Tax and Spending Incentives and Enterprise Zones

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In early 1994, IPSCO steel corporation, which had been negotiating with several states regarding the location of a new steel mill, was offered a $75.6 million package of incentives by the State of Iowa if the company located its plant along the Mississippi River in Muscatine County, Iowa. The package included several new incentives available only in the "Quality Jobs Enterprise Zone" created especially for IPSCO: a complete exemption from local property taxes on all manufacturing machinery and equipment in IPSCO's plant, exemption from paying sales tax on construction materials and services, and a 10 percent investment tax credit. The package also included incentives from programs already in existence: a $1.5 million grant from a state program to build access roads, a $1 million forgivable loan from the Community Economic Betterment program, $0.5 million from the state's Economic Development Set-Aside Program, a $1.2 million job training agreement (under the "260E" program) with the local community college, and creation of an economic development TIF (Tax Increment Financing) district that would divert almost all of the property taxes on the building over the next 10 to 12 years to retire bonds issued to pay for plant construction costs.

This is not the end of the story. The county government agreed to cover about $1.5 million in additional infrastructure costs, and IPSCO also benefited from the New Jobs Tax Credit program, providing corporate income tax credits based on the 300 new jobs promised. Furthermore, the company's bottom line was undoubtedly enhanced by Iowa's corporate income tax system which, despite a high 12 percent marginal tax rate, imposes very low taxes on corporations such as IPSCO that export most of their output. This occurs through the use of single-factor apportionment, whereby a firm's total U.S. profits are apportioned to Iowa solely according to the percentage of total sales that are destined for Iowa.

This incentive package illustrates the whole range of economic development incentives offered to expanding and relocating firms in the United States. We divide these incentives into five classes, from the most specific to the most general:

(A) One-time deals negotiated with a specific firm, such as the property tax exemption granted exclusively for IPSCO or an agreement to finance road access to a site.

(B) Grants and loans provided under programs that receive annual state appropriations, where the firm must apply for funding.[1]

(C) Programs with established parameters and limits but with some degree of local government discretion allowed. This would include property tax abatements in some places (where the abatement is discretionary or the abatement schedule can vary) and TIF districts. These programs require no explicit funding, and so have no annual limits statewide.

(D) Tax incentives that function as entitlements: investment tax credits or jobs tax credits under the state corporate income tax, and local property tax abatements in many places. Here the firm receives the benefit automatically, provided the investment is in an eligible sector and the size of the investment or number of new jobs exceeds some threshold. There may be geographic targeting: enterprise zones are the major example.

(E) Features of the tax code that apply to every corporation, but benefit some more than others and are often advertised by economic development agencies as reasons to locate in that state. Examples are single-factor apportionment, exemption of inventories from property taxation, and exemption of fuel and utilities from the sales tax.

Much of the popular debate over economic development incentives has focused on the highly publicized deals that states have negotiated in their attempts to attract a major new industrial facility—mostly the type A incentives listed above. Many have called for an end to such "firm-specific incentives" (for example, Burstein and Rolnick 1995). The majority of the research, on the other hand, has been concerned with measuring the effects of the average level of business taxation (or of the level of public services) on economic growth. The average effective tax rates employed in such studies presumably reflect differences only of the type E variety, though some state-level type D incentives may be reflected as well. Only a modest amount of research has focused on entitlement incentives (type D), including studies of enterprise zones, and an even smaller body of research on nontax discretionary incentives. Little systematic research has been conducted on the one-time deals.

Much of the popular debate over economic development incentives has focused on deals that states have negotiated to attract a major new industrial facility, while most research has been concerned with measuring the effects of the average level of business taxation or public services on economic growth.

This raises several questions:

- Do incentives work? Does the research on incentives corroborate the findings on the effects of taxes and spending generally?
- How should tax and spending incentives be measured?
- What do we know about the relative size and importance of the various kinds of incentives, and of enterprise zone incentives versus other kinds? Does incentive competition narrow the differences across sites in terms of the after-tax return available?
- What are "firm-specific incentives" anyway, and could they be outlawed?

We have discussed the last question elsewhere (Fisher and Peters 1996a). Suffice it to say here that the practical difficulties in defining the firm-specific incentives that would be prohibited seem insurmountable, and that the efficiency arguments against such deals apply with nearly equal force to discretionary incentives and entitlements. Furthermore, banning firm-

[1] The agency typically will have some discretion in awarding funds and, in some instances, will apply eligibility criteria and clawback provisions. These grants and loans are usually applied to the cost of capital, but are also provided for infrastructure and job training.

specific deals may simply accelerate the process by which unique incentives are turned into established programs.[2]

In the remainder of this paper, we review the existing literature related to the first question above: What do we know about the effects of development incentives and enterprise zones on investment and job growth? We also draw on our own research to address the second and third questions.

The Impact of Development Incentives

It should be obvious that to claim any benefits from economic development policy we must be reasonably sure that it works—that incentives can reasonably be expected to influence the investment behavior of expanding and relocating firms. From a theoretical perspective, taxes and development incentives are a spatially variable business cost, and thus should influence location and investment decisions at the margin. However, the costs of locally supplied labor are about 14 times state and local business tax costs.[3] Regional variations in construction, transportation, and energy costs are often larger than variations in state and local taxes and, presumably, development incentives. The result is that small differences in labor and other costs can outweigh quite large differences in tax costs and incentive awards. Cornia, Testa, and Stocker (1978, p. 2) find that "a mere 2 percent difference in wages could offset as much as 40 percent in taxes." Thus some have claimed that where taxes and development incentives do influence location decisions, it is largely as tie-breakers between essentially similar locations (Schneider 1985). Unfortunately, our ability to measure the impact of incentives on location and investment decisions is circumscribed by often significant (and variable) time lags between the introduction of a policy instrument, spending allocations to that instrument, offers to individual firms, investment decisions on the part of a particular firm, the actual construction of a factory by the investing firm, and the achievement of a "normal" employment level at the factory site.

While a large literature exists on the investment and locational impacts of state and local taxes, much less work has been done on nontax or firm-specific development incentives. Part of the reason for this is the difficulty in measuring a locality's commitment to or generosity with incentives. In the United States, four methods of evaluating the impact of state and local taxes on growth have been developed: (1) econometric models of the impact of taxes on growth; (2) surveys of people making location and investment decisions; (3) hypothetical firm models looking at the effect of spatial tax differentials on a firm's income; and (4) general equilibrium models. The development incentive literature has generally followed the tax literature, except that considerable work has been done using one further method—(5) case studies of particular incentives—and there is no work using the general equilibrium approach. This section of our review focuses mainly on the econometric studies. Later sections—on enterprise zones—pay greater attention to the survey and case-study literatures. We also cover some recent results using the hypothetical firm method.

The Econometric Method

The econometric literature is very large indeed, but nearly all published models concern taxes. Moreover, of the tax models, very few have included data on local abatements or the various tax credits commonly in use at either the state and local levels. With a few notable exceptions, most models merely use effective tax rates (ETRs) as the exogenous tax variable within the location equation. Effective tax rates are calculated by dividing gross receipts (say, corporate income and franchise tax receipts) by some base (employment or population). Econometric models have been developed for various spatial scales and for a number of different state and local taxes. "Aggregate" or "macro" models have used state and local growth measures such as "levels of" or "changes in" employment, Gross State Product, per capita personal income, new plant openings, birth rates of small firms, and so on. "Micro" models, often using conditional logit techniques, focus on the decision to locate a new plant. The models also range widely in their technical sophistication—from simple regressions with poorly specified locality growth variables, no treatment of time lags in the growth variable, or fixed-effects con-

[2] In the IPSCO case, a companion piece of legislation to the IPSCO enterprise zone bill was passed in the same 1994 session. This bill established the New Jobs and Income Program (NJIP), which extended the machinery and equipment exemption and the investment tax credit to other firms making large investments, paying above a threshold wage, and creating a substantial number of jobs. The remaining incentive that was unique to IPSCO, the sales tax exemption for construction materials, was incorporated into the NJIP program in 1996. Meanwhile, in the 1995 session, the property tax exemption for machinery and equipment was extended to *all* new investment, with no restrictions.

[3] These estimates are from Bartik (1991, p. 61). It is likely that this number varies considerably by sector. See, for instance, J. Papke (1995).

trols or endogeneity in the explanatory variables, to considerably more complex models which address most, if not all, of these issues. Almost all develop equations that use local labor costs, transportation costs, energy costs, infrastructure provision, tax costs and so on—in other words, the traditional location factors—to explain local growth.

A number of impressive reviews of this tax literature have been published in the recent past, and we will not repeat that work here.[4] While no definite conclusions can be reached on the basis of the extant literature, there is a growing consensus that "The most recent studies, employing more detailed data sets and more refined econometric techniques, have generated results which cast some doubt on the received conclusion that tax effects are generally negligible" (Newman and Sullivan 1988, p. 232). Bartik (1991), in what is probably the most comprehensive assessment of recent research to date, generally supports Newman and Sullivan's conclusions. The reason for this change is that the more recent work is technically and empirically more sophisticated and thus better able to describe the relationship between taxes and growth. However, it is important to note that there have been dissenting voices. McGuire (1993), who has herself produced significant work indicating that taxes do influence growth, argues that Bartik claims too much. In particular, McGuire is concerned that some studies that found that state taxes significantly affect growth have not been replicable and are not robust to changes in specification or time period. She argues that the recent literature is as contradictory and inconclusive as the earlier literature.

Recent studies cast some doubt on the received conclusion that tax effects are generally negligible, but there are still dissenting voices.

The situation with regard to nontax development incentives is that much worse.[5] The literature is tiny and focused over a wide range of incentive types. Moreover, with the possible exception of work on industrial development bonds (IDBs), measures of the state and local development incentive effort are crude and almost certainly misrepresent the true development incentive position of states and cities. Thus, results in the extant literature are preliminary, to say the very least.

Two early and important papers were written by Dennis Carlton (1979, 1983). Carlton (1979) looked at the impact of taxes and incentives on the generation of single-establishment firms and branch plants across standard metropolitan statistical areas (SMSAs, now MSAs). The study focused on three industries. The 1983 paper modeled both the location and employment choices of new branch plants, again across SMSAs. Both these studies have been very important in the tax and growth literature and have been widely discussed. Our interest is in the measurement of development incentives and Carlton's empirical estimates of their impact on growth.

A single "business climate" variable was constructed as an index reflecting the number of state incentives provided to business. The index counted positively revenue and obligation bond financing, state loans for construction and equipment, corporate tax exemptions, property tax exemptions, accelerated depreciation, state programs on research and development, state right-to-work laws, and so on; the index counted negatively state minimum wage laws, state fair employment practice codes, and so on. In both papers the various tax variables were statistically insignificant and often had the wrong sign. The business climate index—essentially a counting of development incentives offered—performed as poorly. Carlton (1983, p. 447) concludes: "We find no support for the view that a favorable 'business climate' alone can substantially stimulate new locational activity for branch plants."

A number of other studies conducted during the 1980s included a "business climate" variable.[6] How-

[4] For a review of this literature, see Wasylenko's paper in this issue.

[5] For a review of the incentive literature focusing on the costs of incentives to government and the quality of the jobs created—issues not dealt with in any detail in this paper—see Wasylenko (1996).

[6] For instance, Plaut and Pluta (1983) analyzed the aggregate percentage change in employment in 48 states during the 1967–72 and 1972–77 periods. They included two business climate variables. The first of these used a principal components index that explained 90 percent of the variance of the Fantus and COSMA (Alexander Grant) business climate rankings. Plaut and Pluta (1983) found that business climate, like taxes, was not significantly related to overall state industrial growth but was significantly related to state employment and capital stock growth. However, since the business climate variables used mostly reflect state variation in average factor and tax costs, it is entirely unclear what the direct relationship is between the provision of development incentives and employment and capital stock growth.

ever, in most cases the incorporation of such variables into econometric models of growth provided only a broad measure of a state's or city's pro-business investment attitudes. Seldom did such variables measure directly the development incentives available in a state or locality. Our focus is on studies, such as Carlton (1979), where the provision of development incentives is measured directly.

Wasylenko (1988), updating earlier work by Wasylenko and McGuire (1985), examined the percentage change in state employment between 1980 and 1985. Among the various fiscal variables entered into different equations was a counting of financial incentive programs. Wasylenko found that development incentives were significantly associated with lower total employment growth and with lower employment growth in manufacturing and retail trade. The association was statistically insignificant for the other industries tested. However, the number of employment training programs did appear to have the desired effect. They had a statistically significant and positive effect on total manufacturing employment growth. He writes that "one cannot reject that fiscal variables may influence firm location" (Wasylenko 1988, p. 20). He also points out that few studies have explicitly addressed the impact of nontax financial incentives; those few studies suggest little effect (p. 23).

In an important variation of this method, Walker and Greenstreet (1990) conducted an extensive survey of 540 new manufacturing plants in the Appalachian region. They then performed various analyses of the data. Incentives (such as site-specific infrastructure, low-interest loans, training subsidies, land and building and tax breaks) were treated as dummy variables. In one set of equations, the presence of an incentive resulted in the incentive dummy being coded "1," while in another set, if the incentive package dominated competing packages, the variable was coded "1." Two different sorts of analyses were undertaken. In the first, location decisions were modeled using discrete choice analysis. In these equations, incentives were consistently and significantly related to location decisions. In the second, on-site employment expansion was examined through a two-stage, generalized least-squares model. In these equations the various programs, though still treated dichotomously, were differentiated by type (for example, leasing, job training, interest subsidy, and so on). They found that such government programs have done "little to accelerate expansion of establishment employment once a plant is operating" (Walker and Greenstreet 1990, p. 24).

While incentive offerings can be effective in attracting industry at the local level, the authors nevertheless caution that their analysis leaves unanswered a number of important questions. In particular, since their work covered Appalachia only, they are unable to estimate the utility of incentives at higher scales in the search hierarchy. In particular, "if incentives work exclusively at the sub-regional level, regional policy based on them could lead to wasteful, intra-regional competition" (Walker and Greenstreet 1990, p. 25).

Of course, research that relies, in one form or another, on simple program counting measures, such as Carlton's (1979, 1983), Wasylenko's (1988), and Walker and Greenstreet's (1990), may seriously misrepresent a state's or city's commitment to economic development and the generosity of the incentives provided. Fisher and Peters (1996a, pp. 3–3 and 3–4) in a detailed survey of development officials in 24 states found the following examples: (1) programs on the books and in incentive directories that no longer are, or never were, funded; (2) states that consolidated several programs into one program or split a program into parts, without actually changing the actual incentives available; (3) states with several programs providing essentially the same thing, such as loans, but in slightly different circumstances; (4) similar-sounding programs that are of very dissimilar magnitude because of differences in program constraints or ceilings; (5) multiple programs in one state that provide less of value to business, taken together, than a single large program in another state; and (6) multiple programs that are independent and additive in one state, but mutually exclusive in another. The number of programs offered by a state or city is clearly close to useless as a summary measure of the state or local economic development effort. Moreover, program counting may result in other problems. Fisher and Peters (1996a) claim that differences can be found in

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the levels of coverage (and thus the programs listed) among the various directories of state-level programs (those published by *Site Selection*, *Area Development*, and The National Association of State Development Agencies (NASDA).[7] While an effort has been made to improve information on programs, most previous research has relied on flawed directories. Part of the reason for this problem is that almost no work has defined exactly what constitutes a development incentive.

A small amount of work over the past few years has tried to find better summary measures of the local development effort. Goss (1994), for instance, developed two-stage, least-squares models with state growth rates in enterprises and growth rates in establishments as alternative dependent variables. Exogenous variables covered the usual proxies for the standard set of factor and transaction costs included in such equations (wages, energy prices, personal and corporate taxes, and so on). Economic development—measured by state agency spending figures provided in the NASDA data base—and infrastructure spending were treated as endogenous. Goss finds that economic development spending had a positive impact on the formation of both new enterprises and new establishments. Moreover, the inclusion of economic development spending in the various equations he presents has an important impact on other variables. Notably, in equations that include economic development spending, both state spending on infrastructure and personal and corporate taxes are significantly (and with the appropriate signs) associated with business formation rates. Equations without economic development spending do not exhibit this result. In fact, Goss argues that a possible reason that so much of the extant literature on state and local taxes and growth has not found taxes significant is that they have failed to control for economic development spending.

In a variation on Goss's work, Goss and Phillips (1994) ran a series of similar models but with employment growth rates dependent. Their results are much the same as those of Goss (1994). They find the economic development spending coefficient statistically significant. However, unlike the result using the Goss (1994) models, the inclusion of the economic development variable did not improve the performance of the effective corporate tax rate variable, although Goss and Phillips do find that the incorporation of economic development spending increases the elasticity of personal taxes from -1.65 to -1.88. The elasticity associated with economic development spending is 0.20. At first glance this number appears small and thus unsuspicious—however, it implies that a mere 10 percent rise in economic development spending would increase state employment by 2 percent. Given the small size of state economic development budgets relative to state employment, the elasticity seems much too optimistic. Goss and Phillips admit that the economic development spending variable may be acting as a proxy for other variables (such as the state business climate) not included in the model, and that the model ignores most substate economic development spending. They conclude that "the impact of economic development spending on employment growth rates may be powerful enough to overcome the negative effect of raising taxes to fund economic development spending within a narrow range of changes" (Goss and Phillips 1994, p. 298).

State development agency spending data from NASDA (in this case, the 1990 data) have also been used by de Bartolome and Spiegel (1997) in models looking at employment growth in manufacturing. Although they find that the state corporate tax rate is a significant determinant of manufacturing employment growth in only one of their models, state spending on economic development is a robust and statistically significant determinant of growth. They claim that their results accord "with the general thrust of the literature that well-targeted development programs have a greater impact than lowering tax rates" (de Bartolome and Spiegel 1997). We do not believe the literature warrants such a conclusion. Overall, the various work using economic development agency spending as an explanatory variable does suggest that spending on development incentives causes employment growth.

However, a major problem with the last three studies reviewed is that economic development expenditure data are very poor indeed. The NASDA (1982, 1986, 1990) expenditure and salary survey data, used in all three, suffer from a number of important problems. Some crucial categories of economic development spending are not included in state development agency expenditure. Loan guarantees, loan subsidies, linked deposit programs, development credit programs, and even long-standing revolving loan funds all present few direct costs to state development agencies but may nevertheless provide the most generous state incentives available. Moreover, in many

[7] For instance, development credit corporations make the *Site Selection* 1992 list for California, but do not make it into the 1992 *Area Development* list.

states, training expenditures and special economic development infrastructure expenditures are not run out of state economic development agencies. Training programs are often run through, and thus funded by, state labor departments, while economic development infrastructure incentives are typically the responsibility of state departments of transportation. In many states training and infrastructure incentives, taken together, are worth more to a firm than all other economic development incentives (Fisher and Peters 1996a). Moreover, state economic development agency expenditures also include a number of noneconomic development activities, most importantly tourism.[8] Admittedly, in places Goss talks of the economic development effort as recruitment, and he reports that in 1986 states spent a median of 63 percent of their industrial development funds on recruitment. Nevertheless, the NASDA expenditure data are a poor measure of the total economic development effort.

Furthermore, Goss's, Goss and Phillips', and de Bartolome and Spiegel's analyses are conducted at the state level. So too is all the work relying on program-counting measures. This parallels a problem in the tax literature. It may very well be that states with overall low state taxes have transferred more of the nonfederal tax burden to the local level. Similarly, generous state development incentives may make generous local incentives unnecessary, while scarce state resources may induce local governments to increase their spending on economic development. We believe this is indeed the case. Focusing on state taxes and incentives may severely distort actual spatial differentials in tax and incentive regimes across the American economy. Finally, some of the most important economic development spending occurs at the local level. Abatements, tax increment financing mechanisms, and the provision of customized infrastructure are often the most generous of all state and local incentives offered to a firm. All three are financed out of local property taxes. In some states, notably Iowa and Missouri, customized job training is also organized at the local level. Focusing merely on state expenditures severely distorts the true level of policy commitment to economic development.

Other work has been much more careful about development incentive measurement. Luger's 1987 study of economic development incentives is a case in point. Luger develops a summary measure of industrial development by measuring expenditures on eight categories of industrial development incentives. Many of these programs are either taxes or broad spending on education. Nevertheless, five—land and building subsidies, the provision of debt and equity capital, subsidized job training, business recruiting and outreach, and research and development support—fall within a more usefully restricted definition of development incentives. Luger then uses regression equations to measure the impact of development incentives on levels of, and changes in, wages and the unemployment rate. His two most important findings are that job training and debt and equity programs may result in lower average wages, but they may nevertheless help reduce unemployment rates. Land and building subsidies, state tax programs, business recruiting, and research and development have no effect on wages or the unemployment rate. However, Gerking and Morgan (1991, pp. 47–48) argue that Luger's somewhat perverse results, on training and debt and equity capital programs on the one hand and wages on the other, could "stem from a failure to eliminate simultaneous equation bias from the estimates or a poor choice of dependent variables."

Some of the most important economic development spending occurs at the local level, and focusing on state taxes and incentives may severely distort actual spatial differentials across the economy.

The Luger index of development effort has been used in a few other studies of investment. Luger and Shetty (1985) used it to look at foreign firms' locational choices, and Woodward (1992) used it to analyze the locational determinants of Japanese manufacturing start-ups at the state level in the United States. The Woodward study is particularly important since it carefully distinguished between location factors at the state and county levels. However, in neither study did the Luger index prove to be statistically significant.

Loh's (1993) recent work tries to resolve some of the measurement issues associated with development

[8] de Bartolome and Spiegel (1997) attempt to improve the basic NASDA data set. They remove expenditures targeted towards tourism promotion and the film industry, and also federal pass-through funds.

incentives. Although the paper focuses on only one state, Ohio, it presents what is probably the best use of incentive data thus far. The various equations include a grants variable—the scaled total dollars granted by the county between 1982 and 1990—and similar variables for loans, community development spending, business subsidy spending, and training outlays. Other reasonably standard local location variables, meant to capture labor force quality and amenities, were included in the equations. County employment growth was dependent. Moreover, concentrating on a single state means that these variables are measured consistently over space. The model allows comparisons between incentive types, and the data used provide a much better approximation of the incentive offers available, by county. Loh found that total outlays or total number of projects significantly increase employment growth. Models were run that disaggregated incentives by type. These showed that grants were better predictors of growth than loans, and business subsidies better predictors than training subsidies or community subsidies. Regressions were also run for specific 1-digit industries. Here Loh found that the significance of the coefficients was not uniform across industries.

Although Loh's measurement of development incentives is probably the best in the econometric literature, it is still vulnerable to some serious criticisms. Presumably, it is hoped that development incentives will encourage investment by reducing the costs associated with a particular site. Thus, location models should be able to catch the way in which development incentives reduce the costs experienced by the firm. The problem with all the measures discussed thus far, including Loh's, is that they do not do this. From the point of view of the firm, the benefits associated with a $1 million grant are not the same as those associated with a $1 million loan. Most obviously, the benefits of a loan are determined by the rate, term, and fees associated with that loan; the benefits are not a function of size alone. More worrying for those developing location models, the benefits of a grant, loan, or any other development incentive are mediated by the tax regime experienced by the firm; a $1 million grant does not raise firm income by $1 million. Measures of development incentives need a method of taking these problems into account. We return to this issue later.

The literature on industrial development bonds (IDBs) is better developed than that for any other single economic development instrument. One reason is that IDBs present fewer problems of measurement than most other economic development instruments. Unfortunately interest in IDBs has declined since federal restrictions, particularly the Tax Reform Act of 1986, reduced their importance to local economic development financing. Almost all studies look at bond issuance as the measure of economic development activity.

Hellman, Wassal, and Falk (1976) analyzed the relationship between IDB volume and total state investment in Kentucky. They found a small yet statistically significant relationship between the two. Steinnes (1984) ran a pooled cross-section, time series study of employment across 15 states. The issuance of IDBs was not statistically significant in any of his regressions. McHone (1984) looked at the relationship across 26 multi-state SMSAs. He found no relationship between IDB issuance and employment. A study by Carlino and Mills (1985), conducted at the county level, came to much the same conclusion. Stutzer (1985) found no effect of small industrial revenue bonds on employment growth in Minnesota. However, Marlin (1990), who used recent Treasury data to look at the relationship between IDBs and change in Gross State Product over the 1983–86 period, found that more intensive use of IDBs was positively associated with faster growth in GSP.[9] This relationship was significant when IDBs were measured in per capita terms or measured in proportion to total tax-exempt debt issued by the state. Interestingly, Marlin finds, "A similar relationship could not be established between the volume of other private use tax-exempt bonds and GSP . . . it may be that the IDB variable used here is serving as a proxy for the aggressive use of economic development subsidies in general. In this case it would be more accurate to conclude that those states that actively pursued economic development strategies were successful in meeting their objectives" (Marlin 1990, p. 21). As with the more general econometric literature on economic development, the work on IDBs does not support any firm conclusions about the impact of IDB issuance and growth, although the majority of the evidence suggests little impact.

The Survey Technique

In a number of studies, researchers have surveyed executives to determine what role taxes and development incentives play in the firm's relocation and expansion decisions. The surveys often distinguish

[9] One other study covered all states—the study was undertaken by Wasylenko and is published as Appendix C in Stutzer (1985).

between "must have" location factors and merely "desirable" factors. Since the evidence shows that large manufacturing firms tend to make their final location choice based on a sequential evaluation of factors at successively narrower spatial scales, deciding first on a broad geographic region, then a state, a metropolitan area (or county), a city, and finally, a plant site, some surveys have attempted to distinguish the impact of incentives (and other locational factors) at various spatial scales (Schmenner 1982).

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The advantages and disadvantages of the survey technique are well known (Calzonetti and Walker 1991). At their best, surveys provide direct information about the actual siting decisions made by executives. Moreover, the more complex statistical assumptions that beset econometric analyses can be avoided. Unfortunately, survey researchers often have difficulty finding the cohort of individuals within a corporation who were responsible for a particular location decision. Moreover, executives may have a direct interest in saying that incentives are important even if they were not (admitting that an incentive had little effect on one's location decision might cause later political problems), although, given the findings of the literature, this problem may have been exaggerated. Finally, while surveys may rank the importance of various locational factors, they do not provide a precise measure of the impact of each locational factor on local growth.

The results from the survey-based tax literature are unclear, with some research indicating that incentives are indeed important to location decisions (Premus 1982; Walker and Greenstreet 1989; Calzonetti and Walker 1991; M. Rubin 1991), and other studies indicating the opposite (Morgan 1964; Stafford 1974; Schmenner 1982).[10] Part of the confusion may have to do with research methodology. But it is also possible that some development incentives are important while others are not, and the confusion in the literature merely reflects this fact. Work by Glaser and Yeager (1990), who attempted a comprehensive survey assessment of the various classes of incentives, appears to support this conclusion. Their research covered everything from tax incentives, capital assistance, property development, and zoning flexibility to labor force development assistance. The study focused on Wichita-Sedgewick County, Kansas. They find that property tax abatements and regulatory flexibility are widely considered important by firms. Direct low-interest loans were the only capital-related incentive to enjoy broad-based support from most business types. More esoteric capital assistance, such as venture capital and incubator funds, was not valued highly by business.

The Case Study Technique

Some researchers have used variations on the case study method to evaluate the impact of specific economic development programs. The advantage of this method is that the work has covered a variety of different incentive instruments, from enterprise zones, research parks, and property tax abatements to export promotion schemes. Unfortunately, this approach also has major problems. In the first place, incentive programs are often very small relative to the local economy in which they operate. Thus, even where subsidies are effective, measuring their impact on a local economy is rendered difficult by economic white noise—the other local factors that influence growth. Moreover, impact evaluations need to establish some sort of comparative control economy in order to measure the effect of incentives precisely. But choosing a control—in the best of all worlds, the control economy would be identical to the economy receiving the incentive, except that the control would not receive the incentive—is itself fraught with practical methodological and political difficulties.

Not surprisingly, given the range of programs covered, the published research using the single program approach is as contradictory—in terms of both detailed method and results—as the survey-based literature.[11] Moreover, much of the research concerns

[10] For recent reviews of the survey literature, see Calzonetti and Walker (1991), Eisinger (1988), and Blair and Premus (1987). For a review of the early literature, see Morgan (1967).

[11] Bartik (1991) has also provided a recent review of this literature. He finds that the literature is generally supportive of the notion that incentives influence the locational behavior of firms.

issues of fiscal impact or cost-benefit ratios of programs. Apart from the topic of enterprise zones, little work has considered the impact of incentives on location and investment decisions. However, even work focusing on broadly similar types of programs shows discrepant results. For instance, in a recent, widely quoted volume on enterprise zones, one paper found clear evidence of impact success (M. Rubin 1991), while two other papers found little or none (Elling and Sheldon 1991; Grasso and Crosse 1991). We will return to these issues in our more detailed discussion of enterprise zones.

The Hypothetical Firm Technique

Given the difficulty of drawing any firm conclusions based on the existing literature, a few researchers have opted for an entirely different approach to the problem of taxes, incentives, and growth. This solution involves looking at the impact local taxes and incentives have on a firm's actual income. In order to accomplish this, researchers build models that replicate the operating ratios, balance sheets, and income and tax statements of real, or at least "potentially" real, firms. This allows researchers to calculate exactly what impact a state's or city's taxes have on a firm's income.[12] Almost all the work in this tradition has looked at comparative tax burdens and has ignored incentives. In a few cases, the results of hypothetical firm studies have been included in econometric analyses of the relationship between taxes and growth (or, at least, taxes and investment). Industry-specific measures of the burden of taxes deriving from the hypothetical firm model replace effective tax rates (ETRs) as one of the independent variables in the econometric equation (Steinnes 1984; L. Papke 1987, 1991; Tannenwald and Kendrick 1995; Tannenwald 1996).

Very little work has used the hypothetical firm method within an explicitly economic development framework, although the benefits of doing so are large. Bartik et al. (1987) analyzed the location of GM's Saturn Plant. Using realistic simulations of transportation, labor, and tax costs[13] they calculated that the best location for the new plant would be Nashville, Tennessee, about 30 miles from the actual site chosen by GM, Spring Hill. They then incorporated estimates of incentives offered to see how these would influence the location decision. For instance, Tennessee's training and property tax subsidies lowered Saturn's cost per car by $34, with $4 of this coming from the training subsidy and $30 from the property tax reduction (Bartik et al. 1987, p. 32). The implication of this finding is that where subsidies result in the lowering of a potential plant's cost structure, they may influence a firm's locational choice. Bachelor (1991) estimated the value of incentives received by Mazda from the state of Michigan.[14] However, neither Bartik et al.'s (1987) nor Bachelor's (1991) work explicitly incorporates economic development incentives such as grants, loans, training awards, and the like into a hypothetical firm framework. We believe one way for research on incentives and growth to move forward is to rigorously implement a hypothetical firm model covering both taxes and nontax economic development incentives.

One way for research on incentives and growth to move forward is to rigorously implement a hypothetical firm model covering both taxes and nontax economic development incentives.

Over the past few years, we have attempted to develop a hypothetical firm model (the Tax and Incentive Model or TAIM) that fully incorporates nontax development incentives (Fisher and Peters 1996a, 1996b). TAIM measures competition among places, based on the dollar value of the locality's standing incentive offer to industrial firms expanding or locating there. The standing offer includes the whole range of competitive incentives over which state or local governments have some direct control. Since incentives may be embedded in tax codes, and since the value of incentives to a firm must be measured net of income tax effects, we also model the federal corporate income tax, each state's and city's corporate income and net worth taxes, the major state and local sales taxes paid by business, and property taxes.

[12] Examples of this literature include Papke and Papke (1984), Hunt (1985), Brooks et al. (1986), and Laughlin (1993).

[13] Using the Papkes' AFTAX hypothetical firm model.

[14] This did not use a hypothetical firm model but a related cost-to-government/benefit-to-firm technique developed by Rasmussen, Bendick, and Ledebur (1984).

We constructed financial statements for 16 hypothetical firms, representing the characteristics of typical large and small firms in each of eight fast-growing manufacturing industries. The model then measures the net returns on a new plant investment, after state, local, and federal taxes, and after state and local competitive incentives. The new plant is located in one of the 24 states that account for most of the manufacturing employment in the United States, and in one of 112 cities, randomly selected from within these 24 states.[15] TAIM allows simultaneous taxing over multiple states—thus, the effects of multi-state taxation across the 50 states are modeled. The firm is assumed to sell to a national market (apportionment formulas and throwback rules are included). The model incorporates federal income taxes and each state's and city's corporate income and net worth taxes; it also includes the major state and local sales and property taxes paid by business.

Tax incentives modeled include state corporate income tax credits for investment or job creation and local property tax abatements. Using a series of computerized expert systems, TAIM also models likely development incentive awards, based on the historical record of each incentive program under consideration.[16] Incentives include all major state and city grants, loans, loan guarantees and subsidies, linked deposits, and tax increment financing instruments, including those restricted to training or infrastructure. The difference between returns on investment with incentives and returns with only basic taxes modeled measures the value to the firm of the incentives offered. Calculations are done over a 20-year period in order to capture the full effects of incentives. Project returns are the incremental value of cash flow over the 20-year period.

Is there sufficient variation in returns on investment across states and cities that tax and incentive differences could plausibly affect location decisions? Table 1 provides summary information on project returns for each of the 16 firms at the 112 city locations. The coefficient of variation and the range both suggest substantial differences in returns among sites. For instance, the 20-year cash flow difference between a small furniture and fixtures firm investing a new plant in the least profitable city and the most profitable city is just under $1 million. (The small furniture and fixtures plant involves an original asset investment of $5 million.) For the large drugs firm, the difference is $58 million (for an investment of $470 million), for the large motor vehicles firm, also $58 million (for an investment of $600 million). Clearly, these differences are substantively significant—but are they significant enough to influence location decisions?

The last column of Table 1 translates the range between the best and worst cities into wage equivalent figures. Given the level of employment modeled for each plant, and assuming that all employees work a 40-hour week over a 50-week year, what is the present value wage equivalent of the range? For some firms, the results are startling. For the large drugs plant, the differences between the best and worst sites translate into an average hourly wage difference, for the full 20-year period, of $1.82 per employee. Moreover, the spreads between cities at the 80^{th} and 20^{th} percentiles or between the 75^{th} and 25^{th} percentiles, for instance, remain large. Thus, it seems reasonable to conclude that, *at least at the extremes*, taxes and incentives are potentially large enough to influence location decisions. The worst cities are substantially worse than the best cities. Nevertheless, for most states and cities, small changes to their tax and incentive systems are unlikely to make much of a competitive difference. In hourly wage terms, most cities are separated from the city just above them in rank by less than a penny. We doubt such separation is substantively significant. In fact, a rank position change of as many as 20 places often represents less than a 25-cent difference in hourly wages per employee.

For the handful of cities and states at the top, or the handful at the bottom, of the rankings for any particular firm type, we find very substantial differences in returns between one city or state and the next due to differences in taxes and tax incentives; and the inclusion of nontax incentives very often did little to change the identity of the majority of cities in the top 20 or bottom 20. Mostly, cities that were highly competitive after taxes and tax incentives were also highly competitive after the inclusion of nontax incentives. Overall, nontax incentives do not ameliorate, but actually accentuate, the tax differentials between the best and worst cities.

[15] Together, these 24 states account for about 87 percent of U.S. manufacturing employment.

[16] For instance, the expert system would first apply the historical per employee, per investment dollar, per equity dollar average award to the new project to come up with a simulated initial award. Explicit and implicit program rules, and rules governing the award of multiple programs, would then be applied to (usually reduce) this simulated initial award. In the case of multiple competing non-additive programs (for example, two capital grants offered by a state), the final simulated award is the one that minimizes the cost of financing to the firm. Simulated awards are not risk-adjusted according to the probability of receiving an award; thus, the final simulated award represents the likely offer if the firm actually received an award.

Table 1
Project Returns After All Taxes and Incentives, Multi-State Firms Locating Across 112 Cities

Firm's Industry and Size	New Plant Assets ($ millions)	Mean Return	Coefficient of Variation	Range	Hourly, per-employee wage equivalent of the range between best and worst sites
Furniture and Fixtures, Small	5	$ (18,434)	−9.03	$ 883,219	$ .72
Furniture and Fixtures, Large	40	$ 9,346,248	.11	$ 5,461,309	$ .48
Drugs, Small	50	$ 18,592,101	.05	$ 5,237,500	$ .95
Drugs, Large	470	$272,501,918	.04	$58,097,457	$1.82
Soaps, Small	20	$ 8,613,846	.05	$ 2,363,827	$ .94
Soaps, Large	110	$ 53,803,767	.05	$13,344,649	$ .82
Plastics, Small	5	$ 320,545	.48	$ 789,037	$ .84
Plastics, Large	70	$ 20,141,723	.08	$ 9,111,314	$ .90
Industrial Machinery, Small	10	$ 356,586	.80	$ 1,266,732	$ .86
Industrial Machinery, Large	250	$ 24,464,584	.21	$23,877,858	$ .66
Electronics, Small	20	$ 1,135,210	.43	$ 2,425,336	$ .66
Electronics, Large	200	$ 7,951,177	.53	$20,631,480	$ .76
Autos/Auto Parts, Small	120	$ 15,742,613	.18	$16,897,421	$ .70
Autos/Auto Parts, Large	600	$ 9,189,576	1.35	$57,782,121	$ .81
Instruments, Small	10	$ 2,024,889	.12	$ 1,017,677	$ .58
Instruments, Large	180	$ 58,935,884	.05	$15,861,121	$ .65

Note: "Hourly, per-employee wage equivalent of range" assumes that each employee in the plant works a 40-hour week over a 50-week year, for a 20-year period. The numbers reported are the present value equivalents (discounted using the firm-specific discount rates) of the ranges between the best and worst sites.
Source: Fisher and Peters (1996a).

Our research has not looked at spatial differentials in other factor costs (such as labor, energy, and transportation) or at the benefits firms receive from taxes, so we are not able to say whether a state's or a city's tax and incentive regime could reasonably be expected to alter a firm's location decisions. However, our results suggest that for the firm types simulated by TAIM, the range of results across all 112 cities (and 24 states) is not trivial. It is of course quite possible that factor cost differentials—in labor, energy, and transportation—could amount to much more than tax and incentive differentials. For instance, it is possible that labor costs may be much lower in a bottom-ranked city than in a top-ranked city. But if tax and incentive regimes were designed to make up for locally high labor costs (or other factor costs), it would be reasonable to assume that southern states would tend to have burdensome tax and incentive regimes, while California and states in the Northeast and Midwest would tend to have much lighter loads. We found no evidence to support this claim; spatial variation in the tax and incentive burden appears to be quite random (Fisher and Peters 1996a, ch. 4). Given that the severity of local tax and incentive regimes does not appear to bear an inverse relationship to factor costs, it seems reasonable to assume that in some cases tax and incentive differentials between top- and bottom-ranked locations could sway plant location decisions.

Enterprise Zones

With the passage of the Empowerment Zone and Enterprise Community Act in 1993, the federal government jumped into the enterprise zone arena, until then exclusively a state and local policy domain in the United States. Between 1981 and 1991, 38 states and the District of Columbia passed enterprise zone legislation. As of 1995, 34 of those programs remained active and in those states 2,840 zones had been established (Wilder and Rubin 1996).[17] The zones vary widely in size, from sites smaller than 50 acres to entire counties (Rubin and Richards 1992); Erickson

[17] Of these zones, 2,083 were in just two states—Arkansas and Louisiana—and another 227 were in Ohio. At the other extreme, seven states had three or fewer zones. See Wilder and Rubin (1996).

and Friedman's (1990a) study of 357 zones found a median population of 4,500 and size of 1.8 square miles. The federal program added 11 empowerment zones and 99 enterprise communities to this list in late 1994 (Hambleton 1996). The empowerment zones are located in eight major cities, each eligible for about $100 million in federal aid, and three multicounty rural areas, each eligible for $40 million. The enterprise communities are smaller cities and rural areas, each eligible for $3 million in federal assistance.[18]

We will first examine the nature and size of enterprise zone incentives and their importance relative to other kinds of incentives, drawing on our own research. Then we will review selected previous studies of enterprise zones that have attempted to identify their effects on growth. Reviews of the enterprise zone literature have been conducted in recent years by others (Rubin and Richards 1992; L. Papke 1993; Wilder and Rubin 1996), so we will focus our attention on what appear to be the most important results. We conclude with a discussion of two issues: the factor substitution effects of incentives and the job or investment relocation issue.

The Size and Nature of Enterprise Zone Incentives

In terms of the types of incentives offered, enterprise zones are little more than geographically targeted versions of standard state and local economic development programs.[19] The typical state enterprise zone program includes investment tax credits, jobs tax credits, sales tax exemptions or credits, and property tax abatements. In Fisher and Peters' (1996a) study of incentives in 24 states in 1992, 22 of those states had enterprise zone programs, and 20 of them were active programs whose incentives were modeled as part of the study.[20] Five of the 24 states provided statewide investment tax credits (ITCs), and four of these five provided more generous versions within enterprise zones. Another four states provided ITCs only in zones. Similarly, four of the states provided statewide jobs tax credits, and two of these provided more generous versions in enterprise zones. Another 14 states provided jobs credits exclusively to firms locating in zones. Seven states provided a full or partial exemption of income taxes on profits attributable to zone investment. Thus state corporate income tax credits in general were much more prevalent and more generous in enterprise zones than statewide, and within enterprise zones, jobs credits were employed twice as often as investment credits.

In terms of the types of incentives offered, enterprise zones are little more than geographically targeted versions of standard state and local economic development programs.

Sales tax exemptions, on the other hand, generally are offered statewide. Exemptions for sales taxes on manufacturing machinery and equipment were permitted statewide in 19 of the 24 states, while only two states restricted such exemptions to enterprise zones. The corresponding figures for fuel and electricity exemptions were 17 and two. Four states exempted from sales taxation virtually all personal property purchased for business use in an enterprise zone. Of the 22 states with enterprise zone programs, 19 permitted local property tax abatements in the zones, although in 13 of those 19 states, abatements were permitted outside zones as well.

In our sample of 112 cities, 44 contained enterprise zones. The total incentive package available in these zones was, on average, worth two to three times as much as the incentive package available in the average city without an enterprise zone. Table 2 shows the average value of incentive packages inside and outside zones for five of the firms in our study, to illustrate the range of effects found, and the average for all 16 firms (a large and a small firm in each of

[18] There are also four "enhanced enterprise communities" in large cities, each eligible for $25 million in aid.

[19] Some researchers place considerable importance on the community development component of zones, asserting that the more successful zones are those that are better managed and involve close ties between the public and private sectors (Rubin and Richards 1992). It is difficult to provide convincing empirical support for the latter contention since the standard of "success," the number of new jobs created, does not measure the impact of the zone per se but simply the total employment gains claimed by administrators.

[20] Michigan is often listed as a state with an "active" program though it has only one small enterprise zone. Minnesota phased out part of its enterprise zone program but allows a handful of small zones in border communities to continue. Neither of these state programs was considered significant enough to model in the authors' study.

Table 2
The Importance of Enterprise Zone Incentives

Firm:	Furniture	Soaps	Plastics	Autos	Instruments	Average:
New Plant Assets (millions):	$40	$20	$5	$600	$180	16 Firms
42 Cities of 25,000 or More Population with Enterprise Zones (EZs)						
Total EZ tax incentive package	$ 743,711	$366,505	$ 86,654	$ 5,922,955	$2,196,186	$1,639,759
Total incentive package	$1,705,411	829,040	$305,571	$11,666,932	$4,351,090	$3,332,581
State EZ incentives as % of total EZ package	79.1	82.6	80.4	61.9	72.1	69.1
EZ tax incentives as % of all tax incentives	70.8	72.9	73.8	58.6	66.2	52.7
EZ tax incentives as % of total incentive package	43.6	44.2	28.4	50.8	50.5	49.2
Ratios: Incentives within EZs in 42 Cities versus Incentives in 49 Cities without EZs (25,000 or more population)						
Non-EZ tax incentives (available citywide)	2.79	2.54	2.66	2.62	2.91	2.70
Total tax incentive package	9.55	9.37	10.15	6.34	8.62	7.32
Non-tax incentives available citywide	.95	.95	1.24	.90	.95	.95
Total incentive package	2.13	2.08	1.89	3.50	2.95	2.96
For a Representative City in Each State That Has an Enterprise Zone Program (20 states)						
Average State-Local Tax Rate on New Investment (%)						
Outside enterprise zones	7.7	7.1	11.5	13.7	6.0	9.1
Within enterprise zones	6.1	5.7	9.4	11.1	4.9	7.3
Enterprise zone effect	1.7	1.3	2.2	2.6	1.1	1.7
Percent reduction in tax rate due to EZ	21.4	19.0	18.8	18.9	18.0	19.1
Lowest State-Local Tax Rate on New Investment (%)						
Outside enterprise zones	5.1	4.3	7.5	7.0	4.3	5.6
Within enterprise zones	3.4	3.1	4.9	4.1	2.6	3.7
Highest State-Local Tax Rate on New Investment (%)						
Outside enterprise zones	11.6	13.4	17.0	23.9	8.3	14.7
Within enterprise zones	9.6	10.2	15.9	23.8	7.2	13.2

Note: Because only two of the 21 cities under 25,000 population had enterprise zones, we confined the analysis to cities of 25,000 or more. The value of EZ incentives is measured by the difference between the present value of the cash flow attributable to the new plant, after all taxes and tax incentives, and the present value of new plant cash flow given all tax incentives except enterprise zone incentives. The total incentive package includes job training, infrastructure, and general financing programs. Representative cities were given the approximate median property tax rate for that state, and a typical property tax abatement program based on the sample cities in that state. For the definition of tax rates on new investment, see footnote 22.
Source: Fisher and Peters (1996a).

eight industries).[21] Within the enterprise zones, zone incentives accounted for 35 percent of the total incentive package, on average, with substantial variation depending on firm characteristics. State incentives represented about two-thirds of the total enterprise zone package, the remainder consisting of local incentives (mostly property tax abatements). It is significant that general incentives (available to non-EZ firms) were two to three times as generous in the EZ cities as in the non-EZ cities. In other words, the cities that contained enterprise zones were providing larger incentives citywide than the average city. The average city with an enterprise zone also had a 27 percent higher unemployment rate, a 45 percent higher poverty rate, and an 85 percent higher proportion of blacks in the population, as the cities without zones.

The average effective state-local tax rate in the 20 states with active enterprise zones was measured by constructing a representative city for each state, with a property tax rate equal to the average or the median for that state and a property tax abatement program typical of the cities in our sample for that state.[22] The tax rate in these cities averaged 9.1 percent outside of enterprise zones, but 7.3 percent within a zone. The average state enterprise zone program thus reduced

[21] The five firms are a large furniture manufacturer (SIC 25); a small maker of soaps, cleaners and toiletries (SIC 284); a small miscellaneous plastic products firms (SIC 308); a large motor vehicle manufacturer (SIC 371); and a large instruments firm (SIC 382 and 384).

[22] Effective tax rate is the difference between the present value of the cash flow from a new plant investment after all federal, state, and local income, sales, and property taxes, and the present value of new plant cash flow in the absence of any taxes levied by the state and locality in which the plant is located, divided by the present value of before-tax income attributable to the new plant.

the state and local tax burden on new investment by about 19 percent. Although effective tax rates differ dramatically depending on firm characteristics, zones had similar effects (in terms of the percentage reduction in the tax rate) across firms. There was considerable variation across the 20 cities, however. Outside enterprise zones, the effective tax rate was 5.6 percent in the lowest-tax city (averaged over the 16 firms) and 14.7 percent in the highest tax city. With zone incentives included, the tax rates among the states ranged from 3.7 percent to 13.2 percent. Tax rates are lowered by EZs, but the variation across states remains high. Some states with high average tax rates, such as California, have very generous enterprise zone incentives, while other high-tax states, such as Washington, have enterprise zone programs that do very little to offset these taxes.

Previous Research on the Effects of Zones on Growth Rates

Bartik's (1991) review of the literature on the effects of taxes on economic growth leads him to conclude:

> The long-run elasticity of business activity with respect to state and local taxes appears to lie in the range of −0.1 to −0.6 for intermetropolitan or interstate business location decisions, and −1.0 to −3.0 for intrametropolitan business location decisions. That is, if a given small suburban jurisdiction within a metropolitan area raises its taxes by 10 percent, it can expect in the long run a reduction in its business activity by from 10 to 30 percent (p. 43).

If these elasticities are applicable to the kinds of tax reductions typical of enterprise zones, then we could expect the average 19 percent reduction in state-local taxes provided by an enterprise zone to produce a 19 percent to 57 percent increase in economic activity within the zone. What does the existing research on enterprise zones tell us? Is this a realistic range of effects to expect?

Studies of the effects of enterprise zones on investment or job growth fall into three categories. First are studies of one or a few enterprise zones, where measures of total gross or net employment growth in the zone since zone designation are compared, either to growth rates in the zone area prior to designation, or to growth rates during the same period of time in the metropolitan area as a whole or in a comparable but non-zone area.

Dabney (1991) looked at the effect of enterprise zone incentives on business location decisions employing this growth-rate comparison approach. Dabney argues that enterprise zone incentives are unlikely to make up for the significant locational disadvantages presented by inner-city enterprise zones. He argues that on most location factors—costs of transporting materials, commuting costs, access to airports, infrastructure, building functionality—enterprise zones do poorly. Dabney then uses analysis of variance procedures to determine whether zone designation had an impact on rates of change in the number of business establishments. The analysis uses Dun & Bradstreet's DMI file (Dun's Market Identifiers) and covers eight enterprise zones in eight different states during three years prior and three years after zone designation. He finds no significant difference in the rate of growth in the zones versus the rest of the zone city.

Rubin and Wilder (1989) studied the Evansville, Indiana zone, established in 1983. During its first three years, there was a net increase in employment of 1,878. Using a shift-share analysis to decompose the total job growth, Rubin and Wilder estimated that 325 of these jobs would have occurred if the enterprise zone had grown just at the average rate of growth for the entire metro area, and another 123 jobs could be attributed to the fact that the zone's industrial composition in 1983 would have produced above-average growth. The remaining 1,430 jobs (76 percent of the total) were attributed to the comparative advantage of the zone.

The Evansville zone area grew more slowly than the metro area prior to designation, yet it grew at over five times the rate of the metro area in the first three years after designation. This dramatic change certainly begs for an explanation. Rubin and Wilder's conclusion that a large part of this shift can be attributed to zone designation is plausible but, as they admit, cannot be established with any certainty using their method. Areas do sometimes reverse their fortunes after an extended period of decline; economic theory would tell us that capital may return to an area when that decline finally reduces factor costs (land and labor) to the point that the area is once again competitive. The zone may simply have arrived at a fortuitous moment.

The second approach to the study of enterprise zone effects involves the study of one or a few zones, in which measures of investment or job growth (gross or net) are supplemented by questionnaires administered to zone firms to determine the extent to which zone incentives were perceived as important or decisive factors in their investment decisions. This literature has been extensively reviewed elsewhere (Wilder and Rubin 1996; Rubin and Richards 1992) and the conclusions are not surprising: Other factors are con-

sistently rated more important than zone incentives, but incentives may nonetheless make a difference at the margin, when other factors such as access and labor costs are equal. This, of course, begs the question that has been pursued by researchers for the past 20 years: Just how large is the marginal effect of a tax or incentive difference?

Finally, there are studies of a large number of zones, employing regression analysis in an attempt to explain differences in zone growth rates, cross-sectionally or over time. Erickson and Friedman (1990b) studied 357 enterprise zones in 17 states. Average employment in these zones at time of designation was 4,776, and subsequent gross job growth averaged 232 jobs (about 5 percent) per year. Erickson and Friedman conducted a regression analysis to explain variation in investment growth rates and job growth rates across a subset of these zones. The number of zone incentives was positively and significantly related to both investment growth and gross job growth in models that included a variety of policy-related variables. In more complete regression models that also included other non-policy variables thought to affect zone growth rates, the incentive variable remained positive but was not statistically significant. As noted earlier, program counting is a very unsatisfactory method of measuring the value of an incentive package to a firm. In addition, as Rubin and Wilder (1989) note, the job data are imperfect since they come from zone coordinators (who have an incentive to exaggerate) and because the figures do not net out job losses. There are significant problems, in other words, with both the explanatory and the dependent variable.

Dabney argues that enterprise zone incentives are unlikely to make up for the significant locational disadvantages presented by inner-city enterprise zones. Papke found that enterprise zone designation did reduce unemployment claims.

In an interesting variation on the econometric approach, L. Papke (1994) studied the effects of enterprise zone incentives in Indiana on unemployment claims filed in the office of the city that included the zone. This approach focuses on the presumed ultimate goal of enterprise zone incentives—the reduction in unemployment—rather than the change in employment levels, though we do not know how much of the reduction in unemployment occurred among zone residents rather than persons elsewhere in the labor market. Papke found that enterprise zone designation reduced unemployment claims filed at the area office by about 19 to 25 percent, depending on the specification of the model. These are surprisingly large effects, and appear to be permanent effects as well.

The Indiana incentives consist of a jobs tax credit, the exemption of inventories from property taxation, and the exemption of profits attributable to new zone investment from the state income tax. The jobs credit is typical of credits provided elsewhere; it is equal to 10 percent of wages, but with a ceiling of $1,500 per employee. Fisher and Peters (1996a) found that the Indiana jobs credit was worth a little less than the average jobs credit among 15 states with such credits; the size of the employment effects found by Papke cannot be attributed to an unusually generous jobs credit. The capital incentive is peculiar. Most EZ capital incentives are directed at plant and equipment, and most states exempt inventories from property taxation everywhere.[23] Thus, her study provides a good test of the effects of jobs credits on unemployment, but does not tell us much about the effects of more typical capital incentives, most of which clearly lower the price of capital goods and can be expected to have much larger substitution effects than an inventory exemption, and hence to have potentially negative effects on employment.

In a follow-up to this research, L. Papke (1993) used Census block-group data to compare the fortunes of zone residents between 1980 and 1990 with the fortunes of residents of a randomly selected set of non-zone urban Census tracts in Indiana during the same period. Here she was able to identify the rates of unemployment for the actual zone population (or at least a close approximation) rather than for an entire unemployment claims office. Unemployment rates of zone residents did fall during the 1980s more than did unemployment rates of non-zone areas, but the difference was small. This suggests that new zone employment produces labor market benefits—measured by

[23] Among the 24 most industrialized states, only two tax inventories fully. Another four tax inventories in part, or at a lower rate. The remaining 18 exempt inventories.

the reduction in area-wide unemployment rates—but that much of the benefit accrues to non-zone residents, directly or indirectly. Population in zone areas declined more than in non-zones, and per capita incomes declined in zone areas but rose in non-zone areas. Papke concludes that enterprise zones apparently have not made zone residents appreciably better off.

Enterprise Zone Incentives and Factor Substitution

Much of the discussion of enterprise zones appears to assume that all incentives can be expected to stimulate the creation of jobs. As Papke (1993) points out, this is not the case. Incentives may affect factor prices, and incentives that lower the price of capital goods have both an output effect (whereby production and employment increase because costs are lowered) and a substitution effect (whereby capital is substituted for labor). If the substitution effect is stronger, a capital incentive could reduce employment.

It is useful to think of tax expenditures for business firms in the same way that one thinks of intergovernmental grants. First are capital matching grants—incentives that lower the price of capital goods—such as sales tax exemptions for the purchase of machinery and equipment, investment tax credits, and property tax abatements. Second are labor matching grants—incentives that lower the price of labor—such as corporate income tax credits equal to a dollar amount per job or a percentage of wages, and job training programs that underwrite a portion of the initial cost of labor. Third are general matching grants—incentives that simply reduce taxes and raise profits and therefore effectively reduce all factor prices proportionately—such as exemptions of all or a portion of the profits from operations in an enterprise zone from income taxation. Fourth are lump-sum grants, such as free access roads, that do not vary with plant size or employment levels or profits.

Finally, there are closed-ended matching grants. Most investment and jobs tax credits programs have a statutory ceiling on the total allowable credit, and therefore may fall into this category whenever that ceiling is reached for a particular firm. Firms bumping against that ceiling receive in effect a lump-sum grant rather than a matching grant, because further increases in investment or employment produce no increase in the credit; there is no factor price effect at the margin. Typically, the credits are one-time credits taken in the year the new investment is placed in service; they are not refundable (though they may be carried forward) and the ceiling is therefore the firm's tax liability before credits (or, in some instances, 50 percent of its tax liability). This is not a trivial category. Using data from Fisher and Peters (1996a) on 13 states with jobs credits in enterprise zones, and considering simulations of 16 different firms in a representative city with an enterprise zone in each state, in six of the 13 states most or all of the firms were able to use 100 percent of the jobs credits. But in the other seven states, most firms were able to use only a fraction of the potential jobs credits for which they were eligible. The fraction, averaged across the firms, ranged from 35 percent in Texas to 67 percent in North Carolina. For the 34 cities in the study, the overall average percentage of jobs credits used was 67 percent, and in 17 of the 34 it was typically well below 100 percent. Clearly, it cannot be assumed that jobs tax credits have the kind of employment-inducing effects one would anticipate from a wage-reducing incentive, if wages are not reduced at the margin in about half the cases.

It is useful to think of tax expenditures for business firms in the same way that one thinks of intergovernmental grants: incentives that lower the price of capital or labor, matching grants that reduce taxes or raise profits, lump-sum grants, and closed-end grants.

The Fisher and Peters (1996a) study sheds some light on the relative importance of different kinds of incentives. Considering all 24 states studied, jobs tax credits represented about 24 percent of the total tax incentive package on average, while the credits that unambiguously lower the price of capital (sales tax exemptions and property tax abatements) accounted for about 44 percent (see Table 3). (The value of incentives to the firm is measured by the present value over 20 years of the increase in cash flow attributable to the incentive.) The remaining 32 percent is accounted for by other corporate income tax credits: investment tax credits and enterprise zone income exemptions. While an investment tax credit appears to

Table 3
The Importance of Jobs Credits

Firm:	Furniture	Soaps	Plastics	Autos	Instruments	Average:
New Plant Assets (millions):	$40	$20	$5	$600	$180	16 Firms
For the 13 Study States with Enterprise Zone Jobs Tax Credits						
Value of EZ Jobs Credits Per New Plant Job						
Mean	$ 421	$ 847	$ 438	$ 380	$ 606	$ 560
Highest	$2,068	$2,918	$1,342	$2,152	$2,626	$2,217
EZ Jobs Credits as a Percent of Total EZ Incentive Package						
Mean	59	61	50	47	58	56
Highest	100	100	100	100	100	100
For All 24 States in the Study						
Jobs Credits as a Percent of Total Tax Incentive Package						
Mean	25	25	23	22	25	24
Sales Tax Exemptions and Property Tax Abatements as a Percent of Total Package						
Mean	41	38	47	53	44	44

Note: Of the 24 states in the authors' study, 20 had active enterprise zone programs that were modeled, with 13 of those 20 offering jobs tax credits. For all firms, the lowest EZ jobs credit among the 13 states is zero because there is always at least one state (not always the same state) whose eligibility criteria exclude that firm. Among all 24 states, the lowest job credit percentage is zero and the highest is 100 percent for all firms, and the same can be said of the sales tax exemptions and property tax abatements. In other words, in each instance at least one state does not offer that kind of tax incentive.

Source: Fisher and Peters (1996a).

lower the price of capital, the same phenomenon exists here as with wage credits: The firm may be unable to use all of the investment credits, in which case they have no price effect at the margin. Thus wage-reducing credits account for, *at most*, 24 percent on average, since some of the jobs credits will have no price effects, at least for some firms. Capital-price-reducing credits, on the other hand, account for *at least* 44 percent, since some of the remaining 32 percent of the credits will also have the effect of reducing the price of capital goods.

Considering only the 20 states with active enterprise zone programs, however, there is more emphasis on labor incentives. Thirteen of those 20 states provided special enterprise zone jobs credit programs; these jobs credits accounted for about 56 percent of the total enterprise zone incentive package, on average across the 16 types of firms and 13 states. These credits provided on average $560 per job, with the highest credits around $3,000 per job. In five of the 13 states, the jobs credits accounted for 100 percent of the enterprise zone package. In seven enterprise zone states, on the other hand, the EZ packages contained only incentives that reduced the price of capital or were factor neutral.

The above figures show wide variation across states in the relative importance of labor versus capital incentives, both inside and outside of zones—from 0 to 100 percent of the tax incentive package consists of labor incentives. However, the effect of incentives on factor prices depends not on the total dollar amount, but on the percentage reduction in price. Let us consider the typical kinds of incentives offered. The exemption of capital equipment from a 6 percent sales tax reduces its acquisition cost by 4 to 4.5 percent.[24] The exemption of machinery and equipment from a 2.4 percent property tax on its depreciated value over the life of the asset effectively reduces its price by 3 to 5.5 percent.[25] A 1 percent state investment tax credit reduces the price by about 0.65 percent (after federal tax effects) if the credit can be fully utilized the first year. These are all typical capital incentives provided by states, inside and outside enterprise zones.

The average jobs credit per job, on the other hand, represented only 0.2 to 0.5 percent of the present value

[24] The 4.0 to 4.5 percent reduction is equal to the 6 percent median state plus local sales tax rate in the 24 states in the authors' study, less the increase in federal and state income taxes as a result of the lower depreciation deductions, since sales taxes are capitalized into the depreciable basis of an asset. The range is for equipment lasting five to 10 years and discount rates of 10 to 15 percent, assuming straight-line depreciation and a combined marginal federal-state-local income tax rate of 39 percent.

[25] The median property tax rate in the authors' study was 2.4 percent. The 3.0 to 5.5 percent range is after the income tax effects of property tax deductions, and the range is calculated given the assumptions as in the footnote above.

of wages over 10 years.[26] The maximum jobs credit represented from 1 to 1.6 percent of the present value of wages over this period. Typical capital incentives clearly have much larger effects on the price of capital goods than average labor incentives have on wages. This heightens the importance of knowing the relative size of the output and substitution effects for incentives that lower the price of capital, since those incentives appear to dominate wage-reducing incentives in most, but not all, places. (In only six of our 24 states, considering statewide and enterprise zone credits together, did jobs credits average 50 percent or more of the total tax incentive package, and in only three states was it 100 percent.)

Enterprise Zones and Plant Relocation

Early critics of enterprise zone proposals argued that they would simply pirate jobs from elsewhere in the metropolitan area, or elsewhere in the country, rather than create new employment. Indeed, studies of firm location have frequently concluded that taxes (or incentives) are much more likely to be decisive in picking a site *within* a metro area, since the most important location considerations—labor costs and access to markets—will be roughly the same. On this ground, one would expect that enterprise zones would indeed draw jobs from elsewhere in the metro area. That the elasticities cited by Bartik (1991) are much higher for intra-metropolitan location decisions than for intermetropolitan or interstate decisions corroborates this supposition.

In response to this criticism, enterprise zone studies have frequently focused on the relocation issue by determining the proportion of new jobs in the zone that resulted from plant relocations from outside the zone; from expansions by firms located outside the zone; from the expansion of firms already in the zone; and from new business formations. The studies have concluded that the majority of jobs come from the expansion of existing zone businesses and from new business formations (Wilder and Rubin 1996). Erickson and Friedman (1990a), in a study of 357 zones in 17 states, found that of 1,623 zone establishments that made new investments in the zones, 26.4 percent were new firms and 57 percent were existing zone firms that had expanded (54.8 percent) or that changed plans to contract (2.2 percent). Only 7.5 percent were new branch plants of non-zone firms, and 9.1 percent were relocations of existing establishments from outside the zone, although these establishments tended to produce larger employment gains per establishment.

Even if no net increase in employment occurs nationally as a result of enterprise zones, the redirection of jobs into zones from somewhere else is desirable if we believe that residents of the zones need the jobs more.

Part of the reason for the small proportion of jobs in relocating establishments may be that many enterprise zone programs prohibit the provision of incentives for plants relocating from within the same city or state. However, such restrictions do not affect decisions involving where to build a *new* branch plant or where to locate a new firm. More important, the focus on relocations as the problem misconceives the issue in the first place: The impact of a zone is measured, not by a before–after comparison, but by a with–without comparison. Some or all of the jobs attributed to firm expansions (whether by firms already in the zone or not) or to new business formations could well have resulted from a redirection of investment that would otherwise have taken place elsewhere.

The concern of the enterprise zone literature with the extent of job relocation is surely misplaced. If enterprise zones had been established to nurture new business development, the incentive programs would have been structured quite differently and an emphasis would have been placed on small business incubators, technology transfer programs, management assistance, and the provision of venture capital. Instead, one finds a menu of incentives, dominated by state and local tax incentives, that looks just like the traditional menu served up by states and cities attempting to attract new branch plants from somewhere else. In other words, redirection of investment is precisely

[26] The jobs credits are almost always one-time credits with carryforwards, not continuing wage reductions. Clearly a comparison with capital incentives requires discounting the effects over a period of time, rather than looking only at the first-year wage reduction, since we are considering factor price effects on long-term decisions regarding the choice among more or less labor-intensive technologies. We chose 10 years to represent the typical life of the industrial machinery that would be complementary to the labor employed. We used about a 10 percent discount rate for both the incentive and the wage present-value calculations.

what we should expect, and indeed it may be what we should desire. Even if no net increase in employment occurs nationally as a result of enterprise zones, the redirection of jobs into zones from somewhere else is desirable if we believe that residents of the zones need the jobs more than residents of the various (and largely unknown) somewhere else. This is the argument made by Bartik (1991), an argument that is most plausible in the case of enterprise zones: that incentives redirect employment to places with the highest rates of unemployment and the lowest reservation wages, and hence the highest net gains from employment at a given wage rate. As a result, incentives might provide net benefits to the nation as a whole even if they merely reshuffle the location of employment.[27]

Conclusions

The results of studies of tax and development incentives are mixed. While a majority of the most recent econometric work has found that development incentives can produce growth effects, no econometric study has measured development incentives entirely appropriately. Part of the confusion in the literature is certainly due to the way in which tax and development incentives have been measured. Ideally, the impact of a tax regime and incentive package should be measured according to its impact on a firm's return on new investment. Both tax incentives and nontax financial incentives are, in many places, quite large relative to the state and local tax burden imposed in those places. Measures of state and local tax burdens that do not account also for the effects of incentives can seriously mis-measure the fiscal variable of concern. It is also important to include local taxes and incentives; property taxes and tax abatements have a major impact on the total costs of doing business at various sites.

By the same token, it can be misleading to focus on incentive differences without placing incentives in the context of the total state-local tax system. What is an incentive in one place (say the exemption of machinery and equipment from the property tax in Iowa) is simply part of the basic tax system in another state (say Pennsylvania, whose property tax has always applied only to real estate). To compare places only in terms of the value of the incentive packages they offer ignores the underlying tax differences, which could more than offset the incentive advantage one place provides.

Our research indicates large differences across sites in returns on new investment, but incentives do not on average reduce those differences. That is, neither tax incentives, nor nontax incentives, nor enterprise zone incentives operate to offset the effects of the basic state-local tax systems. The locations that offer the highest returns without incentives are pretty much the locations with the highest returns after incentives are included. Our research also confirms that capital incentives dominate incentive packages for the most part, although this is less true for enterprise zones. It also shows that tax credits that appear to reduce the price of labor or capital often do not have this effect at the margin because firms are unable to use all of the credits.

Neither tax incentives, nor nontax incentives, nor enterprise zone incentives operate to offset the effects of basic state-local tax systems. The locations that offer the highest returns without incentives are pretty much the locations with the highest returns after incentives are included.

Research on incentives seems to corroborate the proposition that tax and spending incentive differences are more significant the smaller the area adopting the incentive; this implies that enterprise zones, generally the smallest geographic units offering distinctive packages of fiscal inducements, are most likely to produce significant effects, other things equal. However, only a limited amount of the research specific to enterprise zones has employed a methodology that would allow identification of the unique contribution of the enterprise zone to the investment or employment growth observed in the zone. Most of

[27] The study by Fisher and Peters (1996a) sheds considerable doubt on this hypothesis. Even though explicit incentives of all kinds, on the average, do appear to be more generous in states and cities with higher rates of unemployment, the tax systems pull investment in the opposite direction. The result is that the overall spatial pattern of returns on new investment, after taxes and incentives, ends up bearing little or no relation to measures of state or local economic distress.

the research is merely suggestive, and rather contradictory. Part of the problem lies in the measurement problems discussed above and in the use of inappropriate methodologies. Part of the problem also lies in the very diverse nature of enterprise zones, in terms of the severity of the inherent economic disadvantages confronting the zone at its inception, the range and value of the incentives offered, the underlying tax systems and quality of public services, and the economic performance after zone designation. It is difficult to generalize other than to say that, given the similarity of enterprise zone incentives to the kinds of incentives and tax differences that have been the subject of most research, it is likely that the incentives offered will, in some zones, produce measurable gains in investment or employment.

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Discussion

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Early in their paper, Peter Fisher and Alan Peters quote the widely cited explanation offered by Newman and Sullivan (1988) as to why more recent empirical studies of the effects of state and local tax differentials on the location of economic activity support the entirely logical proposition that those effects must be negative and significant, while earlier studies did not (with only a few exceptions). The explanation is highly gratifying to economists: Recent studies are just better than the earlier ones, using better data and more sophisticated econometric techniques. The explanation is valid, by and large.

That explanation also answers the question of why more recent articles reviewing the state of knowledge about the effects of tax and nontax economic development incentives are so much more satisfying to read than earlier ones: The recent ones are just better. This paper is an outstanding case in point. It is extremely thorough in its coverage of the literature. Its economics are impeccable, not something that can be said about some of the literature reviewed, which at times seems to seek explanations in what reduces to irrational choice by business location decision-makers. This is the case with some of the "business climate" literature. And this paper correctly identifies the major defects in most (but not all) of the studies of the effects of nontax incentives and highly targeted tax incentives, although the authors are too courteous to excoriate appropriately those who have generated such errors.

"Omitted Variables"

Later on, I expand on the above paragraph, but first, two points which the paper slides over deserve attention. The first is the overwhelming, in many cases exclusive, focus on manufacturing industries in the studies noted and in the authors' own work. This is of course understandable; varied though the outputs are, manufacturing industries do not differ greatly in the basic nature of their inputs and their locational requirements. The supply of data that are highly disaggregated—sectorally and spatially—is enormously larger than for other nonagricultural sectors of the economy, as is the supply of analytical studies of the characteristics of this sector. Studies of the location of manufacturing go back decades, to an era when manufacturing was a far larger component of the economy than it is now. And, even today, many of the most dramatic location decisions involve large manufacturing establishments. For many places, economic development policy rightly focuses on manufacturing, because such places are highly unlikely to attract any truly large nonmanufacturing establishments (except as companions to new manufacturing ones).

But a virtually exclusive focus on manufacturing leaves many places untouched by economic development policy and analyses of such policies. That includes a substantial number of cities in the northeastern quadrant of the country and surely many enterprise zones, including those that do have a relatively large inventory of vacant land. The point is not that such enterprise zones are plausible competitors for the location of high-end service establishments, but rather that they can be more plausible competitors for establishments in other sectors ("big-box" retailing, for example) than for the kinds of manufacturing firms that have deserted those areas in relatively recent times. The story on tax and nontax incentives is incomplete, as long as it is confined to manufacturing as the target for policy.

A virtually exclusive focus on manufacturing leaves many places untouched by economic development policy and analyses of such policies, and the story on tax and nontax incentives is incomplete.

The second point that warrants some attention is the one-sidedness of the economic development bargain and how that bears on the effectiveness of tax and nontax incentives. An economic development incentive is effective only if it reduces the costs of whatever it is that the firm is promising to do. The cost reduction takes various forms: reducing out-of-pocket investment or training costs, reducing potential or existing tax liabilities, and reducing or sharing risks. All these increase the costs to the bribing governmental unit.

What happens if the bribee does not comply with the terms of the bargain?

In some cases, as when a firm receives a property tax abatement to persuade it not to abandon its current location, with the abatement automatically discontinued if the firm does pick up and leave, the cost to the governmental unit ends when the promised behavior stops. But even so, the departure of the firm may occur after the firm has received a much larger percentage of its benefits over the entire term of the deal than is the case for the governmental unit.[1] When this occurs, the general view is that the firm has defaulted on its bargain.

The one-sidedness of the economic development bargain warrants some attention. What happens if the firm does not comply with the terms of the bargain?

For most other types of incentives, the firm either receives all the promised benefits in Year 1—for example, a credit against income tax in the year an investment is made—or the stream of benefits cannot be turned off—for example, a permanent exemption from property tax. Then, the main sanction may be that the resale value of a newly built structure may be such that the firm suffers a capital loss. Now, many bargains do provide for some type of "clawback" of benefits. The problem is that the firms tend to be judgment-proof, for obvious reasons, if they are failing enterprises. And if a firm is large and rich, it often can threaten to do even worse things if the government tries to press matters. Thus, in a spectacular case in New York City, NBC reneged on its deal to maintain employment above a specified level in return for a huge property tax abatement, and threatened further reductions if the city government pressed hard. And the city government took that threat seriously.

[1] This would be the case for a property tax abatement that declines to zero over a period of years, while the promised number of jobs remains constant over that period. For example, if both parties have a discount rate of 8 percent and the tax abatement declines arithmetically over a 10-year period, but the firm departs after 3 years, the firm would have received 53 percent of its potential benefits, while the government would have received 38 percent. If the firm's discount rate is higher, as would be expected, the situation can be much worse.

These unequal bargaining positions should encourage firms to take flyers and make deals, because the deal is even better than it seems on its face, if the costs of reneging are small or nonexistent. But another implication is that analysts should discount the gain to the state or local government to take into account the probability of default or incorporate some sort of default factor (perhaps only a lagging of the employment or investment effects to reflect early-year defaults). While some of the literature speaks of "clawback provisions," they do not appear to be modeled in any studies.

The Policy Variable

As the Fisher and Peters paper notes, there is hardly a consensus in either the tax differentials and location literature or the tax and nontax incentives literature about the appropriate dependent variable; a number of different ones have been used, often in the same study. All too often, the authors of the studies do not bother to justify their specific choice of outcome variable. Not surprisingly, some strange ones have been selected. But without exception, the outcome variable is something on which the data are good.

This is not the case with the right-hand-side variables that describe the policy choices or instruments in the tax and nontax incentives literature.[2] Fisher and Peters note that a number of studies have relied on one or the other of two ridiculous (my adjective, not theirs) policy variables, whose relation to any measure of economic growth is probably inversely proportional to the square of the distance between the Earth and Jupiter (to plagiarize a recent paper by Robert Solow). These two variables are, first, the number of incentive programs in existence and, second, state government spending for economic development.

It takes only a casual acquaintance with public choice theory (even a casual reading of newspapers) to see why even a good count of the number of programs in existence tells us nothing at all about outcomes. (Fisher and Peters show that we do *not* have good counts of the number of programs.) The immediate rents to be gained from creating yet another new program to foster some clearly desirable goal like

[2] In the tax differentials and location literature, there is considerable disagreement about what those variables should be, but virtually all of the studies choose policy variables that have some degree of plausibility and for which the data are good.

economic development accrue to lobbyists, legislative sponsors, and the newly hired program staff. The budgetary costs of providing these rents are very low, relative to total spending by the unit of government. So programs are created; for the rent-seekers, a large part of their goals has been accomplished. In the late 1970s, I directed a study of housing policies for the burned-out parts of a number of large eastern and midwestern cities for HUD. One not very surprising discovery was the very large number of discrete

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federal subsidy programs alleged to be operating—as many as 40 in a given city—but with very low output. If memory serves, the median number of annual housing starts under each program in Detroit was roughly 20. Differences among cities in the number of programs in operation surely could tell us almost nothing about variations in output.[3]

State government spending for economic development purposes may not sound that illogical at first blush, but it is, for the simple reason that in quite a few states, local governments are big spenders for these purposes. No one would expect state government expenditures for the state police to be related to differences among the states in any crime variable—we know that the action is at the local government level. At least in some states, this is true of economic development as well.

Here too, Fisher and Peters show that the data themselves are poor, for a number of reasons, including the omission of the costs to the state of anything that is not a direct expenditure by the designated state economic development agency; that is, most of the costs of credit programs and all of the costs of infrastructure and training incentives provided by other state agencies. In light of these criticisms, I find it surprising that the authors conclude that, "Overall, the various work using economic development agency spending as an explanatory variable does suggest that spending on development incentives causes employment growth."

One way of truly ensuring that the right variables will be addressed is to turn to a wholly different analytical approach, what the paper calls "the hypothetical firm technique," that is, simulation models that tell us the effect of variations in tax and other policy instruments on the net returns (or other measures of outcome) of hypothetical but realistic firms in specific industries in specific locations. This technique was pioneered by James and Leslie Papke in their AFTAX model, which provides far and away the most convincing stories about tax policy variation and location. Fisher and Peters report their own work on a simulation model that incorporates nontax incentives, the Tax and Incentive Model, TAIM.

The TAIM model is a very good one, well presented here. It shows that incentives can have a sizable impact on project returns, with considerable variation among cities and industries in their effects. There is one problem with Table 1. The authors suggest that the quantitative differences among the cities tend to be of importance, for any given industries, only at the extremes of the range. However, some of the cities must be places where some of the industries are so unlikely to locate that the "standing incentive offer" for firms in that industry would be quite small or irrelevant. This would be the case for most of the "large" versions of the eight industries in the more densely settled urban areas in the Northeast with few large tracts of vacant industrial land, such as Boston and New York, among others. So some of the extreme differences shown by the model may be irrelevant for policy.

Factor Substitution

Fisher and Peters utilize the TAIM model successfully to deal with two other issues. One use is to describe the characteristics of the incentives offered in enterprise zones and appraise the likely effects of these incentives (Table 2). Another use is to address the

[3] Our sample was very small, but it showed a negative relation between number of programs and total output, which I attributed to the confusion on the part of the intended beneficiaries that was created by the existence of all those programs.

factor substitution question in the context of job credits in enterprise zones (Table 3).

A major conceptual failing in the discussion of economic development incentives in the literature—particularly among pop economists and economic journalists—is the casual assumption that any incentive that produces a response will help achieve the final goal, usually the creation of jobs, especially in enterprise zones. That denial of the elementary economic phenomenon of factor substitution is pointed out in the paper, with a nice analysis using parallels with the way economists think about intergovernmental grants. Over the years, economic development incentives generally have worked mostly by reducing the cost of capital, rather than on the labor side. But it is not likely that all the reduction in the cost of capital will lead to employment increases. Instead, we should expect some substitution of capital for labor, possibly a great deal of it. Given that the new capital induced by cheap credit or state corporate tax credits is likely to be largely exempt from local property taxes, it may be no great success from a local standpoint if the labor intensity of a new plant is quite low (often the case with the more spectacular industrial development bond issues of past decades). So it is important to distinguish between capital and labor subsidies, especially in enterprise zones, where job creation is usually by far the overriding objective. The TAIM model does this very well, and adds considerably to our understanding of the likely results of the enterprise zone gambit.

Some Studies Should *Be Trashed*

The authors are much too kind in their review. For one thing, they report entirely preposterous coefficients without comment. For example, an elasticity of employment of +0.20 is reported with respect to state economic development expenditure. Ignore the comments above about the defects of the independent variable, but consider what this means for a largish state with a generous economic development budget of $50 million and total employment of 3 million people. The coefficient says that a $5 million increase in the state agency's budget will increase employment in the state by 60,000, at a cost per job of $83. And a doubling of that budget would increase employment by 600,000. Who needs oil wells, when a state can be another Kuwait just by increasing the budget of a tiny agency?

A coefficient that is in the same "field of dreams" class is, in my view, Bartik's well-known conclusion—reported in the paper without comment—about the effects of local tax differentials on location within metropolitan areas: as high as −3.0. Surely, that provides a solution for the Chelseas and Camdens; they too can become like Kuwait.

The paper does not say complimentary things about the survey method of appraising the effectiveness of incentives programs, but the treatment and tone are nonetheless inappropriate. We have had 40 years of such studies. After all this time, we have no more reason to place any credence in what the respondents tell the interviewer or write on the questionnaire forms than we had 40 years ago. The interviewees often are not the location decision-makers. It is inevitable that they provide off-the-cuff answers (is anyone who is gainfully employed willing to do research to prepare for such a survey interview?) that may be wildly unfactual. They typically have every reason to dissemble, by answering questions in ways that may help induce policymakers to do nice things for them in the future, to continue generous but ineffective programs, and to believe that beneficiaries of incentive programs are grateful for past favors. One indication of the unreliability of the survey approach is the frequency with which respondents report that some factor is important in their industry, but not for their own firm. I keep hoping that review articles will confine their reference to the survey approach to a dismissive endnote.

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The paper also is too kind to the literature specifically dealing with enterprise zones. In my reading of it, the papers by Leslie Papke are the only ones that merit attention. In this regard, I urge readers not to overlook a point in a paragraph near the end of Fisher and Peters' paper on the question of plant relocation: The authors correctly note that the essence of the enterprise zone idea is to capture enterprises for the

zone that might have located elsewhere, not to create new enterprises from scratch, whatever the rhetoric may be, and that this is not necessarily a bad aim (though it may be unattainable for many zones) if the true objective is to lower unemployment among the residents of especially distressed small areas.

Incentives and the Basic Policy Structure

The paper notes, correctly, that some of the literature has focused on differences in explicit incentives, without recognizing that the states do not have identical provisions of general tax, spending, and regulatory policies: An incentive in one place may be part of a system (tax or nontax) that has been in place for decades in another place, like the treatment of business personal property in the different states. This raises a more fundamental question, the extent to which state and local economic development policy should be based on exceptions to the general rules, rather than broader changes in those rules.

A few years ago, Assar Lindbeck, longtime chairman of the Nobel Prize in Economic Science selection committee, wrote these words concerning national economic development policies in Sweden:

> It is not by planting trees or subsidizing tree planting in a desert created by politicians that the government can promote . . . industry, but by refraining from measures that create a desert environment (quoted in *The Economist*, March 3, 1990).

Those words seem quite relevant to the discourse on state and local incentives for economic development in the United States, too.

Discussion

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The primary issue Peter Fisher and Alan Peters address in their paper is the accurate measurement of the value of state and local economic development incentives. While the literature already includes hypothetical firm simulations of after-tax rates of return that account for federal, state, local taxes, and some tax credits, Fisher and Peters have added to this calculation the value of additional tax and nontax incentives. These incentives include, for example, credits for job creation, loan guarantees, and grants. This is no small undertaking.

Their work has important implications for the consensus formed in recent years about the partial effect of state tax burdens on industrial activity. Over the past several years, improvements in both the measurement of state and local tax burdens and the econometric techniques applied have led many researchers to conclude that, for certain industries, an increase in state and local taxes will lead to a small decrease in business investment. Most studies have produced statistically significant estimated elasticities that range between -0.1 and -0.6. That is, a 1 percent increase in a state's effective tax rate is associated with a 0.1 to 0.6 percent drop in business activity.

Confusion sometimes surrounds the interpretation of this finding. The academic literature is interested in, among other things, measuring effects precisely—that is, the issue of *statistical* significance. An important distinction must be made between that concept and *economic* significance—the magnitude of the coefficient. A precisely measured coefficient may be too small to be of much economic significance. The

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range of elasticities above suggests that business investment appears to be relatively inelastic with respect to state and local tax burdens. However, if Fisher and Peters were to show that development incentives systematically work to offset existing state and local

tax burdens, then this would cast doubt upon these findings.

The paper begins with a review of the literature on the influence of state and local development incentives on economic activity. I agree with the authors' conclusion that the findings in this area are preliminary. The measurement of development incentives—often a count of the number of available programs—does not accurately reflect the value of the incentives to firms. While their review of econometric studies is extensive, it would be useful for the authors to distinguish between economic and statistical significance in their discussion, and to discuss a range of coefficients so the reader can get a feel for the estimated effects. For example, in the discussion of Wasylenko (1988), the authors report that "development incentives were significantly associated with lower total employment growth" and that the number of employment training programs "had a statistically significant and positive effect on total manufacturing employment growth." The *magnitude* of the coefficient is also of interest here, not just statistical significance. How large is the measured effect? What is the estimated elasticity? Similarly, in the discussion of Walker and Greenstreet (1990), the authors report that "incentives were consistently and significantly related to location decisions." This way of summarizing empirical work is of limited use to the reader.

The paper also includes a brief discussion of survey research and case studies. It is the only section of the paper that mentions the cost of development incentives—a topic I will return to—and some references to papers that include cost calculations would have been useful.

Now I would like to turn to the summary of Fisher and Peters' own research findings and their measure of economic development incentives. The literature already includes calculations of after-tax rates of return and effective tax rates that incorporate federal, state, and local taxes as well as common tax credits and abatements. But, this is the first time that all major state and city grants, loans, loan guarantees, and the like have been incorporated into a hypothetical firm simulation model.

I have several comments. First, I commend the authors for undertaking such a mammoth project. Second, while the methodology is not the subject of this paper, this calculation is still relatively new and I would have liked a brief discussion of its unique features—for example, the valuation of less tangible incentives like loan guarantees. Does the value of a guarantee vary with the risk of the industry? What assumptions about employment and job growth allow you to value job creation tax credits?

Third, the summary statistic that the TAIM model produces is the present value of the incentives over 20 years, in dollars. Table 1 in the paper presents a range of project returns by industry type and size. It is hard to get a feel for the magnitude of these differences. The authors do calculate the ratio of incentive dollars to expected wages paid over 20 years, but I would prefer a measure that does not depend on additional assumptions about the path of wages and employment. That is, I would like to avoid additional factors that differ across firms and states, in order that the calculated differences will be due solely to the development incentives.

Take-up rates for job tax credits are much lower in enterprise zones than elsewhere. Therefore, capital incentives may still dominate the value of the EZ package.

Further, these cash flows are calculated using firm-specific discount rates. The present value of a 20-year cash flow is sensitive to the chosen discount rate. How is this rate selected? How much of the variation in the incentive value is due to different discount rates?

In other work, this discount rate problem has been avoided by the use of the internal rate of return. In work by Robert Tannenwald, James Papke, and me (referenced by the authors), the return on new investment is the discount rate that equates the present value of the difference in after-tax cash flows with the initial investment. The internal rate of return is a scale-free, intuitive measure of the return on the investment or, in this case, the economic development incentives, that is easily compared across firms. Its calculation would require only a few additional steps, and it would be a valuable summary statistic.

Finally, since the valuation relies on the interaction of many factors, I would like to see development incentives added to the model in increments. Adding incentives by type will clarify the source of the differences in project returns. For example, I would like to

see project returns when only tax credits on capital investments are included. Differences will be due to firm operating characteristics and the interaction of state, local, and federal taxes, and not due to differing assumptions about the path of employment across firms. Second, assumptions about the path of employment could be made and project returns calculated that include job credits. Finally, the expert systems used to predict offerings of intangible incentives could be added and incentives like loan guarantees analyzed. The goal here is to clarify as much as possible the source of the differences.

In the final section of the paper, the authors apply their model to state enterprise zone incentives. Enterprise zone (EZ) incentives apply to operations in a particular geographic area. These areas are typically small, and severely depressed economically. The economic characteristics of a central city will be magnified in its enterprise zone. Zone incentives are offered to offset some of the costlier features of the location. The job tax credits that are offered often require that a zone resident be hired. Evidence from zone programs indicates that the take-up rates for these incentives are much lower in zones than elsewhere. Therefore, differences in the values of development incentives in zones do not have the same implication as differences elsewhere, and I would not expect them to have effects similar to incentives generally. Nevertheless, I found this section of the paper interesting since it includes a breakdown of EZ incentives into those applying to labor and those applying to capital. Fisher and Peters find that job credits account for slightly over one-half of the value of EZ incentives. Of course, since these are the credits that may not be claimed, capital incentives may still dominate the value of the EZ incentive package.

Overall, I think the most significant result from Fisher and Peters' painstaking work is the conclusion that the economic development incentives serve to accentuate, not offset, existing state tax differentials. That is, low-tax states tend to offer more lucrative development incentives—the rank ordering of cities or states does not change much. We do not see a trade-off between taxes and incentives.

This finding has a number of implications. First, since incentives do not tend to offset tax differences across states and localities, prior analyses that rely on tax burdens calculated without these incentives are still likely to be valid. Econometric work using these estimates undoubtedly will change the magnitude of the estimates, but not the major findings. I look forward to future econometric analyses by the authors.

More important from a policy perspective, if the development incentives only serve to accentuate existing tax differentials, states and local governments need not feel pressured to offer them to lure businesses. In hypothetical firm simulation models such as this one, reducing the state corporate income tax rate by 1 percentage point leads to a much larger increase in project returns than even a generous investment tax credit on machinery and equipment. If a state already has a low corporate income tax rate, there is no need to offer additional incentives to attract business investment. These incentives are not free. When a state offers business tax incentives, but must also provide its usual level of services, then the costs of the incentives are borne by other state taxpayers—usually residents—in the form of higher taxes.

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In addition, with their huge data base on state and city incentives, the authors are in a position to address a critical issue that we need more information on. What is the cost of these incentives to the state and to the local government? Calculating the value of the development incentive to the firm is only one-half of the story. Presumably we are interested in incentives with a positive net benefit, so we need estimates of the costs as well as the benefits. If indeed it is true that the incentives only serve to make a competitive area more competitive, then the incentives are not having a marginal effect on firm location and perhaps not much of a marginal effect on investment.