

# State Regulatory Policy and Economic Development

This symposium is concerned with the impact of state policies on economic *development*—long-term increases in levels of employment, income, or wealth that are widely distributed among a state’s residents. State economic regulatory policy<sup>1</sup> is designed to further the “public interest.” The compatibility of the two goals depends on a number of complicated relationships that are difficult to identify and to quantify.<sup>2</sup> Consequently, studies of the impact of state regulation on economic development are every bit as controversial and difficult to interpret as those evaluating the impact of tax and spending policies.

## *The Breadth of State Regulations That Potentially Affect Economic Development*

Regulations at all levels of government attempt to promote economic welfare primarily by correcting imperfections in private markets, such as monopolistic and oligopolistic practices, negative externalities generated in both production and consumption, imperfect information, and fraud. Since virtually every regulation has some economic impact, a complete review of those potentially affecting economic development is far beyond the scope of this paper. An important subset are those that most directly affect businesses. They include, but are not limited to:

1. *Environmental protection and land use.* States set zoning rules; regulate practices in agriculture, forestry, fishing, and construction; and control the processing and disposal of hazardous waste. They have an important role to play in the enforcement of federal pollution control standards, with plenty of room for discretion in methods of enforcement.

2. *Regulation of labor markets and the workplace.* States determine whether union membership is a necessary condition of employment (“right to work” laws); set rules governing workers’ compensation; set

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minimum wage laws that in some cases extend coverage beyond federal requirements and exceed the federal minimum; along with the federal government, set standards for occupational safety; and grant occupational licenses.

3. *Regulation of financial institutions.* States are the primary regulators of life insurance companies and public pension funds and regulate commercial banks and thrift institutions jointly with the federal government. States regulate these institutions' chartering, branching, and sources and uses of funds.

4. *Energy production, distribution, and conservation.* State public utility commissions control the pricing and investment decisions of energy-producing utilities and the allocation of scarce gas and electricity in times of shortage.

5. *Transportation.* States have full regulatory power over intrastate transportation of freight and commercial travel. They constrain transport rates, the routes available to carriers, and the frequency of service on various routes.

### ***Why the Impact of Regulation Is So Hard to Predict and to Measure***

Many state regulations raise the costs of production and diminish factor productivity by internalizing negative externalities, constraining technological choice, and requiring outputs (such as periodic reports and the provision of information to consumers) that producers would not normally furnish. Economic development, arguably, is thereby retarded.

However, to the extent that regulations promote economic welfare, they can enhance a jurisdiction's attractiveness as a place in which to live, work, and vacation. Consequently workers (including executives) may be willing to sacrifice monetary compensation in order to work in a safe environment with insurance against (or the promise of compensation for) disability or injury incurred on the job, to live in communities relatively free of pollution, or to have assurance that they will be provided with adequate health care and financial services if they pay for them.<sup>3</sup> Such amenities may also attract retirees and tourists with considerable purchasing power. In this manner,

<sup>1</sup> In this paper I include laws as well as regulations in the term "regulatory policy." I am concerned with the consequences for economic development of all attempts by state government to mandate or proscribe specific forms of economic behavior.

<sup>2</sup> See Courant (1994) for a discussion of the potential incompatibility of the two goals.

regulation can indirectly create compensating reductions in production costs and enhance a state's attractiveness to retail and service establishments in search of lucrative geographic markets.<sup>4</sup>

These potential compensating effects complicate the task of evaluating the net impact of regulation on economic development. They imply endogeneity and simultaneity among variables, creating biases that are difficult to offset.

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How regulations are implemented in practice is at least as important as their requirements on paper. In general, controlling for enforcement behavior is more important in evaluating the impact of regulation than the impact of taxation. Yet, measuring regulatory stringency is generally more difficult than measuring the burden of taxation.

Industries differ widely in the degree to which major state regulations are applicable. Disaggregation by industry, although limited by available data, is therefore important in evaluating the impact of regulations on economic development.

<sup>3</sup> The leadership provided by Pittsburgh's business executives in securing the passage of smoke control ordinances in the 1940s exemplifies the manner in which environmental regulation can enhance competitiveness. In 1945 several of Pittsburgh's largest corporate employers were planning to move their headquarters to another location, in part because corporate managerial and technical personnel and their wives "didn't want to live and raise their families under . . . prevailing environmental conditions" (Schmidt 1958). Alarmed by the potential for such migration, business leaders committed to the city pushed for pollution control laws. See Lubove (1969).

<sup>4</sup> Many studies have estimated the extent to which the quality of life in a particular locale is capitalized into relative wage rates and real estate prices. See, for example, Rosen (1979); Roback (1982, 1988); Hoehn, Berger, and Blomquist (1987); Berger and Blomquist (1988); Gyourko and Tracy (1989a, 1989b, 1991); Leven and Stover (1989); Voith (1991); and Gabriel, Matthey, and Wascher (1996).

### *Three Foci of Research Concerning State Regulation and Economic Development*

Studies of the impact of state regulation on economic development have concentrated on rules governing environmental protection, labor markets, and to a lesser extent financial institutions.

#### *State Environmental Regulation and Economic Development*

Researchers' relatively strong interest in the economic impacts of state environmental regulation arises from at least four factors. First, environmental regulation at all levels of government was tightened in the 1970s and 1980s at the same time that concerns about the international competitiveness of U.S. industry began to intensify. U.S. industry, particularly those

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components most severely affected by environmental regulations, raised the specter that strict pollution control was contributing to the flight of factories to developing nations. Consequently, the international locational impact of environmental regulation became a salient political issue, creating a demand by policymakers for research and analysis. This interest in international competitiveness spilled over into the interstate arena.

Second, interstate and interindustry differences in the cost of complying with environmental regulations have been quantified by the U.S. Census Bureau. The costs of regulations in other areas have generally not been quantified to the same degree.

Third, as explained in the next section, in theory one can measure differences across *counties* in environmental stringency. Because there are only 50 states, interstate studies are limited by the number of available observations (statistically speaking, degrees of freedom). This constraint is relaxed when county-level observations are available.

Finally, as both Bartik (1988) and McConnell and Schwab (1990) point out, the federal government explicitly assumed that lax environmental regulation is a potent competitive tool that states would use to maintain or augment their industrial base. Congress preempted the states in the area of pollution control explicitly:

to preclude efforts on the part of States to compete with each other in trying to attract new plants and facilities without assuming adequate control of large-scale emissions therefrom (*Legislative History of the Clean Air Act*, 1979).<sup>5</sup>

Since the validity of the federal government's presumption had not been evaluated, researchers were naturally drawn to the issue.

Analyses of the impact of state environmental regulation on economic development employ both survey research (surveys of executives of manufacturing firms) and econometric estimation. Surveys generally find that pollution control laws and regulations are at most a moderate influence on the location of a new plant (see, for example, Schmenner 1982; Wintner 1982; Duerksen 1983; Stafford 1985; Epping 1986; Tong 1978; and Lyne 1990). However, as Levinson (1996a) points out, differences in methodological approach make comparisons among these studies difficult. Some ask open-ended questions about which factors most influence locational decisions, while others ask respondents to rank a prespecified array of factors.

I will focus on 10 econometric studies, summarized in the Appendix Table, that form the backbone of the research on the impact of state and local environmental regulation on economic development. Taken as a whole, they permit the following generalizations:

**1. The majority of the studies evaluate the impact of environmental regulation on new plant locations and business start-ups.** Researchers justify their focus on the determinants of the location of new branch plants and business start-ups on several grounds. As Bartik points out, more direct and comprehensive measures of economic development, such as changes in aggregate employment or in value added, are the product of many different decisions such as expansions, contractions, closings, and openings. Factors determining each type of decision are likely to differ. By focusing only on branch plant

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<sup>5</sup> The willingness of the federal government to intervene in this arena stands in stark contrast to its unwillingness to dampen the use of tax incentives in interstate economic rivalry. See Enrich (1996) and Burstein and Rolnick (1995).

openings, one can have more confidence in the validity of one's model. Moreover, new branch plants are likely to be more responsive to regulatory differences than existing branches, because the mobility of existing plants is limited by physical capital, and state environmental regulations on new plants are usually more stringent than those on existing ones. Thus environmental regulations are likely to exert an especially strong effect on the location of new branch plants and independent start-ups.

While Bartik is right, focusing exclusively on the locational decisions of individual firms diminishes policy relevance. Too often, state and local governments equate successful economic development policies with the retention or attraction of firms. Yet the ultimate goal of such policies is to promote widespread growth in employment and income. New or retained firms may simply hire away employees from existing establishments, which then shrink, offsetting the gains obtained through recruitment and retention. Such an outcome is especially likely if the rise in aggregate demand for labor drives up wages, forcing some firms out of business or inducing substitution of capital for labor.<sup>6</sup>

**2. All 10 studies struggle with the issue of how to measure regulatory stringency.** This is perhaps the most difficult problem encountered. Measures of stringency either are not comparable across states or partially reflect state-specific characteristics that have nothing to do with stringency.

As Levinson points out, the measures fall into three closely related categories: estimates of enforcement effort, estimates of compliance costs, and direct measures of stringency. Each type of measure has distinct advantages and disadvantages.

*Measures of enforcement effort* include, but are not limited to: (1) the number of employees working for state environmental agencies per manufacturing plant; (2) state spending on air and water pollution control divided by state employment in manufacturing, state value added in manufacturing, or total state employment; and (3) a dummy variable indicating whether a state charges fees for permits allowing the construction or operation of pollution control plant

<sup>6</sup> Indirect evidence of such substitution effects can be found in Papke (1987) and Tannenwald (1996). Both studies, which examine the effects of taxation, spending, and other variables on interstate and interindustry differences in capital spending per production worker, found a small and statistically insignificant coefficient on wages, usually thought to be a prime locational determinant. One explanation is that high wages, although a deterrent to location, induce substitution of capital for labor.

and equipment. The inclusion of such explanatory variables is testimony to the importance of enforcement effort in determining the effects of environmental controls. Imagine if investigators of the competitive impact of state tax policies measured tax burden by the number of employees in the state department of revenue per taxpayer!

Researchers utilizing these measures of enforcement effort explicitly recognize their potential biases.

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For example, Bartik notes that states whose environmental protection departments are well-staffed may be able to explain pollution control requirements relatively clearly and process environmental permits relatively quickly. He and McConnell and Schwab also warn that the size of the work force devoted to enforcement may simply reflect the concentration of heavily polluting industries within the state rather than the intensity of enforcement effort.

In estimating the costs of compliance, some researchers have used the average cost of purchasing and operating pollution control plant and equipment. Estimates of compliance costs disaggregated at the state and 2-digit SIC level have been published for many years by the U.S. Census Bureau's *Survey of Pollution Abatement Costs and Expenditures* (PACE). In using the PACE data, investigators have had to confront the problem of controlling for state-specific characteristics affecting compliance costs that have nothing to do with stringency, such as the aggregate size of the manufacturing sector, industry mix (disaggregated beyond the 2-digit level), average firm size, and mix of old versus new plants.

As Crandall (1993) notes, average compliance cost does not necessarily reflect the marginal cost at a given location, that is, the cost that would have been incurred by failed firms had they stayed in business, or by relocated firms had they not moved. These firms

may have failed or left because their compliance costs would have been prohibitively high.

A less direct but commonly used indicator of the compliance costs incurred within a jurisdiction is the quality of the jurisdiction's air. If the jurisdiction is located in a county whose air quality is below federal standards (that is, has "non-attainment status"), then federal law requires that its businesses be subjected to stricter pollution control regulations than those in counties in "attainment status." It is usually assumed that, given non-attainment status, the dirtier the air, the greater the compliance costs within the jurisdiction.

Unfortunately, dirty air can stunt a county's economic growth in two ways: by making it unattractive to workers, thereby driving up labor costs; or by precipitating more stringent regulation, driving up compliance costs and thereby deterring businesses from locating or expanding within its territory. The former effect has nothing to do with environmental stringency. In fact, if more stringent regulations were not precipitated under federal law, the negative impact of dirty air on economic development arguably would be greater.

Direct measures of environmental standards are both qualitative and quantitative. Qualitative standards, used by Levinson, include modified versions of the FREE Index (The Fund for Renewable Energy and the Environment 1987), the Conservation Foundation Index (Duerksen 1983), and the Green Index (Hall and Kerr 1991). Elements of all three indices include the existence and stringency of common environmental laws and regulations, such as requirements for state environmental impact statement, air quality, hazardous waste, superfund laws, air toxics programs, and water permit requirements.

Direct quantitative measures of stringency (for example, maximum permissible daily level of particulate emissions) are not especially useful because they are so numerous and varied, often not comparable across states, and highly industry-specific. Bartik attempts to overcome these limitations by using a quantitative environmental standard that applies to many different industries and that is applied by most states: particulate emissions from industrial boilers. McConnell and Schwab use quantitative restrictions on emissions of volatile organic compounds (VOCs). VOCs are a major pollutant created in spraying and painting operations, both of which are widespread in the manufacture of motor vehicles (the only industry they study).

**3. Most of the 10 studies find negative, statistically significant relationships between some mea-**

**asures of regulatory stringency and their measure of economic activity. However, these estimated effects tend to be small. Moreover, the models of many investigators explain little of the variation in their dependent variable.**

For example, Bartik (1989) finds that the stringency of a state's environmental regulations, as rated by the Conservation Foundation, has a negative, statistically significant impact on the rate of small business formation within the state. According to his estimate, however, an increase in regulatory stringency of one standard deviation is associated with a change in the small business formation rate of only 1 percent.

Levinson (1996a) finds a negative, statistically significant relationship between a state's FREE Index and the probability that a new plant will locate in the average state (the largest impact he finds among all his various measures of regulatory stringency). This negative impact is comparable in magnitude to that of such widely recognized determinants of business activity as the rate of unionization and more than three times the estimated impact of energy costs. A one-standard-deviation increase in a state's FREE Index value is associated with a decline of 1.73 percent in the probability that a plant will locate in the state. Levinson estimates that, for the average state, this effect translates into a loss of a little more than 500 production jobs over a five-year period. Levinson's conditional logit models explain only about 11 percent of the variation in their dependent variable, however (that is,  $R^2$ s of about 0.11).

Gray (1996) estimates that a one-standard-deviation increase in stringency, as measured by a cluster of indicators gauging the intensity of indigenous political support for environmental protection, is associated with a decrease in a state's annual birth rate of new plants of about 0.7 of a percentage point, larger than the impact of unionization, but not especially large in absolute terms.

Duffy-Deno (1992) finds that a 10 percent increase in total pollution abatement spending by manufacturers per dollar of value added (his measure of stringency) reduces manufacturing employment per capita by between 0.75 percent and 1.05 percent. Resultant percentage reductions in per capita earnings levels range between 0.64 percent and 0.75 percent.

Kahn (1996) and Henderson (1996) find larger effects than the studies cited above. According to Kahn's results for manufacturing as a whole, the rate of growth in manufacturing employment in counties that are not in attainment with federal particulate

standards was 9 percent slower than in "attainment" counties. However, his  $R^2$  is 0.13. My concerns about the biases inherent in the "non-attainment" dummy indicator of stringency have already been noted above.

Studying selected industries, Henderson found that counties with stringent regulations have between 7 percent and 10 percent fewer establishments than those with less stringent regulations. He may have found such relatively strong effects in part because his

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data set was more disaggregated by industry (3-digit level) than those of other analysts and limited to five pollution-intensive industries. Henderson also uses a unique measure of regulatory stringency: a dummy variable indicating whether the county in which a plant is located in year  $t$  had been in attainment for all three years preceding and including  $t$ . He reasons that firms will be attracted to a county where stringency is relatively low only if they are reasonably confident that the regulatory environment will remain favorable. Counties that go in and out of attainment are not significantly more attractive than those always out of attainment. (When Henderson uses a dummy variable simply indicating attainment status in year  $t$ , he finds a significant coefficient in only two of five industries.)

**4. Two studies suggest some interesting possibilities concerning intra-industry allocative effects.** One interesting set of results, found by both Kahn and Crandall, suggests that pollution control regulations may affect the allocation of resources between existing and new plants. Kahn found that existing plants were less likely to close in non-attainment counties but, conditional on their survival, grew more slowly in such counties than in those having achieved attain-

ment status. Using state-level data, Crandall similarly found that a state's rate of contraction in manufacturing employment attributable to plant closings was negatively related to pollution abatement expenditures per dollar of output (although the relationship was small and not statistically significant). Crandall also found, like Kahn, that pollution control retarded employment growth from existing plant expansions. However, Crandall also found that pollution control retarded employment shrinkage from existing plant contractions. Taken together, Kahn's and Crandall's results suggest that environmental regulation, more severe on new firms, gives existing firms some competitive advantage, which increases their chances of "staying afloat" and reduces pressure to lay off workers. These results are consistent with Pashigian's (1985) hypothesis that environmental regulatory policy is designed partially to protect the interests of large, existing firms.

#### *State Regulation of Labor Markets and Economic Development*

The state laws and regulations pertaining to labor markets whose effects on economic development have been studied most widely are right-to-work laws and workers' compensation. In both cases, more attention has been devoted to the indirect effects of these laws and regulations, exerted through wage rates and rates of unionization, than to their direct effects on employment, income, or firm location.

*Right-to-work laws.* Right-to-work laws, legalized by the Taft-Hartley Act of 1947, prohibit the imposition of union membership as a condition of employment. Today such laws are in effect in 21 states, most of them in the West and Southeast. It is widely assumed that right-to-work laws attract businesses by weakening unions, thereby dampening wages, lowering the probability of work stoppages, and generally widening managerial discretion. Their impact has been more widely studied than any other labor market law/regulation, perhaps because their existence is a simple dichotomous variable: a state either does or does not have such a law. The problem of measuring the stringency of the law, so nettlesome in the analysis of the impacts of environmental regulation, is absent.

I identified 11 studies that estimate the impact of right-to-work laws on either plant location, the rate of business formation, employment, or some other manifestation of economic development (Coughlin, Terza, and Arromdee 1991; Carlton 1979; Newman 1983, 1984; Friedman, Gerlowski, and Silberman 1992;

Schmenner, Huber, and Cook 1987; Soffer and Korenich 1961; Wheat 1986; Plaut and Pluta 1983; Garofalo and Malhotra 1992; and Holmes 1996). Eight of them find that the existence of a right-to-work law exerts a positive, statistically significant impact on economic activity.<sup>7</sup>

Despite this apparent robustness, scholars who have studied the economic impacts of right-to-work laws have made some cogent points that raise doubts about the results' validity (see especially Moore and Newman 1985). Evidence documenting the paths through which right-to-work laws purportedly promote economic growth is elusive. For example, in theory right-to-work laws weaken unions. However, one could argue with equal force that weak unions, and the underlying attitudes responsible for their weakness, promote the enactment of right-to-work laws. Given the latter direction of causality, the repeal of right-to-work laws would have no independent effect on economic development. The underlying hostile attitude toward unions would assure that the weakness of unions would continue.

In studies that treat right-to-work laws as endogenous, the results are mixed. While some find that right-to-work laws diminish unionization, others find small, statistically insignificant effects. One study (Lumsden and Peterson 1975) that controls for anti-union sentiment finds no impact of such laws on union membership.

Similar concerns about simultaneity bias cloud estimates of the direct impact of right-to-work laws on average wage levels, independent of the impact on wages of unionization. Whether such simultaneity is controlled for or not, most investigators failed to detect such an effect.

Even if right-to-work laws, unionization, and wage levels are not jointly determined, they covary, and wage levels and economic development are simultaneously determined. Thus, in any ordinary least squares equation that includes all three hypothesized determinants of economic growth, estimated coefficients will be biased. The models used in several studies are so biased.

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<sup>7</sup> Soffer and Korenich (1961), one of the two studies failing to detect any effect, use simple cross-tabulations in their investigation. They fail to control for all but a few other influences that may cause interjurisdictional economic differences.

Plaut and Pluta (1983) include the presence of a right-to-work law as an element in a principal-components index of labor power. They find that this index exerts a negative, statistically significant influence on growth in manufacturing employment and value added.

Newman's 1984 examination of variation over time in the economic impact of right-to-work laws raises questions about the likely economic impact of introducing such a law today. He found that the size and statistical significance of coefficients on right-to-work dummies peaked in the 1950s, shortly after the enactment of the Taft-Hartley Act, but had shrunk and become statistically insignificant by the 1970s. As Moore and Newman (1985) point out, two possible interpretations are: (1) by the 1970s, the effect of right-to-work law enactment had played itself out; or (2) over time, employers came to realize that right-to-work laws merely symbolize underlying attitudes. If the latter interpretation is accurate, then enactment of right-to-work laws today would promote economic development only if employers were convinced that such legislation represented a fundamental change in a state's attitude toward unions.

Holmes (1996) adopts an approach that attempts to minimize the various potential sources of bias outlined above. He notes the presence of a long, practically continuous border, stretching almost 4,000 miles, that separates regions with and without right-to-work laws. He reasons that counties on either side of the border are quite similar in all respects except the attitudes of their government toward business in general and manufacturing in particular. He hypothesizes that, as one crosses this border coming from the right-to-work side, one should find a sharp drop in the average rate of growth in manufacturing and the share of manufacturing in total employment since 1947 (when Taft-Hartley was passed). This is in fact the pattern that he finds. While his results suggest that right-to-work laws have exerted a strong economic effect, he warns that the results could also be attributable to other cross-border differences that he could not identify or take into account.

*Workers' compensation.* Dissatisfaction with workers' compensation laws and regulations has intensified dramatically since 1980 as the costs of state programs have skyrocketed. Yet I could find only two econometric studies that include the costs of workers' compensation as an explanatory variable in a model predicting business location. Bartik (1985) found that the probability of a new branch plant's being located in a state was *positively* related to the state's average workers' compensation rate. In his preferred specification, the coefficient on this variable was statistically significant and the second largest in absolute value. Schmenner, Huber, and Cook (1987) found that, for plants that were especially footloose, a high workers' compensation rate at a particular site was a moder-

ately large and statistically significant deterrent to location.<sup>8</sup>

Many of the major indices of state "business climate," such as those constructed by the Fantus Company (1975), and Grant-Thornton (1990), include workers' compensation rates or average workers' compensation payments as a component. In the Grant-Thornton index, components were ranked and weighted in importance, depending on the responses to a closed-end survey conducted by Grant-Thornton of a sample of approximately 1,000 manufacturers. In Grant-Thornton's 1989 analysis (the latest one published), the average workers' compensation rate was ranked third in importance among 16 factors, trailing only education level and wage rates. In light of the importance attached to workers' compensation in such surveys, the lack of attention paid by researchers to its impact on economic development is puzzling.

Perhaps scholars have been deterred by empirical studies strongly suggesting that workers, not employers, bear most of the burden of workers' compensation (evidence on this issue is summarized nicely in Chelius and Burton 1994). This empirical evidence, both direct and indirect, is analogous to wage reductions that workers apparently are willing to accept in return for a clean environment and other amenities.

Analyzing the determinants of the wages of a national sample of blue-collar workers, Dorsey and Walzer (1993) found that, for nonunion workers, every 1 percent increase in workers' compensation costs resulted in a 1.4 percent decline in wages. Gruber and Krueger (1991) found that between 56 percent and 87 percent of all workers' compensation costs are borne by employees in the form of lower wages. Moore and Viscusi (1990), evaluating three samples of heads of households, found a similarly large wage offset. They conclude:

under a wide range of assumptions a substantial wage offset is generated by the provision of [workers' compensation] benefits. This offset is expected on economic grounds since boosting one attractive feature of the compensation mix (workers' compensation) will reduce the wages needed to make a hazardous job acceptable to the worker. . . . Although workers' compensation increases do not provide an economic 'free lunch' to firms, they are cheaper fare on average than is generally believed (1990, p. 68).

<sup>8</sup> They identified such firms in their sample by determining, through a mail survey, that the company owning the plant had a strategy of specializing by product; that is, each plant produced a particular product, which was then sold over a broad geographic area, including locations far from the plant location.

Indirect evidence of a wage offset can be gleaned from studies examining the impact on wages of the following: (1) state government mandates that employers pay for maternity leave. Wages for married females in states without mandates were higher than

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those in states with mandates (Gruber 1994); (2) the riskiness of occupations. There is a measurable wage premium for risk; and (3) payroll taxes in general. The evidence on the degree to which these taxes are borne by worker is mixed, although strongest in Europe, where payroll taxes are higher and statistical difficulties in identifying the effect less severe.

Two studies (Gruber and Krueger 1991 and Durbin 1993) suggest that, even if workers' compensation costs are borne largely by employees, the portion borne by employers is sufficiently burdensome to induce employers to lay off workers. Of the two studies, Durbin's reports the stronger employment effect. Using state-level data from selected years between 1981 and 1989, he found that each 10 percent increase in workers' compensation costs was associated with a nationwide loss of approximately 900,000 jobs. According to Chelius and Burton (1994), had this employment impact not occurred, the national unemployment rate in 1991 would have been 5.9 percent instead of 6.7 percent. Gruber and Krueger also find a positive employment effect, but one not significantly different from zero.

#### ***State Regulation of Banks and Economic Development***

Over the past two decades, several states have loosened regulations governing depository institutions, such as those concerning usury, interstate bank-

ing, intrastate branching, and the range of products that banks can offer. The pace and extent of deregulation have varied considerably across states, creating possibilities for empirical estimation of its effect on a state's economy.

During the 1980s, some states relaxed their constraints on banks explicitly to attract or to retain banking facilities. South Dakota and Delaware are the two most prominent cases in point. South Dakota permitted out-of-state bank holding companies to establish a single-state or national credit-card subsidiary and removed interest rate ceilings on all consumer loans. As a result of this legislation, Citicorp

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and several other large banking institutions established a credit-card subsidiary in South Dakota. Delaware enacted similar legislation that provided even more locational incentives, such as a reduction in tax rates and capital requirements. Unlike South Dakota's package, which was targeted exclusively on credit-card operations, Delaware's incentives were also extended to "nonbank" banks and certain types of foreign depositories.

Fearing that their banks would relocate some of their operations to South Dakota or Delaware, New York, Virginia, Maryland, and Pennsylvania countered with packages of their own. However, strong opposition from public interest groups slowed these states' response and weakened the incentive package that they ultimately enacted (Erdevig 1987).

Few economists have analyzed the impact of South Dakota and Delaware's strategy. Superficial statistics suggest that its effect has been large. Erdevig (1987) and Moulton (1983) document the large number of bank subsidiaries that located in each state in response to its incentive package. According to Erdevig, from 1980 to 1987 employment at commercial banks grew 347 percent in Delaware and 75 percent in

South Dakota. The comparable rate of growth for the United States as a whole was 7 percent.

Evidence gleaned from a more sophisticated analysis by Fox and Black (1994) tends to buttress this assessment. Using the 50 states as observations, they regressed bank employment and gross state product originating in the banking sector on a variety of variables measuring the tax and regulatory environment faced by commercial banks. They performed cross-sectional analysis for the years 1978, 1986, and 1989. Among their control variables was a dummy variable equal to 1 if the observation was either Delaware or South Dakota. This dummy variable was large and statistically significant at the 10 percent level in equations explaining interstate variation in employment in 1989. It was smaller and statistically insignificant in their estimates for 1986 and 1978.

Fox and Black's results suggest that states trying to emulate Delaware and South Dakota have not enjoyed comparable results. Delaware and South Dakota evidently have enjoyed a "first-mover" advantage not gained by their slower-moving competitors. Fox and Black then expanded their dummy variable to cover all states offering an incentive package comparable to those of Delaware and South Dakota. When thus defined, the coefficient on the variable is small and statistically significant in all three years. As the authors point out, this result might reflect the endogeneity of bank regulatory policy. Delaware and South Dakota's competitors responded with tax and regulatory relief only after they began to lose bank facilities to these two states. This simultaneity could bias downward the coefficient on the dummy variable. Had New York, Pennsylvania, Virginia, and Nebraska failed to counter South Dakota and Delaware's challenge, they could have lost even more bank employment (or gained less bank employment) than they did.

The only other econometric study I found that estimated the link between interstate differences in bank regulation and state economic development was Bartik (1989). In his preferred model, Bartik found a positive, statistically significant relationship between the absence of constraints on intrastate branching and a state's rate of small business formation. Lack of such restrictions raised a state's small business formation rate by an estimated 5 percent.

### ***Suggestions for Further Research***

The impacts of many types of state economic regulatory policy have not been explored. Those that

have the potential to exert the greatest effects on firms' production costs should be the focus of future empirical analysis. Two prime examples are health care and energy production. The costs of health care benefits have escalated dramatically. HMOs have helped to slow the rate of increase in these costs, with the enthusiastic support of many employers. Public interest groups and consumer advocates have introduced legislation to constrain HMOs' discretion (for example, in shortening hospital stays for women who have just given birth). Large employers in some states have fought these bills on the grounds that the regulatory burden they would impose would increase compensation costs and drive them out of the state. An example is Bath Iron Works' lobbying activity in Maine during the past two years (Lemov 1996).

With respect to energy production, many studies of the determinants of the location of economic activity have found that energy costs are a significant determinant, more important than taxes. Recently the Federal Electric Regulatory Commission has ruled that utility companies must open up their transmission lines to all wholesalers of power. State regulators must now reevaluate their rules governing retail transmission. Several New England states, such as New Hampshire, Rhode Island, and Vermont, have taken the lead in deregulating electricity at the retail end. Can we use estimates of the impact of deregulation on energy prices to assess indirectly the conse-

quences of such policies for economic development? Other areas whose impact can be examined include asset regulation of life insurance companies; the regulation of mortgage instruments and its effect on housing costs; and the regulation of intrastate shipping and freight rates, especially trucking.

In evaluating the impact of state regulations on economic development, we need to learn more about the degree to which households value those regulations as a means of improving their quality of life. Having done that, we need to learn more about how the value placed on regulatory outcomes by households translates indirectly into reduced costs for producers, in the form of lower labor costs or higher worker productivity, for example.

In addition, we need better measures of the character of regulatory enforcement. Informal surveys of employers, such as those conducted by Mass Insight (1995), and case studies, such as those presented in Duerksen (1983) and Moore and Moskovitch (1994), suggest that the process of regulatory enforcement is as important as the degree of regulatory stringency. With respect to regulations governing pollution control and occupational health and safety, issues of central concern to business, the most important aspects of enforcement appear to be the ease, speed, and uniformity of the permitting process; the attitude of regulators toward the firms they regulate; and the degree of engineering specification required as a condition of permit acquisition.

## Appendix Table

*Recent Econometric Studies of Impact of State Environmental Regulation on Economic Activity*

Dependent variable	Description of data set	Measure(s) of regulatory stringency	Estimation procedure	Results
<u>Bartik (1988)</u>				
New plant location.	New branch plants of Fortune 500 companies opened between 1972 and 1978. Source: Dun & Bradstreet Corp, corrected by Schmenner (1982).	State governmental spending on water quality control as a fraction of manufacturing employment, average for 1972–78. State government spending on air quality control as a fraction of manufacturing employment, average for 1972–78. State water pollution compliance costs relative to expected costs given state industry mix, 1978. State air pollution compliance costs relative to expected costs given state industry mix, 1978. Percentage reduction in particulate emissions from industrial boilers required by state regulations. Percentage reduction in particulate emissions from industrial boilers required by state regulations, adjusted for statewide fuel mix.	Conditional logit.	None of the coefficients on measures of regulatory stringency are significant. Some have the wrong sign.
<u>Bartik (1989)</u>				
Number of new firms in an industry and state divided by the total number of employees in industry and state.	U.S. Establishment and Longitudinal Microdata file of Small Business Data Base, Small Business Administration. Compiled by Dun & Bradstreet 1976, 1978, 1980, 1982, 2-digit manufacturing industries.	Rating of state stringency by Conservation Foundation, 1983.	Conditional logit, both cross-section and panel data.	Regulatory stringency exerts negative, statistically significant effect on small business start-up rate.
<u>Duffy-Deno (1992)</u>				
Manufacturing employment and earnings per capita.	Data from U.S. Census Bureau and U.S. Bureau of Labor Statistics for 63 SMSAs, 1974, 1978, and 1982.	Manufacturers' air pollution abatement costs as a fraction of manufacturing value added. Manufacturers' total pollution abatement costs as a fraction of manufacturing value added.	Regression: error components and fixed effects model.	Small statistically significant negative coefficients found for total pollution abatement costs variable on both employment and earnings levels; for air pollution abatement costs variable, on employment level only.
<u>Friedman, Gerlowski, and Silberman (1992)</u>				
Location of foreign branch plants, by state.	884 instances of foreign branch plant location, 1977–88, as reported by the International Trade Administration, U.S. Department of Commerce.	State pollution abatement capital expenditures as a fraction of gross state product originating in manufacturing.	Conditional logit.	Japanese plants are sensitive to pollution abatement expenditures. Investment in new plants by companies from other countries are not.

### *Recent Econometric Studies of Impact of State Environmental Regulation on Economic Activity*

Dependent variable	Description of data set	Measure(s) of regulatory stringency	Estimation procedure	Results
<u>Grandall (1993)</u>				
Rate of growth in manufacturing employment 1977–89, 1977–91, by state. Rate of growth in manufacturing employment attributable to new plants, expansions, contractions, and plant closures, 1976–88, by state.	All state-level Census data.	State pollution abatement operating expenditures as a fraction of gross state output originating in manufacturing, 1977.	Regression.	Regulatory stringency inhibits employment growth attributable to expansions and retards employment shrinkage due to contractions.
<u>McConnell and Schwab (1990)</u>				
New plant location.	Start-up plants in SIC industry 3711 (automobile assembly and emissions) in 1973, 1975, 1979, and 1982 as reported by Dun & Bradstreet, corrected by follow-up telephone calls.	<p><i>County-level nonattainment measures</i></p> <p>Nonattain: Dummy variable equal to 1 if county had not attained the ozone standard in 1977, 0 otherwise (USEPA 1978).</p> <p>Nonattain82: Dummy variable equal to 1 if county had not attained the ozone standard in either 1977 or 1982, 0 otherwise (USEPA 1983).</p> <p>Extended: <math>\text{Nonattain82} \times</math> a dummy variable equal to 1 if county either (a) had requested an extension to 1987 to meet the ozone standard, or (b) was subject to a "SIP call," 0 otherwise (USEPA 1985).</p> <p>Ozone: <math>\text{Nonattain82} \times</math> ozone concentration at highest meter (2nd highest reading in the county, 1977–79; USEPA 1988).</p> <p>Days: <math>\text{Nonattain82} \times</math> number of days per year highest meter in county was out of compliance, 1977–79 (USEPA 1988).</p> <p><i>State-level industry variables</i></p> <p>Opcost: State pollution abatement operating costs in industry 37 as a proportion of state value of shipments in industry 37 (USDC 1977).</p> <p>Topreg: Permitted lbs of VOC/gallon of solvent excluding water in industry 3711, 1982–83, (USEPA).</p>	Conditional logit.	Only the coefficient on OZONE is negative and statistically significant. Suggests that regulatory stringency (as measured by air quality) must be extremely severe to deter plant location.

*Recent Econometric Studies of Impact of State Environmental Regulation on Economic Activity*

Dependent variable	Description of data set	Measure(s) of regulatory stringency	Estimation procedure	Results
		<p><i>State-level all manufacturing variables</i></p> <p>Pace: State total abatement capital expenditure as a proportion of new capital expenditures in all manufacturing industries, 1977 (U.S. Department of Commerce 1977a, 1977b).</p> <p>Fees: Dummy variable equal to 1 if state set fees for operating and construction permits as of 1978, 0 otherwise (State and Territorial Air Pollution Program Administrators 1987).</p>		
<p><u>Gray (1996)</u></p> <p>New plant birth rate, numbers of new plants.</p>	<p>Data on plant openings gleaned from longitudinal research data, based on U.S. Census of Manufactures, 1963, 1967, 1972, 1977, 1982, and 1987. Plant-level data aggregated to get observations on gross and net birth rates (openings as a fraction of total plants) at five-year intervals between 1967 and 1987.</p>	<p>Green "policies" and green conditions from <i>Green Index</i> (Hall and Kerr 1991). Qualitative indices of regulatory stringency and quality of environment.</p> <p>Number of environmental inspections per manufacturing plant.</p> <p>State governmental spending per capita on programs for environmental and natural resources (Council of State Governments 1991). Pollution abatement operating costs as a fraction of total manufacturing shipments, relative to predicted costs based on state industry mix.</p> <p>Percent of population who are members of conservation groups.</p> <p>Average environmental rating of state delegation in U.S. House of Representatives, assigned by League of Conservation Voters.</p>	<p>OLS regression:</p> <p>Pooled cross section time series.</p> <p>Panel estimation</p> <p>—fixed effects model</p> <p>—random effects model</p> <p>Conditional logit, Poisson.</p>	<p>Some measures of stringency exert statistically significant negative effects. However, sign of coefficient varies with estimation method and dependent variable. Impact of regulatory stringency less or no greater in pollution-intensive industries than in industries as a whole.</p>
<p><u>Henderson (1996)</u></p> <p>Plant location.</p>	<p>Panel data set 1980–87. Number of plants in each of 742 urban counties, high-polluting industries and a control set of industries that are not pollution-intensive.</p>	<p>Whether county is in attainment with federal ozone standards.</p> <p>Whether county has been in attainment for the current year and two preceding years.</p>	<p>Conditional logit, Poisson.</p> <p>Nonattainment status exerts a statistically significant negative effect on plant location in only two of five pollution-intensive industries.</p>	<p>Being in attainment for three straight years exerts statistically significant positive effect on plant location in all five pollution-intensive industries.</p> <p>No significant effects found in control industries.</p>

### Recent Econometric Studies of Impact of State Environmental Regulation on Economic Activity

Dependent variable	Description of data set	Measure(s) of regulatory stringency	Estimation procedure	Results
<u>Kahn (1996)</u>				
Growth in manufacturing employment. Probability that existing plant will close. Conditional on survival, rate of growth in employment in surviving firm.	County manufacturing employment 1981–88. U.S. Department of Commerce LRD plant data 1977, 1982, and 1987, for plants with at least 100 employees.	Does county where plant is located have monitoring station? Was county in attainment with federal particulate emissions standards?	Logit OLS regression.	Nonattainment status exerts statistically significant negative effect on manufacturing employment growth.  An existing firm is less likely to close, other things equal, if located in county not in attainment.  Conditional on survival, a firm grows more slowly, other things equal, in a nonattainment county.
<u>Levinson (1996)</u>				
New plant location, Fortune 500 companies.		Conservation Foundation Index FREE Index Green Index Number of employees at state environmental agency per existing manufacturing plant, 1982. Aggregate pollution abatement cost per production worker in state, 1982. Difference between pollution abatement cost per production worker in state and predicted abatement cost per production worker, based on state's volume of capital, number of production workers, value added, and industry, 1982.	Conditional logit.	In aggregate, FREE Index and difference between actual and predicted abatement costs exert negative, statistically significant impact on branch plant location. At industry level, statistically significant impacts found for more than one measure of stringency only in instruments; food; stone, clay, and glass; and chemicals. Impact not much greater in pollution-intensive industries than in others.

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## Discussion

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At the outset of my comments on Robert Tannenwald's paper, "State Regulatory Policy and Economic Development," I should note that it takes a great deal of work to prepare any literature review, especially one as comprehensive as this one. The pages of references reflect the large amount of existing work in this area; it can be difficult to summarize so many different papers in a way that allows the reader to judge fairly what has been learned, and what remains to be done. The author has tried, with some success, to identify areas of agreement and disagreement among the papers, as well as to provide a broader framework within which to place the results.

Fortunately for me, I am not called upon to create, but to critique—a much easier task. My comments will approximately parallel the topics covered in the paper. I begin by considering why regulation might (or might

not) be expected to influence economic development. I discuss a variety of econometric and data issues among the papers he reviews, emphasizing the papers with which I am most familiar, those dealing with state environmental regulation. I then present some observations based on environmental regulation in the pulp and paper industry, before suggesting avenues for future research.

### Theory of Regulatory Impact

Tannenwald first notes that when government imposes a regulation it is (or should be) motivated by a sense that the benefits from the regulation outweigh its costs. If so, stricter regulation might not be associated with slower economic development. Suppose stricter state environmental regulations raise the cost of production, but also improve the quality of life for state residents. Workers may be willing to accept lower wages to live in the state, lowering labor costs enough to offset the increased compliance costs. This argues for including measures of environmental quality, as well as environmental stringency, in the analysis.

Tannenwald raises a good point that is not always recognized in the empirical rush to correlate higher regulatory costs with slower development. However, I would further focus his argument in order to consider exactly what the “margin” is along which these adjustments occur. In general, I would expect environmental amenities to be most strongly connected with housing values, assuming that location is the factor with the most “fixed” supply curve. Depending on the size of the regulatory jurisdiction, it may be fairly simple to live in one area and work in another. Some residents will also be retired or unemployed. For these

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reasons, many of the benefits of regulation cannot be captured by employers. For regulatory programs more closely tied to employment, such as workers’ compensation, it seems likely that employers would capture most or all of the benefits from regulation. This may help explain differences in results across different types of regulation.

Two additional issues arise when considering political support for these regulations. First, the people and firms affected by the regulations may be *inframarginal*. Workers with seniority might prefer to keep their current jobs, even if their wages fell somewhat; firms with large investments in existing plants might not relocate, even if their costs rose somewhat. The behavior of competitive markets depends on the desires of *marginal* firms and workers, those who are just indifferent between one job and another, or one location and another. For example, suppose *inframarginal* (high seniority) workers have stronger preferences for protection against injuries (being older, they may take longer to heal). Strict workplace

safety regulation might be justifiable as raising the utility of senior workers and would not be provided by a competitive labor market focusing on marginal workers.

The existence of *inframarginal* firms could also raise the econometric issue of endogeneity between development and regulation over time. A state that has been very successful in its past development may be more willing to impose regulatory costs, knowing that it has a large stock of existing firms that are unlikely to move. A state that has been less successful in the past will have fewer existing firms from which to extract compliance costs, and a greater desire to bring in new firms (which are by definition marginal ones).

A second source of political support for regulation may arise if the benefits or costs of a particular regulation are borne by people who live outside of the jurisdiction. A good example is provided by the laws in South Dakota and Delaware relaxing interest rate regulations on credit cards issued in those states. This was done to attract new banking business from elsewhere, and it was successful in doing so. The benefits (of cutting regulation) accrued largely to banks and workers in the state, while the costs (of higher interest payments) were imposed on the banks’ customers, spread across the country. Such an “externality” affecting other states may be less likely with other types of regulation but might influence environmental regulations for air or water pollutants that move naturally across state boundaries, for example, sulfur dioxide emissions in Ohio causing acid rain in Massachusetts.

### ***Empirical Studies of Environmental, Labor, and Banking Regulation***

I will focus on the studies of environmental regulation, but many of the comments will apply to other types of regulation. A useful framework for considering empirical studies is the enumeration of the different ways they can differ: data sources, dependent and independent variables, and estimation methods. Tannenwald makes this clearer in his paper with a table comparing the data, models, and results of 17 studies, falling into two major groups. Seven studies use case study or survey methods and are summarized as generally showing that environmental regulations are “at most a moderate influence” on economic development. The 10 econometric studies are discussed in more detail, with the conclusion being

that most find negative, but small, impacts of environmental regulation on economic development (where development is measured in most cases by the location of new plants).

This conclusion may be a bit misleading, because it depends on one's prior expectations of what constitutes a "large" effect. In recent papers by Gray (1996) and Levinson (1996), the coefficients on environmental variables are similar to those on other variables (such as unionization) that are acknowledged to play a role in plant location decisions. It seems to me that the most meaningful comparison, especially between econometric studies and surveys, would be the relative importance of different factors: Is environmental regulation more or less important than wages, unionization, or tax rates?

Much of Tannenwald's discussion of differences between studies focuses on the different measures of environmental regulation being used, including enforcement effort, compliance costs, and "direct" measures of regulatory stringency. These differences are certainly important, and in my work I found some variation in the significance of results when using six different measures of environmental regulation. However, I would like to add a few points to the discussion of these regulatory measures.

First, Tannenwald's background includes extensive work on the impact of tax policy on economic development, which is shown by his comments on the use of enforcement measures (imagine if we used the number of state department of revenue officials to measure differences in tax burden!). The important difference between the two literatures is that the "stringency" of taxes can be precisely measured (in principle) by the marginal tax rate. Some cheating on taxes may occur, but on the whole this is relatively uncommon, so a firm considering where to locate a new plant can be expected to know nearly exactly what its tax bill would be. For environmental regulation, there is much less certainty. The Census Bureau's Pollution Abatement Costs and Expenditures survey, which asks manufacturing plants how much they spent on pollution abatement, might be expected to provide the "best" answer. However, one must consider the mental exercise that plants are required to go through when filling out the survey. Since most new production facilities are both cleaner and more efficient, any allocation of investment costs to pollution abatement involves considerable guesswork. This provides one reason that so many different measures of regulation are used in these studies: No perfect measure is available.

Second, I believe that county attainment status can be profitably used to measure regulatory stringency, despite the comment that dirty air may make the county "unattractive to workers, thereby driving up labor costs." It is true that attainment status is based on air quality: Counties whose air quality readings are below the national standards are put on the list of "non-attainment" counties, triggering more stringent air pollution controls on plants located there. Still, the actual definition of attainment status is a "bureaucratic" one. Many counties were mistakenly classified in early years, and plants there faced the stricter controls until the county status could be updated. Also, air quality varies a great deal within each

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group of counties (attainment and non-attainment), so it should be possible to separate out the influence of actual air quality in the county from the bureaucratic designation of attainment status, for a better test of the regulatory impact.

Third, I would dispute the characterization of direct quantitative measures of stringency as "not especially useful because they are so numerous and varied." They are difficult to put together, and it is important to ensure that the particular measure chosen is one that is expected to matter for the plants being studied. (McConnell and Schwab's (1990) use of VOC (volatile organic compounds) regulations to study auto assembly plants seems especially apt.) Since we expect regulations to grow increasingly more costly as they become more stringent, the estimating equations could be modified to allow for a nonlinear relationship between compliance costs and regulatory stringency. *Quantitative* measures have a key advantage over the usual *qualitative* measures of stringency, because they allow us to measure the impact of one

more "unit" of regulation (the elusive "marginal cost" that economists are always seeking).

Three other areas of difference remain largely unexplored: data source, dependent variable, and estimation method. All of these may be important. Some studies have used measures of aggregate economic activity in the area (number of plants, employment, or output) taken from Census or County Business Patterns data, and the results differ across studies (possibly depending on exactly which measure of activity is used, although this is not explored in the paper). The aggregate data do not permit a focus on the decision to locate a new plant (which may be more sensitive to regulatory differences), so some studies have tried to use plant-level data. Data for the earlier studies of new plant location come from Dun & Bradstreet, while some newer studies use plant-level Census Bureau data. As it happens, both studies using the plant-level Census data (Gray 1996 and Levinson 1996) find some significant impacts, although the magnitude differs. Studies using Dun & Bradstreet data seem to find less significant results. It is not clear why this difference should occur, but it is worth noting. Perhaps it has something to do with the greater sample sizes or broader time period covered by the Census data, which may allow for statistical significance even when the magnitude of the impact being measured is small.

One issue connected with the data source is the period of time covered by the study. The impact of any particular regulation is likely to vary over time as regulatory stringency varies, along with the relative importance of state and federal regulation. For example, one reason for stricter federal regulations was to reduce the temptation for one state to weaken its own rules in order to induce firms to move from another state. In a very brief analysis, I did find some evidence for differences over time in regulatory impacts, particularly in the mid 1970s. To the extent that time series variation is available in the regulatory measures, this provides another avenue for identifying an impact of regulation. The observation that right-to-work laws mattered more in the 1950s seems a good example of this. The national climate was more hostile to union organizing after the 1950s, so that right-to-work states are likely to be less distinct from other states in later years.

The choice of estimation method may also affect the results (as I find in my research). The studies using aggregate economic activity tend to use simple regressions, while the plant-level studies more often use sophisticated econometric methods, such as Poisson or conditional logit, to account for the binary nature

of the location decision. Without a detailed analysis incorporating many different specifications, it is difficult to be sure how these differences affect the results, but the question is worth considering.

One possible approach (for future work) would be to use a "meta-analysis" of the whole set of studies, similar to the one on contingent valuation studies done by Smith and Osborne (1996). This involves collecting the coefficient estimates from the different papers, along with the characteristics of each estimating equation: what data source was used, over what time period, with what econometric technique. A statistical analysis is then run, relating the estimated coefficients to the characteristics of the equations. Such meta-analyses are used infrequently, in part because of the effort required to collect the necessary information, but they can help identify whether particular features of the estimation method or data sources influence the results.

At this point, I would like to comment briefly on the potential econometric problems mentioned in the paper, especially those of endogeneity or simultaneity. For example, right-to-work laws are jointly correlated with both unionization and wage levels, and wage levels and economic development are simultaneously determined. It seems to me that a key issue is the timing of the dependent and independent variables. Since it will usually take a few years for firms to change location, the independent variables should be lagged, reducing simultaneity concerns. Apart from simultaneity, the presence of other explanatory variables correlated with the regulatory measure should be taken care of by multiple regression, which will let the data determine which variable is most strongly related to economic development.

In general, it is best if the regulatory variables can be thought of as a "natural experiment," not directly connected with the other variables in the model. In this regard, two examples of other (non-environmental) regulation that Tannenwald cites seem particularly strong. The first is the reduction in banking regulation in South Dakota and Delaware. The timing of changing laws and expanding banking activity is quite convincing, in a way that is difficult to match for other regulations. The second was the study by Holmes (1996), observing that the right-to-work states are geographically contiguous, so one can identify a "border" between the sets of right-to-work and non-right-to-work states. The finding of significant differences between measures of manufacturing activity between counties on the two sides of the border is quite convincing. It is true (as Holmes notes) that one

cannot say for certain that the right-to-work laws are the reason for this difference; many of these states are also those with relatively lax environmental regulation. However, in a sense the important result is that state borders matter. More work is needed to identify the key state characteristic(s), always a desirable situation for the research community.

### *Insights from the Pulp and Paper Industry*

I have spent some time over the past year visiting plants and corporate headquarters in the pulp and paper industry, touring the plants and talking with people about how environmental regulation influences their activities. A particular emphasis was trying to find out whether differences in regulatory stringency across states matter for plant location decisions. The results suggest concerns that may extend beyond this particular industry.

First, a variety of factors influence plant location, the most important of which are the location of demand for the firm's product and the location of existing facilities in the firm. Wage rates, tax incentives, and other economic variables are also identified as important. Environmental regulations are important, but not among the most important factors.

The main influence of environmental regulations is said to come through difficulties in getting construction permits, due either to delays in permit issuance or to uncertainty about whether the permit would be issued at all. The consequences of delays in the paper industry are especially severe, as the industry is both capital-intensive and cyclically sensitive. Firms are leery of tying up hundreds of millions of dollars in a new paper mill, if delays in permitting will delay the plant's opening until the next cyclical downturn in demand for paper. Those states (such as Maine) that were identified as having uncertainty about final

permit approval were viewed as especially unfavorable for new investments.

The absolute stringency of a state's regulations was viewed as less important than its efficiency in issuing new permits. Several people indicated they would rather have stricter regulations that were clearly specified, so that they could be incorporated in the design of the new facility, raising costs somewhat but not delaying the project. Of course, quick approval without any environmental restrictions would be even more attractive, but delays and uncertainty were the main concerns.

### *Future Research*

Again, I am most familiar with the impact of environmental regulation on economic development. As we have seen, many choices of data set, variables, and specification could influence study results. Existing studies tend to focus on one or two regulatory measures, and a single econometric specification. One worthwhile project would involve trying out a variety of these differences to see which matter the most.

A more significant extension would involve examining the differences across industries in how they respond to environmental regulation. This is difficult to do with most data sets, because large numbers of new plants are needed to be able to estimate differences across industries. The plant-level Census data are probably the best source for this. I am currently working with these data to see whether highly polluting industries are more sensitive to differences in environmental regulation across states. Preliminary results suggest that they are not, which raises issues about how to interpret the estimated impacts of regulation. We may be left with an observation similar to that reached by Holmes about right-to-work laws: Something matters about states, but it is hard to be exactly sure what it is.

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## Discussion

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As economists, we all “know” that higher wage costs lead to lower employment at the firm level, as demonstrated in the recent controversy over the minimum wage and its employment effects. Some of us also think we “know” that higher wage costs lead to less employment growth, although the evidence is mixed and there is more room for debate here. Robert Tannenwald’s paper is a search for evidence that ties the impact of state-level regulatory practices to employment costs, employment levels, or rates of growth in employment.

As one who comes to this search largely unhindered by prior knowledge of the literature, I found the search for empirical evidence of the impact of regulation on economic development to be disappointing. As Tannenwald points out, if the impacts of regulation were solely on the side of raising costs of production or lowering factor productivity (that is, demand factors), we would have unqualified expectations that the impacts would be negative. However, given that it is possible, even likely, that these regulations also have an impact on the quality of life in a state (that is, supply factors), their impact is theoretically indeterminate. One has only to cite the sales success of the *Places Rated Almanac* to see that such factors as regulatory outcomes, and other quality-of-life indicators, are important in labor supply choices as well.

The primary question that occurs to me is whether these differences are sufficient in magnitude to make a discernible difference. In other words, where is the evidence that the differences (even assuming they can be measured adequately) are large enough to worry about? How do differences in regulatory environments compare to differences in average wage levels, overall unit labor costs, taxes, or profit levels? Second, are these differences somehow offsetting, or do they tend to reinforce and amplify each other?

Are we, as economists, guilty of “looking under the lamppost because the light is better” rather than “looking in the dark corner of the parking lot where we dropped the car keys”? As a profession, we are strong on comparative static equilibria, but weak on dynamic political economy issues. In the few minutes I have to share with you today, I would like to

examine these questions from the perspective of one program with which I am very familiar, the state workers’ compensation programs for workers disabled at work by injuries or diseases.

In 1966, John Burton, Jr. completed a modest study that he undertook as an extension of his Ph.D. dissertation in economics at the University of Michigan. It was sponsored by the W. E. Upjohn Institute for Employment Research as part of a broader inquiry into the determinants of regional economic growth

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and development. Burton found that interstate differences in workers’ compensation costs at that time (using 1965 data) were quite trivial, on the order of 1.5 percent of the average wage bill, or about 0.25 percent of average total costs for a “typical” manufacturing firm (Burton 1966, p. 61). Since such magnitudes would be dwarfed by other, larger differences between states in employment costs, raw material costs, energy costs, transportation costs, and the like, he concluded that workers’ compensation costs could not be playing a significant role in determining plant location. At best, workers’ compensation costs might be a significant influence only on a subjective, or indicator level.

Dr. Burton went on to chair the National Commission on State Workmen’s Compensation Laws that reviewed the overall performance of these systems from 1971 to 1972. The Commission requested an update of the “Upjohn Study.” Watkins and Burton replicated and expanded the earlier work, stating:

The *Dissertation* and the *Upjohn Study* deprecated the possibility that these cost differences could cause interstate movements of employers. Nonetheless, some States fear that such cost differences can drive employers elsewhere. Reforms in State programs which will lead to higher insurance costs are sometimes avoided because of the specter of the vanishing employer, even if the apparition is a product of fancy and not fact (Watkins and Burton 1973, p. 224).

Table 1  
*Workers' Compensation Costs by Region, 1995*

	Workers' Compensation Cost per Employee Hour (\$)	Hourly Wages and Salaries (\$)	Workers' Compensation Costs as a Percent of Wages
West	.44	14.98	2.94
Northeast	.41	13.81	2.53
South	.38	12.39	3.07
Midwest	.36	12.70	2.83

Source: U.S. Bureau of Labor Statistics, Employee Benefits Survey, and Burton (1995).

But the issue refuses to die, and Burton has subsequently made a career of estimating the costs of workers' compensation insurance to employers.<sup>1</sup> In Burton's latest effort (Burton 1995), he reports that U.S. Bureau of Labor Statistics data from the 1995 Employee Benefits Survey show that the Midwest has the lowest workers' compensation costs, at an average of \$0.36 per employee hour, while the West has the highest, at an average of \$0.44 per employee hour. This is a gross difference of only eight cents per employee hour.

Moreover, when workers' compensation costs are expressed as a percentage of regional wages and salaries, the South turns out to have the highest costs (at 3.07 percent), while the Northeast has the lowest (at 2.53 percent). The reason is that regional wage differences are much larger than regional workers' compensation cost differences (Table 1).

Do we expect to see business rushing back to the Northeast because workers' compensation costs are lower, relative to wages and salaries? Do we believe that the employment growth in the Sun Belt over the last 30 years has been the result of the 3 cent per hour advantage of the South over the Northeast in gross workers' compensation costs? And if the Midwest is the cheapest place for workers' compensation insurance, why haven't we seen more business moving in to take advantage of this "fact"? Instead, we see incessant complaints about the cost of workers' com-

<sup>1</sup> The Upjohn Institute is currently supporting a study by Burton, Timothy P. Schmidle, and Terry L. Thomason which consolidates cost and benefit estimates for state workers' compensation programs for the period 1975 through 1994. They will then use these data to estimate the impact of different insurance market structures on workers' compensation costs.

pensation insurance from the business community, at least in every state that I have visited in the last 20 years.

Obviously, the answer is that many factors influence employers as they decide where to locate new plants, where to expand employment, and where to reduce employment or close plants. Workers' compensation cost differences would be only one, and perhaps a financially insignificant one, of these factors. Nevertheless, the various "business climate" efforts demonstrate that workers' compensation costs are regarded as a very significant factor in business location decisions, at least by the people who are making those decisions. In fact, the message seems to be that interstate competition is thriving, and everybody seems to be losing, at least so far as workers' compensation costs are concerned. I can validate that from my personal experience in Michigan.

Rather bold legislative changes that brought Michigan's workers' compensation costs down from 33 percent above the national average in 1978 to 6 percent above the average by 1984 did not materially reduce business complaints about excessive workers' compensation costs, nor did they change our ranking in business climate studies (Hunt, Krueger, and Burton 1988). This was very disappointing for me as a new, and rather naive, policy researcher who had never doubted that the facts would carry the day once they were known.

What is going on here? First, it is clearly possible that some firms, with particular risk profiles, may experience much greater differences in their workers' compensation costs among states than are shown in the average BLS survey results. Our work at the Upjohn Institute shows that the differences in the incidence of injury and of disability claims and in workers' compensation costs among establishments in the same industry and located in the same state are enormous. We found that "Each of the 29 (2-digit) industries . . . was found to have more than a tenfold variation between the claims experience of the lowest claim firms and the highest claim firms in the industry" (Habeck et al. 1991, pp. 215-17). But our research also shows that those performance differences appear to be associated with deliberate firm policies and procedures; that is, they are the result of specific behaviors that some firms have encouraged and supported and others have not (see Hunt et al. 1993). Thus, substantial cost differences seem to exist within a common statutory and regulatory environment.

Second, because the experience rating of workers' compensation premiums is quite aggressive, at least

for medium and large firms, it is possible that firms with good or bad workers' compensation records know that they will not pay the average premium anyway, regardless of where they are located. Thus, it may be that much of the interstate difference in workers' compensation costs actually reflects the industry composition of the economic base, rather than the statutory and regulatory regime adopted by the state.

Third, since information about price and service in this market is very imprecise, firms may not have an adequate grasp of how existing differences might affect them. It would be difficult to act on information that one does not have. It is also possible that firms truly understand that economists have "proved" that the financial burden of workers' compensation and other social insurance programs is transferred to employees in the form of lower wages, or offsets of other labor costs (Chelius and Burton 1994).

Fourth, since workers' compensation costs can be measured so many ways, there is no single unquestioned version of the truth, and it takes an expert to dig to the bottom of the matter.<sup>2</sup> This is also very convenient for nervous policymakers or aggressive lobbyists who might prefer to cloud the issue, rather than get at the "facts." For example, in the most recent Burton study of workers' compensation benefits, my own state of Michigan ranks between #1 and #35 among the states, depending on the specific measure. Michigan ranks #1 in the average cash benefits provided by statute, but #35 in maximum weekly benefits for temporary total disability as a percentage of the state average weekly wage. Yet Michigan lies 17 percent below the national average in workers' compensation benefits paid as a percent of wages for covered employees (Burton and Yates 1996).

But finally, and most important, the manifestation of these differences in the political arena, where policy is made, is undoubtedly much greater than the objective facts would support. That is why it does no good to rail against the latest business climate, workers' compensation costs, or quality-of-life rankings. "I know what I believe, so do not try to confuse me with the facts" is still alive and well at the close of the twentieth century. We have all been witness to a good deal of this through the recent Presidential campaign. Apparently these are both "the best of times and the worst of times," at least in the eyes of some beholders.

The reason that facts (and research studies) are

not all that matters is that facts are subject to interpretation, and misinterpretation. Facts are marshaled to support "outrageous" policy positions, as well as "reasonable" ones.<sup>3</sup> Business interests generally favor lower workers' compensation costs, regardless of how that is achieved. Labor interests generally favor higher workers' compensation benefits, regardless of cost impact. In this kind of policy world, facts become little more than bargaining chips. Each side "selects" the facts, and the studies, that support its position.

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*Many factors influence business location decisions, and workers' compensation costs may be a financially insignificant one. Nevertheless, "business climate" efforts demonstrate that these costs are regarded as a very significant factor, at least by the people who are making those decisions.*

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But the situation is not hopeless. I believe a very important truth is concealed here that has to do with the role of empirical research in policy formation and evaluation. Economists are strong in deductive reasoning and the use of empirical analytical techniques to test specific hypotheses; we are weak in inductive reasoning and the use of empirical findings to formulate new testable hypotheses. We are good at finding proxy variables, which are usually items that just happen to have been measured for some other purpose; we are poor at developing measures of the critically important variables that need quantification. We are expert at adding covariates to our regression models; we are failures at understanding the degree to which outcomes may be codetermined in the context of particular social systems.

In the absence of clear, incontrovertible facts, we are all capable of believing what we want to believe. But our role as researchers in policy-relevant areas is to clarify the facts, and to develop them where they do

<sup>2</sup> Witness the growing complexity of Burton's work over the years.

<sup>3</sup> Understanding that these terms are only interpretable in the eyes of the beholder, as well.

not yet exist. That is where I join with Tannenwald in appealing to you to focus your research efforts on the areas that appear to be most significant by virtue of their impact, not their measurability. If we can develop empirical studies that illuminate basic questions of fact, we will make a greater contribution than if we

fit a slightly different model to the same data set to justify another publication. Let's all stop "looking under the lamppost" and start shining our lights into the dark places. In this way we might contribute to solving the problems, rather than just restating them with greater clarity.

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