Has Devolution Injured American Workers? State and Federal Enforcement of Construction Safety

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Although the issue of regulatory devolution has received much scholarly scrutiny, rigorous empirical studies of its effects on important policy outcomes are scarce. This article explores the effects of partial regulatory devolution in the occupational safety arena by exploiting a unique historical anomaly whereby some US states enforce protective labor regulations that are enforced elsewhere by the federal Occupational Safety and Health Administration. Analyzing data from the construction industry, this article contains several important findings. First, state inspectors use traditional enforcement tools more sparingly than their federal counterparts, typically citing fewer violations and collecting lower fines per violation. Second, although federal enforcement significantly lowers the estimated frequency of nonfatal construction injuries, it also predicts a significant *increase* in occupational fatalities. I suggest that although higher underreporting of nonfatal injuries in federally regulated states could explain this puzzling finding, it is equally possible that different regulatory styles have different "comparative advantages" in deterring nonfatal injuries on one hand and occupational fatalities on the other. (*JEL* D73, D78, H73, I18, J08, J28, J88, K00, K23, K31, K32, L51, and L74)

1. Introduction

Although the merits of transferring regulatory authority from federal to state governments have been widely debated, few studies have sought to quantify

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the effect of devolution on key policy outcomes. This article explores the effects of partial devolution in the field of occupational safety and health, an area that has received little scholarly attention.

For nearly three decades, 21 states (concentrated in the West and in a U-shaped cluster spanning the upper South Atlantic and East North Central regions) have relied on state officials to enforce occupational safety and health regulations. The Occupational Safety and Health Administration (OSHA), a federal agency, oversees enforcement within the remaining jurisdictions.[1] The genesis of this bifurcated enforcement regime was the 1970 Occupational Safety and Act (OSH Act), which simultaneously established OSHA and permitted individual states to opt out of the federal enforcement system by developing their own, independently run programs (state plans). Although the federal government pays up to half of the cost of each state plan, Section 18 of the Act specifies that each state plan must be "at least as effective" as the federal regime.[2] The OSH Act authorized OSHA to decertify ineffective state plans and permitted states to adopt or withdraw state plans at any time. In practice, however, the distribution of federal OSHA and state-plan states has remained virtually static since the 1970s.[3] Unlike many other devolutionary contexts, the content of the regulations varies little across states.

The only prior study to compare the average effectiveness of state plans to the federal OSHA floor finds that state-plan states have lower rates of workplace fatalities than federal OSHA states, controlling for a variety of state-variant characteristics (Bradbury 2006). This article brings together several data sets on the construction industry, one of the most dangerous industrial sectors and a primary target of OSHA regulation, in an effort to round out the picture of the effects of

1. The following jurisdictions enforce OSHA regulations in private-sector workplaces: Alaska, Arizona, California, Hawaii, Indiana, Iowa, Kentucky, Maryland, Michigan, Minnesota, Nevada, New Mexico, North Carolina, Oregon, Puerto Rico, South Carolina, Tennessee, Utah, Vermont, Virginia, Washington, and Wyoming. In the remaining jurisdictions, OSHA officials conduct private-sector enforcement activities. (State plans in Connecticut, New York, New Jersey, and the Virgin Islands cover only public-sector employers.)

2. See OSH Act of 1970, Section 23(g). For several reasons, the 50–50 allocation figure only roughly approximates the true distribution of program costs. Not only are state-plan states permitted to keep revenues from fines collected but also some states (such as Washington) allocate additional money from other budgetary sources, such as state workers' compensation (WC) funds, to the program. Given the difficulty of calculating the true "net" costs of enforcement in individual state-plan states, I focus my analysis instead on the number of actual inspections conducted, for which reliable data are available.

3. There have been two brief exceptions. First, in the wake of a widely publicized fire at a chicken processing plant in North Carolina in 1991, which killed 25 workers, OSHA temporarily asserted concurrent enforcement jurisdiction pending the state's demonstration that it was taking action to correct deficiencies in its program (see 56 FR 55192). By June of 1992, the state had given "fully satisfactory" assurance that the necessary corrective actions would be taken, although OSHA did not formally relinquish its concurrent jurisdiction until March 7, 1995 (see 29 CFR, Part 1952). The second instance in which a state-plan state changed its status occurred in 1987, when California Governor George Deukmejian issued an executive order signaling his intention to terminate California's state plan. Within 2 years, however, the political winds shifted and funding for the program was restored.

partial devolution. My analysis suggests that in general, state-plan inspectors apply traditional enforcement techniques less stringently than their federal counterparts. Most importantly, although fatality rates are generally lower with state enforcement, the prevalence of nonfatal injuries is also *higher* in state-plan states. I suggest that higher underreporting of nonfatal injuries in federally regulated states could explain this puzzle. Alternatively, however, it is possible that different regulatory styles have different "comparative advantages" in deterring nonfatal injuries on one hand and occupational fatalities on the other.

2. Theoretical Background

The issue of regulatory devolution implicates a cluster of theories with widespread currency in the social sciences and contemporary legal scholarship. Generally speaking, there are two opposing schools of thought. The pro-devolutionary perspective stresses the efficiency gains to be captured by reducing the size of the jurisdictional unit. Decentralizing the task of enforcement, scholars such as Scholz (1989) and Revesz (2001) have argued, places responsibility in the hands of those with the most complete access to information, as well as the greatest ability to tailor enforcement strategies to local conditions. Since the early 1970s, business leaders and OSHA officials have highlighted such arguments in supporting the development of state plans under Section 18, describing state enforcement regimes as more responsive, innovative, and well informed than federal OSHA.

In contrast to this sanguine view, antidevolutionary writers have stressed the potential risks and inefficiencies of administering social programs at the state level. Soon after the OSH Act's passage, for example, the AFL-CIO Executive Council (1977:2405) denounced Section 18 as "contrary to the interests of workingmen and women." A concern regarding a possible "race to the bottom" has been a prominent theme among antidevolutionary scholars (Peterson 1995; Swire 1996; Engel and Saleska 1998). Some public choice theorists have criticized devolution on the grounds that interest groups most likely to favor strict regulation exert greater influence at the federal level (Stewart 1977:1213; Esty 1996:597–598; Swire 1996; Sarnoff 1997:285–286; Engel and Saleska 1998). Finally, the theory of regulatory "capture" also tends to counsel against devolution if, as is commonly assumed, the conditions conducive to regulatory capture are strongest at the state level.[4]

These two schools of thought yield quite different empirical predictions regarding the comparative effectiveness of state and federal regulation. The pro-devolutionary perspective implies that state governments' lower information costs and superior adaptability will enable them to choose a more efficient and effective portfolio of enforcement techniques than federal OSHA. Meanwhile, the antidevolutionary perspective suggests that "captured" state officials, who must compete with other states to attract mobile firms, will probably enforce

4. Stigler (1971) remains one of the seminal works on regulatory capture theory. For analyses of the increased risk of regulatory capture in smaller political jurisdictions, see Madison (1738) and Bardhan and Mookherjee (2002).

regulations less vigilantly than their federal counterparts. Of course, some states may *choose* not to impose costly safety measures on local businesses, even at the price of increasing injuries, because they believe that regulation reduces social welfare. Yet, whatever the net efficiency consequences of safety regulation—an issue that this article does not address—the antidevolutionary perspective suggests that enforcement will be conducted more stringently (and for a given level of expenditure, more effectively) by federal actors.

Importantly, fines and penalties are not the only devices in the regulatory toolbox. Inspectors may also use "cooperative" approaches such as consultation programs, voluntary partnerships, and the like. The most important form of cooperative enforcement under the OSH Act is the OSHA Consultation Program, which permits any state (regardless of whether it has adopted a state plan) to conduct sanctions-free, on-site consultation visits at the employer's request, free of charge. Under the program, state officials conduct three types of visits: "initial" visits, "follow-up" visits, and "training" visits. Initial visits consist of "mock inspections" during which hazards are identified and suggestions are given for improvement. Follow-up visits are designed to monitor progress made since the prior visit and identify areas for further improvement. Finally, training visits consist of instructional seminars presented to the firm's employees. Although a state may subsidize consultation visits out of its general state-plan budget, the federal government has also earmarked special funds with which it pays up to 90% of the cost of many such programs.[5]

Interestingly, both pro- and antidevolutionists might expect officials in state-plan states to utilize the Consultation Program more than their counterparts in OSHA-regulated states, albeit for different reasons. On one hand, if state-plan officials are truly best equipped to use nontraditional approaches and tailor regulatory strategies to local conditions, they might view the Consultation Program as an ideal vehicle through which to undertake regulatory innovations. On the other hand, if officials in state-plan states are especially beholden to local business interests, they might seize upon the Consultation Program as a convenient form of "window dressing" that helps satisfy the requirements of Section 18 of the OSH Act without antagonizing local firms.

In short, given the wide divergence of theoretical perspectives and the paucity of empirical scholarship, it remains a largely open question how transferring authority from federal to state governments drives real-world behavior.

5. Interestingly, for two distinct reasons, one might expect consultation visits to be less efficacious than inspections in the construction industry. First, as Scholz (1991) has argued, cooperative regulation is achievable only when both sides in the regulatory relationship anticipate that they will interact on a long-term, if not a permanent, basis. Of all OSHA-regulated industries, construction most closely resembles a one-shot game, since the physical worksite, production process, and workforce typically change with each project. Secondly, given the inherently short "life span" of each construction site, a disproportionately high fraction of inspections in the construction sector are likely to be first inspections of the inspected site. Scholars such as Gray and Jones (1991), Stanley (2000), and Weil (2001) have consistently found that the first inspection has the greatest effect on workplace safety. Consequently, one might expect inspections to have a particularly large impact on firm compliance in the construction industry.

The task of this article is to explore the practical consequences of partial regulatory devolution in the arena of occupational safety and health.

3. Overview of Empirical Methodology and Description of Data

The empirical analysis is divided into four sections, each of which compares a different set of outcomes. The first empirical section (Section 4) compares the use of regulatory "inputs" in each regime, such as the frequency of inspections and consultation visits, the expected value of penalties, and the overall stringency of inspection behavior. The second empirical section (Section 5) examines the effect of an inspection on the number of violations *subsequently* found at the inspected site or firm, that is, the "specific deterrence" effect of regulation. The third and fourth sections of the empirical analysis (Sections 6 and 7) test whether state-plan status has a statistically significant impact on the frequency of nonfatal injuries and fatalities, respectively.

The investigation relies primarily on four data sets. The first data set contains detailed establishment-level data on inspections conducted by both federal and state personnel from 1987 through 1993, for all construction firms that fall at or above the 80th percentile in annual revenues. "Inspection sequence" variables were generated to indicate whether any given inspection was the first, second, third, fourth, and so forth, conducted at that construction site or at that firm across all of its sites statewide.[6] The second data set tabulates the number

6. The data set was obtained from OSHA by David Weil of Boston University, who compiled it using data extracted from OSHA's Integration Management Information System (IMIS). Through an extensive collaboration with national construction unions, Weil and his colleagues standardized all of the firm names in the original IMIS data, enabling one to accurately track the inspection history of each firm at a single site, at all of its sites in a single state, and nationwide. To date, OSHA's IMIS database neither standardizes its name and address fields nor uses site-level identifiers, making it very difficult to track firm behavior accurately across states and across time. For these reasons, I chose to conduct the inspection analysis using the superior data set refined by David Weil, even though it only encompasses the years 1987 through 1993. Even in the Weil data set, however, there is no foolproof method of tracking inspections of a single inspection site over time. For the purposes of my analysis, I treat inspections of the same firm, occurring in the same zip code, as occurring at the same construction site. Since a single firm may be involved simultaneously (or sequentially) in two different construction projects in the same zip code, this methodology is not foolproof. Detailed scrutiny of the data suggests that it is likely to be accurate in the large majority of cases. Moreover, there is, at present, no feasible alternative. (Although some researchers have developed complex algorithms based on the "address" field, my own analysis suggested that this approach performs poorly on construction data, since many large construction sites encompass several street addresses with completely distinct street names and numbers.) In addition to defining construction "sites" in this manner, I also dropped the following small groups of observations: (1) those that took place in state-plan states but were coded as having been conducted by federal OSHA, presumably because they took place on federally owned land, parks, waterways, and American Indian reservations; (2) those that took place in federal OSHA states but were coded as being conducted by state officials, suggesting possible data entry errors; (3) those for which state and/or year were missing; and (4) those for which only a "records check" inspection, or no inspection at all, was conducted. (I included both "complete" and "partial" inspections in the data set, since partial inspections typically involve a thorough inspection of one facility in a multistructure campus, and inspectors may, and often do, convert a partial inspection into a complete inspection after it has begun.) The handful of observations lacking SIC codes were also omitted from all regressions in which SIC codes were used as control variables and from all summary statistics that were calculated across SIC code groupings.

of on-site Consultation Program visits conducted by state inspectors—including initial, follow-up, and training visits—from 1987 through 1998.[7] Although both data sets were culled from OSHA's IMIS system, there are crucial differences between them. Whereas the inspection data set records extensive information on the characteristics of each worksite and each inspection, the consultation data set contains no such details. Most importantly, although the inspection data record the name and address of each firm inspected—enabling one to construct an "inspection history" for each firm—the identity of all firms that receive consultation visits is kept confidential.

The third major data set relied upon is the Survey of Occupational Injuries and Illnesses (nonfatal injury data), a confidential establishment-level survey collected by the Bureau of Labor Statistics (BLS). The nonfatal injury data set contains the number of injuries reported by each establishment included in a stratified sample of construction firms between 1987 and 1998. Injuries are divided into three categories: those that cause an absence from work; those that trigger a restriction of work or job transfer; and those that do not affect working capacity. (For convenience's sake, I refer to the first two types of injuries as "lost work" injuries and to all three categories combined as "total" injuries.) The nonfatal injury data also record the average number of workers employed and the total number of hours worked at each sampled establishment. Unlike the IMIS data, the BLS injury data include firms of all sizes. Although it was not feasible to match the individual firms included in the IMIS data set with those included in the BLS injury data, in order to probe whether the largest construction firms exhibit idiosyncratic trends, I conducted separate regressions on the subset of firms whose total employment placed them in the top 20% of all sampled establishments.

The fourth and final major data set examined is the Census of Fatal Occupational Injuries (CFOI). A confidential data set also collected by the BLS, the CFOI enables me to compare trends in fatality rates from 1992 (the first year in which it was administered) through 1998. Both the CFOI and the data sets that Bradbury uses in his study, the National Institute of Occupational Safety and Health's National Traumatic Occupational Fatalities (NTOF) surveillance system, are censuses of occupational fatalities. However, the CFOI is a more detailed and complete data source than the NTOF and therefore likely to yield more reliable estimates of cross-regime differences.[8]

7. The IMIS data record a handful of instances in which a firm requested a consultation by phone, but no visit actually took place. Such cases are omitted from the analysis.

8. First, the NTOF data are available only in aggregated form or, when nonaggregated data are provided, small-count data are masked or obscured for reasons of privacy and confidentiality. These obstacles preclude comprehensive comparisons of state-level fatality rates by industry and year. In contrast, the BLS has made the complete CFOI microdata available on-site to authorized outside researchers. Second, as Layne (2004) has noted, the NTOF lists only the industry in which the decedent worked for the plurality of his or her working years, whereas the CFOI lists the industry of the job held by the decedent at the time of death. The latter is much better suited to a comparison of occupational safety across states. Finally, Layne (2004) finds that since the NTOF data are compiled solely from death certificates, they contain about 15% fewer observations than the CFOI, which obtains data from a variety of sources. For these reasons, the CFOI is likely to be a more credible measure of occupational fatalities than the NTOF.

Although the OSH Act presents a relatively clean case of regulatory devolution, it is not a natural experiment: state-plan states were not randomly assigned to state-plan status, but rather each state chose whether to opt out of the federal system. It is therefore important to consider two potential types of selection bias.

First, it is possible that some states are inherently more "injury prone" than others and that this was precisely what motivated them to adopt state plans in the 1970s (or, alternatively, to remain within federal OSHA). If so, then any observed correlation between state-plan status and injury rates would not convey meaningful information about regulatory devolution per se, but simply reflect the inherently higher (or lower) risk of performing construction work in a state-plan state. Given the absence of detailed state-level injury data prior to the 1980s, one cannot disregard this possibility.

As a practical matter, however, this form of selection bias seems unlikely. First, even if state policymakers had tried to determine the relative "injuriousness" of their state before deciding whether to submit a state plan in the 1970s, the absence of contemporary data on state-level injury rates would have all but precluded such a comparison. Second, although I conducted extensive interviews with state-plan officials in an effort to determine why their predecessors chose to adopt state plans, none cited this as a factor that motivated the state's decision.[9] Finally, interviews with safety experts in the construction industry did not corroborate the assumption that construction work is "inherently" more dangerous in some states than in others.[10]

The second possible form of selection bias could arise from political differences across states. Suppose, for example, that some state policymakers in the early 1970s shared the belief that: (1) given the importance of creating a "business friendly" environment, lax enforcement was preferable to strict enforcement and (2) lax enforcement would be easier to achieve with a state plan. If one observed higher injury rates in state-plan states, this would simply reflect the state policymakers' preexisting tastes for lax enforcement.

Interestingly, available evidence provides scant support for the theory that state-plan states were more "pro-business" than their peers during the 1970s. Not only were many state-plan states forerunners in the field of occupational safety and health but also many won the support of local labor leaders before submitting state plans.[11] Qualitative studies by Thompson and Scicchitano

9. Notes from interviews with state-plan officials, on file with the author, are available upon request.

10. I consulted with two experts on this issue: Barrien Zettler, OSHA's former Deputy Director of the Directorate of Construction, who spent his 30-year career at OSHA conducting inspections in federal OSHA states, monitoring South Carolina's state plan, interpreting OSHA standards in construction, and coordinating OSHA's inspection activities in the construction industry; and Stephen Cloutier, who spent more than 25 years managing construction safety programs at large national construction firms (including J. A. Jones and Bechtel) before joining OSHA in 2003 as a Senior Occupational Safety and Health Specialist. Both interviews were conducted telephonically on May 11, 2005.

11. Detailed qualitative information regarding state-plan states' motivations for opting out of the federal system, compiled from interviews and other sources, is available from the author upon request.

(1985) and Scholz (1986) also fail to detect evidence of a pervasive antiregulatory bias among state-plan states.

Yet, it is crucial to recognize that even if this latter form of selection bias does exist, it would not undermine the theoretical validity or real-world relevance of this study. Without Section 18 of the OSH Act, all enforcement would have been federal enforcement. Even if some states had a preexisting preference for lax regulation, they could not have acted on this preference without explicit statutory authorization to do so. In other words, even if its sole effect was to permit some states to satisfy their taste for lax enforcement, the optional, partial devolutionary regime codified in the OSH Act could still be meaningfully interpreted as having "caused" any observed disparity in outcomes between state-plan and federal OSHA states.

4. Comparing Enforcement Inputs in State and Federal OSHA

Comparing enforcement inputs across regimes is an important first step in understanding the real-world consequences of regulatory devolution. Table 1 introduces the analysis by comparing patterns of regulatory behavior across state-plan and federal OSHA states. Table 1 focuses on inspections (with all dollar figures converted to constant 1987 dollars) and compares the frequency of consultation visits.

Table 1 suggests that traditional regulatory tools are used more sparingly in state-plan states. Although state-plan officials conduct more inspections than their federal counterparts, the probability of an inspected company *receiving a penalty* is markedly higher in federal OSHA. Most important—regardless of whether one focuses on the penalty initially imposed or the penalty collected after postinspection bargaining has taken place between the firm and the inspector—fines are dramatically lower in state-plan states. Although state-plan states are only slightly more likely to "discount" a fine once it has been imposed, the average percentage discount in state-plan states is more than 60% larger than that in the federal OSHA regime. The comparisons that focus on serious violations exhibit similar trends, with federal inspectors citing roughly twice as many serious violations per inspection, and per inspection hour, as their state counterparts. Finally, a firm's expected value of noncompliance (measured as the probability of an inspection times the mean penalty) is more than three times as high in the federal OSHA regime.[12]

12. All means presented in Table 1 were calculated, for each regime, across all inspections that took place across all years. Consequently, larger states (which typically conduct more inspections) are implicitly weighted more heavily than smaller states. Alternatively, in order to weight every state equally regardless of population, one can calculate mean values for each state/year/Standard Industrial Classification (SIC)–code combination, and then "average the averages" within each regime. Using this approach slightly alters the mean values but does not alter the pattern of cross-regime disparities, displayed in Table 1. Moreover, with only one exception—the annual inspection probability for companies in the sample—all cross-regime disparities remain statistically significant.

Interestingly, however, Table 1 does not bear out the expectation that state-plan states rely more heavily on nontraditional enforcement methods. In fact, the mean number of consultation visits varies little across the two regimes. The distribution of visit types is also substantially similar: about 85% are initial visits, roughly 9% are devoted to training, and the remainder consists of follow-up visits. In short, the data provide little support for the hypothesis that state-plan states use cooperative techniques more intensively than their federal counterparts.[13]

Comparing summary statistics in isolation, however, could be misleading. Although the cross-regime disparities revealed in Table 1 are consistent with regulatory laxity, they also could indicate that compliance is truly higher in state-plan states. Table 2 tries to distinguish between these two possibilities. Suppose, for example, that a firm found to have committed three serious violations is fined $1000 in the state-plan regime. Is its counterpart in the federal OSHA regime penalized a greater or lesser amount for the same number of serious violations? If enforcement is equally stringent across regimes, one would expect firms with similar violations (and prior inspection histories) to incur similar penalties.

To undertake this comparison, Table 2 models the amount of penalties collected during initial and repeat inspections, respectively, as a function of serious violations, nonserious violations, year, and SIC code. For repeat inspections, I also control for the average number of violations cited across all prior inspections. The probit models estimate the probability of any fine at all being imposed, whereas the OLS models examine the magnitude of fines *only* in cases where they were imposed. The models tell a consistent story: not only are state-plan states less likely to collect any fines but also even when fines are collected, they are smaller than those imposed in federal OSHA states.[14]

To summarize the findings so far, the examination of regulatory inputs reveals disparate patterns. Although the frequency and distribution of on-site consultation visits vary little across regimes, traditional enforcement methods are applied less stringently in state-plan states. Not only do state-plan states issue fewer violations but also even when one confines the comparison to

13. To calculate the summary statistics presented in Table 1, I first calculated mean values for each state/year cell and then calculated the mean value of all such cells within each regime.

14. The robustness checks performed on the results presented in Table 2 included the following: (1) a specification replacing the fifth and sixth independent variables in the model with analogs computed from inspections (a) at the *current construction site only* or (b) at *all of the firm's construction sites nationwide*; (2) a specification adding independent variables for the number of (a) serious and (b) nonserious violations in the inspection *immediately prior* to the current inspection; and (3) specifications iteratively modeling groups of inspections (a) with equal inspection sequence number and (b) of sites with equal total number of inspections. I also estimated an alternative version of the OLS model in which the dependent variable was the fines collected (rather than the log of fines collected). In addition, the following single-component models were run: an OLS regression model with the natural logarithm of (1 + fines collected) as the dependent variable; a Tobit model with fines collected as the dependent variable; and a truncated regression model with fines collected as the dependent variable. The significant cross-regime disparity reflected in Table 2 was robust across all such specifications. Since the logarithmically transformed version of the dependent variable exhibited a considerably more linear relationship to the four continuous independent variables in the model and exhibited higher R^2 values, I chose to present that model in Table 2.

Table 1. Cross-Regime Comparisons of Regulatory Behavior

	Federal regime		State regime	
Means of inspection and compliance parameters, 1987–93				
Total inspections conducted in sample	12,567		14,647	
Annual inspection probability among sampled companies	34.89%	(0.065)	41.58%	(0.106)
Annual inspection frequency among sampled companies	3.044	(2.59)	6.478	(8.24)
Probability that an inspection results in a penalty	20.22%	(0.151)	17.46%	(0.156)
Average penalty *imposed* per worker per inspection	$142.5	(1860)	$42.51	(278)
Average penalty *collected* per worker per inspection	$70.80	(265)	$20.18	(114)
Likelihood of penalty reduction if penalty imposed	86.73%	(0.339)	89.94%	(0.301)
Average percentage penalty reduction if penalty reduced	18.52%	(0.265)	30.61%	(0.371)
Average penalty per worker per inspection if penalty imposed	$162.0	(382)	$75.27	(210)
Probability inspection × mean penalty across all inspections	$26.50	(113)	$7.522	(41.7)
Probability inspection × mean penalty *if* penalty is imposed	$44.14	(131)	$19.29	(88.4)
Probability of no serious violations	58.02%	(0.494)	74.55%	(0.436)
Average serious violations cited per inspection	1.019	(2.55)	0.459	(1.09)
Number of serious violations cited per inspection hour	0.0452	(0.098)	0.0229	(0.062)
% inspections that are first at site	79.08%	(0.407)	65.02%	(0.477)
% inspections that are sixth or higher at site	0.517%	(0.071)	5.750%	(0.233)
% inspections that are first of firm	23.77%	(0.426)	17.28%	(0.378)
% inspections that are 51st or greater of firm	0.573%	(0.075)	5.996%	(0.237)
% inspections triggered by a complaint	7.77%	(0.268)	6.71%	(0.250)
% inspections triggered by an accident	2.32%	(0.150)	3.26%	(0.178)
% inspections of unionized firms	51.80%	(0.500)	52.67%	(0.499)
Average number of workers in inspected establishments	44.73	(421)	32.20	(137)

Means of consultation program parameters, 1987–98				
Total consultation visits conducted	29,108		32,072	
Annual consultation visits per 1000 construction workers	1.95	(5.64)	1.89	(2.44)
Proportion of visits by type				
Initial	83.9%		87.1%	
Follow-up	6.5%		4.1%	
Training	9.7%		8.8%	

The sample for the inspection and compliance parameter means consists of inspections of the top 20% of construction firms by annual revenue in the period 1987–1993. The sample for the consultation program parameter means consists of all consultation visits to construction firms during the period 1987–1998. Data on consultation visits were calculated using data provided by Joseph DuBois, Director of OSHA's Office of Statistics. Inspection variables were calculated using a data set obtained from David Weil, who compiled it using data from OSHA's IMIS. Construction employment figures were calculated using data provided by Thomas Krolik of the BLS. Inspection parameter means represent mean values over all sites inspected during the time period examined. Inspection probabilities were calculated as the number of inspections meeting the parameter criterion within a given state, year, and SIC code, divided by the number of such inspections within the same state and SIC code over the time period examined, averaged across all states within the regime. Firm inspection probabilities were calculated as the number of inspections meeting the parameter criterion within a given state and year, divided by the number of such inspections in the same state over the time period examined, averaged across all states within the regime. Unless specified otherwise, penalties refer to penalties collected. Consultation visit parameters were calculated as mean proportions and ratios averaged over state-year values within each regime. "Inspections of firm" refers to inspections of firm within a given state. Standard deviations are given in parentheses. All cross-regime differences between parameter means were statistically significantly at the 1% level (unpaired *t*-test assuming unequal variances), with the exception of the cross-regime differences between the "% inspections of unionized firms" and "Annual consultation visits per 1000 construction workers" parameter means. Those cross-regime differences were both insignificant at the 5% level.

Table 2. Cross-Regime Comparisons of Enforcement Stringency: Fine Collection Behavior When At Least One Violation Is Found (1987–93)

	Probit model		OLS Regression model	
	"Was a fine collected?"		"If a fine was collected, what was its magnitude?"	
	Dependent variable = 1 if fines were collected, 0 otherwise		Dependent variable = natural logarithm of total fines collected	
	First inspections of firm	Subsequent inspections of firm	First inspections of firm	Subsequent inspections of firm
Number of observations	3538	12,127	1876	6266
R^2/pseudo R^2	0.29	0.21	0.48	0.41
State-plan state dummy	–0.148***	–0.175***	–0.275***	–0.358***
	(0.020)	(0.011)	(0.043)	(0.025)
Serious violations in *current* inspection	0.204***	0.124***	0.299***	0.289***
	(0.007)	(0.002)	(0.010)	(0.006)
Nonserious violations in *current* inspection	–0.038***	–0.032***	0.011	0.002
	(0.005)	(0.003)	(0.009)	(0.006)
Serious violations in *prior* inspections (average)		0.021***		0.069***
		(0.005)		(0.010)
Nonserious violations in *prior* inspections (average)		–0.002		–0.009
		(0.004)		(0.009)
Year and SIC dummies	Included	Included	Included	Included

Sample consists only of those inspections in which at least one violation was found. All inspection variables were calculated using a data set obtained from David Weil, who compiled it using data from OSHA's IMIS. Figures in probit model are marginal effects on probability of a fine being collected. ***Statistical significance at the 1% level. Standard errors are given in parentheses.

similar inspections and similar firms, penalties are typically smaller and less frequently imposed.

5. Comparing Specific Deterrence Effects in State and Federal OSHA

Given these disparities in enforcement stringency, one might wonder whether there are similar differences in the *response* of inspected firms to regulatory interventions. For example, since federal OSHA states typically impose higher penalties, do they also induce greater specific deterrence effects among inspected firms?[15] Similarly, do consultation visits alter firms' behavior more dramatically in one of the two regimes? Although the consultation visit data

15. As noted earlier, the effect of inspections on the firms that experience them is often known as the specific deterrence effect of enforcement. Regulatory actors may be induced to comply with regulations by the knowledge that they *might* experience an inspection, regardless of whether one ever takes place. The latter effect is often referred to as a "general" or "indirect" deterrence effect.

contain too little detail to facilitate such an inquiry, the inspection data are better suited to the purpose.

In order to test the specific deterrence effect of OSHA regulation, prior researchers, such as Gray and Jones (1991) and Weil (2001), have used OSHA inspection data to model the number of violations at any given inspection as a function of its "sequence number" (i.e., whether it is the first inspection, second inspection, third inspection, etc.). Such models assume that, *ceteris paribus*, if inspections are deterring firms from violating regulations, then any given inspection should induce a higher level of compliance at the subsequent inspection. Following this approach, Table 3 explores whether repeated inspections of a single site—and of a single firm across all of its sites statewide—have different marginal impacts across regimes. Each sequence number variable is assigned a value of 1 for the first inspection, 2 for the second inspection, and so forth. The probit model presented in the left-hand panel follows Weil (2001) in examining the likelihood of *any* serious violations, whereas the negative binomial in the right-hand panel follows Gray and Jones (1990) in modeling the *number* of serious violations found. Both models also include a host of factors that are likely to affect the frequency of detected violations, such as whether an on-site accident or complaint triggered the inspection, whether the worksite is unionized, the size of the workforce, the total inspections of the firm in that state during the sample period, penalties assessed at the first inspection, year, and SIC code. Each of the control variables is also interacted with state-plan status.

Both the site-level and firm-level sequence number variables in this model —along with their respective interaction terms—reveal noteworthy patterns. Although the coefficients on "sequence number of inspection of site" and "sequence number of inspection of firm" are negative and statistically significant, the two corresponding interaction terms ("sequence number at site × state plan" and "sequence number of firm × state plan") are significant, positive, and large enough to offset the effect of the former two coefficients. These results suggest that although repeated OSHA inspections of the same construction site and firm incrementally enhance firms' future compliance in federal OSHA states, the same apparently is *not* true among state-plan states.[16]

For several reasons, however, the disparity revealed in Table 3 may be less meaningful than it appears. First, the comparison is complicated by the fact that the distributions of inspection sequence numbers vary so markedly across regimes. As Table 1 reveals, a markedly higher proportion of inspections in state-plan states are high–sequence number inspections. In fact, if one reestimates the models in Table 3, for example, but confines the comparison to the first several inspections of a site (or the first dozen inspections of a firm), then

16. As checks for robustness, I computed the sequence number and "total inspections" independent variables at each possible level of inspection aggregation (site level, state level, and regime level) and estimated both probit and negative binomial regression models using each possible combination of these control variables. In addition, I estimated models containing *both* the site-level sequence number *and* either the state- or regime-level sequence number. In total, 108 robustness check specifications were run. All supported the inference that high–sequence number inspections of sites and/or firms were driving the cross-regime disparity reflected in Table 3.

Table 3. Cross-Regime Comparisons of Specific Deterrence Effects (1987–93)

	Probit model	Negative binomial model
	Dependent variable = 1 if at least one serious violation, 0 otherwise	Dependent variable = number of serious violations
Number of observations	27,200	27,210
Pseudo R^2	0.069	0.048
State-plan state dummy	–0.2433***	–1.0993***
	(0.018)	(0.077)
Sequence number of inspection of site	–0.0236***	–0.0894***
	(0.005)	(0.021)
Sequence number at site × state plan	0.0196***	0.0682***
	(0.006)	(0.022)
Sequence number of inspection of firm	–0.0041***	–0.0095**
	(0.001)	(0.004)
Sequence number of firm × state plan	0.0052***	0.0149***
	(0.001)	(0.004)
Employee complaint trigger	–0.0131	–0.0223
	(0.022)	(0.084)
Employee complaint trigger × state plan	–0.0427	–0.0556
	(0.031)	(0.131)
On-site accident triggered the inspection	0.1750***	0.4192***
	(0.031)	(0.096)
On-site accident trigger × state plan	0.0263	0.1194
	(0.036)	(0.127)
Union shop	0.0085	0.0804*
	(0.009)	(0.033)
Union shop × state plan	–0.0278*	–0.1080*
	(0.012)	(0.050)
Union shop × complaint	–0.0164	0.0479
	(0.030)	(0.114)
Union shop × complaint × state plan	0.0549	0.0724
	(0.047)	(0.175)
Ln(workers)	0.0283***	0.1862***
	(0.003)	(0.012)
Ln(workers) × state plan	0.0103*	0.0281
	(0.005)	(0.019)
Ln(1 + penalties from first inspection)	0.0102***	0.0397***
	(0.001)	(0.006)
Ln(1 + first inspection penalties) × state plan	–0.0003	0.0063
	(0.002)	(0.008)
Total inspections of firm	–0.0016***	–0.0094***
	(0.001)	(0.002)
Total inspections × state plan	–0.0006	0.0052*
	(0.001)	(0.002)
Year and SIC dummies	Included	Included

All inspection variables were calculated using a data set obtained from David Weil, who compiled it using data from OSHA's IMIS. Figures in probit model are marginal effects on probability of at least one serious violation being found. "Sequence number of firm" and "total inspections of firm" are both computed at the state level. ***Statistical significance at 0.5% level. **Statistical significance at the 1% level. *Statistical significance at the 5% level. Standard errors are given in parentheses.

the cross-regime disparity in the corresponding interaction term disappears. Upon closer inspection, it is precisely the high–sequence number inspections that are driving the observed disparity. Since such inspections constitute only a small fraction of inspections conducted in both regimes, this differential may be of modest practical import. Second, the fact that one can only quantify the effect of any given inspection by observing the number of violations found at the *subsequent* inspection could bias the results.[17] Third, the method conventionally used to measure specific deterrence itself rests on particular assumptions about regulatory behavior; altering these assumptions may yield different results.[18]

Finally, the metric of "efficacy" analyzed—although the only one feasible given the available data—may fail to capture important behavioral effects of regulation. For example, suppose that the true objective of high–sequence number inspections is not to reduce the number of violations per se, but to ensure that those contractors revealed as "bad apples" in early inspections never place their workers in imminent danger of catastrophic injury or death. If so, then the protracted, long-term monitoring to which a few firms are subjected in state-plan states may be quite effective in reducing deadly hazards, whereas appearing "ineffective" in models such as those presented in Table 3.

In short, for both practical and theoretical reasons, it is difficult to compare in a methodologically conclusive manner the success of state and federal inspectors in achieving specific deterrence effects. Yet, to the extent that such disparities exist, they are probably confined to high–sequence number inspections, which are more both more numerous and (apparently) have less measurable impact on regulatory compliance in state-plan states. Since the data do not enable one to examine the outcomes of consultation visits, it remains an open question whether cooperative enforcement programs exhibit similar patterns.

17. Suppose, for example, that inspectors only reinspect a construction site or firm if they believe that the first inspection is likely to have had little impact. Under these conditions, the estimated impact of the first inspection will be downward biased because the very firms that respond the most—that is, those that are inspected just once—are omitted from the calculation of the first inspection's "average" specific deterrent effect. To generate more reliable estimates, one could conduct random, independent, sanctionless "audits" of firms with different inspection histories. This approach would enable one to estimate compliance levels among *all* firms or sites that have undergone their first (or second or third) inspection, regardless of whether they are inspected again in later periods.

18. The conventional approach, which I adopt here, treats each firm's observable characteristics and the sequence number of its inspection as exogenous factors helping to predict its observed level of compliance. Alternatively, however, one might begin with the assumption that inspectors consciously seek to maximize the marginal decline in violations between adjacent pairs of inspections and that they enjoy considerable de facto discretion in choosing which firms to inspect. Under these conditions, the outcome of interest arguably should be the average *decline* in violations between inspections. Moreover, the observable characteristics of each firm become "choice variables" that inspectors deliberately manipulate to maximize their marginal impact. Posttreatment variables of this sort are properly excluded from the regression model if one's goal is to measure the effect of the treatment (here, state-plan status) on the outcome (the average marginal decline) (Gelman and Hill 2007). If one reconceptualizes the question in this manner, quite different results are possible. For example, if one simply regresses state-plan status and year dummies on the decline in violations between each pair of inspections, the statistically significant disparity between regimes disappears.

6. Comparison of Nonfatal Accidents

Comparing patterns in inspection behavior and firm compliance is an important first step in understanding the real-world consequences of devolution. Yet even if enforcement is more lax in state-plan states—and even if OSHA truly has a greater impact on regulatory compliance than state-plan regimes—such a disparity does not necessarily provide cause for alarm. Several recent studies have found that at least since the early 1990s, OSHA has had little, if any, effect on injury rates (Smith 1992; Kniesner and Leeth 1995; Gray and Mendeloff 2005). From a public policy perspective, then, the most important "litmus test" of regulatory devolution is not the comparison of regulatory compliance as such, but of occupational safety outcomes.

To explore the latter linkage, I first examine confidential microdata from the BLS. Using a stratified sample design, the BLS injury data record the number of injuries reported annually by each sampled establishment from 1987 through 1998.[19] Each injury rate model includes "regime membership" (i.e., membership in a state-plan or federal OSHA state) as a dummy variable predicting the frequency of injuries while controlling for a range of other independent factors hypothesized to affect the likelihood of injury.[20]

My ability to analyze the BLS injury data in the form of establishment-level injury counts—rather than in the highly aggregated form in which the data are publicly available—enables me to include two important control variables in the analysis. First, I am able to control precisely for the *size* of each sampled firm. As authors such as Leigh (1989) and Weil (2001) have found, size matters in the OSH regulatory arena. Large firms are typically among the safest places to work and exhibit comparatively high rates of regulatory compliance. To control for firm size, I adopt BLS's own technique of dividing firms into five "size classes," which are included as control variables in each model.[21] Second, I am able to account for each firm's highly detailed subindustry by controlling for the 26 different four-digit SIC codes in the construction industry. This is also a major empirical advantage, since the inherent injuriousness of construction work depends in part on the type of construction work undertaken, and the mix of construction projects undertaken may vary across states.

19. The BLS's definition of "establishment" tracks the definition used for unemployment compensation, which is a creature of state law. Therefore, a large construction firm that does business in several states is composed of multiple establishments, each of which is confined by state boundaries and submits its own injury data for BLS purposes. To the best of my knowledge, BLS has never undertaken a study to verify that the injury data it receive comports with this definition. Since the mid-1980s, however, its state-level injury estimates have been premised on the assumption that the definition of establishment in construction (as in other industries) does not cross state boundaries (interview with Jim Barnhardt, OSHA, July 18, 2006).

20. Because I use negative binomial models in all specifications, which are "count" models, the dependent variable is technically the number of injuries. However, in order to account for the fact that the duration of workers' "exposure" to risky construction work varies across firms, I use the log of the number of hours worked in each sampled establishment as the offset term in all regression models.

21. The size classes were constructed as follows—size class 1: 1–10 employees; size class 2: 11–49; size class 3: 50–249; size class 4: 250–999; size class 5: 1000 and up.

I estimate two clusters of models, one examining the total number of injuries and the other focusing on the subset of more serious lost work injuries.[22] Each cluster contains three model specifications: a "baseline" model, a "core" model, and a "broad" model. The baseline model controls only for size class, year, and four-digit SIC code. The core specification adds several control variables that have been robustly linked to injury rates in the occupational safety/epidemiological literature: average worker age and three state-variant characteristics of WC systems (the number of days preceding a worker's receipt of benefits for temporary total disability; the maximum dollar amount available as a fraction of the state average weekly wage; and whether or not the system is deregulated).[23]

Finally, the broad specification contains additional state-variant factors that at least one prior study suggests may affect occupational safety and that OSHA officials have identified as likely to affect injury rates. The four variables that met these criteria were as follows: the percentage of construction workers covered by labor unions[24]; the percentage of employed construction workers who

22. To address the possibility that observations within the same state are not independent—thus downward biasing ordinary "robust" standard errors—I follow Wooldridge (2002) in "clustering" all standard errors by state. As is conventional when using count data characterized by "overdispersion," I use a negative binomial model in all specifications. In addition to size class and four-digit SIC code, all models include year dummies. As a robustness check, I also estimated several "intermediate" model permutations between the "naive" and "core" specifications and between the "core" and "broad" specifications, respectively. Although the exact coefficient estimates fluctuated slightly across models, the state-plan dummy remained significant across all specifications.

23. For literature on the effects of these specific variables on reported injuries, see, for example, Krueger (1990); Ruser (1998); and Barkume and Ruser (2001). For additional studies of the effects of WC systems on reported injury rates, see, for example, Thompson (1981); Butler and Worrall (1985); Viscusi and Moore (1987); Ehrenberg (1988); Krueger and Burton (1990); Ruser (1991); Meyer et al. (1995); Card and McCall (1996); and Thomason et al. (2001). Monthly Current Population Survey (CPS) data from 1987 through 1998 were used to calculate the average age of construction workers by state. For the years 1987 and 1988, the CPS data were obtained from the National Bureau of Economic Research Web site, which contains CPS data from both years in readily accessible format. Data from 1989 through 1998 were obtained from CPS's Basic Survey data sets using the Census Bureau's DataFerrett microdata extraction software. Initially, the data set was restricted to workers employed in the construction industry, in construction-related occupations, above 16 years of age. Next, weighted average ages were calculated by year and state. Although the weighting variable names changed over time as the CPS sampling methodology changed, most weights were final-stage composite weights. For 1994–97, second-stage weights were used because final-stage weights were not available in the microdata. The weights chosen were those recommended by the BLS to calculate state- and year-level averages. Historical data on state waiting periods, maximum WC dollar recovery amounts, and state average weekly wages were obtained from the BLS and from the Department of Labor's Office of Workers' Compensation. Data on the deregulation of state WC systems were obtained from John Ruser of the BLS.

24. Two studies by David Weil (1996, 2001) find that unionized construction firms are more compliant with OSHA regulations than their nonunionized counterparts. Accordingly, I control for each state's average percentage of construction workers belonging to labor unions, calculated using the CPS. The data on unionization in the construction industry were obtained from http://www.unionstats.com, a Web site on US unionization statistics constructed by Professors Barry Hirsch and David Macpherson. The figures posted on the Web site were calculated from the CPS Outgoing Rotation Groups and represent the percentage of workers in the construction industry, by state, who are covered by a union contract.

speak little or no English[25]; the number of on-site Consultation Program visits conducted per 1000 employed construction workers; and a dummy variable indicating whether the state had a law on the books mandating the formation of joint labor-management safety and health committees.[26] The risk in including the latter two variables is that a state's initial decision whether to opt out of the federal enforcement regime may *itself* have affected the intensity of its subsequent participation in the on-site Consultation Program and/or the likelihood of its passing a mandatory "safety and health committee law." If so, then including such "posttreatment" variables in the regression model may bias the estimated effect of the treatment (state-plan status) on the outcome (injury rates) (Gelman and Hill 2007). The fact that state-plan states are more than twice as likely to pass safety and health committee laws, as shown in Table 4, lends some credence to such concerns. Yet, given how little is known about what motivates states to adopt these laws and the fact that the frequency of consultation visits is so similar across regimes, I have chosen to include both variables in the third model specification.

Table 4, which presents an initial "raw" look at the variable means, reveals that the frequency of total injuries is about 16% higher in state-plan states. Limiting the analysis to lost work injuries slightly magnifies this disparity. The regression results presented in Table 5 similarly show striking cross-regime disparities in injury rates. In the models of total injuries, the estimated coefficients on the state-plan dummy are significant at a 10% level in all but the broad specification. Meanwhile, the coefficient is significant at the 10% level in all of the lost work injury models (the smallest *p*-value, appearing in the broad specification, is 0.06). Extrapolating from these results, one would expect the frequency of lost work injuries among construction firms to be about 7%–15% higher, on average, in state-plan states.

The control variables included in the models display disparate patterns. As expected, the coefficients on the five size class dummies (not presented in the table) exhibit the familiar inverted-U–shaped pattern, with very small and very large firms having the lowest reported rates of injury. Two

25. In recent years, OSHA has stressed that workers with poor English skills may be uniquely vulnerable to workplace hazards, issuing "preliminary findings" that "about 25% of the fatalities the agency investigates are in some way related to language or cultural barriers" (OSHA). In order to calculate the proportion of construction workers who spoke little or no English, I used a two-step procedure. First, using data from the 1980, 1990, and 2000 Census Public Use Microdata Samples, I calculated, for each state, the percentage of all employed construction workers above 16 years of ago, working in construction-related occupations, that reported speaking English poorly or not at all. (The "person weight," labeled "PERWT" in the Census data, was used as the sample weight.) Second, I imputed proportions for the intervening (i.e., non-Census) years by assuming that the annual proportions increased by a constant amount between 1980 and 1990 and between 1990 and 2000, respectively.

26. In the early 1990s, many states began to pass laws mandating the establishment of safety and health committees at companies of a certain size or insurance experience rating. These committees were designed as forums for managers and employees to discuss and make joint recommendations regarding safety and health issues. Although there have been no empirical studies of the effects of such laws, their express purpose is to enhance corporate safety practices in affected states. Therefore, I control for whether each state had a law on the books (during a given year) mandating the establishment of such committees.

Table 4. Variable Means from Injury and Fatality Rate Models

	Federal regime		State regime	
Dependent variables				
Injuries per 100,000 construction hours worked				
All injuries	5.61		6.50	
Lost workday injuries	2.74		3.28	
Fatalities per 100,000 construction workers				
All fatalities	17.93		15.10	
Nonvehicular fatalities	14.01		11.23	
Control variables				
WC waiting period (in days)	5.10	(1.83)	4.81	(1.95)
Deregulated WC system dummy	0.15	(0.36)	0.28	(0.45)
Dollar cap on WC as % of state average weekly wage	92.7%	(24.4)	101.1%	(32.2)
Average age of construction workers	36.9	(1.43)	36.3	(1.58)
% unionized workers	19.5%	(12.0)	19.6%	(13.0)
% construction workers who speak no/little English	2.90%	(0.038)	3.09%	(0.037)
State law mandating safety and health committees	0.086	(0.28)	0.190	(0.39)
Number of consultation visits per 1000 workers	1.95	(5.64)	1.89	(2.44)

All injury and fatality data were obtained from the BLS. Historical data on state waiting periods, maximum WC dollar recovery amounts, and state average weekly wages were obtained from the BLS and from the Department of Labor's Office of Workers' Compensation Programs. Data on the deregulation of state WC systems were obtained from John Ruser of the BLS. Data on average ages and percentage of unionization were calculated from the CPS. Data on percentage of construction workers who speak little or no English were calculated from the 1980, 1990, and 2000 Decennial Census Public Use Microdata Samples. Data on mandatory safety and health committees were obtained through online research and personal communications with state officials. Data on consultation visits were calculated using data provided by Joseph DuBois, Director of OSHA's Office of Statistics. Consultation visits per capita were calculated as number of Consultation Program visits per 1000 employed construction workers for each state and year. Means of injury rates were calculated across all sampled establishments and years. Means of fatality rates were calculated using SIC-based industry employment figures from the Local Area Unemployment Statistics Program. Means of control variables were calculated across state-year values over the time period examined. All dependent variables presented in the table were calculated using data from 1987 to 1998, although the fatality rate regressions only use these controls for the years 1992 through 1998. All cross-regime differences between inspection parameters were statistically significant at the 0.5% level (unpaired *t*-test assuming unequal variances), with the exception of the waiting period, percentage unionization, percentage little or no English, and consultation visits per capita variables. Standard deviations are given in parentheses.

of the three WC variables—the "waiting period" and deregulation of the state system—have robust negative impacts on injury rates, dovetailing with the findings of prior researchers (Krueger 1990; Ruser 1998; Barkume and Ruser 2001). The insignificance of the third WC variable—the maximum allowable recovery as a percentage of the state average weekly wage—suggests that a worker's likelihood of reporting an injury is far more sensitive to how soon any wage replacement will become available than to what precise fraction of the wage will be replaced.

The insignificance of the average age coefficient can be plausibly explained by two factors. First, although young workers are more injury prone, the marginal effect of age is not constant across the lifecycle: it is very young workers, especially teenagers, who are the most vulnerable. The data reveal that the mean age of construction workers is about 36–37 years and varies only slightly across states. Second, under federal and state child labor laws, teenagers are banned from the most dangerous types of construction work. Therefore, the

Table 5. Negative Binomial Models of Nonfatal Injuries (1987–98)

	Dependent variable = total injuries			Dependent variable = lost work injuries		
	Model 1(a) 435,740 observed $\hat{y} = 0.3046$	Model 2(a) 433,978 observed $\hat{y} = 0.3064$	Model 3(a) 433,978 observed $\hat{y} = 0.3156$	Model 1(b) 435,740 observed $\hat{y} = 0.1633$	Model 2(b) 433,978 observed $\hat{y} = 0.1657$	Model 3(b) 433,978 observed $\hat{y} = 0.1658$
State-plan regime dummy	0.1264* (0.0738) dy/dx = 0.0410 [Δ = 13.46%]	0.1015** (0.0518) dy/dx = 0.0327 [Δ = 10.67%]	0.0384 (0.0376) dy/dx = 0.0123 [Δ = 3.90%]	0.1410** (0.0640) dy/dx = 0.0247 [Δ = 15.13%]	0.0994*** (0.0380) dy/dx = 0.0173 [Δ = 10.44%]	0.0625* (0.0337) dy/dx = 0.0109 [Δ = 6.57%]
WC waiting period in days		−0.0638*** (0.0135)	−0.0583*** (0.0146)		−0.0681*** (0.0100)	−0.0632*** (0.0109)
WC deregulation dummy		−0.2286*** (0.0592)	−0.1625*** (0.0489)		−0.1775*** (0.0443)	−0.1372*** (0.0415)
WC cap as % of average weekly wage		0.0008 (0.0007)	−0.0005 (0.0005)		0.0003 (0.0004)	−0.0003 (0.0004)
Average age of construction workers		0.0011 (0.0176)	−0.0302 (0.0189)		−0.0059 (0.0122)	−0.0211 (0.0138)
% unionized construction workers			0.0007 (0.0022)			0.0006 (0.0018)
% workers with no/little English			−1.285** (0.5362)			−0.3811 (0.4407)
Safety and health committee law			0.2399** (0.1134)			0.1685** (0.0813)
Consultation visits per worker			0.0205* (0.0120)			0.0144* (0.0080)
Year, size, and SIC dummies	Included	Included	Included	Included	Included	Included

All models presented are negative binomial models. The exposure term is the total hours worked at the sampled establishment. All injury data were obtained from the BLS. Each cell is weighted by the weighting it was accorded during OSHA's stratified sample aggregation process, to reflect the relative frequency of similar firms within the state. Historical data on state waiting periods, maximum WC dollar recovery amounts, and state average weekly wages were obtained from the BLS and from the Department of Labor's Office of Workers' Compensation Programs. Data on the deregulation of state WC systems were obtained from John Ruser of the BLS. Data on average ages and percentage of unionization were calculated from the CPS. Data on percentage of construction workers who speak little or no English were calculated from the 1980, 1990, and 2000 Decennial Census Public Use Microdata Samples. Data on mandatory safety and health committees were obtained through online research and personal communications with state officials. Data on consultation visits were calculated using data provided by Joseph DuBois, Director of OSHA's Office of Statistics. Consultation visits per capita were calculated as number of Consultation Program visits per 1000 employed construction workers for each state and year. Means of injury rates were calculated across all sampled establishments and years. Means of fatality rates were calculated using SIC-based industry employment figures from the Local Area Unemployment Statistics Program. ***Significance at the 1% level. **Significance at the 5% level. *Significance at the 10% level. Standard errors are clustered by state and given in parentheses. dy/dx, given for the state-plan regime dummy, represents change in the predicted number of events ($\hat{y}$) associated with a discrete change of the state-plan regime dummy from 0 to 1. The associated predicted percentage ($\Delta = (dy/dx)/\hat{y}$) is presented in brackets.

irrelevance of age in the above models is understandable despite its overall importance in the safety and health literature.

On its face, the significant negative coefficient on the "percentage of workers with little or no English" in the total injury rate model—and its insignificance in the lost work injury model—seems puzzling. OSHA has frequently cautioned that workers with poor English skills may have trouble understanding safety instructions and communicating with supervisors, leaving them uniquely vulnerable to workplace hazards (OSHA). Underreporting of injuries among non-English-speakers provides one possible explanation. If workers with poor English language skills are more likely to be undocumented and have greater difficulty finding alternative employment, they may be particularly reluctant to report minor injuries. More serious injuries resulting in a loss of work, however, may be less susceptible to underreporting.

The insignificance of union membership in predicting nonfatal injury rates contrasts with several prior studies by Weil (1996, 2001), finding that unionized workplaces are more likely to comply with OSHA regulations. This finding suggests that the link between unionization and nonfatal injuries is more attenuated than that between unionization and compliance or that unionization's effects are too subtle and context-specific to show up in a comparison of statewide averages.

Most counterintuitive is the fact that the safety and health committee law dummy and the control for the frequency of consultation visits both carry positive and significant coefficients. Although both initiatives are intended to help firms improve their safety practices, little is known about what motivates states to emphasize such regulatory approaches. If states with high injury rates are disproportionately likely to embrace such techniques, this could help explain this curious finding.

Importantly, Table 5 does not isolate the effect of enforcement regime on the subset of very large construction firms analogous to the group included in the IMIS inspection data. As noted earlier, prior research suggests that large firms typically have high rates of regulatory compliance and relatively low injury rates. To probe whether firm size affects the cross-regime disparity, I estimated the same models on a subsample of construction establishments with at least 40 employees, placing them in the largest 20% of all sampled firms. As earlier work would lead one to expect, there was no significant cross-regime disparity in injury rates for this group of firms. (Although the coefficient on "state-plan status" was positive, it was small and statistically insignificant in all specifications.) This result suggests that to the extent that state enforcement magnifies construction injury rates, the effect is not universal, but is most pronounced among smaller and medium-sized construction firms.

7. Comparison of Occupational Fatalities

In the final stage of the empirical analysis, I use the CFOI to examine state-level fatality rates. I model the total number of construction workers killed on the job—by state and year, and accounting for differences in the size of the workforce—from 1992 (the first year for which data are available) through 1998. In most respects, the modeling technique closely resembles that used in analyzing the BLS injury data. As before, I estimate baseline, core, and broad negative binomial models controlling for the same set of state-variant characteristics. Importantly, however, instead of focusing on the number of injuries experienced annually by a particular firm per hour worked, this phase of the analysis examines the number of annual construction-related fatalities *by state* per construction worker. Since the unit of observation is now the state rather than the firm, it is no longer possible to control for firm size. (Although in theory one might control for state-level differences in the distribution of construction firms by size, to the best of my knowledge, the data do not exist that would enable one to implement such a technique.)

The detailed nature of the CFOI data also enables me to use an alternative metric of construction safety. Although transportation accidents are the second leading cause of construction fatalities (US Bureau of Labor Statistics 2004a), they are arguably the least amenable to OSH regulation. To be sure, some fatalities coded as "transportation related," such as falls from construction vehicles, meaningfully reflect the safety of a construction job. However, fatal collisions that occur away from the actual worksite are probably far most sensitive to other state-variant factors, such as population density and speed limit laws. Unfortunately, one cannot discern from the CFOI data fields whether any given transportation fatality is truly "work related." Therefore, to account for the likelihood that the frequency of fatal car crashes reveals little about the safety of construction work, I estimate one set of models that excludes all transportation-related fatalities.

A preliminary glance at the summary statistics presented in Table 4 reveals, surprisingly, that construction fatality rates are *lower* in state-plan states. The regression models presented in Table 6 generally replicate this disparity. The state-plan regime dummy is large and negative in all models, and statistically significant at the 5% level in all of the models that exclude vehicular fatalities. In short, although the analysis of nonfatal injuries implies that construction work is generally *more* dangerous in state-plan states, the models of fatality rates suggest precisely the opposite.

The control variables display interesting patterns. Despite the insignificance on union membership in the nonfatal injury models, union membership emerges here as negative and highly significant, suggesting that a strong union presence may provide workers with some protection against life-threatening occupational hazards. The positive, significant coefficient on the "workers' compensation waiting period variable" suggests that in deterring workers from seeking treatment for minor injuries, long waiting periods may also place them at greater risk of sustaining fatal injuries.

Once again, the regression results leave us with a few perplexing questions. First, the coefficient on the "percentage of workers with little or no English" variable remains significantly negative. As noted earlier, OSHA's own internal studies suggest that if anything, nonnative-speakers may be *more* likely to die on the job (OSHA). Perhaps, notwithstanding the efforts of the BLS to ensure

Table 6. Negative Binomial Models of Occupational Fatalities (1992–98)

	Dependent variable = all fatalities			Dependent variable = nonvehicular fatalities		
	Model 1(a) 357 observed $\hat{y} = 11.29$	Model 2(a) 357 observed $\hat{y} = 11.05$	Model 3(a) 357 observed $\hat{y} = 11.76$	Model 1(b) 357 observed $\hat{y} = 8.843$	Model 2(b) 357 observed $\hat{y} = 8.571$	Model 3(b) 357 observed $\hat{y} = 9.116$
State-plan regime dummy	−0.1344 (0.0871) d*y*/d*x* = −1.420 [Δ = −12.58%]	−0.1181 (0.0793) d*y*/d*x* = −1.231 [Δ = −11.14%]	−0.1773*** (0.0715) d*y*/d*x* = −1.910 [Δ = −16.24%]	−0.1698** (0.0840) d*y*/d*x* = −1.381 [Δ = −15.62%]	−0.1463** (0.0719) d*y*/d*x* = −1.166 [Δ = −13.60%]	−0.1981*** (0.0704) d*y*/d*x* = −1.638 [Δ = −17.97%]
WC waiting period in days		0.0396* (0.0232)	0.0369** (0.0191)		0.0534*** (0.0214)	0.0570*** (0.0199)
WC deregulation dummy		0.0009 (0.0829)	0.1310* (0.0793)		0.0274 (0.0748)	0.1307* (0.0750)
WC max wage as % of average weekly wage		−0.0001 (0.0017)	−0.0010 (0.0018)		0.0003 (0.0014)	−0.0006 (0.0016)
Average age of construction workers		0.0157 (0.0290)	−0.0043 (0.0203)		0.0307 (0.0270)	0.0090 (0.0219)
% unionized workers			−0.0089*** (0.0025)			−0.0047** (0.0025)
% construction workers with no/little English			−2.197*** (0.6239)			−1.671*** (0.6435)
Safety and health committee law			0.0971 (0.0893)			0.0898 (0.0927)
Consultation visits per worker			0.0424*** (0.0044)			0.0436*** (0.0045)
Year dummies	Included	Included	Included	Included	Included	Included

All models presented are negative binomial models. The exposure term is the total number of employed construction workers (by state and year). Data from the CFOI were obtained from the BLS. Historical data on state waiting periods, maximum WC dollar recovery amounts, and state average weekly wages were obtained from the BLS and from the Department of Labor's Office of Workers' Compensation Programs. Data on the deregulation of state WC systems were obtained from John Ruser of the BLS. Data on average ages and percentage of unionization were calculated from the CPS. Data on percentage of construction workers who speak little or no English were calculated from the 1980, 1990, and 2000 Decennial Census Public Use Microdata Samples. Data on mandatory safety and health committees were obtained through online research and personal communications with state officials. Data on consultation visits were calculated using data provided by Joseph DuBois, Director of OSHA's Office of Statistics. Consultation visits per capita were calculated as number of Consultation Program visits per 1000 employed construction workers for each state and year. Means of injury rates were calculated across all sampled establishments and years. Means of fatality rates were calculated using SIC-based industry employment figures from the Local Area Unemployment Statistics Program. Means of control variables were calculated across state-year values over the time period examined. ***Significance at the 1% level. **Significance at the 5% level. *Significance at the 10% level. Standard errors are clustered by state and given in parentheses. d*y*/d*x*, given for the state-plan regime dummy, represents change in the predicted number of events ($\hat{y}$) associated with a discrete change of the state-plan regime dummy from 0 to 1. The associated predicted percentage ($\Delta = (dy/dx)/\hat{y}$) is presented in brackets.

that the CFOI is a true "census" of all work-related deaths, fatalities among nonnative workers (especially those performing construction work illegally) are systematically underreported. The second puzzle is the fact that the coefficient on consultation visits, just as in the earlier nonfatal injury regression, is positive and significant. This finding underscores the need for more research on OSHA's Consultation Program in general, and in particular, on which factors omitted from the above models may encourage some states to use the program more intensively than others.

8. Explaining the Disparity between Fatal and Nonfatal Injury Rates

The striking divergence in outcomes between the two safety metrics examined—nonfatal injuries and fatalities—raises deep and perplexing questions about the consequences of regulatory devolution. Two possible explanations for these puzzling findings merit further inquiry.

First, it is possible that nonfatal construction injuries are just as common in OSHA-regulated states as in state-plan states, but firms in the federal regime are simply less likely to report them. The disparities in enforcement stringency revealed in the first phase of the analysis could conceivably help explain this puzzle. If firms believe that reporting higher injuries will subject them to increased regulatory scrutiny, then OSHA's comparatively stringent enforcement approach might have the perverse effect of encouraging more firms to underreport.[27]

Alternatively, the two safety metrics examined could be different in kind. Even in the aggregate, the distributions of events that trigger fatal and nonfatal injuries differ markedly. For example, while "falls" cause more than a third of all fatalities, they trigger only 22% of nonfatal injuries, and although exposure to a harmful substance induces only 3% of nonfatal injuries, it causes one-seventh of fatalities (US Bureau of Labor Statistics 2004a, 2004b). If injuries and fatalities are triggered by distinct causal mechanisms, they may respond differently to different styles of regulatory intervention. Scholars such as Ayres and Braithwaite (1992) and Piore (2004) have suggested that although granting regulators wide discretion to adapt their behavior to the specialized needs of individual firms might "open the door to corruption" (Piore 2004), it may also have significant practical advantages over the "command and control" enforcement approaches that OSHA has traditionally employed. In a similar vein, an interesting study of safety practices in high-risk environments suggests that although systems exhibiting considerable slack and adaptability may be best equipped to handle systemic breakdowns that cause unexpected catastrophes, tightly rule-bound systems may be more effective in preventing

27. Many prior studies have confirmed that the BLS injury data suffer from underreporting (Ruser and Smith 1988, 1991; Seligman et al. 1988; Glazner et al. 1998; Azaroff et al. 2002; Leigh et al. 2004). To the best of my knowledge, however, no prior study has suggested that the magnitude of underreporting varies systematically among federal and state-plan OSHA states.

predictable, run-of-the-mill hazards (Perrow 1984). Drawing upon such work, one might hypothesize that since state-plan states and OSHA exhibit such different regulatory "styles"—including marked differences in the frequency of inspections and citations, sizes of penalties, emphasis on repeat inspections, and so forth—their comparative effectiveness may depend on which outcome one examines. If occupational safety is truly a multidimensional phenomenon, then perhaps the choice among regulatory approaches involves real-world trade-offs between minimizing the frequency of occupational injuries on one hand and avoiding deadly accidents on the other.

9. Conclusion

Since the passage of the Occupational Safety and Health Act of 1970, the provision permitting state governments to opt out of the federal system has remained controversial. Focusing on the construction industry, this study seeks to examine the real-world consequences of partial regulatory devolution, whereby 21 states have conducted their own enforcement of safety regulations. My goal is to examine whether regulatory behavior, nonfatal injuries, and occupational fatalities differ significantly between the state-regulated and federally regulated regimes.

The empirical analysis unfolds in four stages. In the first stage, I focus on enforcement inputs in each regime, as measured by the frequency of inspections, magnitude of fines, and so forth. I find that state-plan regimes apply traditional enforcement tools less stringently than their federal counterparts. In the second stage of the analysis, I find that although the impact of early inspections differs little across regimes, once many inspections of a given site or firm have taken place, further inspections may have a larger impact on future violations in the federal OSHA system. In the third and fourth stages of the analysis, I examine injury rates and fatality rates, the outcomes of ultimate policy interest. These two metrics of occupational safety yield very different conclusions regarding the efficacy of state versus federal enforcement. Although state enforcement significantly increases the predicted number of nonfatal injuries relative to federal enforcement, it also significantly *decreases* the frequency of predicted fatalities.

I offer two possible explanations for these surprisingly disparate results. First, while nonfatal injuries may be similarly prevalent in both regimes, underreporting might be more pervasive within the federal regime. Alternatively, the choice of regulatory strategy may involve real-world trade-offs between alternative outcome measures. The sparing use of fines and greater emphasis on repeat inspections that characterizes state-plan behavior—although appearing more "lax" from a conventional viewpoint—may be most efficacious in deterring fatal construction accidents. In contrast, OSHA's more rule-driven, "command and control" style of enforcement may be best suited to reducing more minor injuries. Examining whether either of these theories can explain this article's seemingly contradictory findings would be a profitable area for future research.

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