Investing in Kids: Early Childhood Programs and Local Economic Development

Addendum: Technical Appendices

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Appendix 2A

Plausible Calculations of Medium-Term Benefits and Costs of Economic Development Incentive Programs for a State or Metropolitan Area

This appendix presents in table form plausible medium-term flows of benefits and costs associated with an economic development incentive program. This appendix is a slightly revised version of what was previously presented in Bartik (2005). This table is an expansion and updating of calculations I have previously presented in Bartik (1991a, 1992, 1994b, 2004) and Bartik, Eisinger, and Erickcek (2003). The incentive program considered is for a state or a metropolitan area. The benefits and costs estimated are for the residents of the state and metropolitan area, and their governments and businesses. Benefits and costs to the federal government, or to persons, businesses, or governments outside the state or metropolitan area, are not considered. The benefits and costs are calculated as annual flows over some medium term after the incentives are provided, say five years. Some table entries also speculate about shorter-term or longer-term benefits and costs. Benefits and costs are calculated in two ways: 1) as percentages of annual state or local personal income for a 1 percent once-and-for-all labor demand increase to state or metropolitan area employment, with this 1 percent employment increase induced by incentives of average effectiveness; and 2) as real dollars, using prices of 2003, per job induced by incentives of average effectiveness. This appendix will go through Table 1 line by line, explaining how each line is calculated based on the research literature and various data sources.

Table 2A.1 Estimated Benefits and Costs of Economic Development Incentives

Category	Benefits/costs as % of local personal income for 1% induced employment growth (column A)	Benefits/costs in annual real 2003 dollars per induced job (column B)
(1) Incentive costs	−0.218	−9,699
(2) Fiscal effects		
(2.1) Induced revenue from additional business tax base	0.055	2,425
(2.2) Net incentive cost = (1) + (2.1)	−0.163	−7,274
(2.3) Net long-run fiscal effects of equal employment and population growth	0	0
(2.4) Gross effects of extra jobs on revenue from business tax base	0.011	485
(2.5) Required public services for extra jobs	−0.006	−285
(2.6) Net fiscal effects of "profit" on extra business tax base = 2.4 + 2.5	0.005	200
(2.7) Reduced social spending and unemployment benefits due to higher employment rates	0.019	845
(2.8) Sales/income taxes on increased personal income of local residents = 3.8	0.018	795
(2.9) Property taxes on increased real estate values	0.008	343
(2.10) Short-run fiscal effects: positive if underutilized infrastructure, negative if growth requires expensive new infrastructure	?	?
(2.11) Net quantifiable fiscal effect = 2.6 + 2.7 + 2.8 + 2.9	0.049	2,183
(3) Labor market effects		
(3.1) Gross real earnings gains for local residents	0.317	14,104
(3.2) Extra real earnings on new job	0.916	40,766
(3.3) Subtracting out earnings of in-migrants = 80% of (3.2)	−0.733	−32,613
(3.4) Net earnings of local residents on new jobs = 3.2 + 3.3	0.183	8,153
(3.5) Increase in real wages due to promotion of local residents to better-paying occupations = 3.1 − 3.4	0.134	5,950
(3.6) Loss of social spending transfers = 2.7	−0.019	−845
(3.7) Net increase in real income of local residents before taxes = 3.4 + 3.5 + 3.6 = 3.1 + 3.6	0.298	13,258
(3.8) Sales/income taxes on increased income of local residents	−0.018	−795
(3.9) Increase in income of local residents after taxes = 3.7 + 3.8	0.280	12,463
(3.10) Reservation wages in low unemployment local area = 90% of 3.4	−0.165	−7,338
(3.11) Reservation wages in high unempl. area: assumed zero	0	0

Category	Benefits/costs as % of local personal income for 1% induced employment growth (column A)	Benefits/costs in annual real 2003 dollars per induced job (column B)
(3.12) Net labor market benefits in low unemployment area = 3.9 + 3.10	0.115	5,125
(3.13) Net labor market benefits in high unemployment area = 3.9 + 3.11	0.280	12,463
(3.14) Shorter-run or longer-run labor market benefits: probably greater in short run, less in long run	?	?
(4) Real estate effects		
(4.1) Gross gains in real estate values, as annual income flow	0.077	3,426
(4.2) Increased annual property tax	−0.008	−343
(4.3) Net gain to property owners	0.069	3,083
(5) Locally owned business effects: Profit increase at businesses serving local market, decrease at export-base businesses.	?	?
(6) Environmental/congestion effects: likely to be negative unless project involves restoring brownfields	?	?
(7) Community effects: Some loss in community character and increased rents for local residents for growth beyond original community size, some gain for growth that restores community's customary size	?	?
Total quantifiable effects		
(8.1) In low unemployment local labor market = 1 + 2.11 + 3.12 + 4.3	0.016	692
(8.2) In high unemployment local labor market = 1 + 2.11 + 3.13 + 4.3	0.181	8,030

The gross incentive costs (line [1]) are derived assuming that the response of state or metropolitan employment with respect to an incentive will be equivalent in gross costs to the foregone business tax revenue needed to induce increased local activity if the elasticity of local employment with respect to state and local business taxes is −0.25. As derived in footnote (4), the gross foregone business tax revenue per induced job (line [1], column B) is $dR/dJ = (JdT)/dJ$

$= T(1/E)$, where dR is the gross change in business tax revenue due to a reduction in business taxes, J is the number of jobs, dJ is the number of induced jobs, T is the business tax rate calculated as state and local business taxes per job, dT is the change in that tax rate, and E is the elasticity of state and local employment with respect to the business tax rate, which is assumed to be −0.25, a compromise between the −0.3 preferred in the literature review of Bartik (1992) and the −0.2 preferred by Wasylenko (1997). Business tax revenue per job is calculated as detailed in endnote (4) of the chapter. Line (1)(A) is derived in a similar manner, using the equation that the foregone taxes as a percentage of income needed to induce 1 percent employment growth will be $= 100(dT/Y) = ((1\%\text{ employment growth})/E)(T/Y)$, where T is now business taxes in dollar terms, Y is personal income, and state/local business taxes as a percentage of personal income are assumed to be 5.46 percent, based on calculations in Bartik (1991a, p. 180).

Lines (2.1) through (2.2) present a side calculation showing fiscal effects and net incentive costs if the only fiscal effect considered is the extra business tax revenue from enhancing the business tax base. This extra business tax revenue is simply the business tax revenue associated with the induced jobs. Line (2.1)(B) is business tax revenue per job in 2003 dollars, based on calculations in endnote (4) of Bartik (2005), as the (B) column expresses everything per one induced job. Line (2.1)(A) is 1 percent of average business tax revenue as a percentage of personal income, because the (A) column expresses everything per 1 percent in induced extra employment. Line (2.2) then shows a supposed "net incentive cost"—which is, however, erroneous, because it omits all the fiscal effects from the public services associated with the extra business tax base, as well as the taxes and public services associated with the extra households, and also the effects of higher employment on the need for social services and revenue from the property tax. This erroneous calculation is the style of calculation frequently done by advocates for incentives who claim that such incentives “pay for themselves.” Line (2.2) shows that such incentives clearly don’t pay for themselves even if we only look at the business tax base gains.

To simplify the analysis of the full fiscal effects, I start from the baseline of the long run fiscal effects of employment growth and population growth when both increase by the same percentage. This baseline is straightforward to analyze, and the actual net fiscal effects of the incentive-induced growth are then analyzed as effects of deviations from this baseline. Line (2.3) assumes that if state and local public services are constant returns to scale in the long-run, as indicated in Fisher (1996) and Inman (1979), a balanced increase in employment and production should produce equal tax revenue and public service needs. But of course we don’t expect that induced jobs will bring about the same percentage increase in population. Based on Bartik (1991a, 1993a), we expect that for a given percentage increase in induced employment, the percentage increase in population will be about four-fifths as much. So if we analyze the fiscal effects as if for every 1 percent in induced jobs, we have four-fifths of 1 percent in increased population. The fiscal effects, then, are a combination of a “balanced” increase of four-fifths of 1 percent in both employment and population, which should have zero fiscal effects in the long run, and the fiscal effects of the “extra” one-fifth of 1 percent of jobs. The effects of these extra jobs are the effects of these extra jobs on the business tax base and required public services, as well as the effects of the extra jobs, via a higher employment rate, on the taxes and transfers associated with higher earnings for local residents, considered in the section on labor market effects.

Line (2.4) calculates the business taxes from the extra jobs as one-fifth of the business taxes from the business tax base associated with all the jobs, from line (2.1). Line (2.5) is based on Oakland and Testa's (1996) calculation that business tax revenue is 70 percent greater than public services directly required by businesses. Line (2.7)(A) is based on estimates from Bartik and Eberts (1999) that 1 percent employment growth reduces welfare caseloads by 6 percent, and estimates by Chernick and McGuire (1999) that own-source state and local spending on social services is 1.3 percent of personal income; social services spending is assumed to decline by the same percent as welfare caseloads. This yields a decrease in social services spending as a percent of income of −0.008 percent. This may seem small compared to overall earnings gains of 0.317 percent (see line [3.1] below), but growth is only modestly progressive; about 4.2 percent of the total earnings gains from stronger regional labor demand goes to the bottom income quintile, which is not much more than their share of income (Bartik 2001, Table 5.3). In addition, line (2.7)(A) is based on estimates (Bartik 1991b, Table 2) that 1 percent extra local employment growth in the short run reduces unemployment payments by 3.4 percent. Based on 1995 statistics from O'Leary and Wandner (1997, p. 733), and 1995 personal income data from the Regional Economic Information System, UI benefits are 0.33 percent of personal income, so a 3.4 percent reduction in such payments will reduce unemployment benefit payments by 0.011 percent of personal income. Adding 0.011 percent to the 0.008 percent reduction in social spending yields the 0.019 percent figure shown in line (2.7)(A). Line 2.7(B) is derived from (A) by using ratios. Line (2.8) is based on the taxes associated with the extra earnings of local residents and will be discussed further when the labor market section of the table is discussed. Line (2.9) is based on the property taxes on the increased real estate values associated with growth and is discussed further in that section. Line (2.10) is based on case studies by Altshuler and Gomez-Ibanez (1993) that show that new required infrastructure frequently vastly exceeds tax revenue from growth; because existing infrastructure will eventually require replacement, this suggests that depreciation charges for existing infrastructure are understated.

Line (3.1)(A) is based on estimates reported in Bartik (1991a, p. 163) on effects of growth on real earnings, expressed as a percentage of personal income by assuming earnings are 73.5 percent of personal income (Bartik, 1991a, p. 163). Line (3.1)(B) uses ratios to calculate this on a per job basis. Lines (3.2) through (3.5) attempt to divide line (3.1) into various components: gains for workers newly employed versus gains for workers already employed who get better jobs. Line (3.2) attempts to replicate what a naive benefit-cost analysis would assume about earnings gains: they are equal to the earnings on the induced jobs. Line (3.2)(B) is based on dividing total earnings by total employment, using 2002 data from the Regional Economic Information System. Line (3.2)(A) uses ratios to calculate this as a percentage of personal income. Line (3.3) subtracts out the earnings of in-migrants to get the effects on local residents who get jobs in line (3.4). The rationale for subtracting line (3.3) is twofold: first, this analysis takes a local perspective in which only the original residents count, and second, the analysis in Bartik (1991a) suggests that the well-being of in-migrants is not substantially affected by extra jobs in this local area, as the in-migrants would otherwise move to a similar local area. After subtracting line (3.4) from line (3.1), the remaining earnings gain must be from local residents moving up to better-paying jobs. The residual calculation for line (3.5) appears roughly consistent with data from Bartik (1991a) on how employment growth affects occupational upgrading for local residents. The loss of transfer income in line (3.6) was previously derived for line (2.7). Line 3.8 is based on estimates from Citizens for Tax Justice that state and local personal sales and income taxes in 1995 averaged 6 percent of income for households in the middle-income quintile (Ettlinger et al. 1996, Appendix 1, p. 51). In line (3.10), the reservation wage figure of 90 percent is used in Bartik (1991a) based on a review of the reservation wage literature. The assumption that reservation wages are zero for the unemployed in high unemployment areas is arbitrary. This assumption might be justified, even if nonwork time has some value for the unemployed, as seems likely if unemployment has sizable social costs such as increased crime, increased social problems for the children of the unemployed, or an increase in the employed's perceived risk of unemployment. Lines (3.12) and (3.13) emphasize how different the labor market benefits are based on different assumptions about reservation wages. Line (3.14) reflects that estimates suggest that the earnings effects reported in this table for local residents are probably greater in the short run and less in the long-run than the medium-run figures used here (Bartik 1991a; Bartik 1994b). The question mark for line (3.14) suggests that it is unclear how this would affect a present-value analysis compared to simply using the medium-run annual flow benefits and costs reported in the table.

Line (4.1)(A) comes from Bartik (1994b, Table 3) and is based upon estimates by Bartik (1991a) that 1 percent employment growth increases real estate values by 0.451 percent. A 10 percent real discount rate is used to convert changes in capital values to annual flows. Line (4.1)(B) is derived from (A) using ratios. Line 4.2 is based on Table 3.13 of the 2001 American Housing Survey, which estimates that the median residential property tax rate for owner-occupied housing is 1 percent of value. (The AHS is available from the U.S. Census Bureau at http://www.census.gov/hhes/www/housing/ahs/ahs01/ahs01.html.)

The line (5) discussion assumes that only locally owned businesses should be considered in this local benefit-cost analysis. This is consistent with this analysis focusing on the perspective of the state or metropolitan area and ignoring effects on the federal government or other state or metropolitan areas. Growth will clearly increase nominal wages and prices, as shown in Bartik (1991a), which reduces profits for businesses selling to outside markets. But businesses with some comparative advantage that they can maintain as the area grows (e.g., a local newspaper) will likely increase profits due to growth, as discussed in Bartik (1991a).

For more discussion of the environmental effects of local economic development, and of brownfields, see Bartik (2004).

The line (7) entry assumes that in a world with imperfect mobility, changes in a community's "character" that bring it away from the originally chosen amenity package of the area's households, with the accompanying wage and price changes, will reduce utility of the area's original residents, as these original residents must have preferred the original amenity package given prevailing wages and rents. For more on this type of model, see Bartik (1991a, pp. 73–76) and Bartik (1986).

There is considerable uncertainty in these figures; for example, I could come up with a rationale for adjusting the incentive cost figures and the earnings gains numbers up or down by 50 percent or more. Stating the numbers in this table to three, four, or five digits is an aid to calculation, but is a misleading indication of how much we really know. Therefore, it would be relatively easy to come up with a scenario under which quantifiable net benefits of economic development in a low unemployment area are negative.

Appendix 2B

Estimating the Displacement Effects and Wage Effects of Labor Supply Programs in Local Economies and National Economies

This appendix explores what might be plausible estimates of the displacement effects and wage effects in local labor markets and national labor markets of shocks that increase "internal labor supply." By shocks to internal labor supply, I mean exogenous increases in the labor force participation rates or labor supply quality of local residents. Displacement effects represent the reduction in employment or earnings in the relevant labor market, local or national, due to a labor supply shock as a proportion of the employment or earnings effects of the labor supply shock on those whose labor supply increases within that market. Wage effects represent the reduction in wage rates in the relevant labor market due to these local labor supply shocks.

We have much weaker empirical information on the local labor market effects of labor supply shocks to "internal labor supply" than we do on shocks to local labor demand or shocks to local labor supply through migration or immigration. For local labor demand, there are good indicators of shocks to local labor demand. For example, the local employment growth predicted by the local industry mix, and by national growth rates of each industry, has been shown to be a good proxy to shocks to national demand for the local area's export-based industries (Bartik 1991a). These local labor demand shock indicators can be used as instruments for local employment growth to consistently estimate reduced-form models of the local labor market effects of labor demand shocks. As discussed in the text of the book, these estimates suggest persistent effects of local labor demand shocks in increasing local labor force participation rates and occupational attainment.

For immigration, there are some reasonable natural experiments such as the Mariel Boatlift (Card 1990) that can be used to estimate the local labor supply effects of immigration. There are also some reasonable instruments for supply shocks to immigration. For example, exogenous immigration supply shocks can be predicted using data on prior local concentrations of different immigrant nationalities as well as using national trends in immigration from different countries, as immigrants will tend to cluster with previous immigrants from that same location. Most of these studies suggest that exogenous increases in local immigration have small if any displacement effects and wage effects. However, immigration is a different type of labor supply shock from increases in a local area's internal labor supply that are due to increased labor force participation rates or higher labor skills of current residents. The population increase due to immigration will have demand-side effects by increasing assets in the community, and by increasing short-run demands for housing construction and other infrastructure construction (Greenwood and Hunt 1995).

For increases to "internal labor supply," there are few obvious variables that would produce large observable exogenous variation in such labor supply. This in part explains the relative paucity of research literature in this area. I have written some prior papers and sections of books on displacement effects, and there are a few other papers, mostly on welfare reform effects, but there is relatively little other literature.

Given problems in finding exogenous sources of internal labor supply shocks, it seems unwise to rely on reduced form estimates of the local labor market effects of such shocks.

Rather, it seems preferable to set up a plausible structural model and see if we can find plausible estimates of the structural parameters.

In setting up this model, what we are aiming at estimating are the effects of the labor supply shock in the local or national labor market on the employment or earnings in that labor market. That is, we are aiming at estimating effects of the labor supply shock on employment to population ratios and earnings to population ratios.

The model I use is a simple efficiency wage model;

(Labor supply) $L^s = L^s\ (W, U)$

(Labor demand) $L^d = L^d\ (W, U)$

(Definition of unemployment rate) $L^s\ (1 - U) = L^d$

(Wage curve) $W = W(U)$

(Population equation) $P = P(W, U)$

where L^s is the overall labor supply in the relevant labor market (initially a local labor market, later the national market), L^d is the overall labor demand in the relevant labor market, W is the real wage rate, U is the unemployment rate, and P is the population. The model has five equations in five unknowns (labor supply, labor demand, wages, unemployment, and population). The behavior of the model in response to shocks depends on the parameters of the various equations.

This model can be rationalized by efficiency wage models of the labor market. Employers are assumed to find it profitable to set wages above the wage level that would clear the labor market. Such "above-market-clearing wages" are profitable for each individual employer because the higher wage reduces turnover and hiring costs, and it increases worker productivity by improving worker morale and providing a greater incentive for staying at this job. But if many employers increase wages, this results in a market wage above the market-clearing wage, with involuntary unemployment. This involuntary unemployment restrains the extent of wage increases because unemployment also reduces turnover and hiring costs, and it increases worker productivity on the job.

It is probably obvious that labor supply is positively affected by wages and negatively affected by unemployment, and that labor demand is negatively affected by wages. The model assumes that labor demand is positively affected by unemployment, which seems initially counterintuitive. However, in this case higher unemployment does not proxy for lower output demand. (In fact, in this simple model, output is implicitly endogenously determined by labor demand.) Unemployment here is merely a labor market phenomenon that holds output determinants constant. The assumption here is that higher unemployment, by reducing hiring and turnover costs and by increasing worker productivity, will increase labor demand, holding other output determinants constant.

We will solve this model for the effects of a labor supply shock. We will consider percentage effects on employment-to-population ratios ($dL^d / L^d - dP/P$) and percentage effects on real earnings per capita ($dL^d / L^d + dW/W - dP/P$). We consider these percentage effects relative to the direct percentage employment and earnings effects of the supply shock on the individuals affected by the supply shock who stay in the local labor market.

A word on how to interpret the effects on real wages that make up part of the effects on real earnings per capita. These effects may not take place through wage changes for a given occupation. In the case of local demand shocks, we know that all of the wage effects take place through changes in occupational attainment. Controlling for worker characteristics, wages do change. Controlling for occupation, wages don’t change. Similar effects may occur for internal local supply shocks. These wage changes can be seen as a type of displacement. The labor supply shock may displace some other local residents from better-paying occupations.

If percentage effects on employment-to-population ratios or earnings per capita are less than the direct percentage effects that are due to the increased employment and earnings of those whose labor supply in the local labor market is increased, then the inference is that some other local residents are displaced in the labor market—that is, that they suffer reduced employment or earnings because of the internal labor supply increase. Both employment and earnings displacement are important. Earnings displacement directly affects the "bottom line" in measured dollar effects on earnings of the labor supply increase. Employment displacement may have some extra social effects if special social value is placed on employment.

Solving the model for percentage effects on employment, wages and earnings gives the following equations:

$d\text{Employ}/\text{Employ} - d\text{Pop}/\text{Pop} = Sq\ \{[1/(F + G)] * [G + a_p(1/g) + e_p]\}$

$d\text{Earn}/\text{Earn} - d\text{Pop}/\text{Pop} = Sq\{[1/(F + G)] * [G - 1 + a_p(1/g) + e_p]\}$

$F = ((-1/(1-U))(1/g) + e + a(1/g))$

$G = (-h - b(1/g))$

And note for reference also that

$dW/W = Sq(-1/(F+G))$

$d\text{Employ}/\text{Employ} = Sq(G/(F+G))$

$d\text{Pop}/\text{Pop} = Sq\ (1/(F+G))\ (-a_p(1/g) - e_p)$,

where

Sq is the direct labor supply shock expressed as a percentage effect on employment or earnings in the local labor market of those individuals whose labor supply is increased;

g is the percentage change in wages for a one-point increase in the unemployment rate (this parameter expected to be less than zero);

e is the total elasticity of labor supply with respect to the wage due to both effects on the labor force participation rate and the population (expected to be positive);

a is the total percentage change in labor supply, from both changes in labor force participation rates and population, due to a one-point increase in the unemployment rate (expected to be negative);

h is the elasticity of labor demand with respect to wages (expected to be negative);

b is the percentage change in labor demand due to a one-point increase in the unemployment rate;

a_p is the percentage change in the local population due to a change in the unemployment rate (expected to be negative);

e_p is the percentage change in the local population due to a change in the wage rate (expected to be positive).

Both F and G must be positive if all parameters have their expected sign. The three products summed in F will then each be positive (e.g., in the first term, we have −1 multiplied by the positive term $(1-U)$ and then multiplied by the negative term $(1/g)$, so the product is positive). Similarly, the two terms in G will be positive.

F can be roughly interpreted as the effective responsiveness of labor supply to wages. It includes both how labor supply responds directly to wages, and how labor supply responds indirectly to the unemployment changes associated with a change in wages. Similarly, G can be roughly interpreted as reflecting the effective responsiveness of labor demand to wages, both through the direct response of labor demand to wages, and the indirect responsiveness through the response to the unemployment change associated with a wage change via the wage curve.

More responsive labor supply and demand (higher *F* or *G*) will be unambiguously associated with lower wage declines in response to a labor supply increase. (See equation.) The employment demand response then depends upon how labor demand responds to these wage changes and to the associated unemployment changes—that is, the wage change is multiplied by *G*. More responsive labor demand (higher *G*) will increase the employment effects of an increase in labor supply. More responsive labor supply (higher *F*) will lower the employment effects of an increase in labor supply. The net effects on employment-to-population ratios and earnings per capita then must subtract out the effects on population, which depend on how population responds to the wage changes and the associated unemployment changes.

For this book, we are interested in the labor market effects of permanent preschool and other early childhood programs that raise labor force participation rates or wage rates of former participants when they reach working age, or of the parents of participants. These labor supply and labor market effects take place over a long time. Therefore, long-term elasticities would seem to be the most relevant.

The relevant labor supply elasticities are probably less controversial than the labor demand elasticities. Based on Fuchs, Krueger, and Poterba (1998), I assume that the labor force participation rate elasticity with respect to wages is 0.15. The available evidence suggests that population elasticities with respect to wages are also small. I assume 0.1. I assume that the labor force participation percentage response to a one-point increase in the unemployment rate is −0.492, and that the population percentage response to a one-point increase in the unemployment rate is −0.632, based on estimates in Bartik (2001, p. 328). These estimates are consistent with other research (Bowen and Finegan 1969; Herzog, Schlottmann and Boehm 1993).

Based on Blanchflower and Oswald's (1994) exhaustive review, I assume that the elasticity of wages with respect to the unemployment rate is −0.1. At an unemployment rate of 5 percent, this is a percentage response of wages to a one-point increase in the unemployment rate of −2.

The responsiveness of labor demand with respect to wages and unemployment is more controversial. At the local level, there are strong theoretical reasons to think that the labor demand elasticity with respect to wages will be quite large in magnitude. In his authoritative book *Labor Demand*, Dan Hamermesh (1993) concluded that the elasticity of substitution between labor and capital was close to one. This implies that the capital-constant but output-varying elasticity of labor demand with respect to the wage rate is −3.33. If capital varies as well, the elasticity of labor demand will be even more negative.

However, actually observed elasticities of employment or business location with respect to wages are much smaller. In my 1991 book, I found that the mean long-run elasticity of state or metropolitan business activity with respect to wages across 28 studies was −0.67. Across a more limited set of 10 studies with an employment-dependent variable, the elasticity was −0.89.

As I pointed out in my 1991 book, there are good reasons to think that these estimated local elasticities with respect to wages are biased towards zero. Real wages, controlling for labor quality, are hard to measure. Any measurement error will bias estimates towards zero. Wages are endogenously determined. Unobserved factors that shock labor demand will tend to also raise wages. These unobserved demand shocks will tend to cause a positive correlation between state and local business activity or employment and state and local wages.

Larger responsiveness of state and local business activity with respect to wages are suggested by empirical estimates of the effects of business taxes on wages. As shown in my 1991 book, we would expect the responsiveness of state and local business activity to any different business-cost factor to be roughly proportional to the share of that cost factor in overall business costs. Labor compensation is estimated to be about 14.4 times the cost share of state and local business taxes. Reviews of the literature on state and local business activity and business taxes suggest that the elasticity of state and local business activity with respect to business taxes is −0.3 (Bartik 1991a) or −0.2 (Wasylenko 1997). Therefore, based on the literature on state and local business activity and taxes, we expect the elasticity of state and local business activity with respect to wages to be −2.88 (= 14.4 × −0.2), or −4.32. State and local employment would be expected to be somewhat more responsive than this to wages, because of factor substitution effects. (The cost proportionality calculations assume that there are no factor substitution possibilities; with factor substitution, changes in wages will cause additional changes in the ratio of employment to business activity.)

For the baseline calculations of local-employment-and-earnings-per-capita effects of local labor supply shocks, I assume a wage elasticity of −2.88. This is consistent with the −0.2 effect of business taxes that is used in this book to simulate the effects of business incentives. I also consider a lower value consistent with the empirical literature on wage effects on state and local business activity, or −0.89. In addition, I consider a higher responsiveness of −6, which is consistent with some theoretical calculations of long-run labor demand elasticities, with capital and output allowed to vary.

There is not much information on how local labor demand might directly respond to unemployment. From my previous work, I calculate that the average percentage response of local labor demand to a labor supply shock, controlling for wages, is 0.72 (Bartik 2001). At a 5 percent unemployment rate, this corresponds to an average percentage effect on labor demand of a 1 percent change in unemployment of −2.73. (Some differentiation shows that the percentage effect on labor demand of a one-point change in the unemployment rate is equal to $(z/1-z)(1/(1-U))$, where z is the elasticity of labor demand with respect to labor supply.) I use this in my baseline calculations. I also consider the possibility that labor supply may have no direct effects on labor demand.

Table 2B.1 shows the baseline estimates and some alternative calculations. In the baseline, the percentage effects on local earnings per capita of a shock to labor supply, as a proportion of the percentage increase in earnings of those persons who increase their labor supply and stay in the local labor market, is 0.66. This implies about one-third displacement taking place in the local labor market. The ratio of the percentage effects on the employment population ratio, to the increase in jobs due to those directly affected by the labor supply shock, is 0.83. Thus, only one-sixth of the increased employment due to the labor supply shock ends up displacing other labor residents from jobs.

Table 2B.1 Estimated Responsiveness of Overall Earnings and Employment to a Labor Supply Shock

Local or national?	Assumptions	Ratio of percentage increase in earnings per capita to percentage labor supply shock	Ratio of percentage increase in employment per capita to percentage supply shock
Local	Baseline local assumptions	0.66	0.83
Local	Less responsive labor demand (−0.89)	0.47	0.74
Local	More responsive labor demand (−6)	0.78	0.89
National	Baseline national assumptions	0.68	0.85

NOTE: The calculated numbers are the ratio of the overall labor market earnings and employment effects to the change in earnings or employment of the group whose labor supply is increased. One minus these ratios would equal the displacement effects—that is, the loss in earnings and employment of other groups in the labor market. The earnings effects reflect both the displacement effects in employment and the effects on wages. These numbers reflect four distinct sets of assumptions about various demand and supply elasticities in the labor market. The first three are for a local labor market; the last set of assumptions is for the national labor market. The specific assumptions made are described in the text of Appendix 2B.

The variants in these baseline assumptions give ratios of percentage effects on earnings to direct earnings effects of the labor supply shock of 0.47 to 0.78. The employment per capita effects ratios in these alternative scenarios are 0.74 to 0.89. Thus, depending upon what measure is being used, and what scenario's assumptions are being used, labor supply shocks have net local labor market effects of 50 to 90 percent of their direct effects. Displacement is sometimes significant but does not eliminate the local labor-market effects of a labor supply shock.

What about effects at the national level? At the national level, I adjust the assumptions on wage and unemployment effects on labor supply to not allow for population effects. U.S. population is assumed to not adjust in response to labor supply shocks. For labor demand, we need to consider how demand is managed in the United States by the Fed and other macroeconomic policymakers. I assume output is set by national macro policy to moderate swings in prices, and to keep output demand in balance with the supply capacity of the economy. To incorporate this assumption, I assume that overall national labor demand is highly directly responsive to labor supply shocks. This responsiveness reflects national macro policy in response to unemployment or wage fluctuations. Specifically, I assume that the direct elasticity of labor demand elasticity with respect to labor supply is 0.9. This corresponds to an elasticity of labor demand with respect to the unemployment rate of 9.47. I also assume that the relevant labor demand with respect to the wage is the output-constant elasticity of demand for labor. Based on Hamermesh (1993), this elasticity is assumed to be −0.3.

The resulting national estimates show net effects on earnings of a labor supply shock, as a ratio to its direct effects, of 0.68. Net effects on employment are 85 percent of the direct effects. Therefore, earnings displacement is 32 percent and employment displacement only 15 percent. At the national level, the estimated modest displacement effects reflect the assumption that aggregate demand is managed to be quite responsive to labor supply.

The bottom line is that although displacement effects of labor supply shocks cannot be ignored, they are not so large as to eliminate significant positive effects on employment and earnings at both the local and national level. In the empirical estimates used in this book, I assume that displacement in both employment and earnings, at both the local and national level, is only one-third. That is, employment and earnings effects are two-thirds of the direct effects of preschool or other early childhood programs on the employment and earnings of former participants or their parents. This assumption may slightly understate likely effects on earnings. If displacement effects are as large as one-half, which is about the maximum in the scenarios considered here, then the employment and earnings effects estimated in this book are overstated by about two-thirds over one-half, or by a factor of [(2/3 / (1/2)] = (4/3). The true effects will be about 75 percent of the effects estimated here (75% = [(1/2 / (2/3)] = (3/4).

Appendix 3A

Ratio of State Economic Development Benefits to Incentive Costs for a Permanent Business Incentive Program

This table gives the ratio of the annual effects on state residents' earnings per capita of a permanent business incentive program, to the annual costs of the business incentives. The program is assumed to start in 2011 and continue at the same level forever. These are the numbers behind text Figure 3.1. The earnings increases that are counted are the increase in employment rates and wage rates of the state residents who lived in the state and had their employment prospects affected by the new jobs attracted by business incentives. Earnings of those who moved into the state because of the incentives are not counted. In addition, earnings increases for state residents who move out of the state are not counted.

Table 3A.1 Ratio of Annual State Economic Development Benefits to Annual Program Costs, Permanent Business Incentive Program

Year	Business incentive	Year	Business incentive
2011	1.897	2052	3.510
2012	1.843	2053	3.524
2013	1.787	2054	3.536
2014	1.729	2055	3.547
2015	1.668	2056	3.555
2016	1.605	2057	3.563
2017	1.561	2058	3.569
2018	1.527	2059	3.574
2019	1.501	2060	3.578
2020	1.479	2061	3.582
2021	1.594	2062	3.585
2022	1.707	2063	3.587
2023	1.816	2064	3.589
2024	1.922	2065	3.591
2025	2.025	2066	3.592
2026	2.125	2067	3.593
2027	2.221	2068	3.594
2028	2.314	2069	3.595
2029	2.403	2070	3.595
2030	2.488	2071	3.596
2031	2.570	2072	3.596
2032	2.649	2073	3.596
2033	2.723	2074	3.596
2034	2.794	2075	3.596
2035	2.861	2076	3.596
2036	2.925	2077	3.596
2037	2.985	2078	3.596
2038	3.042	2079	3.596
2039	3.095	2080	3.596
2040	3.145	2081	3.596
2041	3.191	2082	3.596
2042	3.234	2083	3.596
2043	3.275	2084	3.596
2044	3.312	2085	3.596
2045	3.346	2086	3.596
2046	3.377	2087	3.596
2047	3.405	2088	3.596
2048	3.431	2089	3.596
2049	3.454	2090	3.596
2050	3.475		
2051	3.493		

Appendix 3B

More Details on the Calculations of the Effects and Costs of Business Incentives

The Chapter 3 text states that a ten-year business tax incentive of $1,149 per job would be expected to affect the business location decision about 3.6 percent of the time. The Chapter 3 text also states that the present value of the costs of business tax incentives to the number of jobs created is about $200,000. Where do these numbers come from?

These numbers are derived from the extensive literature on how business location decisions and business growth respond to state and local business taxes. (See Bartik [1991a] or Wasylenko [1997] for the most extensive reviews.) Why use the literature on state and local business taxes rather than research specifically on business tax incentives? Largely this is done because the literature on the effects of overall state and local business taxes is much more extensive and in many cases uses better methodologies than the research literature on business tax incentives.

The essential assumption that is used to derive these estimates is that incentives for business location decisions 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 business tax incentives. Although state and local governments may seek to be more selective and choose to give business tax incentives 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 incentive to just tip the location decision.

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 percent reduction in all state and local business taxes will increase a state or metropolitan area's business activity by 2 percent. This elasticity holds all other variables constant, including the quality of state and local public services. Annual state and local business tax revenue per job seems to average about $4,269 per job in 2007 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 percent business tax cut of $426.90 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 426.90 × *x* workers per firm × 100 firms in tax revenue to gain in jobs *x* workers per firm × two firms. The annual cost per job gained is equal to (426.90 × *x* × 100) / (2 × *x*) = $21,345 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 $4,269 / (0.2) = 21,345, based on Peters and Fisher and Wasylenko. Note that this implies that the cost per job created varies proportionately to one over this business tax elasticity. Therefore, if the true business tax elasticity is 0.1 rather than 0.2, the costs per job created by business tax incentives will be doubled.

To determine what effects incentives 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 percent (Summers and Poterba 1994). Using this discount rate, the present value of a permanent tax cut of $21,345 per year is $199,220. This implies that giving firms a lump sum of cash upfront would have a cost per job created of $199,220. A lump sum of $199,220 corresponds to a 10-year subsidy, using a 12 percent discount rate, of $31,481 per job per year. If the average business tax incentive of $1,149 per job per year is to result in the same cost per job created, the incentive would have to be decisive in 3.6 percent of subsidized firms, as 3.6 percent = 1,149 / 31,481.

In the calculations for this book, the business incentive provided is assumed to be a tax incentive provided uniformly per job in real dollars over a ten-year period. Its value to the assisted business, and hence its effects on state job creation, are calculated using this 12 percent discount rate that is assumed to be used by corporate decision-makers. The social costs of this incentive are calculated using a 3 percent real discount rate. It is the present value of these social costs of the incentive that are entered in the denominator for calculating the ratio of the present value of state economic development benefits to the present value of program costs. On the other hand, the calculations of the ratios of annual state economic development benefits to program costs (Figure 3.1 and Appendix 3A) simply use the tax incentives actually provided each year.

Appendix 3C

Figures for Assumed Responsiveness of State Unemployment Rates and Labor Force Participation Rates to An Employment Increase in the State

Table 3C.1 presents the numbers behind Figure 3.2. The derivation of these figures is described in the text. These numbers show the assumed proportion of new jobs that come from reduced unemployment and increased labor force participation at various points after a one-time increase in employment in a state economy. The numbers are based on the empirical work in Bartik (1991a) and Bartik (1993a), as well as on assumptions about decay rates.

These estimated effects of new jobs on unemployment and labor force participation match what is observed during the first 10 or 20 years after a shock to jobs in a state or local economy. Beyond that time period, the simulation relies on a theoretically plausible extrapolation. The assumption is that effects on labor force participation rates only decay, because of mortality and out-migration. This roughly matches the slow decay observed during the first 10 to 20 years.

Table 3C.1 Assumed Effects over Time on State Unemployment Rates and Labor Force Participation Rates of Increase in State Employment

Year	Proportion of New Jobs that Reduce Unemployment Rate	Proportion of New Jobs that Reduce Labor Force Participation Rate
0	0.000	0.000
1	0.300	0.300
2	0.240	0.299
3	0.180	0.296
4	0.120	0.293
5	0.060	0.288
6	0.000	0.283
7	0.000	0.277
8	0.000	0.271
9	0.000	0.265
10	0.000	0.258
11	0.000	0.251
12	0.000	0.244
13	0.000	0.237
14	0.000	0.230
15	0.000	0.222
16	0.000	0.215
17	0.000	0.208
18	0.000	0.200
19	0.000	0.193
20	0.000	0.185
21	0.000	0.178
22	0.000	0.170
23	0.000	0.163
24	0.000	0.155
25	0.000	0.148
26	0.000	0.140
27	0.000	0.133
28	0.000	0.126
29	0.000	0.119
30	0.000	0.112
31	0.000	0.105
32	0.000	0.098
33	0.000	0.091
34	0.000	0.085
35	0.000	0.078
36	0.000	0.072
37	0.000	0.066
38	0.000	0.061
39	0.000	0.055
40	0.000	0.050
41	0.000	0.045
42	0.000	0.040
43	0.000	0.036
44	0.000	0.031
45	0.000	0.027
46	0.000	0.024
47	0.000	0.020
48	0.000	0.017
49	0.000	0.015
50	0.000	0.013
51	0.000	0.011
52	0.000	0.009
53	0.000	0.008
54	0.000	0.006
55	0.000	0.005
56	0.000	0.004
57	0.000	0.003
58	0.000	0.003
59	0.000	0.002
60	0.000	0.002
61	0.000	0.001
62	0.000	0.001

Appendix 3D

Responding to Lynch's Arguments Against the Effectiveness of Lowering Business Taxes

The estimates in this book of the costs of creating jobs through business incentives rely on the research literature on how taxes affect business location. This research literature provides various estimates of the elasticity of business location decisions and related measures (employment, investment, etc.) with respect to various types of state and local taxes. The revenue cost of creating additional business activity through tax reductions will be equal to (total revenue raised by the relevant tax per unit of business activity) / (elasticity of business activity with respect to the relevant tax). (This revenue-cost calculation only looks at the revenue loss on jobs that would have occurred anyway; the fiscal effects of the new jobs include any spending impacts as well as the additional taxes from new jobs.)

In my work, I have generally assumed that the relevant taxes to consider are state and local business taxes. The argument is that even in studies that look at all taxes, it seems likely that the main taxes that affect business location decisions are business taxes. Household taxes only indirectly affect business location decisions by affecting labor supply or other location variables. Therefore, it is assumed that a given percentage effect of all taxes really is proxying for a given percentage effect of state and local business taxes.

This approach is challenged in a 2004 monograph by Robert Lynch. Lynch argues that because many business location studies have been based on broader tax measures, which include nonbusiness taxes, the relevant tax measure should be all state and local taxes. He further argues that business location elasticities for business taxes tend to be smaller than the −0.20 figure preferred by Wasylenko, which I have sometimes used as well, for example in the research supporting this book. Finally, he argues that household taxes should affect business activity in a state, largely because of the demand-side effects of changing household taxes while holding public spending constant.

The practical implication of Lynch's argument is that the revenue loss per new business activity created will be much greater than I calculate. His proposals imply that the revenue cost per job can, under one alternative, be calculated as all state and local taxes divided by −0.20. This will yield a considerably more negative number than dividing all state and local business taxes by −0.20. Alternatively, if we want to use state and local business taxes in the numerator for this calculation, we should divide these total state and local business taxes by a tax elasticity that is less negative than −0.20.

I disagree with Lynch's argument, for several reasons. First, we are unlikely to get reasonable measures of the revenue cost of creating new business activity unless we recognize the possibility of a given tax measure proxying for other tax measures. For example, it is also true that many business location studies use narrower measures than all state and local business taxes. For instance, these studies may use only state corporate income taxes. If we simply divided state corporate income taxes by one of these studies' tax elasticity, we would get very small costs per job created. However, this is misleading: it is probably the case that states with low corporate income taxes also have low rates on other business taxes. Similarly, I would argue that it is misleading to ignore the possibility that a given percentage variation in overall state and local taxes is proxying for a given percentage variation in state and local business taxes.

Second, I don't think Wasylenko's review of the research evidence, which largely relies on studies from my previous review, really shows that the tax elasticity of business location with respect to state and local business taxes is less negative than −0.20. In the studies reviewed by Wasylenko, the most relevant studies in looking at business incentives are those that look at how births or other micro measures of individual manufacturing firms respond to business taxes. These include 19 studies, with a median elasticity of −0.20.

Third, the business location literature is relying on the observed variation in tax rates and public services. This observed variation is in a world where state and local governments are generally forbidden by state constitutions and state law from running planned or persistent operating budget deficits. Therefore, lower personal tax rates in a state do not imply a state that is deficit spending. In general, states may be able to have lower tax rates, holding public services constant, because of variation in such factors as tax bases per capita that allow a state to have lower tax rates and yet afford similar public services. If public services are not held constant, then lower tax rates may be due to greater tax bases, or may reflect a preference in that state for lower taxes and lower public services. In any event, if there is any demand-side effect of lower personal taxes, it would take place through a balanced-budget multiplier effect of lower tax rates and lower public spending, which would cause *negative* demand-side effects of lower personal taxes. However, in many cases tax rates may be lower in a state without necessarily implying lower tax collections or public spending per capita. Therefore, I believe that Lynch is incorrect to assert that the natural variation in tax rates and business activity growth across states necessarily reflects demand-side effects.

By this argument, I don't mean to imply that policymakers don't have to pay attention to demand-side effects in changing state and local tax rates. If policymakers lower one type of tax rate in a state, they will have to lower public spending on public services or lower public spending on transfer payments or raise other tax rates, in order to maintain a balanced budget.

These balanced budget shifts may have demand-side consequences. These consequences should be taken into account by policymakers. However, the natural variation in taxes across states is accompanied by other natural variations—in tax bases per capita, for example—that mean that such demand-side effects don't necessarily occur across different states because of "natural" variation in household taxes or business taxes.

If demand-side effects of state and local taxes do not occur, then the effects of state and local household taxes or business taxes on business location decisions must be due to supply-side effects—e.g., effects on the costs of various factors of production in a state. Business taxes directly affect the costs of various factors of production in a state, in particular the cost of capital. Household taxes do not directly affect business costs in a state. They may indirectly affect costs if some of these household taxes are shifted into higher costs of labor or land, or other business inputs. However, many studies of determinants of business location already control for labor costs, and some control for land costs as well. Once these other business costs are controlled, household taxes should not have an independent supply-side effect on business location decisions. As argued above, if household taxes do have some demand-side effect on business location decisions, this demand-side effect will, if anything, be positive, because of the balanced budget multiplier.

Appendix 4A

Ratio of Annual Program Effects on State Residents' Earnings to Program Costs, Each Year After Permanent Program Is Begun

Table 4A-1 presents the numbers behind Figure 4-2. These numbers are annual ratios of effects on state residents' earnings per capita to program costs, for three early childhood programs and a business incentive program. Each program is assumed to begin in 2011 and stay at the same scale until 2090.

Table 4A.1 Ratio of Annual Program Effects on State Residents' Earnings to Program Costs, Each Year after Permanent Program Is Begun

Year	Abecedarian	NFP	Business incentives	Universal pre-K
2011	0.307	0.173	1.897	0.225
2012	0.307	0.170	1.843	0.213
2013	0.308	0.168	1.787	0.201
2014	0.310	0.206	1.729	0.189
2015	0.314	0.248	1.668	0.176
2016	0.324	0.244	1.605	0.163
2017	0.341	0.245	1.561	0.161
2018	0.365	0.252	1.527	0.160
2019	0.396	0.263	1.501	0.159
2020	0.432	0.276	1.479	0.157
2021	0.472	0.290	1.594	0.155
2022	0.514	0.306	1.707	0.152
2023	0.557	0.322	1.816	0.170
2024	0.602	0.340	1.922	0.199
2025	0.648	0.359	2.025	0.240
2026	0.695	0.379	2.125	0.305
2027	0.742	0.416	2.221	0.376
2028	0.790	0.453	2.314	0.450
2029	0.839	0.500	2.403	0.529
2030	0.910	0.551	2.488	0.612
2031	0.984	0.603	2.570	0.697

Table 4A.1 (Continued)

Year	Abecedarian	NFP	Business incentives	Universal pre-K
2032	1.062	0.659	2.649	0.785
2033	1.141	0.717	2.723	0.876
2034	1.224	0.782	2.794	0.959
2035	1.305	0.850	2.861	1.048
2036	1.384	0.921	2.925	1.137
2037	1.460	0.993	2.985	1.229
2038	1.534	1.068	3.042	1.321
2039	1.607	1.145	3.095	1.420
2040	1.681	1.223	3.145	1.518
2041	1.755	1.302	3.191	1.620
2042	1.830	1.383	3.234	1.724
2043	1.903	1.464	3.275	1.829
2044	1.977	1.545	3.312	1.940
2045	2.049	1.626	3.346	2.051
2046	2.120	1.706	3.377	2.165
2047	2.191	1.785	3.405	2.278
2048	2.260	1.862	3.431	2.393
2049	2.329	1.938	3.454	2.513
2050	2.398	2.011	3.475	2.628
2051	2.466	2.083	3.493	2.746
2052	2.533	2.151	3.510	2.863
2053	2.599	2.217	3.524	2.974
2054	2.663	2.280	3.536	3.090
2055	2.725	2.339	3.547	3.202
2056	2.785	2.394	3.555	3.311
2057	2.843	2.445	3.563	3.421
2058	2.897	2.493	3.569	3.529
2059	2.949	2.537	3.574	3.634
2060	2.999	2.578	3.578	3.736
2061	3.047	2.616	3.582	3.832
2062	3.090	2.650	3.585	3.919
2063	3.132	2.681	3.587	4.008
2064	3.170	2.709	3.589	4.089
2065	3.205	2.734	3.591	4.168
2066	3.236	2.755	3.592	4.241
2067	3.263	2.774	3.593	4.306
2068	3.288	2.790	3.594	4.363
2069	3.311	2.804	3.595	4.416
2070	3.331	2.816	3.595	4.461
2071	3.347	2.827	3.596	4.503
2072	3.361	2.836	3.596	4.532
2073	3.374	2.844	3.596	4.556

Table 4A.1 (Continued)

Year	Abecedarian	NFP	Business incentives	Universal pre-K
2074	3.385	2.850	3.596	4.576
2075	3.394	2.856	3.596	4.597
2076	3.401	2.860	3.596	4.612
2077	3.406	2.863	3.596	4.626
2078	3.411	2.866	3.596	4.637
2079	3.414	2.868	3.596	4.646
2080	3.416	2.869	3.596	4.654
2081	3.416	2.870	3.596	4.659
2082	3.416	2.871	3.596	4.663
2083	3.415	2.871	3.596	4.667
2084	3.414	2.870	3.596	4.671
2085	3.412	2.870	3.596	4.673
2086	3.409	2.869	3.596	4.675
2087	3.406	2.868	3.596	4.677
2088	3.403	2.867	3.596	4.679
2089	3.399	2.865	3.596	4.681
2090	3.395	2.864	3.596	4.682

Appendix 4B

More Details on Procedures Used to Simulate State Economic Development Benefits of Early Childhood Programs

This appendix describes in more detail the procedures used to simulate the state economic development benefits of early childhood programs. More details are in Bartik (2006, 2008).

I first consider some common procedures used for analysis of all four programs. I then consider specific estimates for each program that were used to drive the simulations. The universal pre-K analysis, and the analysis for the other two early childhood programs, were done for two separate projects. However, I tried to follow similar procedures as much as possible. Of course, sometimes different procedures made sense based on differences in the programs.

COMMON PROCEDURES

Excel-Based Model

All the simulations were run on Excel. Based on program-specific evidence, impacts were calculated for a specific cohort of children, along with their parents. The U.S. totals were used to represent a blown up version of an average state. These impacts were state impacts in that they allowed for the out-migration rates that would be expected for a typical state. Impacts for subsequent cohorts were then calculated. The size of subsequent cohorts was assumed to grow with the economy-wide population growth of 0.3 percent per year. Cohort impacts were also assumed to grow with the economy-wide real wage growth rate of 1.2 percent. (More details below on the sources for these growth assumptions.) Impacts for all cohorts were summed to get total impacts per year for a permanent full-scale program.

Program costs were also calculated over time. Real program costs per participant were allowed to increase with the assumed economy-wide trend growth in real wages.

Program benefits and costs were discounted back to the first year of the permanent program to get present values. Impacts for each year were divided by simulated total employment and earnings for the United States to get percentage impacts for a typical state.

Simulated U.S. Economy

The estimates were embedded in a U.S. economy that was assumed to have long-run annual growth rates of 1.2 percent in real wages and 0.3 percent in population and employment. Real wage growth projections come from Holtz-Eakin (2005). Population growth projections come from the Board of Trustees of the Social Security System (OASDI 2005).

The economy was initialized using BEA figures for full-time equivalent wage and salary employment, and average hourly wage data from the Current Population Survey (CPS) Outgoing Rotation Group (ORG). Total U.S. earnings were calculated by multiplying full-time equivalent employment by the average hourly wage by 2,000 annual work hours. The CPS wage number was used because the impact numbers rely on CPS wages.

The initial FTE wage and salary employment data come from 2006. The CPS wage data come from 2004. However, both employment and wage estimates were adjusted to 2009 using the assumed economy-wide real wage growth and employment growth estimates. Earnings and wages were calculated in 2007 prices.

Program costs and enrollment were also adjusted to 2009 using the same assumptions about growth in real wages and employment and population. For this book, which updated the starting point to 2011, no further adjustments were made. The implicit assumption is that program size, costs, and benefits would all vary proportionately with the economy.

The aggregate U.S. employment concept used in the current book was adjusted so that it is consistent across the analysis of pre-K and business incentives, versus the analysis of the Abecedarian program and NFP. The employment concept used initially for pre-K and business incentives (Bartik 2006) implied a somewhat too low ratio of employment to the number of four-year-olds, compared to an FTE concept. The employment concept used initially for the Abecedarian program and NFP was total wage and salary employment, which is somewhat higher than FTE employment (Bartik 2008). Therefore, the percentage employment and earnings impact numbers used in this book were adjusted somewhat compared to those in Bartik (2006) and Bartik (2008). The percentage impacts in Bartik (2006) were adjusted downwards slightly. The percentage impacts in Bartik (2008) were adjusted upwards slightly.[1] These adjustments make the percentage impact figures for all these programs more comparable. These adjustments do not affect the estimates of the ratios of state economic development benefits to program costs, which are independent of the assumed scale of the program or the economy. In addition, the adjusted percentage impact figures do not change any qualitative conclusions from Bartik (2006) and Bartik (2008).

Balanced Budget Multiplier Effects

The balanced budget multiplier (BBM) for a typical state for each early childhood program was calibrated to estimates from the REMI model. The calibration was derived from estimates of the BBM for pre-K education. My colleague George Erickcek and I simulated the job creation effects of the spending and taxes required by a universal pre-K 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. REMI models are documented and explained extensively in scholarly work (Greenwood et al. 1991; Treyz 1993; Treyz et al. 1993; Treyz, Rickman, and Shao 1992).

All other balanced budget multiplier (BBM) effects were calibrated to these Michigan pre-K estimates. BBM estimates were based only on the size of the program's spending and taxes relative to the size of the state's economy. This procedure implicitly assumes that a "typical state" will have BBM effects similar to Michigan. The procedure also assumes that BBM effects per dollar of spending and taxes are the same for the four early childhood programs.

The BBM effects are for a permanent program in an economy with population and employment growth of 0.3 percent and real wage growth of 1.2 percent. The early childhood program's number of child participants will grow by 0.3 percent annually. The early childhood program's real spending will grow by about 1.5 percent each year, due to 0.3 percent participant growth and growth in program costs per participant of 1.2 percent. The total number of jobs in the economy due to BBM effects will be higher each year by 0.3 percent. However, after the first year, the only job growth induced by BBM effects is this 0.3 percent increase.

The resulting impact on the employment rates and wage rates of state residents is similar to that of an economic development program that initially permanently creates some number of jobs, but each year after that only creates 0.3 percent of that initial job spurt. The initial job and earnings impacts are similar to those of a business incentive program of the same costs. However, the BBM impact in subsequent years is only a fraction of the effects of a permanent business incentive program. BBM spending must be permanently maintained to keep the same jobs. In contrast, business incentives induce business investment that maintains itself in the private marketplace.

Child Care Effects

The universal pre-K program and the Abecedarian program will increase parental labor supply by providing free child care for varying periods of time. (NFP does not provide significant free child care. NFP may affect mothers' labor supply in other ways.) My estimates of the labor supply effects of these two programs rely on the child care research literature. This literature has examined how maternal labor supply responds to the price of child care. These estimates suggest that a reduction in child care costs of *x* percent will increase the employment-to-population ratio of mothers by one-fifth of *x* percent, in percentage points. (By "percentage points," I mean that an increase in the employment-to-population ratio from 50 percent to 60 percent is 10 percentage points, rather than 20 percent of the base.) The impact of universal pre-K and the Abecedarian program on mothers' employment rate, in percentage points, is assumed to be equal to one-fifth of the percentage reduction in child care costs made by each.

Blau (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 that the average employment rate in Blau and Hagy's sample appears to be 56.4 percent, the elasticity in percentage points would be (−0.11). However, the Blau estimates are towards the low 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. This would imply an elasticity in percentage points of close to (−0.20). In addition, the careful work by Anderson and Levine (2000) suggests that the elasticity with respect to child care price is higher for the poor and near-poor, and higher for women with children under six than for women with older children. Anderson and Levine, for example, find an elasticity in percentage points of (−0.28) for all women with children under age six (Table 10.8). They also find an elasticity in percentage points of (−0.15) for poor and near-poor women with children under age 13 (Table 10.9). An elasticity around (−0.20) in percentage points seems reasonable for the mothers whose children would be involved with pre-K or the Abecedarian program.

Educational Attainment Effects for Former Child Participants and Mothers

Part of the economic development benefits of these programs occur through increasing the educational attainment of former child participants. For the Abecedarian program and the NFP program, there also are estimated program effects on the educational attainment of mothers of child participants. I measure the employment rate and wage rate effects of increased education by using data from the Current Population Survey on employment rates and wage rates of different education groups.

The Abecedarian and NFP analysis was done at a later time than the pre-K analysis. Therefore, it uses a somewhat more complex estimation procedure. In addition, the Abecedarian and NFP programs target disadvantaged groups. Universal pre-K targets a broader group. This difference also requires some differences in the data used. I first describe the CPS procedures used for the pre-K analysis. I then describe the CPS procedures for the Abecedarian and NFP analyses. I finish with some discussion of common procedures for processing the CPS raw data and calculating wages.

The pre-K analysis used Current Population Survey, Outgoing Rotation Group (CPS-ORG) data for all persons. The data come from 2004. The data used were on employment rates and hourly wage for various education groups for each age from ages 16 to 79. These data were used to calculate the employment and wage impact of pre-K stemming from its effects on educational attainment. Additional employment rate impacts for the program group were also incorporated to reflect evidence that pre-K has effects on employment beyond its effects on educational attainment. (See Chapter 4 text discussion of this topic. See also program-specific discussion for pre-K below in this Appendix for more details on how these additional employment rate impacts were calculated.) Earnings impacts for each age were calculated by multiplying the employment rates and wage rates for the control group by 2,000 work hours per year, and doing the same for the program group. This procedure may overstate the earnings impact to the extent to which average work hours may be less than 2,000 work hours per year. On the other hand, this procedure may understate the earnings impact because the procedure omits impacts of pre-K on work hours. Such impacts on work hours could be due to changes in educational attainment, as well as to similar forces to those that cause extra increases in employment rates.

The Abecedarian program and NFP target disadvantaged groups, in many cases groups with a high percentage of African Americans. Therefore, the CPS data I use for these programs all is for African Americans. I enlarge the estimating sample for African Americans by pooling data from the Current Population Survey Outgoing Rotation Group from 2002 to 2006. For these three programs' estimates for former child participants, I calculate employment rates, wage rates, and average weekly hours for black males and black females for each age from age 18 to age 79. For mothers of participants, I calculate employment rates, wage rates, and average weekly hours for black unmarried females for each age from age 18 to age 79. Various educational breakdowns of these groups are used, depending upon what data on educational attainment are available for a particular program.

For this CPS-ORG sample of African Americans, there were some jumps in employment rates and wage rates by age for some of these groups. Therefore, I did some additional analysis to smooth the changes in these variables with age. To accomplish this smoothing, I did a set of regressions in which the initial estimates for each variable and group were regressed on a quartic in age. The fitted values from these regressions, which change smoothly with age, are then used to estimate how changes in educational attainment will change employment rates, wage rates, and weekly work hours at different ages.

For the Abecedarian and NFP programs, employment rate, wage rate, and weekly work hour impacts for each program are initially calculated by estimates of how each program affected educational attainment. In addition, as outlined in the program-specific discussion below, I also allowed for some program-specific effects on employment rates and wage rates that go beyond effects due to educational attainment. Program impacts on earnings for each age are calculated by multiplying, for the control group, employment rates times hourly wages times weekly work hours times 52, and similarly for the program group.

For all four early childhood programs, the initial measured CPS-ORG employment rates, wage rates, and average weekly hours are only measured using non-imputed data. For workers paid by the hour, I use hourly wages. For other workers, I measure wages as usual weekly earnings divided by usual weekly hours, where available. For workers whose usual weekly hours vary, I use actual weekly hours the previous week. For workers whose usual weekly earnings are top-coded, earnings are estimated by multiplying the top code by 1.4. Wage observations are treated as outliers and dropped if the real wage is less than $2 per hour or more than $200 per hour in 2004 dollars, deflated using the Consumer Price Index research series produced by the U.S. Bureau of Labor Statistics. (As the reader might suspect, I have processed such data using identical procedures for previous projects.) For the wage and earnings impact analysis, all wages are adjusted to 2007 dollars. Impacts were also adjusted to a 2009 start date based on the economy-wide assumed wage and population growth rates. For this book, the percentage impacts for a 2011 start date were assumed to be the same as for a 2009 start date.

Mortality Projections

The simulations adjusted for expected mortality at different ages. Simulations for all four of the programs used the same method. The simulations sought to measure how many former child participants or their parents would survive to various ages. Survival rates are measured using 2003 data from the National Center for Health Statistics (2005) on the expected survival rates of black men and black women from birth to age 79. (Excel spreadsheet versions of these data are available as Tables 8 and 9 at ftp://ftp.cdc.gov/pub/Health_Statistics/NCHS/Publications/NVSR/54_14/.) For the Abecedarian program and NFP, survival rates for black men and women are used because these programs serve a disadvantaged clientele that includes many African Americans. Universal pre-K serves a broader clientele. However, it tends to serve families with lower educational attainment. Survival rates are not readily available by educational attainment. My belief is that survival tables based on African Americans are a better match to survival rates of the universal pre-K clientele than survival rates based on all persons. This assumption is conservative, in that it tends to reduce the estimated effects of pre-K.

State Out-Migration

Measures of state economic development benefits of early childhood programs only include estimated increases in earnings for persons who remain in the state. Therefore, I had to develop procedures for estimating what proportion of former child participants and mothers will remain in the state at various ages.

For the universal pre-K study, I estimated state out-migration using a combination of data from the Panel Survey of Income Dynamics (PSID) and the Public Use Microdata Samples (PUMS) of the 2000 Census. For each year of age from 5 through 39 for the PSID, I calculated the proportion of various education groups still living in the same state they lived in at age four. For each age, I then calculated a weighted average of these proportions based on the education distribution of pre-K participants. For each age from ages 16 through 79 in the PUMS, I calculated the proportion of various education groups still living in their state of birth. I also calculated a weighted average for each age of these proportions, using the proportion of pre-K participants in each education group. Because of the larger sample size, the PUMS data series was much smoother. The PUMS data series also extends beyond age 39 to the entire working career. For each year from age 16 to 39, I calculated the ratio of the PSID "staying proportion" to the PUMS "staying proportion." These ratios tended to be similar for different ages. I calculated an adjusted PUMS staying proportion for each age by multiplying the observed PUMS staying proportion by the average PSID-to-PUMS ratio from ages 16 to 39. (See Bartik 2006, Table 12, for the PSID and PUMS numbers, and the ratios and adjusted PUMS numbers.)

For the other two programs, I used a simpler procedure. I found in the universal pre-K analysis that adjusted PUMS numbers using the PSID were quite close to a much simpler calculation: dividing the proportion living in the birth state at age *x* by the proportion living in the birth state at age four. Therefore, for these other programs, I projected the proportion staying by using PUMS data for proportion staying in their birth state at age *x* compared to proportion in their birth state as of some base year. For the mothers of program participants, I estimate the proportion of mothers remaining as of age *x* in the state in which the program was delivered by the following ratio from the PUMS: the ratio of the proportion of persons living in their birth state at age *x* to the proportion of persons living in their birth state at the mean age of program mothers at program onset. For the former child participants, I estimate the proportion remaining as of age *x* in the state by the following ratio: the ratio of the proportion living in their birth state as of age *x* to the proportion living in their birth state when they entered the program. Date of program entry is assumed to be age 0 for Abecedarian, and after birth for NFP.

For these two programs, the PUMS proportions living in the birth state are measured using data on black men and women. The rationale is that these three programs serve disadvantaged families, including many African American families. The staying proportion is likely to be better matched using African American data than using data for all persons. For the mothers of participants, the PUMS proportions living in their birth state is measured using data for black unmarried women. A high percentage of families served by these three programs are single parent families.

Peer Effects

In addition to the direct effects of early childhood programs 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. Other spillovers are effects on the overall productivity of the labor market that occur during participants' working lives. I consider the spillover effects in the labor market below. 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 nonparticipants. In addition, the interactions among early childhood participants in school may have peer effects on each other that may further improve participants' educational achievement, behavior, and motivation. Such peer effects may further improve educational attainment. These peer effects may also have effects on employability that go beyond what would be predicted 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 allow teachers to increase the level at which instruction occurs. (Mechanisms for peer effects are further discussed by Hoxby [2000] and Hanushek et al. [2003], and these discussions have influenced my ideas.)

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. This means that, holding constant the characteristics of an individual student, an increase of 1 percent in prior average achievement levels in a class is associated with an individual student having test score gains of 0.15 of 1 percent 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 percent, the aggregate gain in test scores for his or her class will be 0.15 percent greater during the next school year. An individual student with an achievement level of 1 percent greater increases average prior student achievement levels by 1 percent/*N*, where *N* is class size. Average student achievement gains during the next school year will then go up by 0.15 percent/*N* during the next year. Aggregating over all *N* students in the class, total test score gains go up by the *N* students, the average gain of 0.15 percent/*N*, or by 0.15 percent. Whatever beneficial effects higher average test scores have on later employment and earnings will increase because of 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. This assumption is 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. The evidence of Hanushek et al. and Hoxby is 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 four 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 also determine 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 early childhood programs on later employment and earnings is at least 0.6.

But not all early childhood program participants survive to enter elementary school, and not all early childhood program participants stay in the same state. Therefore, I adjust the 0.6 peer effects by a factor of 0.904 to reflect both mortality and out-migration. This yields a final peer effect of 0.54.[2] The employment and earnings effects of early childhood programs that occur directly for participants are then multiplied by 0.54 to yield estimated peer effects. Total effects of early childhood programs on the employment and earnings of both participants and peers are then 1.54 times the originally calculated figures.

Displacement Effects

As mentioned in Chapter 2, there is good reason to think that an increase in the labor supply of former child participants in these programs, and their parents, will result in some displacement effects within state economies. As outlined in Chapter 2 and Appendix 2B, I assume displacement effects of one-third in state economies for both employment and earnings. Therefore, the estimated employment and earnings effects of early childhood programs on former child participants, their peers, and their parents, are multiplied by two-thirds to get net effects on state residents' per capita employment and earnings.

Spillover Effects of Education on Labor Productivity

Early childhood programs may have spillover effects that occur in the labor market. There may be effects on productivity and wages in the labor market beyond the increase in productivity and wages that occur for the individual. In other words, the productivity, employability, and wage rates of an individual may depend not only on his or her 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. As a result, employers may find it more feasible to use more advanced technologies, more capital intensive production techniques, and other forms of workplace organization that have higher productivity levels. In addition, productivity growth rates may depend on average educational attainment. Workers may get ideas from each other about how to improve technologies and production techniques more rapidly. Productivity growth rates may also increase with average education if this allows employers to more rapidly introduce new capital, technologies, and workplace organization systems.[3]

Assume that higher productivity is to some extent reflected in higher wages. If so, an individual's wages, holding the individual's characteristics constant, would be positively affected by the average educational level of the local workforce. If local wages do not instantly and fully adjust upwards to reflect higher overall productivity, then an increase in average educational levels would 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. 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. 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 available empirical evidence seems to support some "extra" labor productivity effects of education, beyond the effects on the individual. Average educational levels in a metropolitan area appear 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 (Moretti 2003, 2004). Higher average education levels also lead to higher metropolitan growth (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 (Acemoglu and Angrist 2000; Glaeser and Saiz 2003; Moretti 2003, 2004). Whether a higher percentage of college graduates has more effects through affecting productivity levels or productivity growth is harder to sort out. For this book, 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. Assuming effects on productivity growth rates could result in huge measured earnings effects as we follow a state economy into the long-term future. There is no definitive evidence of such productivity growth effects.

Simulating these labor productivity spillovers requires thinking through the mechanisms by which average college graduation rates affect a state's economy. Productivity increases have 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. This labor demand shock is similar to what is induced by business incentives. As individuals migrate into the state, attracted by the greater employment opportunities, the initial effects on employment rates are moderated somewhat. 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 permanently benefit. They will be able to use their greater labor market experience, better reputation with employers, and greater motivation to keep their new jobs or get better jobs. As these individuals age and die or move out of the state, 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.[4]

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 the percentage of college graduates in a local area is causally associated with nominal wages that are 0.6 to 1.2 percent 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 percent to 3 percent.[5] On the other hand, Glaeser and Saiz (2003) directly estimate that a 1 percent increase in the percentage of college graduates increases the 10-year population growth rate by perhaps 0.5 percent. 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 1993a). In addition, if there is some lagged adjustment of employment, the 10-year effect on growth will be below the long-run effect. Under plausible assumptions about adjustment, Glaeser and Saiz's results imply that 1-point increase in the local area's percentage of college graduates will increase the local employment level by 1 percent in the long run.[6]

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 percent. 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 equilibrium level by 8.9 percent per year. 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. 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 business incentives.

Part of this calculation involves simulating how early childhood programs will affect the percentage of college graduates in a state's economy. This simulation starts with program-specific evidence on how the program affects college graduation rates for former childhood participants and parents. I then simulate how effects for a given cohort would be expected to affect the college graduation proportion as successive cohorts that have participated in early childhood programs enter the labor market. This calculation of course allows for peer effects, outmigration, mortality rates, and population growth.

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 out-migration of early childhood participants. Based on migration statistics, of the 21-year-old college graduates in a state, we would expect 83 percent to still be in the state five years later, declining to 74 percent still in the state 24 years later, and averaging 78 percent over all the different time periods considered in the Bound et al. estimates.[7] 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 calculations always show quite small “labor productivity spillovers” of early childhood programs. For example, for universal pre-K, the present value of the real earnings increase due to these “labor productivity spillovers” is only four cents per dollar of program costs. In the tables and figures of this book, these labor productivity spillovers are combined with effects on former child participants. Effects are small in part because effects on overall college graduation end up being relatively small. Universal pre-K is projected to have relatively modest effects on college graduation. The other two programs have important effects on participants, but are targeted programs, which limits effects on the overall college graduation percentage. In addition, effects on college graduation are quite delayed. Finally, the model I use of “labor productivity spillovers” has gradual responses of wages and employment rates to the more productive state environment resulting from more college graduates.

PROGRAM-SPECIFIC PROCEDURES

This section of the appendix goes through more specific estimates and assumptions used to generate simulation results for each of the three early childhood programs.

Estimated program effects are based on the best research evidence available. Point estimates are used even when the estimated effect is not statistically significant. I note below when the estimates are based on empirical results that are not statistically significant at conventional levels. I use the terminology "statistically significant" for estimates that are significant at the 95 percent confidence level for a two-tailed test (e.g., probability of result of this magnitude occurring by chance in this sample is less than 5 percent). I use the term "marginally statistically significant" for estimates that are not significant at the 95 percent level for a two-tailed test, but are statistically significant at the 90 percent level (e.g., probability of result of this magnitude occurring by chance in this sample is less than 10 percent but more than 5 percent). I use the term "close to being statistically significant" for estimates that have a probability of occurring by chance of more than 10 percent but less than 15 percent.

For each program, I first consider estimates and assumptions used to generate effects for mothers of program participants, and then consider former child participants. The estimates and assumptions are used to calculate gross effects of the program. As outlined above, these gross effects were then adjusted for mortality, out-migration from the state, K–12 peer effects (for child participants), displacement effects, and spillover labor productivity effects from more college graduates. The discussion below also clarifies how gross effects on state college graduation rates were estimated.

Universal Pre-K

Effects on mothers

The Chapter 4 text describes how the labor supply effects on mothers of universal pre-K were estimated. I assume these additional women workers are paid a wage equal to the 40th percentile of women's wages ($11.90 in 2007 dollars). I should note that unlike I did with some of the other early childhood programs, I did not estimate subsequent effects on wage rates and employment rates of the extra labor market experience provided by this free child care. The percentage reduction in child care costs and the effects on labor supply are so small that these subsequent effects would be quite small. In addition, there is no independent evidence that universal pre-K increases the long-run employment and earnings of mothers of child participants.

Effects on former child participants

As mentioned in the chapter text, I assumed, following Karoly and Bigelow (2005), that the effects of a universal pre-K program on former child participants would be 23 percent of the effects estimated for participants in the Chicago Child-Parent Center program. This 23 percent figure makes assumptions about how much lower pre-K's benefits will be for children from different income groups. These distributional assumptions are further explored in this book in Chapter 8.

How do I estimate the long-run employment and earnings effects of a CPC-style pre-K program on its participants? I am seeking to estimate effects for participants similar to CPC participants. These effects will then be multiplied by 23 percent to get effects per participant for a universal pre-K program. The CPC's pre-K program is estimated to have increased high school completion at age 20 or 21 by 11 percentage points, an effect that is statistically significant (probability of 0.01, *t*-statistic not given) (Reynolds et al. 2002, Table 4). However, I wanted to estimate longer-run effects of pre-K than is allowed by the CPC. I also wanted to see if there might be employment rate effects of a CPC-style pre-K program that extend beyond those predicted by educational attainment. Such effects are not observed in the CPC because it does not report results for employment rates.

To do these adjustments, I relied on data from the Perry Preschool program. Perry preschool increased high school graduation rates at age 19 by 22 percentage points, an effect that is at least marginally statistically significant. (This effect is reported as statistically significant at the 5 percent level for a one-tailed test, but not at a 1 percent level, which means that this effect is at least "marginally statistically significant" in my terminology, and possibly statistically significant. Exact probability numbers are not reported.) (Schweinhart et al. 2005, Table 3.1). Thus the Perry Preschool program's educational effects, as of just after a normal high school graduation age, are about twice those of the CPC program.

To get longer-term results for CPC-style participants in a CPC-style program, I assumed that the program's long-term effects on educational attainment and employment rates will be one-half of those of the Perry Preschool program. For educational attainment, at age 27, Perry Preschool's results showed the following: control group, 47 percent no high school degree, 51 percent high school degree but no higher degree, 2 percent associate degree or higher; program group, 28 percent no high school degree, 69 percent high school degree but no higher degree, 3 percent associate degree or higher. These educational attainment differences are reported to be statistically significant (probability of one-tailed test is less than 0.01, exact probability not reported) (Schweinhart et al. 2005, Table 3.1). At age 40, Perry Preschool's results showed the following: control group, 40 percent no high school degree, 55 percent high school degree but no higher degree, 5 percent associate degree or higher; program group, 23 percent no high school degree, 68 percent high school degree but no higher degree, 9 percent associate degree or higher. These educational results are reported to be at least marginally statistically significant. (These results are reported to be statistically significant at a one-tailed 5 percent level but not a one-tailed 1 percent level. In terms of my terminology, these results are clearly marginally statistically significant and may even be statistically significant. Exact probability levels are not reported.) (Schweinhart et al. 2005, Table 3.1.) In addition, it should be noted that none of the college graduation results by themselves in the Perry Preschool program are statistically significant, although the exact probabilities are not reported.

To use these results, I assumed that the control group in a CPC-style pre-K program would have had educational attainment levels similar to what was observed in Perry Preschool at ages 27 and 40. Educational attainment levels were assumed to change smoothly between ages 21 and 27, and ages 27 and 40. The CPC-style program group differentials in extra high school graduation and college graduation at ages 27 and 40 were assumed to be half of those observed in the Perry Preschool program. The CPC-style program group educational attainment levels were also assumed to change smoothly between age 21 and age 27, and then between age 27 and age 40. Beyond age 40, the education levels were frozen for both CPC-style program and control groups. The actual assumptions for CPC-style educational attainment at different ages are shown in Bartik (2006, Table 10).

These projected educational attainment differentials were used to initially predict the employment rates and wage rate differentials due to a CPC style program at different ages. In addition, these educational attainment differentials were used to predict the number of college graduates at different ages. For this prediction, I assumed all of the associate's degree and higher differential was a bachelor's degree or higher differential. As noted earlier in this appendix, these estimated effects were multiplied by 0.23 to reflect estimated effects of a universal program per participant relative to the CPC, and then further adjusted for out-migration and the displacement effects on state college graduation rates noted by Bound et al. (2004).

However, these initial employment rate differentials were then adjusted to reflect the possibility of employment rate effects beyond those predicted by educational attainment. Perry Preschool estimated substantively large employment rate differentials between the program and control group at ages 19, 27, and 40. At age 19, while only 32 percent of the control group was employed, 50 percent of the program group was employed, a statistically significant difference (Barnett 1993, Table 2. Exact probability is not reported, but I recalculated it without controls as 0.04). At age 27, 56 percent of the control group was employed, versus 69 percent of the program group (Schweinhart et al. 2005, Table 4.1). This difference appears to be at least marginally statistically significant. At age 40, 62 percent of the control group was employed, versus 76 percent of the treatment group (Schweinhart et al. 2005, Table 4.1). This difference also appears to be at least marginally statistically significant.

In Bartik (2006, Table 11), I showed that these employment rate differences are far in excess of what would be predicted by educational attainment differentials between the control group and the program group. The higher educational attainment of the program group would be expected to increase its employment rate by two or three percentage points. But the observed employment rate differentials are 10 to 15 percentage points greater than that differential.

I assumed that a CPC-style pre-K program also will produce employment rate differentials higher than predicted by educational attainment. But I assume they will be half the size of those predicted by Perry. I took the Perry differentials as a percentage of the control group mean at ages 19, 27, and 40. I assumed that a CPC-style program would yield an extra employment rate effect of half as much as a percentage of the control group mean at those ages. These effects were allowed to smoothly change between those ages. The differentials were frozen, as a percentage of the control group mean, beyond age 40.

All of these effects are for participants in a CPC-style program who are similar to CPC participants. That is, these participants are economically disadvantaged, and few in the control group participated in any pre-K program. For universal pre-K, all the estimated employment and earnings effects per average participant were assumed to be 23 percent of those estimated for a person similar to a CPC participant.

Abecedarian Program

Effects on mothers

Based on research on the effects of child care prices on mothers' labor supply (see above), I initially assume free child care will increase employment rates by 0.20 during the five-year life of the program. In addition, I project changes in employment rates and earnings rates based on Abecedarian program information that postsecondary education of mothers increases because of the program. The eventual increase, as of 15 years after the program began, is from 34 percent attending postsecondary among the control group to 52 percent for the treatment group (Ramey et al. 2000). (This difference is almost but not quite statistically significant at normal significance levels, with a *t*-statistic for the difference of 1.66, and a probability of 0.102.) As outlined in the general procedures, I use CPS-ORG data on black unmarried mothers to do initial projections of the employment and earnings impact on mothers of the Abecedarian program. For these simulations, I assume all the mothers have the program mean initial age of

20.

However, this initial increase in employment experience due to the free child care should increase subsequent employment and wage rates of the mothers. To measure this increment to employment rates, I use information from the program research (Ramey et al. 2000) that the employment rate of moms after 15 years increased by 10 points. This increase is 6.4 points greater than predicted by their increase in educational attainment. (This difference of 10 points is statistically insignificant at conventional levels, with a *t*-statistic of 1.11 and a probability of 0.274.) I assume this extra employment rate increment applies throughout the work career of these women. This is not an unreasonable increase in work given that the program's effects on child care increased their initial employment rate by 20 points for five consecutive years. Previous research suggests substantial persistent effects of employment experience on subsequent work, particularly on low-income workers (Bartik 2001).

To measure the increment in wage rates, I see what additional increase in wage rates would be needed to match the earnings increases for mothers 21 years after program start that are reported in Masse (2002, p. 208; the reported probability level is 0.06). It turns out that these earnings increases can be matched if we assume that the extra work experience, beyond what is predicted by education, has an effect on the log of wage of 0.0274 per year of work experience for the first 10 years of work experience. Because these women obtain a little more than a year of additional work experience from age 20 to age 29, the natural log of their wage eventually is assumed to increase by 0.0343 beyond what would be predicted based on their increase in post-secondary education alone. This return to work experience is not unusually high. The research of Gladden and Taber (2000) suggests a return to actual work experience of black women during their early careers of an increase of 0.0535 in the ln(wage) per year of work experience.

The number of mothers affected by this intervention will be less than the number of child participants, as a full-fledged Abecedarian program will have multiple children ages 0–4 per mother. Calculations I made from the CPS-ORG suggest that among unmarried black mothers with any child less than age five, the mean number of children less than age five is 1.27.

The effects of the program on mothers graduating from college are estimated by the following calculation. Based on my tabulations of CPS-ORG data on black unmarried women ages 25 and over, I assume that 36.2 percent of the additional women who get some postsecondary education due to the Abecedarian program will eventually get a BA.

Effects on former child participants

I seek to model effects on the employment rates and earnings of former child participants in the Abecedarian program when they are ages 19 to 79 (labor supply effects prior to age 19 or after age 79 are ignored). I use both estimated effects of this program on educational attainment and directly measured effects on employment rates.

I use data from Masse's (2002) dissertation to turn observed Abecedarian program effects on education as of age 21 to effects on the final educational attainment of males and females. My chi-squared tests using the Masse data show that the estimated differences between the educational distribution of the program versus control group of women have a probability of 0.103. This is almost but not quite statistically significantly different at normal levels of statistical significance. Chi-squared tests show that the differences between the educational distribution of program versus control group men have a probability of 0.208. This is not statistically significant at normal levels of statistical significance. On the other hand, some of the educational attainment distributions do differ significantly between the program and control group. For example, the proportion enrolled in BA programs at age 21 is statistically significantly higher for women in the program group than in the control group, with a *t*-statistic of 2.21 and a probability of 0.034. The proportion of men enrolled in BA programs at age 21 is marginally statistically significantly higher for men in the program than men in the control group, with a *t*-statistic of 1.92 and a probability of 0.061.

I initially assume that the program's effects on educational attainment will be translated into effects on employment rates and earnings at each age by using tabulations from the CPS-ORG on black men and women, as outlined earlier in this appendix. For ages 19 through 22, I only use the Abecedarian program's effects on high school dropouts versus high school graduates or higher to infer initial effects on employment rates, wages, and weekly hours. For ages 23 through 79, I define educational attainment based on the four mutually exclusive and exhaustive categories of 1) high school dropout, 2) high school graduate but no higher degree, 3) associate degree but no higher degree, and 4) bachelor's degree or higher degree.

However, based on Campbell et al.'s (2002) research on age 21 effects of the Abecedarian program, it appears that the program's effects on employment rates of participants at age 21 are about 12 rate points higher than would be predicted based on the program's effects on educational attainment. (The observed employment rate differences at age 31 have a *t*-statistic of 1.46, which has a probability of 0.147. This is close to but not quite statistically significant at normal significance levels. However, the proportion of program individuals employed in more skilled jobs, out of the total program sample, is significantly greater than the proportion of individuals in the control group holding skilled jobs, with a *t*-statistic of 2.53 and a probability of 0.0126.) This result is consistent with previous research on the Perry Preschool program. As discussed previously in this appendix, Perry Preschool had much greater effects on employment rates than would be predicted by only looking at effects on educational attainment. Based on the Perry Preschool results, I assume that the Abecedarian program's "extra employment rate effect" of 12 points diminishes to 6 points by age 27. After age 27, the extra employment rate effect is assumed to be the same percentage of the control group mean employment rate as it is at age 27.

Nurse Family Partnership

Effects on mothers

The effects of the NFP program have been studied in three different experimental studies of the program, first in Elmira, then in Memphis and Denver. For effects on employment and earnings of mothers, I largely rely on the results from Memphis and Denver. This is important as the effects on mothers' employment appear to be larger at the Elmira site than at the Memphis and Denver sites. Where the Memphis and Denver results overlap, average effects across these two sites are used. (The Elmira results that are most relevant to this paper are in Olds et al. 1997, 1998. The Memphis results that are most relevant to this paper are in Kitzman et al. (2000) and Olds et al. (2004a). The Denver results that are most relevant to this paper are in Olds et al. 2004b.)

Why do I rely on the Memphis and Denver results rather than the Elmira results? The Elmira program took place prior to welfare reform in a state with relatively liberal welfare laws. It is thought that some of the effects of the NFP program take place because the program reduces subsequent pregnancies. This in turn may affect welfare usage and employment incentives. Therefore, the effects of the program on mothers' employment may differ greatly pre– and post–welfare reform. Post-welfare results for mothers seem more applicable to the effects of an NFP program today.

Estimated effects per mother are based both on effects of the program on educational attainment, and direct measures of effects on mothers' employment rates. Based on Memphis and Denver results, on average the NFP program has slight effects on educational attainment, increasing the high school graduation rate by 3.25 points, from 71 percent to 74.25 percent. (Neither estimated change in the graduation rate is close to being statistically significant. The Memphis results, in Olds et al. 2004a, have a probability of 0.54. The Denver results, in Olds et al. 2004b, have a probability of 0.35.) These effects on educational attainment, along with data from the CPS-ORG, are used to make an initial estimate of the effects of the NFP program on the employment rate, wage rate and weekly hours of NFP mothers for all ages, from age 19 to age 79. (I assume that all NFP mothers are age 19. This is the average age of NFP mothers at program onset across the three study sites.)

In addition, the information from these three studies suggests that the NFP program may have some extra effects on employment rates of NFP mothers above and beyond the effects predicted by the program's small effects on educational attainment. Memphis (Kitzman et al. 2000) and Denver (Olds et al. 2004a) results suggest an increase of one month in employment during the period from 24 months to 48 months after the child's birth. (These increments in employment are both statistically insignificant. The Denver employment differences between program and control groups have a probability of 0.22. The Memphis employment differences have a probability of 0.24.) The Elmira results (Olds et al. 1997) suggest an additional 16 months of employment during the period from birth up to the child's fifteenth birthday. The Memphis data (Olds et al. 2004a) suggest no such long-run effect. (For the Elmira results, I focus on the results for the low-SES unmarried sample. This sample is similar in its composition to program participants at the Memphis and Denver sites, and is similar to the group targeted in the full-scale program considered in this book. The reported confidence interval suggests a probability of 0.13, which is close to being marginally statistically significant.)

Based on the Memphis and Denver results, I assume that the mother's employment rate increases by 1/24 = 0.043 when the child is ages two and three. At age four and beyond, I assume no subsequent boost to employment beyond what would be predicted based on the change in education. This discounts the Elmira results, because they are contradicted by the Memphis results. I allow for the extra employment experience at ages two and three to increase the mother's subsequent wages. I assume that an extra year of actual work experience increases the natural log of subsequent wages by 0.0274, which is what was derived above for the mothers in the Abecedarian program. This appears to be a conservative estimate of likely returns to actual experience.

Based on the proportion of black women ages 25 and over who have a high school degree, versus the proportion who have a BA degree, I assume that about one-fourth of black women who obtain a high school degree will obtain a BA degree (*Digest of Education Statistics* 2006). Therefore, about one-fourth of the extra 3.25 percent of NFP mothers who obtain a high school degree because of the NFP program are assumed to obtain a BA degree.

Effects on former child participants

Studies of the NFP program have had little data on the later employment and earnings of former NFP children. Two types of data allow some projection of future effects on employment and earnings of former child participants. The first type of data is estimates of how the NFP Memphis program affected reading and math scores at age six. The second type of data is estimates from the NFP Elmira program on how the NFP program affected children's arrests and other criminal activity through age 15.

The estimated effects of the NFP Memphis program on age six reading and math scores are positive but statistically insignificant. (The study by the Washington State Institute for Public Policy of NFP cited significant effects of NFP on test scores [Aos et al. 2004]. But these significant test score results are for a mental processing composite test and for a receptive vocabulary test. Unlike reading and math test scores, these other types of tests have not been directly linked to adult employment and earnings.) A study by Currie and Thomas (1999), using British data, links reading and test scores at age seven to male and female employment rates and wage rates at ages 23 and 33. (Krueger [2003] provides helpful advice on interpreting the Currie and Thomas results when measuring effects of change in test scores in effect size units.) Combining the NFP Memphis results with the Currie and Thomas study allows us to estimate the effects of the NFP program on employment rates and wage rates when former child participants are ages 23 and 33. The employment rate effects and wage rate effects in between ages 23 and 33 are interpolated from these results. Effects before age 23 are assumed to be zero. Effects from ages 34 to 79 for employment rates are assumed to be the same percentage of the control group mean that they are at age 33.

I also add in projections of how the program's estimated effects on arrests and other criminal behavior will affect future employment rates and earnings. The NFP Elmira program appears to have reduced the proportion of former NFP child participants being arrested from 0.45 to 0.20, a reduction of 55.6 percent. (Olds et al. 1998, Table 3, p. 1242. These effects are significant at a probability level of 0.03. These estimated effects are for the low-socioeconomic-status unmarried NFP families in Elmira.)

I use data from Raphael and Stoll (2007) to estimate the proportion of the disadvantaged males and females targeted by the NFP program that will be in jail or prison at different ages. (I use their Table 7 figures on black males. The implicit assumption is that the disadvantaged group's experience with jail and prison will more closely match black males than it will other racial groups. I interpolate and extrapolate using the data they give on imprisonment for different age ranges. I use their information in Figures 29 and 30 to infer imprisonment rates for black females.) I use information from Bonczar (2003) on the cumulative proportion of the black male or black female population that has been incarcerated at various ages as an estimate of the cumulative incarceration experience rates of the NFP-targeted groups. I assume that the results through age 15 of a reduction in arrests of 55.6 percent will persist into adulthood, but at a reduced rate of a reduction of half as much in percentage terms. Therefore, I assume that imprisonment at any point in time, and cumulative incarceration experience, is reduced because of the NFP program by 27.8 percent (= half of 55.6 percent).

The NFP program's assumed reduction in imprisonment at a particular age will increase employment. I calculate this employment impact by the proportion of individuals who are not in prison because of the NFP intervention at that age, times the normal employment rate for blacks for that age and gender. The NFP program's assumed reduction in cumulative incarceration experience will increase the employment rate if prior incarceration experience reduces employment probabilities. Such an employment reduction seems likely, because of employers being more reluctant to hire former inmates. According to Holzer, Offner, and Sorensen (2005), prior incarceration experience probably reduces employment population ratios by somewhere in the range of 0.10 to 0.25. I use a midpoint value of 0.175. I assume that such a midpoint effect applies to males age 40. Effects at other ages are assumed to be the same percentage of the control group employment rate that is true for males age 40. This assumption allows employment effects of prior incarceration to decline in absolute terms as individuals age and leave the workforce. These employment rate effects of the NFP program due to reduced crime in turn imply effects of the NFP program on increasing earnings.

Social spillover effects of extra college graduates among the former child participants are not considered, as there is no quantitative basis for any estimates of the extra college graduates among these former child participants.

Appendix 4B NOTES

1. Specifically, the percentage impacts in Bartik (2006) were multiplied by 0.8874. The percentage impacts in Bartik (2008) were multiplied by 1.1276. These percentage impact adjustments also affect some of the numbers in the working paper versions of chapters 7 and 8.

2. 0.904 = 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 99.8–99.9 percent, and the percentage staying in the state, which declines from 91.7 percent at age 8 to 89.5 percent at age 11.

3. Some 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).

4. I 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 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.

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

6. Glaeser 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 percent (= their effect of 0.5 percent times 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 percent 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 = (10-year effect) / (1 − (0.9104^{10})). Applying this equation to a 10-year effect of 0.6 percent suggests that the long run effect is around 1 percent.

7. This is based on using the PSID-corrected PUMS data on percentage in the same state as at age four 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.

Appendix 7A

More on Discounting

This book's baseline estimates assume a real social discount rate of 3 percent. Is this the appropriate rate? What alternatives might be considered?

The most commonly used equation for deriving appropriate discount rates for discounting future flows of consumption is the well-known Ramsey equation:

$$r = d + ge ,$$

where r is the social discount rate, g is the assumed annual growth rate of per capita consumption, e is the elasticity of personal utility with respect to per capita consumption, and d is the assumed annual discount rate for future utility (sometimes called the pure rate of time preference). The basic idea is that we should discount the future more heavily either because we have an inherent preference for the present over the future, or because the future will be wealthier, and a dollar of real per capita consumption in the richer future should be valued as having a lower social value than a dollar of real per capita consumption in the poorer present.[1]

Whatever we assume about the parameters d (the rate of pure time preference) and e (the elasticity of marginal utility with respect to per capita consumption), we must choose g to be consistent with the model used in this report. This report assumes that real wages will increase by 1.2 percent per year. It is the difference between this assumed growth rate of wages and the social discount rate that mainly affects the present value calculations. It would be inconsistent to use a social discount rate that used rates of growth of per capita consumption other than 1.2 percent without also altering the rate of wage growth assumed in the model's calculations.[2]

There are a variety of perspectives for appropriate values of *d* and *e*. The *Stern Review* (Stern 2007) assumes values for *d* of 0.1 and for *e* of 1. The low value of *d* rests on the notion that there is no reason that increases in utility in the future should be valued differently from increases in utility today. The value of *e* of 1 corresponds to assuming that a given percentage change in per capita consumption has the same effect on utility for all persons at all times. The Stern report therefore implies a social discount rate for future consumption of 1.3 percent.

A recent prominent report in the *Journal of Policy Analysis and Management* by Moore et al. (2004) advocates a value for *d* of 1 and for *e* of 1. This yields an implied discount rate for future consumption of 2.2 percent.

Nordhaus (2007) and Weitzman (2007) advocate for assumptions about the parameters *d* and *e* that yield real discount rates that are closer to real rates of return on investment that we observe in the market. Nordhaus assumes a value for *d* of 1.5 and for *e* of 2, which yields a discount rate on future consumption of 3.9 percent. Weitzman assumes a value for *d* of 2.0 and for *e* of 2, yielding a discount rate on future consumption of 4.4 percent.

These seem to encompass the plausible range of rates for discounting future consumption under the assumptions about wage growth used here. Higher rates of annual wage growth could yield higher discount rates. But then future earnings would also be higher.

The rates implied by the *Stern Review*, the Moore et al. paper, the 3 percent figure of the current book, Nordhaus, and Weitzman, are used in the table in the text. One exception is that the Stern discount rate of 1.3 percent is adjusted upward to 1.6 percent. The discount rate of 1.3 percent causes present values to blow up to infinity, as the sum of 1.2 percent wage growth plus assumed population growth of 0.3 percent yields a rate of aggregate earnings growth of over 1.5 percent. A rate of 1.6 percent avoids these infinities.

APPENDIX 7A NOTES

1. This is the appropriate risk-free discount rate, but models of the appropriate discount rate do not show that a large amount should be added for risk (Weitzman 2007). Furthermore, even if we assume that a large amount should be added for risk, based on the discrepancy between real interest rates on government bonds and rates of return to equities, if program benefits are only moderately correlated with per capita consumption, the rate of discount for benefits that are 30 or 40 years in the future, when the bulk of the benefits from early childhood programs occur, will be closer to risk-free rates than to rates incorporating risk (see Weitzman 2007, pp. 711–712).

2. For example, one could argue for using a rate of per capita consumption growth equal to 1.6 percent per year, as that is the rate of per capita GDP growth used in this report and in my previous reports. However, if we are going to use that rate of per capita consumption growth to generate a discount rate, we probably should focus on total labor compensation rather than only on straight earnings. It seems likely that the labor share of GDP will not significantly decline, which implies that overall labor compensation will grow at 1.6 percent per year, even though the earnings growth figure is only projected to grow at 1.2 percent per year.

Appendix 7B

Effects of Reduced Special Education Costs on Benefits vs. Costs of Universal Pre-K and the Abecedarian Program

This appendix presents the numbers behind Figures 7.2 and 7.3 in the text. The assumptions and methods used to generate these numbers are presented in the text and endnotes to the text. Table 7B.1 presents the numbers behind Figure 7.2. Table 7B.2 presents the numbers behind Figure 7.3. Each table compares the ratio of economic development benefits to net costs associated with early childhood programs, for each year after a permanent program is enacted in 2011. One column shows the ratio when special education cost savings are not considered. The other column shows the ratio when special education cost savings are considered. Table 7B.1 and Figure 7.2 consider universal pre-K education. Table 7B.2 and Figure 7.3 consider the Abecedarian program.

Table 7B.1 Ratio of Annual Economic Development Benefits to Net Program Costs, Universal Pre-K Education, with and without Allowing for Special Education Cost Savings

Year	Universal pre-K without adjustments	Ratio of universal pre-K earnings to net costs (special ed. cost savings included)
2011	0.225	0.225
2012	0.213	0.215
2013	0.201	0.204
2014	0.189	0.193
2015	0.176	0.180
2016	0.163	0.167
2017	0.161	0.168
2018	0.160	0.170
2019	0.159	0.171
2020	0.157	0.172
2021	0.155	0.173
2022	0.152	0.174
2023	0.170	0.210
2024	0.199	0.275
2025	0.240	0.358
2026	0.305	0.489
2027	0.376	0.629
2028	0.450	0.776
2029	0.529	0.931
2030	0.612	1.092
2031	0.697	1.259
2032	0.785	1.431
2033	0.876	1.608
2034	0.959	1.772
2035	1.048	1.945
2036	1.137	2.120
2037	1.229	2.299
2038	1.321	2.479
2039	1.420	2.673
2040	1.518	2.864
2041	1.620	3.064
2042	1.724	3.267
2043	1.829	3.471
2044	1.940	3.688
2045	2.051	3.906
2046	2.165	4.128
2047	2.278	4.348
2048	2.393	4.573
2049	2.513	4.806
2050	2.628	5.031

Table 7B.1 (Continued)

Year	Universal pre-K without adjustments	Ratio of universal pre-K earnings to net costs (special ed. cost savings included)
2051	2.746	5.259
2052	2.863	5.488
2053	2.974	5.705
2054	3.090	5.931
2055	3.202	6.148
2056	3.311	6.359
2057	3.421	6.574
2058	3.529	6.784
2059	3.634	6.989
2060	3.736	7.187
2061	3.832	7.374
2062	3.919	7.543
2063	4.008	7.714
2064	4.089	7.872
2065	4.168	8.025
2066	4.241	8.167
2067	4.306	8.293
2068	4.363	8.405
2069	4.416	8.508
2070	4.461	8.594
2071	4.503	8.676
2072	4.532	8.731
2073	4.556	8.779
2074	4.576	8.818
2075	4.597	8.858
2076	4.612	8.887
2077	4.626	8.915
2078	4.637	8.936
2079	4.646	8.954
2080	4.654	8.968
2081	4.659	8.979
2082	4.663	8.986
2083	4.667	8.994
2084	4.671	9.001
2085	4.673	9.006
2086	4.675	9.009
2087	4.677	9.013
2088	4.679	9.017
2089	4.681	9.020
2090	4.682	9.024

Table 7B.2 Ratio of Annual Economic Development Benefits to Net Program Costs, Abecedarian Program, with and without Allowing for Special Education Cost Savings

Year	Ratio of benefits to gross costs (no special education cost savings)	Ratio of benefits to net costs (special ed. cost savings included)
2011	0.307	0.307
2012	0.307	0.307
2013	0.308	0.308
2014	0.310	0.310
2015	0.314	0.314
2016	0.324	0.329
2017	0.341	0.351
2018	0.365	0.383
2019	0.396	0.426
2020	0.432	0.477
2021	0.472	0.536
2022	0.514	0.601
2023	0.557	0.673
2024	0.602	0.752
2025	0.648	0.837
2026	0.695	0.930
2027	0.742	1.029
2028	0.790	1.139
2029	0.839	1.218
2030	0.910	1.328
2031	0.984	1.443
2032	1.062	1.565
2033	1.141	1.687
2034	1.224	1.815
2035	1.305	1.940
2036	1.384	2.061
2037	1.460	2.178
2038	1.534	2.292
2039	1.607	2.405
2040	1.681	2.519
2041	1.755	2.634
2042	1.830	2.749
2043	1.903	2.863
2044	1.977	2.977
2045	2.049	3.089
2046	2.120	3.199
2047	2.191	3.307
2048	2.260	3.415
2049	2.329	3.522
2050	2.398	3.628
2051	2.466	3.733

Table 7B.2 (Continued)

Year	Ratio of benefits to gross costs (no special education cost savings)	Ratio of benefits to net costs (special ed. cost savings included)
2052	2.533	3.837
2053	2.599	3.939
2054	2.663	4.039
2055	2.725	4.135
2056	2.785	4.227
2057	2.843	4.316
2058	2.897	4.401
2059	2.949	4.482
2060	2.999	4.559
2061	3.047	4.632
2062	3.090	4.700
2063	3.132	4.764
2064	3.170	4.823
2065	3.205	4.877
2066	3.236	4.925
2067	3.263	4.968
2068	3.288	5.006
2069	3.311	5.042
2070	3.331	5.072
2071	3.347	5.098
2072	3.361	5.120
2073	3.374	5.140
2074	3.385	5.156
2075	3.394	5.170
2076	3.401	5.181
2077	3.406	5.190
2078	3.411	5.197
2079	3.414	5.201
2080	3.416	5.204
2081	3.416	5.206
2082	3.416	5.205
2083	3.415	5.204
2084	3.414	5.202
2085	3.412	5.199
2086	3.409	5.195
2087	3.406	5.190
2088	3.403	5.185
2089	3.399	5.180
2090	3.395	5.174

Appendix 7C

Detailed Numbers for Simulations of Financing Universal Pre-K Education by Reallocating Funds from K–12 Education

This appendix provides the detailed numbers behind Figure 7.4 of the Chapter 7 text. These numbers show the percentage effect on state residents' earnings of a permanent universal pre-K program that is financed by cutting K–12 education spending. This budget reallocation is assumed to be a permanent change enacted in 2011. Table 7C.1 shows the numbers behind the three lines in the figure. One column shows the percentage effect on earnings of universal pre-K. A second column shows the percentage loss in earnings due to cutting K–12 spending. The remaining column shows the net effect of both changes.

Table 7C.1 Effects on State Economic Development of Financing Universal Pre-K through Reducing K–12 Spending

	Universal pre-K	Reduced K–12 $ effects	Net effects
2011	0.060	−0.046	0.014
2012	0.057	−0.043	0.014
2013	0.053	−0.040	0.013
2014	0.050	−0.038	0.013
2015	0.047	−0.035	0.011
2016	0.043	−0.033	0.010
2017	0.043	−0.035	0.007
2018	0.042	−0.038	0.005
2019	0.042	−0.041	0.001
2020	0.042	−0.045	−0.003
2021	0.041	−0.050	−0.009
2022	0.040	−0.055	−0.015
2023	0.045	−0.061	−0.016
2024	0.053	−0.068	−0.015
2025	0.064	−0.076	−0.012
2026	0.081	−0.084	−0.003
2027	0.100	−0.093	0.006
2028	0.119	−0.103	0.016
2029	0.140	−0.113	0.027
2030	0.162	−0.124	0.039
2031	0.185	−0.134	0.051
2032	0.208	−0.146	0.063
2033	0.232	−0.157	0.075
2034	0.255	−0.169	0.086
2035	0.278	−0.181	0.097
2036	0.302	−0.193	0.109
2037	0.326	−0.205	0.121
2038	0.351	−0.217	0.134
2039	0.377	−0.229	0.148
2040	0.403	−0.241	0.161
2041	0.430	−0.254	0.176
2042	0.457	−0.266	0.191
2043	0.485	−0.278	0.207
2044	0.515	−0.290	0.224
2045	0.544	−0.302	0.242
2046	0.575	−0.314	0.260
2047	0.604	−0.326	0.279
2048	0.635	−0.337	0.298
2049	0.667	−0.349	0.318
2050	0.697	−0.360	0.337
2051	0.728	−0.371	0.357
2052	0.760	−0.382	0.378
2053	0.789	−0.392	0.397
2054	0.820	−0.402	0.418
2055	0.850	−0.412	0.438
2056	0.878	−0.421	0.458
2057	0.908	−0.429	0.478
2058	0.936	−0.437	0.499

Table 7C.1 (Continued)

	Universal pre-K	Reduced K–12 $ effects	Net effects
2059	0.964	−0.445	0.520
2060	0.991	−0.452	0.540
2061	1.017	−0.458	0.559
2062	1.040	−0.463	0.577
2063	1.063	−0.468	0.595
2064	1.085	−0.473	0.612
2065	1.106	−0.476	0.630
2066	1.125	−0.479	0.646
2067	1.142	−0.482	0.660
2068	1.158	−0.484	0.673
2069	1.172	−0.486	0.686
2070	1.184	−0.488	0.696
2071	1.195	−0.489	0.706
2072	1.202	−0.490	0.712
2073	1.209	−0.491	0.718
2074	1.214	−0.492	0.723
2075	1.220	−0.492	0.727
2076	1.224	−0.493	0.731
2077	1.228	−0.493	0.734
2078	1.230	−0.494	0.737
2079	1.233	−0.494	0.739
2080	1.235	−0.494	0.741
2081	1.236	−0.494	0.742
2082	1.237	−0.494	0.743
2083	1.238	−0.494	0.744
2084	1.239	−0.495	0.745
2085	1.240	−0.495	0.745
2086	1.241	−0.495	0.746
2087	1.241	−0.495	0.746
2088	1.241	−0.495	0.746
2089	1.241	−0.495	0.746
2090	1.241	−0.495	0.746

NOTE: Estimated universal pre-K effects are as described in Chapter 4. The effects of reduced K–12 spending are as described in Chapter 7. Net effects simply sum the pre-K plus reduced K–12 spending effects. These three columns are the basis for Figure 7.4.

Appendix 7D

More Details on Methodology of Adding Adult Training to Early Childhood Programs, with Estimates

This appendix presents some of the numbers behind Figures 7.5 and 7.6. The calculation of these numbers is explained in the text.

Adding in a job training component affects these calculations of economic development benefits and costs in three ways. First, it adds in earnings effects on parents. Second, it adds in some extra budgetary costs of the programs. Third, that extra spending adds in some balanced budget multiplier effects.

As explained in the text, the earnings effects on parents are assumed to be the same for parents as was estimated for two different job training programs: 1) the results for adult women in the JTPA experiment and 2) the community college job prep results for the state of Washington in Hollenbeck and Huang (2006). Costs per trainee are also assumed to be the same, respectively, as for adult women in the JTPA experiment, and for a community college program preparing students for a job.

I had to make some assumptions about how persistent these earnings effects would be. I also had to assume when these earnings effects would begin. For the JTPA-style training program results, I assumed that earnings effects begin at age 20 and persist the same in real terms until age 50. It should be noted that keeping earnings effects the same in real terms implies a smaller percentage effect as earnings grow over the early part of the life cycle. Persistent earnings effects are consistent with post-program evidence from the GAO (1996). After age 50, I assumed earnings effects declined similar to what was assumed in the Abecedarian program's baseline calculations for the effects of the original program on adult women. These declining earnings effects reflected the decline in control group earnings as individuals age, which is accompanied by lower employment rates and lower real wage rates.

For the community college results, I assumed that earnings effects follow the actual pattern observed in the 12 quarters after training. After those 12 quarters, I assume that earnings effects decay in an annual pattern that is identical to the quarterly decay pattern assumed by Hollenbeck and Huang (2006). I also used their figures for the forgone earnings effects during training for trainees. Training was assumed to last for 1.9 years.

As with the original calculations, all estimates adjust for death rates at different ages and for the proportion of the adults likely to stay in the same state. Estimates also assume that one-third of this supply shock results in displacement, so estimated effects are scaled back by one-third.

Finally, estimates had to assume something about how many adults will participate in this program. As the Abecedarian program targets a disadvantaged group, I assumed that 75 percent of Abecedarian families would have adults participating in job training. But universal pre-K includes many middle- and upper-class families that are less likely to participate in these training efforts, at least training efforts similar to JTPA. I assumed 75 percent participation in training of the high-risk group of families, 25 percent participation of the medium-risk group of families, and zero participation of the low risk group of families. The proportions for the different risk groups are derived from Karoly et al., and are reproduced and discussed in Bartik (2006).

Table 7D.1 shows the estimated effects for universal pre-K education with these two add-on training programs. Table 7D.2 show similar estimates for the Abecedarian program. These tables are the numbers behind Figures 7.5 and 7.6.

Table 7D.1 Ratio of Annual Economic Development Benefits to Costs, Universal Pre-K Education Program, with and without Alternative Adult Training Components

Year	Ratio of benefits to costs without adult training	Ratio of benefits to costs with JTPA training	Ratio of benefits to costs with community college training
2011	0.225	0.220	0.180
2012	0.213	0.248	0.146
2013	0.201	0.276	0.239
2014	0.189	0.302	0.324
2015	0.176	0.327	0.401
2016	0.163	0.351	0.470
2017	0.161	0.386	0.540
2018	0.160	0.420	0.606
2019	0.159	0.452	0.666
2020	0.157	0.483	0.719
2021	0.155	0.513	0.767
2022	0.152	0.541	0.810
2023	0.170	0.587	0.862
2024	0.199	0.643	0.918
2025	0.240	0.709	0.977
2026	0.305	0.796	1.048
2027	0.376	0.887	1.120
2028	0.450	0.981	1.193
2029	0.529	1.079	1.266
2030	0.612	1.180	1.339
2031	0.697	1.282	1.412
2032	0.785	1.386	1.484
2033	0.876	1.492	1.557
2034	0.959	1.591	1.624
2035	1.048	1.694	1.692
2036	1.137	1.797	1.760
2037	1.229	1.900	1.827
2038	1.321	2.004	1.894
2039	1.420	2.113	1.965
2040	1.518	2.220	2.033
2041	1.620	2.331	2.104
2042	1.724	2.442	2.175
2043	1.829	2.554	2.246
2044	1.940	2.670	2.320
2045	2.051	2.786	2.394
2046	2.165	2.903	2.469
2047	2.278	3.018	2.542
2048	2.393	3.135	2.617
2049	2.513	3.254	2.694
2050	2.628	3.369	2.769
2051	2.746	3.485	2.844
2052	2.863	3.601	2.919
2053	2.974	3.710	2.991
2054	3.090	3.823	3.065
2055	3.202	3.931	3.136
2056	3.311	4.036	3.205

Table 7D.1 (Continued)

Year	Ratio of benefits to costs without adult training	Ratio of benefits to costs with JTPA training	Ratio of benefits to costs with community college training
2057	3.421	4.141	3.275
2058	3.529	4.244	3.344
2059	3.634	4.343	3.411
2060	3.736	4.440	3.475
2061	3.832	4.530	3.536
2062	3.919	4.611	3.591
2063	4.008	4.694	3.648
2064	4.089	4.770	3.699
2065	4.168	4.842	3.749
2066	4.241	4.910	3.795
2067	4.306	4.970	3.836
2068	4.363	5.023	3.873
2069	4.416	5.072	3.906
2070	4.461	5.113	3.935
2071	4.503	5.151	3.961
2072	4.532	5.177	3.979
2073	4.556	5.198	3.994
2074	4.576	5.217	4.007
2075	4.597	5.235	4.020
2076	4.612	5.249	4.029
2077	4.626	5.262	4.038
2078	4.637	5.272	4.045
2079	4.646	5.280	4.051
2080	4.654	5.287	4.056
2081	4.659	5.292	4.059
2082	4.663	5.295	4.061
2083	4.667	5.299	4.064
2084	4.671	5.302	4.066
2085	4.673	5.305	4.068
2086	4.675	5.306	4.069
2087	4.677	5.307	4.067
2088	4.679	5.309	4.068
2089	4.681	5.310	4.069
2090	4.682	5.312	4.070

NOTE: Methodology behind these numbers is explained in text. The program is assumed to start in 2011 and be ongoing. These numbers are used to generate Figure 7.5.

Table 7D.2 Ratio of Annual Economic Development Benefits to Costs, Abecedarian Program, with and without Alternative Adult Training Components

Year	Ratio of benefits to costs without adult training	Ratio of benefits to costs with JTPA training	Ratio of benefits to costs with community college training
2011	0.307	0.296	0.251
2012	0.307	0.318	0.252
2013	0.308	0.327	0.307
2014	0.310	0.333	0.337
2015	0.314	0.340	0.357
2016	0.324	0.356	0.385
2017	0.341	0.379	0.417
2018	0.365	0.409	0.455
2019	0.396	0.446	0.498
2020	0.432	0.487	0.544
2021	0.472	0.532	0.593
2022	0.514	0.578	0.641
2023	0.557	0.627	0.691
2024	0.602	0.676	0.740
2025	0.648	0.726	0.790
2026	0.695	0.777	0.840
2027	0.742	0.828	0.890
2028	0.790	0.881	0.940
2029	0.839	0.934	0.991
2030	0.910	1.008	1.060
2031	0.984	1.086	1.132
2032	1.062	1.167	1.208
2033	1.141	1.249	1.283
2034	1.224	1.335	1.362
2035	1.305	1.419	1.439
2036	1.384	1.500	1.513
2037	1.460	1.578	1.585
2038	1.534	1.655	1.654
2039	1.607	1.730	1.722
2040	1.681	1.806	1.792
2041	1.755	1.883	1.861
2042	1.830	1.959	1.930
2043	1.903	2.034	1.998
2044	1.977	2.110	2.066
2045	2.049	2.183	2.133
2046	2.120	2.256	2.198
2047	2.191	2.327	2.263
2048	2.260	2.398	2.327
2049	2.329	2.468	2.390
2050	2.398	2.537	2.453
2051	2.466	2.605	2.515
2052	2.533	2.673	2.577
2053	2.599	2.740	2.637
2054	2.663	2.805	2.696
2055	2.725	2.867	2.753
2056	2.785	2.926	2.807
2057	2.843	2.984	2.860
2058	2.897	3.039	2.909
2059	2.949	3.091	2.957
2060	2.999	3.140	3.002
2061	3.047	3.188	3.046

Table 7D.2 (Continued)

Year	Ratio of benefits to costs without adult training	Ratio of benefits to costs with JTPA training	Ratio of benefits to costs with community college training
2062	3.090	3.231	3.086
2063	3.132	3.272	3.123
2064	3.170	3.310	3.158
2065	3.205	3.345	3.190
2066	3.236	3.375	3.218
2067	3.263	3.403	3.244
2068	3.288	3.427	3.266
2069	3.311	3.450	3.287
2070	3.331	3.469	3.305
2071	3.347	3.485	3.320
2072	3.361	3.500	3.333
2073	3.374	3.512	3.344
2074	3.385	3.522	3.354
2075	3.394	3.531	3.362
2076	3.401	3.538	3.369
2077	3.406	3.544	3.374
2078	3.411	3.548	3.378
2079	3.414	3.551	3.381
2080	3.416	3.553	3.382
2081	3.416	3.554	3.383
2082	3.416	3.553	3.383
2083	3.415	3.553	3.382
2084	3.414	3.551	3.381
2085	3.412	3.549	3.379
2086	3.409	3.547	3.376
2087	3.406	3.543	3.373
2088	3.403	3.540	3.370
2089	3.399	3.537	3.367
2090	3.395	3.533	3.363

NOTE: Methodology behind these numbers is explained in text. The program is assumed to start in 2011 and be ongoing. These numbers are used to generate Figure 7.6.

Appendix 8A

Distributing Pre-K Benefits by Household Income Quintile

The baseline distributional effects of universal pre-K are based on a model used by Karoly and Bigelow (2005). The same model is crucial to estimating the overall effects of universal pre-K, as outlined in Chapter 4. This is because all the estimates of overall effects of universal pre-K are based on how effects for middle- and upper-class groups compare to effects on lower-class groups. The lower-class group effects are derived from studies of the Chicago Child-Parent Center (CPC) program and the Perry Preschool program. Both the distributional and overall effects then depend upon the extent to which these middle- and upper-class group effects are below those of the CPC and Perry programs.

Table 8A.1 summarizes the distributional assumptions made by Karoly and Bigelow. (A version of this table was previously included as Table 7 in my 2006 paper.) These numbers dictate exactly how much overall benefit there is in each income classification, and what overall benefit there is from universal pre-K, compared to the expected benefit for a low-income child from participating in universal pre-K versus no pre-K at all. It is assumed that the CPC and Perry estimates reveal the benefits of high-quality pre-K for a low-income child versus having no pre-K experience at all.

Table 8A.1 Assumptions about How Enrollment in Universal Pre-K is Divided among Different Groups

How group is affected by universal pre-K program (% of four-year-olds)	High risk: < $33,805 in family income	Medium risk: $33,805–$56,342 in family income	Low risk: > $56,342 in family income	Total four-year-olds in program usage category
In public now, none otherwise	5(100)[a]	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 pre-K now	19	16	45	80
None now	6	4	10	20
Total four-year-olds in risk group	25	20	55	100

NOTE: These numbers are taken from Table 7, Bartik (2006). They are based upon Karoly and Bigelow (2005). The income categories are updated to 2007 prices. The first number in each cell is the percentage of all four-year-olds in that cell. The columns divide all four-year-olds by their family income. The rows divide all four-year-olds by whether they are enrolled in the universal pre-K program, and by what type of pre-K, if any, they would have been enrolled in if the universal pre-K program did not exist. For example, for the high-risk group, the bottom row shows that this group constitutes 25% of all four-year-olds. The top row for the high-risk group shows that 5% of all four-year-olds are high-risk group members who enroll in the universal pre-K program but would otherwise have not been enrolled in pre-K; we can calculate from these numbers that this 5% of all four-year-olds is 20% (= 5% / 25%) of all high-risk four-year-olds.

[a]The number in parentheses in the nine cells in the 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 pre-K.

I combine these assumptions with estimates of the household income distribution to generate numbers for how pre-K impact and enrollment is divided among these three groups, and how this compares with how households are divided among these groups (Table 8A.2). Pre-K's total impact is defined as the product of the benefit percentage in the above table for each cell times the percentage of all four-year-olds in that cell. This calculation results in an index of the size of the impact in each income category, which can then be reindexed to a 100 percent total for all three groups. To calculate how households are divided among the three groups, I use published data on the household income distribution from the Current Population Survey to calculate what percent of all households are in these three groups.[1]

Table 8A.2 Allocation of Pre-K Impact, Pre-K Enrollment, and Households among Three Different Income Categories, and Implications of This Allocation for Relative Impact per Participant, Relative Impact per Household, and Relative Enrollment per Household

	Income group			
	< $33,805	$33,805 to $56,342	> $56,342	Total, all income groups
Impact (%)	67.7	21.5	10.8	100.0
Enrollment (%)	25.7	20.0	54.3	100.0
Four-year-old population (%)	25.0	20.0	55.0	100.0
Households (%)	34.5	20.7	44.8	100.0

Relative impact per participant	1.00	0.41	0.08	0.38
Relative enrollment per household	1.00	1.30	1.63	1.34
Relative impact per household	1.00	0.53	0.12	0.51

NOTE: The first three rows, dividing impact, enrollment, and the four-year-old population among these three income groups, are derived from Table 8A.1, and in turn are based on Karoly and Bigelow. The impact index is the product of the sum of the benefit percentages times the four-year-old percentages for each income category, and is reindexed to total 100%. The household percentages are derived from Current Population Survey information on-line from the Annual Social and Economic Supplement for 2008, which reports data for households in 2007. The particular table used is Table HINC-06. The remaining rows are based on dividing the allocation factors by each other and then reindexing so that the lowest income group is equal to 1.00. For example, the impact per participant is based on dividing the allocation percentage for impact by the allocation factor for enrollment, and then dividing this calculation for each cell by the same calculation for the low income group. I do not calculate the enrollment per four-year-old, but merely note that while it is close to the same for each income group, there is a very slight tendency for enrollment per four-year-old to decline with increased income.

These figures are then used to generate an index for relative benefits or impact per participant, relative participation or enrollment in pre-K per household, and relative benefits or impact per household (Table 8A.3). These figures are derived by dividing how the numerator variable is allocated across the three groups by how the denominator is divided among the three groups, and then reindexing so that the index for the lowest income group equals 1.00.

[APPENDIX TABLE 8A.3 ABOUT HERE]

Table 8A.3 Relative Benefits per Participant, Relative Enrollment per Household, and Relative Benefits per Household, for Five Household Income Quintiles, and Overall, with Lowest Income Quintile Indexed to 1.00

	Income quintile					
	Lowest		Middle		Highest	All
	1	2	3	4	5	households
Relative benefits per participant	1.00	0.81	0.31	0.08	0.08	0.38
Relative enrollment per household	1.00	1.08	1.38	1.63	1.63	1.34
Relative benefits per household	1.00	0.87	0.43	0.12	0.12	0.51

NOTE: These figures for each income quintile are derived from Table 8A.2. Numbers for enrollment per household and benefits per household are allocated to each income quintile by assuming that these same figures for enrollment per household and benefits per household are uniform within each of the three income groups defined in Table 8A.2. These three income groups are then divided up among the five income quintiles, and the figures for each income quintile are a weighted average of the income groups that make up that income quintile. For the lowest income quintile and the top two income quintiles, these three income quintiles are only made up of one income group each. The second income quintile is made up of a combination of the lowest and middle income group, with (34.5 − 20)/20 from the lowest income group and (40 − 34.5)/20 from the middle income group. The third income quintile is made up of a combination of the middle income group and upper income group. (34.5 + 20.7 − 40)/20 comes from the middle income group, and (60 − 43.5 − 20.7)/20 comes from the upper income group. Benefits per participant are then calculated by dividing benefits per household by enrollment per household.

I then translate these figures into similar relative indices for the different income quintiles. I generate these numbers by acting as if the relative benefit and participation per household figures are uniform within each of the three income groups. These three income groups are then parceled out among the five income quintiles. The figure for the income quintile will then be a weighted average of the income groups making up that quintile, with the weights equal to the proportion of each income group in that quintile. As the lowest income quintile and the top two income quintiles are each made up of only one of the three income groups, no weighted average calculation is needed for those three income quintiles. However, income quintiles 2 and 3 are each a weighted average of two of the three income groups. The figures for relative benefits per participant are then calculated by dividing relative benefits per household by relative enrollment per household.

This calculation procedure is admittedly somewhat arbitrary. It is based upon Karoly and Bigelow's assumptions, which are plausible and reasonable guesses about how enrollment and benefits will vary across income groups.

However, the bottom line is that the benefits per participant and benefits per household indices follow a plausible pattern across income quintiles. The benefits decline greatly going from the lowest income quintile to the middle income quintile, but not to zero. The benefits undergo an even more drastic decline for the top two income quintiles, so that benefits for upper income quintiles are but a small percentage of those for the lowest income quintile. This pattern is qualitatively consistent with the positions of various pre-K experts such as Steven Barnett and James Heckman as to how pre-K benefits vary with income. Furthermore, the pattern of how enrollment per household varies with income is also reasonable. We would think that the lower income quintiles would have more single individual households who would not have any four-year-olds.

In addition to providing reasonable patterns of how benefits vary by income quintile, these procedures also ensure that the distributional effect calculations in Chapter 8 are consistent with the overall effect calculations in Chapter 4. Both are based on the same assumptions about how pre-K effectiveness tails off in a universal pre-K program, compared to a targeted pre-K program, as we include middle- and upper-class children in the program.

APPENDIX 8A NOTES

1. These calculations require a little interpolation, as the published data only report the income distribution in $2,500-per-year intervals.

Appendix 9A

How Economic Development Returns to Early Childhood Programs Vary by State

This appendix reports how returns to early childhood programs other than pre-K vary by state. The early childhood programs considered are the Abecedarian program and the Nurse Family Partnership Program.

Economic development benefit ratios are adjusted based on the percentage of state residents born in the state who remain in the state. The adjustments are similar to what was done for universal pre-K in Table 9.2. Adjustments are done based on how national benefits compare with benefits in the typical state for different classes of benefits.

Table 9A.1 presents the results. It is clear that although the ratio of economic development benefits to costs does vary across states, it is always considerably greater than one in all states.

Table 9A.1 How Ratios of Economic Development Benefits to Costs Vary across States for the Abecedarian Program and the Nurse Family Partnership Program (NFP)

	Abecedarian	NFP
Alabama	2.25	1.85
Alaska	1.88	1.55
Arizona	2.29	1.88
Arkansas	2.01	1.66
California	2.46	2.02
Colorado	2.09	1.72
Connecticut	2.17	1.78
Delaware	2.10	1.73
Florida	2.38	1.95
Georgia	2.37	1.95
Hawaii	2.12	1.74
Idaho	1.93	1.59
Illinois	2.18	1.80
Indiana	2.27	1.87
Iowa	2.02	1.66
Kansas	1.94	1.60
Kentucky	2.19	1.80
Louisiana	2.33	1.91
Maine	2.20	1.81
Maryland	2.25	1.85
Massachusetts	2.20	1.81
Michigan	2.38	1.96
Minnesota	2.32	1.90
Mississippi	2.05	1.69
Missouri	2.19	1.80
Montana	1.88	1.55
Nebraska	1.94	1.60
Nevada	2.12	1.74
New Hampshire	2.07	1.70
New Jersey	2.15	1.77
New Mexico	2.04	1.68
New York	2.12	1.75
North Carolina	2.41	1.98
North Dakota	1.67	1.38
Ohio	2.31	1.90
Oklahoma	2.05	1.69
Oregon	2.19	1.80
Pennsylvania	2.27	1.86
Rhode Island	2.05	1.69
South Carolina	2.29	1.88
South Dakota	1.79	1.48
Tennessee	2.30	1.89
Texas	2.53	2.08
Utah	2.31	1.90
Vermont	2.02	1.67
Virginia	2.22	1.83
Washington	2.30	1.89
West Virginia	1.85	1.53
Wisconsin	2.37	1.95
Wyoming	1.62	1.34
Typical state	2.25	1.85

NOTE: Methodology used to estimate benefits is similar to what was used for pre-K. For each class of benefits that differs nationally from the typical state, the national benefit ratio for that class of benefits was adjusted by multiplying by the following ratio: (1 − (((National ratio − State ratio) ÷ State ratio) × (100 − % born in state who remain) ÷ (100 − 68.4)). Here, 68.4 is the percentage of those born in the state that live there still, in the typical state. The class of benefits considered are: spending; parent benefits; child benefits. Spending benefits never vary significantly between the state and national perspective. This methodology essential ly fits the adjustment to two observed data points, the national perspective, and the perspective for the typical state.

Appendix 9B

How Economic Developments for Other Early Childhood Programs Vary by Metro Area vs. State, and with Size and Growth Rate of the Metropolitan Area

This appendix shows how the ratio of economic development benefits to costs varies for the three early childhood programs considered in this book. I consider how these benefits will differ for the typical metropolitan area compared to the typical state. I also consider how these benefits differ depending upon the size and growth rate of the metro area.

These adjustments are based on Bartik (2009). Specifically, I make adjustments based on how many four-year-olds will stay at ages 30–35 in these different metro areas, compared to the typical state. I extrapolate for how benefits change in going from the national perspective to the perspective of the typical state.

Table 9B.1 shows the results. The results for pre-K were stated in the text and in Figure 9.2. The results for other programs are derived in a similar manner.

The main point of these results is that the ratio of economic development benefits to costs for all these programs exceeds one even in small or slow-growing metro areas.

Table 9B.1 How Ratios of Economic Development Benefits to Costs Vary in Metro Areas vs. States, and in Metro Areas of Different Sizes and Growth Rates, for Three Early Childhood Programs

	Typical state ratio	Typical MSA ratio	Small MSA ratio	Slow-growth MSA ratio
Pre-K	2.78	2.20	1.74	1.96
Abecedarian	2.25	1.80	1.45	1.62
NFP	1.85	1.49	1.20	1.34

NOTE: The numbers for the typical state were presented in Chapter 4. Other numbers were derived similarly to the derivations of pre-K numbers presented in the chapter text.

Appendix 10A

National vs. State Benefits of Business Incentives

This appendix considers the national versus state effects of business incentives. I consider three topics:

1) The costs of creating a job by business incentives in the nation vs. the state;

2) The national vs. state multiplier for created jobs; and

3) The effects on earnings when only state residents are included versus including persons regardless of residence.

The fourth topic, how the nation responds to a labor demand shock versus how a state responds, is considered in Appendix 10B.

NATIONAL VS. STATE COSTS OF CREATING A JOB

I derive estimates of the costs of creating an additional national job from a study by Jonathan Gruber and Joshua Rauh (2005). They estimated the national elasticity of corporate investment with respect to the federal corporate tax rate. Their estimates imply that the cost of creating a job in the nation through federal corporate tax breaks is $155,549 (in 2007 dollars) in annual foregone revenue. This estimate relies on estimates of corporate tax revenue per employee.[1] This estimate also ignores increases in the corporate tax base due to the expanded business activity. The net fiscal impact of expanded business activity is uncertain.

As explained in Chapter 3, and in Appendix 3A, the cost to create a job through business tax incentives at the state level is $21,345 in annual foregone business tax revenue. This estimate is derived from studies of how sensitive business location decisions are to state and local business tax rates. The estimate also relies on estimates of average state and local business tax revenue.[2]

For a given business incentive, the ratio of the total "incented" jobs in the nation versus the state is given by the ratio of state costs of job creation to national costs of job creation. The resulting ratio is 0.137 (= $21,345 ÷ $155,549).

NATIONAL VS. STATE MULTIPLIERS

Estimates from the REMI model for Michigan suggest that the average ratio of national to state multipliers is 1.407. The REMI model estimates average multiplier effects across all manufacturing industries of 2.95 for Michigan and 4.15 for the United States. These averages use national industry employment shares as weights.[3]

STATE RESIDENTS' EARNINGS VS. EARNINGS EFFECTS REGARDLESS OF RESIDENCE

From a national perspective, the earnings effects of business incentives should be counted regardless of where the state resident ends up living. To reflect this national perspective, I recalculated the earnings effects of business incentives under the assumption that out-migration was zero. This ended up increasing earnings benefits by 7.3 percent.

APPENDIX 10A NOTES

1. In 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 the 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 $0.115 \times (-1) \times ETR \div (1-ETR)$. 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, $d\ln(invest) \div d\ln ETR = -0.0247 = 0.115 \times (-1) \times (0.177) \div 0.823$. 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 2007 dollars gives a cost per job created of $155,549.

2. The annual forgone tax revenue needed to create one job is equal to (State and local business tax revenue per job divided by the elasticity of state business activity with respect to state and local business taxes). Based on Peters and Fisher's research (2002, p. 106), annual state and local business tax revenue per job is about $4,269 (in 2007 dollars). The estimated elasticity of state business activity with respect to state and local business activity is −0.2. Therefore, the cost of creating one state job by business tax incentives, in foregone business tax revenues, is $4,269 ÷ (0.2) = $21,345.

3. The 2.95 state multiplier is larger than the 1.80 multiplier that was used for state business incentives in Chapter 3. The 1.80 multiplier was based on projects subsidized by the MEGA business incentive in Michigan. This

business incentive also includes service firms whose multiplier effects will be lower because of less dense supplier networks and lower wages. In any event, what I am assuming here is that the ratio of national to state multipliers will be similar for a wide variety of industries.

Appendix 10B

Labor Market Response to a Demand Shock at the National Level vs. the State Level

For this appendix, I use the model discussed in Appendix 2B. I will assume that the reader of this appendix has recently read Appendix 2B.

In the model given in Appendix 2B, a shock to labor demand of *Dq* percent will have the following percentage effects on wages, employment, and population:

(Eq. 10B.1) $dW/W = Dq / (F + G))$

(Eq. 10B.2) $dEmploy / Employ = Dq(F / (F + G))$

(Eq. 10B.3) $dPop / Pop = Dq(1 / (F + G)) (a_p(1/g) + e_p)$

Based on these equations, and assumptions about the parameters, we can calculate the percentage effects on earnings per capita (the earnings-to-population ratio). Appendix 2B explains the baseline assumptions used at the state and national level. Based on these assumptions and the above equations, a 1 percent shock to labor demand at the state level will boost earnings per capita by 0.34 percent. A 1 percent shock to labor demand at the national level will boost earnings per capita by 0.32 percent.

The former percentage is quite close to the actually estimated effects of demand shocks at the state level on earnings per capita. The model implies that the national effect will not be much different.

Why are the effects not much different? Labor supply per capita has the same responsiveness at the national as at the state level in this model. Private labor demand is less responsive at the national level than at the state level. However, it is assumed that the demand management of the Federal Reserve Board and other macroeconomic policymakers, which adjusts overall labor demand based on the unemployment rate, makes overall demand similarly responsive per capita at the national level as it is at the state level. With similar slopes of both labor demand and supply curves per capita, the incidence of demand and supply quantity shocks is similar at the national level to that of the state level.

The assumed private labor demand elasticity with respect to wages at the national level is −0.3. This is 10.4 percent of the labor demand elasticity with respect to wages assumed at the state level, of −2.88. It could be argued this is inconsistent with the assumption that job creation at the national level is 19.3 percent as responsive to incentives as it is at the state level. However, nothing much changes if I adjust these labor demand elasticities so that the national labor demand elasticity is 19.3 percent of the state labor demand elasticity. For example, if I adjust the national labor demand elasticity to −0.556, which is 19.3 percent of the state labor demand elasticity, the percentage effect on earnings per capita at the national level of a demand shock is 0.31 percent. I get a larger but still modest effect if I instead adjust the state labor demand elasticity up so it is (1 ÷ 0.193) times a national labor demand wage elasticity of (−0.3) (e.g., I use a state labor demand elasticity of –1.55 rather than the baseline value of –2.88). The percentage earnings effects at the state level of a 1 percent shock to labor demand then become 0.45 percent.

Based on all this, I think it roughly accurate to assume that the percentage effects on earnings per capita of a demand shock is roughly similar at the national and state levels. If anything, the percentage effects on earnings per capita of a demand shock might be slightly less at the national level than at the state level. Adjusting for this slightly lower earnings effect would further reduce national effects of business incentives. As the analysis is arguing that national effects of business incentives are much lower than state effects, I decided to be conservative by not including this further downward adjustment in national effects.

As noted in Appendix 2B and Chapter 2, I assume similar displacement effects in labor supply shocks at the national and state levels. The model instead suggests that effects of supply shocks on earnings per capita will actually be slightly larger at the national level than at the state level. If these model simulations were incorporated, this would tend to increase the effects of early childhood programs at the national level beyond what I report in the text of Chapter 10. However, as I am already arguing that national effects of early childhood programs will be larger at the national level than at the state level, I decided to be conservative by not incorporating this further upward adjustment in national effects.

Appendix 10C

Macroeconomic Effects of Redistributing Unemployment

I assume overall national prices are a weighted average of some non-linear function of the unemployment rate in each region. This nonlinear function is assumed to be such that a given reduction in unemployment has more effects on prices when unemployment is low in a region than when unemployment is high. The evidence from Bartik (2000) suggests that metropolitan price levels are a linear function of (−1) over the local unemployment rate.

For present purposes, I am considering the effects of incentives in a given state *s*. Therefore, I divide up the nation into two regions, state *s*, and all other states designated by subscript "*o*". The weight on state s will then be some small proportion of 1, and the weight on all other states "*o*" will be close to 1. These assumptions can be expressed in the following equation:

(Eq. 10C.1) $\ln(\text{national price level}) = B[Wf(Us) + (1 - W)\, f(Uo)]$

Here, *f* is the function that explains the local price level as a function of the local unemployment rate. *W* is the weight in the national price level on prices in state *s*. *B* is some parameter (assumed positive) that relates this weighted average of local price pressures to national prices. *Us* and *Uo* are the unemployment rates in state *s* and in all other states *o*, respectively.

I am considering how incentives in state *s* will affect price pressures. These effects occur directly through effects on state *s*'s unemployment rate. In addition, there are indirect effects because the redistribution of employment and population to state *s* will affect the unemployment rate in all other states. Furthermore, macroeconomic authorities may respond to any change in price pressures by using macroeconomic policy to offset the change in prices. Therefore, the unemployment rate in state *s* and all other states *o* may also change because of macroeconomic policy. I express this by totally differentiating the equation (10C.1) with respect to the changes in unemployment in state *s* and other states *o* brought about by both incentives in state *s* and the macroeconomic policy response. This total differentiation results in the following equation:

$$\text{(Eq 10C.2)} \qquad \text{dln(national price level)} = B[Wfu(Us)dUsi + (1 - W)\, fu(Uo)dUoi + Wfu(Us)dUsm + (1 - W)\, fu(Uo)dUom] \;.$$

In this equation, *fu* is the partial derivative of the local price pressure variable with respect to the local unemployment rate. As this function is nonlinear, this derivative depends upon the local unemployment rate. The terms *dUsi* and *dUoi* represent changes in the unemployment rate in response to incentives in state *s*. The terms *dUsm* and *dUom* represent changes in the unemployment rates in response to macroeconomic policy.

The idea here is that for any given incentives in state *s*, there is some macroeconomic policy response that will make Eq. 10C.2 equal to zero. That is, if the unemployment rate changes that are due to incentives decrease (increase) price pressures, then macroeconomic policy can keep prices the same by decreasing (increasing) the unemployment rate. This change in overall national unemployment rates will in turn imply increases in overall national GDP and earnings. "Okun's Law" finds that a 1-percentage-point reduction (increase) in the national unemployment due to macroeconomic policy will increase (decrease) national GDP and earnings by 2 percent.

To actually solve for what macroeconomic policy response is feasible for a given incentive policy, we need to know something about the relationships between changes in unemployment in state *s* and all other states *o*. We need to know something about how unemployment in all other states *o* will increase when incentives lower unemployment in state *s*. We also need to know something about the pattern by which macroeconomic policy decreases (increases) unemployment in both states *s* and *o*.

For incentives, I assume that the change in the unemployment rate due to incentives in a given area is at any given time some multiple of the incentive-induced shock to the natural log of the employment-to-population ratio. This function will reflect that initially the incentive-induced shock will be reflected in large part in a change in the unemployment rate, but that over time more of the shock to the employment rate will be accommodated by a change in the labor force participation rate. Essentially I am assuming that the reduced employment-to-population ratio in all other states *o* will have similar percentage effects on the unemployment rate in those states to the effects on the unemployment rate in state *s* associated with an increased employment-to-population ratio in state *s*. The advantage of this formulation is that we have data on how incentives will affect employment, employment-to-population ratios, and unemployment rates in state *s*, and this formulation allows us to express what happens in other states as a function of what happens in state *s*.

Specifically, I assume:

(Eq. 10C.3a) $dUsi = Kt(dEs / Es - dPs / Ps)$;

(Eq. 10C.3b) $dUoi = Kt(dEo / Eo - dPo / Po)$.

Here *Kt* is some parameter that is a function of the time since the incentive was introduced. It will be negative, and its absolute value will decline towards zero over time. *E* is employment and *P* is population. The equation says that the change in unemployment rates in each region are a function of the percentage change in each region's employment-to-population ratio, which in turn depends upon the percentage changes in each region's employment and population. The empirical studies of how local labor markets respond to demand shocks reveal the value of parameter *Kt* at different times since the incentive is introduced.

Now, we know something about how employment and population change in other states because of incentives in state *s*. Specifically, the change in total employment in state *s* and all other states together will be some fraction of the change in employment in state *s*. Based upon the arguments in Chapter 10, this fraction is 0.193, which reflects the lower national effects of incentives on incented industries, which are partly but not completely offset by the higher national multiplier (0.193 = 0.137 × 1.407).[1] Furthermore, any incentive-induced change in population in state *s* will result in an exactly offsetting change in population in other states *o*. These assumptions can be expressed by the following equations:

(Eq. 10C.4a) $dEs + dEo = mdEs$, where $m = 0.193$;

(Eq. 10C.4b) $dPs + dPo = 0$.

These Equations 4a and 4b can be manipulated to express percentage changes in employment and population in all other states as a function of percentage changes in employment and population in state *s*. In writing these equations, I assume that the proportions of employment and population initially in state *s* are equal to the weight *W* on state *s* in the overall national price equation:

(Eq. 10C.5a) $dEo / Eo = (m - 1)(dEs / Es)(Es / Eo) = (m - 1)(dEs / Es)(W/(1 - W))$;

(Eq. 10C.5b) $dPo / Po = (-Ps / Po)(dPs / Ps) = (-W / (1 - W))(dPs / Ps)$.

Using Equations (5) and (3), the incentive-induced change in the unemployment rate in other states can be written as a function of the change in the employment-to-population ratio in other states, which is a function of the change in the employment-to-population ratio in state *s*, which in turn is related to the change in the unemployment rate in state *s*. We end up with the following equation relating the incentive-induced change in the unemployment rate in other states to the incentive-induced change in the unemployment rate in state *s*:

(Eq. 10C.6) $dUoi = (-W / (1 - W))\, dUsi + m(W / (1 - W))(1 / G)\, dUsi$,

where $G = (dEs / Es - dPs / Ps) / (dEs / Es)$.

Parameter *G* is the percentage response of the employment-to-population ratio in state *s* to an incentive-induced demand shock to employment in state *s*. This parameter *G* varies over time in response to such a demand shock, and is known from the empirical literature on the effects of demand shocks on local labor markets.

Using Equation (6) together with Equation (2), for any incentive-induced change in unemployment in state *s*, we can express the combined effect of incentive-induced changes in state *s* and other states *o* on overall prices.

For macroeconomic policy responses, we assume that macroeconomic policy changes all unemployment rates proportionately. This is expressed in the following equation:

(Eq. 10C.7) $dUom = (Uo / Us) dUsm$.

This assumption is justified by empirical findings in Bartik (2000) that unemployment rates of various groups seem to vary at least roughly proportionately.

Using Equations (2), (6), and (7) together, we can calculate the macroeconomic changes in unemployment rates needed to offset the price pressures resulting from incentives in some state *s*. Using Okun's Law, we can calculate the percentage changes in national earnings and output associated with those macroeconomic-induced changes in the unemployment rate.

I do these calculations under two scenarios. One scenario is for incentives in an above-average unemployment-rate state. The other scenario is for incentives in a below-average unemployment-rate state. For both scenarios, I assume that the state is an "average sized" state;

that is, its weight *W* is 2 percent of the national total. For the national and state unemployment rates, I choose actually observed unemployment rates from a year, 2007, that is a business cycle peak year in which inflation might be a significant policy concern. The overall national unemployment rate in that year was 4.6 percent. I use for my "high unemployment state" scenario the unemployment rate of the highest-unemployment-rate state in 2007—Michigan at 7.1 percent. For my "low unemployment rate state" scenario, I use the unemployment rate of the lowest-unemployment-rate state in 2007—Utah at 2.7 percent.

For both scenarios, I assume that local price pressures depend on $(-1 / U)$ in the region. This is consistent with the empirical findings of Bartik (2000). The derivative of local prices with respect to the local unemployment rate will then be equal to 1 over the unemployment rate squared.

Table 10C.1 shows the results. I show the earnings effects of these macroeconomic offsets on an annual basis, as well as the present value of these earnings effects as a proportion of incentive costs. As the table shows, the unemployment rate effects of incentives are short-term. Therefore, any macroeconomic effects from offsetting price pressures of changes in the distribution of unemployment are also short-term.

Table 10C.1 Macro Earnings Effects as a Percentage of State Earnings Effects of Incentives as of Different Years after Incentives Started

	High-unemployment-state macro effects	Low-unemployment-state macro effects
Immediate incentive effects (%)	16.2	−131.8
1 year later (%)	12.0	−115.0
2 years later (%)	7.8	−95.2
3 years later (%)	3.7	−71.2
4 years later (%)	0.6	−40.8
Later-year effects are zero		
Present value as proportion of present value of incentive costs	0.09	−0.98

As the table shows, these short-term macroeconomic effects are substantial percentages of the effects of incentives in the state offering the incentive. From a present value standpoint, incentives in a low unemployment state create price pressures that require sufficient macroeconomic restraint to substantially lower the national benefits of business incentives.

APPENDIX 10C NOTES

1. For this present model, I ignore the national versus state differential due to out-migration, as this will only increase national benefits by 7 percent, and relatively little out-migration by former state residents will occur in the short run, which is what these unemployment shocks deal with.

Appendix 12A

More Details on the Methodology and Results for Economic Development Benefits from Specific Human Development Outcomes

This appendix explains the methodology for determining economic development benefits at the state level for changes in human development outcomes brought about by some intervention. I then present the year-by-year results that are summarized in the chapter text.

I take the following steps to determine economic development benefits from human development outcomes. First, I estimate an appropriate baseline earnings at each age of an individual's life cycle that are somehow altered by some human development intervention. Baseline earnings are derived from estimates from the Current Population Survey, Outgoing Rotation Group. These estimates are taken from the CPS-ORG for 2007. I calculate employment rates, wage rates per hour, and weekly work hours separately for men and women for each age from 16 to 79. Annual earnings are calculated as the product of these factors times 52 weeks.

Second, I then find some estimate of how this human development intervention will at each age affect earnings outcomes. I also make some assumption at what age this intervention occurs. The intervention is assumed to start in the year 2011. Before adding in the earnings effect of the human development intervention, I adjust the baseline earnings figures over the life cycle of the person for assumed real wage growth of 1.2 percent per year from 2007 to that particular year.

Third, I adjust the earnings effects downward based on mortality rates and out-migration from the state. Mortality rate adjustments are done separately for men and for women.

Adjustments are based on the official life tables of the United States for 2004, downloaded from the National Center for Health Statistics at http://www.cdc.gov/nchs/data/nvsr/nvsr56/nvsr56_09.pdf. The adjustments are ratios of survivors to a given age from the age at which the intervention is assumed to begin.

Out-migration adjustments are based on 2000 census data from the Public Use Microdata Samples. I take ratios of the percentage of persons living in their birth state at different ages to the percentage living in their birth state as of the year the intervention began. This ratio is meant to provide an approximation to the percentage living in a state at the intervention age that will stay until later ages.

I also adjust downwards for displacement effects. As discussed in Chapter 2, I assume displacement is one-third. This means that the net effect on state earnings of some gross increase in some individual or group's earnings is two-thirds of the gross increase in earnings.

Fourth, if the intervention is early enough, I adjust upwards for positive peer effects. These positive peer effects are assumed to occur in elementary school. Positive peer effects occur because of higher academic achievement for an assisted student or assisted students helping to increase the academic achievement for other students. Therefore, I only adjust upwards for peer effects for interventions that occur before or during elementary school. These adjustments are the same as those made for pre-K programs. Based on prior research, as discussed in the appendix to Chapter 4, the peer effect adjustment is to multiply earnings effects by 1.54. I indicate in Chapter 12 which outcomes I adjust for peer effects.

Fifth, I calculate the present values of the effects on earnings for this outcome change for a specific individual. This calculation represents the net effect on state earnings, after displacement and out-migration and any peer effects. This calculation averages the net earnings effect, weighting men and women equally. As discussed in Chapter 7, this estimation uses a social discount rate of 3 percent. The present value is calculated as of the age the program is initiated. The program is also assumed to be initiated in the year 2011.

Sixth, for each outcome, I make some assumptions about how many individuals in the state will benefit from this outcome change. I then multiply the earnings effect at each age for an individual by the number of individuals in the state who benefit, to get a gross effect at each age for a particular cohort. The actual calculation is done at the national level. I take the number of men and women in the nation as of the intervention age for July 1, 2008, from the official Census Bureau estimates. I adjust the figures for that age up to a 2011 intervention by assuming the baseline annual increase in population of 0.3 percent. As will be described below, estimates are rescaled to the state level by being divided by national earnings for each age. Earnings effects for each cohort are derived from separate earnings effects at each age for men and women.

Seventh, I then assume that this human capital development intervention will continue indefinitely. I calculate the estimates for each subsequent cohort, assuming normal population growth and economic growth. I sum estimated effects over each cohort to get total effects as of a particular year. Mechanically, each subsequent cohort has effects for year t that are equal to those of the previous year's cohort for year $t - 1 \times 1.003$ to allow for population growth and $\times$ 1.012 to allow for growth in real wages. These calculations assume the same population growth and real wage growth in each state. They therefore are best interpreted as effects for the average state.

Eighth, I then divide the earnings effects for all cohorts in each year by state earnings in each year. This gives a percentage effect on state economic development in each year of some permanent change in economic development outcomes initiated in some baseline year. This percentage effect generally takes some time to get going. The initial effect is small or zero, as few or none of the initial cohorts have entered the labor market at the time the intervention is initiated in 2011. The effect then increases over time as cohorts reach peak earnings ages and other affected cohorts enter the labor market. The effect of this change in human development outcomes eventually reaches some peak effect on economic development outcomes as a percentage of the state economy. This occurs when all cohorts participating in the state economy have benefited from the human development intervention. I report this peak percentage effect in the chapter text. At this peak point, the earnings effects on new cohorts grow at (1.003) times (1.012) per year, while state earnings grow at the same rate. National earnings totals are used in the denominator to calculate percentage earnings effects, as the calculation of the effects of this human development intervention use national population figures. The percentage effects are meant to be appropriate for a national average effect. The national numbers are based on average real wages from the CPS-ORG, as well as average full-time equivalent employment from the Bureau of Economic Analysis. The average real wage is multiplied by national full-time equivalent employment and then by 2000 annual work hours. Effects for each year are adjusted upwards by a multiple of 1.003 for population growth and a multiple of 1.012 for real wage growth. These same national earnings numbers are used for all the early childhood program percentage calculations and all the business incentive program percentage calculations.

Ninth, I also calculate the present value over an infinite future of these economic development benefits. This present value is calculated as a percentage of the present value of state earnings. The present value is calculated as of the year 2011, when the intervention is initiated. This present value reflects both early-year and late-year effects of the intervention. Early-year effects are low or zero because few if any cohorts benefiting from the human development intervention have entered the labor market. These present-value percentage effects are therefore in-between zero and the peak percentage effects.

Table 12A.1, below, gives the percentage effects for each year of each type of intervention. Each intervention is assumed to begin in the year 2011. One intervention is omitted from the table. Reducing adult mental illness, or drug abuse or alcohol abuse, by 1.0 percent of the population is estimated to increase earnings by 0.10 percent in all years, with a present value of 0.10 percent of earnings.

Table 12A.1 Year-by-Year Net Effects on State Residents' Earnings, as a Percentage of State Earnings, of Permanent Programs Begun in 2011 to Improve Various Human Development Outcomes

Year	All students' elementary test scores increase by 0.1 effect size (%)	All students' secondary test scores increase by 0.1 effect size (%)	Reduction in high school dropouts by 1% of population (%)	Increase in bachelor's degrees by 1% of population (%)	Increase in associate's degrees by 1% of population (%)	Reducing crime rates by 10%	Reduce low-birth-weight births by 1% of all births (%)	Reduce repeat ADHD by 1% of population (%)
2011	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
2012	0.000	0.000	0.002	0.000	0.000	0.000	0.000	0.000
2013	0.000	0.000	0.003	0.000	0.000	0.001	0.000	0.000
2014	0.000	0.000	0.005	0.000	0.003	0.001	0.000	0.000
2015	0.000	0.000	0.006	0.005	0.005	0.002	0.000	0.000
2016	0.000	0.000	0.009	0.010	0.007	0.004	0.000	0.000
2017	0.000	0.013	0.011	0.016	0.011	0.006	0.000	0.000
2018	0.000	0.027	0.014	0.022	0.013	0.008	0.000	0.000
2019	0.000	0.042	0.018	0.029	0.015	0.010	0.000	0.000
2020	0.000	0.059	0.022	0.038	0.018	0.013	0.000	0.000
2021	0.000	0.076	0.026	0.046	0.022	0.015	0.000	0.000
2022	0.000	0.095	0.030	0.055	0.026	0.018	0.000	0.000
2023	0.000	0.114	0.033	0.064	0.030	0.022	0.000	0.000
2024	0.000	0.133	0.038	0.074	0.034	0.025	0.000	0.000
2025	0.000	0.153	0.043	0.085	0.037	0.028	0.000	0.000
2026	0.000	0.175	0.048	0.096	0.041	0.032	0.000	0.000
2027	0.010	0.196	0.053	0.108	0.045	0.036	0.000	0.000
2028	0.023	0.219	0.057	0.120	0.050	0.040	0.000	0.000
2029	0.037	0.241	0.063	0.131	0.053	0.044	0.000	0.000
2030	0.052	0.265	0.068	0.143	0.057	0.048	0.000	0.000
2031	0.071	0.288	0.073	0.156	0.061	0.052	0.001	0.001
2032	0.090	0.311	0.078	0.168	0.064	0.056	0.001	0.001
2033	0.112	0.335	0.084	0.179	0.068	0.060	0.002	0.002
2034	0.134	0.357	0.089	0.191	0.072	0.064	0.003	0.003
2035	0.160	0.380	0.095	0.204	0.075	0.069	0.005	0.004
2036	0.188	0.402	0.101	0.215	0.079	0.073	0.007	0.005
2037	0.217	0.425	0.107	0.227	0.082	0.077	0.009	0.006
2038	0.248	0.448	0.113	0.240	0.086	0.081	0.012	0.007
2039	0.278	0.471	0.118	0.252	0.090	0.085	0.014	0.008
2040	0.310	0.493	0.124	0.264	0.095	0.088	0.018	0.009

Table 12A.1 (Continued)

Year	All students' elementary test scores increase by 0.1 effect size (%)	All students' secondary test scores increase by 0.1 effect size (%)	Reduction in high school dropouts by 1% of population (%)	Increase in bachelor's degrees by 1% of population (%)	Increase in associate's degrees by 1% of population (%)	Reducing crime rates by 10%	Reduce low-birth-weight births by 1% of all births (%)	Reduce repeat ADHD by 1% of population (%)
2041	0.342	0.515	0.130	0.276	0.098	0.092	0.021	0.010
2042	0.374	0.537	0.136	0.288	0.101	0.096	0.025	0.012
2043	0.405	0.558	0.141	0.300	0.104	0.099	0.029	0.013
2044	0.436	0.580	0.146	0.311	0.108	0.102	0.034	0.014
2045	0.467	0.600	0.152	0.322	0.111	0.105	0.039	0.015
2046	0.498	0.620	0.157	0.331	0.114	0.108	0.043	0.017
2047	0.528	0.640	0.162	0.342	0.117	0.111	0.048	0.018
2048	0.560	0.659	0.166	0.352	0.119	0.114	0.053	0.019
2049	0.590	0.677	0.171	0.361	0.122	0.116	0.057	0.020
2050	0.621	0.695	0.175	0.370	0.124	0.119	0.062	0.021
2051	0.651	0.710	0.180	0.379	0.127	0.121	0.067	0.023
2052	0.681	0.726	0.183	0.387	0.131	0.123	0.072	0.024
2053	0.710	0.741	0.187	0.394	0.132	0.124	0.076	0.025
2054	0.739	0.755	0.190	0.402	0.135	0.126	0.081	0.026
2055	0.767	0.768	0.193	0.408	0.138	0.128	0.086	0.027
2056	0.795	0.778	0.196	0.414	0.138	0.130	0.091	0.028
2057	0.822	0.786	0.198	0.420	0.140	0.131	0.095	0.030
2058	0.848	0.794	0.199	0.426	0.141	0.132	0.100	0.031
2059	0.872	0.800	0.201	0.431	0.142	0.132	0.105	0.032
2060	0.896	0.805	0.202	0.433	0.144	0.133	0.109	0.033
2061	0.917	0.808	0.203	0.436	0.144	0.134	0.114	0.034
2062	0.939	0.812	0.203	0.439	0.145	0.134	0.118	0.035
2063	0.959	0.815	0.204	0.440	0.145	0.135	0.122	0.036
2064	0.978	0.817	0.204	0.442	0.145	0.135	0.127	0.037
2065	0.995	0.818	0.205	0.443	0.145	0.135	0.131	0.038
2066	1.009	0.820	0.206	0.445	0.145	0.135	0.135	0.039
2067	1.020	0.822	0.206	0.446	0.145	0.136	0.138	0.039
2068	1.031	0.823	0.206	0.447	0.146	0.136	0.142	0.040
2069	1.039	0.824	0.207	0.448	0.146	0.136	0.145	0.041
2070	1.046	0.825	0.207	0.449	0.146	0.136	0.148	0.041

Table 12A.1 (Continued)

Year	All students' elementary test scores increase by 0.1 effect size (%)	All students' secondary test scores increase by 0.1 effect size (%)	Reduction in high school dropouts by 1% of population (%)	Increase in bachelor's degrees by 1% of population (%)	Increase in associate's degrees by 1% of population (%)	Reducing crime rates by 10%	Reduce low-birth-weight births by 1% of all births (%)	Reduce repeat ADHD by 1% of population (%)
2071	1.051	0.826	0.207	0.449	0.146	0.136	0.151	0.042
2072	1.055	0.827	0.207	0.451	0.146	0.136	0.154	0.042
2073	1.059	0.827	0.207	0.451	0.146	0.136	0.156	0.042
2074	1.062	0.827	0.207	0.451	0.146	0.136	0.158	0.043
2075	1.064	0.827	0.207	0.451	0.146	0.136	0.160	0.043
2076	1.067	0.827	0.207	0.451	0.146	0.136	0.161	0.043
2077	1.069	0.827	0.207	0.451	0.146	0.136	0.162	0.043
2078	1.070	0.827	0.207	0.451	0.146	0.136	0.163	0.043
2079	1.072	0.827	0.207	0.451	0.146	0.136	0.164	0.043
2080	1.074	0.827	0.207	0.451	0.146	0.136	0.164	0.044
2081	1.074	0.827	0.207	0.451	0.146	0.136	0.165	0.044
2082	1.075	0.827	0.207	0.451	0.146	0.136	0.165	0.044
2083	1.076	0.827	0.207	0.451	0.146	0.136	0.165	0.044
2084	1.076	0.827	0.207	0.451	0.146	0.136	0.166	0.044
2085	1.076	0.827	0.207	0.451	0.146	0.136	0.166	0.044
2086	1.076	0.827	0.207	0.451	0.146	0.136	0.166	0.044
2087	1.076	0.827	0.207	0.451	0.146	0.136	0.166	0.044
2088	1.076	0.827	0.207	0.451	0.146	0.136	0.167	0.044
2089	1.076	0.827	0.207	0.451	0.146	0.136	0.167	0.044
2090	1.076	0.827	0.207	0.451	0.146	0.136	0.167	0.044
Present value of earnings effects as percentage of present value of state earnings:	0.632	0.569	0.144	0.309	0.104	0.096	0.088	0.025

NOTE: Procedures to make these calculations are described in Chapter 12 text. All effects are net effects on state residents' earnings of specified permanent changes in human development outcomes. Net effects account for mortality, out-migration, and displacement, as well as (in some cases) peer effects. The program changing outcomes is assumed to begin in year 2011, with a cohort of a particular age, as indicated in chapter text or endnotes. Effects build over time until all cohorts have benefitted from the change in human development outcomes. Present value is calculated as of year 2011, and reflects an infinite time horizon for both state earnings and the earnings effects. Present value calculations use a 3 percent discount rate.

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