

**Taking Preschool Education Seriously as an Economic Development Program:
Effects on Jobs and Earnings of State Residents Compared to
Traditional Economic Development Programs**

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EXECUTIVE SUMMARY

Taking Preschool Education Seriously as an Economic Development Program: Effects on Jobs and Earnings of State Residents Compared to Traditional Economic Development Programs by Timothy J. Bartik, Upjohn Institute for Employment Research, bartik@upjohn.org

This report takes preschool education seriously as a state economic development program by measuring the effects of a high-quality universal preschool program on the economic development goals of more jobs and earnings of state residents. Universal preschool's effects on state residents' jobs and earnings over a 75-year period are compared with the effects of a program of economic development subsidies to businesses of the same cost.

These jobs and earnings effects of preschool programs are calculated using estimates on the effectiveness of the Chicago Child Parent Center (CPC) program in increasing educational attainment. These estimates are extrapolated to future effects on employment and earnings. These calculations also use new estimates of the proportion of preschool participants who remain in the same state in which they attended preschool. In addition, a regional econometric model is used to estimate the economic effects of spending more government funds on universal preschool. Previous estimates of how lower child care costs affect parents' labor supply are used to estimate earnings effects of the part-time free child care provided by universal preschool. Finally, comparable effects are calculated for economic development subsidies by using previous estimates of how subsidies affect job growth, and how state job growth affects the employment and wages of state residents.

The key conclusions of the report are the following.

- **Preschool programs and economic development subsidies both have about the same cost-effectiveness in producing earnings benefits for state residents, yielding about \$3 in present value of earnings benefits for every dollar invested in these programs.**
- **In achieving economic development goals of more jobs and earnings for state residents, preschool programs and economic development subsidies should be viewed as complementary programs, rather than as potential substitutes for one another.** Economic development subsidies offer greater benefits over a 5 to 10 year period, while preschool programs offer more longer-term benefits.
- **Most of the earnings benefits of preschool are due to effects of high-quality preschool in increasing the future educational attainment and employability of preschool participants.** Other economic development benefits of preschool education—such as the effects of extra government spending or lower priced child care—are present but modest in size.
- **State government investments in preschool education not only produce sizable economic development benefits for the state economy, but also produce large**

positive spillovers on other states and the nation, due to out-migration of preschool participants with better job skills. These spillover benefits of preschool make a case for federal subsidies to encourage greater state government investment in preschool education. In contrast, a state's use of traditional economic development subsidies often harms the economies of other states by attracting job growth that would have otherwise gone to those states.

Chapter 1

UNIVERSAL PRESCHOOL AND ECONOMIC DEVELOPMENT

Analyzing Preschool as Economic Development

Universal, high-quality preschool education in the United States has recently been advocated by many groups, including the Committee for Economic Development, the Pew Charitable Trusts, and the group Pre-K Now (e.g., see CED 2002, and websites at ced.org, pewtrusts.org, and preknow.org). The case for universal preschool education is based in part on rigorous research that shows that high-quality preschool has large positive effects on participants and society, including reductions in special ed costs and costs from students having to repeat grades, reductions in criminal activity, increases in educational attainment, and increases in earnings (Schweinhart et al. 2005; Belfield et al. 2004; Reynolds et al. 2002).

A prominent recent argument to promote preschool education expansion is that preschool programs may be superior in achieving economic development goals to traditional economic development programs. An early prominent argument in this vein was made by Art Rolnick and Rob Grunewald, economists at the Federal Reserve Bank of Minneapolis: “The conventional view of economic development typically includes company headquarters, office towers, entertainment centers, and professional sports stadiums and arenas... Early childhood development programs are rarely portrayed as economic development initiatives, and we think that is a mistake. Such programs, if they appear at all, are at the bottom of the economic development lists for state and local governments. They should be at the top” (Rolnick and Grunewald 2003). Rolnick and Grunewald’s argument has been widely cited by preschool advocates and scholars. More recently, the CED has published a paper elaborating on Rolnick

and Grunewald's argument (Ehrlich and Kornblatt 2004). A related initiative is a large scale research project at Cornell University that has over the past few years published numerous papers and reports exploring the links between regional economic development and child care (see the website government.cce.cornell.edu and the recent paper by Warner et al. 2004).

The economic development argument for preschool may be made in part to try to create a broader constituency for preschool programs that will include business groups and state and local economic developers, and not just the usual advocates for better social services or education. Furthermore, by an interesting coincidence, the governmental resources devoted to state and local economic development are of the same order of magnitude as the resources required to fund universal preschool, which is estimated to require at least \$25–\$35 billion nationwide (CED 2002).

What are these traditional economic development programs? Traditional economic development programs, mostly pursued by state and local governments in the United States, provide financial incentives or customized services to individual businesses to encourage the creation of more or better jobs in a particular local labor market. Table 1 lists some examples of these types of financial incentives or services. Estimates are that economic development programs may cost at least \$30 billion a year (Bartik 2001), and possibly over \$50 billion a year (LeRoy 2005; Peters and Fisher 2004; Thomas 2000), with some uncertainty depending upon whether we only count tax breaks that are at least somewhat discretionary, or count any state business tax credit or deduction that is rationalized on economic development grounds. Most of these costs are in the form of customized tax breaks. Potential beneficiaries of traditional economic development programs include the businesses who directly receive the subsidies, those out of work or at risk of being out of work (potentially most of the adult working-age

population), workers who might receive upgrading to a better job (potentially most workers), owners of property who might gain from higher property values, owners of businesses whose profits go up with higher local retail demand (e.g., local newspapers), and state and local governments that might hope to gain tax revenue exceeding the costs of needed additional public services and the costs of the economic development subsidies. The potential constituency for economic development programs is large.

The Rolnick/Grunewald argument, that preschool education may be a superior way to achieve economic development goals, has so far been made rather loosely. The first part of the argument is that traditional economic development programs are costly and ineffective. Many economic subsidies do not affect location decisions, which means that “from a state and local perspective, the economic gains are suspect because many would have been realized without the subsidies” (Rolnick and Grunewald 2003). Even if subsidies are effective from a state and local perspective, the subsidy competition is a “zero-sum game,” from a national perspective, with a zero national gain as one state’s gain is matched by another state’s loss: “jobs are not created—they are only relocated” (Rolnick and Grunewald 2003).

The second part of the argument is to use the high quality research on preschool programs to argue that these programs have high benefit-cost ratios, or equivalently, high rates of return. For example, Rolnick and Grunewald argue, based on research on the Perry Preschool program, that preschool programs have rates of return of 16% for program participants and the rest of society, and 12% for the rest of society. These high rates of return are taken to imply that preschool programs have large economic benefits, which are equated with economic development benefits, and are asserted to be far greater than typical returns to economic development projects. For example, Rolnick and Grunewald mention various economic

development projects in the Twin Cities area, and ask, “Can any of these projects stand up to a 12% public return on investment?”

There are two major problems with this argument. The first problem is that “economic development” is being defined much more broadly than is typical. State and local policymakers think of economic development as being mainly about the goal of jobs: more and better jobs for the local area, particularly those that go to local residents, and the tax base these more and better jobs bring. According to surveys of elected local officials in cities with more than 100,000 in population, by far the most frequently mentioned “first priority” for local economic development is increasing the jobs located in the city, listed by 48% of all respondents, with the next most mentions for increasing the local tax base, listed by 18% of all respondents (Furdell 1994). Some of the key benefits of preschool education, such as reduced crime of participants, and reduced special ed and other K-12 education costs, while important social benefits, have little directly to do with the economic development goals of more and better jobs. For example, in the latest benefit-cost analysis of the Perry Preschool program, two-thirds of the total benefits of the program to participants and the rest of society are due to the benefits of reduced crime rates of participants, and reduced crime is an even larger percentage of the benefits that accrue to society excluding participants (Belfield et al. 2004, Table 6.2, p. 57). Thus, much of the 12% public return cited by Rolnick and Grunewald has nothing to do with economic development as usually defined.

We could define economic development as anything that increases any social benefit. In that case, “economic development” is synonymous with social benefit, and becomes a superfluous term. But the constituency that interprets economic development as more and better jobs is unlikely to perceive preschool education as a substitute for traditional economic

development programs simply because preschool has large social benefits unrelated to jobs, regardless of how we label those benefits.

A second problem with the Rolnick and Grunewald argument is that instead of subjecting traditional economic development programs to a rigorous evaluation, they only point out that some benefits claimed for traditional economic development programs are exaggerated. Much research analyzes the benefits and costs of traditional economic development programs (see reviews by Bartik 2004a and 2004b). This research corrects for the usual exaggerations of the benefits of traditional economic development programs, for example, it considers that most subsidies for location or expansion will not affect the location decision. A fair review of this research suggests that it is an exaggeration to claim that traditional economic development programs are always and everywhere a mistake. A more accurate summary of the research evidence is that traditional economic development programs too often are excessively costly, compared to their likely benefits. However, properly designed economic development programs can sometimes provide useful benefits for a reasonable cost. This is true both from a state and local perspective, and from a national perspective. These arguments will be elaborated later.

This report takes preschool education seriously as an economic development program by focusing on how effective preschool education is in accomplishing the economic development goals of more jobs and earnings for a state's residents. I also consider the effectiveness of traditional economic development programs, using the same metric of more jobs and earnings for a state's residents. This allows a fair comparison of preschool education and traditional economic development from the perspective of constituencies interested in economic development. Can preschool education be a useful substitute or complement to traditional economic development programs in achieving the traditional goals of economic development

programs? I also consider how alternative policies for designing and implementing preschool education or traditional economic development programs may alter their effectiveness in achieving economic development goals.

My analysis focuses on the effectiveness of these programs in increasing jobs and earnings for current state residents, and not on increases in jobs and earnings that accrue to in-migrants. Faster state growth is not good in and of itself, but only because it improves the opportunities of specific persons. Research on state and local economic development programs suggests that the great majority of the benefits from local job growth come from increasing employment rates or wage rates of the original local residents. In-migrants who take jobs gain but slight benefits as they could have otherwise moved to another similar local economy. In contrast, because local residents have valuable attachments to their current local area that make them reluctant to move, they can gain significantly if local job growth increases that local area's employment rate (the employment/population ratio) or real wage rate. Local growth also may have benefits for local property owners in increased property values, and local business owners in increased sales and profits. But, research suggests that the magnitude of these property value and business profit benefits are small compared to the effects on the earnings of local residents (Bartik 1991, 1994, 2005). Finally, research suggests that the fiscal effects of increasing both employment and population by the same magnitude are often negative, particularly if there is no excess capacity in local infrastructure and services (Bartik 2004b, 2005). Businesses tend to pay more in state and local taxes than they directly require in state and local public services, whereas households tend to pay less in state and local taxes than they require in state and local public services; if new infrastructure is required, the cost of providing additional services for new households will be particularly high. Therefore, fiscal benefits of job growth are likely to be

much greater if local employment increases faster than local population, increasing local employment rates.

Focusing on how preschool education and traditional economic development programs affects employment rates and earnings of a state's residents therefore focuses on the main sources of the usual benefits from traditional economic development programs. I emphasize that a measure of these effects on state residents' jobs and earnings is by no means a comprehensive benefit-cost analysis of either preschool education or traditional economic development programs. As previously mentioned, this analysis omits the benefits of preschool education for reducing crime or saving public education costs, which previous research suggests are important components of the overall benefits of preschool programs. As just mentioned, this analysis also omits the slight benefits of traditional economic development programs for the jobs gained by immigrants, as well as the somewhat larger benefits of job growth for property value gains and the profits of state businesses. Any anti-crime benefits of faster state job growth from traditional economic development programs are also omitted. Fiscal benefits of either preschool education or traditional economic development programs are also not directly measured, except insofar as fiscal benefits tend to be positively related to higher employment rates of state residents. Finally, any adverse effects of increased jobs and economic activity from either preschool or economic development subsidies in creating environmental problems, or traffic jams or other congestion problems, are also not considered.

I do try to measure some of the effects of higher employment rates and earnings of state residents in various forms that may be useful to state and national policymakers. In addition to measuring effects on jobs and earnings of state residents, I also measure the associated increase

in gross state product, and the tax revenue raised by this increase in gross state product. These effects are measured for each state and for the nation.

Finally, a full benefit-cost analysis of the net benefits of increasing employment would subtract some opportunity cost of the time that is devoted to increased employment, which presumably had some value to those who are working more. However, the dollar value of the time of the unemployed is a topic on which there is little good research. Labor economists commonly presume that the unemployed place a lower value on their “non-work” time in a high unemployment local labor market, in which jobs are hard to find even for those who are desperate for jobs. In addition, a full analysis of the benefits of increasing employment rates of local residents would recognize that there is some social value to lower unemployment rates, because research shows that even the happiness of the employed is increased by lower unemployment, whether by sympathy with the plight of the unemployed or the worker's self-interest in lowering his or her own risk of becoming unemployed (Layard 2005).

Although this report is not a comprehensive benefit-cost analysis of either preschool education or traditional economic development programs, the effects of these programs on jobs and earnings of state residents is important to many constituencies. The claimed effects of traditional economic development programs on jobs and earnings are the main reason the general public might support these programs.

Outline of This Report's Approach

I estimate the effects on the economic development goals of more jobs and higher real earnings for state residents of a permanent universal preschool program, and of a traditional economic development program, with both programs scaled to the same present value of costs

each year. More jobs are measured as the increase in the employment rate of state residents. Higher real earnings of state residents occur through higher employment rates, or higher real wages per hour.¹ All costs and real earnings are measured in year 2004 dollars.

I emphasize that I only count the jobs and earnings created that raise employment rates and annual earnings of state residents, and not all the jobs and earnings generated by higher state growth. If jobs in a state grow, but population expands sufficiently that employment rates and annual real earnings of state residents is unchanged, then these additional jobs and earnings are not counted. Jobs growth and real earnings growth is only counted to the extent to which it improves the employment and earnings opportunities of individuals.

These effects are analyzed over a 75-year period following enactment of the new permanent universal preschool education program, or alternatively following enactment of a new economic development program. In comparing the present values of earnings effects with the present value of costs, both earnings effects and costs are projected into the indefinite future, beyond the 75-year time horizon. Throughout the report, in all tables, the dating system used is that the initial year in which the new program is adopted, either the universal preschool program or the new economic development program, is dated as year 4, with the year number indicating the age of the first cohort that would potentially experience the universal preschool program.

Both the traditional economic development program and the universal preschool program are assumed to be financed in the same way. The easiest way to think of the financing is that both programs are assumed to be financed by some neutral tax that does not affect incentives to migrate, work, or produce for any household or business. For example, it might be plausible that

¹Higher weekly hours can also raise annual earnings, but I ignore this factor, as there is not good information on the effects of preschool on weekly hours, and effects of traditional economic development programs

a land tax would not significantly affect incentives. The tax financing, however, could potentially affect demand for goods and services in the state, by affecting after-tax income of state residents.

In the real world, of course, the actual mix of taxes to finance these programs might affect incentives for households to migrate or work, and incentives for businesses to expand or contract. However, the substitution of what economists call “distortionary taxes” (because they distort incentives) for a neutral tax such as a land tax will alter effects in the same way for both economic development programs and preschool education. We can think of the enactment of the proposed programs as taking place in two stages: first, the economic development program or preschool program is expanded and financed by a neutral tax; second, some distortionary tax is substituted for the neutral tax. The report shows the effects of each program under the first type of financing. The second stage substitution of a new financing method will change the absolute effects of the two types of policies, but will leave their relative effects unchanged. Therefore, this report’s discussion of the relative effects of traditional economic development programs versus preschool education will be unaffected by the use of a wide variety of imperfect, distortionary sources of financing.

Most of this report analyzes all costs and effects from the perspective of the state carrying out the program. Chapter 5 considers how these effects are altered from a national perspective.

Some of the assumptions made in modeling are outlined in Table 2. The characteristics of the universal preschool program being considered are closely modeled after the program considered in the comprehensive recent study by Karoly and Bigelow (2005). It is assumed that

the free universal preschool program is aimed at only 4-year-olds, and enrolls 70% of all 4-year-olds. The gross costs per 4-year-old participant are derived from Karoly and Bigelow, as is the net cost after allowing for current publicly subsidized preschool costs, although I make some slight modifications to their estimates of net costs.² The state economy in which these programs operate is assumed, based on CBO projections, which in turn are slight variations of Social Security Administration assumptions, to have average employment and population growth over the next 75 years of 0.3% annually. Average real wage rates per hour are assumed to grow at 1.2% annually. The cost of the universal preschool program are assumed to grow proportionally both with the rate of population growth and average real wage rates, given that most preschool education costs are personnel costs, or the costs are assumed to grow at 1.5% per year (to be exact, 1.5036%, as $1.015036 = 1.012 \times 1.003$). The number of graduates from this hypothetical preschool program grows proportionately with the population, or at 0.3% per year.

Some of the effects of these programs are calculated for a hypothetical state in which the universal preschool program would start out at a cost of \$1 billion per year, and then grow with the population and economy. Based on the assumption that 70% of all 4-year-olds are served, and national ratios of employment and population to the number of 4-year-olds, this hypothetical state would have employment and population as outlined in Table 2.

For this hypothetical \$1 billion per year program (in initial costs), effects are calculated in terms of jobs created for state residents, and additional real earnings created for state residents. In addition, I calculate the effects of these programs in terms of job creation effects as

²Karoly and Bigelow's estimates of net preschool costs are based on data for California. However, as will be explored further in Chapter 3, so are their estimates of net benefits of preschool education. In general, net costs and net benefits of moving to universal preschool education will both go down to the extent to which the state already has a greater proportion of state residents in preschool education. Therefore, the net effects per dollar of

a percentage of total state employment, and earnings creation effects as a percentage of total state earnings. These percentage calculations would apply to a state of any size with similar characteristics, not just this hypothetical state.

I also calculate the present value of the earnings effects of the traditional economic development programs, and the preschool education program, per present value dollar of costs. I do this using a real social discount rate of 3% annually to convert future flows of earnings benefits or program costs equivalent to today's dollars in present value terms. This 3% social discount rate is a commonly used real discount rate in benefit-cost analysis.

For each state, I also estimate the long-run effects of traditional economic development programs and universal preschool education programs of the same annual costs on each state's employment and earnings of residents, gross state product increases associated with increased earnings of state residents, and increases of state and local tax revenue associated with this increase in gross state product. These estimates are calibrated to each state, and reflect a number of state-specific factors, such as the size of the state in terms of total employment, average wage rates in the state, the labor share of gross state product, the share of state and local government revenue in the state economy, and the propensity of state residents to leave the state over time. As outlined in Table 2, the costs of this "universal" preschool program in each state, or traditional economic development program of the same costs, is calibrated to each state's size based on each state's wage and salary earnings.³

incremental new preschool spending should be similar in different states with different preexisting preschool programs.

³How exactly to define the net incremental costs of a "universal" preschool program is somewhat arbitrary. One could just consider the number of 4-year-olds, but this ignores the numbers currently being served, and ignores cost differences in different states. The definition used in this report has the virtues of simplicity, and of controlling for earnings, which is one highly relevant factor in determining the costs of and demands for universal preschool.

For both traditional economic development programs and preschool education programs, the programs considered in the baseline simulations are reasonably well-run from the perspective of having effects on employment rates and wages, but not necessarily ideally run. Chapter 4 considers policy alternatives that can either magnify or shrink the effectiveness of these programs in creating more and better jobs for local residents.

Most of this analysis considers effects from an individual state's perspective. Chapter 5 analyzes how effects on jobs, earnings, gross product, and tax revenue change from a national perspective. To what extent does one state's decision to devote more resources to traditional economic development programs or preschool education cause either spillover benefits or spillover costs for other states?

Chapter 2

TRADITIONAL ECONOMIC DEVELOPMENT PROGRAMS

Methodology of Estimating Effects

To analyze how traditional economic development programs affect jobs and earnings for state residents, we need to consider all the many links in the chain of causation between expending state and local resources on economic development and the ultimate effect on state residents' employment rates and earnings. Some of these links augment the effect of traditional economic development programs on employment rates and earnings, but most of these links diminish the effects.

The traditional economic development subsidy analyzed is assumed to be a financial subsidy for business location or expansion, as such financial subsidies comprise the bulk of the resources devoted to economic development. Based on my knowledge of typical economic development programs, the subsidy is assumed to be paid out over a 10-year period. This reduces the cost-effectiveness of the subsidy compared to a 100% up-front subsidy, as businesses apply a relatively high discount rate to future costs, profits, and subsidies, but has some advantages for state and local government officials. One advantage is that delaying some of the subsidies reduces potential problems with the business receiving the subsidy but later reversing its location decision. An alternative is to give more subsidies up-front, but negotiate "clawback" agreements allowing most or the entire up-front subsidy to be recovered if the business relocates, but such clawback agreements have enforcement costs. In addition, delaying some of the subsidies has the political advantage of postponing some of the costs of the subsidy program to the next Governor.

For these baseline estimates, the business being subsidized is assumed to be an “export-base” business, which means that the good or service produced by the business is either supplied to markets outside the state, or supplied to a local buyer who otherwise would have purchased this good or service from outside the state. If a subsidized business was not an export-base business, then its goods or services are being sold to local buyers who customarily buy from other local suppliers, and consequently any increased business activity of the subsidized business reduces activity in other local businesses one-for-one. The need to focus subsidies on export-base businesses is conventional wisdom in the economic development profession. Although such an export-base focus is not always followed in practice, there are many state and local economic development programs that do focus subsidies on export-base businesses. For example, many property tax abatement programs only allow property abatements for manufacturing companies, which are mostly export-base businesses. The later section of this report on policy alternatives considers subsidies to non-export base businesses, such as subsidies for sports stadiums, whose effects on job creation for state residents would be far less.

The subsidy modeled is based on data on average economic development subsidies. Based on Peters and Fisher’s research, this average subsidy is assumed to be \$1,047 per job per year in 2004 dollars, for ten years.⁴ Therefore this subsidy is about 50 cents per hour of labor.

The probability that such a subsidy will prove decisive in positively affecting the location

⁴This figure is derived from Table 3.7 in Peters and Fisher (2002, pp. 74–75) that gives the average present value of state and local economic development subsidies, across 75 cities in 13 leading industrial states, and including enterprise zone incentives, at \$5,048 in 1994 dollars, where they calculate present value using a 10% discount rate. This present value and discount rate corresponds to annual subsidies of \$1,047 over 10 years in 2004 dollars. Note that I am not assuming that a 10% discount rate is a valid discount rate; in fact, in later calculations, I assume that 3% is the appropriate social discount rate, while 12% is the discount rate actually used by corporate decisionmakers. Rather, I use the 10% discount rate actually used by Peters and Fisher to translate their present values into corresponding annual flows, as their present values were actually calculated using the 10% discount rate for annual flows; I am merely reversing the process. My calculation of annual flows will not depend on the discount

decision is estimated at 3.7%, based on business location research. In other words, in only 3.7% of the cases is the subsidy needed, and in the other 96.3% of cases, the subsidy simply benefits the subsidized business with no economic development benefits for the state economy.

The essential assumption to derive this estimate is that subsidies to business location will be no more effective on average than general business tax cuts in inducing business location decisions. Hence, the net cost of creating a job should be similar in general business tax cuts and economic development subsidies. Although state and local governments may seek to be more selective and choose to give subsidies only when needed to induce a location decision, in practice government officials lack sufficient knowledge of a business's relative profitability at different locations to be able to offer the optimal subsidy to just tip the location decision. It is also politically difficult to restrict subsidies to just a few eligible companies without definitive rules outlining which companies are eligible and ineligible based on objective criteria. In economic development, this problem of refusing to give out subsidies to eligible firms is referred to, after an old TV ad, as the "reverse potato chip rule": you can't give out just one.

What is the cost of creating a job through state and local business tax cuts that is implied by previous research? The research literature on state and local business taxes and business activity has been reviewed by Wasylenko (1997), who concludes that a plausible elasticity of state and local business activity with respect to state and local business taxes is -0.2 , that is a 10% reduction in all state and local business taxes will increase a state or metropolitan area's business activity by 2%. This is a little below the -0.25 or -0.3 elasticity suggested in research reviews by Bartik (1991, 1992). Annual state and local business tax revenue per job seems to

rate used by Peters and Fisher to the extent to which typical economic development subsidies in fact are flat annual subsidies lasting around 10 years.

average about \$3,889 per job in 2004 dollars, based on Peters and Fisher's research (2002, p. 106). The Wasylenko elasticity figures implies that if we offered a group of firms a 10% business tax cut, of \$388.90 per job, then if we would have attracted x jobs without the business tax cut, now we will attract x times 1.02 jobs, for example if we would have attracted 100 identical firms each employing x workers, now we will attract 102 identical firms each employing x workers. We sacrifice 388.90 times x workers per firm times 100 firms in tax revenue to gain in jobs x workers per firm times two firms. The annual cost per job gained is equal to $(388.90 \text{ times } x \text{ times } 100/2 \text{ times } x) = \$19,445$ in annual foregone business tax revenue per job gained. More formally, a little algebra shows that the cost in foregone tax revenue in creating jobs through business tax cuts is equal to the annual business tax revenue per job divided by the elasticity of business activity with respect to taxes, or $\$3,889 / (0.2) = 19,445$, based on Peters and Fisher and Wasylenko.⁵ Note that we do not count the revenue gained on the attracted firms, as the fiscal effect of new firms will also include any impacts on public service and infrastructure needs due to the new employment and the population it attracts, along with the tax impact of the new population. The intuition is that the cost per job created goes down if we think that businesses are very responsive to a percentage cut in state and local business taxes in making business location decisions, and goes up if state and local business taxes are higher, which causes a given percentage tax cut to be more costly.

⁵I have used similar cost per job calculations in various refereed papers, most recently in Bartik (2005), which references other published papers with similar calculations over the last 15 years. This is not a new type of calculation that I have invented out of nowhere for the present report, but rather a calculation that has at least gained some credence in the scholarly literature. Note that the elasticities here are long-run elasticities of state and local business activity, but that such elasticities should be the relevant one for evaluating the immediate elasticity of new business location decisions, under arguments outlined in Bartik (1991, pp. 235–236). Also note that this elasticity is derived from a mix of studies that look at corporate location decisions and studies that look at general responsiveness of overall state or local employment or other business activity measures to state and local business taxes.

To determine what effects subsidies of different lengths will have, we have to make some assumptions about how firms discount future cash flows. Research on firm's discount rates suggests that corporate decisionmakers use a real annual discount rate of about 12% (Summers and Poterba 1994). Using this discount rate, the present value of a permanent tax cut of \$19,445 per year is \$181,487, or the implication is that just giving firms a lump sum of cash up-front would have a cost per job created of \$181,487. A lump sum of \$181,487 corresponds to a 10-year subsidy, using a 12% discount rate, of \$28,679 per job per year. If the average economic development subsidy of \$1,047 per job per year is to result in the same cost per job created, the subsidy would have to be decisive in 3.7% of subsidized firms, as $3.7\% = 1,047 / 28,679$.

Given that I am assuming that the effect of an economic development subsidy on the probability of inducing a positive location decision is proportional to the size of the subsidy, the cost of creating a job through incentives does not depend on the annual subsidy considered. It is easier to get some intuition if we consider a larger subsidy. Also, such large subsidies more closely resemble the subsidies given in the high-profile cases that receive the most media attention. According to Peters and Fisher, the largest subsidies commonly encountered amount to about \$4,704 per job per year (in 2004 dollars) for ten years, or about a little over \$2.35 per hour of work.⁶ This subsidy of a little over 4 times the average subsidy would be expected to have effects a little over 4 times as great, or affect the probability of choosing that state by about 16%.

These assumed effects of different-sized subsidies are based on the business location literature. However, perhaps that is not particularly convincing unless the reader is prepared to

accept this on authority. But we can ask whether this effectiveness of subsidies seems intuitively plausible, even if we had no reliable knowledge from the business location literature.

Everyone's intuition varies, but I would argue that these numbers seem at least somewhat intuitively plausible. If average real wages per hour in 2004 were \$17.67 (author's calculations using data from the Current Population Survey), then surely an economic development subsidy that essentially made labor free for ten years would be decisive in influencing location decisions in many, although perhaps not all cases. It is then perhaps somewhat intuitively plausible that a subsidy of \$2.35 per hour might be decisive in tipping location decisions 16% of the time.

If the subsidy induces the subsidized business to make a positive location decision, and the business is 100% an export base business, then the business location decision will have some positive "multiplier" effects by increasing local demand for other goods and services.

Specifically, the subsidized business's induced new activity will increase demand for local suppliers, who will as a result expand their employment. In addition, the additional wage and salary income for the subsidized business's additional employees, and the additional employees at local suppliers, will increase demand for goods and service at local retail industries, which as a result will also expand employment. Multipliers can vary widely, and will increase for businesses with stronger local supplier links and businesses that pay their workers higher wages.

We assume an average multiplier of 1.80, based on data on multipliers calculated by the University of Michigan for Michigan's MEGA economic development subsidy program.⁷

⁶This is based on Peter and Fisher's figures for the highest average state and local economic development subsidy provided in the 75 cities they consider, which has a present value at a 10% discount rate of \$22,678 per job (Peters and Fisher 2002, Table 3.7, pp. 74–75).

⁷This calculation is based on data on MEGA that was used by the author and colleagues in previous work on Michigan's economic development programs (Bartik, Erickcek, and Eisinger 2003). The median multiplier that is reported by the MEGA program for its subsidized firms is 1.98. This is calculated by the University of Michigan using the well-respected REMI model (Treyz 1993). Therefore, I suspect the multiplier figures are reasonable.

These assumptions together determine the average cost per job created in a state or local area by this typical economic development subsidy program. A later section will consider variations based on different policy choices. For the baseline simulation, the present value, at a 3% discount rate, of the \$28,679 in annual subsidies over 10 years that would be needed to create one export-base job, is \$251,975.⁸ Including multiplier effects, the present value of the subsidies needed to create one job is \$139,986 (= 251,975 / 1.8). A subsidy program that lasted for one year, that cost \$1 billion in present value, and offered 10-year subsidies to firms willing to locate or expand in a state, would be estimated to create 7,144 permanent jobs in the state (7,144 = \$1 billion / \$139,986). That is, the level of jobs in the state would be permanently higher than it otherwise would be by 7,144 jobs. The one-time subsidy program of \$1 billion in present value therefore is estimated to cause a permanent shock to the level of state jobs, but only a one-time shock to the growth rate of state jobs.⁹ However, if the subsidy program is ongoing in future years, it would also increase jobs in future years, and therefore have a

However, the MEGA program is much more selective than the typical economic development program, and in particular is one of the few programs I know of that has a professionally done impact analysis done before the subsidy is awarded. In addition, Michigan may have higher multipliers for export-base businesses than the average state, as Michigan has particularly dense networks of industrial suppliers. So, I suspect that MEGA multipliers are higher than average for economic development subsidies. For the analysis in this report, the assumed multiplier of 1.80 is equal to the 40th percentile of the multipliers reported for MEGA subsidies. The reader might also wonder about displacement effects of shocks to employment. In the preschool chapter, I take account of displacement effects of shocks to a state's labor supply. Analysis should also take account of displacement effects of shocks to a state's labor demand. Displacement effects of labor demand shocks occur because the initial shock to employment affects state wages, land prices, and other prices, thereby affecting household migration and firm location decisions. The REMI-calculated multipliers already take account of such effects, as well as effects of agglomeration economies due to increasing overall employment density or increasing concentration in the state of a particular industry.

$$^8 251,975 = 1.03 \times (28,679 / 0.03) \times (1 - 1 / (1.03)^{10}).$$

⁹It may seem strange that a one-time subsidy causes a permanent increase in the level of state jobs, but this is consistent with research on how local economies behave. The economic development of state and local economies seems to behave according to a random walk, in which shocks to local economic activity from a variety of causes tend to persist indefinitely. One rationale for this behavior is that although the initial causes of the shock may not persist, the agglomeration of economic activity in a local activity tends to encourage further economic activity. The net effects of regression to the mean of the initial cause of the shock, and agglomeration, appears to be that local economic shocks persist in terms of levels.

permanent effect on state job growth corresponding to the permanent nature of the subsidy program.

But how many of these new jobs will go to state residents? Ultimately, the new jobs created in the subsidized business, and any jobs created in suppliers and retailers as a result of the multiplier, must ultimately either go to non-employed state residents or to individuals who otherwise would have lived in another state, that is the job increase must be divided among increasing the employment rate of state residents or increasing the state population.¹⁰ Of course, some of the newly created jobs go directly to other state workers who already were employed. But this hiring of current state workers results in vacancies that either are filled by state residents who otherwise would be nonemployed, persons who otherwise would have lived outside this state, or state residents who otherwise would be employed. The chain of created vacancies goes on until eventually each new job either goes to a new resident or a newly employed state resident.

There is extensive research on how local job growth is typically apportioned among new jobs for local residents vs. local population growth (for a review, see Bartik 1993). This research suggests that in the short run, after a year or two, between 50 and 80% of the new jobs go to increasing the employment rates of local residents, and the rest go to persons who otherwise would have lived elsewhere, such as in-migrants. The proportion going to in-migrants increases over time, but even after 17 years, perhaps 2 of 10 new jobs are still reflected in higher employment rates for local residents. All of this extra local employment is due to higher local

¹⁰More formally, the percentage increase in state jobs is approximately equal to the percentage increase in the state's employment to population ratio and the percentage increase in the state's population. This approximate equality is exact if we use logarithmic percentage growth, or $d\ln E = d\ln(E / Pop) + d\ln(Pop)$, where d is some change in the variable over time, \ln is the natural logarithm of the variable to the right, E is state employment, and Pop is state population.

labor force participation rates; effects of local job growth on local unemployment rates dissipate after 4 to 8 years.

Why does local job growth have such persistent effects in increasing local residents' employment rates? After all, over a typical four-year period, over 13 percent of the population moves between metropolitan areas (Marston 1985). Individuals switch jobs frequently, with between 20 and 30% of all employed workers having been at their current job for less than 18 months (Jaeger and Stevens 1999). If local residents are no more competitive in the labor market after the job growth than they were before, then as individuals move in and out of jobs, we would expect this competition from in-migrants to quickly dissipate any temporary gains in employment rates.

One theory offered to explain these highly persistent effects on local employment rates is that the short-run increases in employment rates for local residents due to job growth lead to permanent advantages for a few local residents in the labor market. Due to the short-run job growth, local employers hire some local residents who otherwise they would have avoided hiring. As a result, these local residents gain valuable job skills, self-confidence, and a stronger reputation with that employer and other employers. These changes in worker skills, confidence, and reputation allow these local residents to more easily be hired in the future, and allow them to maintain their higher employment rates even after more in-migrants are able to enter the local labor market. Some of the evidence for this theory is further reviewed in Bartik (2001) and Bartik (1991).

If this is the cause of persistent effects of job growth on local residents' employment rates, then these effects will dissipate as the local residents who experienced the initial job growth either die or relocate to other local areas. Therefore, the effects of a one-time local job

growth shock on local employment rates are not permanent, but are extremely persistent, and gradually fade over about a 50-year period.

To simulate these likely persistent but gradually decaying effects on state employment rates, I assume initial effects on unemployment rates that fade over five years, consistent with the empirical literature (Bartik 1993, 1991; Blanchard and Katz 1992). Effects on local labor force participation rates fade based on assumed patterns of out-migration from the state and mortality rates. This fading is based on dividing the initial employment shock as an equal increase in employment rates among all workers ages 16–79 in the state’s workforce. Table 3 shows the pattern of change over time in assumed employment rates and labor force participation rates, which closely matches direct evidence for the effects of a growth shock during the first 20 years after the shock, but after that is a plausible extrapolation of what we know.¹¹

In addition to persistent effects on local employment rates, job growth seems to have persistent effects on local real hourly wage rates (Bartik 1991). These effects seem to be due mainly to individuals with given credentials being successful in moving up to higher paying occupations; the real wage for a given occupation seems to not increase much if at all due to job growth.

A plausible explanation for these persistent real wage effects via higher occupational attainment is that some currently employed workers are also successful in gaining extra job skills, self-confidence and employer reputation due to the one-time shock to job growth. As a

¹¹Note that the assumed pattern of only gradual decay in labor force participation rates over 20 years is consistent with Bartik (1991, 1993), but not the widely discussed article by Blanchard and Katz (1992). Bartik (1993) argues that Blanchard and Katz’s model is misspecified. Bartik reestimates a more flexibly estimated model using Blanchard and Katz’s data, and finds much more persistent effects of state job growth shocks on labor force participation rates for at least 17 years. More recently, Rowthorn and Glyn (2003) have made a similar argument that Blanchard and Katz’s estimates find less persistent effects of job growth on employment rates due to problems caused by measurement error.

result of the job growth, in the short-run some local residents are hired by employers for higher paying occupations that they otherwise would have been able to attain. These higher wage effects of economic development programs reflect the considerable job churning as numerous job vacancies open up due to the new jobs created by economic development; estimates suggest that for every job created in a local area, in many cases at least 3.5 vacancies are created (Persky, Felsenstein, and Carlson 2004). Some of these short-run promotions result in persistent advantages for those promoted, who continue in the future to attain higher-paying occupations than would otherwise have been expected.

If this explanation is accurate, then the persistent effects of local job growth on local occupational attainment and hence real wages should also eventually fade due to worker mortality and out-migration. In the simulation, I assume that real wage gains fade in a similar pattern to the fading of effects on labor force participation. The level of worker real wage gains is set so that it matches empirical estimates for worker real wage gains during the 10 years after the job shock; the remaining 65 years of real wage effects is a plausible extrapolation.

Simulated Effects for Typical State

To help clarify the time pattern of effects, I first show the jobs and earnings over time created for local residents, for \$1 billion in present value of economic development subsidies committed to during the initial year of the simulation. This \$1 billion in economic development subsidies is promised to various businesses during the initial year, although the actual subsidies are paid out over 10 years, as described above. In this first simulation, to isolate the effects of this \$1 billion in subsidies, I assume that after this initial year, no further economic development subsidies are promised, although the initially subsidized businesses continue to receive their 10-

year promise of subsidies. Figure 1 and Table 4 shows jobs generated for state residents per \$1 billion in present value of resources devoted to economic development, which can be scaled up or down to show absolute numbers of jobs generated for state residents for an economic development subsidy program in a given year of any present value. The jobs for state residents that are counted in this figure and table are only those that increase the employment rate of state residents.

As previously mentioned, \$1 billion in economic development subsidies offered in a given year would be predicted, based on the business location research literature, to create about 7,144 in total jobs. Of course, the number of jobs in businesses that received subsidies would probably be much larger than that. The 7,144 in total jobs only counts jobs that were induced to be created in the state because of the subsidies. Many businesses that receive subsidies would have located in the state or expanded similarly without the subsidy, and the jobs in these subsidized businesses whose behavior is unaltered are excluded from the 7,144 jobs that are counted. Because of this job growth of 7,144 jobs, the employment rates of state residents initially increase by about 4,300 jobs, with the other job held by migrants. The 4,300 jobs created for local residents gradually declines over time, but is still over 2,000 jobs after 5 years, and over 1,000 jobs after 25 years.

For this same one-time use of \$1 billion in present value of financial subsidies for economic development in one year, I also show the effects on the real earnings of state residents, in Figure 2 and Table 4. These effects on real earnings are due in part to the increased employment of local residents. The effects on real earnings are also due in part to the increased wage rates of local residents who move up to higher paying occupations. In addition, the real earnings of everyone increases by 1.2% per year with the general real wage growth. As this

figure shows, the initial effect is to increase earnings by about \$216 million per year, an amount that quickly declines to \$145 million in extra earnings per year, and then much more gradually declines to zero over the next 60 years. After 25 years, the annual earnings increment is just under \$100 million per year. Again, all these real earnings increments are only for the increase in annual real earnings per person of the original state residents, ignoring effects on in-migrants.

Table 4 also shows the proportion of these earnings effects due to increased employment rates of the original state residents. The remaining proportion is due to the increased wage rates from occupational upgrading of the original state residents. With the initial reduction in unemployment rates, over 70% of the boost to earnings is due to increased employment rates. But as unemployment rates decline back to their previous level, in the long-run about 55% of the earnings effects is due to higher labor force participation rates, and about 45% due to higher occupational attainment.

These effects, however, only reflect the initial economic development subsidies provided for one year, whereas what we are considering is the effects of a permanent economic development program equal in present value to a proposed permanent universal preschool program. This program would devote \$1 billion in present value of resources to subsidies the first year, and the annual present value of subsidies devoted to this program would henceforth grow at 1.5% per year, equal to what would be required for the universal preschool program with population growth of 0.3% and real earnings growth of 1.2%. I assume that the cost of creating a job due to economic development subsidies also grows proportionately with real wage growth, or by 1.2% per year, as labor costs are the majority of locally based costs, and it makes some sense that the effectiveness of economic development subsidies should depend upon their relationship to total business costs. Hence, the jobs created by this subsidy program each year

will eventually grow by 0.3% per year. With real earnings growth, the real earnings of state residents created by these economic development subsidies will eventually grow by 1.5% per year.

Table 5 shows the job creation and earnings generation effects of this ongoing economic development subsidy program in this hypothetical state economy. The table also shows the job creation and earnings generation effects as a percentage of total state jobs and total state earnings, respectively. These percentage effects would apply to the typical state that adopted a permanent economic development program of this relative size compared to the state economy, regardless of the state's size. These percentage effects are shown in Figure 3. Absolute job creation and earnings generation is not shown because much of the job creation and earnings generation effects are due to the overall growth of the state economy. I emphasize that the job creation and earnings generation numbers and percentage effects are the cumulative numbers of jobs and additional dollars of earnings generated from this ongoing subsidy program at various lengths of time after the program is implemented. These figures do not reflect the job growth and earnings growth compared to the preceding year, but rather the cumulative effect by that year relative to the baseline. This effect compares a simulated world in which the state has an ongoing subsidy program versus a world in which the state does not have such a program.

As Table 5 shows, jobs created for the original state residents are about 19,000 after 5 years, about 30,000 after 10 years, about 54,000 after 25 years, about 72,000 jobs after 50 years, and eventually the cumulative number of jobs created due to the ongoing economic development subsidy program grows at 0.3% per year, the same as the state economy. To aid in interpretation, the 72,000 jobs after 50 years does not mean that there are 72,000 more jobs for state residents in year 54 of the simulation than in year 53, but rather that the number of jobs for

state residents in year 54 of the simulation are higher by 72,000 in the simulation with an ongoing economic development subsidy program, compared to what would have happened during year 54 in the baseline without an ongoing economic development subsidy program. As the figure and table show, the program eventually creates jobs for state residents due to higher employment rates equal to 0.61% of the baseline employment in the state economy, which is a significant boost. Real earnings increase by \$1.1 billion after 5 years, \$2.0 billion after 10 years, \$4.5 billion after 25 years, and \$8.0 billion after 50 years. As a reminder, the \$8.0 billion increase after 50 years, or year 54 of the simulation, compares the earnings for state residents in year 54 in a world with an ongoing economic development subsidy in this state, versus earnings in the state in year 54 in a world where the state had no such program. The \$8.0 billion does not reflect growth from year 53 to year 54 of the simulation. As a percentage of the state economy, the cumulative real earnings effects for state residents due to higher real earnings per capita eventually amount to a little less than a 1.2% boost to total real wage and salary earnings. This is about twice the percentage effect of the economic development subsidy program on employment, which reflects the finding that about half the effects of economic development subsidies on increased earnings come from increased employment, and about half come from increased real hourly wage rates from occupational advancement.

Using a 3% social discount rate, we can calculate the discounted present value of future real earnings associated with this economic development subsidy. Per dollar of present value of economic development subsidies, the resulting increase in present value of real earnings of local residents is \$3.14. This of course does not mean that the “benefit-cost” ratio of these traditional economic development subsidies is 3.14. A complete benefit-cost analysis would have to

consider many other factors, such as the opportunity cost of the extra time spent working after economic development subsidies, environmental and congestion effects, fiscal effects, etc.¹²

State-by-State Effects

Table 6 reports estimated long-run effects of economic development subsidies on each state. These estimates allow policymakers and observers in any state to measure these effects using a variety of metrics, including jobs, earnings, GSP, and tax revenues.

The estimated effects are for a permanent economic development subsidy program. This program's annual size in each state is equal to the assumed costs in that state of a universal preschool program. This sizing allows a comparison in each state of the effects of the economic development subsidies with the effects of the same size universal preschool program, which will be reported in Chapter 3.

For many states, these assumed annual costs of a preschool program happen to be of similar magnitude to current annual resources devoted to traditional economic development

¹² One question that might occur to readers after reading the preschool chapter is why there are no demand effects of the tax financing of the traditional economic development subsidies. Essentially, my implicit assumption is that the mix of traditional economic development programs is such that there are no net demand effects. The neutral tax financing has negative demand effects. If the economic development subsidies were delivered 100% in the form of services, such as small business services and manufacturing extensions services, then these added services would have a positive demand effect. As argued in the chapter on preschool education, it is well accepted that the net demand effects of such a balanced budget increases in services will be positive. However, in the preschool chapter, I find that this demand effect is small, only \$0.04 in present value of earnings created per dollar spent. The net balanced budget multiplier effects from increasing taxes to expand economic development services should be similar in magnitude to the balanced budget multiplier effects of increasing taxes to spend more on preschool education. Alternatively, at the other extreme, the economic development subsidies could be delivered totally through tax subsidies to businesses. We would expect the net demand effects of this to be negative, as it is likely that most of the subsidies largely affect flow to business owners who live out of state. However, even if none of the tax subsidies to business have any positive effect on demand for this state's goods and services, the negative effect of the increase in some neutral state tax will be modest. Because demand effects of traditional economic development subsidies are likely to vary with what type of economic development program is expanded, and because these demand effects are likely to be modest, for simplicity I assume that the mix of economic development programs among business tax breaks and public services to business is such that net demand effects of the higher neutral tax to finance more economic development program activity is zero.

subsidies. (There is no logical necessity for this to be so, but it turns that in many cases the costs are of similar magnitude.) For example, in Table 6 we examine for Michigan the effects of an economic development subsidy program of \$490.9 million per year, based on the assumed costs of a universal preschool program for Michigan. However, it also turns out that as of 2003, annual resources devoted to traditional economic development programs in Michigan were around \$706 million a year (Bartik, Erickcek, and Eisinger 2003). Of that \$706 million, \$531 million was the annual cost of various tax breaks, of which \$330 million was in the form of property tax abatements. So the figure in Table 6 is roughly equivalent to the resources Michigan currently devotes to tax subsidies for economic development.

The estimates reported in Table 6 of effects of economic development subsidies on job creation, real earnings, GSP, and state and local tax revenues, are all long-run effects. These are meant to be annual effects on the state economy as of 75 years after the economic development subsidy program was created. However, the effects are calculated based on the size of the state economy in 2004. Therefore, the numbers are best interpreted as the long-run effects that would have occurred in 2004 if a permanent economic development subsidy program had been initiated 75 years previously. The long-run effects 75 years from now would be larger, reflecting the growth in the state economy.

The Table 6 estimates are based on the effects reported in previous tables for a typical state. However, they are adjusted based on differences across states in residents' propensity to migrate outside the state. This is only a minor factor in evaluating the effects of economic development programs, but is much more important in evaluating the effects of preschool education programs. For economic development programs, much of the effects of economic development on employment rates and earnings occur immediately. However, these effects

decay over time in part due to residents leaving the state. For a state or jurisdiction with more out-migration—the District of Columbia, for example—the economic development effects on residents decay faster, because more of the residents who benefited from the jobs attracted by economic development leave for other states. The table notes describe the exact adjustments used. Differences in out-migration across states are further considered when preschool education’s effects across states are discussed in Chapter 3.

As can be seen in Table 6, economic development’s effects on jobs, earnings, gross state product, and state and local tax revenues are sizable. In many states, these effects to tens of thousands of jobs, and hundreds of millions of dollars in earnings, GSP, and tax revenue. In the typical state, effects on real earnings and GSP are many multiples of the annual spending on economic development. The annual state and local tax revenue generated tends to be a bit less than the annual state subsidies for economic development, but not much less for most states.

A final note: The table reports effects for the United States as a whole. These are not the actual effects for the U.S. as a whole, but rather the sum of the effects that each state perceives for itself. As will be explored in Chapter 5, attracting jobs to one state through economic development subsidies to some extent takes jobs away from other states. This “zero sum game” aspect of economic development is not reflected in Table 6.

Chapter 3

PRESCHOOL EDUCATION PROGRAMS

Assumptions

As mentioned, the preschool education program's estimated costs come from Karoly and Bigelow, and these gross costs are estimated to be \$5,856 per child participant in the program, before allowing for offsets from reduced current public spending for preschool. As in Karoly and Bigelow, I assume that such costs are sufficient to buy a program whose benefits for disadvantaged 4-year-olds, relative to no preschool, are similar to those achieved in the Chicago Child Parent Center (CPC) preschool program. This is certainly plausible because the CPC program spent only \$5,099 per participant per year in 2004 dollars, and the ultimate CPC effects on educational attainment did not vary statistically significantly with whether the child participated for one or two years (Reynolds et al. 2002).¹³ In the policy alternatives section, I consider whether a truly high quality preschool with lasting impacts may require greater resources.

I adopt Karoly and Bigelow's assumptions about how the universal preschool program's slots are distributed among high risk (family income less than \$30,000), medium risk (family income between \$30,000 and \$50,000), and low-risk 4-year-olds.¹⁴ I also adopt Karoly and Bigelow's assumptions about what the 4-year-olds in each risk group would have been doing without the universal preschool program, that is what proportion would have been participating

¹³The two-year participants seemed to have somewhat higher achievement effects in early elementary school, but did not do significantly better than one-year participants in later elementary school, avoiding juvenile crime, or educational attainment (Reynolds et al. 2002, p. 278; Karoly and Bigelow 2005, pp. 34–35).

¹⁴These divisions implicitly use the year 2003 dollar divisions of family income of Karoly and Bigelow. Depending on how family income did relative to inflation from 2003 to 2004, year 2004 divisions might be somewhat different.

in a current public preschool program, a private preschool program, or no preschool at all. I use Karoly and Bigelow's fairly conservative assumptions about what percentage of the benefits of the CPC-quality program for high-risk participants, relative to no preschool, would be realized by different sub-group of participants. The assumption is that high-risk participants benefit significantly more than medium and low-risk participants, and that benefits of the new preschool program are greatest for participants who otherwise would have attended no preschool, and greater for those who otherwise would have attended current public preschool programs than for those who would have attended a private preschool. Table 7 summarizes these assumptions. These assumptions lead to the conclusion that the net effect of the program for the average participant is only 23% of the effects the program would have on high risk participants who otherwise would have had no preschool.¹⁵

Karoly and Bigelow's estimates are based on detailed data from California. Why, then, do I use these estimates for all states? States no doubt differ in the extent to which 4-year-olds are currently in preschool. However, variations across states in current enrollment of 4-year-olds in preschool should change the costs and benefits of universal preschool in the same direction. For example, in a state with a greater percentage of 4-year-olds in preschool, the costs of moving to universal preschool will be lower, but so will the benefits. The ratio of benefits to costs of moving to universal preschool should be similar. Ideally, one might want detailed data for each state on current spending on preschool and the percentage of 4-year-olds in preschool, along with detailed data on the how enrollment in preschool varies with family income. But such detailed data for each state are unavailable.

¹⁵This 23% is derived by weighting the percentage benefits for 4-year-olds in each cell of Table 6 by the percentage of participants in universal preschool who fall into each cell. The calculation is:

In addition, the estimated effects are sensitive to assumptions about who will sign up for universal preschool, and how the benefits for different groups from a high quality universal preschool program compare to the benefits estimated in experimental studies. Karoly and Bigelow provide these assumptions. I prefer using someone else's assumptions about these key parameters. By using someone else's assumptions, I avoid the accusation that I have created arbitrary assumptions to manipulate my results. In Chapter 4, I consider how the estimates vary with alternative assumptions.

Types of Economic Development Effects

The potential economic development effects of a universal preschool program, on increasing employment rates and earnings of state residents, fall into five categories:

1. Effects due simply to the program spending money on public services, financed by local taxes. These are effects that would also occur from any public spending, regardless of its usefulness, for example it would occur from hiring people to dig holes and fill them up again.
2. Effects due to the free child care provided by the program, and the resulting effects upon labor supply. These effects would occur regardless of whether preschool education had any long-run effects upon its child participants, as long as parents perceived the preschool to provide high quality free child care for part of the year.
3. Effects that occur because participants in the program are more likely in the long-run to have higher educational attainment and be more employable.
4. Effects that occur because participants in the program have positive effects on their peers during K-12 education, and those peers in turn have higher educational attainment and become more employable.
5. Effects that occur because the higher average educational attainment in the state's labor force, as a result of effects on the educational attainment of participants and their peers, increases the productivity of the state's workforce beyond what would be expected based on the effects of educational attainment on individual wages. This increase in

$$23\% = (5 \times 100 + 3 \times 50 + 7 \times 25 + 12 \times 50 + 8 \times 25) / 70.$$

productivity from higher average education increases the profitability of the state for businesses, which attracts additional employment, raising employment rates and occupational attainment.

Each of these avenues by which preschool education may affect state economic development is considered in turn.

Balanced Budget Multiplier Effects

First, there is some stimulative effect of public spending on the economy, even if it is financed by increased taxes. This is a standard notion taught in most introductory macroeconomics course that is referred to as the “balanced budget multiplier.”

The basic idea of the balanced budget multiplier is that spending money stimulates the economy by increasing spending on goods and services and hence on the workers needed to produce those goods and services. In the context of a state or local economy, the stimulus occurs because it increases spending on locally produced goods and services and local workers. Of course, the increased taxes to pay for this spending also reduce demand for local workers and local goods and services. But this reduction in demand is only a portion of the increased taxes, as some of the funds taken away in higher taxes would not have been spent on local workers and local goods and services: the funds would have been saved, or would have been spent on goods and services from outside the state. In addition, the increased spending on local goods and services generates income for the workers hired and other factors of production. This income, which is equal to the tax increase, would also be expected to in part be respent on local goods and services, and in part respent on goods and services from outside the state or saved. In the simplest models in introductory economics courses, the stimulative effect of the respending on local goods and services generated by the initial spending on local goods and services exactly

offsets the economically depressing effect of the reduced spending caused by the higher taxes. In these simplest models, the stimulus to local production is exactly equal to the initial spending, and the employment effect is exactly equal to the workers initially hired to produce the public good or service. In more complex models, we recognize that the taxpayers paying higher taxes may not spend and save money in the same way as the individuals respending the income generated by the initial boost to spending; in particular, in the regional context, some of these individuals may live outside the state, which dramatically affects the likelihood that the respending will be on goods and services produced within the state.

To ascertain the size of the balanced budget multiplier for preschool education, my colleague, George Erickcek at the Upjohn Institute and I simulated the job creation effects of a universal preschool education program in the state of Michigan. We used a model developed by REMI (Regional Economic Models Incorporated) for the Upjohn Institute. REMI regional models are some of the most widely used and respected regional econometric models, and are documented and explained extensively in scholarly work (Treyz 1993; Treyz et al. 1992; Greenwood et al. 1991; Treyz et al 1993). In estimating this model, we also allowed for the public spending on universal preschool in part replacing previous public spending on preschool, and in part replacing private spending on private preschool.¹⁶

¹⁶The number of estimated universal preschool participants in Michigan, assuming 70% of preschoolers participate, as in Table 7, is 93,148. I assume that universal preschool spending is \$5,856 per participant, as assumed by Karoly and Bigelow (2005) or gross public spending of \$545.5 million. But we assume that for 47.1% of the participants ($47.1\% = 33\% / 70\%$ from Table 6), the universal preschool program replaces other public preschool programs that would have spent \$3,441 per participant (a figure derived from Karoly and Bigelow 2005), so there is a reduction in spending on current public preschool programs of \$151.0 million. Therefore, the net new public spending is \$394.4 million ($= \$545.5 \text{ million} - 151.0$ with correction for rounding error). In addition, 31.4% of participants ($31.4\% = 22\% / 70\%$ from Table 7) would otherwise have enrolled in private preschool. I assume that these participants' families would have spent \$1,995 per participant on private preschool, so the reduced private spending on preschool from the universal preschool program is \$58.4 million. Therefore, the net additional new spending in the state is \$336.1 million. The impact of this net \$394.5 million in additional public spending on public preschool is simulated using the Upjohn Institute's REMI model for Michigan, and yields a job creation figure for

Although these estimates are for Michigan, similar effects should apply in other states. States will of course differ in the proportion of income typically spent in-state and out-of-state. However, these differences will have similarly-sized effects on the magnitude of the depressing effects caused by increased taxes and the stimulative effects caused by increased respending. For example, in a state where a higher proportion of income is typically spent in state, both the depressing effects of increased taxes and the stimulative effects of respending will be higher, and these two effects will tend to offset each other.

The resulting estimates conclude that one-time spending of \$1 billion on preschool education produces an immediate “balanced budget multiplier” of creating 5,708 jobs. This might appear to be comparable to economic development subsidies, which create about 7,100 jobs from one-time spending. However, in the case of economic development subsidies, the subsidy induces, in some percentage of the subsidized firms, a particular location decision, and thus has a permanent or at least persistent effect on local jobs. In the case of the balanced budget multiplier effects of preschool education, the 5,708 jobs only exist while the money is being spent, so one-time spending on preschool of \$1 billion during a given year will only create the 5,708 jobs for one year.

Of course, we are considering a permanent universal preschool program in an economy with population and employment growth of 0.3% and real wage growth of 1.2%. Preschool

the state of 2,742 jobs in terms of total employment. But the estimated ratio of wage and salary employment to total employment from the Regional Economic Information System (REIS) for 2003 is 137.3 million / 167.2 million = 0.821. Therefore, projecting from Michigan, \$1 billion in net new public spending on universal public preschool will have job creation effects on wage and salary employment of $(\$1 \text{ billion} / \$394.4 \text{ million}) \times (0.821) \times 2,742 = 5,708$ wage and salary jobs, which is what is used in this report. Note that the estimated public spending on universal preschool education in this calculation of \$394.4 million is not the same as the \$490.9 million used in other tables. The former is derived using direct data on 4-year-olds and various assumptions about costs, the latter simply as a percentage of Michigan wage and salary earnings. Both are similar in magnitude, and which is more accurate is unclear. In any event, the balanced budget effects of preschool spending per dollar spent should not vary much using either figure.

spending will grow by 1.5% per year, and will provide 0.3% more slots for 4-year-olds each year. So the number of jobs created will grow next year by 0.3%. However, the only increase in jobs is this additional 0.3%, as the initial 5,708 of the jobs created next year only maintain the same 5,708 jobs created last year.

The resulting impact on the employment rates and wage rates of state residents is similar to an economic development program that initially permanently creates 5,708 jobs, and in all years after that only creates very small additional numbers of jobs. Table 8 and Figure 4 show the job generating and earnings generation effects on state residents of a universal preschool program, via this avenue of “balanced budget multiplier” effects of spending extra public monies. The short-run balanced budget multiplier effects are just a little below the short-run effects of a permanent economic development program of the same costs. For example, comparing Tables 5 and 8, the effects in the first year of program implementation, year 4, are initially 0.04% of state employment for the balanced budget multiplier effects, and 0.05% of state employment for the effects of the economic development program. But the balanced budget multiplier effects die out over time, whereas the effects of a permanent economic development program continue to increase. Eventually, the balanced budget multiplier effects on jobs or real earnings are close to zero, while the long-run effects of a permanent economic development program of the same costs is to increase employment for state residents by about 0.6% of baseline, and earnings for state residents by about 1.2% of baseline, which are both over 15 times the balanced budget multiplier effects for any year. In addition, a calculation of the present value of the balanced budget multiplier effects on earnings of a preschool education program is only about \$0.04 for every dollar of present value spending, whereas economic

development subsidies yield \$3.14 in extra present value of earnings for each present value dollar of costs.

It is useful to understand why the balanced budget multiplier effects on jobs and earnings of spending more on preschool is so much smaller than the effects on jobs and earnings of devoting resources to economic development programs. In both cases, jobs are being created by government activity, but they differ in what percentage of the cost of creating those jobs is being paid for by government. Under the balanced budget multiplier, essentially the new jobs and earnings that are created cost, to a rough approximation, about one dollar of extra spending for each dollar of real earnings created in the short-run, and then, as previously discussed, in the long-run only a portion of this short-run increase in jobs and earnings goes to increased jobs and earnings of state residents. In the case of economic development programs, the inducements needed to create a job generally are considerably less than the earnings associated with the subsidized jobs, even allowing for the likelihood that many subsidies have no effect on location decisions. If you can get away with it, spending a small amount of money to leverage a much larger behavioral change is the preferable way to achieve some policy goal.

Much larger estimates of the job creation and earnings generation effects of spending more money on preschool can be obtained if we count all jobs and earnings in the state; this report only counts the increased jobs and earnings that increase employment rates and annual earnings per person of state residents. But counting all jobs and earnings abandons a focus on jobs and earnings generation that is truly associated with higher individual economic well-being. If we count all jobs and earnings, we implicitly are valuing growth of any kind, regardless of the effect on individuals. Furthermore, if we did choose to count all jobs and earnings growth, regardless of whether it increased employment rates and earnings rates, we would have to also

do the same when analyzing traditional economic development programs. These traditional economic development programs have much larger effects on overall labor demand and overall state growth than the balanced budget multiplier effects of preschool programs. Traditional economic development programs by their nature are designed to directly boost labor demand. Preschool programs are unlikely to win the contest for the best way to boost labor demand, because preschool programs are not primarily designed to do so, and have such labor demand effects only as a side effect of their main purpose of providing services to children.

We could also obtain larger total job effects and total earnings effects from the balanced budget multiplier effects of an early childhood program that went beyond universal preschool to also fund a wide variety of child care and other developmental services to children younger than age 5. More taxes and more spending yield a larger total balanced budget multiplier effect. However, even with a much larger early childhood program, the present value of the earnings effects per dollar spent due to balanced budget multiplier effects is probably much the same, at about \$0.04 per dollar spent.

However, even if balanced budget multiplier effects of preschool education are modest, there are other avenues of effects of preschool education programs on jobs and earnings of state residents that we must also consider.

Child Care Effects and Parental Labor Supply

Another economic development impact of universal preschool is the economic effects of the offering of free, high quality child care on the parents of the child participants. In this report, I try to measure the effects on state residents' jobs and earnings due to the effects of lower cost

child care on the labor supply choices of the parents of participants, and the effects on this enlarged labor supply on job creation and earnings generation in state labor markets.

The preschool education program being considered operates for 525 hours during the year (Karoly and Bigelow 2005). If we assume that full-time work year is 2,000 hours of work (50 weeks of work \times 40 hours per week), and then add in annual commute time of 192 hours (192 hours = 50 weeks \times 10 commutes per week \times average commute of 23 minutes),¹⁷ then the offer of free child care for 525 hours a year reduces child care costs by 24%.

However, under the assumptions outlined in Table 7 for the preschool program, 47% (= 33 / 70) of participants in the universal preschool program already are in an existing free public preschool program, and therefore experience no reduction in child care costs. Therefore, child care costs for the average participant only decline by $24\% \times 0.53 = 12.7\%$.

In addition, calculations suggest that only 60.7% of all 4-year-olds are the youngest child in the family (author's calculations using March 2004 Current Population Survey). I assume that child care costs for the youngest child are the "marginal costs" that really drive labor supply decisions. Therefore, effective child care costs for the average participant are reduced by only $12.7 \times 0.607 = 7.7\%$.

The best estimates from the child care research literature suggest that the percentage point increase in the labor supply for a given reduction in child care costs might be around one-fifth of the reduction in child care costs.¹⁸ This suggests that a 7.7% reduction in average child

¹⁷Commuting figure comes from Bartik (2001, p. 62), based on the U.S. Census.

¹⁸Blau (2001) and Blau and Hagy (1998) estimate an elasticity of employment with respect to the price of child care of -0.20 . Given that this elasticity refers to the percentage change in the employment rate, and the average employment rate in Blau and Hagy's sample appears to be 56.4%, the elasticity in rate points would be -0.11 . However, the Blau estimates are towards the bottom end of other estimates in the literature; as pointed out by Blau and Hagy (1998, footnote 17 on p. 124), there are other estimates with about twice as large an elasticity, which would imply an elasticity in rate points close to -0.20 . In addition, the careful work by Anderson and Levine (2000)

care costs will increase overall labor force participation rates by 1.54% ($= 0.2 \times 7.7$). In the case of the universal preschool program in our hypothetical state, with an initial cost of \$1 billion, the program enrolls 236,183 4-year-olds, so the lower child care costs would be expected to increase labor supply by 3,637 additional labor force participants ($3,637 = 1.54\% \times 236,183$).

A crucial issue for analyzing the jobs and earnings generating effects of this labor supply increase is how local labor markets respond to labor supply shocks. This issue is also crucial for analyzing the jobs and earnings effects of the eventual labor supply increase of preschool participants when they grow up, which is analyzed below. It should be understood that an increase in the number of persons who are able and willing to work at particular types of jobs does nothing directly to create any jobs. What does create jobs is that employers have incentives for increasing the number of workers demanded in response to a labor supply shock. These incentives occur because a labor supply shock results in higher unemployment rates, as well as perhaps some reductions in wage rates, which makes it considerably cheaper and easier for employers to hire workers in this particular local labor market. On the other hand, any increase in unemployment rates and reduction of wages will discourage labor force participation of residents of the local labor market, and discourage in-migration. In general, we would expect a shock to local labor supply to result in some combination of an expansion to the number of workers demanded, as well as some “displacement” of some other labor supply.¹⁹

suggest that the elasticity with respect to child care price is higher for the poor and near-poor, and higher for women with children under 6 than for women with older children. Anderson and Levine, for example, find an elasticity in rate points of -0.28 for all women with children under age 6 (Table 10.8), and an elasticity in rate points of -0.15 for poor and near-poor women with children under age 13 (Table 10.9). Of course, not all the parents affected by the changes in child care prices due to universal preschool will be poor or near-poor. Overall, an elasticity in the range of -0.20 in rate points seems reasonable for this sample. As will be clear in the analysis, even tripling or quadrupling these elasticities, which seems implausible, would not dramatically affect the overall results.

¹⁹What about multiplier effects of the increase in employment brought about by the labor supply shock? As least as a first approximation, there will not be any multiplier effects. In the case of economic development

I assume, based on evidence reviewed in much more detail in Bartik (2001), that the net effects of these “displacement” effects is that the labor demand response to a labor supply shock is about two-thirds, that is the jobs created will be about two-thirds of the increase in labor supply. I also assume that the resulting adjustments to a new local labor market equilibrium involve relatively small changes in wages, which I ignore; this is consistent with the evidence, also reviewed in Bartik (2001), that relative wages at the local level are quite resistant to change.

Most economic analysis would get somewhat larger effects of a labor supply shock because the analysis implicitly assumes no long-run displacement, that is a labor supply shock will be fully accommodated in the long run by increased labor demand. However, some displacement is more likely at the state level, where additional labor supply adjustments are possible through migration. Furthermore, the empirical evidence seems to support some displacement.

Therefore, the labor supply shock of 3,637 will result in an increase in state jobs of 2,425. It is likely that most of the new labor force participants are female and of relatively modest pay. I assume that these additional workers are paid at the 40th percentile of women’s earnings, which in 2004 dollars is \$10.84 per hour (data from the Current Population Survey for 2003, available from the Economic Policy Institute at

subsidies, these subsidies have typically targeted export-base employment, which will then have multiplier effects on non-export-base employment. With labor supply shocks, any induced expansion will occur in both export base and non-export base employment. One way of looking at what occurs is that as a first approximation, the expansion of the non-export base will be the multiplier of the export-base. Another way to look at it is that with imports (to the local economy) equal to the local economy's exports, the expansion of local demand due to the extra local production will be exactly equal to the expansion of locally oriented production: the income generated will be spend on imports and local goods, and the production will go to exports and local goods, and if imports and exports are equal, the additional local production will just equal the additional local demand. Of course, in the real world it may not be the case that the induced employment expansion due to a labor supply shock will proportionately expand both the export-base and non-export base of the local economy; this would be an interesting area of study.

http://www.epi.org/content.cfm/datazone_dznational). Multiplying by 2,000 hours per year and 2,425 yields an increase in local earnings from this extra labor supply of \$52.6 million.

For permanent universal preschool programs, these jobs creation and earnings generation impacts will each increase with the economy, the job creation at 0.3% per year, and the earnings impact at 1.5% per year. Table 9 shows the effects over time. In addition, the present value of the increased earnings from the labor supply induced by lower child care costs is about \$0.05 for every \$1 in present value of costs for a universal preschool program.

These estimates do not attempt to capture all the potential effects on jobs and earnings of the child care provided by a universal preschool program. For example, there is some evidence that high quality child care may increase employee productivity, which may encourage job expansion and increase wages (Baughman et al. 2002; Shellenback 2004). In addition, the extra labor market experience gained because of the induced labor supply may increase future employment and wages of the additional labor force participants. Although these effects are potentially important, I did not add them in because our knowledge of the magnitude of these effects is so fragile that I think any such estimates would be highly speculative.²⁰

²⁰Not allowing for labor market experience effects of increased employment of parents from lower price child care may seem to contradict allowing economic development programs to have long-run effects by increasing the labor market experience of state residents. However, there is very strong evidence that shocks to local labor demand do have long-run effects on employment rates and wages. In addition, the case of a once-and-for-all shock to labor demand is quite different from a one-time shock to labor supply which later reverses. In the case of the job growth brought about by economic development subsidies or other demand shocks, we have a permanent change in labor demand. Individuals who previously could not be hired for a particular quality of job are able to be hired due to the demand shock. We are asking whether the individuals who gain employment that they would otherwise be unable to obtain due to that change are able to maintain their new status in the labor market, given that labor demand stays at this new higher level. In the case of lower price child care, we are considering a temporary lowering of child care costs for one year. Persons choose to participate in the labor force due to this lower-priced child care, presumably taking jobs they could have obtained previously with their qualifications. The next year, the lower price of child care disappears. With relative prices reverting to their previous levels, the most natural assumption is that observed behavior will revert to its old pattern.

In addition, these estimates do not capture other benefits of high-quality free child care that do not increase jobs or wages. For example, a household whose labor force participation is unchanged because of the free preschool will still perceive significant benefits from the free child care provided by the preschool program. In many analyses of the benefits and costs of preschool, the provision of free child care is a key component of the benefits. For example, in Karoly and Bigelow (2005, Table 3.2, pp. 94–95), the value of free child care from preschool is about equal by itself to the costs of universal preschool. However, in this report I am only looking at the benefits of high quality free child care that are reflected in increased employment and earnings.

As Table 9 in comparison with Table 8 shows, the child care effects I have measured are similar in magnitude to the balanced budget multiplier effect of universal preschool, but have a different time pattern of effects. The demand side effects of the balanced budget multiplier tend to occur immediately and then die out over time, whereas the effects of child care occur fairly uniformly over time.

Both child care effects and balanced budget multiplier effects are quite modest compared to effects of an equal cost program of traditional economic development subsidies. I already explained the lower balanced budget multiplier effects as due to the government in the balanced budget multiplier case paying 100% of the costs of a job, whereas in the economic development case the government is using leverage by paying considerably less than 100% of the costs of a job. In the child care case, in theory one could imagine that very small changes in costs could leverage much larger changes in earnings. However, in practice, child care is expensive, in many cases amounting to a sizable percentage of the earnings of the parent, and many of those offered free child care already participate in the labor market, so this limits the scope of the

effects on labor force participation. Furthermore, the type of high quality preschool offered here largely replaces other lower-quality public preschool programs that also offer free child care, which also reduces the effects of the program. Finally, the high-quality preschool program that is modeled here is not optimized to provide child care benefits, and is definitely not a universal child care program, as it does nothing to deal with problems of child care for younger children, problems in child care for sick children, or problems in child care for non-day shift workers. Programs optimized to deal with these child care problems could probably deliver more parental labor supply effects per dollar spent.

Effects on Preschool Participants and Their Peers

I now turn to considering effects of preschool on state employment rates and real earnings that occur due to effects of preschool education on participants' long-run success in the labor market. I also consider effects of preschool on employment rates and real earnings that occur due to effects of preschool participants on their peers in K-12 education. I consider these two types of effects of preschool together because peer effects end up being modeled as some multiple of direct effects on participants.

Evaluations of the Perry Preschool program and the CPC program suggest that high-quality preschool education has significant effects on the long-run educational attainment and employability of the children participating in the preschool. These increases in educational attainment and employability represent increases in both the quantity and quality of labor supply of these participants. I attempt to trace these labor supply increases over the entire working life of the average preschool participant. Such labor supply increases would be expected to affect the economic development goals of the number and quality of jobs created for local residents.

As mentioned above, I assume with Karoly and Bigelow (2005) that the effects of this model universal preschool program on participants should be modeled as being in the proportion 0.23 of the effects on the participants observed in the CPC; the participants observed in the CPC were high-risk children who otherwise would not have been in preschool, and a real-world universal program would have many lower-risk child participants, and many participants who otherwise would have been in some sort of preschool, albeit in many cases of lower quality. However, there are many other calculations that must be made to simulate the lifetime effects of a universal preschool program on the local employment and earnings of CPC-type participants.

First, the CPC data only reports educational attainment data at ages 20–21. We know from the Perry Preschool program data that educational attainment continues to increase beyond this time, both in individuals getting some sort of high school diploma or GED, and in individuals going on to get an associate's degree or bachelor's degree (Schweinhart et al. 2005, Table 3.1, p. 52). However, the effects at later ages, up to age 40, that are observed in the Perry program cannot be used without some alteration because we know that at age 19, the effects of Perry Preschool on educational attainment are twice as great as for the CPC program. Therefore, as outlined in Table 10, I assume that the effects of a CPC style program on the most high-risk participants will over time follow a pattern similar to that of Perry Preschool, but with only about half as much of a program effect on high school graduation. I also assume that the proportion going on to a postsecondary degree out of those who get a high school degree will at later ages in the CPC program group be the same as in the Perry Preschool program, and similarly for the CPC control group. Estimates of educational attainment effects for years other than those directly observed are interpolated or extrapolated. Extrapolation beyond age 40 assumes educational attainment does not change past age 40.

Second, the research on the CPC program provides no direct estimates of the employment and wage rate effects of the program on participants. Previous studies have essentially projected employment and wage effects based on educational attainment effects on employment and wages for various ages obtained from the Current Population Survey or U.S. Census (Reynolds et al. 2002; Karoly and Bigelow 2005). But there is strong evidence from the Perry Preschool program that the program's employment rate effects are considerably greater than what would have been predicted based on educational attainment. As shown in Table 11, most of these "unpredicted" employment rate effects occur because the Perry control group tends to underperform what one would expect based on its educational attainment. This presumably reflects the disadvantaged status of this control group, which is not fully captured by educational attainment data. The program appears to move the program group up to employment rates that are close to what would be expected based on educational attainment. Therefore, in projecting the employment rate effects of a CPC style program, I assume that there is a "bonus" effect of the program beyond that predicted by its effects on educational attainment, but only half as much as the bonus effect observed for Perry Preschool. The logic here is that the CPC program appears to have half as large an effect on educational attainment as Perry Preschool, and it seems a reasonable supposition that the bonus effects of the CPC on employment rates should also be half as large.

Third, in calculating effects on state and local economic development, which I interpret as meaning effects on the employment rates and earnings of state residents, we need to worry about migration. From a state's perspective, we don't want to count any extra jobs and wages generated by preschool participants who move outside the state. I work with two sources of data to make reasonable estimates of what proportion of 4-year-old preschool participants would be

expected at various ages to live in the same state they attended preschool in. First, from the Panel Survey of Income Dynamics, I obtain data that provides direct information on what proportion of 4-year-olds at later ages live in the same state they lived in at age 4, for different education groups. The PSID data have a reasonable sample size up to age 30, and provide some information up to age 39. Second, from the 2000 Census PUMS data, we obtain data on the proportion of individuals at all ages from 16 to 79 who live in the same state they were born in, for different educational groups. These data do not follow people over time, as the data are cross sectional, but if the mobility behavior of different cohorts does not change too much, it may give a reasonable guess about the pattern of mobility. Furthermore, these data by implication show out-migration since birth, which we expect would be greater than out-migration since age 4. But the sample sizes are huge, and the data cover the entire working life of the population.

Table 12 shows some of the results of these calculations. For both the PSID and PUMS data, we summarize for each age, based on the educational mix at that age of the preschool participants, the average expected percentage of persons expected to live either in the same state as they did at age 4 (the PSID) and their birth state (PUMS). A comparison shows that during the overlap of these two data sets, they follow roughly similar patterns of a gradually declining percentage of persons staying in the same state. As expected, the PSID percentages living in the age-4 state are somewhat greater on average than the PUMS percentages living in the birth state, by about a ratio of 1.08 to 1. To provide a reasonable simulation of the number of 4-year-old preschoolers who would live in their age-4 state at different ages, we multiply the PUMS percentages by this average ratio. The results are presented in the table.

These estimates of the percentages living in their age-4 state may be surprisingly large for many academic researchers and policy analysts, starting out at 85% at age 16 but only

gradually declining to 63% at age 79. The implication is that states can expect much of the benefits resulting from the long-run changed behavior of preschool participants to accrue to the state. Several points should be noted. First, these results are perhaps surprising because the average academic or policy researcher attended elite higher educational institutions and was far more active in national labor markets than is typical of average college graduates, who tend to stay closer to home both in college and thereafter. Second, preschool education's main effect on educational attainment is to raise high school graduation, not college graduation. Therefore, the typical preschool participant is considerably less mobile than the average college graduate. Third, these surprising estimates do appear consistent with the only known direct evidence, and may be conservative estimates of the propensity to stay in the home state. Schweinhart et al. (2005, p. 40) report that at age 40, 82% of Perry Preschool participants were still living in Michigan. The predicted percentage from the table is 72%. Finally, these estimates are for a typical state. Out-migration obviously varies across states. I will consider such variations in a later section that will produce state-by-state estimates of preschool's effects on economic development.

Fourth, I allow for the mortality of preschool participants. I use mortality tables for blacks from the National Center for Health Statistics to project the percentage of 4-year-old preschoolers who will be alive at various ages from 16 to 79.

Fifth, as with the labor supply effects of lower cost child care on parents, I allow for displacement. The effects of preschool are merely to raise the quantity and quality of labor supply. The actual change in job quantity and quality depends on how employers respond to this labor supply increase. Although the CPC studies and Perry Preschool studies do an excellent job of estimating the effects of these programs on preschool participants versus non-participants,

they cannot by their very design show whether some of the jobs and earnings gained by participants come at the expense of displacing non-participants from jobs and earnings. Such displacement will inevitably occur if the net increase in the state economy's overall jobs and earnings are less than the employment and earnings that is estimated to be gained by participants. I assume based on estimates from Bartik (2001) that the net increase in overall employment and earnings is two-thirds of the estimated gross increase in employment and earnings for participants. In other words, one-third of the gross increase in employment and earnings for preschool participants is offset by reduced employment and earnings for other workers.

Finally, the effects of preschool education for the average high-risk participant who otherwise would not have participated in any preschool are translated into effects for the average participant in a universal preschool program by multiplying by 0.23.

These calculations can be summarized in the following equation. This equation is calculated for each year of the working life for an average universal preschool participant:

Average effects directly associated with the average universal preschool participant

=

(CPC effect on employment or earnings via effects on educational attainment, calculated using CPC data on educational attainment extrapolated to higher ages using information from Perry Preschool, and data from the Current Population Survey Outgoing Rotation Group on employment rates and wage rates at each age and educational attainment group)

×

(a multiplier factor greater than one derived from Perry Preschool for how much extra employment rates increase over what would be expected based on effects on educational attainment)

×

(a multiplier factor less than one to account for preschool participants who move out of the state)

×

(a multiplier factor less than one to account for preschool participants who die)

×

(a multiplier factor of 2/3rds to account for the displacement effects of the extra employment and earnings of preschool participants in reducing the employment and earnings of some non-participants)

×

(a multiplier factor of 0.23 to account for preschool education having less effect on the average universal program participant vs. the average CPC participant).

This calculation is done separately for each age from 16 to 79. These effects on the employment and earnings of the average preschool participant, when multiplied by the number of participants, gives total effects on jobs and earnings, and allows for calculations of the ratios of these effects to dollars spent on preschool education, total state employment, or total state earnings.

In addition to these direct social effects of preschool on participants' later education and earnings, there are social spillovers on the employment and earnings of others. Some of these are "in-school peer effects" that occur during K-12 education; others are "social productivity effects of higher average education in the labor market" that occur during individuals' working lives. I consider the spillover effects in the labor market during participants' working careers in a later section; here I consider peer effects during K-12 education.

"In-school peer effects" may occur if the increase in the educational achievement of participants, and their changes in behavior and motivation, increase the educational achievement

and improve the behavior of non-participants. In addition, the interactions among preschool participants in school may have peer effects on each other that may further improve participants' educational achievement, behavior, and motivation. The economic development effects of such peer effects occur if these improvements in peer educational achievement and behavior and motivation result in later increases in educational attainment, as well as effects on employability that go beyond what are measured by improved educational attainment.

Peer effects in school may occur by students learning from others, or serving as role models for others and influencing their behavior. Peer effects may also occur by students affecting the classroom atmosphere, for example, the degree to which learning is interrupted by disruptive students. Peer effects may also occur if higher average student achievement levels allows teachers to increase the level at which instruction occurs (the mechanisms for peer effects are further discussed by Hoxby 2000, and Hanushek et al. 2003, and these discussions have influenced some of my ideas on peer effects).

Both Hanushek et al. (2003) and Hoxby (2000) have found evidence of significant peer effects on test score gains in grades 3–6. These peer effects seem to be at least as strong as 0.15, which means that, holding constant the characteristics of an individual student, an increase of 1% in prior average achievement levels in a class is associated with an individual student having test score gains of 0.15 of 1% more during a school year. A equivalent way of expressing the magnitude of these peer effects is that if an individual student increases his or her achievement level by 1%, the aggregate gain in test scores for his or her class will be 0.15% greater during the next school year. An individual student with a higher achievement level of 1% greater increases average prior student achievement levels by $1\%/N$, where N is class size, and average student achievement gains during the next school year will then go up by $0.15\%/N$ during the next year.

Aggregating over all N students in the class, total test score gains go up by the N students \times the average gain of $0.15\%/N$, or by 0.15% . Whatever beneficial effects higher average test scores have on later employment and earnings will increase due to peer effects during that single year by a factor of 1.15.

I assume that in fact such peer effects on test scores are reflected proportionately in later employment and earnings. There is no direct evidence for this assumption, but it seems reasonable, and certainly more reasonable than ignoring possible implications of in-school peer effects for later employment and earnings.

A tough issue is what to assume about the overall magnitude of peer effects from the evidence of Hanushek et al. and Hoxby for peer effects during a particular school year. The effects estimated by these researchers occur during a typical school year from grades 3 to 6. If we assume peer effects accumulate across years for these 4 years, then we get a cumulative peer effect of 0.60. Of course, in the real world peer effects may diminish beyond a certain point, so that the cumulative peer effect of these four years alone may be less than 0.6. In addition, it is possible that peer effects on test score gains may be greater than peer effects on the non-cognitive behavioral and motivational characteristics that may be at least as important in determining future educational attainment and employability. On the other hand, peer effects may occur throughout a student's K-12 experience, not just in grades 3–6. Overall, I think it is a conservative assumption to assume that the ultimate peer effect of preschool education on later employment and earnings is at least 0.6.

Because not all preschool participants survive to enter elementary school, and not all preschool participants stay in the same state, I adjust the 0.6 peer effects by a factor of 0.904 to

reflect both mortality and out-migration, yielding a final peer effect of 0.54.²¹ The employment and earnings effects of preschool that occur directly for participants are then multiplied by 0.54 to yield estimates of effects on the eventual employment and earnings of peers. Total effects of preschool on the employment and earnings of both participants and peers are then 1.54 times the originally calculated figures.

To help clarify the time pattern by which labor market effects occur for preschool participants, I first examine the effects of a one-time expenditure of \$1 billion for preschool programs. This analysis includes both direct effects on participants and peer effects.

Table 13 and Figure 5 show the job creation effects over time for state residents through effects on preschool participants (and their peers) of a one-year expenditure of \$1 billion for a universal preschool program in a hypothetical state. For comparison, the table and figures also show the job creation effects for state residents of devoting the same present value of resources to a one-time program of economic development subsidies. As can be seen in the figure and tables, and as one would expect, economic development subsidies create considerably more jobs for local residents than preschool programs create for their participants and peers during the years that these preschool participants are growing up to working age. However, as early as when preschool participants are 17-years-olds, preschool programs create more jobs in a state for preschool participants and their peers than the remaining job creation effects of a one-time economic development program. Preschool programs' job creation effects continue to increase as the preschool cohort ages and attains additional educational credentials and enters peak job-holding years. In contrast, economic development programs from their beginning affect adults at

²¹0.904 = average for ages 8, 9, 10, and 11 of the product of black survival rate from age 4 to ages 8, 9, 10, and 11, which is close to one at 99.8% to 99.9%, and the percentage staying in the state, which declines from 91.7%

all ages, and their effects—as these adults age, out-migrate, and die—have nowhere to go but down. Of course, eventually the job creation effects of a one-time universal preschool program for one cohort also depreciate as this cohort ages, out-migrates, and dies.

Table 14 and Figure 6 show the real earnings creation over time for state residents through effects on preschool participants (and their peers) of a one-year expenditure of \$1 billion for a universal preschool program in our hypothetical state. The effects of an economic development subsidy program are shown for comparison. Although the time pattern of earnings effects is similar to the time pattern of job effects, the annual real earnings effects of preschool do not exceed the annual real earnings effects of economic development programs until the preschool cohort is 24 years old.

Why does economic development do somewhat better relative to preschool when using a real earnings metric rather than a jobs metric? For two reasons. First, as shown in Figure 7, a higher percentage of earnings effects are due to effects on employment rates, versus increased wage rates for individuals, for preschool programs than for economic development programs. As discussed previously, the evidence suggests that preschool programs produce a large employment rate boost for participants, above what would be expected based on educational attainment, while the effects on wage rates via higher educational attainment are more modest. In most years, over 70% of the real earnings effects of preschool are due to effects in increasing employment rates. In contrast, close to half the earnings effects of economic development programs are due to individuals moving up to higher-paying occupations. These higher wage effects of economic development programs reflect the considerable job churning as numerous job vacancies open up due to the new jobs created by economic development; estimates suggest

that for every job created in a local area, in many cases at least 3.5 vacancies are created (Persky et al. 2004). In contrast, the job-upgrading opportunities from preschool are more limited to those participating in the programs, with some limited spillover effects on peers.

Second, as shown in Figure 8, the average real wages of jobs created via economic development programs are considerably higher for many years than the average real wages of jobs generated by preschool programs.²² In the simulation, the average real wages of jobs created by economic development programs are assumed to equal the average real wage in the current economy, and then to evolve based on the changing age distribution of the individuals who initially benefited from this job growth shock (assume to be initially a random cross section of all ages, but this cohort then ages over time), as well as growing 1.2% per year with the economy-wide growth in real wages. In contrast, the real wages of jobs generated by preschool programs are initially quite low. This is in part because the preschool participant cohort is initially quite young, and I use actual figures from the Current Population Survey on wages at different ages. Of course, as this preschool participant cohort ages, their wages increase, and eventually catch up and surpass the wages of the older individuals (on average) who are affected by the new jobs attracted by traditional economic development subsidies. But this catch-up takes some time to occur. In addition, even with the preschool program, the educational attainment of preschool participants is below the population-wide average, which further depresses their wages. Although the nature of the two programs means that preschool programs are assumed, for some time and on average, to generate lower wage jobs than economic development programs, in large part this is a function of which groups are gaining jobs due to

²²This statement refers to average wages. In the real world, there is a range of wages of every group, and many individuals in the preschool group will probably earn good wages at good jobs.

the program. Preschool programs' benefits, even with a universal program, are targeted on a disadvantaged group that tends to get low wages, and economic development programs' benefits are typically not strongly targeted on the disadvantaged. There is research, which is ignored for simplicity in these economic development simulations, that suggests that market forces tend to make the effects of economic development modestly progressive, but the research suggests that this progressivity is far short of what can be achieved by social welfare programs that explicitly target a disadvantaged group (Bartik 1994).

But all these are the effects of a one-year universal preschool program, and we want to know the effects of a permanent universal preschool program. In our hypothetical state, this program would cost \$1 billion in its first year and serve 70% (236,183 participants) of all 4-year-olds. In this model economy, population grows by 0.3% per year, so the number of preschool participants grows by that rate as well. Real wages grow by 1.2% per year, and the cost of the universal preschool program per preschool participant is projected to grow by 1.2% annually as well, meaning that total expenditures on the universal preschool program grow by about 1.5% per year. Because each successive cohort of 4-year-olds is 0.3% greater per year, the number of jobs created for preschool participants and their peers in each cohort also grows by 0.3% per year. Furthermore, because the wages of each succeeding cohort grow by 1.2% per year, the real earnings effect of preschool on each cohort grows by about 1.5% per year. Therefore, the costs and benefits associated with each succeeding cohort grow proportionately.

Table 15 and Figure 9 show the job creation effects of this permanent universal preschool program, compared to a permanent economic development subsidy program with the same annual present value of costs. For a permanent universal program with an initial cost of \$1 billion per year, the number of jobs created is zero for the first 12 years, about 46,000 after 25

years, and about 120,000 jobs after 50 years. These figures could be scaled up or down to a preschool program of any size, assuming the same program design and modeling assumptions as are used in this report. The table and figure also show the job creation effects as a percentage of total employment without the program, a calculation that would be valid for a universal preschool program of this design in a typical state of any size. As the table shows, after creating no jobs for 12 years, the program creates jobs equal to 0.45% of total employment after 25 years, and 1.09% after 50 years, before finally slowing down and topping off after about 65–75 years at 1.29% of total employment. This is about twice the long-run equilibrium level of job creation for state residents of traditional economic development subsidies.

Table 16 and Figure 10 show the real earnings effects of this permanent universal preschool program, compared to a permanent economic development subsidy program with the same annual present value of costs. For this program with initial costs of \$ 1 billion per year, the cumulative increase in annual real earnings for state residents is zero for the first 12 years, \$1.5 billion per year after 25 years, and \$7.7 billion per year after 50 years. The real earnings effects eventually increase with the real earnings growth in this model economy, which is 1.5% annually. These real earnings effects could be scaled up or down for a preschool program of any size, assuming the same program design. The real earnings effects of a universal preschool program as a percentage of baseline state earnings are zero for the first 12 years, a 0.30% boost in real earnings after 25 years, and a 1.10% boost after 50 years, before the increase eventually tops out at 1.34% after 70 or 75 years. This long-run permanent boost to real earnings but slightly exceeds the boost to jobs from preschool programs of 1.29%, and is modestly greater than the long-run boost to real earnings from a permanent economic development program with the same present value, which would be 1.15%.

It is apparent that although a permanent universal preschool program has larger long-run effects than traditional economic development programs on both job creation and real earnings creation, the advantage is less for real earnings than for jobs. As mentioned before, this occurs because a greater percentage of preschool's effects on earnings are due to effects on employment rates, not increased wage rates, compared to traditional economic development programs. In addition, many of the jobs created by preschool programs are low-wage jobs, particularly when participants are in the early part of their career, given the still relatively low educational attainment of program participants. In addition, it may be initially somewhat surprising that the long-run percentage effects of a permanent universal preschool program on jobs and real earnings are quite similar, even though I estimate that in the long run, about one-quarter of the real earnings increase due to a preschool program is due to increases in wages. Although from an individual perspective, preschool programs only increase wage rates, by increasing educational attainment for program participants, from an aggregate state perspective, preschool programs have a counterpoising tendency to reduce average wage rates by increasing employment rates for persons with low educational attainment. This reducing effect on average state wages does not harm any individual worker, but does mean that on net, even with increased wages for program participants, a universal preschool program has little net effect on average economy-wide real wages, so percentage effects on employment and real earnings are similar.

Another trend that is apparent from perusing these figures for a permanent preschool program, versus a one-time program, is that it takes more years in the permanent case for preschool's effects on jobs and real earnings to exceed that of economic development programs. For a one-time program for one cohort, the effects of preschool on jobs exceeds that of economic development programs after 13 years (when the original cohort is 17), whereas for

permanent programs, the effects of preschool on job creation exceeds that of economic development programs only after 28 years. For real earnings, for one-time programs, the effects of preschool on real earnings exceeds that of economic development programs after 20 years, whereas for permanent programs, the effects of preschool on real earnings generation exceeds that of economic development programs after 52 years.

Why does this difference occur? The one-time program's "long-term" shows effects for one cohort. A permanent program's effects at any point in time are a mixture of long-term, medium term, and short-term effects from different cohorts. Economic development programs have an advantage over preschool programs, at least insofar as we focus on effects on the participants (not their parents or the program's employees), of having much greater short-run effects on employment and earnings, whereas preschool programs have essentially no employment and earnings effects via participants for at least 12 years. For permanent programs, these short-run advantages of economic development programs for a single cohort are averaged into the long-run effects of the permanent programs along with the long-run advantages of preschool programs for a single cohort. John Maynard Keynes famously said that "in the long run we are all dead," as an argument to encourage economists and policymakers to think more about how to deal with the short-run. It is perhaps more accurate to say that for any permanent policy, we have to pay attention to its short-run and long-run consequences for specific individuals, as its long-run effects on the overall population are a mixture of both short-run and long-run forces for specific individuals.

Finally, with these estimated effects of a permanent universal program on real earnings, I can calculate the discounted present value, at a 3% discount rate, of the effects on real earnings per one dollar of present value of costs. The real earnings effects on participants and peers

amount to \$2.65. For comparison, the present value of the real earnings effects of traditional economic development subsidies per present value dollar of costs are \$3.14.

Effects of Group Education on Labor Productivity

In addition to spillover effects of preschool participants that occur due to peer effects during K-12 education, there may be spillover effects that occur in the labor market. There may be effects on productivity and wages that occur in the labor market beyond the increase in productivity and wages that occurs for the individual. In other words, the productivity, employability, and wage rates of an individual may depend not only on their educational attainment level, but also on the average educational attainment of others in the labor market.

Labor productivity levels or productivity growth rates may depend on average educational attainment for several reasons. The productivity level of an individual may depend on others' educational attainment if workers learn from each other about how to be more productive. In addition, if the average worker is more productive, employers may find it easier to hire and retain skilled workers, and as a result may find it more feasible to use more advanced technologies, more capital intensive production techniques, and forms of workplace organization that have higher productivity levels. In addition, productivity growth rates may depend on average educational attainment if workers get ideas from each other not only about current technologies and production techniques, but about how to improve such technologies and techniques more rapidly. Productivity growth rates may also increase with average educational

attainment if employers find they can more rapidly introduce new capital, technologies and workplace organization systems with a workforce with higher average education.²³

Assuming that higher productivity is translated into higher wages, in either case we would expect that an individual's wages, holding their individual's characteristics constant, including holding the individual's education constant, would be positively affected by the average educational level of the local workforce. We might also expect that if local wages do not instantly adjust upwards to reflect higher overall productivity, then an increase in average educational levels would tend to encourage local employment growth.

On the other hand, the observed gains to wages of individuals from education may overstate the true productivity effects of education to the extent to which education does not truly improve productivity, but merely serves as a signaling device for productivity differentials that are not altered by small marginal differences in education. An individual who gets a little more education than average could receive more wages because employers infer from the greater education that the individual is more productive than average, even if that small increase in the individual's education does not truly affect his or her productivity. But if everyone gets more education, then productivity may not increase, and in fact everyone will have to go to school more to get the same wages as before. In this case, increases in the average education levels of others will tend to decrease an individual's wages, holding individual characteristics constant.

The whole issue of the social productivity effects of education—what society gains in economic output from higher average educational levels—and how it differs from the individual productivity effects of education, what an individual gains from an increase in their own

²³Some of my ideas expressed here of possible reasons for a social return to education in the labor market are influenced by writings on this subject by Moretti (2003) and Glaeser and Saiz (2003).

education, is a controversial one. As of right now, the available empirical evidence seems to support some “extra” positive social productivity effects of education, beyond the effects on the individual, although the amount is in dispute. Average educational levels in a metropolitan area appears to have some causal effects on higher real wages in the metropolitan area (beyond what would be expected based on the effects of each individual’s education on wages) and on higher growth in the metropolitan area (Moretti 2003, 2004; Glaeser and Saiz 2003). These positive effects of average educational attainment seem to be mainly associated with the percentage of college graduates in the labor market, not the percentage of high school graduates (Moretti 2003, 2004; Acemoglu and Angrist 2000; Glaeser and Saiz, 2003). Whether a higher percentage of college graduates have more effects through affecting productivity levels or productivity growth is harder to sort out, as either mechanism could result in some estimated effects of increases in average local educational levels on wages and growth. For the purposes of this report, I adopt the more conservative assumption that the percentage level of college graduates in a state has effects on an individual’s labor productivity level, rather than the more radical assumption that this affects productivity growth rates. With the power of compounding increased growth rates over time, assuming effects on productivity growth rates could result in huge measured earnings effects as we follow a state economy into the long-term future, and there is no definitive evidence of such productivity growth effects.

In simulating how preschool education’s effects on average graduation rates might affect a state’s overall wages and earnings, we must think carefully about how a presumed effect of the college graduation percentage on productivity is likely to affect a state economy. The fact that productivity goes up has immediate effects in increasing business profitability, but need not immediately increase wages. Wages will not increase until employers are attracted by the higher

labor productivity environment, thereby increasing labor demand relative to labor supply. This will drive up local employment rates and allow individuals to move up to higher paying occupations, in other words, this is a labor demand shock similar to that brought about by economic development programs. As individuals migrate into the state, attracted by the greater employment opportunities, the initial effects on employment rates are moderated somewhat, and housing prices and other local prices will increase. However, some of the original local residents who gained greater employment experience or moved up into higher paying jobs will be able to use their greater labor market experience, better reputation with employers, and greater motivation, to keep their new jobs. As these individuals age, move out of the state, and die, these persistent effects on local employment rates and wage rates gradually die out. In the very long run (after 50 years or so), the effects of the shock to the productivity level on local real wages and employment rates die out. Local prices and nominal wages will have risen enough with in-migration to just offset the profitability effects of the higher productivity level. The local employment level is permanently higher, as is local productivity, but local prices and nominal wages will have risen enough with in-migration to just offset the profitability effects of the higher productivity level.²⁴

The available estimates of how the percentage of college graduates affects wages and growth are not completely consistent. Moretti's (2004) estimates imply that a 1-point increase in

²⁴I do not think this issue has been adequately attended to in the research literature on how average educational levels affect real wages and employment growth. We need to think carefully of how with free migration we would ever observe effects of higher local educational levels on local real wages, as individuals from other areas can migrate in and get the benefits of the social productivity effects of education. My suggestion is that there is a very long-run adjustment to this new equilibrium. Both financial and psychological moving costs prevent instant adjustment to the new equilibrium. The lack of instant adjustment allows the original residents of the local area to gain new and better jobs because of the jobs attracted by the higher social productivity. These initial advantages allow the original residents to gain some permanent advantages in their human capital. As a result, the adjustment to

the percentage of college graduates in a local area is causally associated with nominal wages that are 0.6 to 1.2% greater. If all of these higher wages were due to the medium-run effects of a shock to labor demand, this would imply that a 1-percentage point increase in college graduates increases the employment level by perhaps 1.5% to 3%.²⁵ On the other hand, Glaeser and Saiz (2003) directly estimate that a 1% increase in the percentage of college graduates increases the 10-year population growth rate by perhaps 0.5%. In general, we expect a demand shock to the local economy to increase local employment in the long run by somewhat more than local population (in some models, by roughly a factor of 5 to 4, see Bartik 1993). In addition, if there is some lagged adjustment of employment, the 10-year effect on growth will be below the long-run effect. Under some plausible assumptions from the research on how local business activity gradually adjust to shocks, Glaeser and Saiz's results imply that in the long-run, a 1-point increase in the local area's percent of college graduates will increase the long-run equilibrium local employment level by 1%.²⁶

To be conservative, I use the lower estimate that a 1-point increase in the college graduate percentage will increase the long-run employment level by 1%. However, this growth shock will only occur gradually. Results from Helms's (1985) influential paper on the determinants of state employment levels suggests that state employment adjusts to its long-run

the new long-run equilibrium takes a very long time. In the interim, we can observe effects of average local educational levels on real wages and growth.

²⁵Bartik's (1991) findings suggest that a shock to local employment growth affects nominal wages about twice as much as real wages, and that a 1% growth shock increases nominal wages by 0.4% and real wages by about 0.2%. Dividing Moretti's estimated effects of 1% more college graduates on nominal wages by 0.4% yields the estimated effects on desired employment growth that are cited in the text.

²⁶Glaeser and Saiz's results are consistent with an effect of a one point increase in college graduates on the 10-year growth of local employment of perhaps 0.6% (= their effect of 0.5% × the ratio of employment to population growth of 5 to 4). Helms's (1985) widely cited paper on the determinants of state employment levels implies that state employment adjusts to its long-run equilibrium level by 8.9% per year. The long-run equilibrium effect of any shock to local employment will be related to the effect after 10 years by the following equation: Long-run effect =

equilibrium level by 8.9% per year, and I use this figure in the simulations. I simulate an increase in the percentage of college graduates as affecting the long-run equilibrium employment level, and then the actual employment level will gradually adjust to this higher equilibrium employment level. These growth shocks then have effects on local employment rates, wage rates and earnings similar to those of an economic development program, or indeed, of any factor that makes an area more attractive for expanding labor demand.

Part of this calculation involves simulating how a permanent universal preschool program will affect the percentage of college graduates in a state's economy. I simulate this using the information on how the college graduation proportion of preschool participants varies with age, and how this would be expected to alter the college graduation proportion as successive cohorts that have participated in universal preschool enter the labor market. This calculation of course allows for outmigration, mortality rates, and population growth, as well as for nonparticipants in universal preschool. In addition, based on research by Bound et al. (2004), it appears that there is some displacement of college graduates from a state's labor market. Bound et al. estimate that the number of college graduates in a state's labor market is on average equal to only 0.30 of the flow of college graduates in the state from 5 to 24 years previously. This proportion is significantly less than the proportion one would expect simply on the basis of the outmigration of preschool participants; based on the migration figures used earlier in the paper, of the 21-year-old college graduates in a state, we would expect 83% to still be in the state five years later, declining to 74% still in the state 24 years later, and averaging 78% over all

(10-year effect) / (1 - (0.9104¹⁰)). Applying this equation to a 10-year effect of 0.6% suggests that the long run effect is around 1%.

these different time periods considered in the Bound et al. estimates.²⁷ Presumably, there is some differential in- and out-migration of college graduates due to effects of the flow of college graduates on the employment prospects of college graduates. I multiply the projected proportions of college graduates at different ages by (0.30/0.78) to simulate the actual expected change in college graduates in the labor market.

The resulting calculation is shown in Table 17. This table shows how the percentage comprised by college graduates in a state's economy would be expected to evolve over the first 75 years after a universal preschool program is instituted. As the table shows, the effects of universal preschool education on the proportion of college graduates is quite modest. This is because, first of all, the effect of universal preschool on college graduation rates even for the high risk groups that would otherwise have had no preschool is modest, only 3%, and takes some time to occur, with much of the effect occurring when former preschool participants are in their late 20s and 30s. Second, the effects are reduced by out-migration of college graduates over time. Third, the presumed effects on the overall preschool population is much less than the effects on the high-risk group that benefits the most from preschool, and under the assumptions used here the effects on college graduation for the average preschool participant in universal preschool are only 23% of the effects for the highest-risk group. Fourth, only 70% of any given cohort participates in universal preschool. Finally, the Bound results suggest that some additional differential in-migration and out-migration of college graduates will further dilute

²⁷This is based on using the PSID-corrected PUMS data on percentage in the same state as at age 4 for age 21, compared to the same statistic for later years. This is not exactly the migration data we want, but it may approximate the pattern of college graduates leaving the state. In any event, if I am underestimating how many college graduates move out of state, the use of the Bound et al. figures should correct for that underestimation as well as for any displacement.

effects on the state's college graduation percentage. As a result, one would expect any economic development benefits from extra social productivity effects of preschool to be modest.

These modest effects are found when I estimate preschool's effects on job creation and real earnings growth through the "social productivity" return from increasing the average college graduation percentage in the state economy. As Table 18 and Figure 11 shows, a permanent universal preschool program in our hypothetical state, which spends \$1 billion and then expands in costs by 1.5% per year forever, takes a long time to have much in the way of job creation effects via the social productivity effects of education. Eventually the social productivity effects max out in total job creation effects at only a little more than 2,000 jobs after 70 years. The increase in college graduates does eventually increase the desired employment level by a little more than 10,000 jobs after 70 years. This increase in employment takes place through a series of small annual "demand shocks" to the number of jobs in the state, typically about 200 or 300 jobs per years. Each one of these shocks is equivalent in its effects to the effects of a demand shock brought about by economic development programs, but are much smaller: recall that devoting \$1 billion per year to economic development subsidies is thought to create over 7,000 jobs per year. The actual effects on employment rates of state residents are reduced by in-migrants getting the jobs, as well as out-migration of some state residents who originally obtained the jobs.

The effects on job creation on the employment rates of state residents of 2,000 after 70 years are minuscule compared to either traditional economic development subsidies or direct effects of preschool education on the employment of participants and their peers. After 70 years, traditional economic development programs in our hypothetical state increase the employment rates of state residents by over 71,000 jobs. After 70 years, the direct effects of preschool

programs on participants and their peers are over 150,000 jobs. These minuscule effects via the social productivity effects of education are mostly attributable to preschool education not having a huge aggregate effect on the percentage of college graduates in the state. Similar small “social productivity effects” are found on real earnings, and when the job or earnings effects are expressed as a percentage of the state economy.

Finally, we can calculate the present value of the real earnings associated with the social productivity effects of preschool, per dollar of present value costs of preschool. The present value of these “social productivity” real earnings effects amount to only \$0.04 per dollar of costs.

Adding Up the Effects

The total effects of universal preschool education on job creation and real earnings generation for state residents of course reflect the sum of all the effects I have discussed: balanced budget multiplier effects, child care effects on the labor supply of parents, effects on participants’ and their peers’ employability and real wage rates, and effects on the state economy’s productivity. Table 19 shows the sum of all these preschool effects, with economic development effects for comparison, for each year of the 75 years after the initial cohort of preschool participants is 4 years old, for job creation and real earnings generation, both for the hypothetical state and as a percentage of a state’s baseline employment and real earnings.

I could also produce figures showing the sum of all preschool effects compared to economic development effects. However, on the scale that must necessarily be used for such figures, such figures would be almost indistinguishable from the figures that previously showed effects on participants and their peers. Such effects are by far the overwhelming proportion of

preschool's total job creation and earnings generation effects. In terms of present value, for each one dollar of present value resources devoted to preschool, the total effect of preschool is to generate real earnings whose present value is \$2.78, of which \$2.65 is due to effects on participants and their peers.

The one significant change from adding other preschool effects to participant and peer effects is that preschool has immediate effects on job creation and earnings creation; during the year the original cohort is 4 years old that slightly exceed the effects of traditional economic development subsidies. However, the economic development program's effects by even the next year begin to exceed the effects of preschool, and this advantage continues to expand until the original cohort is in its late teens, and in real earnings until the original cohort is in its early 20s. At that point, preschool education programs begin to catch up to economic development programs. In the long run, preschool's effects are significantly greater than economic development programs for both job creation and real earnings generation.

Comparing Preschool with Traditional Economic Development Programs

One bottom-line interpretation of these simulations is that universal preschool yields real earnings effects whose present value is \$2.78 per dollar of resources used, whereas traditional economic development programs yield real earnings effects whose present value is \$3.14 per dollar of resources used. Given the many uncertainties and policy variables involved in these calculations, it is probably unwise to focus too much on the modest difference between these two present values of real earnings effects. A more appropriate interpretation is that both preschool and economic development yield real earnings effects whose present value is of similar magnitude.

Is there some underlying reason for this similarity in present value effects of preschool and traditional economic development programs? Or is this similarity just a coincidence, just as it is a strange coincidence that the sun and moon happen to have almost exactly the same apparent size as viewed from earth? Although the similar present value earnings effects of preschool and traditional economic development programs are in some sense just a coincidence, there is some reason to think there will be some rough similarity in successful but politically disputed programs that seek through investments today to have permanent effects in generating economic activity in a state. Such programs will always seek to leverage some investment, whether in the skills of preschoolers, or in enticing firms to locate or expand, that will yield permanent effects on employment and earnings. If such programs are successful, a modest investment will have significant long-run earnings effects, whose magnitude in many cases will have a larger present value than the investment. On the other hand, if such a program had real earnings effects whose present value was 100 times of the cost of the program, it is doubtful that there would be any discussion of whether the program was successful, or any demand for analyzing the economic development effects of the program. The program would simply be accepted as a necessary part of what government does.

Although these present value calculations in some sense reflect the average social valuation of effects at different time periods, policymakers and the public may also care about the details of the dynamic effects of traditional economic development programs and preschool education, not just the bottom-line present value figure. Consider the typical performance expectations for an economic developer. If a job applicant for an economic developer position told his or her various constituency groups that only small results could be expected for the first 15 years, the probability of being hired would be small. Although the rhetoric of traditional

economic development programs is that economic development is a long-term endeavor, most economic development constituency groups expect some substantial results within a 5 to 10 year period. A universal preschool education program would not meet the need for job creation and earnings generation within that time frame.

This is not only my judgment. I mentioned this project to a researcher I know who frequently works closely with economic developers and their organizations. His comments were as follows: “The timing issue is key. Economic development efforts are directed toward today’s problems—high unemployment, poor jobs and blighted areas. It would be a tough sell to any economic development organization that its resources should be used to help improve the state’s workforce 15 to 20 years from now.”

On the other hand, an economic developer might be very pleased that 15 or more years ago, some previous policymakers in the state or metropolitan area had instituted a universal preschool education program. This program would help provide more employable and better educated workers who would help attract jobs to the area, thereby making it more likely that the economic developer can claim success.

Therefore, from the perspective of achieving economic development goals, it is probably most appropriate to view traditional economic development programs and universal preschool education as complements. Both can have significant effects in increasing job creation and real earnings generation in a state. Traditional economic development programs have the advantage in generating jobs and earnings over a 5 to 15 year time horizon. Universal preschool education has the advantage of generating jobs and earnings over a 30-year or more time horizon. Because both time horizons are relevant to policymakers, various interest groups, and the public in general, both types of programs are needed. This is true even if there is no direct interaction

between the programs in which the presence or success of one influences the success of the other. In Chapter 4, I will consider alternative estimates of the effects of preschool and traditional economic development subsidies that allow for the possibility of such interactions.

State-by-State Effects

I now turn from effects in a hypothetical, typical state to effects in each actual state. In each state, I estimate effects of universal preschool education on a variety of economic development metrics, including jobs, earnings, gross state product, and state and local tax revenue. These estimates provide a variety of ways to understand the magnitude of the economic development effects of preschool.

The magnitude of preschool's effects on economic development in each state depends greatly on out-migration from the state. Preschool education's costs are incurred at age 4, whereas most of the economic development effects do not begin until at least age 16. This lapse of time allows considerable out-migration of preschool participants from the state. In contrast, much of the effects of traditional economic development programs occur almost immediately.

For example, simulations suggest that the ratio of the present value of earnings effects of universal preschool to its costs will be 3.79 if a state had no out-migration, whereas this ratio is 2.78 for a state with a typical out-migration rate. For such a typical state, out-migration reduces the earnings benefits for the state's residents by 27% ($= (3.79 - 2.78) / 3.79$).²⁸

The prevalence of out-migration varies significantly across the states. As shown in Table 20, of U.S. residents born in a particular state, on average 68% were living in their birth state in

²⁸This leakage of benefits to out-migration suggest that the national effects of preschool education will be greater than the effects perceived by each individual state. This suggestion is explored in Chapter 5.

the year 2000. This percentage is much lower for smaller states with less diverse economies. For example, this percentage is 43% for Wyoming, and 45% for North Dakota. In contrast, this percentage is much larger for larger, faster growing states. Eighty percent of those born in Texas are still living in the state as of 2000, whereas this percentage for California is 77%.

At the extreme, out-migration is extremely large for a city, particularly smaller cities and cities with low economic growth. For example, of those born in the District of Columbia, only 17% were still living in DC in 2000. Because of this high out-migration, most cities are unlikely to find that investments in preschool education lead to significantly more jobs and earnings later on for city residents.

The incremental costs of a universal preschool program in each state are estimated by assuming that a universal preschool program will have net costs of 0.299% of the state's wage and salary earnings. Although this is a rough estimate that does not directly take into account many factors affecting the cost of universal preschool programs, for example the size of the preschool population, this rough estimate is surprisingly close to state-by-state estimates that do take into account more detailed data. Table 21 includes a column giving rough estimates of the state-by-state costs of moving to a universal preschool program. The California estimate of \$1,904 million in Table 21 is quite close to the Karoly and Bigelow estimate using more detailed California data of \$1,716 (in 2004 dollars). Table 21 includes estimated costs of universal preschool for Massachusetts, Wisconsin, and Ohio of \$436 million, \$270 million, and \$548 million, respectively; Belfield (2005), using more detailed data for these three states, estimated incremental costs of a universal preschool program of \$593 million, \$213 million, and \$496 million.

For each state, Table 21 presents estimates, for the assumed size of the preschool program in the state, and the state's tendency for out-migration, of the long-run effects of universal preschool programs on jobs, earnings, gross state product, and state and local tax revenue. As the table shows, these estimated effects are clearly large enough to be politically significant in the states. Long-run job creation effects are often in the tens of thousands in a state, and universal preschool education is usually estimated to increase earnings, gross state product, and state and local tax revenue by hundreds of millions of dollars per year. The long-run increase in the flow of state and local tax revenue is frequently close to the long-run annual costs of the preschool education program.

Chapter 4

ALTERNATIVE ESTIMATES

Given the many assumptions made in creating these complex simulation models of the effects of economic development programs and preschool education on state job creation and real earnings generation, it is possible to imagine almost an infinite number of variant simulations that tweak one or more assumptions. I focus here on seven alternative approaches to doing the simulations that tweak assumptions relevant to important policy issues relevant to these programs. Three alternative types of simulations explore policy options relevant to economic development programs: (1) subsidizing non-export base businesses; (2) making incentives more up-front and in-kind; (3) targeting jobs more to current residents vs. in-migrants. Four alternative types of simulations explore policy options relevant to preschool education programs: (1) the magnitude of incremental benefits of universal preschool for participants who otherwise would have been in some type of preschool; (2) the optimal quality and expenditure per participant of preschool programs; (3) targeting preschool education more at at-risk children; (4) lessening displacement effects of preschool education.

Subsidizing Non-Export-Base Businesses

The economic development simulations assumed the subsidized businesses were export-base businesses, in which 100% of the sales of the businesses represented income that otherwise would not have been received by residents of the state. Furthermore, I assumed that this business paid sufficiently high wages and had sufficiently strong local supplier links that we could reasonably assume a multiplier effect of 1.8 for jobs directly created.

Focusing on export-base businesses with reasonably large multipliers is conventional wisdom in the economic development profession. However, this focus is not always successfully practiced, in at least two major types of economic development subsidies: subsidies for sports teams; subsidies for retail businesses.

One significant type of economic development subsidy is a subsidy for sports teams and sports stadiums. The average subsidy from a host city to a typical major league sports team now exceeds \$10 million per year (Noll and Zimbalist 1997, p. 494). In the case of sports stadiums, only a portion of the revenue gained is truly new outside the state revenue for local employees, for several reasons. First, many of the fans attending the games are local residents who would have otherwise spent their money on some other in-state activity. Second, much of the revenue from sports goes to athletes who largely spend their money outside the state. Counterbalancing this is that to some extent the sports stadiums may attract outside visitors who otherwise would not have visited, and who spend money in the state on activities other than the sports itself. However, careful analyses suggests that this additional spending on non-sports activities by visitors does not come close to offsetting the fact that much of the operation of stadiums displaces local fan spending from other activities, and that an unusually high proportion of the revenue from these sports activities immediately flows outside the state (Blair and Swindell 1997).

In addition, most visitor spending supports relatively low-wage employees working concessions or working in local hotels. The resulting multiplier effects are likely to be modest.

Another significant type of economic development subsidy is subsidizing retail businesses that largely sell to a local market. Such subsidies are more common in the 31 states in which local governments are to some extent financed by local sales taxes (Cline and Neubig

1999). Local governments in these states often engage in incentive wars to attract retailers, in order to raise their local sales tax base. For example, I know from personal conversations with economic developers and local news media that such incentive wars over retail businesses are common in Alabama and Arizona.

Even for “big box” retailers or specialty retailers that offer some unique product or pricing, most of the new sales for such subsidized retailers will come from state residents who would otherwise have spent money at some other retailer in the state. Therefore, a sizable portion of the new jobs at the subsidized retailers are offset by displacement of state jobs at other state retailers who lose sales.

Furthermore, the new jobs at retailers frequently are relatively low pay. This reduces the size of the multiplier effect of any new retail jobs that are directly created due to attracting out-of-state spending, as the new employees’ increased expenditures on state consumer goods are necessarily modest.

To simulate how these changes in assumptions about the export base and the multiplier can alter the effectiveness of economic development subsidies, I consider the effects of economic development subsidies that are identical to what was previously assumed, except that only 30% of the subsidized jobs are assumed to be truly new activity in the state. The other 70% of the subsidized jobs are assumed to displace other spending and jobs that would have taken place in the state. This assumption about outside spending has some intuitive plausibility, and also seems to be roughly consistent with some of the analyses of sports stadiums.²⁹ In addition, I assume a lower multiplier of 1.5.

²⁹For example, in the study of Cincinnati sports stadiums by Blair and Swindell (1997), about 50% of the fans attending Reds and Bengals games come from outside the Cincinnati area. But 20% of those fans said they

Under these assumptions, economic development subsidies to retailers or sports teams will only generate \$0.79 in present value earnings per dollar of costs. Under the baseline assumptions, economic development subsidies to high-paying export-base firms generated \$3.14 in present value earnings per dollar of costs. The 30% export-base assumption means that even if the new business activity is induced by the subsidy, only 30% of this new activity is truly net new activity to the state. As a result, the effectiveness of the subsidy is decreased by 70%. The lower multiplier of 1.5 vs. 1.8 further decreases the effectiveness of the subsidy in creating jobs by another 17% ($17\% = (1.8 - 1.5) / 1.8$).

Up-front and In-kind Incentives

The estimated effectiveness of the economic development subsidies in the baseline simulation relies on the assumption that the economic development subsidies are paid out over a 10-year period. This reflects customary practice. Economic development subsidies are paid out over a number of years in part because this allows some costs to be postponed, which is politically preferable. In addition, this allows costs to be credited against tax payments rather than paid out explicitly in cash. Finally, postponing some payments lessens potential problems caused by subsidized firms leaving after a few years. If a firm leaves, state and local economic developers would like if possible to “claw back” all or part of the subsidy given to induce the location decision. This can be done through legal clawback agreements, but such agreements

would have visited the area anyway, so not all this spending can be counted. In addition, for the 50% of the fans who are local, the effectiveness of their concessions spending in creating local jobs is probably less than their alternative uses of their spending in the local area, given the high profit margins on concessions which largely go to player salaries that are spent out of state. Furthermore, although there is some local spending by visitors outside the stadium, it appears to generally be considerably less than spending at the stadium.

can sometimes be difficult to enforce. But if part of the subsidy has only been agreed to, and not yet paid, then there is no problem in not paying the remaining subsidy if the firm leaves town.

However, we know that firms apply relatively high real discount rates to future flows of profits in making corporate decisions. Studies suggest that the real discount rate used by corporate executives in making decisions averages about 12% (Summers and Poterba 1994). If the social discount rate is 3%, this means that we can affect corporate location decisions more, at the same present value using the social discount rate, by paying a higher proportion of the economic development subsidy upfront.

Paying more of the subsidy upfront worsens potential issues of whether the subsidy can be in practice recovered if the subsidized firm leaves. One way to make upfront subsidies more recoverable is to pay more of them in the form of worker training, or infrastructure such as access road improvements and utility provision (e.g., this was suggested in Bartik 2005). This assistance is valuable to firms, but is largely recoverable if the firm leaves. If the firm leaves, the access roads and utility infrastructure will still be there, and most of the trained workers may also stay. This physical capital and human capital will help the area attract a replacement firm.

In addition, paying the subsidy in the form of services also provides an immediate boost to the economy in the form of the balanced budget multiplier. The economic stimulus of hiring workers to provide the training, or build the infrastructure, will exceed the depressing effect of the added taxes to pay for the training and infrastructure.

Resimulating the model with these upfront, in-kind subsidies results in a present value of earnings generated from economic development subsidies, per dollar of present value costs, of \$4.40, compared to \$3.14 under the baseline assumptions. Of that \$4.40, \$4.36 occurs because these up-front incentives are more cost-effective in creating jobs. The other \$0.04 is due to the

balanced budget multiplier effects of spending government money on job training and infrastructure.

Targeting Jobs to Residents

The effectiveness of economic development subsidies in helping state residents is reduced because such a significant proportion of induced jobs go to in-migrants. In the baseline simulations, I assume, based on previous studies, that in the short run, 6 out of 10 jobs go to state residents, and the others 4 to in-migrants, with 3 of the 6 jobs to state residents going to the unemployed, and the other 3 to state residents who previously were out of the labor force. The effects of job creation on state residents' unemployment are assumed to quickly fade. The effects on state labor force participation are assumed to slowly fade with out-migration and mortality of the original state residents. In addition, more jobs going to in-migrants reduces the possibilities of state residents getting promotions to better paying occupations.

As discussed before, the assumption is that the gains to in-migrants from this job creation will be minimal, as in-migrants could have otherwise moved to some other area with similar economic opportunities. Only current residents with strong ties to this state get special benefits from state job creation.

Increasing the proportion of jobs that go to state residents can have considerable effects on the jobs and earnings generated for state residents. Suppose we increased the proportion of new jobs going to state residents from 6 out of 10 to 8 out of 10. I assume that this is evenly divided between more jobs for the unemployed and more jobs for state residents who are out of the labor force. The pattern of "fading" of effects over time is assumed to be similar. But because the initial effect is that more state residents get job experience, with its attendant effects

on job skills, self-confidence, and reputation with employers, the effects on future employment rates of state residents are always higher than under the baseline assumptions. In addition, I assume that effects on occupational upgrading are blown up proportionately with the effects on labor force participation. With fewer in-migrants being hired, more job vacancies will go to already employed state residents as well as to non-employed state residents, which provides more opportunities for occupational upgrading.

Under these alternative assumptions, the present value of real earnings generated, per \$1 of present value of economic development subsidies, is \$4.19. This is one-third higher than under the baseline assumptions.

Presumably, the share of jobs that go to in-migrants depends on the availability of state residents with suitable skills for the job opportunities created by economic development programs. The in-migrant share may also depend on the ease with which employers and local job-seekers can get information about possible suitable job matches. Programs that provide carrots or sticks to encourage local residents to be hired for the jobs created by economic development programs may be helpful. For example, some local areas, such as Berkeley CA and Portland Oregon, have used “First Source” programs under which firms receiving economic development subsidies are required to consider local job-seekers who are screened and referred via the local job training and placement system.³⁰

In addition, the sheer availability of suitably skilled state residents may also increase the share of jobs that go to state residents. For example, any program that improves the employability and job skills of state residents may increase the proportion of the jobs created by

³⁰For more information on Portland’s programs, see Bartik 2001, pp. 256–258, and for more information on Berkeley’s programs see Bartik 2004b, p. 363. For more information on both, see Molina 1998.

economic development that will go to state residents. One such program could be preschool education.

Preschool education may then be complementary to economic development programs in a different sense. I argued above that preschool education and economic development programs may be complementary in that they provide jobs at different time frames, which may meet the needs of different groups. But the argument here is that better and more universal preschool education in the past may make today's economic development programs more cost-effective in creating jobs for local residents. This is a more direct complementarity.

Different Assumptions about Preschool Benefits

The baseline model for universal preschool effects in this report relies on good evidence on the benefits for high quality preschool for high-risk children who otherwise would not be in preschool. This good evidence comes from studies of the Perry Preschool program and the Chicago Child Parent Center program. On the other hand, we know less about the benefits of high-quality preschool for high-risk children who otherwise would have been in some type of preschool, or benefits for lower-risk children from moving to high-quality preschool. The baseline simulations used moderately conservative assumptions, originally made in Karoly and Bigelow (2005), and detailed in Table 7, about how effects for these other groups compared to effects for high-risk children who moved from no preschool to high quality preschool. These assumptions were used in this report because they appeared reasonable, and appeared likely to if anything understate the effects of universal preschool.

However, even though these assumptions are reasonable and moderately conservative, these assumptions are also arbitrary. Therefore, it seems wise to consider alternative

assumptions about how the benefits of moving to universal preschool for various groups compare with preschool's benefits for the high-risk group who otherwise would not have been in any preschool.

I consider the alternative assumptions about benefits for various groups proposed by Karoly and Bigelow (2005, Table 3.5 on p. 107). These assumptions are used because the range of assumptions is so extreme that it is likely to encompass the possible range of effects of universal preschool that we should sensibly consider.

Table 22 summarizes the results when these alternative assumptions are plugged into this report's simulation model. Across the various alternative assumptions, the ratio of PV earnings effects to costs ranges from 1.97 for the most conservative assumptions to 4.85 for the most generous.

For those readers who are interested, these alternative assumptions can easily be used to generate any of the previous tables on preschool effects. The estimates for effects for preschool participants and their peers for jobs or earnings for any particular year just need to be multiplied by the ratio of the average benefits per universal preschool participant under a particular set of assumptions, to the 23% assumed in the baseline preschool model.

Different Assumptions about Preschool Design and Benefits

The baseline assumptions used in this report for costing out the preschool program come from Karoly and Bigelow (2005). These assumptions seem a reasonable attempt to come up with a design for a preschool program that will make the program affordable for a universal preschool program, and yet be of sufficient quality to have significant effects on participants. The program modeled has some quality features in that each class is assumed to have a college-

educated teacher with an early childhood education certification, and a student to staff ratio of no more than 10 to 1, with 20 students in a class for a teacher and a teacher aide. Preschool class sizes of no more than 20 and ratios of no more than 10 students per teacher are the maximum allowed for accreditation purposes for preschool programs for 4-year-olds by the National Association for the Education of Young Children (see http://www.naeyc.org/accreditation/performance_criteria/teacher_child_ratios.html). On the other hand, the modeled program tries to control costs by having one staff position be a teacher's aide, as was done in the Chicago CPC preschool program. Costs are also controlled by assuming the program is only a half-day program. Finally, costs are controlled by assuming that results similar to the CPC program can be achieved with a program that only offers services to children for one year. The one-year program design is rationalized by findings from studies of the CPC program that the second year seemed to have limited additional benefits over the first year.

However, we should recognize limits of what we know about the benefits of preschool. We presume, but do not know for certain, that there is some threshold of quality of preschool programs below which the program has few benefits. We presume that at some much higher quality level the marginal return to additional quality diminishes. To maximize benefits relative to costs, we would like to design a preschool program that will definitely have a quality significantly above the minimum threshold for significant benefits, but not go too much above the quality at which the marginal benefits to increased quality rapidly diminish. But we don't know exactly the quality levels that define that range of desirable quality for maximizing the benefit/cost ratio for preschool programs. Given our uncertainty, we might want to err on the side of making sure we do have a significant impact, as long as we still have benefits greater than costs. In the future, as our knowledge about the implications of different quality levels

improves, we may be able to identify significant cost-savings with little impact on results. The danger of choosing too little quality for a universal preschool program is that the program's failure could discredit the concept of universal preschool.

For example, it could be argued that to be assured of getting benefits of a magnitude similar to the CPC program, we should more closely mimic some of the features of the CPC program in our design of a universal preschool program. The average participant in the CPC program participated for 1.5 years, so for many participants it was a two-year program (Reynolds et al. 2002). So, let us assume that to assure ourselves of the CPC level of benefits, we will design our universal preschool program as a 2-year program for both 3- and 4-year-olds. In addition, the student to teacher ratio in the CPC program was 17 to 2 rather than the 20 to 2 ratio assumed in the baseline simulation, following Karoly and Bigelow (2005). Therefore, let us further assume that we decide to aim for a 17 to 2 ratio in our universal preschool program. These changes provide greater assurance that we will have the CPC level of benefits that we want to claim.

Under these alternative assumptions, the annual per participant costs of a universal preschool program increase from \$5,856 under the baseline simulation to \$6,889 under this alternative simulation, due to the lower 17 to 2 student to teacher ratio. With the program lasting for two years for each participant, and allowing for offsets in reduced spending for existing programs for 3-year-olds as well as 4-year-olds, the present value of costs per participant for this universal program will be 2.70 times as much as was assumed for the baseline simulation.³¹ The

³¹The original program had an offset from reduced spending of \$1,622 per participant, for a net cost per participant of $\$4,234 = \$5,856 - \$1,622$. The new program's costs for 4-year-olds will have the same offset, so will have net costs of $5,267 = 6,889 - 1,622$. The 3rd grade program will have costs that we will assume to be 1.2% lower due to lower wages a year ago, but inflated by 3% to be in PV dollars of year 4, so will have present value costs of 7,012. I assume the offset is only half as much, based on information from Karoly and Bigelow (2005, pp.

earnings effects on peers and participants are assumed to be at the level we would expect based on the evaluations of the CPC program, and so should still be the same as in the baseline simulation per participant, that is the present value of such earnings effects has a ratio of 2.65 to the original costs per participant. Earnings effects via the social return to education from more college graduates should also be similar to the baseline simulation, at a ratio in present value terms of 0.04 to the original costs per participant. The earnings effects via increased labor supply of the participants' parents should be roughly twice as great, because the program now provides free child care for two years rather than only one year, so the earnings effects of free child care increase from a present value of a 0.05 ratio to the original costs per participant under the baseline simulation to a 0.10 ratio under these alternative assumptions. The earnings effects of the balanced budget multiplier effects of preschool per participant should go up roughly proportionately with costs per participant, so balanced budget multiplier effects increase from a ratio of 0.04 of original costs per participant under the baseline simulation to a ratio of 0.11 ($0.11 = 2.70 \times 0.04$) under these alternative assumptions. Summing up all the avenues of earnings effects, the present value of all earnings effects of preschool increase from a ratio of 2.78 of the original costs in the baseline simulation to 2.90 of the original costs under these alternative assumptions. Dividing these alternative earnings effects per participant by the alternative costs per participant leads to the result that the present value of earnings effects under these alternative assumptions will have a ratio of 1.07 to the present value of the costs per participant.

90–91) that about half as many 3-year-olds as 4-year-olds are enrolled in public programs. So the 3-year-old offset is 811, but after adjusting for 1.2% lower costs a year ago and adjusting to year 4 present value, the offset is 825. So the net costs of the 3-year-old program is $6,187 = 7,012 - 825$. So the total present value at year 4 of program costs per participant is $5,267 + 6,187 = 11,454$. The cost of the original program per participant was 4,234, so the net cost ratio has increased by a ratio of $2.70 = 11,454 / 4,234$.

Therefore, under this higher quality design of a CPC-like universal preschool program, we are clearly spending a lot more to get benefits that we expect to be only little greater than they were under the baseline simulation. But the advantage is that we perhaps have more assurance that these earnings effects will actually occur. Budgeting for a higher-cost, higher quality preschool program may have less downside risk of inadvertently falling below the threshold level of quality that is needed for preschool education to truly matter to the future of the children it serves.

Consider a further alternative design of a universal preschool program. Suppose we want to minimize the risk of being ineffective by mimicking the Perry Preschool program. Perry Preschool was subjected to a somewhat more rigorous research design than the CPC program (random assignment for Perry vs. a quasi-experimental control group for the CPC program). In addition, the Perry Preschool program model is higher in various elements of quality than the CPC program, which also reduces the risk of failing to meet a minimum quality threshold. The Perry Preschool program at capacity was operated with a student to teacher ratio that averaged 6.25 to 1, with class sizes of 12 to 2 or 13 to 2. Instead of a teacher and a teacher aide in each classroom, the Perry Preschool program used two teachers per class. About 80% of the Perry Preschool students participated in the program for two years. In addition to class time, the Perry Preschool program had teachers do weekly home visits.³²

It is unclear whether all these expenditures are needed to get the same results for children as were achieved by the Perry Preschool program. For example, researchers associated with the Perry Preschool program believe that similar results could be obtained with student to teacher

ratios of 8 to 1 or even 10 to 1 (Schweinhart et al. 2005, pp. 33–34, p. 202). However, to minimize the downside risk of achieving lower results than the original Perry Preschool program, suppose we design our “Perry-style” universal preschool program model to be as close as possible to the original design, with the very low original class sizes.

Based on an examination of the original costs of the Perry Preschool program when it was operated during the 1960s, and considering changes since then in prices and real wages of teachers, the annual per participant costs today, in 2004 dollars, of a Perry-style universal preschool program would be estimated to be \$10,399.³³ If we further designed the program to be two years for each participant, and allowed for offsets and converted to present value costs per participant as of age 4, the present value of costs for a Perry-style universal preschool program per participant would be 4.38 times the baseline simulation’s costs per participant.

The balanced budget multiplier effects per participant of a Perry-style program would roughly increase proportionately with costs per participant, so they would increase from a present value that is a ratio of 0.04 to the original costs per participant to 0.18 of original costs. I again assume the earnings effects of a two-year preschool due to effects on the labor supply of the participants’ parents would be roughly twice those of the baseline one-year program, so these

³²The Perry Preschool program is described in Schweinhart et al. (2005, pp. 33–37), although Barnett (1996, p. 25) is the source for the information that the capacity student to teacher ratio for Perry Preschool was 6.25 to 1.

³³This calculation of annual costs per participant of \$10,399 is based on some plausible extrapolations from the original costs of the program. Barnett (1996, Table 4, p. 20, average of figures for 1964–1965 and 1965–1966) reports average costs of the program on an annual basis, when the program was operating at capacity, of \$8,666 per participant in year 2004 dollars. But Barnett also reports that using current teacher salaries seems to increase costs by perhaps 20% (Barnett 1996, p. 26). Multiplying \$8,666 by 1.20 gives the figure of \$10,399. Heckman and Masterov (2004, p. 26) give a figure for Perry Preschool costing \$9,785 per participant per year if implemented in 2004; no source is given for this figure, but it gives at least some reassurance that I am not vastly underestimating how much Perry Preschool would cost today. With an average offset from reduced public spending on current programs of \$1,622, the net costs of Perry Preschool for 4-year-olds would be $8,777 = 10,399 - 1622$. For 3-year-olds, the net present value in year 4 dollars of costs would be $9,759 = (1.03 / 1.012) \times (10,399 - 811 \text{ offset})$. So net

effects would increase from 0.05 of the original costs in the baseline to 0.10 of original costs. I assume the earnings effects from the social return to more college graduates will remain unchanged from the baseline to a Perry-style program, at a ratio of 0.04 to the original costs. The Perry Preschool program's effects on college graduation rates are only modestly greater than what was assumed for the baseline simulation (see Table 10). As for the effects of a universal preschool program on the earnings of participants and their peers, using the Perry Preschool effects on educational attainment and employment rates, rather than the CPC-style effects assumed in the baseline, increases the present value of real earnings benefits per participant for participants and peers to a 4.96 ratio to the original costs per participant, whereas the baseline simulation had participant and peer effects whose present value was a ratio of 2.65 to original costs. Therefore, the real earnings effects for participants and peers in this Perry-style preschool model are almost twice as great as in the CPC-style preschool model used in the baseline. Adding up all these avenues of effects, this alternative Perry-style universal preschool program has a total present value of earnings effects whose ratio to original costs is 5.28. Dividing this by the 4.38 times greater costs, the Perry-style preschool program is estimated to have earnings effects whose present value is a ratio of 1.21 to the present value of the program's costs.

The bottom line in this Perry-style simulation is that by spending a great deal more per participant, we perhaps are lowering expected earnings effects per dollar spent. But the rationale for considering doing so is that this will increase our assurance that we are spending more than the critical threshold needed to achieve results.

present value at year 4 of two-year costs per participant is 18,536. The baseline simulation had net costs per participant of 4,234, so the ratio is 4.38 ($= 18,536 / 4,234$).

Targeting Preschool at High-Risk Children

If we believe that preschool education has much stronger benefits for high-risk children, usually used to refer to children from lower-income backgrounds, than for other children, then it makes some intuitive sense to target services towards high-risk children. This can be done by focusing whatever preschool resources are assumed to be available on high-risk children, and excluding or discouraging other children from public preschool education. This targeting would seem likely to raise the benefits per preschool participants. Under the assumptions used in this report's simulation model, assumptions which presume that high risk children benefit much more from preschool than other children, the average preschool participant from a universal program gets only 23% of the benefits that would be obtained from preschool by a high-risk child who otherwise would not have been in preschool. In addition to increasing benefits per participant, targeting lowers the net costs of universal preschool per participant by focusing the savings from replacing our current preschool programs on fewer participants.

For example, Nobel prize-winning economist James Heckman argues in an interview that “[i]t is foolish to try to substitute for what the middle-class and upper-middle-class parents are already doing...[W]e can certainly deal with the major problems...by starting first with children from disadvantaged families. As an economist, I would argue, go where the returns are highest” (Clement 2005, interview with James Heckman, p. 24).

One argument against targeting to high-risk children is that we don't really know that the gains to middle class and upper-middle class children from preschool are that much less than the gains to lower-class children. Most of the research on preschool education is on high-risk, lower income children, but the (relative) absence of evidence on middle class children, as the cliché goes, is not evidence of absence. In addition, some researchers argue that “direct evidence has

been growing that all children can benefit from high-quality preschool, including the more economically advantaged” (Barnett, Brown and Shore 2004, p. 6). Barnett and his colleagues do admit that on most indicators of effects of preschool, “effects are somewhat larger for disadvantaged children.” However, the exact quantitative magnitudes of the relative effects on different groups make a big difference in any rigorous evaluation of the benefits and costs of preschool. It is this precise quantitative evidence on relative effects for different income groups of children that is lacking.

Another argument against targeting to high-risk children is political: programs for poor people tend to be poor programs, because they lack broad political support. This argument has been most prominently made by Harvard sociologist Theda Skocpol, who points to the historically strong support for universal programs such as Social Security, as opposed to targeted programs such as welfare (Skocpol 1991). This strong support for universal programs such as Social Security occurs even when there is some targeting of benefits within the framework of universal eligibility. For example the Social Security benefit formula is deliberately designed to replace a higher percentage of earnings for lower-income workers.

Weak political support for a targeted preschool education program may reduce funding both for the numbers of children served and the quality of service received. Barnett and his colleagues argue that “[h]istory provides abundant evidence of weak political support for targeted preschool programs. In four decades, Head Start has failed to achieve full funding for a quality program” (Barnett et al. 2004, p. 12). The hope is that if preschool programs provide valuable services to all children, there will be political pressure to keep the quality up and provide adequate funding.

The counter-argument is that some conservatives may object to programs that provide free or highly subsidized services to more affluent families. For example, Heckman suggests that proposals for universal preschool programs may “create...an opposition group saying, ‘Why should we subsidize affluent working women?’” (Clement 2005, p. 24). My own judgment is that Heckman’s argument applies to a relatively small group of economics-influenced policy wonks. In the real world, political support is usually stronger for programs that have some legitimate claim to providing universal services.

The simulation model used in this paper is biased towards finding that preschool education programs should be targeted, as it incorporates the assumption that benefits from preschool are much stronger for lower-income children, as shown in Table 7. So, in using this model to simulate the effects of different types of targeting, the reader should keep in mind that this is a “best case scenario” for targeting.

First, I consider a strict targeting approach under which our new public preschool program, which replaces current public preschool spending, is strictly limited to high-risk and medium-risk children, of low to moderate income. Low-risk children, whose families earn more than \$50,000 per year, are strictly excluded from public preschool. A considerable number of such children are currently served in public preschool programs. In the numbers given in Table 7, derived from research by Karoly and Bigelow (2005), 24% of low-risk 4-year-olds currently participate in public preschool programs ($24\% = 13 / 55$), and these low-risk 4-year-olds constitute 39% of the current enrollment in public preschool programs ($39\% = 13 / 33$).

On the cost side, this reduces the net cost per participant by spreading the savings from replacing current preschool programs across fewer participants in the new program. The baseline preschool program costs \$5,856 per participant, and the savings from eliminating

current preschool programs amount to \$1,622 per participant, assuming that 70% of all 4-year-olds are served. So the net costs in the baseline simulation of universal preschool are \$4,234 per participant. If we exclude all the low-risk children, the public preschool program now only serves high-risk and medium-risk children, and therefore only has 32% of all 4-year-olds as participants. The savings per participant from eliminating current public preschool programs for 4-year-olds now amount to \$3,548 ($= 1622 \times 70 / 32$). Therefore, the net public cost per participant from operating this strictly targeted preschool program is \$2,308.³⁴ The ratio of the net government cost per participant of this strictly targeted preschool program to the baseline universal program is $0.545 = 2,308 / 4,234$. Even if benefits per participant were unchanged, such a strictly targeted program would close to double the real earnings effects per dollar of net government costs.³⁵

The original baseline preschool program has average benefits per participant that are 23% of the benefits received by a high-risk 4-year-old who otherwise would not have been in any preschool, based on the distribution of participants across different risk categories of child and different categories of what they would do without the program. Doing the same calculation for this new strictly targeted program, the average benefits per participant increase to 45.3% of what

³⁴This calculation of government costs ignores any cost of screening families for income eligibility. Karoly and Bigelow (2005, p. 117) assume administrative costs of running a means-tested transfer program of \$552 per participant. While this is perhaps appropriate for a program of collecting fees based on income, it seems somewhat high for a program that simply does a one-time income screen for eligibility.

³⁵One could debate whether this is a complete description of costs by only looking at government costs. Later in this report, I argue that charging fees to some preschool participants should be looked on as an alternative way of financing universal preschool, not as a real cost reduction. Similarly, it could be argued here that a proper cost accounting should consider the reduction in free public preschool services to the low-risk children who otherwise would have participated in a lower cost and lower quality current public preschool program. However, it is somewhat more problematic to put an exact dollar value on this cost; it is presumably some value between zero and what it costs the government per participant to provide current public preschool services, which is assumed to be \$3,441 in this report's model. In any event, as the report text discussion makes clear, whatever the economic costs are of excluding low-risk children from public preschool, in my opinion the political costs are more than sufficient to outweigh any greater earnings effects per dollar of economic costs.

would accrue to the high-risk 4-year-old who otherwise would not be in preschool. This almost doubles the real earnings effects per participant on participants and peers, and the real earnings effects via effects on college graduation rates ($45.3 / 23 = 1.9696$). In terms of the original costs per participant, the present value of real earnings effects on participants and peers increase from the baseline 2.65 to $5.22 (= 2.65 \times 1.9696)$. The effects on real earnings by increasing the state college graduation rate increases from 0.04 to 0.08.³⁶

After some complicated but boring calculations, balanced budget multiplier effects per participant in this new strictly targeted program decline from the baseline simulation's present value of 0.04 of baseline costs to 0.02, because of the larger cutbacks per participant in public spending on current preschool programs.³⁷ The child care effects of the strictly targeted program are likely close to zero. This strictly targeted program provides free child care for an additional 12% of all 4-year-olds who otherwise would not have received free child care, but eliminates 13% of all 4-year-olds in the low-risk category who currently receive free child care during preschool hours.

³⁶It might initially appear that the effects via college graduates would decrease because of the fewer participants in preschool. These effects on college graduates do tend to decline with the number of participants, but the effects per participant do not.

³⁷Under the baseline program, the spending per participant was \$5,856 in public spending on the 15% of all 4-year-olds who otherwise would not have enrolled in preschool plus (\$5,856 in public spending on the new program minus \$3,441 in current public spending) on the 33% of all 4-year-olds who otherwise would have been in the old public preschool program, plus (\$5,856 in additional public spending on the new program minus the typical spending of 1995 on private preschool) for the 22% of all 4-year-olds who otherwise would have been in private preschool, divided by the 70% of all 4-year-olds, or \$3,621 in additional spending per participant generated by the program. The 1995 figure for private preschool is based on Table 7, p. 15 of the U.S. Census Bureau publication "Who's Minding the Kids? Child Care Arrangements: Spring 1997" (Smith 2002), which reports an hourly charge for preschool which in 2004 dollars is \$3.80 per hour, and multiplied by 525 hours is 1995. Under the strictly targeted program, the net new spending per participant from the program compared to current policy is $\$5,856 \times 8\%$ of all 4-year-olds, $+ (5,856 - 3,441) \times 20\%$ of all 4-year-olds, $+ (5,856 - 1,995) \times 4\%$ of all 4-year-olds, $- \$3,441$ in current public spending on 13% of all 4-year-olds, all divided by 32% of all 4-year-olds. This equals \$2,058 in new spending compared to current policy from this strictly targeted program, which is a little more than half ($2,058 / 3,621$) of the additional public spending from the baseline universal program.

The net effect is that in this strictly targeted preschool program, the present value of earnings effects per participant, per dollar of the original costs of the baseline preschool program, increase from \$2.78 to \$5.32. Costs in present value terms are reduced to 0.545 of the original costs. The net real earnings effects in present value terms from this strictly targeted program, per present value dollar of net costs of this strictly targeted program, increase to \$9.76 ($= 5.32 / 0.545$), compared to \$2.78 in the baseline simulation.

From a technocratic, policy wonk perspective, in which all policies are viewed as politically feasible, a policy that offers earnings benefits of \$9.76 per dollar of net costs is preferable to one that offers benefits of \$2.78 per dollar of net costs. So the strictly targeted program would appear clearly superior. However, these greater benefits are to some extent based on assumptions that presume much greater effects of preschool on high-risk children. The research to establish this presumption is sparse. In addition, in my judgment, this strictly targeted program has very little political feasibility. The program categorically excludes 55% of all 4-year-olds from any possible eligibility for program benefits. Some of these “low risk” 4-year-olds currently receive some public preschool benefits. Therefore, the proposed policy involves taking away benefits from this group, not just denying new benefits.

A more politically acceptable mechanism of targeting is to allow universal eligibility for preschool education, but to charge sliding scale fees for enrollment. Enrollment would be free for low-income children, but higher income families would be charged some fees. Fees per child would increase with family income. Karoly and Bigelow (2005), for example, suggest a program under which the low-risk upper income families would pay half of the per participant program costs through fees, which amounts to fees of \$5.58 per hour or \$2,928 per year, and medium-risk moderate income families would pay one quarter of the per participant program

cost through fees, or \$2.79 per hour or \$1,464 per year. As current private preschool hourly fees average \$3.80 per hour (Smith 2002, and see previous footnote), and the proposed program is supposed to have considerably higher quality than the average preschool program, the proposed fees at least appear to be plausible.

What would be the effects of such a sliding scale fee program on the net earnings benefits per dollar of costs of a preschool program? On the cost side, this program certainly affects who pays the costs for preschool. Costs are shifted from taxpayers to moderate income and higher families who use the program. But this cost shift, while lowering government costs, does not really lower the social costs of the program, but merely rearranges its financing. This rearrangement of financing may raise political support for the program among some taxpayers. The social costs of the program do tend to go down if there are fewer participants as moderate and upper families reduce their demand for the program. This allows net costs per participant to go down as the savings from replacing current public preschool programs are spread over fewer participants in the new program. On the other hand, social costs of the program tend to go up due to the administrative costs of running an income-base fee system. Karoly and Bigelow (2005, p. 117) assume that a fee-based system would cost about \$552 per participant in 2004 dollars. This figure seems high to me, but there are no readily available alternative estimates.

On the benefit side, this program would be expected to change the utilization of preschool so that fewer moderate income and upper income children will use the program, which will tend to raise net earnings benefits per participant. From a political perspective, this reduced demand probably reduces political support for the program. The political question is whether this reduced political support from moderate and upper income families is offset by the increased

political support from taxpayers who prefer that moderate and upper income families pay a greater share of the costs of the program.

One extreme assumption is that fees have no effect on demand for this high-quality public preschool program. In this case, the only effect of the fees on the economic cost-effectiveness of the program is to increase its social costs by the administrative costs of the fees. If Karoly and Bigelow are right about the magnitude of these administrative fees, then the net social costs of the program increase by \$552 per participant, from a net cost of \$4,234 per participant (equal to program cost of \$5,856 minus savings from replacing existing programs of \$1,622) to \$4,786 per participant, or by a factor of 13.0%. The net earnings benefits of the program will then decline to \$2.46 in present value per dollar of costs ($2.46 = \text{original } \$2.78 \text{ per dollar of costs divided by } 1.130$). The political question is whether the added political support from taxpayers who prefer that participant families pay fees is offset by reduced political support from participant families who resent paying fees.

Another extreme assumption is that fees could reduce demand to zero among some income groups. For example, if fees reduced demand for public preschool to zero among the low-risk group, then we get results for cost-effectiveness similar to the strict targeting approach. We would also get similar political effects as under the strict targeting approach: dramatically reduced support for public preschool programs among middle income and above families.

It is more reasonable to assume that a sliding-scale fee system will reduce demand somewhat among moderate income and above families, but not to zero. Let us simulate possible effects of the fees assumed by Karoly and Bigelow (2005). Blau and Hagy (1998) provide estimates of the effects of changes in hourly child care prices on the proportion of families using center based child care. If we assume their estimates can be applied to determine the effects on

public preschool use of charging hourly fees, then the \$5.58 per hour fee for “low-risk” families will reduce the number of low-risk children using public preschool by 17.1% compared to the baseline universal free preschool program (from 38% of all 4-year-olds to 31.5% of all 4-year-olds). The \$2.79 per hour fee for “medium-risk” families will reduce the number of medium-risk children using public preschool by 8.6% compared to the baseline universal free preschool program (from 14% of all 4-year-old to 12.8% of all 4-year-olds).³⁸ This extrapolates the relatively moderate price responses found in Blau and Hagy to somewhat larger hourly fee changes. In addition, it assumes that a high-quality public preschool program is a distinct enough mode of early childhood care and education that such moderate demand responses would occur. If private preschool and other modes of child care are assumed to be very close substitutes for a high quality public preschool program, then we would expect the demand response to be considerably higher.

These sliding-scale fees and the resulting response have two effects on costs. First, by reducing the total number of participants in preschool, they increase the offset savings per participant from eliminating current preschool programs. These savings per participant from eliminating current preschool programs increase from \$1,622 per participant with 70% of all 4-year-olds participating, to \$1,822 per participant with 62.3% of all 4-year-olds participating ($1,822 = 1,622 \times 70 / 62.3$).

³⁸Blau and Hagy (1998) estimate that the effect of changing the hourly child care fee, in 2004 dollars, from \$2.22 per hour to zero is to increase use of center based child care by 4.7% of all families. The estimates here rely on extrapolating these responses to changing fees from zero to \$5.58 and zero to \$2.79. A fee increase from zero to 5.58 would then be expected to reduce usage by 11.8% of all low-risk families ($11.8 = 4.7\% \times 5.58 / 2.22$), and since low-risk families are 55% of all families, the reduction would be about 6.5% of all families ($6.5\% = 0.55 \times 11.8$). A fee increase from zero to 2.79 reduces usage by 5.9% of all medium-risk families, or by 1.2% of all families. Of course, the use of their estimates rely on assuming that this high-quality public preschool is a distinct enough mode of care from private preschool that it will have such moderate price elasticities. It also relies on extrapolating Blau and Hagy’s estimates to a somewhat different type of care than they originally considered.

Second, these fees reduce the government's net cost of running the program, while increasing the costs to participating families of the program and to society of running the program. Based on the assumptions outlined, the fees collected average \$1,781 per participant ($1781 = (31.5 \times 2928 + 12.8 \times 1464) / 62.3$). But there are some administrative costs involved with monitoring the income of all participant families, and collecting and processing the fees, and enforcing the fees. Using Karoly and Bigelow's assumption that such fees are \$552 per participant, net government costs of running the program are reduced by \$1,781. The net costs to society from the fees go up by \$552.

Combining these two effects, from a governmental perspective, a sliding scale fee structure reduces the net costs per participant from \$4,234 per participant ($5,856 - 1,622$) to \$2,805 per participant ($5,856 - 1,822 - 1,229$), or only 0.66 of the original cost per participant ($0.66 = 2,805 / 4,234$). From a social perspective, a sliding scale fee increases the net costs to the government plus participants from \$4,234 per participant to \$4,586 ($4,586 = 5,856 - 1,822 + 552$). This is $1.0831 \times$ the original cost per participant ($1.0831 = 4,586 / 4,234$).

The sliding scale fees also increase somewhat the benefits per participant. With fewer low-risk and medium-risk children using the program, the effects on participants will be higher. Rather than being 23% of the benefits for a high-risk child who would otherwise have not been in any preschool, average benefits per participant increase to 25.1% of the benefits for a high-risk child who would otherwise not have been in preschool. This increases effects on participants and peers, and effects from increasing college graduation rates, by a factor of $24.9 / 23 = 1.0913$. Effects on participants and peers increase from a present value of 2.65 as a ratio to costs under the baseline simulation to a present value ratio of 2.89 to the original costs ($2.89 = 2.65 \times 1.0913$). Effects on college graduates still round off at 0.04. Some lengthy and rather

boring calculations reveal that balanced budget multiplier benefits stay almost the same, at 0.04 on the original cost basis, while child care benefits decline from 0.05 to 0.03.³⁹

The net result is that total benefits of the sliding-scale fee program, on the original cost basis, increase to 3.00 from the higher risk mix of participants. If we divide this by the new net governmental costs, the resulting ratio of present value of earnings benefits to governmental costs is 4.55. However, if we divide this by social costs, the present value of earnings benefits per \$1 of present value of costs to the government plus participants is actually slightly lower at 2.77, versus the original figure of 2.78.

The latter analysis in terms of social costs seems more appropriate. Charging fees to participants is really just an alternative to financing the program via taxes. What this analysis reveals is that the modest benefits from shifting the participant mix to higher risk children via fees are probably offset by the administrative hassles of setting up and collecting fees.

One could of course get somewhat more optimistic appraisals of a sliding fee system by assuming it will have larger effects in reducing usage by moderate and upper income families. But this then raises again the issue of whether the program will become politically unpopular if fewer middle income and above families use the program. We also could get somewhat more

³⁹For the balanced budget multiplier, the baseline simulation has net additional spending per participant of \$3,607, equal to $(5,856 \times 15 + 2,415 \times 33 + 3,861 \times 22) / 70$, where 2,415 is the difference between the new public program's costs and the old public program's costs, and 3,861 is the difference between the new public program's costs and private preschool spending. A similar calculation for the sliding scale fee program yields additional public spending per participant of \$3,594, which equals $(5,856 \times 13.5 + 2,415 \times 30.1 + 3,861 \times 18.6) / 62.2$. So the balanced budget multiplier effects will remain unchanged at 0.4. As for child care costs, in the baseline simulation, we had 37 out of 70 households experience a child care cost reduction of 100% for 24% of the time, which equals a 12.7% reduction, and under the assumption that in only 60.7% of the cases the reduction "matters" because the 4-year-old is the youngest child, the average effective price reduction is 7.7%. In the fee simulation, the average child care cost reduction is 4.8%, which equals $(0.607 \times (6 \text{ persons with a 100\% reduction 24\% of the time} + 5.4 \text{ persons with a 75\% reduction 24\% of the time} + 20.7 \text{ persons with a 50\% reduction 24\% of the time}) / 62.2 \text{ participants}$. Some households pay increased costs for public preschool, but I ignore this under the assumption that the quality increase offsets the increased costs. Therefore the earnings effects of reduced child care costs are multiplied by 4.8 / 7.7, which reduces these effects as a ratio to original costs from 0.05 to 0.03.

optimistic appraisals if the analysis incorporated the administrative costs and losses from paying taxes to support the program, which may also be considerable. Finally, we would get a more optimistic appraisal if the costs of administering a fee-based system prove to be significantly less than assumed by Karoly and Bigelow.

Overall, these results suggest that it is unclear whether there will be substantial effectiveness gains from a sliding scale fee system. Gains in effectiveness from a sliding scale fee system depend crucially on such details as the demand responsiveness to fees and the administrative costs of fees. These issues require further research. From a political perspective, the main issue is whether it is politically better to finance all of the universal preschool system through taxes, or whether some of the system should be financed through user fees. Will the financing through user fees help the program politically by reducing objections from the groups Heckman refers to, that object to subsidizing upper income families? Or will the financing through user fees make the program more bureaucratic and administratively cumbersome, and politically less popular because middle class and upper class families perceive the program as providing fewer net benefits?

Reducing Labor Market Displacement from Preschool

One important assumption in the baseline simulation, an assumption that considerably reduces the real earnings effects of preschool, is that the increases in the quantity and quality of labor supply due to preschool education has considerable displacement effects in the labor market. The displacement rate is one-third. This means that for every additional job obtained by parents of participants, or participants and their peers, there is a loss of one job among other local residents.

Suppose we could use a magic wand and eliminate these displacement effects. Then the present value of real earnings effects on the participants in preschool and their peers would increase from a ratio of 2.65 of present value program costs to 3.98 ($3.98 = 2.65 \times 3/2$). The present value of real earnings effects on the parents of participants due to the free child care provided would increase from 0.05 of costs to 0.08 ($0.08 = 0.05 \times 3/2$). Assuming balanced budget multiplier effects and effects via increased college graduation rates are unchanged, the total present value of real earnings effects from preschool education would increase from a ratio of 2.78 of costs to 4.14.⁴⁰

What might reduce displacement effects of preschool education? Displacement effects of labor supply shocks can be reduced if labor demand is more responsive to the supply of labor of different types. This might occur if labor market conditions can be restructured so that it is easier to set up or expand a business, or find and hire qualified new workers. Therefore, policies such as reducing unnecessary red tape, or improving the quality of local labor market intermediaries might be helpful.

In addition, policies that explicitly seek to increase labor demand may also be helpful in reducing displacement. These could include economic development policies. The idea is that as labor supply increases due to preschool education, if matching economic development policies are also expanded, then the labor demand will be there to ensure that all the new workers can be absorbed by the labor market without displacement.

⁴⁰There is no real reason for balanced budget multiplier effects to increase as these estimates do not involve any supply-side displacement. However, implicitly the college graduate effects may involve some displacement, as it could be argued that the use of Bound et al.'s (2004) estimates of how college graduate stocks compare to flows involves some displacement. However, their estimates also involve normal in and out-migration, and it is somewhat difficult to separate out these different influences. Any effects in increasing college graduation effects would not make a large difference to the total ratio of all preschool effects to costs.

This is another way in which preschool education programs and economic development programs may be directly complementary: effective economic development programs may significantly increase the effectiveness of preschool education programs by reducing displacement.

Chapter 5

NATIONAL EFFECTS

Overview

So far, all this discussion has taken a state-level perspective on the earnings generated by traditional economic development programs and preschool education programs. This state perspective just looks at increases in employment rates and real earnings of the state's residents.

We might worry about whether these state policies of economic development or preschool education have spillover effects on businesses and residents in other states that should be taken into account. These spillover effects would be taken into account from a national perspective.

The national perspective would seem superior, as it is broader and considers more interests. If there are spillover effects that are unpriced and hence ignored, then state policies on their own will not be optimal. Some federal intervention to discourage state policies with negative spillovers, and encourage state policies with positive spillovers, would seem to be warranted.

The most obvious spillover effects are due to household migration and employment relocation. I first consider household migration and how it might lead to spillover effects. Some of the state residents who get jobs or higher wages due to the policy intervention, whether it is universal preschool or an economic development subsidy program of the same costs, will end up moving to other states. The increased jobs and earnings of these out-migrants in these other states will probably not be counted from the original state's perspective. In any event, the calculations this report has done up to now have not counted these benefits that occur after out-migration.

Spillover benefits for other states and thus the nation due to out-migration are more significant for preschool education than for traditional economic development subsidies. Preschool education has a long lag between the policy intervention, at ages 3 or 4, and the eventual effects on participants' employment and earnings. In contrast, economic development subsidies have much of their effect on employment and earnings almost immediately. The longer lag with which preschool education affects employment and earnings allows more time for state residents to move out. Therefore, household out-migration is likely to result in a higher ratio of national to state benefits for preschool education than for traditional economic development subsidies.

Spillover effects of economic development policy interventions may also result from employment reallocation. If some of the employment expansion in this state results in less employment in other states than would otherwise be the case, this effect needs to be taken into account in moving the analysis from a state perspective to a national perspective.

It is important to recognize that employment reallocation need not take place explicitly through employment relocation, the obvious case where a firm closes down one facility and replaces it with a facility in another state making the same product. Employment is reallocated across states when a large firm chooses one state for a location, and if this had not occurred, the firm would have created the same number of jobs in some other state. Employment is reallocated when a small business starts up or expands in one state, the small business's sales are to a national market, and it is appropriate to assume that the size of that national market is fixed, so that the small business' increased sales and employment result in reduced sales and employment of businesses in other states.

The argument that state employment growth results in employment reallocation across states, and not national employment growth, is most commonly applied to traditional economic development subsidies. It is argued that this is a zero sum game: any benefit this state gains by expanding employment by some amount will be offset by losses in other states that are argued to lose employment by that same amount.

This zero-sum game argument has been prominently made in papers promoting preschool education as an economic development program. For example, Grunewald and Rolnick (2005) argue that “from a national perspective, jobs are not created [from traditional economic development programs]—they are only relocated; the public return is at most zero.” Ehrlich and Kornblatt (2004, p. 4) argue that “[a]ny benefit derived from the relocation of jobs [due to traditional economic development programs] is experienced at the local level. On a larger scale, the U.S., for example, derives no social benefit when jobs move from Missouri to Mississippi, and any tax dollars spent to fund such a move result in a net loss of social welfare.”

One point countering this zero-sum game argument is that it is probably not true that lowering business costs and raising business profits has zero effect on national employment. Lowering business costs in one state through economic development subsidies does lower average national business costs slightly. We would expect this to provide some stimulus to overall national business activity. However, the immediate national effect of this stimulus will probably be less than the increased business activity that accrues to the state. One would think that the decision about whether to expand business activity at all is a more fundamental decision that is less altered by modest changes in business costs than the decision about where to locate any desired expansion in business activity.

In addition, there may be social benefits from locating more employment in high unemployment areas. It is reasonable to expect that in areas with higher unemployment, in which it is harder to get a job, the average unemployed person who gets a job is more desperate for a job and “needs” the job more than is true in a low unemployment area, in which jobs are plentiful. As a result, the benefits of increased employment for the local unemployed are greater in high unemployment areas than in low unemployment areas. These greater social benefits could result in a net national positive sum game from reallocating employment from low unemployment to high unemployment areas, even if there was zero net national employment growth. On the other hand, reallocating employment from high unemployment to low unemployment areas may result in net social losses or a negative sum game, even if there is zero net national employment growth.

Furthermore, increased net national employment gains may be feasible if employment is reallocated to high unemployment areas. Suppose that national inflation results in part from inflationary pressures in each local economy. Suppose further that local inflationary pressures from a one percent reduction in unemployment in a local area are greater if the local economy has low unemployment than if the local economy has high unemployment, which is supported by research on local wages and prices (Blanchflower and Oswald 1994; Bartik 2001). Then it is possible to increase national employment, with the same national inflationary pressures, by reallocating employment from low to high unemployment areas. However, this argument also implies that if employment is instead reallocated from high unemployment areas to low unemployment areas, then national employment must be reduced somewhat to prevent an increase in inflationary pressures on the national economy.

Another issue is whether the zero sum game argument also applies to employment reallocations associated with labor supply shocks, such as increases in the quantity and quality of labor due to preschool education. The likely answer is that the zero sum game argument is not applicable in this case. Preschool education, or other programs that enlarge the quality or quantity of labor, do enlarge the overall production capability of the national economy and therefore the size of the national market. The analysis of this report has already allowed for the likelihood that not all of this increase in effective labor supply will result in an increase in labor demand, by assuming that the equilibrium increase in employment and earnings is only two-thirds of the initial shock to labor supply.⁴¹

Preschool Education

To obtain the national effects of preschool education, I redo the simulation model assuming zero out-migration. With this assumption, in the typical U.S. state, the present value of real earnings effects per present value dollar of preschool spending is 3.79. This compares with the 2.78 ratio for the typical state with typical out-migration patterns. Hence, on average the national earnings benefits from preschool are 36% greater than the earnings benefits for state residents that are perceived by the typical state. ($36\% = (3.79 - 2.78) / 2.78$).

I also can consider the national effects associated with each state's investment in universal preschool education. Table 23 shows long-run effects of a universal preschool education program on jobs, annual wage and salary earnings, and real GSP in each state. The table includes previously reported figures (Table 21) for these effects from a state perspective,

⁴¹For the current report, I assume that the relevant national and state labor demand and supply curves are such that the relevant displacement effects of shocks to labor supply are similar at the state and national level. I plan

and estimates of these effects from a national perspective. The sum across states of these effects from a national perspective represents the true national effects of all states moving to a universal preschool education program.

The table shows quite large differences between effects from a state vs. national perspective. The true national effects of preschool are much greater than the effects perceived by each state. This is particularly true for smaller states with less diverse and vibrant economies, such as Wyoming and North Dakota. This is also true for cities such as the District of Columbia. For smaller states and cities, out-migration is much more extensive, so much of the national benefits from preschool education do not stay in the state or city.

Traditional Economic Development Programs

For traditional economic development programs, estimates of spillover effects and national effects require some additional assumptions. First, for the estimates in this section, I ignore possible social benefits or reduced inflationary pressures from reallocating employment across states. The effects of reallocating employment could be argued to be eliminated from a national perspective if all states equally engage in a fierce economic development competition.

In this section, what I focus on estimating are the national effects that occur from economic development subsidies when considering that some of the state effects merely represent reallocations of jobs from other states. However, even though there is some reallocation of employment due to state economic development subsidies, these subsidies do produce some national gains in jobs unless one assumes that national levels of business activities are completely insensitive to business costs and profitability.

To evaluate the national effects vs. the state effects of reductions in business costs, we need to have national estimates of the costs of creating a job by business subsidies. For this report, I derive such estimates from a study by Jonathan Gruber and Joshua Rauh (2005) of the national elasticity of corporate investment with respect to the federal corporate tax rate. Their estimates, when combined with estimates of corporate tax revenue per employee, imply that the cost of creating a job in the nation through federal corporate tax breaks is \$141,714 in annual foregone revenue, ignoring the increase in the corporate tax base due to the expanded business activity.⁴²

In Chapter 2, I summarized the consensus from the economic development literature that the cost of creating a job in a state from subsidizing an export-base firm is \$19,445 in annual foregone revenue, again ignoring the resulting increase in the state business tax base. These estimated costs of national and state job creation imply that if we provide an annual subsidy of \$19,445 to create one job in a state, the national job creation will only be a fraction of one job on

⁴²In Gruber and Rauh's Table 4, they estimate that the elasticity of corporate investment with respect to $\ln(1-ETR)$, where ETR is the effective corporate tax rate, is 0.115. I interpret the elasticity of business investment as being a proxy for the elasticity of long-run business activity. Their estimated 0.115 is not statistically significant at usual levels (standard error of 0.085). But they do estimate statistically significant elasticities of corporate tax bases with respect to $\ln(1-ETR)$ of about 0.2, which should overstate the elasticities of business activity, as some of the effect of taxes is on the ratio of reported corporate profits to business activity. The elasticity I need to estimate the costs of creating jobs nationally through corporate tax cuts is the elasticity with respect to ETR . Some derivation shows that this will equal $0.115 \times (-1) \times ETR / (1-ETR)$. The median ETR in their data is 0.177 across the industries considered, according to their Table 1 figures for the 1999–2003 period. Therefore, $d\ln(invest) / d\ln ETR = 0.0247 = 0.115 \times (-1) \times (0.177) / 0.823$. According to the U.S. Office of Management of Budget, corporate income tax revenues in 2005 were \$278.3 billion. Total private sector employment, according to the BLS, was 111.7 million in 2005, and 103.1 million in 1997. According to the last available statistics, from the 1997 Economic Census, Company Summary, in 1997 employment in C corporations was 71.0 million. Assuming that the ratio of C corporation employment to total employment was unchanged from 1997 to 2005, the implied employment in C corporations was 76.9 million. This implies that corporate income tax revenue per corporate employees was \$3,619 in 2005. The cost per job of creating a job through a tax subsidy, ignoring the revenue gained from the expanded base, is given by the tax revenue per job divided by the elasticity with respect to the tax rate, or is here equal in 2005 dollars to $\$3,619 / (0.0247) = \$146,518$. Converting to 2004 dollars gives a cost per job created of \$141,714.

average, specifically the fraction $19,445/141,714 = 0.137$.⁴³ So about seven-eighths of the export-base jobs attracted by economic development subsidies represent reductions in export-base jobs for other states.

These estimates aren't a complete picture of national and state employment effects of subsidies, because jobs created in export-base businesses by economic development subsidies will result in multiplier effects that increase employment in other businesses. We would expect national multiplier effects of a boost to export-base businesses to be greater than state multiplier effects. National multipliers exceed state multipliers because some suppliers to export-base businesses will be located in other states, and some of the expansion in consumer demand will increase sales and production of firms located in other states.

Because of multiplier effects, the costs of creating a job in a state through economic development subsidies is given by $19,445/M_s$, where M_s is the state multiplier. The costs of creating a job nationally through economic development subsidies becomes $\$14,1714/M_n$, where M_n is the national multiplier. Therefore, the ratio of the jobs created in the state from economic development subsidies to the jobs created nationally becomes $(19,445/M_s) / 141,714/M_n) = 0.137 \times M_n / M_s$.

Estimates from the REMI model for Michigan suggest that the average ratio of national to state multipliers (M_n / M_s) is 1.407, that is the national multiplier is 41% greater than the state multiplier.⁴⁴ As a result, the ratio of the jobs created in the nation by the economic development

⁴³The number of state export-base jobs created by an economic development subsidy of $\$S$ is $J_s = S/19,445$. The number of jobs created nationally in export-base industries by this subsidy is $J_n = S/141,714$. Therefore, $J_n/J_s = 19,445/141,714$.

⁴⁴The REMI model estimates average multiplier effects across all manufacturing industries of 2.95 for Michigan, and 4.15 for the U.S. This average is a weighted average using national industry employment shares as weights. The Michigan multiplier for manufacturing is higher than the average multiplier from MEGA subsidized businesses of 1.98 (Chapter 2) because MEGA also subsidizes service firms whose multiplier effect is lower because

subsidy, to jobs created in the state is equal to 0.193 ($= 0.137 \times 1.407$). Therefore, approximately four-fifths of the jobs created by economic development subsidies result in reduced jobs elsewhere, and only one-fifth represent a net increase in national jobs.

However, the state effects estimated for economic development subsidies in Chapter 2 only included effects for persons who stayed in the state. Therefore, the Chapter 2 effects already disregarded some of the effects of a state's new jobs from economic development subsidies. From a national perspective, we don't want to disregard the effects of new job creation on out-migrants. Therefore, the true national effects of economic development subsidies should be calculated as 19.3%, or about one-fifth, of the effects that would have occurred in a state if there was no out-migration, which will be somewhat greater than one-fifth of the state effects that were estimated in Chapter 2.

The resulting calculation of the national effects of economic development subsidies results in the estimate that the ratio of the present value of the national earnings effects to the present value of the economic development subsidies is 0.65. This is considerably below the ratio from the typical state's perspective, reported in Chapter 2, of 3.14.

I also calculate for each state the long-run effects from a national perspective on jobs, real earnings, and GSP of economic development subsidies. Table 24 shows these national perspective effects of economic development subsidies, along with the estimates previously presented in Chapter 2, Table 6, of the effects of economic development subsidies from a state

of less dense supplier networks and lower wages. Ideally, all these state multipliers should be varied for each state rather than using multipliers from Michigan. This is likely to result in more errors in estimating each state's effects from economic development subsidies rather than in estimating the national effects from economic development subsidies.

perspective. As can be seen, the true national effects are only a modest proportion of the effects from each state's perspective.⁴⁵

Comparison

With these estimates of effects for each state from a national perspective, we can compare the national effects across all states of preschool education and economic development. I report these estimates in Table 25, which simply takes the sum of the state by state estimated effects from a national perspective. As this table clearly shows, the national effects of preschool on jobs, earnings, GSP, and total tax revenue are considerably greater than those of economic development subsidies.⁴⁶

⁴⁵This entire calculation essentially assumes a national economy that is demand driven. That is, I use the Gruber and Rauh (2005) estimates of the costs of creating a job through national business tax cuts for each industry, and assume that this can be generalized to the aggregate national level. However, in order for these national effects to fully occur as assumed in this report, additional supplies of capital and labor must be made available at the same prices. It is perhaps not unreasonable to assume that additional supplies of capital might be made available at the same price (same profit rate), but one would generally think that additional supplies of labor might require higher wage rates. However, there is some economic consensus that the aggregate labor supply of at least adult women, and probably teenagers and older workers as well, is at least somewhat responsive to higher wages (e.g., see for example the survey of labor economists by Fuchs, Krueger, and Poterba (1998) for support for a positive labor supply elasticity for adult women). In addition, the wage curve literature suggest that additional labor supply can be elicited, at some wage cost, through lower unemployment and higher labor force participation, as discussed in Bartik (2001). If we allow for higher wages in response to demand shocks from economic development subsidies, this may reduce somewhat the national jobs effects of economic development subsidies. On the other hand, it will increase somewhat the wage rate effects of economic development subsidies, which are assumed to be zero in the current report's models. I hope to explore these issues of national equilibrium responses to economic development subsidies more fully in future work.

⁴⁶The total tax revenue effects are calculated, from a national perspective, considering federal, state and local taxes, whereas from a state and local perspective I only consider state and local taxes in each state.

Employment Reallocation and Inflation

The estimated national effects for economic development subsidies in this chapter have so far ignored any possible national benefits due to the reallocation of jobs. Social benefits from the average unemployed person being more desperate for a job in a high unemployment area are hard to estimate. Good estimates would require knowing how the value of the unemployed's time differs in different local economies, about which we have little reliable knowledge.

However, we do have some evidence on how the effects of unemployment on wages and prices vary with unemployment. And this allows us to do some simple analysis of how the reallocation of jobs to a particular state might affect the net national employment level consistent with no acceleration of inflationary pressures.

I do this in Table 26 for jobs. The table presents two alternative calculations that differ in the assumed functional form for how local prices and wages are affected by local unemployment rates. One calculation is based on research by Blanchflower and Oswald (1994) that the logarithm of local wages is best modeled as a function of the logarithm of local unemployment. The other calculation is based on research by Bartik (2001) that suggests that the logarithm of local prices is best modeled as depending on one over the local unemployment rate. Bartik's estimates suggest that the effect of one point lower unemployment on inflation varies more dramatically with the local unemployment rate than is true of Blanchflower and Oswald's estimates, and therefore suggests more national employment gains (losses) from reallocating employment to high unemployment (low unemployment) states.

Each state's calculation starts with the previously reported estimates of the net effects on jobs, from a national perspective, of the state's economic development subsidies. These previous estimates ignored possible gains from reallocating jobs across states. I then calculate

the possible national jobs gains if the increased jobs in this state are required to be offset by reduced jobs elsewhere in the nation to keep national inflationary pressures constant.⁴⁷ These estimates incorporating possible inflationary effects of job reallocation across states cannot be added across states. Each state's estimate assumes that the job increase in that state is offset with job reductions in other states, and it is therefore inconsistent to sum these effects across all states, which would imply that jobs were being increased in all states. The true national benefits from reallocating jobs across states and thereby changing inflationary pressures would depend upon whatever pattern there was of adding jobs due to economic development subsidies across states, and would have to be calculated separately for different assumptions about economic development subsidies in each state.

As this table shows, adding jobs in high unemployment states due to economic development subsidies can result in considerably higher job creation possibilities than in low unemployment areas. In low unemployment areas, in many cases the net national effects of adding jobs in these areas is negative, as the model assumes that job creation in this state would have to be more than offset by more than one to one job reductions elsewhere. On the other

⁴⁷Suppose $\ln P = B((J_s/J)f(U_s) + (J_o/J)f(U_o))$, where P is national price, J_s is jobs in state s , J is national jobs, J_o is jobs in states other than s , and U_s is the unemployment rate in state s , U_o is the unemployment rate in other states, and f is the functional form by which unemployment affects prices. Then if we totally differentiate this expression with respect to J_s and J_o , but assume that to maintain a viable price index the initial employment weights on each area are fixed, then we get $d \ln P = (B/J)[(df/dU_s)(dU_s/d \ln J_s) dJ_s + (df/dU_o)(dU_o/d \ln J_o) dJ_o]$. We assume that percentage job growth has similar effects on unemployment rates in each state, which is supported by research (Bartik, 1991). We then get $d \ln P = (B/J)(dU/d \ln J)[(df/dU_s)dJ_s + (df/dU_o)dJ_o]$. For zero inflationary pressures to result from this change in J_s and J_o , we must have the expression in brackets equal to zero. Solving for J_o , we get $dJ_o = -dJ_s[(df/dU_s)/(df/dU_o)]$. The total change in national jobs that is consistent with zero inflationary pressures is given by $dJ = dJ_s + dJ_o = dJ_s \{1 - [(df/dU_s)/(df/dU_o)]\}$. When $f(U) = \ln(U)$, which is Blanchflower and Oswald's empirical estimate of the best function form for how unemployment affects wages, this expression in brackets is equal to U_o/U_s . When $f(U) = -(1/U)$, which is what I find in Bartik (2001), then the expression in brackets is equal to U_o^2 over U_s^2 . This expression gives the net national employment expansion possible by expanding employment in this state and offsetting it with contractions in other states. I implement this for this study by assuming each state is small relative to the nation, substituting the national unemployment rate for U_o , and using 2005 state unemployment rates. I assume that this effect from reallocation depends on the size of the initial gain in jobs in the

hand, in high unemployment areas, the national job creation effects of relocating jobs to that state can be one-half to two-thirds of the job creation effects that occur in that state..

Federal Intervention Issues

Finally, one issue is whether these divergences between national effects and state effects justify some federal intervention. The real issue here is whether these spillover effects that aren't considered by states are optimally priced.

In the case of preschool, there are very large spillover effects for other states and the nation from one state's preschool investment. On the other hand, there already are federal subsidies for preschool education. The question is whether on the margin states perceive that additional spending will receive sufficient match from the federal government that they are sufficiently encouraged to expand their preschool investments. Based on the analysis of the PV effects of preschool from a national vs. state perspective, we should want the average federal marginal subsidy for expanded state spending on preschool education to be about 36% ($= (3.79 - 2.78)$).

For economic development subsidies, the issue is more complicated, because each state offers economic development subsidies. In some theoretically perfect world, these subsidies offset the spillover effects. The spillover effects really occur when firms make location decisions. If each state provides the optimal subsidy that truly reflects the social benefits and costs of expanded employment in that state, then employment location decisions will be economically efficient even if no net national employment is created.

state, and that this effect is in addition to the national effects from the lower national business costs resulting from this one state's economic development subsidy.

But several issues should be noted. First, even if we have such a world of perfectly efficient economic development subsidies, the result is a significant redistribution of resources from the general public, that pays for the subsidies, to owners of capital. This redistribution has a highly regressive effect on the income distribution. Second, it is unclear whether states offer optimal economic development subsidies, equal to the true social benefits from expanded employment in that state. It is disturbing that so many states with quite healthy growth and low unemployment also seem to engage heavily in the subsidy competition, even when the likely local as well as national gains from this are slight. This suggests that in many states, economic development policy may have been captured by interests that favor more growth no matter the local economic situation.

Chapter 6

MAIN FINDINGS OF THIS REPORT

In a lengthy report such as this, it is easy for readers to lose the forest for the trees. Here are the main findings of the report that I think deserve special emphasis:

- Economic development subsidies and preschool both have about the same cost-effectiveness in producing earnings benefits for state residents, both yielding about \$3 in present value of earnings benefits for every dollar invested in these programs.
- In achieving economic development goals of more jobs and earnings for state residents, economic development programs and preschool programs should be viewed as complementary programs, rather than as potential substitutes for one another. Economic development programs offer more immediate benefits than preschool programs over a 5- to 10-year-period. Preschool programs offer more longer-term benefits than economic development programs. In addition, simultaneously increasing labor demand, through economic development programs, and labor supply, through preschool programs, may make both programs more effective.
- Most of the earnings benefits of preschool are due to effects of high-quality preschool in increasing the future educational attainment and employability of participants in preschool, and their peers in K-12 education. Other claimed economic development benefits of preschool education—such as the balanced budget multiplier effects of this spending on a state’s economy, the increase in parental labor supply from the free child care provided by preschool—are present but modest in size.
- The key issue in expanding preschool programs to a universal scale is what quality level or cost per participant is needed for these programs to truly be effective. There is an immediate need for research projects that would seek a more systematic understanding of the relationship between preschool cost per participant, preschool quality, and participant outcomes. In the interim, I think it would be wise to err on the side of spending “too much” per participant on universal preschool programs. This avoids the political problems that would occur if a universal preschool program proved to be ineffectual.
- Targeting an expanded preschool program towards more high-risk children, who would presumably benefit more from preschool, raises some difficult issues of what creates political support for a public preschool program and whether a politically acceptable targeting system will actually increase net earnings benefits per dollar invested. Simply restricting access to public preschool programs by middle income and upper income children may significantly raise the cost-effectiveness of public preschool, but requires taking away public preschool access from a majority of U.S. families. The more politically acceptable approach of charging sliding scale fees based on family income has

uncertain effects on the cost-effectiveness of preschool in raising earnings. More research is needed on the administrative costs of such fees and their effect on demand for public preschool by different groups. There is a difficult tradeoff between increased cost effectiveness and reduced political support: the greater the impact of fees on demand for public preschool by middle and upper income groups, the greater the increase in cost-effectiveness of the programs, but this same reduction in demand probably leads to a reduction in political support for public preschool by middle and upper income families. Political opinions probably vary on whether overall public support would be greater for a totally tax-financed universal preschool system, or a universal preschool system financed in part with fees for higher income families.

- Taking a national perspective significantly changes our evaluation of the relative benefits of universal preschool and traditional economic development programs in achieving economic development goals. From a national perspective, the economic development benefits of universal preschool significantly exceed the economic development benefits from an unchecked economic development war among the states. In particular, for low unemployment states, the vigorous use of traditional economic development subsidies may actually have negative effects on the nation's overall economic well-being. Ideally, the use of traditional economic development subsidies should be targeted at high-unemployment local economies. Preschool education programs have benefits that go beyond state borders as preschool participants move to different states. For cities, out-migration of preschool participants is likely to be so large that local governments are unlikely to find preschool programs attractive as an investment in economic development. For states, the magnitude of the positive spillover effects of preschool education, due to out-migration of preschool participants, is large enough to justify a sizable federal subsidy for state governments' investments in preschool education.

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Table 1 A Typology of State and Local Economic Development Programs

<p>1. PROGRAMS PREDOMINANTLY AIMED AT ENCOURAGING LOCATION OR EXPANSION OF FACILITIES OF LARGE CORPORATIONS</p> <p>Financial incentives Property tax abatements Tax credits, often refundable, tied to taxes paid by firm's workers to state Other tax relief, varying widely in whether "automatic" or "discretionary" Industrial revenue bonds Provision of land at below-market prices Direct state loans</p> <p>Nonfinancial incentives to large corporations Customized industrial training Expedited provision of site-specific infrastructure or services Help with regulatory permits: speeding up permits or relaxing regulations Industrial parks Tax-increment financing, allowing property taxes to be used for area-specific or firm-specific purposes</p> <p>Marketing area to new branch plants, corporate headquarters, or distribution facilities Industrial development advertising Marketing trips to corporate headquarters Provision of site information to prospects</p>
<p>2. PROGRAMS PREDOMINANTLY AIMED AT SMALL AND MEDIUM SIZED BUSINESSES, EITHER EXISTING OR NEW</p> <p>Capital market programs Predominantly government-financed loan or equity programs Government support for predominantly privately financed loan or equity programs</p> <p>Information/education for small business Small business ombudsman/information office Community college classes in starting a business Small business development centers Entrepreneurial training programs Small business incubators Business visitation and surveying</p> <p>Research and high technology Centers of excellence in business-related research at public universities Research-oriented industrial parks Applied research grants to faculty or businesses Technology transfer programs/industrial extension services</p> <p>Export assistance Information/training in exporting for businesses Trade missions Export financing</p> <p>Cluster programs/networking programs Identifying local clusters as support for selective marketing and promotion, and overcoming barriers Networks of small businesses working with local community colleges to design better training Networks of small businesses doing cooperative marketing, shipping, and use of consultants</p>
<p>3. PROGRAMS AIMED AT SPECIFIC GEOGRAPHIC AREAS, OFTEN CALLED ENTERPRISE ZONES, WITH SIZE AND NUMBER OF DESIGNATED ZONES VARYING WIDELY, AND WITH POTENTIALLY ALL TYPES OF ECONOMIC DEVELOPMENT PROGRAMS TARGETED FOR THOSE AREAS</p> <p>Tax breaks for firms locating or expanding in a designated area Enhanced services/ infrastructure in designated area, whether firm-specific or general Downtown and neighborhood business district development</p>

SOURCE: Adapted from Bartik (1991, 2001, 2004).

Table 2 Outline of Assumptions Made in Comparing the Economic Development Effects of Traditional Economic Development Programs and High-Quality Preschool Programs

- Universal preschool education program compared with traditional economic development program providing subsidies to firms that has same present value of costs
- Universal preschool program has characteristics assumed by Karoly and Bigelow (2005)
 - 70% of all 4 year olds served by this program
 - Program provides one school-year of part-time services to all participants, which means a 3-hour per day program operating for 175 days per year, or 525 hours per year
 - gross cost of program per 4 year old served is \$5,856 in 2004 dollars
 - net cost of program, after allowing for saved costs on current government preschool programs, is \$4,234 per participant in 2004 dollars. (This slightly modifies Karoly and Bigelow assumptions; their calculations implicitly allowed one-third of current public preschool participants to have zero public costs, whereas I assume these participants had same average costs of \$3,441 per participant as other public preschoolers. 33% of all 4 year olds are assumed to already attend public preschool. $\$4,234 = \$5,856 - ((33/70) \times 3,441)$)
- Without these programs, state economy has default employment and population growth rates over next 75 years averaging 0.3% per year, and real wage growth of 1.2% per year. (Source: Real wage growth projections come from the U.S. Congressional Budget Office (Holtz-Eakin, 2005); population growth projects come from the Board of Trustees of the Social Security program (OASDI Board of Trustees, 2005).
- Some calculations assume program that starts out with costs of \$1 billion per year, and then grows with state's population and economy. This hypothetical state economy would have the following initial characteristics:
 - 236,183 4-year old participants in hypothetical universal preschool program ($=\$1 \text{ billion divided by } \$4,234$)
 - 337,404 total 4-year olds ($= 236,183/0.7$)
 - 24.180 million total state population ($= 337,404 \text{ times ratio of total population to 4-year olds from 2000 U.S. Census}$)
 - 9.464 million in wage and salary employment ($= 337,404 \text{ times estimated ratio of non-farm employment to 4-year olds from year 2000 Census, and ratio of wage and salary employment to non-farm employment from 2003 figures from Regional Economic Information System, U.S. Bureau of Economic Analysis}$)
 - \$334.5 billion in wage and salary earnings per year ($= 9.464 \text{ million in wage and salary employment times 2000 hours per year times average wage per hour of } \$17.67 \text{ per hour, calculated by author from 2004 Current Population Survey Outgoing Rotation Group data}$)
- Some calculations examine the effects of a “universal” preschool program in each state, or an economic development program of similar size. To scale a universal preschool program in each state, I assume that the net incremental costs of such a program are the same percentage in each state of wage and salary earnings in that state, based on the above hypothetical program. This assumes that each state is willing to pay the same proportion of wage and salary earnings for a universal program as it would cost in the hypothetical state for a universal preschool program, or, using above calculations, 0.299% ($=\$1 \text{ billion}/\334.5 billion). This avoids a great many complicated and questionable assumptions about what percentage of 4 year olds would be served in each state and how the wage of preschool workers varies with the state's economy.

Table 3 Assumed Changes in Unemployment Rates and Labor Force Participation Rates Over Time As Proportion of Job Growth Shock

Year	Unemployment rate change (as proportion of growth shock)	Labor force participation rate change (as proportion of growth shock)	Year	Unemployment rate change (as proportion of growth shock)	Labor force participation rate change (as proportion of growth shock)
4	0.300	0.300	36	0.000	0.091
5	0.240	0.299	37	0.000	0.085
6	0.180	0.296	38	0.000	0.078
7	0.120	0.293	39	0.000	0.072
8	0.060	0.288	40	0.000	0.066
9	0.000	0.283	41	0.000	0.061
10	0.000	0.277	42	0.000	0.055
11	0.000	0.271	43	0.000	0.050
12	0.000	0.265	44	0.000	0.045
13	0.000	0.258	45	0.000	0.040
14	0.000	0.251	46	0.000	0.036
15	0.000	0.244	47	0.000	0.031
16	0.000	0.237	48	0.000	0.027
17	0.000	0.230	49	0.000	0.024
18	0.000	0.222	50	0.000	0.020
19	0.000	0.215	51	0.000	0.017
20	0.000	0.208	52	0.000	0.015
21	0.000	0.200	53	0.000	0.013
22	0.000	0.193	54	0.000	0.011
23	0.000	0.185	55	0.000	0.009
24	0.000	0.178	56	0.000	0.008
25	0.000	0.170	57	0.000	0.006
26	0.000	0.163	58	0.000	0.005
27	0.000	0.155	59	0.000	0.004
28	0.000	0.148	60	0.000	0.003
29	0.000	0.140	61	0.000	0.003
30	0.000	0.133	62	0.000	0.002
31	0.000	0.126	63	0.000	0.002
32	0.000	0.119	64	0.000	0.001
33	0.000	0.112	65	0.000	0.001
34	0.000	0.105	66	0.000	0.000
35	0.000	0.098			

NOTES: Dating system used is that first year in which policy changes is year 4, dated after age of first cohort that might experience universal preschool. The unemployment rate and labor force participation rate effects here are proportion of jobs created that go to lower unemployment and raise labor force participation of original state residents. The initial unemployment rate effects and fading over five years are calibrated to roughly match what is observed in research estimated by Bartik (1991), Blanchard and Katz (1992) and Bartik (1993), and reviewed by Bartik (1993). The initial labor force participation rate effect also matches this research. This effect is faded over time by multiplying initial effect by proportion over time of initial state residents in workforce estimated to survive and stay in state. This is calculated separately for each age of initial workforce, weighted by each age's estimated share of total state employment. Predicted proportion for next year derived by using: U.S. year to year life survival tables from *National Vital Statistics Report 53(6)*, November 10, 2004, Table 1; estimated proportion of each age in state one year that is not in state next year based on estimates derived from PUMS data and PSID, using information on proportion staying in birth state from PUMS and age-4 state from PSID, as explained later in this report; age-specific employment rates used to calculate overall surviving in-state employed population. The labor force participation rate fading that results is roughly consistent with empirical results showing that initial labor force participation rates only modestly decline after shock for at least first 17 years after shock (Bartik 1993), which would be year 21 in this dating system.

Table 4 Jobs and Earnings Effects on Original State Residents of One-Time \$1 Billion in Economic Development Subsidies

Year	Jobs generated for original state residents	Real earnings generated for original state residents (in millions of year 2004 dollars)	Percentage of real earnings effect due to effect on employment rates (Remainder due to increased wage rates)	Year	Jobs generated for original state residents	Real earnings generated for original state residents (in millions of year 2004 dollars)	Percentage of real earnings effect due to effect on employment rates (Remainder due to increased wage rates)
3	0	0.0		41	432	47.6	54.6
4	4286	216.0	70.6	42	393	43.4	54.6
5	3849	203.5	68.4	43	356	39.5	54.6
6	3403	190.3	65.9	44	322	35.7	54.6
7	2949	176.3	62.9	45	288	31.9	54.6
8	2488	161.4	59.2	46	256	28.3	54.6
9	2022	145.4	54.6	47	225	24.8	54.6
10	1982	145.6	54.6	48	196	21.5	54.6
11	1938	145.5	54.6	49	169	18.5	54.6
12	1892	144.9	54.6	50	145	15.8	54.6
13	1843	144.1	54.6	51	124	13.3	54.6
14	1794	142.8	54.6	52	105	11.2	54.6
15	1744	141.2	54.6	53	90	9.4	54.6
16	1692	139.4	54.6	54	76	8.0	54.6
17	1640	137.4	54.6	55	64	6.8	54.6
18	1589	135.2	54.6	56	54	5.7	54.6
19	1537	132.8	54.6	57	45	4.7	54.6
20	1484	130.1	54.6	58	37	3.9	54.6
21	1430	127.2	54.6	59	30	3.2	54.6
22	1376	124.2	54.6	60	25	2.6	54.6
23	1323	121.0	54.6	61	20	2.1	54.6
24	1269	117.5	54.6	62	16	1.7	54.6
25	1216	114.0	54.6	63	12	1.3	54.6
26	1163	110.4	54.6	64	9	1.0	54.6
27	1110	106.6	54.6	65	6	0.7	54.6
28	1056	102.5	54.6	66	0	0.0	
29	1003	98.5	54.6	67	0	0.0	
30	951	94.5	54.6	68	0	0.0	
31	899	90.2	54.6	69	0	0.0	
32	848	86.0	54.6	70	0	0.0	
33	798	81.7	54.6	71	0	0.0	
34	749	77.5	54.6	72	0	0.0	
35	701	73.2	54.6	73	0	0.0	
36	654	69.0	54.6	74	0	0.0	
37	607	64.6	54.6	75	0	0.0	
38	561	60.3	54.6	76	0	0.0	
39	516	56.0	54.6	77	0	0.0	
40	473	51.7	54.6	78	0	0.0	
				79	0	0.0	

NOTES: Table shows estimated effects on jobs and real earnings of original state residents who survive and stay in state of one-time commitment of \$1 billion in present value of year 2004 dollars to economic development subsidies. Under assumptions made in report, effects are scaleable to different resource commitments to economic development subsidies.

Table 5 Jobs and Earnings Generated for State Residents From Ongoing Economic Development Program Equivalent in Size to Universal Preschool Program

Year	Jobs generated		Real earnings generated		Year	Jobs generated		Real earnings generated	
	Absolute no. of jobs per \$1 billion initial subsidy, growing w/economy	As percentage of baseline state employment	In millions of year 2004 dollars, per \$1 billion initial subsidy, growing w/economy	As percentage of baseline state wage and salary earnings		Absolute no. of jobs per \$1 billion initial subsidy, growing w/economy	As percentage of baseline state employment	In millions of year 2004 dollars, per \$1 billion initial subsidy, growing w/economy	As percentage of baseline state wage and salary earnings
3	0	0.00	0.0	0.00	41	61638	0.58	6349.5	1.09
4	4286	0.05	216.0	0.06	42	62216	0.59	6488.4	1.10
5	8148	0.09	422.7	0.12	43	62759	0.59	6625.5	1.11
6	11575	0.12	619.4	0.18	44	63269	0.59	6760.8	1.11
7	14558	0.15	805.1	0.23	45	63747	0.60	6894.4	1.12
8	17090	0.18	978.5	0.28	46	64195	0.60	7026.5	1.12
9	19163	0.20	1138.6	0.32	47	64612	0.60	7156.9	1.13
10	21203	0.22	1301.3	0.36	48	65002	0.60	7286.0	1.13
11	23204	0.24	1466.3	0.39	49	65366	0.60	7414.1	1.13
12	25166	0.26	1633.3	0.43	50	65707	0.60	7541.3	1.13
13	27084	0.28	1801.9	0.47	51	66027	0.61	7668.1	1.14
14	28959	0.30	1971.9	0.51	52	66331	0.61	7794.6	1.14
15	30790	0.31	2142.8	0.54	53	66619	0.61	7921.1	1.14
16	32574	0.33	2314.4	0.58	54	66896	0.61	8048.2	1.14
17	34313	0.35	2486.6	0.61	55	67161	0.61	8176.0	1.14
18	36004	0.36	2659.1	0.65	56	67416	0.61	8304.6	1.14
19	37649	0.38	2831.9	0.68	57	67663	0.61	8434.2	1.14
20	39247	0.40	3004.6	0.71	58	67903	0.61	8564.9	1.14
21	40795	0.41	3177.0	0.74	59	68137	0.61	8696.9	1.14
22	42294	0.42	3348.9	0.77	60	68366	0.61	8830.2	1.14
23	43743	0.44	3520.3	0.79	61	68592	0.61	8965.1	1.14
24	45144	0.45	3690.8	0.82	62	68813	0.61	9101.6	1.14
25	46496	0.46	3860.3	0.84	63	69032	0.61	9239.8	1.15
26	47798	0.47	4028.7	0.87	64	69248	0.61	9379.7	1.15
27	49052	0.48	4195.8	0.89	65	69462	0.61	9521.5	1.15
28	50255	0.49	4361.5	0.91	66	69671	0.61	9664.6	1.15
29	51409	0.50	4525.5	0.93	67	69880	0.61	9809.9	1.15
30	52514	0.51	4688.1	0.95	68	70089	0.61	9957.4	1.15
31	53571	0.52	4848.8	0.97	69	70299	0.61	10107.2	1.15
32	54580	0.53	5007.7	0.99	70	70510	0.61	10259.1	1.15
33	55542	0.54	5164.7	1.00	71	70722	0.61	10413.4	1.15
34	56458	0.55	5319.9	1.02	72	70934	0.61	10570.0	1.15
35	57329	0.55	5473.1	1.03	73	71147	0.61	10728.9	1.15
36	58154	0.56	5624.3	1.04	74	71360	0.61	10890.2	1.15
37	58935	0.56	5773.6	1.05	75	71574	0.61	11054.0	1.15
38	59673	0.57	5920.7	1.07	76	71789	0.61	11220.2	1.15
39	60368	0.57	6065.7	1.08	77	72004	0.61	11388.9	1.15
40	61023	0.58	6208.6	1.08	78	72220	0.61	11560.1	1.15
					79	72437	0.61	11733.9	1.15

NOTES: Table shows effects on jobs and real earnings of state residents of an economic development subsidy program that each year from initial year (year 4) to 75 years later (year 79) devotes resources to economic development subsidy program equal in present value to what would be cost of universal preschool program. The absolute jobs and real earnings in millions generated are for program in hypothetical state in which this economic development subsidy program had present value of \$1 billion in year 4, and grows with economy at 1.5% per year for next 75 years. This can be scaled up or down to any economic development subsidy program that grows at 1.5% annually and is in a state with typical growth and migration trends. The percentage numbers would apply to the typical state that adopted subsidy program equivalent in size to universal preschool program.

Table 6 Long-Run Effects of Traditional Economic Development Subsidies on Various Aspects of Economic Development from Each State's Perspective

State	Assumed annual economic development subsidy costs (in millions of 2004 dollars)	PV of real earnings generated divided by PV of costs	Long-run effect on number of jobs	Long-run effects on annual real earnings (in millions of 2004 dollars)	Long-run effect on annual gross state product (in millions of 2004 dollars)	Long-run effect on annual state and local tax revenue (in millions of 2004 dollars)
Alabama	179.9	3.14	12304	691.3	1606.6	126.2
Alaska	35.5	3.04	1952	131.3	376.6	26.2
Arizona	246.9	3.15	15293	953.0	2307.0	192.2
Arkansas	99.2	3.07	7307	372.2	907.7	81.7
California	1903.8	3.20	99007	7475.1	18203.2	1607.6
Colorado	250.2	3.10	13902	946.8	2262.3	173.5
Connecticut	235.8	3.12	10483	899.0	2117.9	191.5
Delaware	50.4	3.10	2672	191.0	614.4	35.1
District of Columbia	124.9	2.78	3803	418.6	768.3	36.9
Florida	759.8	3.18	49357	2959.5	6975.7	599.4
Georgia	422.6	3.17	25662	1645.0	3993.2	312.5
Hawaii	64.6	3.10	3989	245.0	570.8	55.2
Idaho	51.3	3.05	3787	191.0	484.7	41.7
Illinois	689.3	3.12	36718	2632.6	5958.7	510.5
Indiana	282.0	3.15	18555	1086.3	2620.9	219.0
Iowa	131.1	3.07	9116	492.3	1247.5	106.2
Kansas	124.9	3.05	8398	465.2	1102.1	97.8
Kentucky	168.3	3.12	11512	643.0	1558.5	138.1
Louisiana	173.9	3.16	12533	674.1	1772.1	160.7
Maine	55.0	3.13	3880	210.3	495.5	57.7
Maryland	306.1	3.14	16277	1177.1	2621.4	256.8
Massachusetts	436.2	3.12	20285	1667.9	3633.0	302.3
Michigan	490.9	3.18	28038	1912.4	4334.5	383.4
Minnesota	299.8	3.16	17090	1160.3	2590.1	239.9
Mississippi	93.3	3.08	7265	351.7	857.9	81.6
Missouri	265.2	3.12	17177	1013.8	2323.3	187.8
Montana	33.1	3.03	2569	122.5	304.0	27.1
Nebraska	82.9	3.05	5689	308.8	759.6	66.7
Nevada	123.0	3.10	7219	466.6	1137.4	88.8
New Hampshire	68.3	3.09	3909	257.7	585.4	45.7
New Jersey	528.5	3.11	24808	2012.3	4735.8	434.1
New Mexico	72.2	3.08	4995	271.6	686.6	62.7
New York	1175.0	3.10	52768	4460.3	10176.0	1126.5
North Carolina	384.8	3.18	25492	1503.1	3928.6	294.4
North Dakota	28.0	2.98	2051	101.5	245.5	21.2
Ohio	548.2	3.16	34510	2120.9	4856.5	455.4
Oklahoma	128.8	3.08	9284	485.5	1212.0	111.6
Oregon	161.6	3.12	10275	617.7	1463.9	114.4
Pennsylvania	605.3	3.15	35956	2331.7	5390.4	477.4
Rhode Island	51.5	3.08	3056	193.9	469.6	45.9
South Carolina	167.0	3.15	11935	644.3	1570.3	125.2
South Dakota	30.9	3.01	2350	113.4	322.0	23.0

Table 6 (Continued)

State	Assumed annual economic development subsidy costs (in millions of 2004 dollars)	PV of real earnings generated divided by PV of costs	Long-run effect on number of jobs	Long-run effects on annual real earnings (in millions of 2004 dollars)	Long-run effect on annual gross state product (in millions of 2004 dollars)	Long-run effect on annual state and local tax revenue (in millions of 2004 dollars)
Tennessee	264.5	3.15	17346	1021.6	2513.3	170.4
Texas	1035.9	3.22	62919	4096.4	10452.6	795.0
Utah	101.5	3.16	7151	392.5	955.0	78.1
Vermont	28.5	3.08	1919	107.0	246.2	24.9
Virginia	424.5	3.13	23505	1628.0	3775.6	289.3
Washington	324.8	3.15	18132	1255.1	3021.4	252.0
West Virginia	60.0	3.03	4308	221.4	545.6	56.1
Wisconsin	269.9	3.17	17949	1050.7	2462.4	241.8
Wyoming	22.9	2.96	1534	82.4	258.1	23.1
United States	14662.2	3.14	847995	56472.7	134377.8	11672.6

NOTES: Annual economic development subsidy costs are assumed to equal the costs of universal preschool. This is calibrated, as mentioned in Table 2, based on the assumption that costs of such a universal program will be 0.299% of state wage and salary earnings. The present value of real earnings generated is calculated by adjusting the state's PV based on the state residents' tendency to move out compared to the average state. Specifically, as shown in Chapter 5, the PV ratio in an "average state" without migration would be 3.36, whereas in a state with normal out-migration this PV ratio is 3.14. To adjust for differential out-migration, we multiply 3.36 by the following ratio: $3.36[1 - ((3.36 - 3.14) / 3.36) \times R]$, where $R = (\% \text{ born in state who leave state}) / (\% \text{ born in U.S. who leave state})$. The percentage born in state who leave the state is based on the 2000 U.S. Census Public Use Microdata files, which reports both birth state and current state of residence, and is based on all U.S. residents born in the U.S. The effect on jobs is based on the percentage effects on jobs reported in Table 5 as of year 79, that is after the program has been in effect for 75 years. However, the percentage effect is calculated by considering the state's wage and salary employment as of 2004. Therefore, this effect on jobs should be considered the effects that would have occurred as of 2004 if the program had been in effect for 75 years. The percentage effect is also adjusted for differential out-migration in each state. That is, as the long-run percentage effect on employment after 75 years without out-migration is calculated to be 0.66%, and the average U.S. effect with out-migration is calculated to be 0.61%, the percentage effect actually used is adjusted by $\% \text{ job effect} = 0.66[1 - ((0.66 - 0.61) / 0.66) \times R]$. The effect on earnings is also derived from the percentage effect on real earnings after 75 years in Table 5 of 1.15%, and wage and salary earnings in the state as of 2004. Wage and salary earnings is, however, adjusted to wage and salary employment so that the average hourly wage rate is \$17.67 per hour, as explained in Table 2. Each state's percentage effect is also adjusted based on the state's differential out-migration. Long-run percentage effects on real earnings without any out-migration is 1.24%, whereas the typical state's percentage effect, based on Table 5, is 1.15%. Therefore the percentage effect on real earnings in each state is equal to $1.24[1 - ((1.24 - 1.15) / 1.24) \times R]$. The effects on real gross state product are based on the ratio of each state's GSP to real wage and salary earnings. Figures for state GSP are for 2004 and come from the Regional Economic Information System of the Bureau of Economic Analysis of the U.S. Department of Commerce. Figures for state and local tax revenue are based on the ratio of state and local tax revenue to GSP in each state. State and local tax revenues are for fiscal year 2001–2002, and come from the U.S. Census of Governments; real GSP figures for this ratio are for 2002. The U.S. totals sum what each state perceives as the effects on its jobs, earnings, GSP, and tax revenue. These U.S. totals do not reflect the actual national effects after allowing for spillover effects on other states of each state's economic development program, as explained in Chapter 5.

Table 7 Assumptions About How Enrollment in Universal Preschool is Divided Among Different Groups

How group affected by universal preschool program	High risk: < \$30K family income	Medium risk: \$30K–\$50K family income	Low risk: > \$50K family income	Total all risk groups
In public now, none otherwise	5% of 4-year-olds (100% benefits)	3% (50%)	7% (25%)	15%
In public now, lower-cost public otherwise	12% (50%)	8% (25%)	13% (0%)	33%
In public now, private otherwise	1% (0%)	3% (0%)	18% (0%)	22%
Total in public program now	18%	14%	38%	70%
Private now	1%	2%	7%	10%
Total in preschool now	19%	16%	45%	80%
None now	6%	4%	10%	20%
Total in risk group	25%	20%	55%	100%

NOTES: The first number in each cell is the percentage of all 4-year-olds in that cell. The number in parentheses in the nine cells in upper quadrant is the percentage of benefits the group in that cell gets, compared to the benefits assumed for high-risk group members who otherwise would not be in any preschool. The columns divide up all 4-year-olds by their family income. The rows divide up all 4-year-olds by whether they are enrolled in the universal preschool program, and by what type of preschool, if any, they would have been enrolled in if the universal preschool program did not exist. For example, for the high risk group, the bottom row shows that this group constitutes 25% of all 4-year-olds. The top row for the high risk group shows that 5% of all 4-year-olds are high-risk group members who enroll in the universal preschool program, but would otherwise have not been enrolled in preschool; we can calculate from these numbers that this 5% of all 4-year-olds is 20% ($= 5\% / 25\%$) of all high-risk 4-year-olds.

Table 8 Balanced Budget Multiplier Effects of Permanent Universal Preschool Education Program on Jobs and Earnings in a State's Economy

Year	Job generation effects		Real earnings generation effects		Year	Job generation effects		Real earnings generation effects	
	Per \$1 billion of initial state spending	As % of baseline employment in state	In millions of 2004 dollars, per \$1 billion of initial state spending	As % of baseline real earnings in state		Per \$1 billion of initial state spending	As % of baseline employment in state	In millions of 2004 dollars, per \$1 billion of initial state spending	As % of baseline real earnings in state
3	0	0.000	0.0	0.000	41	492	0.005	53.1	0.009
4	3425	0.036	172.6	0.052	42	462	0.004	50.1	0.008
5	3085	0.033	163.1	0.048	43	434	0.004	47.3	0.008
6	2738	0.029	153.1	0.044	44	408	0.004	44.6	0.007
7	2384	0.025	142.4	0.041	45	382	0.004	41.9	0.007
8	2023	0.021	130.9	0.037	46	357	0.003	39.4	0.006
9	1657	0.017	118.5	0.033	47	334	0.003	36.8	0.006
10	1629	0.017	119.1	0.033	48	311	0.003	34.6	0.005
11	1600	0.017	119.4	0.032	49	291	0.003	32.5	0.005
12	1567	0.016	119.4	0.032	50	272	0.003	30.6	0.005
13	1533	0.016	119.1	0.031	51	256	0.002	28.9	0.004
14	1498	0.015	118.5	0.031	52	243	0.002	27.6	0.004
15	1463	0.015	117.6	0.030	53	231	0.002	26.4	0.004
16	1426	0.015	116.6	0.029	54	221	0.002	25.6	0.004
17	1389	0.014	115.4	0.028	55	212	0.002	24.9	0.003
18	1352	0.014	114.0	0.028	56	204	0.002	24.4	0.003
19	1314	0.013	112.5	0.027	57	197	0.002	23.9	0.003
20	1276	0.013	110.8	0.026	58	192	0.002	23.6	0.003
21	1237	0.012	108.9	0.025	59	187	0.002	23.3	0.003
22	1198	0.012	107.0	0.024	60	183	0.002	23.2	0.003
23	1158	0.012	104.8	0.024	61	180	0.002	23.1	0.003
24	1119	0.011	102.5	0.023	62	177	0.002	23.1	0.003
25	1080	0.011	100.1	0.022	63	175	0.002	23.1	0.003
26	1041	0.010	97.5	0.021	64	173	0.002	23.2	0.003
27	1002	0.010	94.9	0.020	65	171	0.002	23.3	0.003
28	962	0.009	92.1	0.019	66	167	0.001	23.1	0.003
29	922	0.009	89.3	0.018	67	167	0.001	23.4	0.003
30	883	0.009	86.5	0.018	68	168	0.001	23.8	0.003
31	844	0.008	83.5	0.017	69	168	0.001	24.2	0.003
32	806	0.008	80.5	0.016	70	169	0.001	24.5	0.003
33	769	0.007	77.4	0.015	71	169	0.001	24.9	0.003
34	732	0.007	74.4	0.014	72	170	0.001	25.3	0.003
35	695	0.007	71.4	0.013	73	170	0.001	25.6	0.003
36	660	0.006	68.4	0.013	74	171	0.001	26.0	0.003
37	624	0.006	65.3	0.012	75	171	0.001	26.4	0.003
38	589	0.006	62.2	0.011	76	172	0.001	26.8	0.003
39	556	0.005	59.1	0.010	77	172	0.001	27.2	0.003
40	523	0.005	56.0	0.010	78	173	0.001	27.6	0.003
					79	173	0.001	28.0	0.003

NOTES: Effects shown are the net effects on the employment and real earnings of the original state residents of a permanent universal preschool program. Two of the columns show absolute numbers of jobs generated and real earnings generated, for a program scaled to \$1 billion in initial net new state spending and taxes, which will then grow at 1.5% per year. This can be rescaled to whatever spending is required in another state. The other two columns show effects as a percentage of baseline employment and real earnings in the state of a universal preschool program with the characteristics assumed under the baseline assumptions of this report. This should be valid for all states that adopt a universal preschool program with similar spending per participant and offsets from displaced public spending and private spending as are assumed here. For characteristics of a state's program with radically different spending per participant or offsets, the figures per \$1 billion should be rescaled to the required net new public spending for that state's program, and the percentage numbers should be recalculated based on that state's total employment and earnings.

Table 9 Preschool's Effects in Generating Jobs and Earnings for State Residents By Increasing the Labor Supply of Parents by Providing Free Child Care to Participants

Year	Jobs generated via free child care per \$1 billion of spending on universal preschool	Real earnings generated via free child care, in millions of 2004 \$, per \$1 billion of spending on universal preschool	Year	Jobs generated via free child care per \$1 billion of spending on universal preschool	Real earnings generated via free child care, in millions of 2004 \$, per \$1 billion of spending on universal preschool
3	0	0.0	41	2709	91.4
4	2425	52.6	42	2717	92.7
5	2432	53.4	43	2726	94.1
6	2440	54.2	44	2734	95.6
7	2447	55.0	45	2742	97.0
8	2454	55.8	46	2750	98.4
9	2462	56.7	47	2758	99.9
10	2469	57.5	48	2767	101.4
11	2476	58.4	49	2775	103.0
12	2484	59.3	50	2783	104.5
13	2491	60.2	51	2792	106.1
14	2499	61.1	52	2800	107.7
15	2506	62.0	53	2808	109.3
16	2514	62.9	54	2817	110.9
17	2521	63.9	55	2825	112.6
18	2529	64.8	56	2834	114.3
19	2536	65.8	57	2842	116.0
20	2544	66.8	58	2851	117.8
21	2552	67.8	59	2859	119.5
22	2559	68.8	60	2868	121.3
23	2567	69.8	61	2877	123.1
24	2575	70.9	62	2885	125.0
25	2582	72.0	63	2894	126.9
26	2590	73.0	64	2902	128.8
27	2598	74.1	65	2911	130.7
28	2606	75.3	66	2920	132.7
29	2614	76.4	67	2929	134.7
30	2621	77.5	68	2937	136.7
31	2629	78.7	69	2946	138.8
32	2637	79.9	70	2955	140.9
33	2645	81.1	71	2964	143.0
34	2653	82.3	72	2973	145.1
35	2661	83.5	73	2982	147.3
36	2669	84.8	74	2991	149.5
37	2677	86.1	75	3000	151.8
38	2685	87.4	76	3009	154.0
39	2693	88.7	77	3018	156.4
40	2701	90.0	78	3027	158.7
			79	3036	161.1

NOTES: The effects generated as a percentage of state jobs and earnings are constant for all years from year 4 to year 79. The job effect of free child care, as a percentage of total state jobs, is 0.0256%. The earnings generation effect of free child care, as a percentage of total state earnings, is 0.0157%. The earnings generation effect is particularly low because of the assumption that the wage paid to the additional labor supply is at the 40th percentile of women's wages, which is considerably below overall average wages.

Table 10 Observed and Projected Effects of Perry Preschool and Chicago Child-Parent Centers on Educational Attainment

	Aged 19			Aged 20			Aged 21			Aged 27			Aged 40		
	Program group	Control group	Difference	Program group	Control group	Difference	Program group	Control group	Difference	Program group	Control group	Difference	Program group	Control group	Difference
Perry Preschool															
HS dropout %	33	55	-22							28	47	-19	23	40	-17
HS only %	67	45	22							69	51	18	68	55	13
Assocs or bachelors degree %	0	0	0							3	2	1	9	5	4
Chicago CPC															
HS dropout %				50	62	-12	38	49	-11	(37)	(47)	(-10)	(31)	(40)	(-9)
HS only %				50	38	12	62	51	11	(60)	(51)	(9)	(61)	(55)	(6)
Assocs or bachelors degree %				0	0	0	0	0	0	(3)	(2)	(1)	(8)	(5)	(3)

NOTE: The figures in the cells are percentages of the control and program groups for the Perry Preschool program, and the Chicago CPC program, at different ages in various educational attainment categories. The numbers without parentheses are actually observed percentages. The numbers in parentheses are projections by this author. The data for the Perry Preschool program reported here for ages 19, 27, and 40 are based on the program statistics presented on Table 3.1 on p. 52 of Schweinhart et al., 2005. The data for ages 20 and 21 for the Chicago Child Parent Center are based on the program statistics presented on Table 4 on p. 279 of Reynolds et al. 2002. The figures presented in this table for a CPC-like program for ages 27 and 40 are the author's projections based on the Perry Preschool figures and the CPC figures. The high school drop-out results for a CPC-like program for ages 27 and 40 are based on the observation that the effects of the CPC program on high school dropouts at ages 20 and 21 average a little more than half of the Perry Preschool program's effects on high school dropouts at age 19. Carrying this differential forward into the future, I assume that a CPC-like program would have a little more than half of the Perry Preschool program's effects on high school dropouts as of ages 27 and 40, which implies, as seems reasonable, that the program's effects in reducing high school dropouts decay somewhat over time as the control group catches up to the program group. In addition, because the average high school dropout rates for the CPC control group at ages 20 and 21 are similar to those of the Perry Preschool control group at age 19, I assume that the control group for a CPC-like program would have similar dropout rates to the Perry Preschool program at ages 27 and 40. This also makes sense because both programs target a similar disadvantaged clientele, and by definition the control group has not experienced either program. Finally, associate/bachelor degrees projected percentages for a CPC-like program at ages 27 and 40 are derived by assuming that the Perry Preschool data on the proportion of high school graduates completing one of these college degrees for the program and control groups, respectively, can be applied to the projected CPC data. For example, at age 40, $9 / 77 = 11.7\%$ of all Perry program group high school graduates had succeeded in getting an associates' degree or bachelors' degree. Multiplying 11.7% times the projected percentage of high school graduates at age 40 for a CPC-like program of 69% yields a projection of 8%.

Table 11 Employment Rate Effects of the Perry Preschool Program: Predicted Effects Based on the Program's Effects on Educational Attainment, Versus Actual Employment Rate Effects

Age		Predicted employment rate based on educational attainment	Actual observed employment rate	Actual difference minus predicted difference	Unpredicted difference as percent of predicted control group employment rate	Extrapolation of unpredicted employment difference as percent of control group for CPC program
19	Program group	51.7	50			
	Control group	49.0	32			
	Difference	2.7	18	15.3	31.2	15.6
27	Program group	72.5	69			
	Control group	70.1	56			
	Difference	2.4	13	10.6	15.2	7.7 (half of average of 15.2 and 15.8)
40	Program group	77.8	76			
	Control group	75.8	62			
	Difference	2.0	14	12.0	15.8	

NOTE: This table first predicts employment rates of program and control group in Perry Preschool program at different ages based on proportion of program and control group in three educational attainment categories (high school dropout, high school degree but no higher degree, associates' degree or higher) and data from 2004 Current Population Survey Outgoing Rotation Group on average employment rate, at time of monthly survey, of individuals in each of these three educational attainment groups. The table also reports the employment rates actually observed for the program and control groups. Educational attainment figures for Perry Preschool come from Table 3.1, p. 52, Schweinhart et al. (2005). Employment rate figures for Perry Preschool come from Table 4.1, p. 74, Schweinhart et al. (2005) and from Barnett (1993). As seen in the table, the observed employment rate difference between program and control groups is much larger than would be predicted based on educational attainment. All of the greater observed difference occurs because the Perry control group has a lower employment rate than would be predicted based on its economic status, which presumably reflects the disadvantaged status of this target group. The Perry program group has a very similar employment rate to what would be predicted based on educational attainment. The implication is that the program allows participants to overcome the disadvantages in employability due to their disadvantaged background and obtain jobs similarly to their educational counterparts. The table also calculates the unpredicted employment rate difference as a percent of the control group predicted mean. I also calculate half of this difference as a percentage of the control group mean. For calculations of employment rate effects of a CPC-like preschool education program, I assume that its unpredicted employment rate effects should be about half the size of those of the Perry Preschool program, as the CPC program has about half the educational attainment effects of the Perry Preschool program. In the simulations, this is operationalized by adjusting down the predicted control group employment rates by the percentage in the right most column. The first percentage given is used from ages 16–19, the second percentage given is used for ages 27 and up, and for years in-between 19 and 27, the percentage is assumed to change at a steady rate. One advantage of this simulation is that the extra employment rate shock of preschool education is assumed to gradually decline as control group employment rates decline with age, which seems reasonable.

Table 12 Percentage Living in Same State, Different Educational Attainment Groups, Based on Data from the Panel Survey on Income Dynamics and Census PUMS data

Age	PSID: % living in same state as at age 4				PUMS: % living in same state as state of birth				Ratio of PSID weighted average to PUMS weighted average	PUMS figure adjusted to PSID concept of percentage in state as lived in at age 4, with adjustment using average observed ratio
	Education < 12 years	Education 12–15 years	Education > 16 years	Weighted average based on distribution of education in preschool program group at each age	High school dropouts	High school degree but no higher degree	Associates degree or higher	Weighted average based on distribution of education in preschool program group at each age		
16	85.8	87.3	84.0	85.8	78.7	74.5	69.6	78.7	109.0	85.1
17	87.7	87.7	82.8	87.7	79.1	78.1	70.7	79.1	110.9	85.5
18	85.2	85.2	82.9	85.2	78.4	71.2	73.0	78.4	108.6	84.8
19	84.8	85.9	87.1	85.3	78.4	70.6	70.7	74.5	114.6	80.5
20	81.9	83.9	88.4	82.9	77.8	70.9	71.2	74.4	111.5	80.4
21	79.2	82.9	89.6	81.5	76.9	70.0	70.0	72.6	112.3	78.5
22	79.0	80.4	81.4	79.9	77.0	70.0	65.3	72.6	110.0	78.5
23	85.5	79.2	71.3	81.5	76.0	70.7	63.0	72.6	112.2	78.5
24	86.3	77.3	65.2	80.5	76.2	70.8	61.2	72.6	110.8	78.5
25	88.7	77.0	61.0	81.1	74.8	69.6	59.2	71.3	113.6	77.1
26	84.2	75.5	58.6	78.3	75.4	69.3	58.4	71.3	109.8	77.0
27	82.1	75.0	57.5	77.1	74.3	69.2	57.7	70.8	109.0	76.5
28	82.6	73.5	58.0	76.3	73.3	68.7	57.1	70.0	109.0	75.7
29	84.6	74.6	60.6	77.7	73.9	68.3	56.1	69.9	111.1	75.6
30	74.3	71.8	59.2	72.2	72.4	67.7	55.6	68.9	104.8	74.4
31	80.4	72.0	61.4	74.5	72.0	67.6	55.4	68.6	108.6	74.1
32	82.8	70.9	59.1	74.4	72.2	67.8	55.0	68.7	108.3	74.3
33	87.3	76.0	60.5	79.0	72.7	67.5	55.1	68.6	115.1	74.2
34	62.1	70.8	57.2	67.1	72.0	67.6	54.9	68.3	98.2	73.8
35	86.7	73.0	57.7	76.6	71.6	66.6	54.6	67.6	113.5	73.0
36	46.1	69.9	54.2	61.1	71.9	66.2	53.6	67.2	90.8	72.7
37	83.9	70.2	52.4	73.4	70.7	66.1	53.3	66.7	110.0	72.1
38	42.6	62.6	54.9	55.6	71.4	66.4	53.6	67.1	82.9	72.5
39	75.3	73.8	57.5	73.0	70.7	66.3	53.3	66.7	109.6	72.1
40					70.7	66.2	53.1	66.5		71.9
41					70.3	66.2	52.6	66.4		71.7
42					70.5	65.9	52.9	66.3		71.6
43					70.6	66.0	52.4	66.3		71.7
44					71.1	65.8	52.0	66.4		71.7
45					69.6	65.2	51.9	65.5		70.8
46					70.2	64.7	52.2	65.4		70.7
47					69.9	64.8	51.8	65.4		70.6
48					69.9	64.4	51.7	65.1		70.4
49					69.1	64.0	51.6	64.6		69.8
50					69.1	63.4	51.7	64.2		69.4
51					68.9	63.5	50.9	64.2		69.4
52					69.0	62.6	49.7	63.5		68.7
53					67.8	62.5	48.6	63.0		68.1

Table 12 (Continued)

Age	PSID: % living in same state as at age 4				PUMS: % living in same state as state of birth				Ratio of PSID weighted average to PUMS weighted average	PUMS figure adjusted to PSID concept of percentage in state as lived in at age 4, with adjustment using average observed ratio
	Education < 12 years	Education 12–15 years	Education > 16 years	Weighted average based on distribution of education in preschool program group at each age	High school dropouts	High school degree but no higher degree	Associates degree or higher	Weighted average based on distribution of education in preschool program group at each age		
54					67.6	60.9	46.6	61.9		66.9
55					67.5	59.5	44.9	60.8		65.7
56					66.0	60.3	45.0	60.9		65.8
57					66.8	60.5	45.4	61.2		66.2
58					67.0	60.7	45.7	61.5		66.4
59					64.8	60.0	44.9	60.3		65.2
60					66.2	59.9	45.7	60.7		65.6
61					65.4	59.6	45.3	60.3		65.2
62					65.5	60.0	45.7	60.6		65.5
63					65.0	59.7	44.6	60.1		65.0
64					65.3	59.6	44.8	60.2		65.0
65					64.6	59.0	45.4	59.6		64.4
66					64.8	59.2	45.3	59.8		64.6
67					64.9	58.8	45.6	59.6		64.4
68					64.7	59.5	44.7	59.9		64.8
69					64.9	58.8	45.8	59.7		64.5
70					64.8	58.7	45.6	59.6		64.4
71					64.9	58.4	45.3	59.3		64.1
72					64.8	58.6	44.7	59.4		64.2
73					64.2	58.0	45.1	58.9		63.6
74					64.5	58.0	44.6	58.9		63.7
75					65.1	57.1	45.4	58.6		63.4
76					64.8	57.1	43.6	58.4		63.1
77					64.6	56.8	45.1	58.3		63.0
78					64.7	56.4	43.8	58.0		62.7
79					64.5	56.6	44.2	58.1		62.8

NOTES: This table reports data from two sources on how inclined different educational groups are to stay in same state as they lived in at an early age. From the Panel Survey of Income Dynamics, I follow all 4-year-olds in the broadly representative Survey Research Center sample in the years 1968–1977 until they are last observed in the PSID, which currently runs until 2003. Given the years in which I select 4-year-olds, the sample size starts to rapidly fall off after age 30; (the 4-year-olds in 1977 are age 30 in 2003, so observations on 31-year-olds only use 4-year-olds from 1968–1976, etc. Total sample size is 953 as of age 16, drops off gradually with sample attrition to 578 at age 30, and then rapidly declines to zero by age 40. For three different educational attainment groups, I calculate the weighted percentage (using sample weights) of individuals at different ages who live in the same state they lived in at age 4. Educational attainment is measured based on last observed educational attainment. The educational classifications are based on total years of education completed, which does not exactly correspond to the categorization by degree attainment. I use 16 years or greater as the cutoff for education beyond college because the available information indicates that most individuals with from 13 to 15 years of completed education do not have an associates degree or a bachelors degree. For example, according to Table 1, p. 37 in Kominski and Siegel (1993), only 36% of those individuals with 14 completed years of school have an associates degree or higher, and only 28% of those with 15 completed years of school has an associates degree or higher. In contrast, 95% of those with 16 years of school have an associates degree or higher. For PSID individuals who eventually graduate from college, the percentage living in same state as they lived in at age 4 is artificially inflated for the college attendance years, as the PSID does not record information on members of household who are away attending college. However, the PSID does collect data on these individuals again once they complete college. The weighted average of percentage of individuals staying in the same state is based on the proportion of individuals in the preschool program group in the simulation in

Table 12 (Continued)

each of the three education groups; I calculate this for the PSID knowing that the three educational groups are not defined exactly the same. The Census PUMS data comes from the year 2000, and is based on cross section data for that year. I calculate using sample weights the percentage of individuals of different ages who live in the same state they were born in, for three different educational attainment groups defined by degree attainment. The weighted percentage for the overall PUMS sample is again based on the assumptions made about the percentages of individuals in each educational attainment category in the preschool program group. The PUMS data is of course a cross section that does not follow any individual over time, but should give a reasonable guide to how mobility varies over time with age if mobility dynamics have not changed dramatically for different cohorts. One would expect, and I in fact find, that a somewhat lower percentage of individuals in the PUMS are in the same state than the corresponding percentage in the PSID, as the PSID measures mobility since age 4, and the PUMS measures mobility since birth, and some individuals will move between birth and age 4. On average, the PSID reported percentages are larger by a factor of 1.081 than the PUMS figures. The PSID figures correspond more closely to what we want to measure, which is the percentage of individuals who live in the same state as the state they attended preschool at age 4. However, the PSID figures only go to age 39, whereas for the simulation we need to know something about mobility throughout the individual's working life. In addition, the PSID numbers are in some cases based on very small sample sizes. Therefore, for the simulations used in the model, I assume that the percentage of individuals in the same state that they attended preschool in at age 4 is given by blowing up the PUMS percentages by the average ratio of the PSID numbers to the PUMS numbers. As the Table shows, the percentage in their "preschool state" declines from 85% as of age 16 to a low of 63% as of age 79. The decline in "same as preschool state" residence is somewhat faster in the late teens and early 20s, presumably as individuals relocate after finishing school, slows down a bit from the mid-20s to about age 50, and then speeds up again in the early 50s before slowing down again. The somewhat greater mobility in the early 50s could reflect some early retirement behavior, or could be a reflection of cohort effects associated with World War II and its aftermath. In any event, the upshot from this table is that mobility rates out of state are much lower than I think most high-powered academics or media pundits would assume, probably because the mobility rates of the average college graduate are quite different from those of the most elite college graduates.

Table 13 Job Generation Effects of One-Time Preschool Program of \$1 Billion, Compared to Job Generation Effects of One-Time Economic Development Subsidy Program of \$1 Billion

Year	Jobs created for local residents from one-time universal preschool program costing \$1 billion	Jobs created for local residents from one-time economic development program with present value of \$1 billion	Year	Jobs created for local residents from one-time universal preschool program costing \$1 billion	Jobs created for local residents from one-time economic development program with present value of \$1 billion
3	0	0	41	2824	432
4	0	4286	42	2829	393
5	0	3849	43	2692	356
6	0	3403	44	2760	322
7	0	2949	45	2791	288
8	0	2488	46	2753	256
9	0	2022	47	2705	225
10	0	1982	48	2694	196
11	0	1938	49	2600	169
12	0	1892	50	2588	145
13	0	1843	51	2573	124
14	0	1794	52	2485	105
15	0	1744	53	2456	90
16	1460	1692	54	2366	76
17	2317	1640	55	2105	64
18	2855	1589	56	2136	54
19	4018	1537	57	2004	45
20	4110	1484	58	2013	37
21	4004	1430	59	1894	30
22	4028	1376	60	1660	25
23	3902	1323	61	1524	20
24	3790	1269	62	1397	16
25	3339	1216	63	1213	12
26	3183	1163	64	1179	9
27	2821	1110	65	834	6
28	2829	1056	66	793	0
29	2804	1003	67	665	0
30	2856	951	68	642	0
31	2771	899	69	509	0
32	2786	848	70	465	0
33	2762	798	71	372	0
34	2860	749	72	355	0
35	2724	701	73	270	0
36	2685	654	74	193	0
37	2692	607	75	179	0
38	2709	561	76	193	0
39	2733	516	77	159	0
40	2673	473	78	145	0
			79	125	0

NOTES: It is important to remember that these are not effects of permanent preschool program or economic development program, but rather the effects of during one year, dated as year 4, of either spending \$1 billion on preschool or offering economic development subsidies whose present value is \$1 billion. These effects can be scaled up or down to programs of different size as long as these programs are similar in their design to the modeled programs.

Table 14 Real Earnings Generation Effects of One-Time Preschool Program of \$1 Billion, Compared to Real Earnings Generation Effects of One-Time Economic Development Subsidy Program of \$1 Billion

Year	Real earnings effects on participants and peers of one-time universal preschool program of \$1 billion	Real earnings effects of one-time economic development program with present value cost of \$1 billion	Year	Real earnings effects on participants and peers of one-time universal preschool program of \$1 billion	Real earnings effects of one-time economic development program with present value cost of \$1 billion
3	0.0	0.0	41	196.7	47.6
4	0.0	216.0	42	205.5	43.4
5	0.0	203.5	43	201.5	39.5
6	0.0	190.3	44	207.4	35.7
7	0.0	176.3	45	210.6	31.9
8	0.0	161.4	46	202.4	28.3
9	0.0	145.4	47	214.2	24.8
10	0.0	145.6	48	208.9	21.5
11	0.0	145.5	49	205.9	18.5
12	0.0	144.9	50	212.5	15.8
13	0.0	144.1	51	210.9	13.3
14	0.0	142.8	52	208.3	11.2
15	0.0	141.2	53	205.1	9.4
16	23.5	139.4	54	196.2	8.0
17	39.1	137.4	55	179.3	6.8
18	52.7	135.2	56	186.2	5.7
19	84.9	132.8	57	173.9	4.7
20	93.2	130.1	58	170.9	3.9
21	99.5	127.2	59	161.2	3.2
22	106.7	124.2	60	145.8	2.6
23	113.4	121.0	61	131.2	2.1
24	118.4	117.5	62	123.0	1.7
25	123.6	114.0	63	105.1	1.3
26	129.2	110.4	64	100.9	1.0
27	120.9	106.6	65	69.6	0.7
28	129.1	102.5	66	60.5	0.0
29	132.8	98.5	67	51.4	0.0
30	136.3	94.5	68	52.5	0.0
31	139.7	90.2	69	40.1	0.0
32	151.8	86.0	70	38.9	0.0
33	151.2	81.7	71	30.9	0.0
34	160.1	77.5	72	26.1	0.0
35	164.7	73.2	73	22.7	0.0
36	167.8	69.0	74	18.3	0.0
37	180.1	64.6	75	13.5	0.0
38	183.2	60.3	76	14.7	0.0
39	189.2	56.0	77	13.8	0.0
40	188.9	51.7	78	11.6	0.0
			79	9.9	0.0

NOTES: It is important to remember that these are not effects of permanent preschool program or economic development program, but rather the effects of during one year, dated as year 4, of either spending \$1 billion on preschool or offering economic development subsidies whose present value is \$1 billion. These effects can be scaled up or down to programs of different size as long as these programs are similar in their design to the modeled programs.

Table 15 Jobs Generated for State Residents by Permanent Universal Preschool Program, Compared to Jobs Generated by Economic Development Subsidy Program of Same Cost

Year	Jobs generated per \$1 billion of initial resources		Jobs generated as % of baseline employment		Year	Jobs generated per \$1 billion of initial resources		Jobs generated as % of baseline employment	
	Preschool program	Economic Development program	Preschool program	Economic Development program		Preschool program	Economic Development program	Preschool program	Economic Development program
3	0	0	0.00	0.00	41	81662	61638	0.77	0.58
4	0	4286	0.00	0.05	42	84735	62216	0.80	0.59
5	0	8148	0.00	0.09	43	87682	62759	0.82	0.59
6	0	11575	0.00	0.12	44	90705	63269	0.85	0.59
7	0	14558	0.00	0.15	45	93768	63747	0.88	0.60
8	0	17090	0.00	0.18	46	96802	64195	0.90	0.60
9	0	19163	0.00	0.20	47	99798	64612	0.93	0.60
10	0	21203	0.00	0.22	48	102791	65002	0.95	0.60
11	0	23204	0.00	0.24	49	105700	65366	0.98	0.60
12	0	25166	0.00	0.26	50	108604	65707	1.00	0.60
13	0	27084	0.00	0.28	51	111503	66027	1.02	0.61
14	0	28959	0.00	0.30	52	114323	66331	1.05	0.61
15	0	30790	0.00	0.31	53	117122	66619	1.07	0.61
16	1460	32574	0.01	0.33	54	119839	66896	1.09	0.61
17	3782	34313	0.04	0.35	55	122304	67161	1.11	0.61
18	6648	36004	0.07	0.36	56	124807	67416	1.13	0.61
19	10686	37649	0.11	0.38	57	127185	67663	1.15	0.61
20	14827	39247	0.15	0.40	58	129580	67903	1.16	0.61
21	18876	40795	0.19	0.41	59	131862	68137	1.18	0.61
22	22960	42294	0.23	0.42	60	133918	68366	1.20	0.61
23	26931	43743	0.27	0.44	61	135844	68592	1.21	0.61
24	30801	45144	0.31	0.45	62	137648	68813	1.22	0.61
25	34232	46496	0.34	0.46	63	139274	69032	1.23	0.61
26	37518	47798	0.37	0.47	64	140871	69248	1.24	0.61
27	40452	49052	0.40	0.48	65	142127	69462	1.25	0.61
28	43402	50255	0.43	0.49	66	143346	69671	1.26	0.61
29	46336	51409	0.45	0.50	67	144442	69880	1.26	0.61
30	49331	52514	0.48	0.51	68	145517	70089	1.27	0.61
31	52250	53571	0.51	0.52	69	146462	70299	1.27	0.61
32	55193	54580	0.54	0.53	70	147366	70510	1.28	0.61
33	58121	55542	0.56	0.54	71	148179	70722	1.28	0.61
34	61155	56458	0.59	0.55	72	148979	70934	1.28	0.61
35	64063	57329	0.62	0.55	73	149696	71147	1.29	0.61
36	66939	58154	0.64	0.56	74	150339	71360	1.29	0.61
37	69832	58935	0.67	0.56	75	150969	71574	1.29	0.61
38	72750	59673	0.69	0.57	76	151615	71789	1.29	0.61
39	75702	60368	0.72	0.57	77	152229	72004	1.29	0.61
40	78602	61023	0.75	0.58	78	152831	72220	1.29	0.61
					79	153414	72437	1.29	0.61

NOTES: The jobs generated for the preschool program are solely due to effects on participants and peers, and omit all other avenues of influence of preschool programs on job generation (e.g., balanced budget multiplier, labor supply of parents, etc.) Jobs generated are for a hypothetical universal preschool program that initially spends an additional \$1 billion per year, versus devoting the same present value per year to economic development subsidies. This program then grows in costs with the economy by 1.5% per year. These jobs generation numbers can be scaled up or down with size of program in a particular state, as long as design of programs is similar to what is assumed here, even if the program is not a universal preschool program. The jobs generated as % of baseline employment compares jobs generated to total state baseline employment, which is assumed to grow at 0.3% per year. These percentage figures for preschool would apply to any state that adopted a universal preschool program of this design, or an economic development program that had same cost as such a program.

Table 16 Real Earnings Generated for State Residents by Permanent Universal Preschool Program, Compared to Real Earnings Generated by Economic Development Subsidy Program of Same Cost

Year	Real earnings generated in millions of 2004 \$, per \$1 billion of initial resources		Earnings generated as % of baseline state earnings		Year	Real earnings generated in millions of 2004 \$, per \$1 billion of initial resources		Earnings generated as % of baseline state earnings	
	Preschool program	Economic development program	Preschool program	Economic development program		Preschool program	Economic development program	Preschool program	Economic development program
3	0.0	0	0.00	0.00	41	3941.3	6349.5	0.68	1.09
4	0.0	216.0	0.00	0.06	42	4206.1	6488.4	0.71	1.10
5	0.0	422.7	0.00	0.12	43	4470.9	6625.5	0.75	1.11
6	0.0	619.4	0.00	0.18	44	4745.5	6760.8	0.78	1.11
7	0.0	805.1	0.00	0.23	45	5027.4	6894.4	0.82	1.12
8	0.0	978.5	0.00	0.28	46	5305.4	7026.5	0.85	1.12
9	0.0	1138.6	0.00	0.32	47	5599.4	7156.9	0.88	1.13
10	0.0	1301.3	0.00	0.36	48	5892.5	7286.0	0.91	1.13
11	0.0	1466.3	0.00	0.39	49	6186.9	7414.1	0.94	1.13
12	0.0	1633.3	0.00	0.43	50	6492.5	7541.3	0.98	1.13
13	0.0	1801.9	0.00	0.47	51	6800.9	7668.1	1.01	1.14
14	0.0	1971.9	0.00	0.51	52	7111.5	7794.6	1.04	1.14
15	0.0	2142.8	0.00	0.54	53	7423.4	7921.1	1.07	1.14
16	23.5	2314.4	0.01	0.58	54	7731.3	8048.2	1.10	1.14
17	63.0	2486.6	0.02	0.61	55	8026.9	8176.0	1.12	1.14
18	116.6	2659.1	0.03	0.65	56	8333.8	8304.6	1.15	1.14
19	203.3	2831.9	0.05	0.68	57	8633.0	8434.2	1.17	1.14
20	299.5	3004.6	0.07	0.71	58	8933.7	8564.9	1.19	1.14
21	403.5	3177.0	0.09	0.74	59	9229.2	8696.9	1.21	1.14
22	516.3	3348.9	0.12	0.77	60	9513.8	8830.2	1.23	1.14
23	637.5	3520.3	0.14	0.79	61	9788.0	8965.1	1.25	1.14
24	765.5	3690.8	0.17	0.82	62	10058.2	9101.6	1.27	1.14
25	900.6	3860.3	0.20	0.84	63	10314.5	9239.8	1.28	1.15
26	1043.4	4028.7	0.22	0.87	64	10570.5	9379.7	1.29	1.15
27	1180.0	4195.8	0.25	0.89	65	10799.1	9521.5	1.30	1.15
28	1326.8	4361.5	0.28	0.91	66	11022.0	9664.6	1.31	1.15
29	1479.5	4525.5	0.30	0.93	67	11239.1	9809.9	1.31	1.15
30	1638.1	4688.1	0.33	0.95	68	11460.5	9957.4	1.32	1.15
31	1802.3	4848.8	0.36	0.97	69	11673.0	10107.2	1.32	1.15
32	1981.2	5007.7	0.39	0.99	70	11887.4	10259.1	1.33	1.15
33	2162.2	5164.7	0.42	1.00	71	12097.0	10413.4	1.33	1.15
34	2354.9	5319.9	0.45	1.02	72	12305.0	10570.0	1.33	1.15
35	2554.9	5473.1	0.48	1.03	73	12512.7	10728.9	1.34	1.15
36	2761.2	5624.3	0.51	1.04	74	12719.2	10890.2	1.34	1.15
37	2982.8	5773.6	0.54	1.05	75	12924.0	11054.0	1.34	1.15
38	3210.9	5920.7	0.58	1.07	76	13133.0	11220.2	1.34	1.15
39	3448.4	6065.7	0.61	1.08	77	13344.3	11388.9	1.34	1.15
40	3689.2	6208.6	0.64	1.08	78	13556.6	11560.1	1.34	1.15
					79	13770.3	11733.9	1.34	1.15

NOTES: The reported real earnings generation in this table for the preschool program are solely due to effects on participants and peers, and omit all other avenues of influence of preschool programs on job generation (e.g., balanced budget multiplier, labor supply of parents, etc.) Real earnings generated are for a hypothetical universal preschool program that initially spends an additional \$1 billion per year, versus devoting the same present value per year to economic development subsidies. This program then grows in costs with the economy by 1.5% per year. These real earnings generation numbers can be scaled up or down with size of program in a particular state, as long as design of programs is similar to what is assumed here, even if the program is not a universal preschool program, and as long as corrections are made for different trends in real wages since 2004. The earnings generated as % of baseline state earnings compares earnings generated to total state baseline earnings, which is assumed to grow at 1.5% per year. These percentage figures for preschool would apply to any state that adopted a universal preschool program of this design, or an economic development program that had same cost as such a program.

Table 17 Effects of a Universal Preschool Program on the Percentage College Graduates in a State's Economy

Year	Effects on percentage college graduates in state's population	Year	Effects on percentage college graduates in state's population
3	0.00	41	0.04
4	0.00	42	0.05
5	0.00	43	0.05
6	0.00	44	0.06
7	0.00	45	0.06
8	0.00	46	0.06
9	0.00	47	0.07
10	0.00	48	0.07
11	0.00	49	0.07
12	0.00	50	0.08
13	0.00	51	0.08
14	0.00	52	0.08
15	0.00	53	0.09
16	0.00	54	0.09
17	0.00	55	0.09
18	0.00	56	0.09
19	0.00	57	0.09
20	0.00	58	0.10
21	0.00	59	0.10
22	0.00	60	0.10
23	0.00	61	0.10
24	0.00	62	0.10
25	0.00	63	0.10
26	0.00	64	0.11
27	0.00	65	0.11
28	0.01	66	0.11
29	0.01	67	0.11
30	0.01	68	0.11
31	0.01	69	0.11
32	0.01	70	0.11
33	0.02	71	0.12
34	0.02	72	0.12
35	0.02	73	0.12
36	0.02	74	0.12
37	0.03	75	0.12
38	0.03	76	0.12
39	0.04	77	0.12
40	0.04	78	0.12
		79	0.13

NOTES: The table shows the effects of a permanent universal preschool program on the percentage of college graduates in the state's workforce in the actual percentage rate points, not the percentage change in the percent. That is, the long-run figure of 0.13% in year 79 should be interpreted as meaning that if the baseline percentage of college graduates in year 79 would have been 25%, then a permanent universal preschool program, started in year 4, would by year 79 increase the percentage college graduates in the state to 25.13%.

Table 18 **Preschool's Effects on Jobs and Earnings Via Its Effects on the Social Productivity Effects of Education by Increasing the State College Graduate Percentage**

Year	Effects per \$1 billion of spending on preschool program		Effects of universal preschool program as a % of the state economy		Year	Effects per \$1 billion of spending on preschool program		Effects of universal preschool program as a % of the state economy	
	Job generation	Real earnings generation, in millions of 2004 dollars	Job generation effects as % of baseline state jobs	Earnings generation effects as % of baseline state earnings		Job generation	Real earnings generation, in millions of 2004 dollars	Job generation effects as % of baseline state jobs	Earnings generation effects as % of baseline state earnings
3	0	0.0	0.000	0.000	41	779	72.4	0.007	0.012
4	0	0.0	0.000	0.000	42	866	82.1	0.008	0.014
5	0	0.0	0.000	0.000	43	955	92.1	0.009	0.015
6	0	0.0	0.000	0.000	44	1044	102.6	0.010	0.017
7	0	0.0	0.000	0.000	45	1134	113.4	0.011	0.018
8	0	0.0	0.000	0.000	46	1222	124.4	0.011	0.020
9	0	0.0	0.000	0.000	47	1308	135.6	0.012	0.021
10	0	0.0	0.000	0.000	48	1393	147.0	0.013	0.023
11	0	0.0	0.000	0.000	49	1475	158.5	0.014	0.024
12	0	0.0	0.000	0.000	50	1556	170.1	0.014	0.026
13	0	0.0	0.000	0.000	51	1633	181.7	0.015	0.027
14	0	0.0	0.000	0.000	52	1709	193.3	0.016	0.028
15	0	0.0	0.000	0.000	53	1781	204.8	0.016	0.029
16	0	0.0	0.000	0.000	54	1846	215.9	0.017	0.031
17	0	0.0	0.000	0.000	55	1905	226.6	0.017	0.032
18	0	0.0	0.000	0.000	56	1958	236.9	0.018	0.033
19	0	0.0	0.000	0.000	57	2007	246.9	0.018	0.033
20	0	0.0	0.000	0.000	58	2051	256.5	0.018	0.034
21	0	0.0	0.000	0.000	59	2091	265.6	0.019	0.035
22	1	0.1	0.000	0.000	60	2125	274.3	0.019	0.036
23	5	0.3	0.000	0.000	61	2155	282.6	0.019	0.036
24	10	0.7	0.000	0.000	62	2182	290.4	0.019	0.037
25	20	1.3	0.000	0.000	63	2203	297.8	0.020	0.037
26	32	2.2	0.000	0.000	64	2221	304.7	0.020	0.037
27	49	3.4	0.000	0.001	65	2236	311.2	0.020	0.037
28	69	5.0	0.001	0.001	66	2247	317.3	0.020	0.038
29	95	7.0	0.001	0.001	67	2255	322.9	0.020	0.038
30	125	9.4	0.001	0.002	68	2260	328.1	0.020	0.038
31	159	12.2	0.002	0.002	69	2262	333.0	0.020	0.038
32	197	15.5	0.002	0.003	70	2263	337.5	0.020	0.038
33	240	19.3	0.002	0.004	71	2260	341.6	0.020	0.038
34	288	23.6	0.003	0.005	72	2256	345.4	0.019	0.037
35	341	28.5	0.003	0.005	73	2249	348.8	0.019	0.037
36	400	34.1	0.004	0.006	74	2240	351.8	0.019	0.037
37	465	40.3	0.004	0.007	75	2229	354.5	0.019	0.037
38	536	47.3	0.005	0.009	76	2216	356.8	0.019	0.036
39	612	55.0	0.006	0.010	77	2200	358.7	0.019	0.036
40	693	63.4	0.007	0.011	78	2183	360.3	0.018	0.036
					79	2163	361.4	0.018	0.035

Notes: This only shows the effects of preschool education on jobs and earnings, due to preschool's effects on the state's college graduation percentage, that occur due to the social productivity effects of education. The effects of increased educational attainment for each individual preschool participant, and his or her school peers, are already reflected in estimates previously presented.

Table 19 Total Jobs and Earnings Effects of Universal Preschool Education, Compared to Economic Development Subsidies

Year	Job effects per \$1 billion spent		Job effects, as % total jobs		Earnings effects, in millions of 2004 \$, per \$1 billion spent		Earnings effects, as % of state earnings	
	Preschool	Economic development	Preschool	Economic development	Preschool	Economic development	Preschool	Economic development
3	0	0	0.00	0.00	0.0	0.0	0.00	0.00
4	5850	4286	0.06	0.05	225.2	216.0	0.07	0.06
5	5518	8148	0.06	0.09	216.5	422.7	0.06	0.12
6	5178	11575	0.05	0.12	207.3	619.4	0.06	0.18
7	4831	14558	0.05	0.15	197.4	805.1	0.06	0.23
8	4477	17090	0.05	0.18	186.7	978.5	0.05	0.28
9	4118	19163	0.04	0.20	175.2	1138.6	0.05	0.32
10	4098	21203	0.04	0.22	176.6	1301.3	0.05	0.36
11	4076	23204	0.04	0.24	177.8	1466.3	0.05	0.39
12	4051	25166	0.04	0.26	178.6	1633.3	0.05	0.43
13	4024	27084	0.04	0.28	179.3	1801.9	0.05	0.47
14	3997	28959	0.04	0.30	179.6	1971.9	0.05	0.51
15	3969	30790	0.04	0.31	179.6	2142.8	0.05	0.54
16	5400	32574	0.06	0.33	203.0	2314.4	0.05	0.58
17	7692	34313	0.08	0.35	242.2	2486.6	0.06	0.61
18	10528	36004	0.11	0.36	295.4	2659.1	0.07	0.65
19	14536	37649	0.15	0.38	381.6	2831.9	0.09	0.68
20	18647	39247	0.19	0.40	477.1	3004.6	0.11	0.71
21	22665	40795	0.23	0.41	580.2	3177.0	0.13	0.74
22	26718	42294	0.27	0.42	692.1	3348.9	0.16	0.77
23	30660	43743	0.31	0.44	812.4	3520.3	0.18	0.79
24	34506	45144	0.34	0.45	939.5	3690.8	0.21	0.82
25	37915	46496	0.38	0.46	1074.0	3860.3	0.23	0.84
26	41181	47798	0.41	0.47	1216.2	4028.7	0.26	0.87
27	44100	49052	0.43	0.48	1352.5	4195.8	0.29	0.89
28	47039	50255	0.46	0.49	1499.1	4361.5	0.31	0.91
29	49966	51409	0.49	0.50	1652.1	4525.5	0.34	0.93
30	52961	52514	0.52	0.51	1811.5	4688.1	0.37	0.95
31	55883	53571	0.54	0.52	1976.8	4848.8	0.39	0.97
32	58834	54580	0.57	0.53	2157.2	5007.7	0.42	0.99
33	61774	55542	0.60	0.54	2340.0	5164.7	0.45	1.00
34	64827	56458	0.63	0.55	2535.2	5319.9	0.48	1.02
35	67760	57329	0.65	0.55	2738.4	5473.1	0.52	1.03
36	70668	58154	0.68	0.56	2948.5	5624.3	0.55	1.04
37	73599	58935	0.70	0.56	3174.6	5773.6	0.58	1.05
38	76560	59673	0.73	0.57	3407.7	5920.7	0.61	1.07
39	79562	60368	0.76	0.57	3651.1	6065.7	0.65	1.08
40	82519	61023	0.78	0.58	3898.6	6208.6	0.68	1.08
41	85641	61638	0.81	0.58	4158.2	6349.5	0.72	1.09
42	88781	62216	0.84	0.59	4431.0	6488.4	0.75	1.10
43	91796	62759	0.86	0.59	4704.5	6625.5	0.79	1.11
44	94890	63269	0.89	0.59	4988.2	6760.8	0.82	1.11
45	98025	63747	0.92	0.60	5279.7	6894.4	0.86	1.12
46	101132	64195	0.94	0.60	5567.6	7026.5	0.89	1.12
47	104198	64612	0.97	0.60	5871.8	7156.9	0.92	1.13
48	107262	65002	0.99	0.60	6175.5	7286.0	0.96	1.13
49	110241	65366	1.02	0.60	6480.8	7414.1	0.99	1.13
50	113216	65707	1.04	0.60	6797.6	7541.3	1.02	1.13
51	116185	66027	1.07	0.61	7117.6	7668.1	1.06	1.14
52	119074	66331	1.09	0.61	7439.9	7794.6	1.09	1.14

Table 19 (Continued)

Year	Job effects per \$1 billion spent		Job effects, as % total jobs		Earnings effects, in millions of 2004 \$, per \$1 billion spent		Earnings effects, as % of state earnings	
	Preschool	Economic development	Preschool	Economic development	Preschool	Economic development	Preschool	Economic development
53	121942	66619	1.11	0.61	7764.0	7921.1	1.12	1.14
54	124723	66896	1.13	0.61	8083.7	8048.2	1.15	1.14
55	127246	67161	1.15	0.61	8391.0	8176.0	1.17	1.14
56	129803	67416	1.17	0.61	8709.4	8304.6	1.20	1.14
57	132232	67663	1.19	0.61	9019.8	8434.2	1.22	1.14
58	134673	67903	1.21	0.61	9331.5	8564.9	1.25	1.14
59	136999	68137	1.23	0.61	9637.7	8696.9	1.27	1.14
60	139095	68366	1.24	0.61	9932.6	8830.2	1.29	1.14
61	141056	68592	1.26	0.61	10216.9	8965.1	1.30	1.14
62	142892	68813	1.27	0.61	10496.7	9101.6	1.32	1.14
63	144546	69032	1.28	0.61	10762.3	9239.8	1.33	1.15
64	146168	69248	1.29	0.61	11027.2	9379.7	1.35	1.15
65	147445	69462	1.30	0.61	11264.3	9521.5	1.36	1.15
66	148680	69671	1.30	0.61	11495.0	9664.6	1.36	1.15
67	149792	69880	1.31	0.61	11720.1	9809.9	1.37	1.15
68	150882	70089	1.32	0.61	11949.1	9957.4	1.37	1.15
69	151839	70299	1.32	0.61	12168.8	10107.2	1.38	1.15
70	152752	70510	1.32	0.61	12390.2	10259.1	1.38	1.15
71	153573	70722	1.33	0.61	12606.5	10413.4	1.39	1.15
72	154377	70934	1.33	0.61	12820.7	10570.0	1.39	1.15
73	155097	71147	1.33	0.61	13034.4	10728.9	1.39	1.15
74	155740	71360	1.33	0.61	13246.5	10890.2	1.39	1.15
75	156369	71574	1.34	0.61	13456.6	11054.0	1.39	1.15
76	157011	71789	1.34	0.61	13670.6	11220.2	1.40	1.15
77	157619	72004	1.34	0.61	13886.6	11388.9	1.40	1.15
78	158213	72220	1.34	0.61	14103.2	11560.1	1.40	1.15
79	158786	72437	1.34	0.61	14320.9	11733.9	1.40	1.15

NOTES: This table simply sums all the avenues by which a permanent preschool program is estimated to affect a state's jobs and earnings in this report, and compares these effects with the effects of permanent economic development subsidies of the same cost.

Table 20 Percentage of Persons Living in Birth State

State of birth	% living in state of birth	State of birth	% living in state of birth	State of birth	% living in state of birth
Alabama	68.2	Kentucky	65.8	North Dakota	44.8
Alaska	53.5	Louisiana	71.6	Ohio	70.9
Arizona	69.9	Maine	66.3	Oklahoma	60.3
Arkansas	58.8	Maryland	68.5	Oregon	66.0
California	76.9	Massachusetts	66.2	Pennsylvania	69.1
Colorado	62.0	Michigan	73.7	Rhode Island	60.4
Connecticut	65.0	Minnesota	71.1	South Carolina	69.8
Delaware	62.3	Mississippi	60.3	South Dakota	49.6
District of Columbia	16.8	Missouri	66.0	Tennessee	70.2
Florida	73.6	Montana	53.3	Texas	79.8
Georgia	73.4	Nebraska	56.0	Utah	70.7
Hawaii	63.0	Nevada	62.9	Vermont	59.1
Idaho	55.4	New Hampshire	61.1	Virginia	67.3
Illinois	65.7	New Jersey	64.5	Washington	70.4
Indiana	69.1	New Mexico	59.9	West Virginia	52.2
Iowa	59.0	New York	63.2	Wisconsin	73.3
Kansas	55.9	North Carolina	74.8	Wyoming	42.8
		U.S. Average	68.4		

NOTE: This is derived from a special tabulation by the U.S. Census Bureau from the 2000 U.S. Census, released on the internet on January 31, 2005, and available at <http://www.census.gov/population/www/cen2000/phc-t38.html>. This includes all U.S. residents in 2000 born in the U.S. The percentage is simply what percentage of this total is living in the same state they were born in, broken down by state of birth.

Table 21 Long-Run Effects of Preschool Education on Various Aspects of Economic Development from Each State's Perspective

State	Assumed annual incremental costs of universal preschool program (in millions of 2004 dollars)	PV of real earnings generated divided by PV of costs	Long-run effect on number of jobs	Long-run effects on annual real earnings (in millions of 2004 dollars)	Long-run effect on annual gross state product (in millions of 2004 dollars)	Long-run effect on annual state and local tax revenue (in millions of 2004 dollars)
Alabama	179.9	2.77	27090	836.8	1944.9	152.7
Alaska	35.5	2.31	3714	135.3	388.2	27.0
Arizona	246.9	2.83	34182	1172.8	2839.0	236.6
Arkansas	99.2	2.47	14703	408.1	995.3	89.6
California	1903.8	3.05	234565	9802.4	23870.6	2108.1
Colorado	250.2	2.58	28902	1076.1	2571.4	197.2
Connecticut	235.8	2.67	22425	1054.4	2484.0	224.7
Delaware	50.4	2.59	5570	217.7	700.5	40.1
District of Columbia	124.9	1.13	3942	215.1	394.8	19.0
Florida	759.8	2.95	113859	3769.7	8885.4	763.5
Georgia	422.6	2.94	59087	2091.1	5076.0	397.2
Hawaii	64.6	2.61	8375	281.5	655.7	63.5
Idaho	51.3	2.37	7356	201.4	511.1	43.9
Illinois	689.3	2.69	79022	3108.1	7035.0	602.7
Indiana	282.0	2.80	41208	1327.4	3202.6	267.6
Iowa	131.1	2.48	18388	541.3	1371.7	116.8
Kansas	124.9	2.38	16401	493.6	1169.4	103.8
Kentucky	168.3	2.70	24815	760.4	1843.3	163.4
Louisiana	173.9	2.88	28423	842.8	2215.7	200.9
Maine	55.0	2.71	8396	249.8	588.6	68.5
Maryland	306.1	2.78	35933	1429.0	3182.3	311.8
Massachusetts	436.2	2.71	43860	1979.3	4311.3	358.7
Michigan	490.9	2.95	64725	2437.8	5525.5	488.7
Minnesota	299.8	2.87	38597	1444.1	3223.7	298.6
Mississippi	93.3	2.52	14850	392.4	957.1	91.1
Missouri	265.2	2.71	37095	1201.5	2753.3	222.6
Montana	33.1	2.30	4874	125.9	312.3	27.9
Nebraska	82.9	2.38	11117	327.9	806.4	70.8
Nevada	123.0	2.61	15137	535.3	1305.1	101.9
New Hampshire	68.3	2.55	8051	289.9	658.5	51.4
New Jersey	528.5	2.65	52790	2346.5	5522.3	506.1
New Mexico	72.2	2.51	10173	301.8	762.9	69.7
New York	1175.0	2.62	110993	5135.3	11716.2	1297.0
North Carolina	384.8	2.98	59386	1935.2	5058.1	379.1
North Dakota	28.0	2.03	3511	93.0	225.0	19.4
Ohio	548.2	2.86	77814	2635.0	6033.9	565.8
Oklahoma	128.8	2.52	18975	541.5	1352.1	124.5
Oregon	161.6	2.71	22188	732.0	1734.8	135.6
Pennsylvania	605.3	2.80	79820	2848.1	6584.0	583.2
Rhode Island	51.5	2.53	6252	216.6	524.4	51.3
South Carolina	167.0	2.83	26661	792.4	1931.1	154.0
South Dakota	30.9	2.18	4275	111.2	315.8	22.5
Tennessee	264.5	2.84	38896	1261.6	3103.7	210.4

Table 21 (Continued)

State	Assumed annual incremental costs of universal preschool program (in millions of 2004 dollars)	PV of real earnings generated divided by PV of costs	Long-run effect on number of jobs	Long-run effects on annual real earnings (in millions of 2004 dollars)	Long-run effect on annual gross state product (in millions of 2004 dollars)	Long-run effect on annual state and local tax revenue (in millions of 2004 dollars)
Texas	1035.9	3.15	152517	5507.3	14052.6	1068.8
Utah	101.5	2.85	16092	486.5	1183.9	96.9
Vermont	28.5	2.48	3875	117.8	271.0	27.4
Virginia	424.5	2.75	51351	1954.0	4531.6	347.2
Washington	324.8	2.84	40699	1551.6	3735.2	311.5
West Virginia	60.0	2.26	8074	224.4	553.0	56.9
Wisconsin	269.9	2.94	41309	1335.0	3128.7	307.2
Wyoming	22.9	1.96	2559	73.4	229.7	20.6
United States	14662.2	2.78	1882870	68949.2	164328.4	14265.2

NOTES: Annual incremental costs of universal preschool program are based on the assumption that costs of such a universal program will be 0.299% of state wage and salary earnings, using calculations reported in Table 2. The present value of real earnings generated is calculated by adjusting the state's PV effects on participants and peers based on the state residents' tendency to move out compared to the average state. Specifically, as shown in Chapter 5, the PV ratio in an "average state" without migration for peers and participants would be 3.66, whereas in a state with normal out-migration this PV ratio is 2.65. To adjust for differential out-migration, we multiply 3.66 by the following ratio: $3.66[1 - ((3.66 - 2.65) / 3.666) \times R]$, where $R = (\% \text{ born in state who leave state}) / (\% \text{ born in U.S. who leave state})$. The percentage born in state who leave the state is reported in Table 20, based on the U.S. Census. I then add in the 0.13 in additional earnings ratio based on the other channels for effects of preschool on earnings, which I assume are unaffected by the relative migration of the state. The effect on jobs is based on the percentage effects on jobs reported in Table 15 and Table 19 as of year 79, that is after the program has been in effect for 75 years. However, the percentage effect is calculated by considering the state's wage and salary employment as of 2004. Therefore, this effect on jobs should be considered the effects that would have occurred as of 2004 if the program had been in effect for 75 years. The percentage effect is also adjusted for differential out-migration in each state. That is, as the long-run percentage effect of preschool, through its effects on peers and participants, on employment after 75 years without out-migration is calculated to be 1.78% (Chapter 5), and the average U.S. effect with out-migration is calculated to be 1.29%, the percentage effect actually used is adjusted by % job effect = $1.78[1 - ((1.78 - 1.29) / 1.78) \times R]$. I then add back in the 0.05% long-run effect on jobs due to the other avenues by which preschool affects jobs, which are assumed to be unaffected by differential migration across states. The effect on earnings is also derived from the percentage effect of preschool on real earnings (due to participant and peer effects) after 75 years in Table 16 of 1.34%, and wage and salary earnings in the state as of 2004. Wage and salary earnings is, however, adjusted to wage and salary employment so that the average hourly wage rate is \$17.67 per hour, as explained in Table 2. Each state's percentage effect is also adjusted based on the state's differential out-migration. Long-run percentage effects on real earnings due to peer and participant effects without any out-migration is 1.88%, whereas the typical state's percentage effect, based on Table 16, is 1.34%. Therefore the percentage effect on real earnings in each state is equal to $1.88[1 - ((1.88 - 1.34) / 1.88) \times R]$. The total percentage effect of preschool on earnings via all avenues of effects is then calculated by adding in 0.05%, which is assumed unchanged with differential migration. The effects on real gross state product are based on the ratio of each state's GSP to real wage and salary earnings. Figures for state GSP are for 2004 and come from the Regional Economic Information System of the Bureau of Economic Analysis of the U.S. Department of Commerce. Figures for state and local tax revenue are based on the ratio of state and local tax revenue to GSP in each state. State and local tax revenues are for fiscal year 2001–2002, and come from the U.S. Census of Governments; real GSP figures used in figuring the ratio of tax revenue to GSP are for 2002. The U.S. totals sum what each state perceives as the effects on its jobs, earnings, GSP, and tax revenue. These U.S. totals do not reflect the actual national effects after allowing for spillover effects on other states of each state's preschool program, as explained in Chapter 5.

Table 22: Alternative Assumptions About Preschool Benefits for Different Groups and How They Alter Earnings Effects of Preschool

	Extremely Conservative Assumptions	Very conservative assumptions	Moderately conservative assumptions: baseline assumptions used up to now in this report for benefits of preschool	Moderate assumptions	Generous assumptions
Assumptions for benefits of each group participating in universal preschool, as percentage of benefits from previous studies of preschool participation vs. no participation for high risk children:					
High risk children who otherwise would not have been in any preschool	100	100	100	100	100
High risk children who otherwise would have been in lower cost public preschool	50	50	50	70	100
High risk children who otherwise would have been in private preschool	0	0	0	0	50
Medium risk children who otherwise would not have been in any preschool	0	50	50	70	50
Medium risk children who otherwise would have been in lower cost public preschool	0	25	25	35	50
Medium risk children who otherwise would have been in private preschool	0	0	0	0	25
Low risk children who otherwise would not have been in any preschool	0	0	25	35	25
Low risk children who otherwise would have been in lower cost public preschool	0	0	0	0	25
Low risk children who otherwise would have been in private preschool	0	0	0	0	0
Based on these assumptions, average benefits per universal preschool participant of universal preschool, as percentage of benefits from previous studies of preschool vs. no preschool for high risk children	16	21	23	30	41
Based on these assumptions, simulated ratio of present value of earnings effects of universal preschool to preschool costs.	1.97	2.55	2.78	3.59	4.85

NOTES: Alternative assumptions about benefit percentages are taken from Karoly and Bigelow (2005), Table 3.5 on p. 107. The average benefits in next to last row are derived based on those assumptions and assumptions about distribution of universal preschool enrollment across these nine groups. The ratio of PV earnings to preschool costs in the last row are derived from simulating the current report's model. These estimates are easily derived by multiplying the baseline assumption results for peers and participants by the average percentage benefits for that set of assumptions (next to last row), divided by the average percentage benefits for baseline case (23%), and then adding in the other avenues of earnings effects of preschool.

Table 23 State-by-State Estimates of Long-Run Effects of Universal Preschool, National vs. State Perspective

State	Jobs		Annual earnings (in millions)		Gross state product (in millions)	
	State perspective	National perspective	State perspective	National perspective	State perspective	National perspective
Alabama	27090	37033	836.8	1163.5	1944.9	2704.1
Alaska	3714	6107	135.3	229.3	388.2	657.9
Arizona	34182	45827	1172.8	1597.2	2839.0	3866.5
Arkansas	14703	22547	408.1	641.5	995.3	1564.4
California	234565	291399	9802.4	12314.0	23870.6	29986.9
Colorado	28902	42525	1076.1	1618.2	2571.4	3866.8
Connecticut	22425	31814	1054.4	1525.1	2484.0	3592.9
Delaware	5570	8168	217.7	326.2	700.5	1049.5
District of Columbia	3942	13212	215.1	807.9	394.8	1482.9
Florida	113859	146494	3769.7	4914.7	8885.4	11584.2
Georgia	59087	76211	2091.1	2733.4	5076.0	6635.0
Hawaii	8375	12170	281.5	417.8	655.7	973.1
Idaho	7356	11790	201.4	332.0	511.1	842.5
Illinois	79022	111244	3108.1	4458.7	7035.0	10092.0
Indiana	41208	55709	1327.4	1823.9	3202.6	4400.5
Iowa	18388	28107	541.3	847.9	1371.7	2148.6
Kansas	16401	26108	493.6	807.6	1169.4	1913.3
Kentucky	24815	34862	760.4	1088.5	1843.3	2638.5
Louisiana	28423	37391	842.8	1125.0	2215.7	2957.5
Maine	8396	11735	249.8	355.7	588.6	838.0
Maryland	35933	48954	1429.0	1979.7	3182.3	4408.7
Massachusetts	43860	61376	1979.3	2821.3	4311.3	6145.3
Michigan	64725	83199	2437.8	3175.2	5525.5	7196.6
Minnesota	38597	51055	1444.1	1938.9	3223.7	4328.0
Mississippi	14850	22325	392.4	603.8	957.1	1472.8
Missouri	37095	51989	1201.5	1715.5	2753.3	3931.1
Montana	4874	8045	125.9	214.1	312.3	531.4
Nebraska	11117	17684	327.9	536.1	806.4	1318.5
Nevada	15137	22030	535.3	795.7	1305.1	1939.8
New Hampshire	8051	11989	289.9	441.5	658.5	1003.0
New Jersey	52790	75400	2346.5	3418.5	5522.3	8045.2
New Mexico	10173	15365	301.8	466.8	762.9	1179.8
New York	110993	160891	5135.3	7600.4	11716.2	17340.2
North Carolina	59386	75429	1935.2	2488.8	5058.1	6504.9
North Dakota	3511	6573	93.0	181.3	225.0	438.7
Ohio	77814	103143	2635.0	3545.6	6033.9	8118.9
Oklahoma	18975	28527	541.5	833.4	1352.1	2080.7
Oregon	22188	31101	732.0	1045.2	1734.8	2477.1
Pennsylvania	79820	107962	2848.1	3915.4	6584.0	9051.4
Rhode Island	6252	9387	216.6	332.8	524.4	805.9
South Carolina	26661	35772	792.4	1080.0	1931.1	2632.2
South Dakota	4275	7432	111.2	200.1	315.8	568.2
Tennessee	38896	51929	1261.6	1710.6	3103.7	4208.2
Texas	152517	183811	5507.3	6700.2	14052.6	17096.5
Utah	16092	21386	486.5	656.5	1183.9	1597.4
Vermont	3875	5915	117.8	184.3	271.0	423.9
Virginia	51351	70907	1954.0	2746.0	4531.6	6368.3
Washington	40699	54267	1551.6	2100.9	3735.2	5057.6
West Virginia	8074	13529	224.4	388.0	553.0	956.3
Wisconsin	41309	53310	1335.0	1746.1	3128.7	4092.0
Wyoming	2559	4945	73.4	148.1	229.7	463.7
United States	1882870	2546076	68949.2	94838.7	164328.4	225577.6

NOTES: Figures from state perspective were previously reported in Table 21, as described in text and notes to that table. These estimated effects are long-run effects, based on present size of state economy, if universal preschool program had been running for 75 years. The national perspective figures simply produce the equivalent numbers in a model in which out-migration from the state is assumed to be zero. This is equivalent to including the effects of out-migration even after they leave a state.

Table 24 State-by-State Estimates of Long-Run Effects of Traditional Economic Development Subsidies, National vs. State Perspective

State	Jobs		Annual earnings (in millions)		Gross state product (in millions)	
	State perspective	National perspective	State perspective	National perspective	State perspective	National perspective
Alabama	12304	2571	691.3	143.8	1606.6	334.2
Alaska	1952	424	131.3	28.3	376.6	81.3
Arizona	15293	3182	953.0	197.4	2307.0	477.9
Arkansas	7307	1565	372.2	79.3	907.7	193.4
California	99007	20232	7475.1	1522.0	18203.2	3706.3
Colorado	13902	2953	946.8	200.0	2262.3	477.9
Connecticut	10483	2209	899.0	188.5	2117.9	444.1
Delaware	2672	567	191.0	40.3	614.4	129.7
District of Columbia	3803	917	418.6	99.9	768.3	183.3
Florida	49357	10171	2959.5	607.4	6975.7	1431.8
Georgia	25662	5291	1645.0	337.8	3993.2	820.1
Hawaii	3989	845	245.0	51.6	570.8	120.3
Idaho	3787	819	191.0	41.0	484.7	104.1
Illinois	36718	7724	2632.6	551.1	5958.7	1247.3
Indiana	18555	3868	1086.3	225.4	2620.9	543.9
Iowa	9116	1952	492.3	104.8	1247.5	265.6
Kansas	8398	1813	465.2	99.8	1102.1	236.5
Kentucky	11512	2421	643.0	134.5	1558.5	326.1
Louisiana	12533	2596	674.1	139.0	1772.1	365.5
Maine	3880	815	210.3	44.0	495.5	103.6
Maryland	16277	3399	1177.1	244.7	2621.4	544.9
Massachusetts	20285	4261	1667.9	348.7	3633.0	759.5
Michigan	28038	5777	1912.4	392.4	4334.5	889.5
Minnesota	17090	3545	1160.3	239.6	2590.1	534.9
Mississippi	7265	1550	351.7	74.6	857.9	182.0
Missouri	17177	3610	1013.8	212.0	2323.3	485.9
Montana	2569	559	122.5	26.5	304.0	65.7
Nebraska	5689	1228	308.8	66.3	759.6	163.0
Nevada	7219	1530	466.6	98.3	1137.4	239.8
New Hampshire	3909	832	257.7	54.6	585.4	124.0
New Jersey	24808	5235	2012.3	422.5	4735.8	994.4
New Mexico	4995	1067	271.6	57.7	686.6	145.8
New York	52768	11171	4460.3	939.4	10176.0	2143.2
North Carolina	25492	5237	1503.1	307.6	3928.6	804.0
North Dakota	2051	456	101.5	22.4	245.5	54.2
Ohio	34510	7161	2120.9	438.2	4856.5	1003.5
Oklahoma	9284	1981	485.5	103.0	1212.0	257.2
Oregon	10275	2159	617.7	129.2	1463.9	306.2
Pennsylvania	35956	7496	2331.7	483.9	5390.4	1118.7
Rhode Island	3056	652	193.9	41.1	469.6	99.6
South Carolina	11935	2484	644.3	133.5	1570.3	325.3
South Dakota	2350	516	113.4	24.7	322.0	70.2
Tennessee	17346	3606	1021.6	211.4	2513.3	520.1
Texas	62919	12762	4096.4	828.1	10452.6	2113.1
Utah	7151	1485	392.5	81.1	955.0	197.4
Vermont	1919	411	107.0	22.8	246.2	52.4
Virginia	23505	4923	1628.0	339.4	3775.6	787.1
Washington	18132	3768	1255.1	259.7	3021.4	625.1
West Virginia	4308	939	221.4	48.0	545.6	118.2
Wisconsin	17949	3701	1050.7	215.8	2462.4	505.8
Wyoming	1534	343	82.4	18.3	258.1	57.3
United States	847995	176779	56472.7	11721.8	134377.8	27880.8

NOTES: Figures from state perspective were previously reported in Table 6, as described in text and notes to that table. These estimated effects are long-run effects, based on present size of state economy, if traditional economic development subsidy program had been running for 75 years. This traditional economic development subsidy program is sized so its annual costs are equal in size to the annual costs in that state of a universal preschool program. As outlined in the text, the national perspective figures simply produce the equivalent numbers in a model in which national effects are assumed to be 19.3% of the state effects that would occur if out-migration from the state is assumed to be zero. This adjusts for the lesser effect on subsidies in changing business activity decisions versus business location decisions, as explained in the text.

Table 25 Comparison of Net Long-Run National Effects of Universal Preschool Education and Traditional Economic Development Subsidies

	Universal Preschool	Traditional Economic Development Subsidies
Ratio of PV of earnings effects to costs	3.79	0.65
Long-run effects on number of jobs	2,546,076	176,779
Long-run effects on annual earnings (in millions of dollars)	94,839	11,722
Long-run effects on annual GSP summed to national total (in millions of dollars)	225,578	27,881
Long-run effects on total government tax revenue: federal, state, and local (in millions of dollars)	56,169	6,942

NOTES: These figures show the national effects of each state simultaneously pursuing a universal preschool program, or a traditional economic development subsidy program of the same size. The national perspective considers spillover effects that occur due to household migration, and spillover effects that occur due to employment reallocation, but ignores spillover effects that occur due to changes in inflationary pressures. The present value calculation is presented in the report text, and is the only calculation that considers effects on all years, and is not scale dependent. The long-run effects in the jobs, earnings, and GSP rows are the same as the sum over all state effects, from a national perspective, reported in Table 23 and Table 24. These are the annual effects on the nation at its current size, assuming these programs had been adopted 75 years ago. The sum of GSP to the national level is virtually the same as GDP. Tax revenue effects are based on the estimate that total federal, state, and local tax revenue is approximately 24.9% of GDP. This is based on figures from U.S. Census of Governments on state and local tax revenue in fiscal year 2003, and figures from the U.S. Statistical Abstract on federal tax revenue in the same fiscal year, compared to the sum of all GSP for 2003.

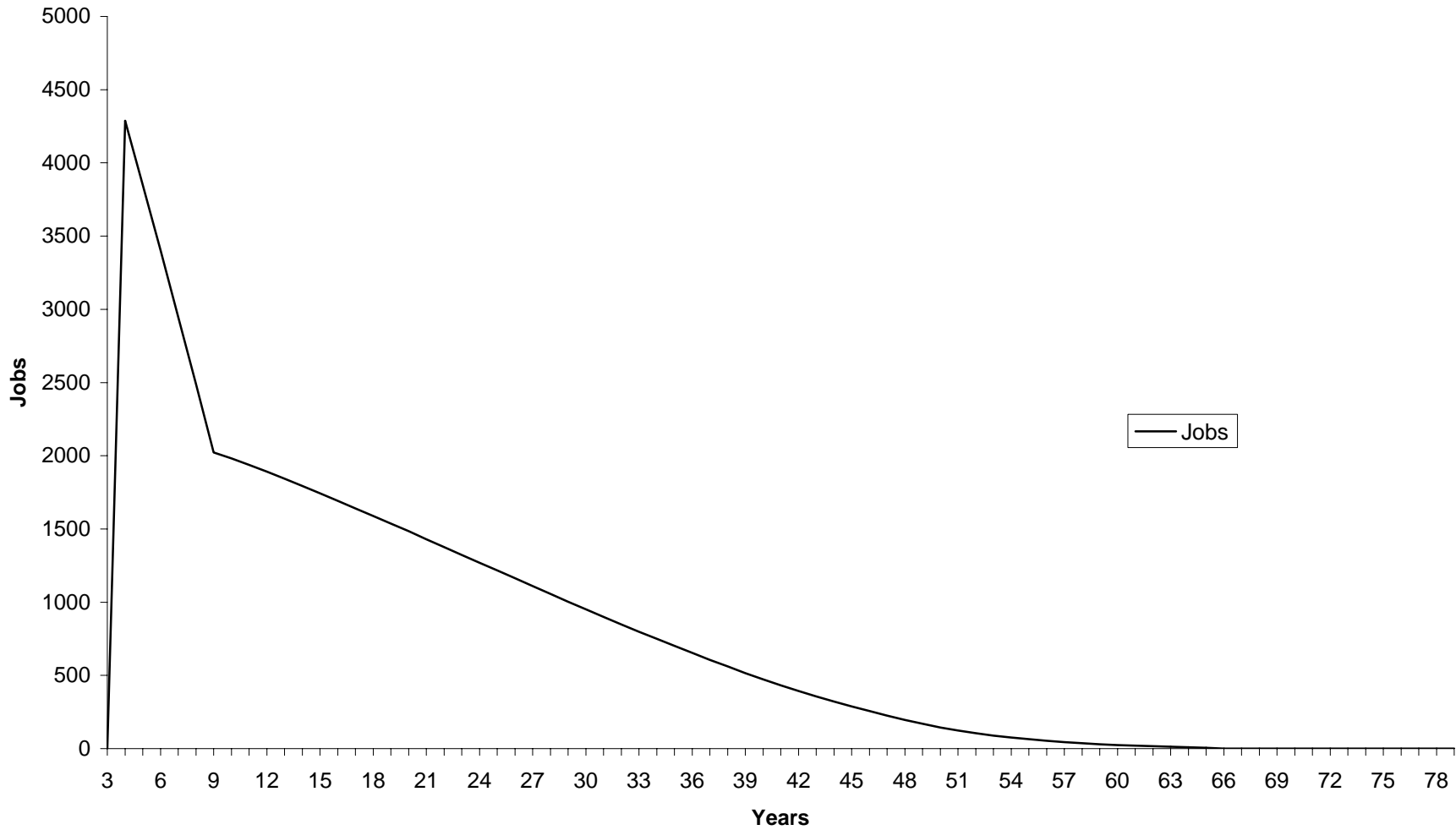
Table 26 State-by-State Estimates of National Job Creation Possible by Reallocating Jobs to That State Due to Economic Development Subsidies

State	Unemployment rate, 2005	State job creation due to economic development program	National job creation associated with state job creation, ignoring inflationary effects	National job creation possible if only that state pursues economic development, inflation equation of Blanchflower and Oswald	National job creation possible if only that state pursues economic development, inflation equation of Bartik
Alabama	4.0	12304	2571	-812	-5126
Alaska	6.8	1952	424	912	1278
Arizona	4.7	15293	3182	1880	468
Arkansas	4.9	7307	1565	1267	957
California	5.4	99007	20232	25733	30928
Colorado	5.0	13902	2953	2675	2391
Connecticut	4.9	10483	2209	1781	1336
Delaware	4.2	2672	567	-6	-701
District of Columbia	6.5	3803	917	1736	2379
Florida	3.8	49357	10171	-6714	-29376
Georgia	5.3	25662	5291	6260	7192
Hawaii	2.8	3989	845	-2432	-8400
Idaho	3.8	3787	819	-477	-2216
Illinois	5.7	36718	7724	11589	15047
Indiana	5.4	18555	3868	4899	5872
Iowa	4.6	9116	1952	961	-138
Kansas	5.1	8398	1813	1813	1813
Kentucky	6.1	11512	2421	4308	5886
Louisiana	7.1	12533	2596	6126	8662
Maine	4.8	3880	815	572	315
Maryland	4.1	16277	3399	-571	-5509
Massachusetts	4.8	20285	4261	2994	1647
Michigan	6.7	28038	5777	12472	17569
Minnesota	4.0	17090	3545	-1155	-7147
Mississippi	7.9	7265	1550	4125	5788
Missouri	5.4	17177	3610	4564	5465
Montana	4.0	2569	559	-148	-1049
Nebraska	3.8	5689	1228	-718	-3330
Nevada	4.1	7219	1530	-231	-2421
New Hampshire	3.6	3909	832	-797	-3104
New Jersey	4.4	24808	5235	1288	-3286
New Mexico	5.3	4995	1067	1255	1437
New York	5.0	52768	11171	10116	9039
North Carolina	5.2	25492	5237	5727	6208
North Dakota	3.4	2051	456	-569	-2107
Ohio	5.9	34510	7161	11841	15886
Oklahoma	4.4	9284	1981	504	-1208
Oregon	6.1	10275	2159	3844	5252
Pennsylvania	5.0	35956	7496	6777	6043
Rhode Island	5.0	3056	652	591	528
South Carolina	6.8	11935	2484	5468	7705
South Dakota	3.9	2350	516	-207	-1153
Tennessee	5.6	17346	3606	5154	6565
Texas	5.3	62919	12762	15137	17421
Utah	4.3	7151	1485	154	-1424
Vermont	3.5	1919	411	-466	-1745
Virginia	3.5	23505	4923	-5822	-21479
Washington	5.5	18132	3768	5087	6309
West Virginia	5.0	4308	939	853	765
Wisconsin	4.7	17949	3701	2174	516
Wyoming	3.6	1534	343	-296	-1202

Table 26 (Continued)

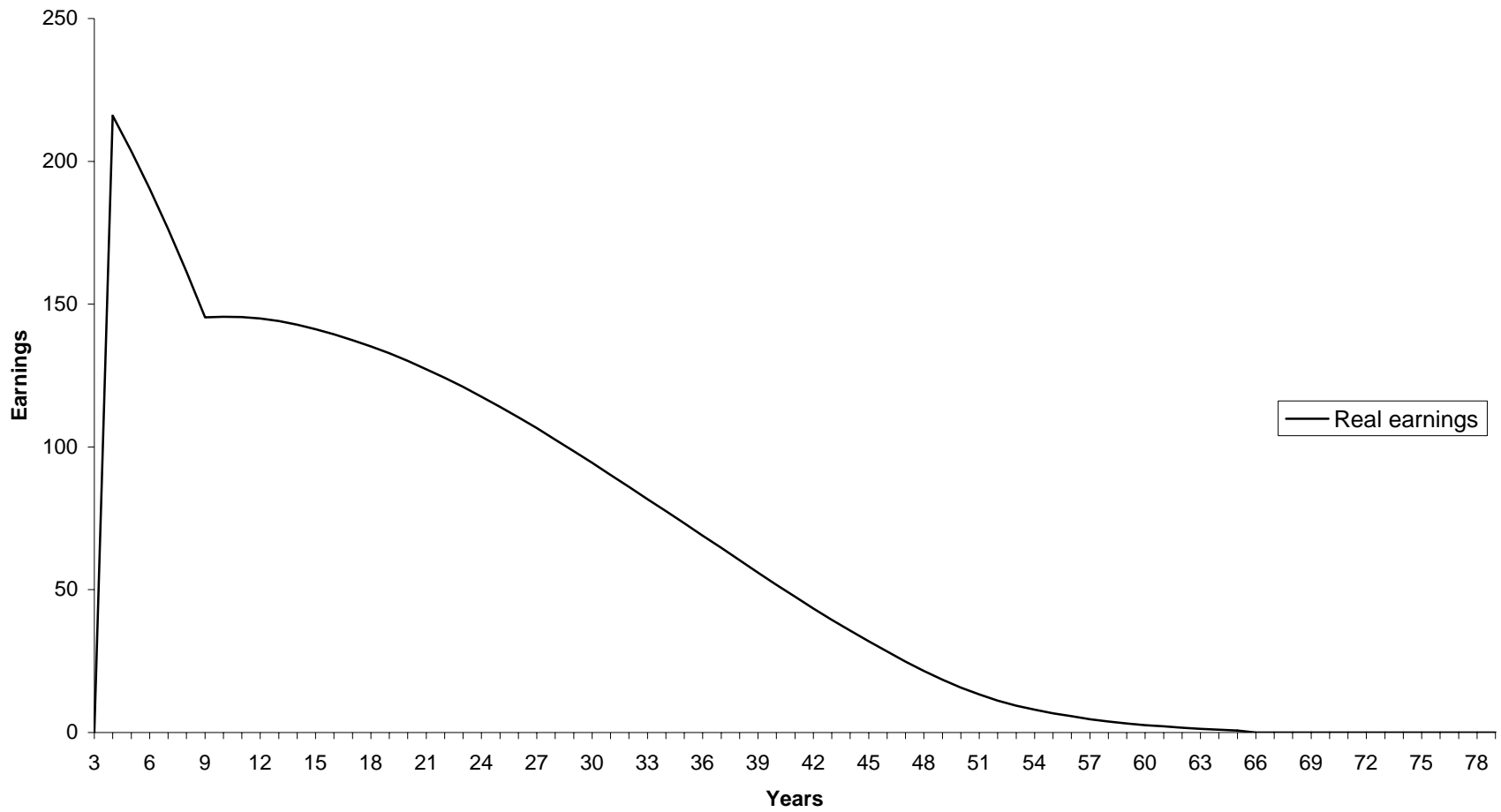
NOTE: Unemployment rate is from BLS. The average U.S. unemployment rate in 2005 was 5.1%. Economic development program considered in each state is same one we have always considered, which is equal in size to assumed size of universal preschool program. Job creation effects reported are the long-run effects we have been considering. The state job creation and national job creation numbers in next two columns are the numbers previously reported in Table 24. The next column adds on to the national effects the additional national job creation possible by the inflationary implications of reallocating jobs to this state. Inflation is assumed to vary with local unemployment as estimated by Blanchflower and Oswald (1994). The last column is the same, except it uses the functional form for how unemployment affects inflation that is estimated by Bartik (2001). These last two columns only make sense if only the state in that row is pursuing economic development. Therefore, the effects in this column, when summed across states, do not correspond to any feasible real-world policy alternative.

Figure 1. Jobs Generated Over Time for Original State Residents from One-Time Economic Development Program of \$1 Billion



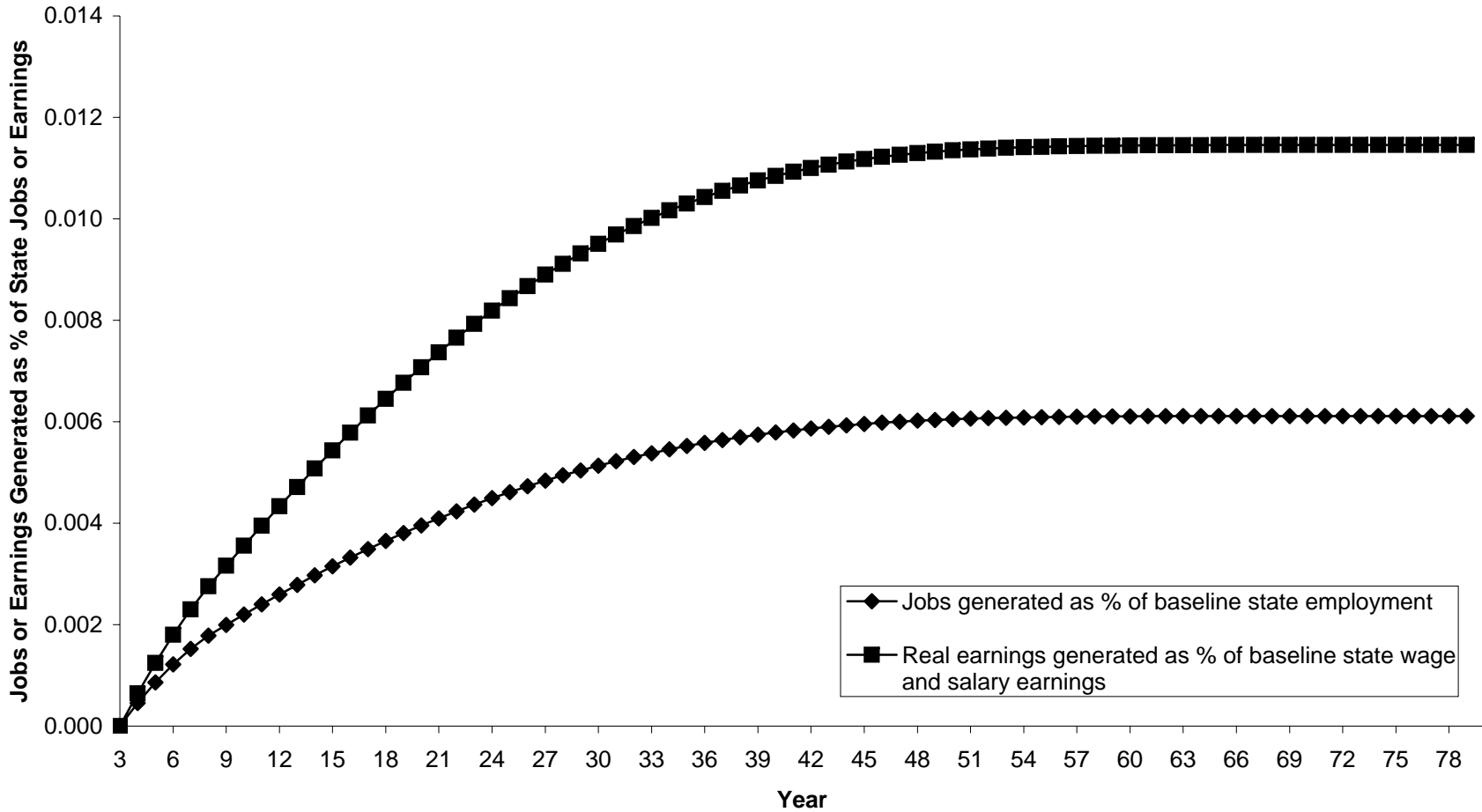
Note: Figure shows jobs generated for original state residents who stay in state from one-time economic development subsidy program of \$1 billion. Jobs are in absolute numbers. Years are labelled so that economic development program is initiated in year 4.

Figure 2. Real Earnings Generated Over Time for Original State Residents from One-Time Economic Development Program of \$1 Billion



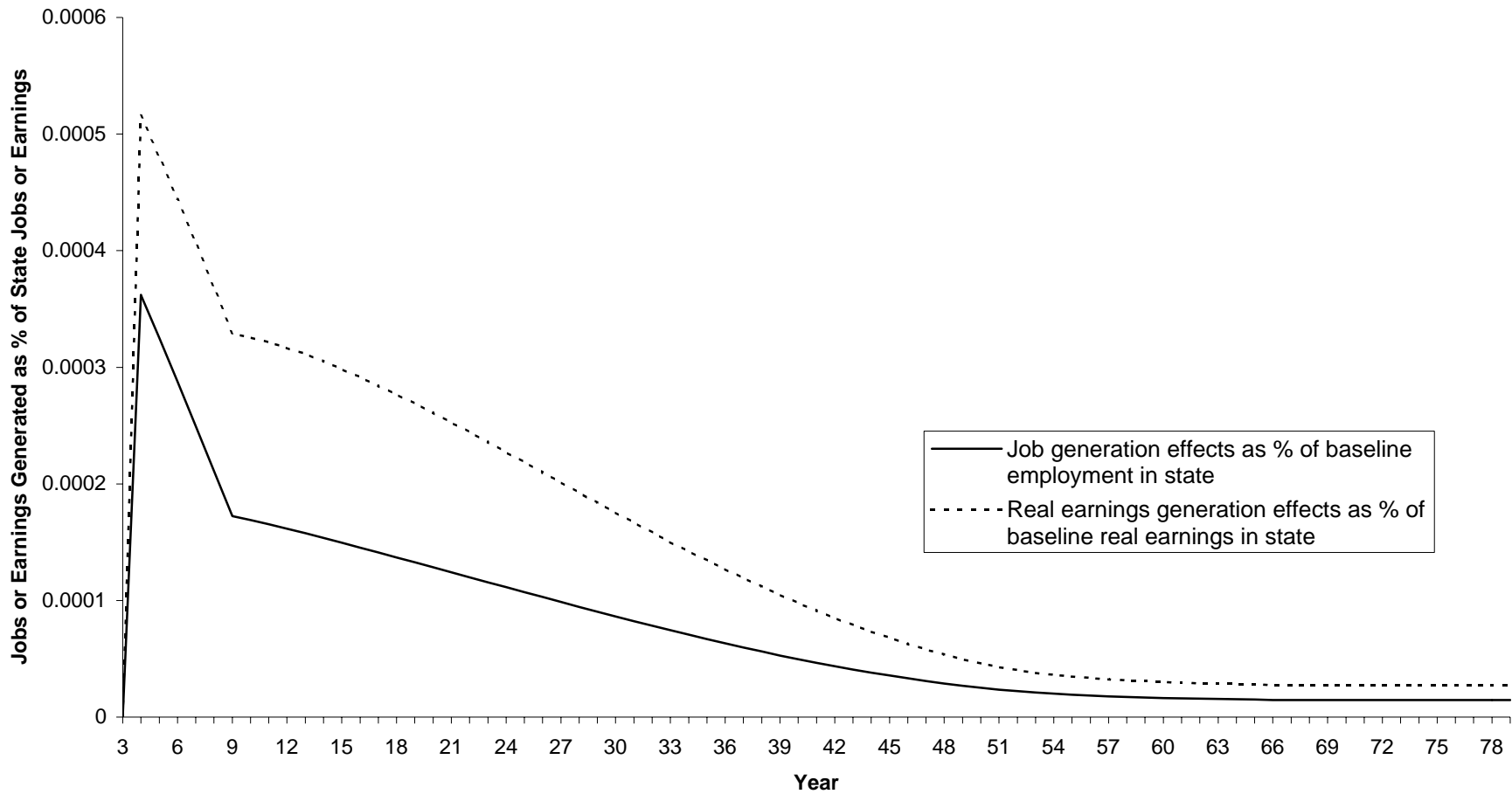
Notes: Real earnings are in millions of year 2004 dollars per year. Year 4 is year the economic development subsidy program is started.

Figure 3. Jobs and Real Earnings Generated from Permanent Economic Development Subsidy Program Equal in Costs to Universal Preschool Program, as Percentage of Baseline State Employment and Wage and Salary Earnings



NOTES: Economic development program examined would each year devote as much resources to new subsidies as the cost of operating a universal preschool program in that state that year. The jobs and earnings generated reflect effects on the current state residents of each year, as a percentage of the state baseline jobs and earnings in each year without the program.

Figure 4. Balanced Budget Multiplier Effects of Universal Preschool on State Residents' Jobs and Earnings, as Percentage of Baseline Jobs and Earnings



Notes: Effects shown are effects of universal preschool program, due to increased net spending and taxes, on the jobs and earnings of state residents, compared to their baseline jobs and earnings.

Figure 5. Job Generation Effects of One-Time Preschool Program of \$1 Billion, Compared to Job Generation Effects of One-Time Economic Development Subsidy Program of \$1 Billion

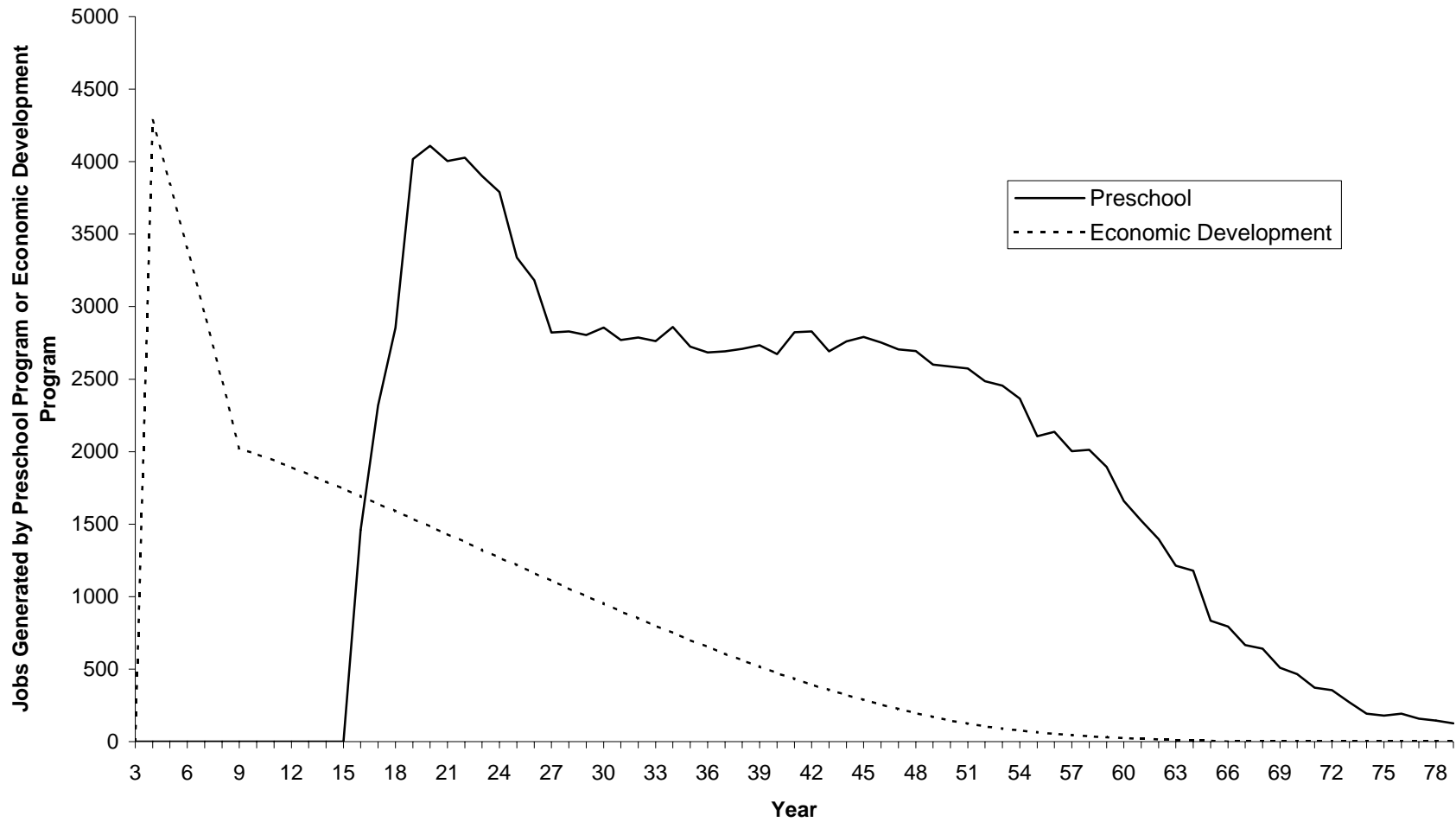


Figure 6. Real Earnings Generation Effects of One-Time Preschool Program of \$1 Billion, Compared to Real Earnings Generation Effects of One-Time Economic Development Subsidy Program of \$1 Billion

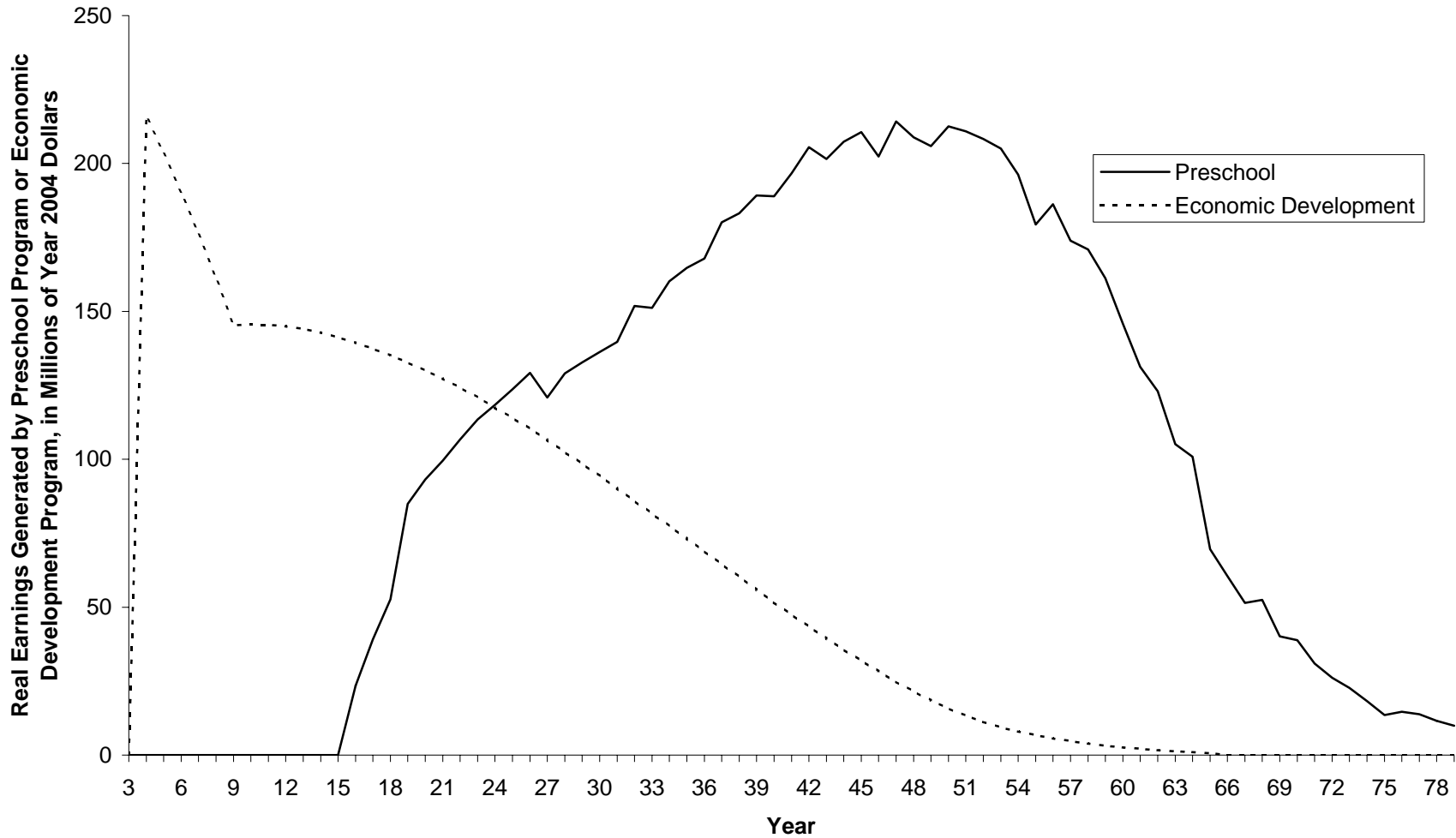
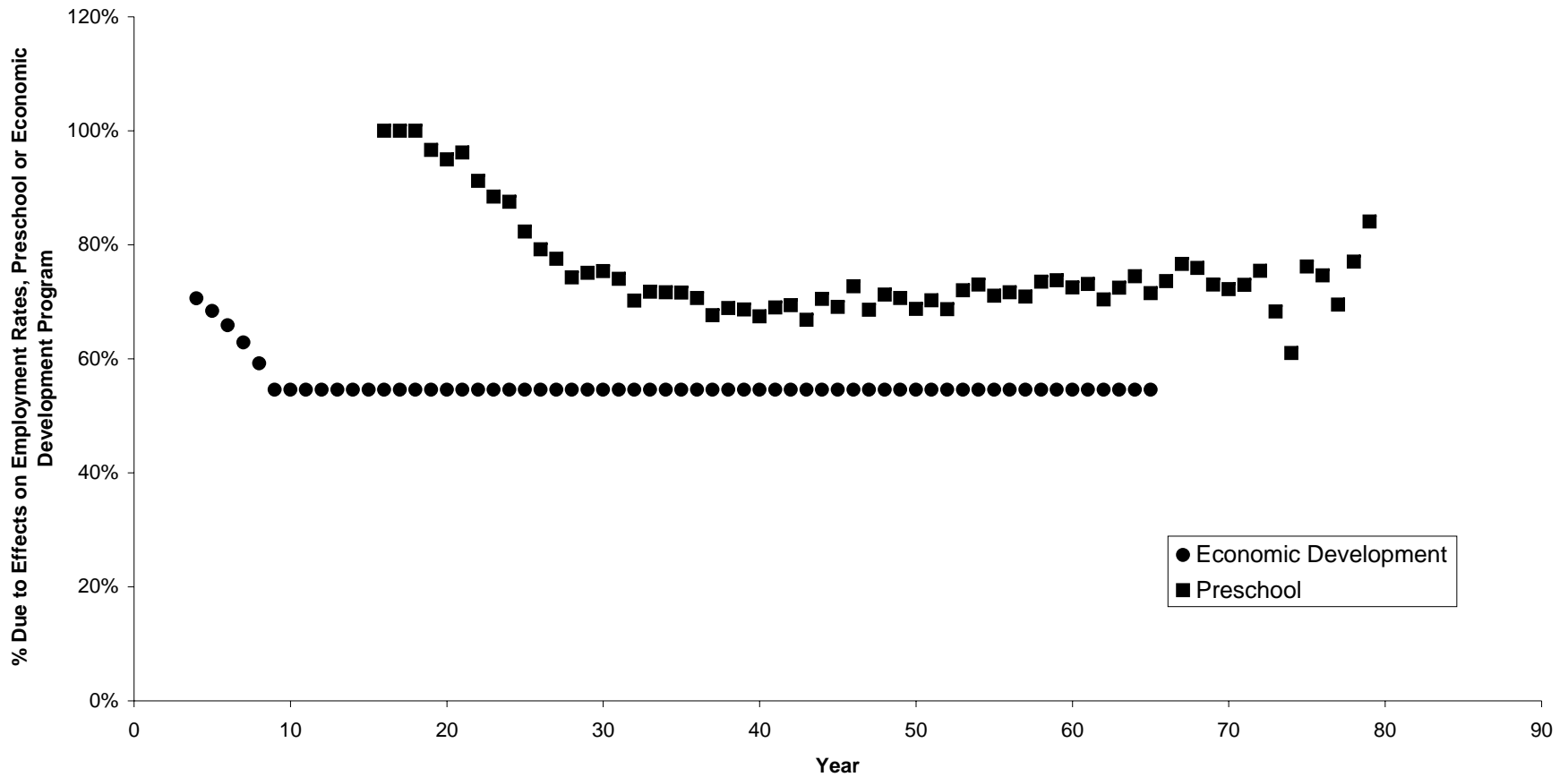
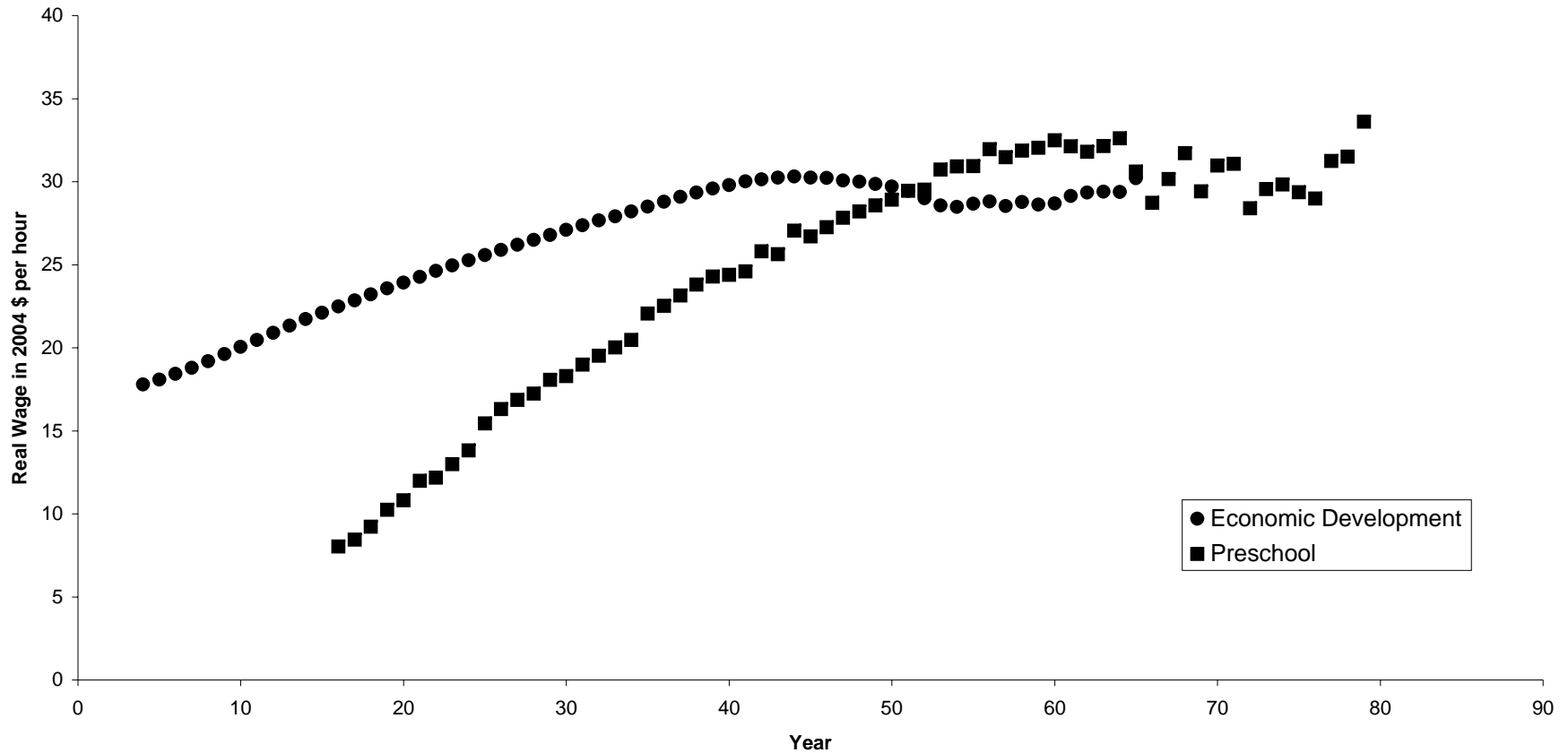


Figure 7. Percentage of Real Earnings Effect Over Time Due to Effects on Employment Rates, One-Time Preschool Program and One-Time Economic Development Program



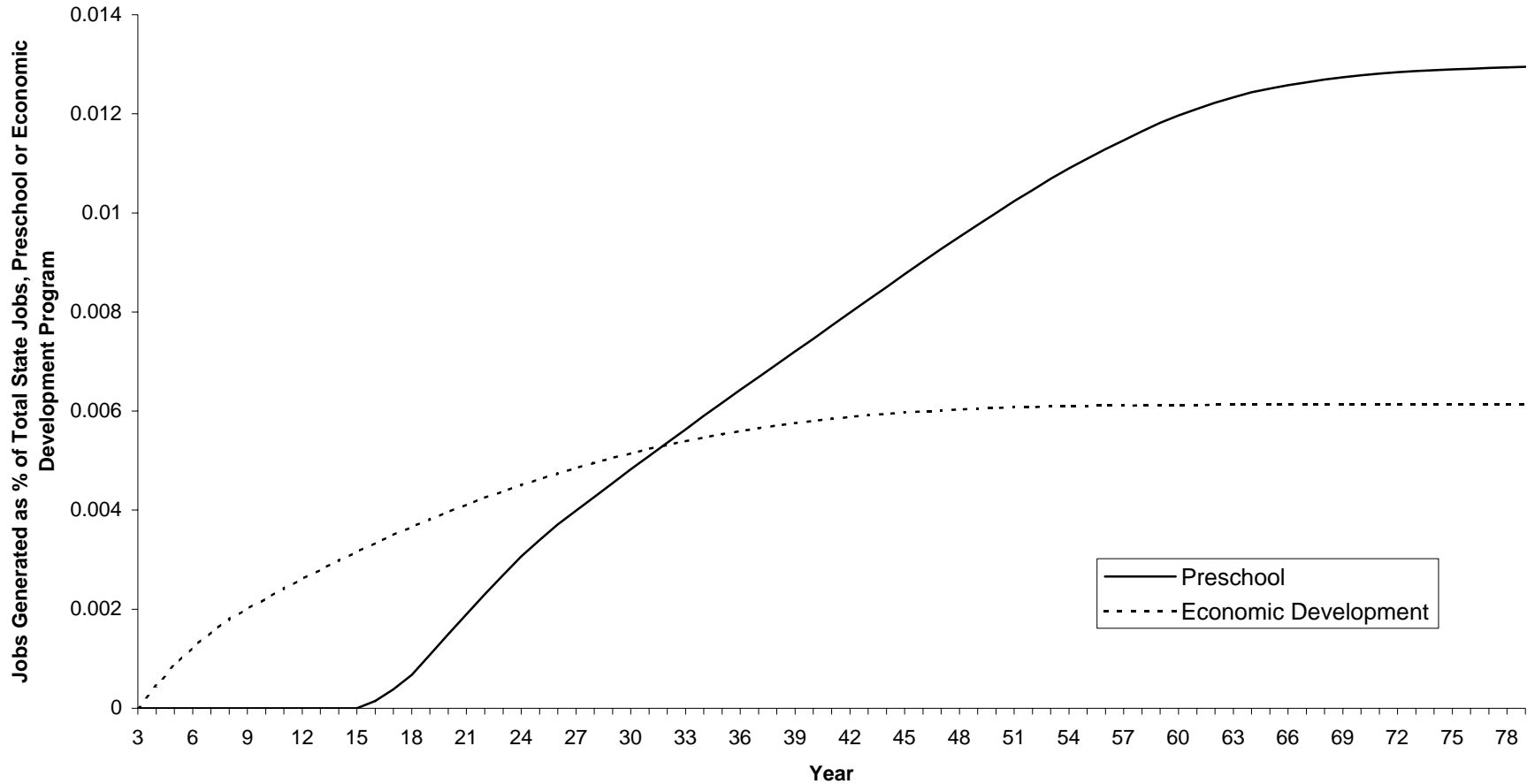
NOTE: Figure shows the percentage of the real earnings increase of a one-time program of either preschool or economic development, that is due to increased employment rates. The remainder is due to increases in wage rates. These percentages are based on real earnings effects for the original state residents, excluding in-migrants and out-migrants. In addition, these effects are based on employment rate gains and wage rate gains for individuals, rather than change in averages for state due to changing composition of workforce. As discussed further elsewhere in this report, preschool programs, by increasing employment of lower education individuals, tends to have depressing effects on state average wages. However, preschool does increase wages of all participants and peers.

Figure 8. Average Real Wages of Jobs Generated Under Preschool Programs and Economic Development Programs, At Various Years After One-Time Program



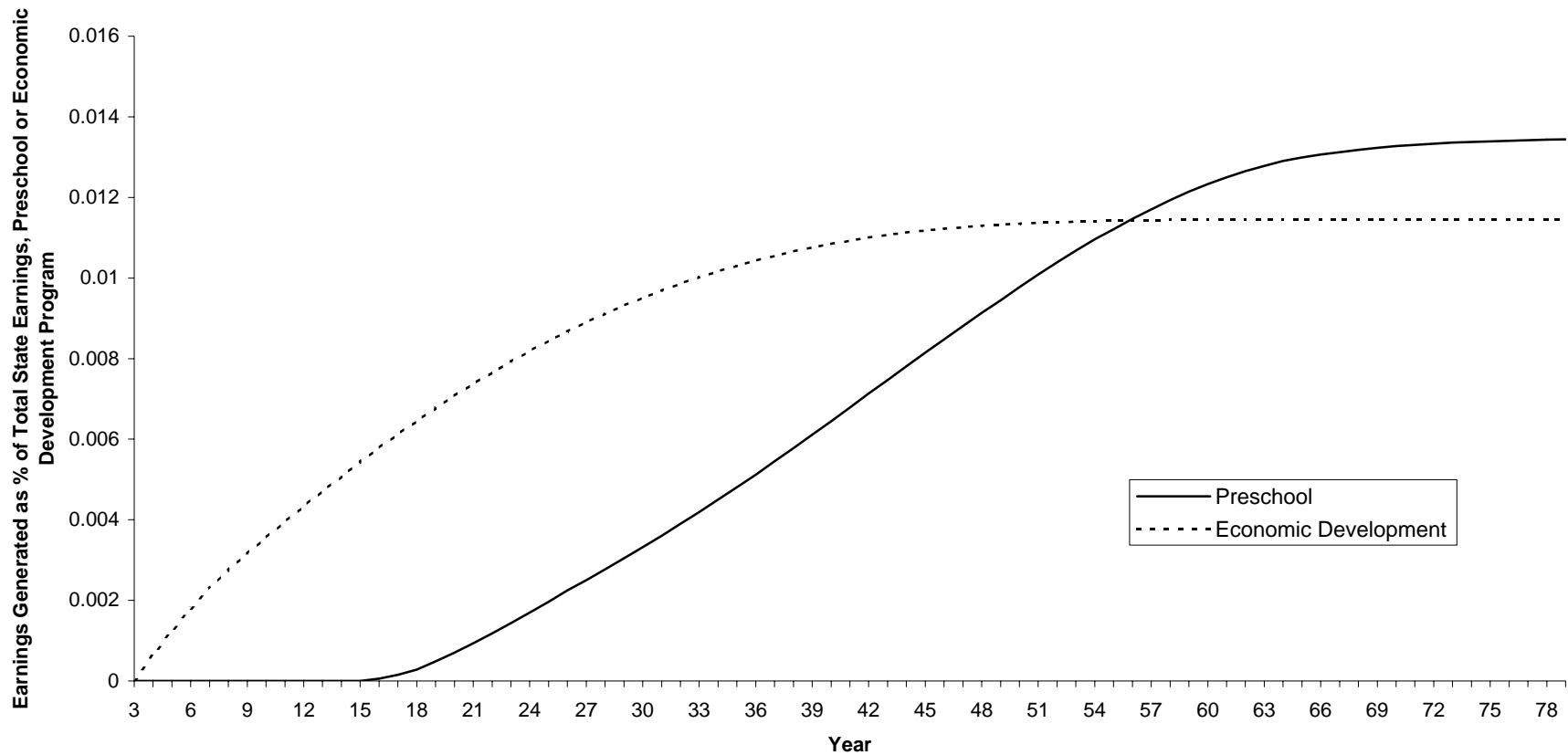
NOTE: This figure shows the average real wage of jobs generated for state residents by a one-time program of either preschool or economic development subsidies, at various years after program is initiated and completed in year 4. The real wages incorporate assumption that overall real wages increase over time by 1.2% per year. The real wage figures for the economic development program assume that real wage is initially equal to average real wage of \$17.79 per hour, and that added jobs are divided among all ages in proportion to their share in employment. Real wage alters over time as this initial cohort ages and as secular increase in real wages occurs. The real wage figures for the preschool program assume that real wages are based on the educational attainment of the program cohort at that age, and the secular increase in wages.

Figure 9. Jobs Generated for State Residents by Permanent Universal Preschool Program, Compared to Jobs Generated by Economic Development Subsidy Program of Same Cost



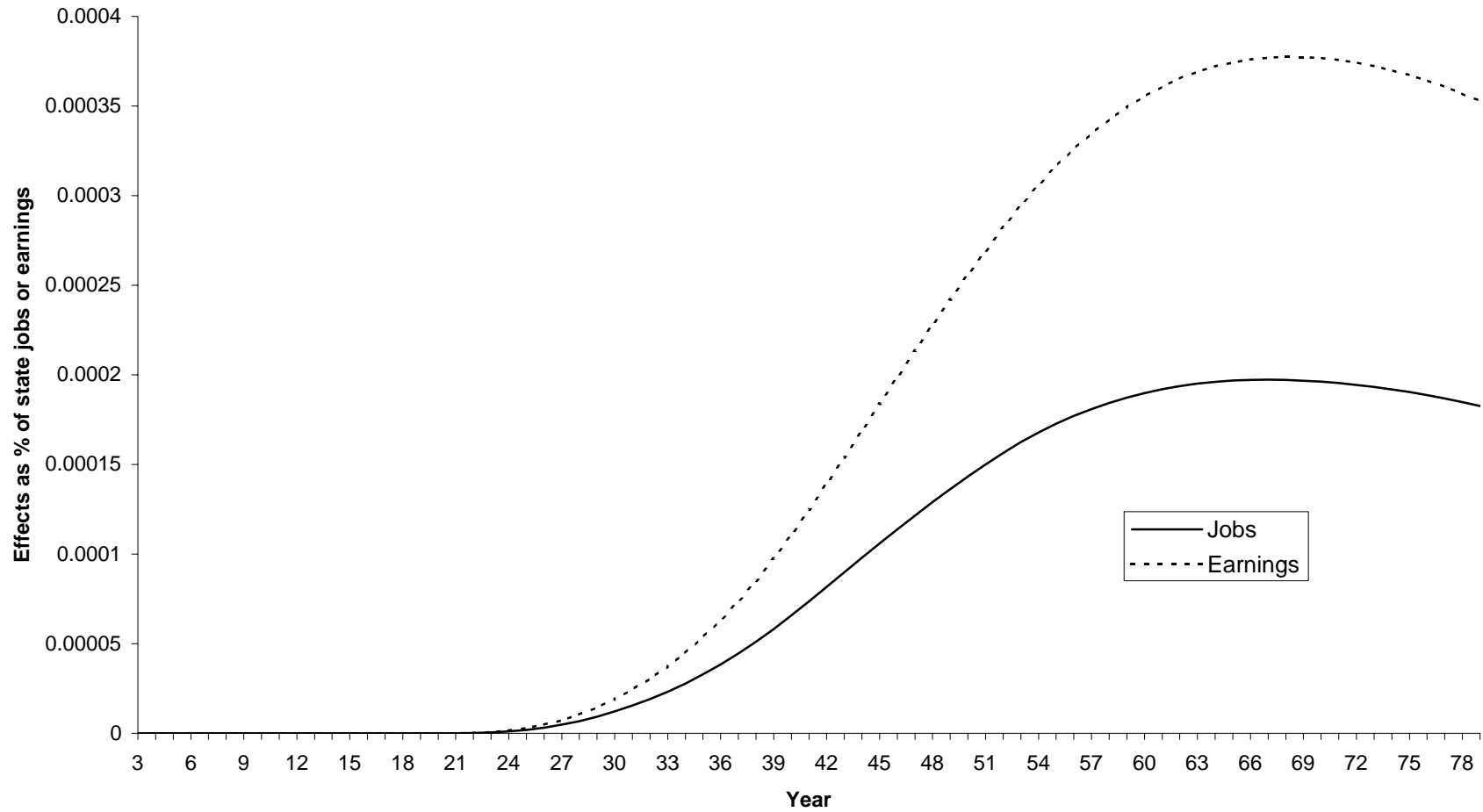
NOTE: The jobs generated for the preschool program are solely due to effects on participants and peers, and omit all other avenues of influence of preschool programs on job generation (e.g., balanced budget multiplier, labor supply of parents, etc.). The jobs generated as % of baseline employment compares jobs generated to total state baseline employment, which is assumed to grow at 0.3% per year. These percentage figures for preschool would apply to any state that adopted a universal preschool program of this design, or an economic development program that had same cost as such a program.

Figure 10. Real Earnings Generated for State Residents by Permanent Universal Preschool Program, Compared to Real Earnings Generated by Economic Development Subsidy Program of Same Cost



NOTE: The earnings generated for the preschool program are solely due to effects on participants and peers, and omit all other avenues of influence of preschool programs on earnings generation (e.g., balanced budget multiplier, labor supply of parents, etc.). The earnings generated as % of baseline state earnings compares earnings generated to total state baseline earnings, which is assumed to grow at 1.5% per year. These percentage figures for preschool would apply to any state that adopted a universal preschool program of this design, or an economic development program that had same cost as such a program

Figure 11. Preschool's Effects on Jobs and Earnings Via Its Effects on the Social Return to Education by Increasing the State College Graduate Percentage



NOTE: This only shows the effects of preschool education on jobs and earnings, due to preschool's effects on the state's college graduation percentage, that occur due to the social return to education. The effects of increased educational attainment for each individual preschool participant, and his or her school peers, are already reflected in estimates previously presented.