

The combined database on occupational injuries is virtually unique in the context of developing countries in that it (a) has national coverage, (b) covers a long time span of nearly 50 years, and (c) allows for multiple levels of disaggregation, which is essential for an analysis of the effects of different kinds of factors and influences.

The study spans the period 1970–2016, which covers most of the post-independence era in India. During the second half of this period, the already ongoing process of structural transformation was juxtaposed against a dramatic policy shift that altered the external orientation of the economy. An examination of trends in occupational injury rates reveals that they started to climb sharply in the aftermath of the large-scale economic liberalization reforms that began in 1991. I shed further light on this phenomenon by disaggregating trends by sectors, and further by implementing a shift-share decomposition in order to disentangle the aggregate contribution of structural shifts in employment from the contribution of within-sector changes in injury risk. This exercise reveals (a) a modest effect of structural change on the overall rate of injuries, and (b) that the shift in the trend of occupational injuries in the 1990s was concentrated in the non-agricultural sector and was not a result of compositional shift across sectors.

The timing of the trend break in overall injury rates suggests a causal link to the economic liberalization policies of 1991. To investigate this hypothesis further, I undertake a finer analysis of changes in fatality rates within the manufacturing sector using more disaggregated data that allows me to look at fatality rates by industry. A simple decomposition reveals that the sharp increase in fatality rates in the manufacturing sector after 1991 was driven by substantial within-industry increases in fatal injury rates. I then undertake a regression analysis that takes advantage of quasi-experimental variation over time and across manufacturing industries in exposure to the different aspects of liberalization reform, and find robust evidence of a causal link between exposure to foreign direct investment and industry-level rates of fatality.

The study makes three key contributions. First, by marshalling data on occupational injuries over a long period, it makes a significant contribution relative to an existing literature (e.g., Hämäläinen 2009; Li et al. 2020) that has typically focused on changes over small time periods or groupings of heterogeneous countries, and/or has had to impute long-term trends from less-specific data on injuries (in combination with assumptions on the fraction of injuries attributable to work-related causes). An equally important contribution is that commonly available sources of data on workplace injuries in developing countries (e.g. cross-country data collated by the International Labour Organization [ILO]) suffer from a number of well-known biases which are usually acknowledged but left unaddressed in existing studies. A particular strength of the analysis in this paper is that it draws on multiple sources of data in order to provide cross-validation of the results, and moreover does so at a level of detail that allows us to examine and account for the biases inherent in each source. By doing so, the present study provides not only a new database but also a new set of procedures for credibly analysing such data that should be valuable for future research in this area.

Second, the study provides the first rigorous examination of the effects of structural transformation on occupational health. A distinct, but related, literature has examined the link between sectoral shifts in employment and workplace injury rates, but in the limited context of the decline in the rate of injuries in the United States in the 1990s (Loomis et al. 2004; Morse et al. 2009; Ruser 2014). I am not aware of any comparable analysis focusing on the health and safety implications of the changing structure of employment in developing economies.

Third, the study provides the first credible empirical analysis of the effects of globalization of production on workplace injuries in the developing world. The existing empirical literature consists of a handful of cross-country studies (Kerrissey and Schuhrke 2016; Blanton and Peksen 2017; Stackhouse et al. 2019) examining the effect of globalization on workplace fatalities. These studies do not rely on quasi-experimental policy changes and utilize composite measures of openness that are difficult to interpret. Accordingly, the findings of this literature are somewhat mixed: Stackhouse et al. (2019) find that a globalization index (but not foreign direct investment) correlates positively with worker injuries, while Blanton and Peksen (2017) find that the policy 'posture' of governments is more important than actual flows of trade and investments, and Kerrissey and Schuhrke (2016) find that labour institutions are important determinants of injury rates but fail to find any effect of measures of globalization. In contrast, the exogeneity of the liberalization reforms in India, in conjunction with detailed information on different aspects of economic openness, allows me to draw more precise conclusions.

The paper proceeds as follows. Section 2 provides background context for the study in terms of an overview of the state of occupational health and safety in India, along with an outline of the economic landscape. Section 3 describes the data sources used in the paper; Section 4 discusses the construction of injury rates; Section 5 discusses the estimated trends in occupational injury rates; Section 6 delves deeper into analysing the causes of the increase in injury rates in the manufacturing sector; and Section 7 concludes.

2 Context

2.1 Occupational safety and health legislation in India

During the period under study (and until as recently as 2022), occupational safety in India was governed by a patchwork of legislations. The Factories Act, which governs formal manufacturing establishments employing more than 10 workers, contains a number of provisions regarding workplace safety and inspections. Three other separate pieces of legislation—the Mines Act, the Building and Other Construction Workers Act, and the Dock Workers Act—specifically govern workplace safety in the respective occupations.

Because labour regulation in India is under the joint purview of federal and state governments, the enforcement of the Acts is also divided up between the central Directorate General, Factory Advice Service and Labour Institutes (DGFASLI), and state-level inspectorates. In addition to the fact that the infrastructure for enforcement of the workplace safety regime is poor (Ramachandran and Sigamani 2014), there is a major hole in coverage of these pieces of legislation corresponding to the agricultural and informal sectors of the economy: taken together, these pieces of legislation cover less than 10% of the workforce (Pingle 2012).

Alongside occupational safety regulations, there are two important pieces of legislation that govern compensation for workplace accidents. The Employees' State Insurance (ESI) Act requires that factories employing more than ten workers must participate in a disability insurance scheme (into which both employers and employees pay premiums).[9] The Workmen's Compensation (WC) Act holds employers liable for compensation for work-related injuries and applies to all workers in a broad set of sectors including mining, manufacturing, ports, railways, and construction, with the exception that workers covered by the ESI scheme are not eligible for additional compensation under the WC Act.

2.2 Occupational hazards and rates of occupational injury

Reliable statistics on occupational injuries in India are hard to come by given that the vast majority of the workforce is not covered by reporting requirements. The incidence of occupational diseases is even more difficult to estimate, given the lack of knowledge about as well as the long latency period of such diseases.[10] The World Health Organization (WHO) and the International Labour Organization (ILO) estimate the incidence of deaths in India due to occupational disease to be 33 per 100,000 workers in 2016,[11] and the incidence of fatal occupational injuries to be eight per 100,000 workers (WHO/ILO 2021), but these estimates are obviously subject to the caveats noted above.

Occupational hazards in India vary by sector of employment and type of employment (formal or informal). In agriculture, heat and physical stresses of manual labour have traditionally been the main hazards, but the introduction of large agricultural machinery in the form of threshers and tractors, as well as pesticides, have introduced a new set of hazards into the workplace (e.g., Mohan and Patel 1992; Nag and Nag 2004). In the agricultural sector, the best available estimates suggest a fatality rate of 18.4 deaths per 100,000 workers due to injuries (Gite et al. 2010), which is comparable to the fatality rate in the agricultural sector in the United States (which however has a far greater degree of mechanization).

Outside agriculture, the three sectors that account for the bulk of employment in India are retail, construction, and manufacturing, of which the latter two are prominently associated with exposure to work hazards. In construction, the most common risks are falling from heights and being hit by falling objects—problems that are exacerbated by reliance on manpower rather than machinery, inappropriate use (or lack of use) of scaffolds, and under-utilization of protective equipment (Ajith et al. 2019). Of the more than 60 million people (as of 2020) employed in the construction sector, the vast majority are informally employed, and approximately a third are inter-state migrant workers, many of whom are seasonal migrants from the agrarian sector (Roy et al. 2017). Given this profile of workers, most injuries are thought to go unreported: Patel and Jha (2016) use a novel methodology to estimate that the rate of fatal injury among construction workers in India is 22 per 100,000 workers, which is more than twice as high as fatal injury rates in the United States' construction sector.

In the manufacturing sector, injuries caused by inappropriate or unsafe use of machinery are common, and the problem is likely to be exacerbated in the informal manufacturing sector which is entirely unregulated. Statistics for the formal manufacturing sector indicate a fatal injury rate of eight

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[9] Over time, the ESIC has been amended by state and central governments to expand the coverage to establishments in other sectors such as retail, hospitality, and entertainment. Workers paid under a specified monthly wage are covered by the scheme. Medical benefits are provided by a dedicated network of hospitals, and the scheme also pays cash benefits for injuries resulting in loss of earnings.

[10] Silicosis and asbestosis are common among mine workers, bysinosis (a lung disease) among textile workers, and pesticide poisoning among farm workers.

[11] The incidence of deaths due to a given occupational disease is approximated by first estimating (for countries where suitable data are available) the fraction of deaths due to that disease that can be attributed to occupational exposure, and then applying this 'attributable fraction' to estimate the number of work-related deaths due to that disease in other countries.

deaths per 100,000 workers (Ministry of Labour and Employment 2019), compared to about 0.6 per 100,000 workers in Great Britain. However, injury rates in the informal sector (which accounts for approximately 80% of manufacturing employment) are not known.

2.3 Economic background

In this study, I consider the effects of structural change and global market exposure. While agriculture has been the predominant sector in terms of employment, there has been a steady reduction in its employment share, as shown in Figure 1. As Sen (2019) has observed, the pattern of transformation in India is distinct from the conventional view, in that the decline in agricultural employment appears to have been absorbed in the non-manufacturing sector. This process appears to have started in the early 1980s, when agriculture's share of employment was nearly 70%. Since then, the share of agriculture has fallen to less than 60% by 2010, while the other non-manufacturing sector's share has increased to about 35%. Also apparent in the figure is that employment in the manufacturing sector has stagnated throughout the period (even while manufacturing sector output has grown considerably)—a phenomenon of 'jobless growth' that has attracted significant attention in the literature (see, for example, Nagaraj 1994; Dasgupta and Singh 2007; Kannan and Raveendran 2009).

On the policy front, India undertook a series of major economic reforms after facing a balance-of-payments crisis in 1991. These amounted to an almost wholesale repudiation of the inward-looking policy of import substitution and trade controls that India had followed for four decades up to that point. Alongside a set of across-the-board reforms including currency devaluation and liberalization of capital markets, the policy changes of 1991 included three major industry-specific reforms. Relaxation of trade barriers was the most prominent aspect of these and has garnered the majority of attention in the literature. Between 1991 and 1997, tariffs were harmonized and dramatically reduced; the average final goods tariff on manufactured goods fell from 95 to 35% (Harrison et al. 2013; see Panagariya (2005) for a broader discussion of the history of trade policies in India).

Second, the 1991 reforms restarted the process of dismantling the industrial licensing scheme (a process that had begun during the 1980s). Under the 'License Raj', large firms had to obtain licenses, which included restrictions on their output and the types of goods they could produce. About one third of industries had been delicensed in 1985; most of the remaining industries were delicensed during the post-1991 reforms (Aghion et al. 2008).

Third, the 1991 reforms introduced a significant change in foreign direct investment (FDI) policy. Prior to 1991, the extent of foreign ownership could not exceed 40%, and various other restrictions were imposed including restrictions on the use of foreign brand names and restrictions on dividend remittances. After 1991, 'automatic' approval of majority foreign ownership was allowed in a number of industries, and the scope of the liberalization was expanded later in the decade to include most of the remaining industries. At present, FDI is restricted in only a small number of strategic industries (such as defence and energy). The panels of Appendix Figure 1 show that there was a dramatic takeoff in the number of registered foreign companies operating in India as well as the inflow of FDI, in the wake of these reforms.

3 Data

3.1 Data on occupational injuries

This study uses four sources of data on occupational injuries in India. The first is a national household survey on disability, which provides information on disabilities arising from occupational injuries. These are arguably high-quality data, as they are based on a rigorous sampling methodology, have national coverage, and importantly, do not suffer from the biases inherent in official injury reports submitted by employers or employees. A coverage limitation of these data, however, is that they do not capture fatalities.

To complement the disability survey data, I have digitized three sources of administrative data on workplace injuries: (1) employer-provided reports of injuries and fatalities in the manufacturing sector, (2) police reports of fatal accidents in factories, and (3) data pertaining to injury compensation paid to injured workers (or families).

These four distinct data sources are described carefully below.

Household survey data

The National Sample Survey Organization (NSSO) of India has conducted the 'Survey of Disabled Persons' (SDP, henceforth) approximately every ten years. Similar to other surveys conducted by the NSSO, the SDP is a large survey with national coverage that is designed to allow for the calculation of population estimates at various levels of aggregation, including rural/urban sectors within a state. As I explain in Section 4, the retrospective nature of these data allow us to examine trends in accident rates over the most significant period of economic change in India since independence.

The SDP collected detailed information on disabilities arising from worksite injuries. I use data from the two most recent rounds of the SDP, conducted in 2002 and 2018.[12] However, estimates of the incidence of workplace injuries obtained from these two rounds are not strictly comparable, as I will explain shortly.

The 2002 round of the survey interviewed 70,302 households that were identified as having at least one disabled member,[13] where disability was taken as indicating a severe, long-term (i.e. not of recent origin or temporary nature) physical or mental loss of function. Within each household, the survey collected demographic information on all members, as well as details of disability for each disabled member. Whereas the 2002 SDP simply asked households to identify disabled members, the 2018 SDP adopted a more systematic approach to identify disability, based on questions regarding specific functional capabilities corresponding to each type of disability. This is a first reason why disability rates estimated from the two rounds are not exactly comparable (National Statistical Office 2019).

Population estimates constructed from the SDP indicate an overall population rate of disability of approximately 1.8% in 2002. This figure is consistent with estimates obtained from the population census of 2001, which also recorded information on disability in a comparable manner to the SDP (the census figure is approximately 2.1%). It is worth noting that the rate of disability, as recorded

[12] The SDP was also conducted in 1981 and 1992. The 1981 data have not been publicly released, and the publicly available 1992 data are lacking sampling weights which would allow for the construction of national estimates.

[13] Households that had disabled members were first identified by a preliminary survey of all households in each sample village. A random sample of these households was then interviewed in the follow-up (main) survey.

in these data, is extremely low compared to survey rates of disability in the developed world. For comparison, the rate of disability in the UK in 2021 was nearly 18%. The difference is partly due to the younger population in India, but also reflects the difference in the definition of disability.

Identifying workplace injuries. Individuals identified as disabled provided detailed information about their disability, including: (i) type of disability (mental, visual, speech, hearing, or locomotor), (ii) severity, (iii) age and employment status at time of injury, and (iv) place of injury, if the disability was the result of a physical injury or burns.

The information on place of injury allows me to identify workplace injuries, although the two survey rounds differ in the way they elicited this information. Whereas the 2018 SDP simply asked if the underlying injury or burn was sustained in the workplace, in the 2002 SDP respondents were asked whether the injury had occurred in an agricultural field, a mine, a factory, or at another worksite. Because of this discrepancy between the two survey questionnaires, the corresponding rates of workplace injuries are unlikely to be (and indeed, do not appear to be) comparable between the two survey rounds.

I restrict the sample to individuals who were of working age (15–65) and employed at the time of injury (this restriction makes it more likely that we are capturing work-related injuries). I also restrict the sample to exclude injuries reported to have occurred in the survey year or in the preceding year—this is because the survey definition of long-term disability excludes most disabilities that are of recent origin, as a result of which injury rates in each survey and preceding year are very low.

The left panel of Appendix Figure 2 breaks down the composition of workplace injuries by type of resulting disability. The right panel of Appendix Figure 2 shows the distribution of workplace injuries across sites. The overwhelming majority (nearly 90%) of workplace injuries result in locomotor disabilities. Consistent with the overall distribution of the workforce across sectors, agricultural injuries account for slightly over 50% of injuries, while factory injuries account for about 10%, and mine injuries make up about 2%. The catch-all 'other worksite' category accounts for the remaining injuries.

Appendix Table 1 summarizes the characteristics of injured persons in these data. The vast majority (87%) of workplace injuries are associated with males, reflecting the highly-gendered distribution of employment, both in terms of work participation rates outside the household as well as in terms of exposure to physical work hazards. Nearly three quarters of injured individuals reside in rural areas, which is reflective of the dominance of agriculture in employment (and the distribution of injuries by worksite). However, I should note the caveat that place of residence at time of survey is not necessarily the same as place of residence at time of injury (which may have occurred many years ago in a different location), for which reason I do not attempt to disaggregate injury rates by rural/urban location in the analysis. Lastly, the summary statistics indicate that approximately three quarters of work injuries in the sample resulted in the worker changing jobs or losing work, which underlines the restrictiveness of the implicit definition of disability in these data.

Factories Act data

The Factories Act of 1948 requires every registered factory to submit an annual filing with the state's Chief Inspector of Factories, detailing (among other matters such as employment and wages) fatal as well as non-fatal injuries at the factory (any injury that results in worker absence for longer than two days must be reported). These data are then reported up to the central level and published at an aggregate level in a number of publications put out by the Labour Bureau and other governmental agencies (including *Indian Labour Statistics*, *Statistics of Factories*, *Pocketbook of Labour Statistics*, and the *Statistical Abstract of India*), which I have digitized for the period 1971–2014. In what follows, I refer to these data as the Factories Act data.

The Factories Act data on injury rates are published at two separate levels of aggregation: the 2-digit industry level and the state level. These data thus allow for construction of state-level and industry-level panels of occupational injury rates.[14] The state-level panel of injury rates is unbalanced due to irregular reporting on the part of states.

The published industry-level injury rates are calculated by the Labour Bureau on the basis of state-level returns; depending on the rate of state-level reporting, injury rates for some industries are occasionally missing. Establishing a long panel of injury rates at the industry level is made difficult by the fact that the national industrial classification system changed significantly starting in 1999, with some industries splitting and others merging. Constructing a mapping between the pre- and post-1999 2-digit classifications is only possible after substantial aggregation of fatality rates across 2-digit industries, and the resulting concorded data have fewer industries. Consequently, in the analysis in Section 6, which relies heavily on variation in reform exposure between industries, I use only the pre-1999 industry panel.

The Factories Act data have three limitations. To begin with, these data only pertain to the formal manufacturing sector,[15] which accounted for only about 15–20% of the manufacturing employment in India during the period in question. Second, in terms of data quality, an obvious limitation is that reports of workplace injuries filed by factory owners are likely to significantly understate the extent of workplace accidents. This limitation is mitigated by the fact that employers are also required to report fatalities, which are presumably harder to conceal—Boone and van Ours (2006) show that, differently from non-fatal injuries, recorded rates of fatal injuries in OECD countries do not appear to be subject to strategic misreporting.

The third limitation of these data is that not all factories actually comply with the statutory reporting requirement. The Factories Act data are therefore based on a self-selected sample of manufacturing establishments. However, the Labour Bureau also publishes information at the state level on the number of factories that did and did not submit returns in any given year, along with total employment in the submitting factories. Based on this additional information, I can calculate the annual rate of compliance in each state, as well as the average size of compliant establishments relative to the population.[16] In Section 4.2 below, I discuss compliance rates in more detail and explain how these statistics can be used to provide a correction for the sample selection inherent in the Factories Act data.

Data from police reports

The National Crime Records Bureau (NCRB) publishes an annual report titled *Accidental Deaths and Suicides in India* that aggregates information provided in First Instance Reports (FIRs) filed by local police stations following a report of an accident or injury. The NCRB reports distinguish deaths

[14] The Labour Bureau also publishes injury data cross-tabulated by state and 2-digit industry, but did not do so over the period 1981-99 (which is central to the analysis in this paper). I therefore do not make use of the cross-tabulated data in this paper.

[15] More precisely, the Factories Act applies to establishments employing at least ten workers and using electricity (the threshold for firms not using electricity is 20 workers).

[16] To estimate average establishment size in the population, I use estimates of state-level factory employment derived from the Annual Survey of Industries (ASI). The Factories Act data also contain estimates of total employment, but these are less reliable than the ASI data because the former are based on extrapolations from outdated data.

due to various causes, including the specific category of factory deaths. These reports are available online (in PDF form) on the NCRB's website and were digitized and cleaned as part of this study, covering the period 1980–2010. As with the Factories Act data, not all states report their information to the NCRB every year. As a result these data form an unbalanced state-level panel. I refer to these as the NCRB data in the rest of the paper. As with the SDP, an ambiguity in these data is that the definition of 'factory' is not specified or standardized. In the analysis, I treat these data as pertaining to the formal manufacturing sector, in order to construct fatality rates.

Police records are routinely utilized to estimate rates of injury and accident due to road transport incidents, and (in that specific context) have been shown to have a reasonable (but not perfect) degree of coverage in terms of fatal accidents (Dandona et al. 2008).[17] In the context of workplace accidents, the NCRB data are in principle more comprehensive than reports filed by factory owners (e.g. the Factories Act data), since workplace accidents can be reported to the police by any person, including workers and their relatives. On the other hand, workers may be disinclined to report injuries to the police for fear of reprisals from the employers—while this concern is mitigated by the fact that we are looking at fatalities rather than non-fatal injuries, one may still be concerned that some deaths will not be reported to the police, even by relatives of the deceased worker, if they believe the accident to have been the fault of the deceased (in Dandona et al.'s study, this was the predominant reason stated for not reporting a traffic fatality to the police). Using a capture-recapture method, Yadav et al. (2021) estimate that police reports only captured around 43% of fatal injuries in the construction sector in Delhi (the coverage of compensation claims data was even worse at about 19%), and Yadav (2019) uses the same method to find that police reports also significantly under-count industrial injuries (and to a much greater extent than hospital records).

In Appendix Figure 3, I use data from 1990 to compare the number of fatal accidents reported in the NCRB and Factories Act datasets. To make the comparison meaningful, I present the figures at state level for each of the 11 states for which there exist data on fatal accidents in both datasets in that year. The pattern of results suggests that the NCRB data are under-counting fatalities relative to the Labour Bureau data (although there is some possibly interesting heterogeneity across states in this respect, which I do not explore in this paper). As I will show in Section 5, fatality rates constructed from the NCRB data are consistently smaller than rates constructed from the Factories Act data across all years of the study period. At the same time, it is worth noting from Appendix Figure 3 that the two sources of data provide a consistent picture of accident rates, as evidenced by their strong degree of positive correlation (the simple correlation coefficient between the state-wise accident estimates is 0.80 and the Spearman rank correlation coefficient is 0.85).

Injury compensation data

Under the Workmen's Compensation (WC) Act of 1923 (described in Section 2), workers in establishments employing more than ten persons are eligible for compensation from their employer for work-related injuries. The WC Act covers a number of sectors outside of agriculture, including manufacturing, plantations, docks, mines, construction, transportation, railways, and municipal workers. Under the WC Act, claims for compensation are handled by specially-appointed state commissioners. Data on compensations awarded is compiled and published at national level. To construct a time series of data on compensated injuries, I digitized published tables in the annual *Indian Labour Statistics* volumes, and the annual *Report on the Working of the Workmen's Compensation Act*. These data cover the period 1975–2005 and include information on fatal injuries, injuries resulting in per-

[17] Dandona et al. (2008) conducted a population-based survey along with a hospital-based surveillance in Hyderabad to estimate the proportion of road traffic injuries that are reported to the police. While all on-the-spot deaths and all hospital deaths were reported to the police, 22% of the remaining fatalities were not reported.

manent disablement, as well as injuries resulting in temporary disablement. For comparability with the other data sources used here, and because coverage of minor injuries in these data is of more doubtful quality, I restrict attention to the first two categories of injury. In what follows, I refer to these as the Compensation data.

A number of caveats regarding the WC data need to be noted. First, the data do not capture all injuries but only those that were officially compensated. Second, the year in which an injury or death was compensated need not be the year in which the injury occurred.[18] Third, because the WC Act only applies to workers who are not covered under the Employees' State Insurance (ESI) Act, and the coverage under the latter has been gradually increasing over time, the comparability of these data over time is questionable. In the analysis, I use these data only as a secondary check on the main results.

3.2 Employment data

I use aggregate sectoral and national employment data from the Reserve Bank of India's KLEMS database (RBI-KLEMS) which provides sectoral employment figures for the period 1980–2020. I extend these data to also include the period 1970–80, by supplementing with sectoral employment data from the GGDC/UNU-WIDER Economic Transformation Database (ETD) (Kruse et al. 2022), whose methodology is consistent with KLEMS. Both databases construct employment aggregates by combining decennial census data with the quinquennial National Sample Survey (NSS) Employment-Unemployment survey data. The sectoral employment data cover both formal as well as informal employment, and include individuals who reported being employed in that sector either as their main work activity or their subsidiary work activity (see the Chattopadhyay et al. (2021) for a discussion of the different work status concepts in the NSS surveys).

For estimates of formal sector manufacturing employment, I use annual estimates of man-days and number of workers at state- and 2-digit National Industrial Classification (NIC-1987) level from the reports published by the Annual Survey of Industries (ASI).

3.3 Data on policy reforms

The analysis also makes use of data on 3-digit industry level tariffs and exposure to delicensing and FDI reform in India used in Harrison et al. (2013).[19] Final and intermediate goods tariffs at the industry level were constructed by mapping applied tariff data from the Government of India's Customs Tariff Working Schedules and Trade Analysis and Information System (TRAINS) to India's 3-digit National Industrial Classification (NIC-87) codes, using the concordance developed by Debroy and Santhanam (1993). The delicensing and FDI reform variables are equal to one if any products in a 3-digit industry have been liberalized and are equal to zero otherwise. These data cover the period 1985–2004 (excluding the year 1995).

For the analysis in this paper, I aggregate the data on policy reforms up to the 2-digit industry level. To do so, I construct 2-digit level employment weighted aggregates of each of the 3-digit level variables described above, where the weights reflect the employment shares in 1990 (prior to the reforms of 1991).

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[18] In Bhatia's (1981) survey of industrial workers in Punjab and Jammu and Kashmir, the majority of claims for compensation had not been settled even after nine weeks.

[19] These data were kindly provided by Shanthi Nataraj.

4 Constructing injury rates

The incidence of occupational injuries can be estimated by dividing the total number of injuries in a given period by the number of workers exposed to workplace hazards in that period. Identifying the workers who are exposed is not straightforward, and data availability usually forces a particular choice. As we will see later, discrepancies between injury rates calculated from different data sources can arise due to differences in how the reference population is defined. At the outset, therefore, it is important to be explicit about these definitions.

I follow the ILO practice of calculating injury incidence rates per 100,000 workers. In settings where a number of workers are working on a part-time basis, the incidence rate may be a misleading indicator of workplace safety unless it is adjusted for the number of days/hours actually worked (the resulting rate is sometimes referred to as a 'frequency rate'). The Bureau of Labor Statistics (BLS) calculates injury incidence rates for a given number of full-time equivalent (FTE) workers, starting from information on total number of work hours of all employees in an establishment and deflating by 2,000 to convert annual working hours to FTEs, assuming an FTE corresponds to 40 hours of work a week for 50 weeks. I follow a similar procedure wherever information on workdays is available, assuming an FTE amounts to 240 days of work a year.

In addition to the definition of the reference group, any analysis of injury rates must take into account the way in which the data collection process results in artefactual variation in these rates across regions and time. This is evident in the case of administrative data on injury rates but also turns out to be an important consideration for the analysis of the household survey data.

4.1 (Retrospective) injury rates from SDP data

To construct injury incidence rates from the SDP data, one must first account for the period in which the injury occurred. To do so, I utilize the survey information in the SDP on age at onset of disability. In conjunction with the information on the age of the respondent, the age at onset allows me to identify the year in which the injury occurred. I can therefore create a retrospective count of injuries occurring in each year prior to the survey.[20]

There are some distinct statistical issues associated with retrospective injury counts. Recall error is an obvious issue in this context. While, in theory, an injury that is severe enough to result in permanent disability is a significant life event that might be likely to be recalled with less error than more minor events, Appendix Figure 4 shows that the data on age at onset do exhibit a significant degree of age heaping, consistent with respondents having a tendency to round their answers up or down to the nearest multiple of 5. The primary effect of this phenomenon is to create a systematic pattern of noise in the data. When considering time trends in injury rates, a straightforward way to smooth out the peaks and troughs created by age-heaping is to construct moving averages. Given that the heaping occurs at five-year intervals, it is natural to consider five-year moving averages.

A second issue arises from attrition over time. Essentially, the retrospective injury counts obtained from the SDP are likely to under-estimate the true count, because individuals who suffered injuries a long time ago may no longer be alive at time of survey, and the extent of under-estimation clearly increases as we move further back in time from the date of survey. However, because our interest is in injury rates rather than injury counts, this kind of 'survivorship bias' can be mitigated by dividing injury counts by an appropriate measure of employment. Specifically, to construct the injury rate in

[20] Injury counts are calculated taking into account the survey multipliers.

year t, I divide the (retrospectively obtained) injury count for year t by the number of individuals alive at time of survey who would have been of working age and employed at time t.[21] Under the assumption that rates of survival are similar for injured persons as well as for the working-age population as a whole, this measure of injury rates should be free of survivorship bias.

I briefly discuss two other distinct sources of survivorship bias in retrospective data. The first is that minor injuries that occurred a long time ago may fail to be recorded in the data, because the affected individuals may have recovered fully (and therefore not present with any disability at time of survey), whereas minor injuries that are more recent are more likely to be accounted for. This concern is to a large extent mitigated by the fact that the SDP records only severe disabilities. For instance, approximately 95% of locomotor disabilities due to workplace injuries in the 2002 data are characterized by paralysis, deformity, or loss of limb, and approximately 79% of workplace injuries recorded in the 2018 survey were severe enough to result in the individual losing or having to change their job. A second, related type of survivorship bias arises from the other end of the distribution of injury severity. It is possible that certain kinds of major injuries may shorten the individual's lifespan (and hence increase their attrition from the sample), which would also manifest as artificially lower injury rates in the past than in the present. Because these instances are likely to be fewer and less important than attrition due to old age, I have ignored this issue. However, I note in passing that both types of survivorship bias can be removed under some assumptions, if we have data from more than one survey round. The intuition behind the adjustment procedure is that if the attrition process can be assumed to be stationary, then the resulting artefactual trend is a function of time-relative-to-survey rather than a function of calendar time—by partialling out the effects of time-relative-to-survey one could therefore obtain the pure calendar time trend.[22] I am not, however, able to implement this procedure in these data as the two SDP survey rounds were not comparable, for the reasons discussed in 'Household survey data' (in Section 3).

Turning to the definition of injury rates, the construction above assumes that the relevant reference group for the worksite injuries reported in the SDP is the entire adult working population, but this assumption may be incorrect insofar as some work-related injuries may occur in settings that are not primarily or exclusively worksites, and may therefore not be captured in the SDP—most notably, there is a huge number of family businesses that operate out of residential premises or non-traditional worksites. In this sense, the incidence rate calculated from the SDP data will tend to be an under-estimate of the actual rate. Second, the employment figures do not take into account the fact that not all employed individuals are working full time, and the resulting rate of incidence will tend to under-estimate the extent of work hazard.

I further disaggregate workplace injury rates into sectoral rates by using the information on place of injury. To do so, I divide the injury counts corresponding to each place of injury by the cor-

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[21] Because the SDP only surveys households with disabled individuals, I use data from a separate national household survey conducted by the NSS in the same year as the SDP, in order to construct the retrospective employment measure (i.e. the number of individuals alive at time of SDP survey who would have been of working age in year t). I then multiply this estimate by the labour force participation rate in year t, which is obtained by interpolating decadal census estimates.

[22] More formally, suppose that injury rates in year t, measured from survey data recorded in year k, can be written in the following separable form:

$$r_{tk} = r_t - f(k-t) \tag{1}$$

where r_t denotes a component that is specific to the calendar year t (this is the true rate of injury in year t), while $f(k-t)$ denotes the attrition component that is being assumed to only depend on how far in the past year t is relative to the year of survey. Because we have retrospective data collected at two different points in time, we can estimate the $f(k-t)$ component separately from the pure time effect r_t (for the calendar years in which the two survey rounds overlap). This procedure can be implemented by estimating Equation (1) as a regression in which $f(k-t)$ is modelled as a polynomial function of $(k-t)$. The r_t can be estimated from this regression as the coefficients on calendar year dummies.

responding employment in that sector and in that year. The employment variable is constructed retrospectively as before, with sectoral employment being inferred using sectoral employment shares from the RBI-KLEMS and ETD databases. This exercise is relatively straightforward for agriculture and mining accidents. The 'factory accidents' category is more ambiguous, since 'factory' may be narrowly interpreted as a formal sector establishment or more broadly as any manufacturing establishment, including what is referred to as the 'unorganized manufacturing sector', which mostly comprises household enterprises. If survey respondents adopt the former interpretation, then injuries in the unorganized manufacturing sector may be classified under the 'other worksite' category. In the main analysis in Section 5.1, I construct factory accident rates relative to total manufacturing workers.

Lastly, for injuries in the 'other worksite' category, I express the rate relative to the total employment in sectors other than agriculture, mining, and manufacturing. The 'other worksite' category potentially encompasses a highly heterogeneous (in terms of occupational hazards) set of occupations and industries, including both high-risk sectors such as construction and port workers, as well as relatively low-risk clerical or white-collar occupations. As a result, the movements in the overall injury rate for this sector are difficult to interpret. Some of these occupations also do not have a fixed worksite (e.g., transport workers, postal workers), and injuries associated with work may not be captured in the SDP data, as a result of which our reference group assumption may be too broad and injury rates will be under-estimated.

4.2 Injury rates from Factories Act

The published data from the Factories Act are already in terms of injuries per 100,000 man-days. I convert these to rates per 100,000 FTEs, assuming an FTE is equivalent to 240 days of work a year.

As I noted in 'Factories Act data' in Section 3, the injury data compiled from the Factories Act returns pertain to those (formal sector) factories/establishments that complied with the reporting requirement. Implicitly therefore, the reference group is workers in these establishments, and there is a potential bias in extrapolating these rates to the rest of the sector. Poor compliance with reporting requirements is arguably a major reason why these kinds of administrative data have not been more frequently utilized (even though the ILO, for instance, regularly gathers and publishes cross-country data derived from similar sources). For instance, when examining trends in injury rates over time (as I will attempt to do in this paper), it is not clear how one could distinguish the true time trend from any trends in compliance.

An advantage of working with the full breadth of the published data, however, is that the extent and nature of compliance can be characterized and controlled for to some extent. In Appendix Figure 5, I graph the average rate of compliance (i.e. the fraction of reporting establishments) against time, along with the relative establishment size (in terms of employment) in the complying establishments (as a ratio relative to average establishment size in the population). The rate of compliance (left panel of figure) has been steadily falling over time from over 70% in the 1970s to less than 50% by 2000. The relative establishment size (right panel of figure) has always been greater than 1, indicating that compliant factories are on average larger than the average factory in the population, and this ratio has increased sharply since the mid-1980s.

The fact that compliant factories are on average consistently larger than the average factory in the population is helpful for assessing the likely direction of bias. Studies that have examined the relationship between firm size and injury rates consistently find that larger establishments have lower rates of severe injuries (e.g., Mendeloff and Kagey 1990; Fabiano et al. 2004; Mendeloff et al.

2006), which may be due to a number of factors, including economies of scale in the provision of safety, differences in socio-economic profiles of workers between small and large establishments, and differences in exposure to safety inspections and the regulatory regime.

In Appendix Figure 6, I utilize more recent establishment-level data on workplace injuries published by the Occupational Safety and Health Administration (OSHA) in the United States in 2022 to demonstrate the negative relationship between size and fatal injury rates for manufacturing establishments. A similar negative relationship can be observed in state-level injury rates from the Factories Act data. In Appendix Figure 7 (which mirrors Appendix Figure 6 for the US data), I group the state-level observations into 25 approximately equal-size groups based on establishment size, and graph the average fatality rate for each size group.

Given that complying factories are on average larger than non-complying factories, one may conclude that the injury rates estimated from the Factories Act data are an under-estimate of injury rates in the formal sector as a whole.[23] But, in addition, the relationship with establishment size suggests a correction for sample selection that can be implemented using the available data on compliance. I elaborate this procedure below.

I start by modeling the establishment-level rate of injury as an additive function of establishment size (in terms of number of employees) and other time-invariant state level factors and time-varying national factors:

$$r_{ist} = \alpha_s + \alpha_t + \beta_t L_{ist} \tag{2}$$

where i indexes an establishment, s and t index the state and year, respectively; L_{ist} denotes establishment size; α_s and α_t are the state and national factors; and the coefficient on establishment size, β_t, is allowed to vary over time.

The linearity of the relationship in Equation (2) implies that the average fatality rate at any level of aggregation is a linear function of average establishment size. The observed state-level fatality rate can then be written as follows:

$$r_{st}^c = r_t + \beta_t(\overline{L}_{st}^c - \overline{L}_t) + (\alpha_s - \overline{\alpha_s}) \tag{3}$$

where r_t denotes the national rate of injury; $\overline{L}_{st}^c$ denotes the average establishment size in the state-level compliant sample; $\overline{L_t}$ denotes national average establishment size in the population; and $\overline{\alpha_s}$ denotes the national average of state-level factors. To see the effect of sample selection more clearly, I average the observed state-level fatality rates at time t and re-write Equation (3) as:

$$E_{t,obs}(r_{st}^c) = r_t + \beta_t E_{t,obs}(\overline{L}_{st}^c - \overline{L}_{st}^{obs}) + \beta_t E_{t,obs}(\overline{L}_{st}^{obs} - \overline{L}_t) + E_{t,obs}(\alpha_s - \overline{\alpha_s}) \tag{4}$$

where $E_{t,obs}$ denotes an expectation conditional on time t as well as conditional on the set of states for which injury rates are observed (since, implicitly, averaging over the observed state-level injury

[23] In addition to selection into compliance in terms of submitting returns, there is a higher level of non-compliance associated with the fact that many firms that are in principle under the ambit of the Factories Act do not actually register themselves. The Factories Act data therefore represent, at best, the set of registered manufacturing establishments. The analysis of Chatterjee and Kanbur (2015) indicates that the median registered establishment is about 2.5 times larger (in terms of employment) than the median non-compliant firm. In turn this suggests that injury rates in the Factories Act data are even more of an under-estimate if we consider the population of manufacturing establishments employing more than ten workers.

rates amounts to also conditioning on the set of states for which injury rates are observed at time t); and $\overline{L}_t^{obs}$ denotes average establishment size in states that report injury data.

Equation (4) above shows that the average of state-level injury rates deviates from the national average because of three selection factors: (i) the difference between average establishment sizes in complying factories and in the universe of factories in the states which do report injury data, (ii) the difference between average establishment sizes in states that report injury data and in the full set of states, and (iii) the difference in the average of time-invariant state-level factors between states that report data and the full set of states.

I remove the effects of these selection factors by estimating Equation (3) as a regression, in which the true national rates, r_t, are estimated as coefficients on year dummies, while controlling for state fixed effects and $(\overline{L}_{st}^c - \overline{L}_t)$, with the coefficient on the latter being allowed to vary by year. Because the data are state-level averages, I weight the regression by the square root of the number of reporting firms in each state, in order to adjust for heteroscedasticity.

It is important to recognize the limitations of this selection correction procedure. A convenient feature of the linearity assumption above is that since the selection bias due to compliance only depends on the size of the average complying establishment relative to the population (which is observed in the data), the exact selection rule need not be specified, assuming it is related to size (e.g. a selection rule in which the probability of complying varies in a complex, non-linear way with establishment size).[24] However, what if the selection is on the basis of some other characteristic that affects injury rates? An obvious example is when compliance varies by industry, in which case one would need to account for state-level industrial composition in the regression. Allowing the α coefficients to vary by state and year (as I have proposed above) is a way to absorb the effects of these other variables, but a finer approach could be implemented if the data were reported at a more disaggregated level.[25]

4.3 Injury rates from NCRB data

The NCRB data provide information on ‘factory deaths’, but as I noted in ‘Data from polic reports’ in Section 3, the term ‘factory’ is not guaranteed to mean the same thing in the different jurisdictions from which the data are collated, so that the reference group is ambiguous. Nevertheless, it seems plausible that ‘factory’ refers to a formal sector manufacturing establishment rather than an informal household enterprise. I therefore use the NCRB data to construct state- and national-level injury rates per 100,000 FTEs, where the denominator is based on data on man-days in the formal manufacturing sector in each state (these data are taken from the Annual Survey of Industries Annual Reports).

As with the Factories Act data, missing data are common, and the state-level panel is unbalanced. To account for non-random missingness, I obtain the national rate by regressing state-level rates on a set of year dummies, while controlling for state fixed effects.

[24] If the relationship between firm size and injury rates is not well-approximated by a linear function, then we may not be able to correct for selection on the basis of knowledge of average establishment sizes alone. A notable exception is when injury rates are inversely related to firm size, i.e. $r_i = \alpha/L_i$, in which case it is easy to show that the aggregate rate of injury is log-linearly related to average establishment size.

[25] In fact, injury rates have been published on the basis of Factories Act returns at the state x industry level, but these more disaggregated rates are not available for the period 1980–2000. In any case, information on compliance rates has never been published at the state x industry level.

4.4 Injury rates from WC data

Injury rates in the WC data are calculated relative to employment in the reporting establishments, and the caveat regarding extrapolating the estimated injury rates to the population of workers applies here as well. Differently from the Factories Act data, the WC data only provide information on the number of workers, and not man-days worked. I therefore calculate injury rates per 100,000 workers in these data. Because I do not have information on compliance or sample selection, I can only present the raw rates as reported.

5 Trends in workplace injury rates: 1975-2016

5.1 Trends in SDP data

I first construct national and sectoral time trends in occupational injury rates using the disability survey data. In the left panel of Figure 2, I show the trends in the unadjusted rates of injury obtained from each of the two survey years. In the right panel, I show the trends obtained after the rates have been averaged using the moving-average method.

Leaving aside the difference in levels (due to the survey differences noted earlier), the data from the two survey rounds are in broad agreement with regard to the trend in workplace injuries. Prior to the 1990s, the injury rate was stable (or increasing slowly), whereas there was a sharp upturn starting in the early-to-mid 1990s (although it is difficult to be precise, the 2002 survey data suggest a trend break around 1991, while the trend break in the 2018 survey data appears to occur a few years later around 1995). In the period 1980-90, the injury rate was approximately six injuries per 100,000 workers (according to the 2002 survey round), and this rate rose sharply to approximately ten per 100,000 workers by the end of the 1990s. The 2018 survey data indicate that the injury rate has continued to rise at a similar pace in the post-2000 period.

Both the sharpness of the trend break as well as the fact that the two survey rounds are in rough agreement on the timing of the break strongly suggests that this is not a statistical artefact (e.g. due to survival bias). In the following sections, I show that this finding is also corroborated by alternative sources of data on injury rates which do not suffer from retrospective biases.

To further unpack this phenomenon, I now turn to a sectoral disaggregation of injury rates. Figure 3 disaggregates the overall injury rates into sectoral rates, using the detailed information on place of injury elicited in the 2002 survey. Because the disaggregated raw series are significantly more noisy than the aggregate series, I graph the smoothed rates using the moving-averages method. I also ignore the mining sector, whose series is extremely noisy (even after smoothing) due to the very small size of this sector.

The sectoral disaggregation reveals that the trend break in the overall series is matched by trend breaks in all three sectors, with the non-agricultural sectors (especially the factory sector) recording the sharpest increase in injury rates. Because inter-sectoral employment shifts have been quite gradual over this time period, Figure 3 suggests that the within-sector increases most likely account for the entirety of the shift in overall injury rates.

As a more formal approach to quantifying the factors underlying the increase in the aggregate level of injury risk, I utilize a shift-share decomposition to decompose changes in the aggregate rate of injury, r_t, as follows:

$$\Delta r_t = \sum_i w_{it-1}\Delta r_{it} + \sum_i r_{it-1}\Delta w_{it} + \sum_i \Delta r_{it}\Delta w_{it} \tag{5}$$

where i indexes sectors and t indexes years. The first term in the decomposition above captures the contribution of within-sector changes in the injury rate (averaged across sectors using the initial year (i.e. $t-1$) employment weights). The second term measures the contribution of inter-sectoral reallocation of workers and will be positive if employment is shifting towards sectors with greater occupational health risk—this part arises from structural transformation. The third term is a covariance or cross-product that is positive (negative) if sectors that are growing bigger are also getting more (less) risky—this term may also reflect the effects of structural transformation if, for instance, agricultural workers with no previous experience of factory work enter into manufacturing, resulting in an increase in the rate of injury in the latter sector (see Kossoris (1938) for a similar hypothesis in the context of the pro-cyclicality of injury rates).

I utilize this decomposition to disaggregate changes in the aggregate injury rate separately over two 'long' periods in the data: 1980–90 (prior to the trend break) and 1990–2000. Table 1 presents the results of this decomposition. The change in the aggregate rate of injury over the first period was an increase in the form of an additional 1.18 injuries per 100,000 workers. Over the second period, aggregate injury rates increased by 3.21 per 100,000 workers. In both periods, we find a quantitatively similar contribution of between-sector shifts in employment. In the overall 1980–2000 period, between-sector reallocation of shares has increased the rate of overall injury by an additional 0.6 injuries per 100,000 workers, which represents an approximately 10% increase over the rate of injury in 1980. Over this period, the employment share in agriculture fell from approximately 70% to 60%, implying a modest (but not negligible) effect of structural change on overall injury rates. The effect of sectoral shifts has been dominated by within-sector changes in fatality rates, with the latter almost entirely accounting for the increase in aggregate fatality rates in the post-1990 period.

The question raised by the evidence so far is why within-sector rates have increased so sharply in the post-1990 period, especially in the manufacturing sector, in the absence of any notable changes in occupational safety and health regulations. I turn to this question in Section 6, focusing on the increase in injury rates in the manufacturing sector (for which more disaggregated data are available). Before doing so, however, I attempt to validate the findings above using the three additional data sources described in Section 3: the Factories Act data compiled from annual filings submitted by registered factory owners; the National Crime Record Bureau's data compiled from police reports of accidental factory deaths; and the WC data which pertain to compensated workplace injuries in a set of non-agricultural sectors.

5.2 Workplace injury trends in the Factories Act data

The left panel of Figure 4 plots the fatality rates at national level against time, using the Factories Act data. To ensure that I am not picking up spurious trends arising from a systematic pattern of non-reporting, I also examine the aggregate fatality rate obtained from the selection correction procedure described in Section 4.2—this graph is reported in the right panel of Figure 4.

Both panels of Figure 4 tell a similar story, although the timing of the trend break is more clearly evident in the adjusted data: the rate of workplace fatalities in the formal manufacturing sector was relatively constant until 1991 at around ten deaths per 100,000 FTEs (as per the adjusted rates), at which point, fatality rates began a steep upward climb, attaining a rate of nearly 17 fatal injuries per 100,000 FTEs by 2010.

A comparison of the panels of Figure 4 does not suggest a substantial difference between the raw and adjusted fatality rates. This is actually due to the fact that the level of mortality is so high, against which the selection corrections are relatively minor. In Appendix Figure 8, I graph the bias factors (averaged over each year) arising from selective compliance and from missingness of state-level data, respectively.[26] While both bias factors are negative (except for the state-reporting bias which turns positive after 1999) and reflect overall trends in compliance and state-level reporting, the magnitude of the bias factors is very small (less than 0.5 deaths per 100,000 FTEs) compared to the average rate of fatality in the data. In fact, both pre- and post-1991 fatality rates in these data are very high by the standards of developed economies. For comparison, the fatality rate in Great Britain in 2010 was only 0.6 deaths per 100,000 workers. In a sense the relative unimportance of the bias correction is reassuring for users of these kinds of data, since the implication is that there would have to be an enormous degree of selective reporting to seriously bias estimates of fatality rates in developing countries like India.

5.3 Workplace injury trends in the NCRB and WC data

I now conduct a second check using the NCRB's data on accidental deaths in factories. Figure 5 graphs the rate of factory deaths (per 100,000 workers in the organized manufacturing sector) in these data. As in the case of the Factories Act data, I address the potential bias due to systematic non-reporting by states. In the left panel of Figure 5, I plot the aggregate injury rate computed from the full sample, and in the right panel I restrict the sample to states that have non-missing data on factory deaths throughout the sample period. The two graphs display a similar trend (although the rate of fatality in the balanced sample is clearly greater than in the full sample). There appears to be a declining trend in fatality rates (from approximately ten per 100, 000 FTEs in 1980 to seven per 100,000 workers in 1990 in the balanced sample), which is reversed in the mid-1990s.

Finally, I use the WC data on compensations to examine trends in the rate of death and disablement. These data cover injuries in a number of non-agricultural sectors, including manufacturing, railways, construction, and mining. The left panel of Figure 6 plots the rate of non-fatal injuries over time, and the right panel plots the rate of fatal injuries. Both figures (especially the left panel) suggest a slight downward trend in the rate of injury (in contrast with the SDP data) prior to 1991. However, consistent with the SDP data, both figures indicate an upward trend in the rate of injury beginning in the early 1990s. The rate of non-fatal injury increased fourfold from approximately 25 per 100,000 workers in 1990 to nearly 100 in 2005; similarly, the rate of fatal injury went from approximately 35 per 100,000 workers in 1990 to about 100 per 100,000 workers in 2005.

The WC death rates are extremely high in comparison to developed countries. For comparison, the fatality rate in Great Britain in 2010 was only 0.6 deaths per 100,000 workers. It is also striking that the rate of non-fatal injury in the WC data is extremely high, compared to the SDP. For instance, the rate of non-fatal injury in the year 2000 is approximately 100 per 100,000 workers, compared to a rate of around eight per 100,000 workers in the non-agricultural sectors in the SDP data (see Figure 7). This may appear even more surprising given that the WC data are almost surely under-counting the number of injuries (as I explained in Section 3). One potential source of the difference in rates compared to the SDP (leaving aside any difference in the underlying definition of injury) is that the SDP rates were constructed relative to total sectoral employment (including both formal as well as informal), which may have been conservatively large.

[26] Recall from Equation (4) that the estimated compliance bias is $\beta_t E_{t,obs}(\overline{L}_{st}^{c} - \overline{L}_{st}^{obs})$, and the state-reporting bias is $\beta_t E_{t,obs}(\overline{L}_{st}^{obs} - \overline{L}_t) + E_{t,obs}(\alpha_s - \overline{\alpha_s})$.

To sum up, it is striking that the phenomenon of rising injury rates in the post-1991 period in the non-agricultural sector that was observed in the SDP data is strongly confirmed by three completely independent sources of data on workplace injuries. Given that there were no significant changes in labour regulation in the post-1990s era, a plausible hypothesis is that the increase in the rate of fatalities is a consequence of the economic reforms implemented by India starting in 1991. I investigate this mechanism in the next section.

6 Reform effects on fatality rates in manufacturing

6.1 Economic liberalization and injury rates: hypotheses

The reforms of 1991 introduced three major policy changes, each of which could have potentially had an impact on workplace safety and injury rates in the manufacturing sector. Trade liberalization in the form of dramatic tariff reductions is the most prominent of these reforms and has been evaluated by a number of authors, in terms of its effects on productivity, wages, poverty, and inequality (e.g. Krishna and Mitra 1998; Goldberg et al. 2010; Topalova and Khandelwal 2011; Hasan et al. 2012). By increasing domestic firms' exposure to international competitive pressures, the reduction in output tariffs may have increased injury rates—this is the channel implicated in McManus and Schaur (2016), who find that increased exposure to import competition increased the productivity of firms in the US manufacturing sector but at the cost of significantly greater rates of workplace injury.

The removal of industrial licensing requirements in 1991 also increased the degree of competition in Indian manufacturing industries by removing barriers to firm entry and by allowing more productive firms free rein to expand their output (Aghion et al. 2008; Chari 2011). In theory, the effect of this aspect of the reform on injury rates should therefore be similar (qualitatively) to the reduction in output tariffs.

The third aspect of reform was the relaxation of restrictions on FDI. Because of its close association with the presence of multi-national corporations (MNCs), the role of FDI is particularly interesting to evaluate empirically. The literature has posited a number of ways in which MNCs can have a negative impact on occupational health, including (i) implementing poor safety and health practices on the factory floor (e.g. Brown 2002), (ii) recruiting workers from vulnerable groups (e.g. Abell 1999), (iii) persuading authorities to turn a blind eye to safety and health violations in order to keep the MNCs from shifting their operations to a different country (e.g. O'Rourke 2001; Loomis 2015), and (iv) importing products, technologies, and processes that are either known to be hazardous or do not transfer to local conditions without substantial risks (e.g. Elling 1977; Castleman 1983; Baram 2009). The empirical literature has, however, not found a consistent correlation between country-level FDI flows and workplace injuries. The exogenous nature of the 1991 reforms in India offers a promising opportunity to test the hypothesis in a more credible way.

6.2 Empirical strategy and results

To estimate the separate effects of these distinct aspects of the economic liberalization reforms, I utilize a difference-in-differences methodology that takes advantage of the exogenous nature of the reforms of 1991, along with variation across industries and over time in the extent of exposure to the various aspects of the reforms.

To provide a better sense of the policy variation generated by the 1991 reforms, I graph the evolution of each of the policy variables (i.e. treatments) over the sample period in the panels of Appendix

Figure 9. In each panel, I show the time series for each of the 16 industries in the data, in order to illustrate the cross-sectional variation in treatment exposure. The sharp change in policy regime in 1991 due to economic liberalization is immediately apparent for all the policy variables, but a number of other observations are also relevant. First, looking at the tariff variables, one can see that all industries are 'treated' in the sense of experiencing a sharp fall in tariff rates after 1991. But the heterogeneity in tariff rates in the pre-1991 period was also sharply reduced by the 1991 reforms, which is due to the fact that the reforms were also intended to produce a rationalization of tariff rates, implying that different industries were subjected to differing levels of treatment intensity. In the case of the FDI and licensing reforms as well, virtually all 2-digit industries experience deregulation, albeit to varying degrees. It can also be seen in Appendix Figure 9 that the initial bout of deregulation in 1991 was followed by further rounds of deregulation in the mid-1990s.

While a number of studies have adopted a difference-in-differences methodology using industry-level policy variation to evaluate the liberalization reforms of 1991 (e.g. Aghion et al. 2008; Goldberg et al. 2009), there are a number of challenges to doing so, some of which are specific to the data on injury rates. I discuss these issues carefully below in the context of the following standard two-way fixed effects specification:

$$y_{it} = \alpha + \boldsymbol{\beta} \boldsymbol{X}_{it} + \boldsymbol{\pi} \boldsymbol{Z}_{it} + \eta_i + \eta_t + e_{it} \tag{6}$$

where y_{it} denotes the fatality rate for 2-digit industry i in year t; $\mathbf{X}_{it}$ denotes a vector of reform exposure variables measured at the 2-digit level (the construction of these variables was described in Section 3.3); $\mathbf{Z}_{it}$ denotes a vector of time-varying controls that I describe below; η_i and η_t denote industry and year fixed effects, respectively, which account for time-invariant differences between the industries and for common shocks over time, respectively; and e_{it} denotes an error term. The standard errors are clustered at the industry level, and (as the observations represent industry-level aggregates) the regression is weighted by average industry employment over the pre-reform period (1986–91).

The regression above uses reform variation at the 2-digit industry level, as this is the level at which the injury data are available. The balanced panel of data consists of 16 industries. The resulting aggregation of the reform variables makes it difficult to control for unobserved time-varying heterogeneity across industries (whereas with more disaggregated data, one could, for example, restrict comparisons to relatively similar treated and control industries within a larger industrial grouping). The specification in Equation (6) attempts to control for time-varying heterogeneity by grouping industries into pre-reform quartiles of each of the three treatment variables, as well as quartiles of pre-reform industry fatality rates, and interacting each of the quartile indicators with a dummy indicator for the post-reform period (i.e. post-1991)—these interactions are denoted by the vector of time-varying controls $\mathbf{Z}_{it}$ in Equation (6) above.

A second consideration is whether to admit the entire variation in the policy variables over the full sample period (1986–98). Whereas the initial set of reforms in 1991 were indeed unanticipated, and have been shown in the literature to be as good as exogenous to industry characteristics, the exogeneity of the subsequent rounds of policy changes is doubtful (Topalova 2007; Topalova and Khandelwal 2011). In the analysis, I therefore restrict the sample to the period 1986–96. Restricting the sample to the pre-1995 period also has the advantage that the setup is closer to a standard difference-in-differences in the sense that almost all of the policy variation occurs at once rather than being staggered over time, thereby avoiding the problematic 'forbidden comparisons' (Borusyak et al. 2022; see also Goodman-Bacon 2021) that can bias two-way fixed effects estimators.

Before presenting the results from estimating Equation (6), I examine whether changes in fatality rates in the pre-reform period vary by treatment intensity in the post-reform period—this is therefore a test of parallel trends. To do so, I estimate the following regression:

$$\Delta y_{i,pre} = \alpha_0 + \sum \boldsymbol{\beta} \Delta \boldsymbol{X}_{i,post} + \varepsilon_{it}$$

where $\Delta y_{i,pre}$ is the long-difference in fatality rates for 2-digit industry i over the pre-reform period 1986–91, and the right-hand side variables are long-differences over the post-reform period 1991–95 for each of the policy variables (denoted by $\Delta X_{i,post}$). The null hypothesis of no differential trends prior to treatment can be assessed from the $\boldsymbol{\beta}$ coefficients. The results of these regressions are reported in Appendix Table 2. I do not find a statistically significant relationship between pre-reform trends in fatality rates and subsequent exposure to any of the policy shocks following the 1991 reforms.

Table 2 presents the two-way fixed effects regression results. Column 1 reports the results from a simpler two-way fixed effects specification that does not include the time-varying controls $\mathbf{Z}_{it}$. Column 2 reports the results from the full specification. In Column 3, I assess the robustness of the results to a more stringent specification by allowing for industry-specific linear time trends.

The estimated coefficient on the FDI reform variable is consistently positive and significant at conventional levels in all specifications, implying that relaxation of FDI rules is associated with an increase in fatality rates. The point estimates vary between specifications, ranging from an implied increase of 2.5–16.1 additional deaths following from full deregulation of a 2-digit industry. The coefficient on the other two reform variables are not statistically significant, with the exception of the tariff variable which is strongly significant (and surprisingly positive) at 1% level in the basic specification without controls (but not significant in any of the more restrictive specifications).

One concern with these results is that with a small number of cross-sectional units, the results may be sensitive to outliers. To assess this concern, I re-estimate Equation (6) repeatedly, dropping one industry each time, i.e. each regression is estimated on a sample of 15 industries. The results are reported in Appendix Table 3. I find that the estimated coefficient on the FDI variable retains its statistical significance (at at least the 10% level) throughout, while the coefficients on the other variables remain statistically insignificant in all samples.

A second concern with the small number of groups is that the standard clustering correction may be biased (McCaffrey and Bell 2006; Cameron and Miller 2015). To address this concern, I reassess the significance of the estimated effects by conducting a permutation test. I first define a 'treatment assignment' for industry i to be a collection of the corresponding time series of the three policy variables of interest. In each permutation, I randomly reshuffle these treatment assignments across 2-digit industries and re-estimate Equation (6). I then repeat this procedure 100 times and obtain a distribution of estimated treatment effects under the strong null of no effect (i.e. the permutation distribution) for each of the three policy variables.

The panels of Appendix Figure 10 display the cumulative distribution function of each of the permutation distributions. In each graph, I indicate the treatment effect estimated from the actual treatment assignment by a vertical line. As is evident from the figure, the pattern of statistical significance of the actual estimates is broadly consistent with the results obtained earlier. In particular, the treatment effect associated with the FDI reform variable is in the far right tail of the permutation distribution, and is significant at the 5% level (the implied p-value is 0.04). None of the other estimated treatment effects are statistically significant.

I now discuss and implement two alternatives to the standard two-way fixed effects strategy above. The aspect of the above analysis that is perhaps most discomfiting (but which is by no means specific to this study of the 1991 reforms) is that we are attempting to estimate the causal effects associated with multiple concurrent policy shocks, in a context in which virtually all industries are exposed to each of the three treatments, and differ only in their degree of exposure, i.e. there are no pure control groups. To assess first the degree of co-movement of the policy shocks, I estimate the following specification:

$$\Delta x_{i,j} = \delta + \boldsymbol{\lambda}\boldsymbol{\Delta x}_{i,-j} + u_i$$

where $\Delta x_{i,j}$ denotes the long difference in the policy variable j for industry i over the period 1991–95 (i.e. the policy shock introduced by the 1991 reforms), and the vector $\boldsymbol{\Delta x}_{i,-j}$ denotes the corresponding changes in the other two policy variables. I estimate this specification using each of the policy variables as the dependent variable. The results are reported in Appendix Table 4. While the treatment intensities are correlated to some extent, it is reassuring that the R^2 in each regression is small, indicating that there is a substantial degree of independent movement in the three treatments.

The lack of well-defined control groups, however, remains an issue. It would be reassuring if we were only estimating treatment effects from comparisons of treated and control units that differed on only one dimension of treatment, e.g. comparing two industries i and j that were subjected to different tariff changes, while experiencing the same exposure to FDI and licensing reforms (the 'nuisance' treatments in this sub-analysis).[27] Because the 'nuisance' treatments are continuous, however, this ideal comparison does not exist in the data, and as a result it is difficult to conceptualize how the two-way fixed effects estimator leverages the variation between the treatment variables. To provide a more transparent alternative, I implement an estimation strategy that discretizes the nuisance treatments and then groups industries into categories, such that industries within each category are roughly similar to each other in terms of exposure to FDI and licensing reforms. Specifically, I create indicators for above- and below-median levels of exposure to FDI and license reforms (separately) and then consider the four groups formed by the intersections of these indicators. I then restrict the pair-wise difference-in-differences comparisons to be within these groups, by interacting indicators for each of the categories with a post-1991 dummy indicator and including these interactions as controls in a regression of fatality rates on tariffs (this is formally equivalent to a triple-differences specification). I then conduct the analogous exercise for each of the other two treatment variables.

Setting aside the complexity introduced by multiple treatments, non-binary treatment variables are now known to be problematic. Recent advances in the literature on two-way fixed effect models when the treatment variable is continuous or multi-valued (Callaway et al. 2021; de Chaisemartin et al. 2023) have clarified that identification of the underlying causal parameters requires stronger assumptions than the traditional parallel trends assumption that is invoked in difference-in-differences evaluations. While these new findings call into question a sizeable empirical trade literature on the effects of tariff changes (including previous studies that have attempted to evaluate the effect of the tariff changes embedded in the 1991 reforms), the problem is more pronounced in the case of the injury data, because the aggregation of the policy exposure variables up to the 2-digit level renders even the binary treatments (FDI reform and license reform) into continuous measures of treatment intensity.[28]

Unfortunately, the econometric literature on this topic is as yet nascent, and there are no established methods for consistently estimating causal effects in such a setting. Essentially, the continuous treatment case is problematic because it involves comparisons between units that have received different

[27] De Chaisemartin and D'Haultfœuille (2020) argue that even such comparisons may be biased if the effects of the 'nuisance' treatments are heterogeneous between units i and j. I do not attempt to tackle this issue, as it appears to be infeasible to address in the present context in which there are no industries in the data that received no treatment whatsoever.

[28] It is worth pointing out, though, that while the FDI and license reforms were binary treatments in that they specified industries that were freed from existing restrictions, the binary indicators for these policy reforms utilized in previous studies are actually dichotomized measures of treatment intensity. For instance, the license reform indicator in Aghion et al. (2008) captures whether any industry within a larger 3-digit grouping was affected by license reforms.

treatment 'doses', while also having potentially different 'dose-response' functions. For instance, if one were to consider the pair-wise difference-in-differences for a pair of industries i and j that respond differently to treatment doses, the comparison does not identify the marginal response of either industry, unless one of the industries did not receive any treatment (this intuition is formalized in Callaway et al. 2021). This does suggest though that if one could compare industries that have the same relationship between treatment intensity and outcome, the pairwise difference-in-differences comparison would be free of bias. Although the underlying treatment response functions are not directly observable, I suggest matching industries that have the same pre-reform levels of both the x and y variables, i.e. industries that were subject to the same level of the policy variable and had the same fatality rate. Given the small number of cross-sectional units in the data, I implement a coarse form of this matching procedure in the data by simply partitioning industries into four cells based on whether they have above- or below-median pre-reform values of fatality rates and above- or below-median pre-reform values of x. I then create an indicator for each cell, interact each cell indicator with a post-1991 dummy indicator, and include these interactions as controls in the regression.

Appendix Table 5 now presents the alternative set of estimation results. In Column 1, the treatment effect corresponding to tariffs is estimated separately while controlling for interactions between the discretized remaining treatment variables and a post-1991 dummy. In Column 2, I extend this specification by further including controls for the pre-reform combination of fatality rates and tariffs, interacted with the post-1991 dummy. Columns 3–6 repeat these exercises for each of the remaining treatment variables. The results are in line with those obtained earlier: the effect of FDI reforms remains positive and statistically significant, while the coefficients on the other two variables are not statistically significant.

Lastly, as we have discussed earlier, the injury data are compiled from the selected sample of establishments that complied with the reporting requirements. To be sure, this is only an issue if sample selection is correlated with the policy variables, but there is no theoretical basis to rule out the possibility (and the direction of any resulting bias is also theoretically ambiguous). Unfortunately, I cannot model the sample selection in this part of the analysis because data on compliance have not been published at the industry level. This therefore remains as a caveat to the interpretation of the results.

7 Discussion and conclusion

This paper has utilized survey and administrative data from India to examine trends in the rate of occupational injury and fatality over the period 1970–2016. I find that rates of occupational injury increased sharply after 1991, and this increase was largely driven by a rise in injury rates in the non-agricultural sector. My analysis suggests that this underlying shift is plausibly attributed to liberalization policies that were initiated in the early 1990s and which exposed the manufacturing sector to greater levels of internal and external competition, as well as to control by foreign firms. More specifically, while I do not find a robust effect of trade openness in terms of tariff rates, I do find that exposure to FDI has resulted in a significant increase in fatality rates.

The notion that exposure to global market forces, especially via MNCs and FDI, can worsen the occupational health of workers in the host countries is sometimes taken as a truism by labour advocates, but has not received careful empirical evaluation. Economists, on the other hand, frequently espouse the counter-argument that increased integration with global markets increases workers' incomes (in the aggregate) and may thereby strengthen their preference for safe work (assuming that the preference for safe work increases with income). For example, Edmonds and Pavcnik (2005,

2006) report, in the context of child labour in developing countries, that the income effect from increased trade tends to largely mitigate the price effect, resulting in a reduction in the incidence of child labour. But whether this argument applies with respect to hazardous work is not clear *a priori*, given that awareness of workplace safety and occupational hazards is not widespread in developing countries.

The analysis in this paper suggests that the overall impact of foreign exposure on workplace injuries in India has been decidedly negative. The finding with regard to FDI reform happens to have a particular resonance in the Indian context: the largest industrial accident in history, the Bhopal tragedy in 1984 in the Indian state of Madhya Pradesh, that resulted in approximately 16,000 deaths (Eckerman 2005), was caused by a gas leak in a US-owned pesticide plant. The finding is also provocative in that it implicates the element of globalization that most labour advocates tend to focus on, i.e. multinational corporations (MNCs). Understanding the specific mechanisms underlying the estimated effects of FDI exposure in India would be of great interest going forward but would also require much richer data. For instance, there is some evidence that a significant proportion of FDI inflows into India during the 1990s represented mergers and acquisitions rather than greenfield investments (Kumar 2005)—understanding the occupational health implications of the two kinds of investment would be interesting and valuable.

Turning to the effect of structural change, I estimate a modest effect of intersectoral employment shifts on overall fatality rates during this period. Going further, one may speculate that the direction of causation between structural change and occupational health could conceivably go in the opposite direction, and thereby provide a partial explanation of the phenomenon of 'jobless growth' in Indian manufacturing (as well as the phenomenon of leap-frogging observed by Sen (2019)). Conventional theories of structural transformation (e.g. Duarte and Restuccia 2010) model the process as being driven by changes in the demand for non-agricultural goods as incomes increase. A hitherto neglected (but conceivably important) driver may be workers' preferences over jobs that carry different levels of occupational risk. Specifically, if factory jobs are perceived as injurious to health, the exodus of workers from the agricultural sector may either be stalled or diverted into tertiary-sector jobs. It is admittedly difficult to assess the empirical significance of this hypothesis in a rigorous fashion, but it is worth noting that recent evidence does point to workers in developing countries being far less satisfied with factory jobs than one often presumes them to be. In Blattman and Dercon's (2018) study in Ethiopia, the majority of workers who were randomized into factory jobs ended up returning to their original occupations within a year, citing poor working conditions and health risks in the factory sector. Understanding what value workers attach to working conditions, and more generally, to job attributes other than the wage, should therefore be an important next step in our research agenda.

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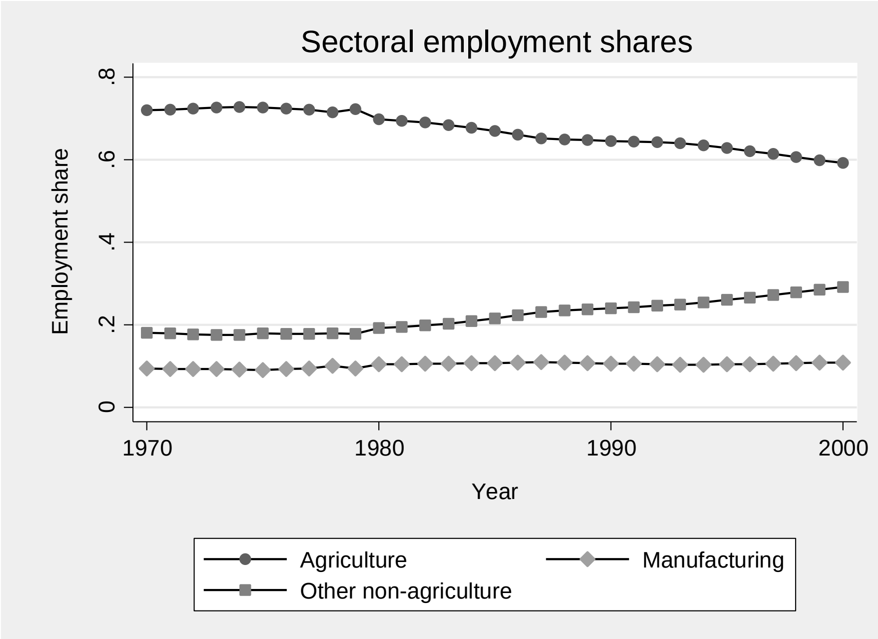
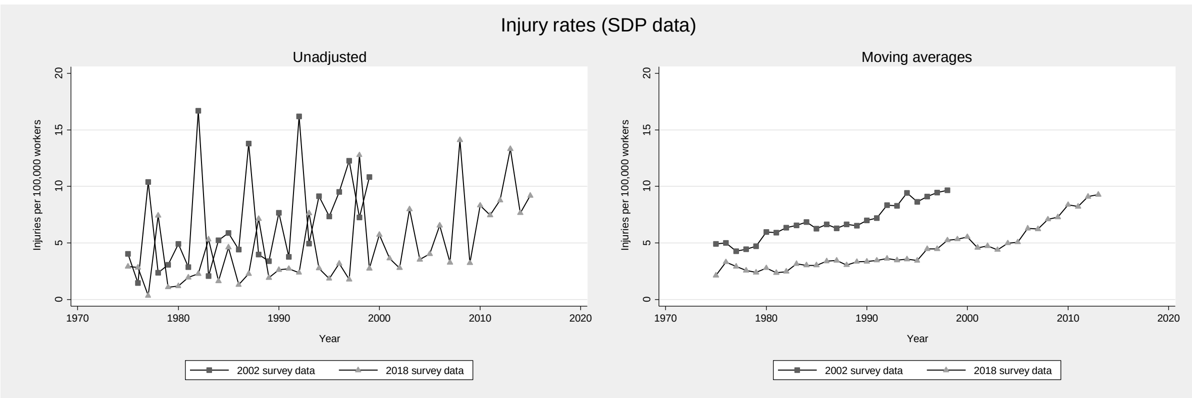
Figures and tables

Figure 1: Sectoral employment shares over the period 1960–2010



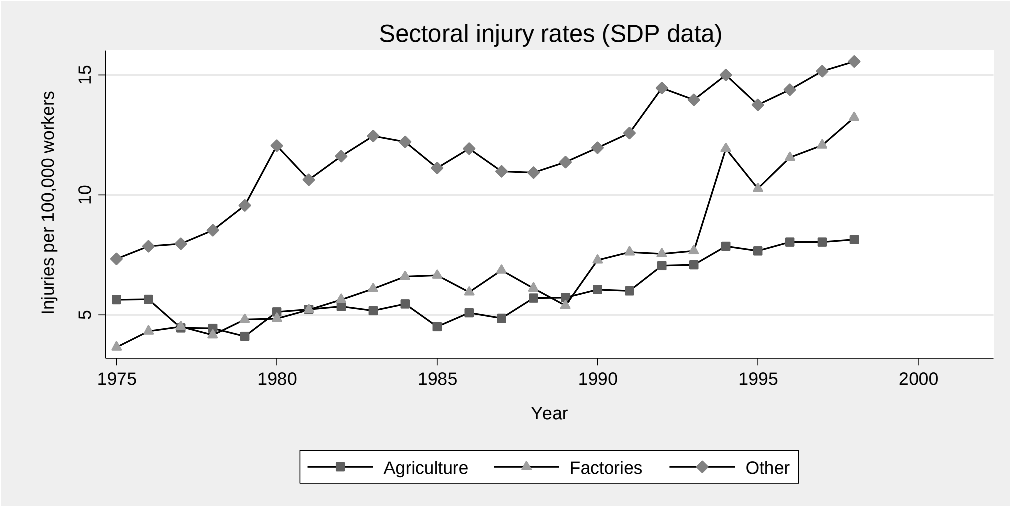
Source: author's calculations using data from the RBI-KLEMS and ETD databases.

Figure 2: The unadjusted (retrospective) injury rates over time derived from the two rounds of the SDP (left); five-year moving averages of these data (right)



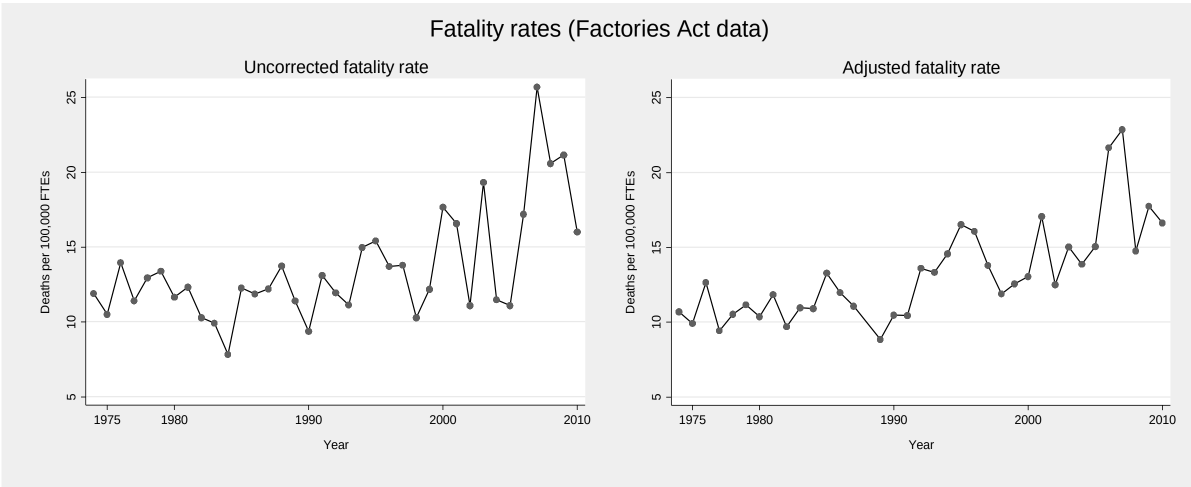
Source: author's calculations using data from the Survey of Disabled Persons, 2002 and 2018.

Figure 3: Five-year moving averages of retrospective fatality rates by sector, using the 2002 round of the SDP



Source: author's calculations using data from the Survey of Disabled Persons, 2002.

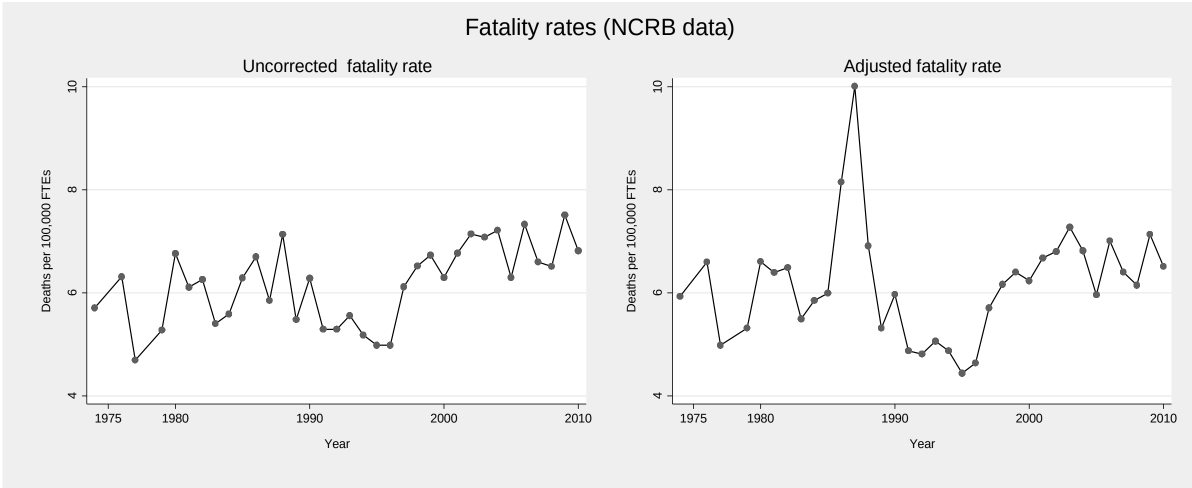
Figure 4: Aggregate fatality rates using the Factories Act data



Note: the left panel shows the unadjusted rates, and the right panel shows the rates that have been adjusted for selection.

Source: author's calculations based on Factories Act data published by the Ministry of Labour.

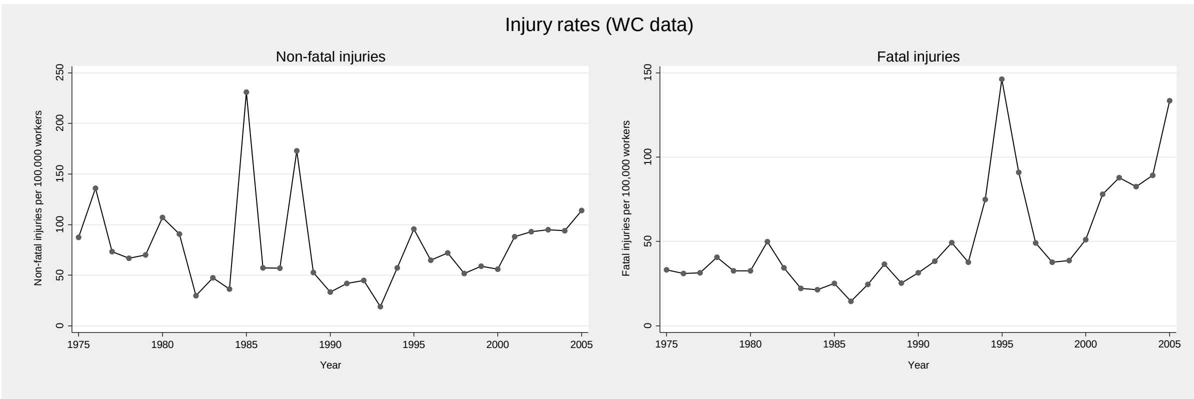
Figure 5: Fatality rates derived from the National Crime Record Bureau's annual statistics on deaths in factories



Note: the left panel plots the unadjusted fatality rates, and the right panel plots adjusted fatality rates.

Source: author's calculations based on NCRB data.

Figure 6: Aggregate fatality rates using the Workmen's Compensation Act data that cover a set of non-agricultural sectors



Note: the left panel plots the rate of non-fatal injuries, and the right panel plots the rate of fatal injuries.

Source: author's calculations based on published Workmen's Compensation act data.

Table 1: Decomposing changes in aggregate fatality rates

	Aggregate change	Within	Between	Covariance
1980–90	1.18	0.87	0.36	-0.05
1990–2000	3.21	2.92	0.23	0.06

Note: the table reports the results from the decomposition of changes in overall injury rates into within-sector, between-sector, and covariance factors, separately for the two long periods 1980–90 and 1990–2000, using SDP data from the 2002 round.

Source: author's estimates using data from the Survey of Disabled Persons, 2002.

Table 2: Effect of liberalization reforms on fatality rates in manufacturing

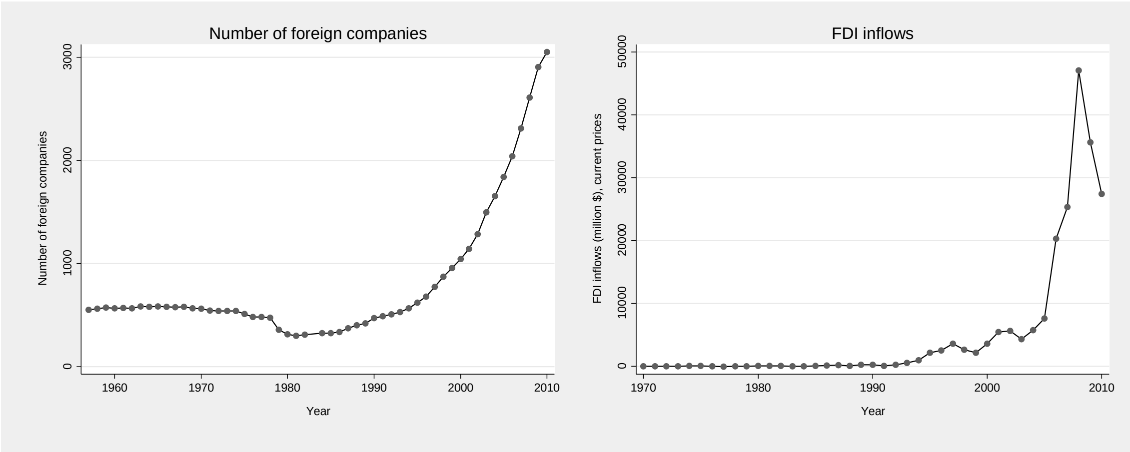
Dependent variable: Fatality rate	(1)	(2)	(3)
Log tariff rate	6.930***	7.066	4.428
	(2.316)	(5.456)	(11.091)
FDI reform (% deregulated)	0.050**	0.129***	0.161***
	(0.022)	(0.031)	(0.051)
License reform (% deregulated)	0.009	0.059	0.038
	(0.030)	(0.037)	(0.116)
Quartile interactions	No	Yes	Yes
Industry linear trends	No	No	Yes
Observations	160	160	160

Note: the table reports the results from the two-way fixed effects regressions of fatality rates on the three policy variables. Each observation is an industry-year, and the regressions are weighted by average industry employment over the period 1986–91. The sample is restricted to the period 1986–95. All regressions include industry and year fixed effects. Standard errors in parentheses are clustered at the industry level. *** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates based on Factories Act data published by the Ministry of Labour.

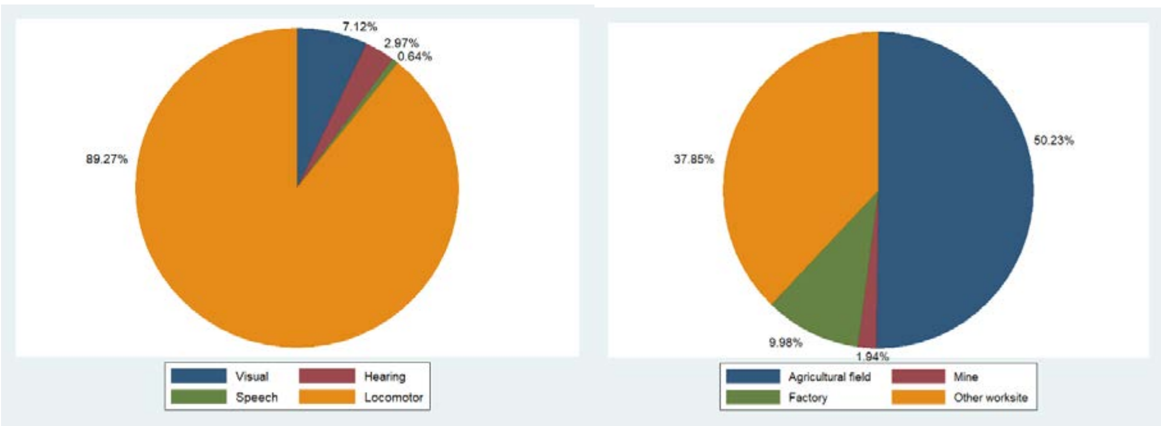
Appendix figures and tables

Figure A1: The number of registered foreign companies in India over time (left) and FDI inflows in millions of US dollars at current prices (right)



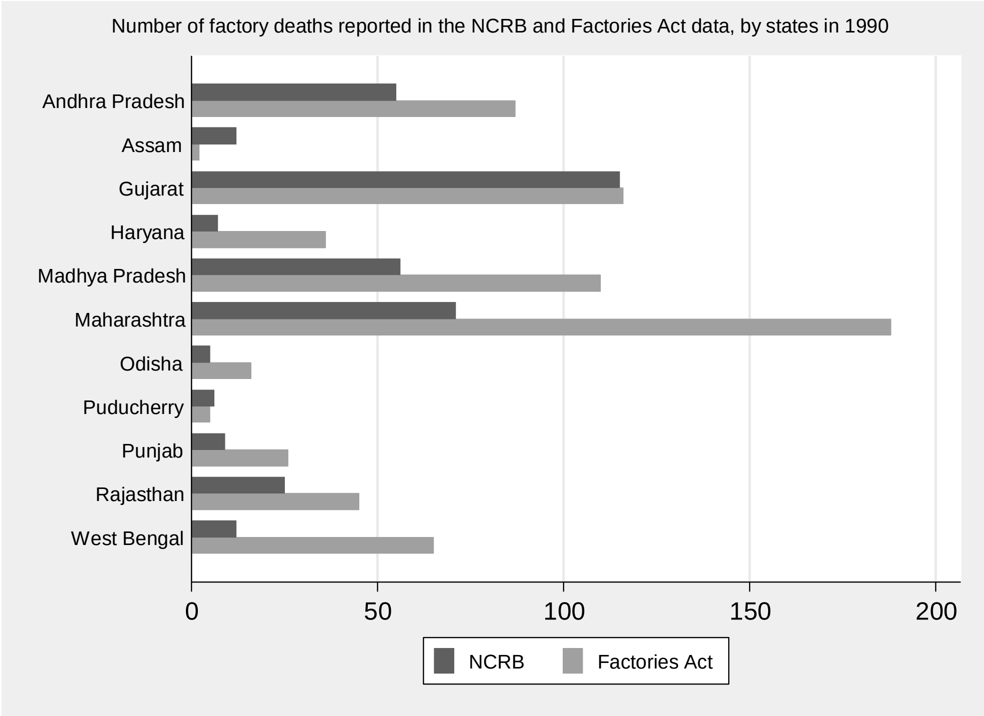
Source: author's calculations based on (i) data reported in the 50th Annual Report on the Working and Administration of the Companies Act, Ministry of Company Affairs, (left panel); and (ii) UNCTADSTAT (right panel).

Figure A2: Distribution of workplace injuries according to type (left) and worksite (right) in the 2002 round of the Survey of Disabled Persons



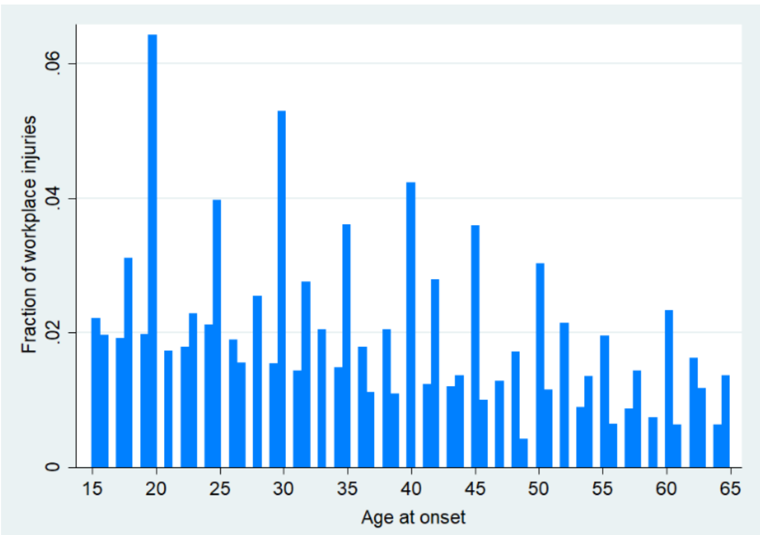
Source: author's calculations using data from the Survey of Disabled Persons, 2002.

Figure A3: Comparison of reported number of factory deaths in the NCRB and Factories Act data, by states in 1990



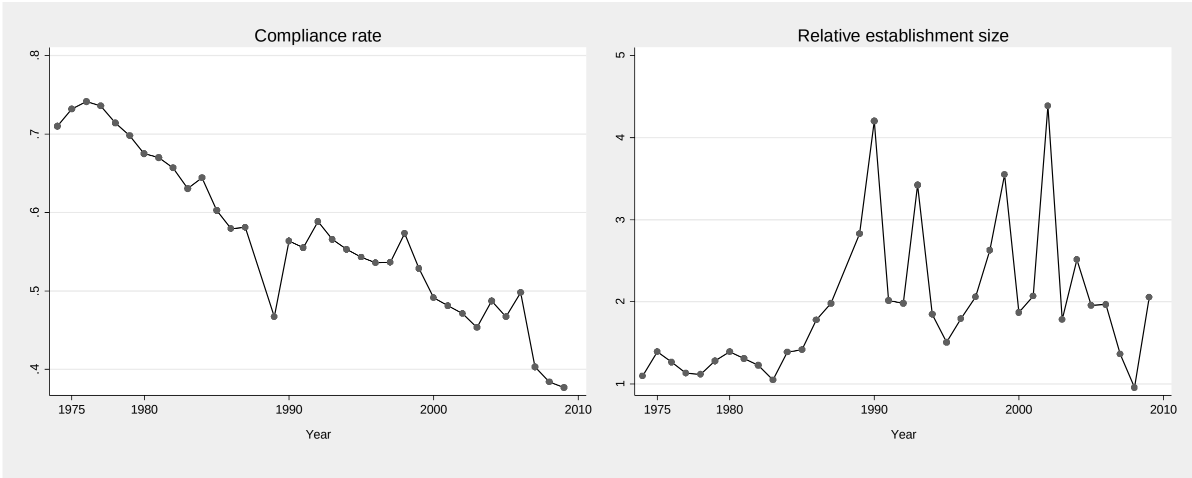
Source: author's calculations using published data from the NCRB and Ministry of Labour.

Figure A4: Distribution of age at onset of workplace injuries



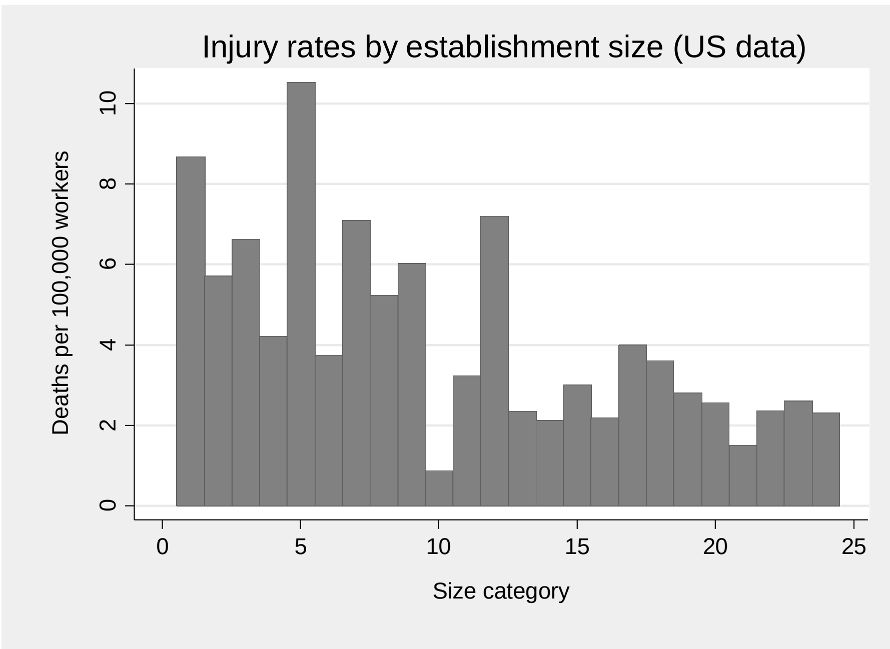
Source: author's calculations based on Survey of Disabled Persons, 2002.

Figure A5: Compliance rate and average size of complying establishments in Factories Act data



Source: author’s calculations based on Factories Act data published by the Ministry of Labour.

Figure A6: Injury rates by establishment size in the United States

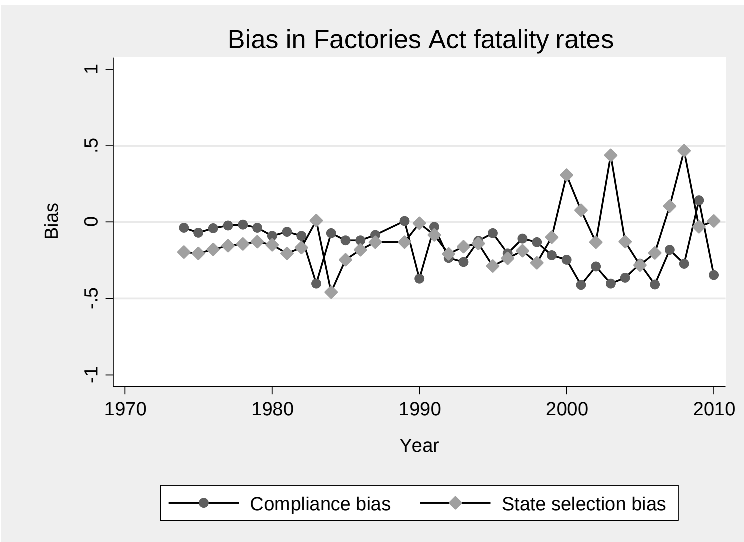


Source: author’s calculations using data from OSHA (2022).

Figure A7: State-level injury rates by average establishment size in India

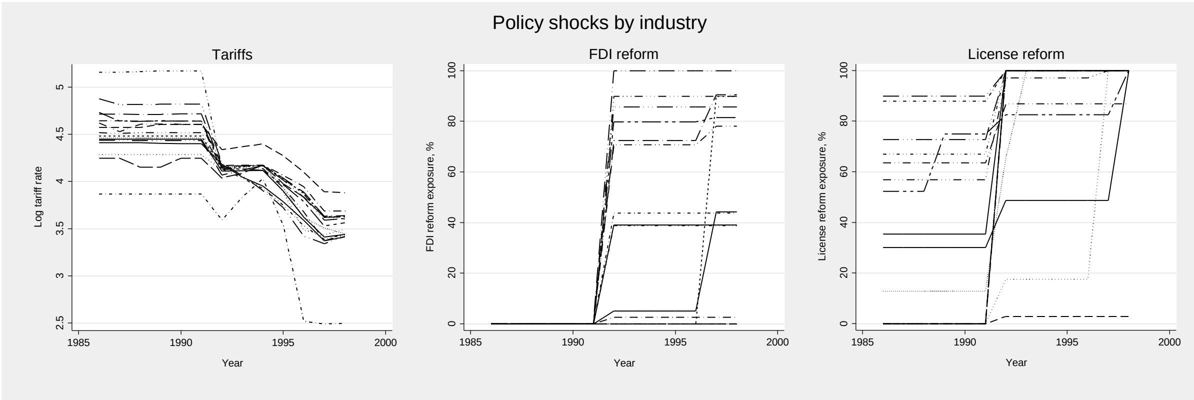
Source: author's calculations based on Factories Act data published by the Ministry of Labour.

Figure A8: Estimated bias factors in the Factories Act data



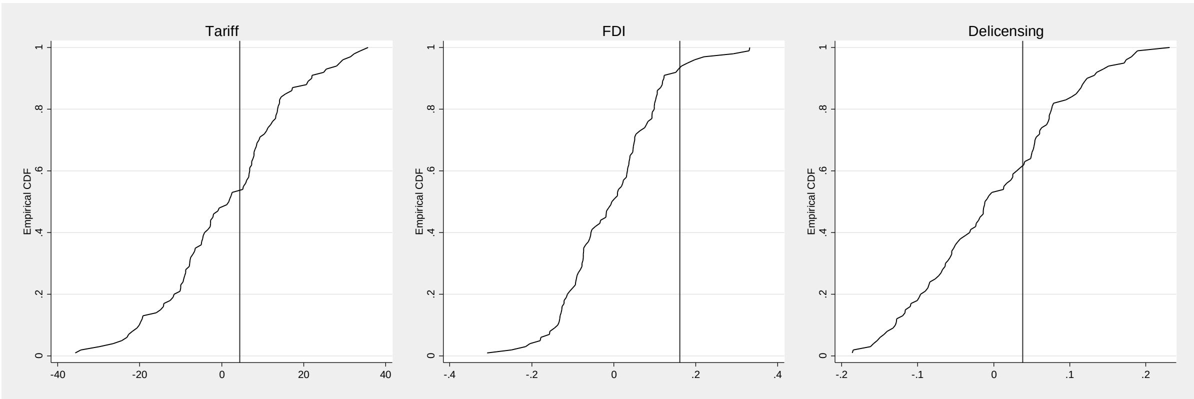
Source: author's calculations based on Factories Act data published by the Ministry of Labour.

Figure A9: Evolution of policy variables, by two-digit industry



Source: author's calculations using tariff, FDI, and delicensing data from Harrison et al. (2013).

Figure A10: The empirical cumulative distribution function corresponding to the permutation distribution of treatment effects for each of the policy reform variables



Note: the vertical line in each panel represents the estimated treatment effect corresponding to the actual treatment assignment.

Source: author's estimates based on Factories Act data published by the Ministry of Labour.

Table A1: Characteristics of injured individuals (SDP data)

	2002	2018
Male	0.87	0.87
	(0.01)	(0.01)
Rural	0.67	0.73
	(0.01)	(0.01)
Age at time of survey	48.70	49.99
	(0.34)	(0.28)
Age at time of injury	36.30	37.99
	(0.35)	(0.26)
Lost or changed jobs as a result of injury	0.73	0.78
	(0.01)	(0.01)
Sample size	2,191	2,487

Note: the table reports sample means and standard errors (in parentheses) of the characteristics of individuals who reported workplace injuries in the 2002 and 2008 rounds of the Survey of Disabled Persons (SDP).

Source: author's calculations based on data from the 2002 and 2008 rounds of the Survey of Disabled Persons (SDP).

Table A2: Testing for pre-reform parallel trends

Dependent variable: Fatality rate	(1)
$(\Delta\text{Log tariff})_{1985\text{-}91} \times t$	-4.906
	(0.419)
$(\Delta\text{FDI reform})_{1985\text{-}91} \times t$	0.011
	(0.873)
$(\Delta\text{License reform})_{1985\text{-}91} \times t$	-0.026
	(0.616)
Constant	-3.160
	(0.577)
Observations	16

Note: each observation represents an industry. The dependent variable is the long difference between fatality rates in 1991 and 1986, and the independent variables are long differences in the treatment variables between 1996 and 1991. Robust standard errors are in parentheses.

Source: author's estimates based on Factories Act data published by the Ministry of Labour.

Table A3: Effect of liberalization reforms—sensitivity check

Dependent variable: Fatality rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Log tariff rate	9.455	4.409	12.053	9.053	4.348	8.758	7.003	8.243	9.795	8.505	10.547	11.313	3.325	6.717	9.204	6.287
	(6.601)	(6.592)	(7.022)	(6.428)	(5.424)	(5.966)	(5.480)	(6.562)	(6.354)	(6.665)	(7.318)	(6.882)	(7.339)	(5.972)	(5.939)	(5.297)
FDI reform (% deregulated)	0.114**	0.138***	0.151***	0.072*	0.136***	0.087**	0.136***	0.126**	0.111**	0.116***	0.155***	0.171***	0.120***	0.119**	0.199***	0.141***
	(0.039)	(0.031)	(0.040)	(0.039)	(0.039)	(0.038)	(0.033)	(0.058)	(0.040)	(0.038)	(0.036)	(0.016)	(0.038)	(0.052)	(0.010)	(0.026)
License reform (% deregulated)	0.075*	0.076	0.083*	0.053	0.069	0.073*	0.046	0.060	0.089**	0.075*	-0.013	0.066***	0.075	0.065	0.068*	0.064*
	(0.035)	(0.056)	(0.043)	(0.033)	(0.088)	(0.040)	(0.048)	(0.044)	(0.035)	(0.036)	(0.066)	(0.018)	(0.047)	(0.041)	(0.038)	(0.034)
Observations	150	150	150	150	150	150	150	150	150	150	150	150	150	150	150	150

Note: the table reports the results from the sensitivity checks of the two-way fixed effects regressions of fatality rates on the three policy variables, excluding one industry from the sample at a time. Each observation is an industry-year, and the regressions are weighted by average industry employment over the period 1986-1991. The sample is restricted to the period 1986-1995. All regressions include industry and year fixed effects, and interactions between pre-reform quartiles of the policy variables with an indicator for the post-1991 period, as well as interactions between quartiles of pre-reform fatality rates and the post-1991 indicator. Standard errors in parentheses are clustered at industry level. *** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates based on Factories Act data published by the Ministry of Labour.

Table A4: Assessing the extent of independent variation in treatment variables

	(1)	(2)	(3)
	ΔLog tariff	ΔFDI reform	ΔLicense reform
ΔLog tariff	-0.003		-0.304
	(0.002)		(0.269)
ΔFDI reform	0.001	-0.311	
	(0.001)	(0.259)	
ΔLicense reform		-54.711	16.148
		(38.230)	(19.651)
Constant	-0.520***	23.753	64.564**
	(0.120)	(34.661)	(25.150)
Observations	16	16	16
R-squared	0.216	0.281	0.161

Note: each observation in the regressions above corresponds to a two-digit industry. The dependent and independent variables are (industry-specific) changes in each of the policy variables over the period 1991–95. The regressions are weighted by average industry-level employment over the period 1986–91. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: author’s estimates based on Factories Act data published by the Ministry of Labour.

Table A5: Effect of liberalization reforms on fatality rates—robustness

Dependent variable: Fatality rate	(1)	(2)	(3)	(4)	(5)	(6)
Log tariff rate	4.702	4.367				
	(3.039)	(3.342)				
FDI reform (% deregulated)			0.025	0.065***		
			(0.015)	(0.020)		
License reform (% deregulated)					0.012	0.031
					(0.027)	(0.034)
Controls	D_{fdi}*D_{delic}**Post*	D_{fdi}*D_{delic}**Post*,	D_{tariff}*D_{delic}**Post*	D_{tariff}*D_{delic}**Post*,	D_{tariff}*D_{fdi}**Post*	D_{tariff}*D_{fdi}**Post*,
		$D_{fataity}$*D_{tariff}**Post*		$D_{fataity}$*D_{fdi}**Post*		$D_{fataity}$*D_{delic}**Post*
Observations	160	160	160	160	160	160

Note: the table reports the results of two-way fixed effects regressions, which estimate the effect of each policy variable separately. D_{fdi}, D_{delic}, D_{tariff}, and $D_{fatality}$ denote indicators for above-median pre-reform values of FDI regulation, license regulation, log tariff rate, and fatality rate, respectively, and *Post* is an indicator for sample years after 1991. Each observation is an industry-year, and the regressions are weighted by average industry employment over the period 1986–91. The sample is restricted to the period 1986–95. All regressions include industry and year fixed effects. Standard errors in parantheses are clustered at the industry level. *** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates based on Factories Act data published by the Ministry of Labour.