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The Future of State and Local Economic Development Policy: What Research Is Needed?

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**“The Future of State and Local Economic Development Policy:
What Research Is Needed”**

Thank you for inviting me to give this speech. What I intend to do in these 45 minutes is discuss what research topics in state and local economic development policy are likely to be most needed by policymakers, what we currently know and don't know about these topics, and how we might best fill these research gaps.

As an introduction, I think it is likely that state and local economic development will in the future become even more important as a policy issue, for several reasons. First, based on current projections, it is likely to be many years before the U.S. on average will get back to full employment. In addition, based on experience with previous recessions, even once the overall U.S. economy is close to full employment, some of the areas hardest hit by the Great Recession are likely to have trouble getting back to full employment. The perceived lack of adequate jobs in most regions for the next few years, and in many regions for the foreseeable future, is likely to lead to intense pressure on Governors, state legislators, and local officials to “do something about jobs.” Furthermore, I think we are going to see a continuation of global trends where communications costs and transportations costs are going to drop relative to other costs, which will make businesses even more footloose, and potentially even more responsive to state and local economic development policy. Finally, the federal government is likely to have enough problems of its own—for example in dealing with health care costs and access—that it is unlikely to have the money or time to be much involved in U.S. economic development policy.

So, state and local economic development policy will expand and become more prominent. Now, on the whole, state and local economic development policy has been much more influenced by politics than by the findings of academic research. There will be some pressure to change that as state and local economic development policies become a bigger share of government budgets and become more visible. But research is unlikely to have much influence on policy unless there are a clear set of accepted research findings that are reliable enough and precise enough to be useful in assessing the net benefits of different policy alternatives.

What can research say about state and local economic development policy with our current knowledge, and what might we be able to say if we had better research knowledge?

First, and perhaps most importantly, I think there are clear research findings about what are the appropriate goals and benefits of state and local economic development policy. The goals and benefits of state and local economic development policy should shape the way we should think

about state and local economic development policy, the questions we should ask about this policy, and how we should organize such policies.

Traditional state and local economic development policies have focused on various types of business incentives to boost local business growth. Sometimes, people claim that the appropriate goal of these policies is simply to boost job growth. But job growth is not good in and of itself, but only good insofar as it brings other benefits. Why should the average taxpayer be willing to pay higher taxes in order to pay for business incentives, even if those incentives succeed in promoting local job growth? Ultimately, because this job growth somehow boosts the incomes of local residents enough to offset the extra costs of paying for the business incentives.

How does job growth boost real incomes? Research on the benefits of state and local economic development policy clearly shows that the main benefits of policies that promote job growth are higher real earnings per capita of local residents. For example, in one study I did, I estimated that that annual income equivalent of a once-and-for-all 1% increase in local employment, as of 5 years after the growth shock occurred, was an increase in real earnings per capita that was 0.28% of local area personal income, a 0.05% increase in the state and local fiscal surplus as a percent of personal income, and a property value increase whose annual income equivalent was 0.07% of local area personal income. About two-thirds of the total real income benefits take the form of increases in real earnings. Of those real earnings benefits, about half are due to higher local employment rates, and the other half are due to local residents moving up to higher paying occupations.

Therefore, the main benefit of traditional economic development policies is a labor market benefit. In my opinion, at the very least we should think about state and local economic development policies as part of local labor market policies, that work on the labor demand side of the labor market using a particular set of policy tools.

An even better approach is simply to broaden the definition of local economic development policies. In my opinion, we should define as “local economic development policies” all the policies that seek to affect the quantity or quality of labor demand or labor supply in order to increase local per capita earnings. The business incentives that are usually called economic development policies would then just become one component of the overall local economic development strategy.

The advantages of this broader definition of economic development policies are several. First, it gets us focused not just on job growth, but on whether the job growth actually boosts local per capita earnings and incomes. This leads to useful questions about the wages paid by these jobs and who gets the jobs. It leads to useful consideration of whether policies can affect wages paid by jobs and who gets the jobs. Finally, this broader definition of economic development policies

leads to the potential of better integrating local labor supply and demand policies, which potentially has great benefits for more effectively boosting local per capita earnings and incomes.

Both for research purposes, and for policy purposes, it is useful to segment local economic development policies—or local labor market policies, whatever you want to call them—into a 2-by-2 matrix, which is segmented along two dimensions: whether we’re considering labor demand or labor supply policies, and whether we’re talking about how policy affects labor demand or supply, or whether we’re talking about the transmission mechanisms for how changes in local labor demand or supply affects local earnings per capita. For example, in the area of labor demand policy, there is the perennial question about whether business tax cuts and various business incentives and services will affect job growth. But there is also the issue of how much business job growth affects local earnings per capita, and whether this might be affected by policies that would seek to better match local job creation to the skills of the local unemployed. In the area of labor supply policies, there are the issues of how preschool or other educational reform or job training programs affect the quantity or quality of local labor supply. But there is also the issue of how these skills actually attract better local jobs, which might be affected by policies that better match the skills created to local economic clusters. Both these dimensions of local labor demand and supply policies are important.

I will focus on the causation from labor demand and supply policies to effects on labor demand and supply, and the causation from effects on labor demand and supply to effects on local earnings per capita, which might be mediated by policy. But I am also aware that there may be feedback effects from earnings per capita, and its distribution and types, to future labor demand and supply. For example, the extent of intense poverty may affect human capital distribution of future generations, because it affects what happens to children during their formative years. As another example, the extent to which local workers’ earnings come from secure jobs in large corporations, versus more volatile jobs in smaller businesses, may affect future entrepreneurship, and hence labor demand. However, these feedback effects are more remote and more speculative than the more immediate demand and supply shock effects and transmission to earnings effects I am focusing on. And such feedback effects may be mediated by public policy. For example, the extent to which income distribution effects are reflected in children’s human capital accumulation is influenced by education policies, and the extent to which local business size distributions affect future entrepreneurship is influenced by local public policies affecting the entrepreneurial climate.

I’m going to at least briefly talk about each quadrant of this diagram, although not in a balanced way, from quadrant 1 (how policies affect labor demand) to quadrant 2 (how labor demand affects earnings per capita) to quadrant 3 (how policy affects labor supply) to quadrant 4 (how labor supply affects earnings per capita). And to give an advance summary of my argument, I think we know much more about quadrants 2 and 3 than we know about quadrants 1 and 4. That

is, we know the most about how various policies affect the quantity or quality of labor supply, which is in quadrant 3, and about how shocks to labor demand, whether occurring naturally or through policies, affect earnings per capita, which is in quadrant 2. We know much less about how specific policies affect labor demand, which is in quadrant 1, and not as much as we should about how shocks to the quantity or quality of local labor supply affect earnings per capita, which is in quadrant 4.

Turning to the first quadrant, what do we know about some important policies affecting labor demand? I'll just focus on a few, and those might well be expanded on: overall public spending and taxes; average state business taxes; marginal state business taxes; and business tax incentives; spatially targeted incentives; customized economic development services.

First, we do know that in the short-run, overall demand effects of changes in state and local public spending across the board exceed overall demand effects of changes in taxes across the board. That is, there is some balanced budget multiplier at the state and local level. This truth seems to have been forgotten during the Great Recession in many debates at the state level about how to deal with state budget deficits. For example, according to the Center for Budget and Policy Priorities, to balance budgets for the 2011-12 fiscal year, Governors in 39 states proposed major spending cuts, and only in 7 states were there proposals for significant tax increases, which was matched by proposals in 7 states for significant tax cuts. The balanced budget multiplier may be less than one, but it is positive, in numerous regional econometric models. In an analysis applying the REMI model to Michigan, George Erickcek and I got a balanced budget multiplier of around 0.4. Of course, we will not get this result if we consider business tax changes, which have incentive effects on labor demand beyond their demand effects.

Second, we know something about the range of effects of average state business taxes on economic development. My research literature summary suggested that the long-run elasticity of state labor demand with respect to overall state business taxes is somewhere in the range of -0.1 to -0.6 . I suggested a central value of -0.3 . Wasylenko suggested -0.2 . Goss and Phillips suggest that the value depends somewhat on how the study is done, with higher values for studies that control for state and local public services, which of course may or may not be appropriately controlled for in real-world policy analysis, depending upon how lower business taxes are financed. Jim Hines has a very good study showing that the value is closer to -0.6 for the highest mobility business cases such as branch plants of foreign corporations. The value is closer to -0.1 , or even below, if one thinks that the negative effects of wages on local job growth tell us how similar increases in costs due to business taxes affect job growth.

Although this is SOME knowledge, the range from -0.1 to -0.6 is so wide that it encompasses too wide a range of costs per job created to be definitive in benefit-cost analyses. For example, I have calculated in previous work that the lower elasticity of -0.1 corresponds to foregone annual

revenue per job created of \$43,000, whereas the larger elasticity of -0.6 corresponds to foregone revenue per job created of \$7,000. The latter is likely to have a positive benefit-cost ratio at most reasonable values of the social benefits of a job, whereas the former is unlikely to pass a benefit-cost test.

Third, unfortunately, I don't think we have as much knowledge as we should on discretionary business tax incentives. In many studies, the variable of interest, which is frequently the amount of incentives received by some firm or some area, is endogenous by definition, and this endogeneity is not able to be overcome in the study by adequate instruments or quasi-experimental design. In my opinion, a better approach to discretionary business tax incentives is simply to assume that it is marginal business taxes on marginal business decisions about locating or expanding in a state or local area that are driving things. Most discretionary incentives aren't very discretionary anyway, and are simply handed out to most firms above a certain size that make significant new investment decisions. Under this assumption, we do know that marginal business incentives, particularly when targeted at export-based companies, are far more effective than simply decreasing business taxes across the board. For example, in a study that I did with my colleague George Erickcek of Michigan's MEGA program, we concluded that this program of export-base targeted marginal business tax incentives was about six times as effective per dollar of foregone revenue as across the board business tax cuts.

For better research in this area, in my opinion, we will actually need to measure what the marginal business tax rates, including tax incentives, are for different types of business investment decisions for different firms. These marginal tax rates aren't much correlated with average business tax rates, as shown in this chart, which compares marginal business tax rates with average tax rates for different states. In addition, there is a wide degree of quasi-experimental variation in marginal tax rates across different states due to various provisions of state tax law and business incentives, which cause wide variation in effective marginal business tax rates across industries and over time in a manner that is unlikely to be intended by state policymakers, or at least is more likely to be driven by political factors than economic factors.

For example, this chart from a paper co-authored by me, Rick Funderberg, Peter Fisher, and Alan Peters shows a wide variation across states, industries, and time in the marginal state and local business tax rates on profits, including incentives. Unfortunately, we do not have data on these variations in marginal business tax rates more recently than over a decade ago. We really need to update these data and start doing some real models of state and local business tax policy, not fake models where we pretend that the top corporate income tax rate is a valid measure of state and local business tax policy.

Fourth, I think we do know something about spatially targeted business tax incentives. We have some studies with very good comparison groups. Specifically we have studies that compare

enterprise zone areas with areas that applied for enterprise zones and were not selected, or were selected but received lower subsidy rates. Studies of state zones, by Boarnet and Bogart of New Jersey; by Neumark and Kolko of California; and by Peters and Fisher of 13 states, all find no statistically significant or substantively significant estimated effects of state enterprise zones. On the other hand, an excellent study by Busso, Gregory, and Kline of the 1990s version of the federal Empowerment Zone program finds strong evidence that this program boosted zone employment levels by 15%. Why the difference? I think a reasonable hypothesis is that the federal Empowerment Zone program's effects were boosted by the large public service grants that accompanied the program's tax breaks. For highly distressed areas, business tax breaks are simply not enough by themselves to make these areas attractive.

Fifth, I think we have some scant good evidence for the effects of customized business services. A few studies suggest that providing businesses with customized extension services or customized job training may be as much as 10 times more effective than similar dollar-sized business tax incentives, although I would feel more comfortable pushing this point if there were more high-quality studies. This high cost-effectiveness may reflect market failures that cause both financing and information difficulties for the small and medium-sized businesses that these services generally target.

For example, there is good evidence of positive effects for manufacturing extension services from Jarmin's quasi-experimental study, in which we observe the effects of variation in business use of MEP services for businesses that are different distances to the nearest MEP office. This evidence is consistent with surveys that ask MEP clients to assess the effects of the services they receive.

There is also good evidence of positive effects of customized job training from a study by Harry Holzer and others that compares the performance of businesses that applied in a timely manner for customized training grants, to businesses that applied in a fiscal year after program funds were exhausted. This evidence is consistent with a regression study of Kentucky's customized job training program by Hoyt, Jepsen, and Troske, and survey evidence on Massachusetts's customized training program from my colleague Kevin Hollenbeck.

Although this evidence is welcome, we need many more studies that will both see how robust these high effectiveness findings are, and to ascertain what factors affect program effectiveness. Additional surveys that ask firms to assess effectiveness might be helpful, as incentives for respondents to claim services are effective when they may not be that strong: why try to promote a program that is not helping you? However, we need even more studies of customized business services that have some experimental or quasi-experimental variation in service provision. It seems unlikely that we will be as willing to experiment with services to firms as much as we are willing to experiment with services to the economically disadvantaged. However, given limited

resources, it makes sense for states to limit funds for customized business services based on some cutoffs by geographic need or firm need, such as by firm size or county job growth or unemployment rate. Such cutoffs give an opportunity to evaluate the effectiveness of these services using some type of regression discontinuity analysis, as the abrupt change in availability of services with geography or firm characteristics can be distinguished from the more gradual natural change in firm's performance with these characteristics.

So, although we have some knowledge of how different labor demand policies affect job growth, we need much more precise and robust estimates for marginal business taxes effects and customized training effects to be more potentially useful to policymakers. Why should policymakers listen to us if all we can tell them is that policy costs per job created are somewhere vaguely in the range from "far too expensive" to "very cheap"?

Turning to quadrant 2, how labor demand shocks affect earnings per capita, I think we have some good knowledge of average effects of labor demand shocks, but less knowledge of how these effects vary in different local economies or how these effects might be altered by public policy.

In terms of average effects, there are good studies by a variety of researchers, not just my own work, but work by Rickman and Partridge and others, that suggest that labor demand shocks have extremely persistent effects on employment rates and earnings rates. Roughly speaking, for a 1% employment shock, about 80% of the employment increase ends up leading to in-migration, but 20% boosts employment to population ratios. In addition to increasing employment rates by 0.2%, a 1% employment shock in the long-run boosts the wages of occupations attained by about 0.2%, so the total earnings effects is about 0.4%. These effects may be due to the long-run benefits of better labor market experiences.

The effects of job growth are higher if we attract higher-wage industries. If the local job mix shifts towards industries that nationally pay 1% more than one would predict based on the characteristics of these industries' workers, estimates suggest that these higher wage standards to have enough spillover effects in local labor markets to boost local wages by 2%.

However, we have insufficient knowledge of how these average effects vary in different types of local labor markets. There is some evidence from a variety of researchers, including Partridge and Rickman, Eberts and Stone, Greenwood and Hunt, and Muth, that labor demand shocks have greater migration effects, and less effect on employment rates, in higher mobility areas such as the U.S. Sunbelt. But we don't have much evidence on how the benefits of creating jobs vary across labor markets with different local unemployment rates. The conventional wisdom is that the benefits of creating jobs are greater in high unemployment local labor markets, as people there have lower opportunity cost of time and are more desperate for jobs. But these benefits

should be empirically reflected in some greater effects of labor demand shocks in high-unemployment rates on local employment rates. However, this hypothesis does not have strong empirical evidence.

We also don't have much evidence on the theoretically plausible hypothesis that labor demand shocks will have greater effects if the mix of jobs created are well-matched to the available local labor force. It's a great hypothesis, but there's not much evidence.

Finally, there is little evidence on how policies to increase the matching of labor demand to local residents affects the impact of local demand shocks. For example, some local areas have "first source" programs, under which firms receiving economic development incentives have to use the local workforce system as "first source" for considering vacancies. There is little evidence on whether such policies make much difference in who is hired.

There is also not much evidence on how the overall effectiveness of the local workforce system affects job matching and hence the degree to which newly created jobs will hire locally. We know from empirical data on local job chains that this should make a difference in the outcomes from job creation. In the end, each newly created job must end up somehow resulting in someone being newly employed within the metro area, or a new employee from outside the metro area. Jobs that immediately go to the local employed result in a chain of job vacancies that is only brought to a halt when someone not working in the local area fills a vacancy. Job chain work by Persky, Felsenstein, and Carlson finds that on average, 13% of job vacancies in local economies go to the local unemployed, but that after we consider all the effects of job chains, 34% of newly created jobs result in some local unemployed person being hired. If we could improve the local workforce system sufficiently that 26% of all job vacancies were filled by the unemployed, then 54% of all newly created jobs would lead to a local unemployed person being hired. New job creation would then be far more effective in raising local employment rates and local earnings.

Turning to the third quadrant, how public policies affect the quantity or quality of labor supply, I can be brief, because there are a wide variety of high-quality studies by education researchers and labor economists. I'm going to focus on policies that raise employment rates or skills of current residents, not attracting in-migrants, because, as I will explain, it is much less clear that attracting in-migrants will have any effect on raising earnings per capita of original local residents, so that policy is less interesting to me.

There is a growing research in education and labor economics that shows long-term impact on adult employment rates and occupational attainment of a wide variety of interventions, including: high-quality preschool; mandatory summer school; high-school career academies; improving teacher quality; demand-oriented job training. In the interests of time, I'm not going to go into all the evidence, but it comes from a variety of random assignment experiments,

frequently with long-term follow-up on adult outcomes, as well as non-experimental studies that arguably have very good comparison groups. The high quality of this research evidence is in part because we are far more willing to experiment in providing services to the poor than we are to businesses.

But for the fourth quadrant, we have far less empirical evidence on how supply shocks to quality of labor supply of local residents end up getting translated into higher local earnings per capita.

There are two issues here. First, there is the issue of how many will stay in the local economy. Second, there is the issue of what is the impact of higher labor force quality of those who stay on the quantity of labor demanded and on its price. In other words, if there is a shock to labor supply quality, will it be matched by an increase in labor demand, or alternatively, will any of the additional labor supply that does become employed simply displace others in the local economy from jobs?

On the first issue, of who will stay, the answer is a higher percentage than some people think. For my recent book on the local economic development effects of preschool programs, I did some perusal of statistics on how many Americans stay in the same state or metro area that they were born in or in which they spent their early childhood. Over 60% of all Americans will spend most of their working career in their early childhood state. Over 50% will stay in the metro area of their early childhood. These percentages do not much decline in smaller metro areas or more economically distressed areas—size or economic distress affects in-migration rates more than it affects out-migration rates.

But there are few direct empirical estimates of displacement effects of labor demand shocks. Most models of local labor market impact of labor supply quality shocks implicitly simply assume zero displacement, which only makes sense if the regional labor supply curve is vertical or the regional labor demand curve is horizontal. Some researchers, for example James Galbraith, take the extreme position that labor demand is completely insensitive to labor supply shocks and thus we have 100% displacement. In my preschool book, I quote Galbraith arguing that a universal preschool version of the famous Perry Preschool program would have no effect on overall economic outcomes. “How would ... economic outcomes [in Ypsilanti] have been changed by universal preschool? Not at all ... It is a false inference that because something works for an individual, it will also change outcomes for the entire population.” In between these extreme cases, I’ve done simulations with structural models that assume more moderate local demand and supply elasticities. I get estimates of about one-third displacement from labor quality shocks. But it would be nice if we had some empirical estimates of displacement effects that were less dependent upon modeling assumptions about labor market structure. This requires finding some good instruments that proxy for local labor force quality shocks.

A closely related issue is the extent to which there are skill spillovers at the local level. There is some evidence from a variety of sources for there being strong positive spillovers of local college graduation. For example, Moretti has found evidence that when 1% extra of the local population gets a college education, the average earnings in the local economy goes up over twice as much as the direct earnings effects on those who get the higher earnings. What's going on here? There may be positive spillovers within firms because stronger skills for my fellow workers make it easier for my employer to introduce new technology, thereby increasing my wages. There may be positive spillovers across firms due to agglomeration economies, such as clusters in which firms steal ideas from other firm's workers or benefit from a network of skilled suppliers.

However, there are big issues about the magnitude and form of these local knowledge spillovers. For example, do skills spillovers affect the level of productivity, or productivity growth? If the effects of higher education spill over into productivity growth, this enormously increases the long-run effects of increasing local labor force quality. For example, in projections that Bill Dickens and Belle Sawhill of the Brookings Institution did of the long-run economic effects of universal preschool, models in which education permanently increases growth rates lead to long-run economic effects of universal preschool that are 3 times as great as for models that assume that education only increases the productivity level.

The spillover idea is also very important to the creative class idea, popularized by Richard Florida, that we should attract highly educated and creative groups to a local economy as a way to promote local economic development. Even if we knew how to attract the creative class to local economies, which we don't, the creative class strategy only pays off for original residents in higher earnings per capita if there are positive skill spillovers that are strong enough. Are there? We simply don't know.

The bottom line is that we don't know as much as we should about how different labor demand and labor supply shocks affect local earnings per capita, the proper goal of local economic development policies. For a previous paper on state economic development policy, I generated some summary estimates of possible ratios of effects on the present value of local earnings per capita per dollar of costs of alternative economic development policies. But all these estimated policy effects are accompanied by widely varied degrees of uncertainty about effects, as well as different types of uncertainty.

For example, with a business tax elasticity of -0.2 , I estimate that across the board cuts in business taxes increase the present value of earnings by about 50 cents per dollar of tax cut, whereas well-designed business tax incentives increase earnings by about 3 times their costs. But these studies are subject to huge uncertainty due to uncertainties about the business tax elasticity. Manufacturing extension and customized job training, based on a few limited studies, might

increase the present value of earnings by as much as 30 dollars per dollar of program costs, but such huge benefit-cost ratios should be backed up by more than just a couple decent non-experimental studies. Preschool and high school career academies have some great experimental studies of impact, but there is still some remaining uncertainty because we are not sure what displacement or skill spillover effects are plausible. Mandatory summer school and adult job training have some of the same uncertainties about displacement effects, as well as some additional uncertainty because estimated program impacts on labor supply rely on non-experimental evidence.

To make progress in more research-based state and local economic development policies, we need to make significant advances in our knowledge of program effects of labor demand policies, such as business tax incentives and customized business services. We need to move our knowledge basis up to at least close to the level of knowledge that education researchers and labor economists have achieved for estimating program effects of various labor supply programs focused on labor quality. But in the regional economic context, we cannot avoid the additional issues that arise from general equilibrium or macro effects of how local labor demand or supply interact. Even if at a national level we can assume that labor supply curves are vertical, which is doubtful, we certainly cannot justify extreme assumptions about either labor demand or supply curves at the regional level. While structural models can sometimes provide reasonable estimates of impacts on local labor market impacts, we also need to go beyond the needed assumptions of such models, to find robust ways of identifying the reduced form outcome impacts of labor demand and supply shocks.

If we develop both better program impact estimates for labor demand policies, and a better understanding of the regional earnings per capita impacts of labor demand and supply shocks, we will be able to give more precise and useful answers to the questions of state and local policymakers about local economic development policy. Politics may always limit the real-world policy influence of research, but we maximize our chances of having influence by developing empirical research that can give reasonable estimates of the benefits and costs of local economic development policies.