Appendix O: Statistical analysis and economic impact modeling

Weldon Cooper Center conducted several statistical and economic impact analyses for this review.

Statistical analysis of incentive program selection and effects

Weldon Cooper Center conducted statistical analysis to estimate industrial park absorption rates and estimate the program effects for the enterprise zone program and the Tobacco Region Opportunity Fund (TROF) grant.

Estimation of industrial site absorption

Weldon Cooper Center staff estimated the absorption rates (or rates at which firms occupy newly available park space) of business and industrial sites funded by the Tobacco Region Megasite Grant and the Business Ready Sites Program using several data sources. The primary source of information was a survey of Virginia local economic development staff. The survey asked local economic developers to

- estimate the size and occupancy levels of existing completed business and industrial parks, year of park opening, source of ownership (public/private/other), and employment of tenant firms for parks in their localities;
- provide information about the availability and characteristics of parks under development, including ownership, size, and business readiness rating; and
- estimate the percentage of occupant firms that were business startups, relocations from within the community or state, or newly located firms in the Commonwealth.

Studies of industrial site absorption sometimes take into consideration various factors, including location, park size, park amenities, transportation infrastructure availability, workforce availability, market conditions, availability of space elsewhere in the region, and other factors in assessing absorption potential. The effectiveness of industrial site owners in marketing and recruitment of industrial park space may also play a role. Vacated space that is not in 'ready to move in' condition (e.g., brownfield industrial space) is generally not considered by firms as available for locational purposes. The only two variables utilized here to model park absorption rates are

- park age (*AGE*) (higher rates of occupancy are expected as time elapses after the opening of a park) and
- location along the rural-urban continuum (reflecting the size of the labor force and market conditions, which should increase park absorption).

A multiple regression model using a fractional probit (*fracreg probit* in Stata) was estimated that linked computed absorption rates to the independent variables (Table O-1). Fractional probit was used because the dependent variable is measured as a proportion bounded by 0 and 1. Locality urbanization (*URCODE*) was measured using a USDA Economic Research Service rural-urban continuum measure varying from 1 (locality is in a large metro area) to 9 (locality is in a remote rural non-metro locality with little urban population). It is interpreted here as a scale variable rather than an ordinal or nominal

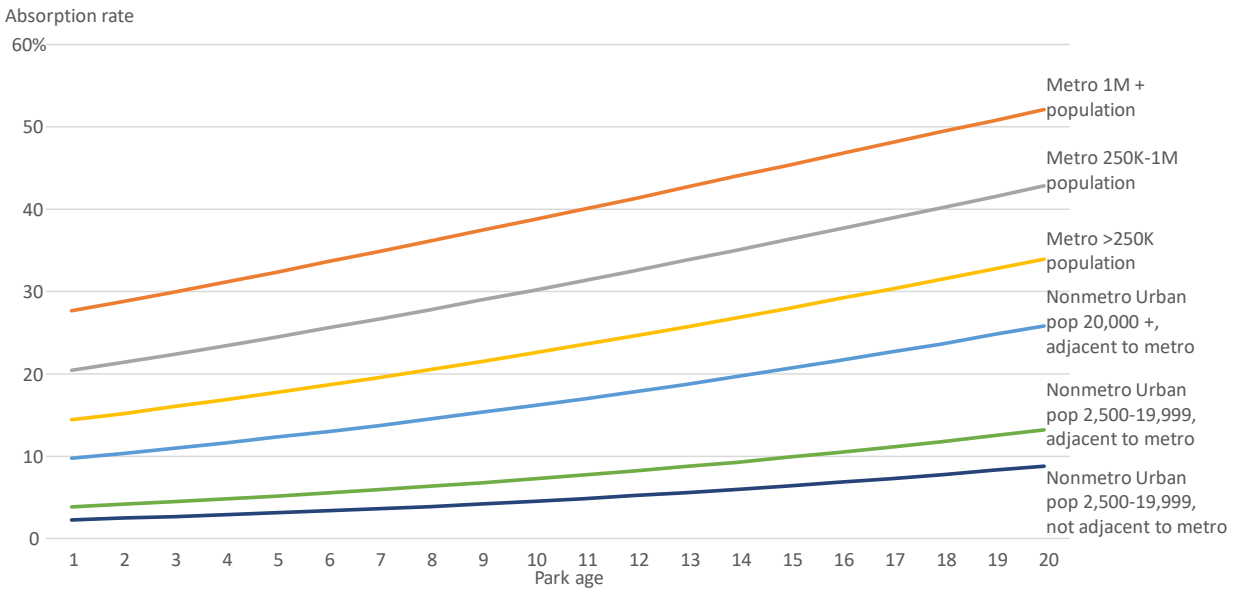
categorical variable for expediency and because of the limitations of competing urban-rural measures (Isserman 2005).

TABLE O-1
Results for fractional probit regression of industrial park absorption rates

Variable	Coefficient	dy/dx	Standard deviation	z	p value
AGE	0.03406	0.15795	0.02031	1.69	0.094
URCODE	-0.23409	-0.18669	0.09092	-2.57	0.010
CONSTANT	-0.35890		0.62416	-0.58	0.565
Number of obs	23				
Wald chi2(2)	1.27409				
Prob>chi2	0.0000				
Log pseudolikelihood	-12.0349				
Pseudo R-squared	0.1363				

Predicted absorption rates (percentage of developed acreage occupied) were then estimated by year for each rural-urban continuum category (Figure O-1). Predicted absorption rates by rural-urban continuum category were matched with business/industrial sites by the same category to estimate the absorption rate for each post-opening year beginning in 2020 for each site. The absorption rates were then multiplied by site acreage to obtain the occupied acreage. The 5-year absorption rate predicted by the model for the Berry Hill megasite was within the range (300–500 acres) predicted by a private consultant's market analysis for the site (Jones Lang LaSalle 2014). Validating data from other sites was not available.

FIGURE O-1:
Industrial park absorption varies by locality urbanization and park age



Employment projections at the sites were obtained by multiplying the estimated developed acreage over time by the average employment per acre as reported in the survey of local economic developers (2.72 jobs per industrial park acre). Not all of the park employment is anticipated to be new employment to the state, since relocations from elsewhere within the community and state to the parks will occur. In the survey, local economic developers reported that 40 percent of businesses that occupy their industrial parks were relocations from within the locality and 23 percent were relocations from elsewhere in the state. Some of these relocations, however, may have occurred for business expansion purposes. Only 29 percent of the businesses were from relocations from outside the state or new establishments to the state, and 8 percent were business startups. Based on these responses, it is assumed that approximately half of industrial site employment is relocation of existing employment from within the state rather than net new employment to the state.

Quasi-experimental statistical analysis of enterprise zone program effects

Quasi-experimental analyses were conducted of the local economic effects of the enterprise zone program as a whole and the effects of program components, the Real Property Investment Grant and Job Creation Grant. The geographical units of analysis included

- localities (i.e., counties and independent cities),
- Census Zip Code Tabulation Areas or ZCTAs (referred to here as "zip codes"), and
- enterprise zones.

The results for enterprise zones were not materially different from other findings reported here. Furthermore, the reliability of the microdata for that exercise is unknown since establishment reporting location may not reflect actual location of employment. For these reasons the results are not displayed.

The first effect estimated is for the enterprise zone programs as a whole. This is the total, reduced form effect of a locality having an enterprise zone designated inside it, meaning that it catches the average impact of Real Property Investment Grant funds, Job Creation Grant funds, and local incentives offered over all enterprise zones. Four measures of enterprise zone "success" are assessed over time:

- employment (measured by unemployment rate for localities and employment for zip codes),
- average wages,
- share of children in local public schools eligible for a free or reduced lunch, and
- house price levels.

The first three measures are chosen because they are explicitly identified in the program's statute as factors to be considered when a locality is granted an enterprise zone. The last measure is chosen because it captures the general "attractiveness" of an area.

The analysis covers the period between FY11 and FY18. Variables, variable abbreviations, data sources, and variable descriptions used in all subsequent empirical analyses are described briefly in Table O-2 and Table O-3.

TABLE O-2
Variable descriptions and sources for locality-level analyses

Variable (abbreviation)	Source	Description
Unemployment rate (unemp)	Bureau of Labor Statistics Local Area Unemployment Series	Annual unemployment rate for locality.
Income (inc)	Bureau of Labor Statistics Quarterly Census of Employment and Wages	Average annual wage for locality.
School lunch eligibility (lunch)	National Center for Educational Statistics Common Core of Data	Share of students in public schools located in the locality eligible for a discounted or free school lunch.
Home prices (price)	Zillow Home Value Index All Homes	Natural log of average home prices for the year in the locality as estimated by Zillow.
Job Creation Grant funds disbursed (jcg)	Department of Housing and Community Development	The natural log of total amount of JCG funds disbursed in the locality plus one.
Real Property Investment Grant funds disbursed (rpig)	Department of Housing and Community Development	The natural log of total amount of RPIG funds disbursed in the locality plus one.
JCG funds requested (jcg_req)	Department of Housing and Community Development	The natural log of total amount of JCG funds requested in the locality plus one.
RPIG funds requested (rpig_req)	Department of Housing and Community Development	The natural log of total amount of RPIG Funds requested in the locality plus one.
Poverty rate (pov)	U.S. Census Bureau, American Community Survey, 5-year estimates	Share of population in the locality below the poverty line.
Education level (educ)	U.S. Census Bureau, American Community Survey, 5-year estimates	Share of population in the locality aged above 25 with a college degree.
Racial composition (blk)	U.S. Census Bureau, American Community Survey, 5-year estimates	Share of population in the locality who are Black/African American.

Population (pop)	U.S. Census Bureau Population Estimates Program	Natural log of locality population.

TABLE O-3
Variable descriptions and sources for zip-code analyses

Variable (abbreviation)	Source	Description
Employment (emp)	U.S. Census Bureau Zip Code Business Patterns	Natural log of number of employees in the zip code.
Income (inc)	U.S. Census Bureau Zip Code Business Patterns	Average annual wage for zip code.
School lunch eligibility (lunch)	National Center for Educational Statistics Common Core of Data	Share of students in public schools located in the zip code tabulation area eligible for a discounted or free school lunch.
Home prices (price)	Zillow Home Value Index All Homes	Natural log of average home prices for the year in the zip code as estimated by Zillow.
JCG funds disbursed (jcg)	Department of Housing and Community Development	The natural log of total amount of JCG funds disbursed in the locality plus one.
RPIG funds disbursed (rpig)	Department of Housing and Community Development	The natural log of total amount of RPIG funds disbursed in the locality plus one.
JCG funds requested (jcg_req)	Department of Housing and Community Development	The natural log of total amount of JCG Funds requested in the locality plus one.
RPIG funds requested (rpig_req)	Department of Housing and Community Development	The natural log of total amount of RPIG Funds requested in the locality plus one.
Poverty rate (pov)	U.S. Census Bureau, American Community Survey, 5-year estimates	Share of population in the zip code below the poverty line.
Education level (educ)	U.S. Census Bureau, American Community Survey, 5-year estimates	Share of population in the locality aged 25 years and above with a college degree.

Racial composition (blk)	U.S. Census Bureau, American Community Survey, 5-year estimates	Share of population in the zip code who are Black/African American.
Population (pop)	U.S. Census Bureau, American Community Survey, 5-year estimates	Population by locality.

The simplest comparison that can be made matches enterprise zones to non-enterprise zones. For this comparison, the following regression is run to establish a baseline:

$$y_{ct} = \beta ez_{ct} + \lambda_t + X_{ct} + e_{ct}$$

- y_{ct} is the outcome of interest (e.g., unemployment rate, income, or house prices) in the locality (or zip code) c during year t.
- ez_{ct} is a dummy variable that takes the value 1 if the locality had an enterprise zone that year.
- λ_t are year fixed effects.
- X_c is a vector containing various control variables such as local poverty rates, share of residents 25 years and older with a college degree, population, and share of residents who are black.
- e_{ct} is the error term.

Note that fixed effects are not added for zip codes or localities in this analysis. This is because it would mean relying on the places that became or ceased to be an enterprise zone during the time period. Since there are so few places like them, enterprise zone designation in and of itself is close to perfectly collinear with the locality dummies, and the results would be too noisy to expect to make any reliable inferences.

Treating the enterprise zone as a dummy variable assumes that it is the same treatment in each county. While this can be informative of the overall success of the program, it is problematic because each enterprise zone varies along a number of different dimensions in the type of treatment it receives. The amount granted under the Real Property Investment Grant and Job Creation Grant programs is different in each zone and the local incentives offered are different in each zone too. Furthermore, some places may have been zones longer than others, and thus been exposed to the treatment longer. Local incentives offered should be captured in the locality fixed effects, and the variation in the Real Property Investment Grant and Job Creation Grant will be dealt with when those incentives are assessed individually. To capture the time a place has been an enterprise zone, ez_{ct} was modified to measure the number of years a place has been designated, but this did not change results from a dummy variable in any substantial way. These results are not presented.

Using localities with enterprise zones has some shortcomings. Localities are typically much larger than enterprise zones, so the effect may be underestimated due to a large part of the sample not having been treated because it is not part of the zone. It is even possible that the enterprise zone could be redistributing activity within the locality to the enterprise zone leading to a positive coefficient within the enterprise zone and a negative one outside of it, creating a net zero effect. Thus, the above regression is also rerun with zip codes as the unit of observation rather than localities as well as with localities

split into their enterprise zone and non-enterprise zone designated sections. These regressions control for locality fixed effects because zip codes within enterprise zones are compared to other parts of the locality that do not have an enterprise zone.

The biggest underlying problem with the baseline results is that the designation of enterprise zone is non-random. They are chosen for specific reasons and the effects that are found may be due to them being economically distressed areas rather than their designation as enterprise zones. Given that enterprise zone incentives do not give a rational reason to deter investment in an area, it is assumed the baseline results are an underestimation. These issues are addressed and causation established by causation using stratification with the regression method proposed by Rosenbaum and Rubin (1983, 1984) and discussed at length in Imbens and Rubin (2015) and Imbens (2015). The approach uses propensity scores to put observations in several strata so that comparison is made to localities that had a similar probability of having been designated and then attempts to adjust for any remaining differences using Ordinary Least Squares (OLS) regressions.

The use of stratification is appealing in this instance because the program statute is quite clear about how enterprise zones should be designated. The legislation specifies:

> "Consideration for enterprise zone designations shall be based upon the locality-wide need and impact of such a designation. Need shall be assessed in part by the following distress factors: (i) the average unemployment rate for the locality over the most recent three-year period, (ii) the average median adjusted gross income for the locality over the most recent three-year period, and (iii) the average percentage of public school students within the locality receiving free or reduced price lunches over the most recent three-year period. These distress factors shall account for at least 50 percent of the consideration given to local governments' applications for enterprise zone designation." *Code of Virginia* (§ 59.1-545).

Creating a propensity score based on these variables is a reasonable identification strategy. However, since the statute also implies that other factors than these variables may be considered, it is assumed that those factors are either random or not substantial enough such that they would change the designation of any enterprise zone or non-zone. Since the designation process refers to locality-wide and time-varying factors, the propensity score is estimated at the locality level in each year.

Stratification on the propensity score is performed by dividing observations into groups based on their propensity scores. Propensity scores measure the likelihood of an observation receiving a particular treatment (in this case the establishment of an enterprise zone) and is implemented here using a logit specification. It has been recommended that such stratification be done by partitioning observations into five equal-size groups based on the quintiles of the estimated propensity score to reduce linear bias (Austin 2011). However, though common, quintiles are arbitrary, and in theory increasing the number of strata used should result in improved bias reduction, even if this is marginal.

Observations are assigned to strata in the following manner. First, the sample is trimmed by dropping areas that had an extremely high (low) probability of being treated. Observations with a propensity score above (below) 95 percent (105 percent) of the largest (smallest) estimated propensity score estimated were dropped. Second, after trimming the sample, the propensity score is re-estimated.

Third, the observations are grouped into strata based on the new propensity scores. To assess the adequacy of the strata, a t-statistic is computed for each where the null hypothesis is that the propensity score is the same for treated and control units. If the null hypothesis is rejected at the 5 percent level, two new strata are created by splitting the existing one at its median propensity score. Starting with the entire sample, this process is iterated until the null hypothesis cannot be rejected or there are fewer than two localities that were designated left in the strata. The result of this process is that within each stratum, treated and untreated observations will not have detectably different values of the propensity score. Therefore, provided the propensity score has been correctly specified, the distribution of measured baseline covariates (both observed and unobserved) will be approximately similar in each stratum.

Once the strata are identified, a baseline OLS specification is estimated within each stratum. The stratum specific average treatment effects are then averaged with the number of areas in each stratum as weights. Standard errors for the treatment effect on the treated across all strata are calculated by weighting the stratum level clustered standard errors using the same set of weights.

As mentioned, estimating the average effect of enterprise zone designation is important to assess the overall efficacy of the program, but doing so means the individual impacts of the Real Property Investment Grant and Job Creation Grant programs are pooled. To assess these individually, regressions are run of the baseline form:

$$y_{ct} = \beta_1 rpig_{ct} + \beta_2 jcg_{ct} + \lambda_c + \lambda_t + X_c + e_{ct}$$

Where $rpig_{ct}$ and jcg_{ct} are the natural logs of the dollar amounts disbursed to an area from the Real Property Investment Grant and Job Creation Grant respectively. Also, since there is year-to-year variation in disbursed funds location fixed effects, λ_c, can now be added to account for any time-invariant omitted variables specific to that location.

Assessing the Real Property Investment Grant and Job Creation Grant like this brings back the selection issues of before, but they cannot be solved in the same way. This is because the grants are examined by looking along the intensive margin (comparing enterprise zones to other enterprise zones) rather than the extensive margin (comparing enterprise zones to non-enterprise zones). Instead one needs to look for exogenous variation in the amounts of the Real Property Investment Grant and Job Creation Grant that are disbursed to each enterprise zone.

For the Job Creation Grant one can use what is known as a regression discontinuity (RD) design. RD designs are applicable in instances where a policy changes at some continuous threshold. The logic is that the difference between observations just above and just below the threshold in question will not be different in any way of importance, and the differences between them are as good as random. The Job Creation Grant is usually not allowed to be used to fund jobs that pay less than 175 percent of the minimum wage unless they are used in an enterprise zone designated as a high unemployment area (HUA), in which case they are allowed to use the grant for any job that pays no less than 150 percent of the minimum wage. The threshold to be designated as a HUA is having an unemployment rate that is 1.5 times the state average. HUAs are updated every year. So, the following regression is run:

$$\widehat{jcg_{ct}} = hua_{ct} + \sigma_1(unemp_{ct-1}/unemp_{st-1}) + \sigma_2(unemp_{ct-1} * hua_{ct}/unemp_{st-1}) + \epsilon_{ct}$$

- hua_{ct} is a dummy variable that takes the value 1 if the area is a HUA in year t,

- $\frac{unemp_{ct-1}}{unemp_{st-1}}$ is the ratio of local unemployment in the previous year to the state unemployment in the previous year, and
- the σ terms represent basis splines so that the ratio variables are modeled smoothly.

Observations are weighted using a tricube kernel with bandwidth set so that only observations that have values for $\frac{unemp_{ct-1}}{unemp_{st-1}}$ between 1.4 and 1.6 are included in this regression. The predicted values of this regression are then used in place of actual values of Job Creation Grant funds and the outcomes of interest are regressed on them.

Identification here rests on the fact that an area just below the threshold should be comparable to one just above the threshold in every way but it is receiving the additional Job Creation Grant funds associated with being a HUA. Any differences that determined being below the threshold should be non-systematic and random. Since the treatment is a continuous variable, this is akin to a fuzzy RD design where the designation as a HUA is an instrument for Job Creation Grant dollars granted.

Regression discontinuity cannot be used to assess the Real Property Investment Grant since it does not have any discontinuity or kink in disbursement. To establish identification, the fact that it takes on a lower priority than the Job Creation Grant is used to estimate the effects of Real Property Investment Grant funds disbursed using the statewide amount of Job Creation Grant requested less the Job Creation Grant funds requested in that zone. This instrument will be valid for identification so long as: (i) the rest of the state's Job Creation Grant requests are not a function of that zone's Real Property Investment Grant requests and (ii) the rest of the state's Job Creation Grant requests only move that zone's Real Property Investment Grant disbursement through the Real Property Investment Grant funds disbursed.

For clarification, a hypothetical example with two enterprise zones (A and B) is provided. There is a total of $100 to be disbursed between the two zones in a given year. Zone A requests $20 in Job Creation Grant funds and $40 in Real Property Investment Grant funds. Zone B requests $10 in Job Creation Grant funds and $80 in Real Property Investment Grant funds. First, Zone A and Zone B will receive $20 and $10 in Job Creation Grant funds, respectively. This leaves $70 in Real Property Investment Grant funds to disburse, but $120 in total Real Property Investment Grant requests from both zones. Real Property Investment Grant funds will then be prorated (per statute) at a rate of 70/120. Zone A will receive $23, and Zone B will receive $46. If Zone A had requested $50 instead of $20, the proration would have been 50/120, and Zone B's disbursed Real Property Investment Grant funds would decline to $33. Thus, Zone A's increased requests cause Zone B's Real Property Investment Grant disbursement to decline. If Zone B requests more Real Property Investment Grant funds, this will not have any effect on Zone A's Job Creation Grant funds. So, it meets assumption (i) listed above. For assumption (ii), it needs to be that Zone A's requests will not have any effect on Zone B's economic outcomes aside from the one they have through decreasing the proration on Real Property Investment Grant funds. This is assumed to be unlikely because they are geographically distant.

Contrary to expectations, results indicate that after accounting for poverty rates, education levels, and racial composition, localities with enterprise zones still do worse than those without enterprise zones (Table O-4). Localities with enterprise zones typically have an unemployment rate that is 0.3 percent higher, are not detectably different in their income, have schools where the share of students eligible for a free or discounted lunch is 4.3 percent higher, and home prices are 9.2 percent lower. This should

not, however, be considered as strong evidence that enterprise zones are ineffective. Localities are specifically given enterprise zones because they are economically distressed, and the zones do not cover the entire locality. It could be that the enterprise zone areas themselves are improving but this is being "washed out" by the rest of the locality. It is also possible that there is some other variable that creates economic distress that has not been controlled for and that it has a larger effect than any benefits of the enterprise zones, making it look like localities with enterprise zones are worse off.

TABLE O-4
Locality-level analysis of enterprise zone effects

	Dependent variables			
	unemp	**inc**	**lunch**	**price**
EZ	0.003***	0.011	0.043***	-0.092***
	(0.001)	(0.013)	(0.008)	(0.015)
Observations	1,075	942	1,054	1,043
R^2	0.806	0.472	0.747	0.862

***p<0.01; **p<0.05; *p<0.1
NOTE: Table shows coefficients from linear regressions of a dummy variable that takes the value 1 if the county has an enterprise zone and 0 otherwise. County demographics and year fixed effects are controlled for but not shown to save space.

Though it does not solve the omitted variables problem, one can get a sense if enterprise zones only have economic development effects within their own areas by looking at smaller geographic areas. Therefore, the analysis described above was replicated, but the unit of observation is changed to zip codes (i.e., Census Zip Code Tabulation Areas or ZCTAs) rather than localities. At the zip code level, information on the unemployment rate is unavailable, so employment is used instead as an indicator of labor market health. Enterprise zones do appear to have positive effects on the zip codes in which they are placed (Table O-5). There is no statistically detectable difference in house prices and school lunch eligibility in zip codes with enterprise zones compared to the rest of their localities. However, they do have 11.7 percent higher income and 30 percent higher employment.

TABLE O-5
Zip-code level analysis of enterprise zone effects

	Dependent variables			
	emp	**inc**	**lunch**	**price**
EZ	0.301***	0.117***	0.002	0.015
	(0.045)	(0.019)	(0.009)	(0.014)
Observations	4,350	4,350	2,863	2,415
R^2	0.900	0.423	0.722	0.913

***p<0.01; **p<0.05; *p<0.1
NOTE: Table shows coefficients from linear regressions of a dummy variable that takes the value 1 if the zip code has an enterprise zone and 0 otherwise. Zip code demographics as well as county and year fixed effects are controlled for but not shown to save space.

Stratified regressions based on propensity scores were conducted to control for non-random selection into enterprise zones (Table O-6). For the county level results, only the effect for income is significant, but it is negative. Counties with enterprise zones experience a decline in wages of around 0.5 percent on average. For the zip code results, only the effect for employment is significant, and it is positive. The effect is impressive, suggesting that zip codes with enterprise zones experienced increases in employment of 9 percent. Overall, these results suggest that the enterprise zone program, taken as a whole, has not been successful at improving the economic performance of the designated counties they reside within. There is some evidence that they improve employment at lower levels (i.e. zip codes), but the economic effects are not pervasive enough to be regarded as compelling.

TABLE O-6
Results of stratified regressions based on propensity scores

	Coefficient	Standard error
	County/city level	
unemp	-0.003	0.002
inc	-0.006***	0.02
lunch	0.017	0.012
price	-0.149	0.085
	Zip code level	
emp	0.095***	0.012
inc	0.01	0.011
lunch	0.012	0.005
price	-0.007	0.013

***p<0.01; **p<0.05; *p<0.1

It is shortsighted, however, to assess enterprise zones as one "catch-all" effect. After all, areas are eligible for two different programs, and the effect may vary according to the level of "take-up" of each program in each zone, particularly if one program is more effective than the other. Baseline results for both programs are shown in the two tables below.

At the county level (Table O-7), funds disbursed for the Job Creation Grant and Real Property Investment Grant seem to have similar negative effects on the variables of interest. The effect on wages is statistically significant at the 5% level for the Job Creation Grant, but it is small (a 1% increase in Job Creation Grant funds will result in lowering the wage by 0.02%). More concerning is the statistically significant effect found for the Real Property Investment Grant that suggests a 1% increase in funds disbursed will lower home prices by 1.2%.

TABLE O-7
Program spending effects from locality-level analysis (baseline)

	Dependent variables			
	unemp	inc	lunch	price
rpig	-0.00003	-0.002	0.001	-0.012***
	(0.0001)	(0.0030)	(0.0010)	(0.0030)
jcg	-0.0002*	-0.002**	0.001	-0.0002
	(0.0001)	(0.0010)	(0.0010)	(0.0030)
Observations	312	271	306	309
R2	0.834	0.774	0.698	0.775

***p<0.01; **p<0.05; *p<0.1

At the zip code level (Table O-8) there is evidence of some positive effects. There is no effect on wages, employment, or school lunch eligibility. However, a 1 percent increase in funds disbursed through either program results, on average, in an increase in home prices of 0.4 percent.

TABLE O-8
Program spending effects from zip-code level analysis (baseline)

	Dependent variables			
	emp	**inc**	**lunch**	**price**
rpig	-0.004	-0.003	-0.002	0.004***
	(0.0060)	(0.0080)	(0.0020)	(0.0004)
jcg	-0.006	-0.005	0.002	0.004**
	(0.008)	(0.011)	(0.003)	(0.001)
Observations	500	500	481	386
R^2	0.548	0.887	0.959	0.996

***p<0.01; **p<0.05; *p<0.1

To test the baseline estimate under stricter conditions, the fuzzy regression discontinuity design discussed earlier is implemented, which effectively compares places that were designated as HUAs to places that just nearly could have been designated as HUAs. Using this specification, there is no evidence of significant effects like the ones found before for either counties or zip codes (Table O-9). In part, this could be a sample size issue, since using the regression discontinuity method requires one to restrict the observations to those close to the cutoff, resulting in a first stage F-Stat of only 8.293 for counties and 9.393 for zip code areas. Stock and Yogo (2002) suggest that this number be at 10 or above as a rule of thumb. Results might change substantially with more observations. However, given what the baseline estimates say, this is regarded as no evidence of an effect of the Job Creation Grant on the unemployment rate, income, free and reduced lunch eligibility, and home prices.

Finally, the Real Property Investment Grant program is assessed using the quasi-experimental design that uses that year's Job Creation Grant funds requested in every other zone to instrument for Real Property Investment Grant funds disbursed in that zone. As explained before, the Job Creation Grant takes precedence over the Real Property Investment Grant so the amount of Job Creation Grant funds applied for outside of that zone have an effect on Real Property Investment Grant funds disbursed inside the zone. It is difficult to believe other zones' Job Creation Grant funds are determining the local zones Real Property Investment Grant funds through any other way but their funds effect on the proration. The first stage F-stats are both well-above the rule of thumb of 10, so one can be confident in the strength of the instrument. In neither case are there significant effects for the Real Property Investment Grant (Table O-10).

TABLE O-9
Regression discontinuity design results for effect of Job Creation Grant on localities and zip codes

	Dependent variables			
	unemp	**inc**	**lunch**	**price**
Localities				
jcg	-0.015	-0.07	-0.028	0.032
	(0.019)	(0.158)	(0.066)	(0.045)
Observations	182	157	182	182
First-stage F-stat	8.293	8.293	8.293	8.293
Zip codes				
Jcg	0.261	2.076	-0.231	0.172
	(5.886)	(7.439)	(0.692)	(3.346)
Observations	172	172	169	168
First stage F-stat	9.393	9.393	9.393	9.393

***p<0.01; **p<0.05; *p<0.1

TABLE O-10
Instrumental variable results for effect of Real Property Investment Grant on localities and zip codes

	Dependent variables			
	unemp	**inc**	**lunch**	**price**
Localities				
rpig	0.044	-0.008	-0.009	0.007
	(0.082)	(0.005)	(0.009)	(0.005)
Observations	207	207	202	206
First stage F-stat	19.583	19.583	19.583	19.583
Zip Codes				
rpig	-0.111	-0.046	0.002	0.008
	(0.082)	(0.034)	(0.005)	(0.005)
Observations	329	329	315	243
R^2	18.88	18.88	18.88	18.88

***p<0.01; **p<0.05; *p<0.1

Overall, basic regressions and a variety of quasi-experimental methods are unable to find consistent evidence for positive effects for enterprise zones as a whole, or for the Job Creation Grant or Real Property Investment Grant programs when they are assessed separately. There are some limitations to this analysis. One cannot rule out the possibility of extremely local effects found within only the

immediate neighbors of a building or business given a grant. However, given the evidence provided earlier it is reasonable to conclude that enterprise zone programs have not improved economic outcomes in enterprise zone localities. These findings are consistent with many assessments of other states' enterprise zone programs and the general consensus that state-level enterprise zone programs have not stimulated economic development. Both the Real Property Investment Grant and Job Creation Grant are relatively small programs that disbursed less than $15 million combined statewide each year: it is unsurprising that positive effects are difficult to measure.

Quasi-experimental statistical analysis of Tobacco Region Revitalization Commission and TROF program effects

The Tobacco Region Opportunity Fund (TROF) was created to provide grants and loans to help create high-wage jobs and capital investment in the tobacco region. The Tobacco Commission's strategic plan states specifically that the targeted outcomes of TROF are increasing local employment, the local average wage, and local capital assets. Locality employment log-levels and locality logged median wage levels are used to assess TROF since they are both readily available and reliably recorded. A simple regression to establish a baseline effect can be run using the following regression:

$$y_{ct} = bTROF_{ct} + B'X_{ct} + f_c + f_t + e_{ct} \quad (1)$$

- y_{ct} is the outcome variable (either logged median wage or logged employment) in locality c in year t,
- $TROF_{ct}$ is the logged number of net funds disbursed (including clawbacks),
- X_{ct} is a vector of locality level control variables including the poverty rate, education levels, population, and racial composition,
- f_c and f_t are locality and year fixed effects,
- e_{ct} is the error term, and
- b is the coefficient of interest that captures the average effect of a percent increase in TROF grants given to a locality.

It may be unfair to consider TROF year-by-year. It could well be that funds disbursed one year do not have an effect that shows up in the data until a few years later, for instance. So a second regression is run also:

$$\Delta y_{ct} = bTROF_c + B'X_{ct-1} + e_{ct} \quad (2)$$

The coefficient has the same interpretation here, since Δy_{ct} captures the percentage growth in the outcome variable from FY11–FY18, it allows TROF funds to have impacts many years after they were granted. This does, however, mean that fixed effects can no longer be used; thus the estimates may now be vulnerable to year-specific effects that affect all localities (such as a drop in the federal fund rates or oil price movements in a given year) and time-invariant local characteristics (such as geographic features) to bias the estimates. The main issue with identifying B in the above two regressions is that TROF grants are not randomly assigned. Furthermore, since the selection process relies, in some ways, on intangible characteristics and the Tobacco Commission's perception of these characteristics, the

grant selection process cannot be explicitly modeled. Instead TROF funds are instrumented for using historical industrial compositions.

The Tobacco Commission was established in 1999 with the specific mission to bring economic growth and development to formerly tobacco-dependent communities. So it stands to reason that, at least partially, grants will be determined by the economic decisions to focus on particular industries a long time ago. In this case grants and loans from TROF are analogous to contemporaneous local shocks to capital investment in an area. While the Tobacco Commission was established based on the regions' past economic conditions, it is mostly the current conditions that the committee are looking at when they make grants. Furthermore, there is no way a contemporaneous shock can have effects on the past industrial conditions of an area. This follows the strategies used in Ciccone and Hall (1996) and Combes et al. (2008).

This strategy relies on the hypothesis that industrial choices made in the past are not related to modern changes in labor market conditions. This is more likely to hold the further in the past the instruments are drawn from. County Business Pattern industry composition data from the U.S. Census Bureau from 1975, 1985, and 1995 are used as instruments. The specific variables are industry division shares of total county employment (except for unclassified establishments and public administration).

There are shortcomings to using historical instruments. They are predetermined; so this rules out concerns about two-way causality since the future cannot determine the past. However, this does not preclude past industrial composition affecting current labor market conditions through some variable besides TROF funds. Though one can control for the present day industrial compositions to mitigate this, there are other possible channels for the past to work through. For instance, investment in a certain industry may have had impacts on the built environment that still remain in the present day as fixed capital.

Because of these concerns, a second instrument is also used; the presence of an enterprise zone in the locality in question. TROF funds require a matching amount from some other source to be received, and applicants often look to other state incentives to find the required match. The enterprise zone Job Creation Grant is a popular source of matching funds, since it is in many ways a less restrictive version of TROF. The rationale for the instrument is that having an enterprise zone will bring in additional TROF funds to a locality because there are more alternatives for matches in those locations. The enterprise zone could also have an effect through the funds it disburses, so it is added as a control variable.

The issue with using enterprise zone designations as an instrument is that their allocation is not random. To solve this, the propensity score strategy from the enterprise zone section is used, and TROF funds are instrumented for using enterprise zones by several different strata. This ensures each locality is compared only to localities that had a similar probability of being designated as an enterprise zone according to their unemployment, income, and share of public school students eligible for school lunches. If specified correctly, comparing one county to another with a similar propensity score means the decision to designate one place but not another is as good as random.

Only the effect of TROF on the counties inside the tobacco region are analyzed; thus many comparable localities are excluded. One can also assess the effectiveness of TROF along the extensive margin by looking at the effect of being in the tobacco region as a whole. Following Glaeser and Gottlieb

(2009), a dummy variable is created that takes the value 1 if a locality was inside the tobacco region and 0 if it was outside it. So the regression is:

$$\Delta y_{ct} = bTR_c + B'X_{ct-1} + e_{ct} \quad (3)$$

where TR_C is the aforementioned dummy variable. The first regression uses all localities in Virginia; then the sample is restricted to exclude those near the Washington, D.C., metro area (the cities Alexandria, Fairfax, Falls Church, Manassas, and Manassas Park; and the counties of Arlington, Fairfax, Loudoun, and Prince William). Next, a regression is run accounting for the degree of urban influence in the locality using the USDA's urban influence ratings as control variables. Finally, each locality is represented by a dummy variable, allowing the distribution of effects of the TR to be calculated, though this foregoes being able to estimate standard errors. The analysis covers the period from FY11–FY18 (Table O-11).

TABLE O-11
Locality level variable descriptions and sources

Variable (abbreviation)	Source	Description
Income (inc)	Bureau of Labor Statistics Quarterly Census of Employment and Wages	Average annual wage for locality.
Employment (emp)	U.S. Census Bureau County Business Patterns	Natural log of total employment in the locality.
TROF funds completed (TROF)	Tobacco Region Revitalization Commission	Natural log of dollar amount of TROF funds disbursed (rather the amount stated on the award).
Poverty rate (pov)	American Community Survey, 5-year estimates	Share of population in the locality below the poverty line.
Education level (educ)	American Community Survey, 5-year estimates	Share of population in the locality aged above 25 with a college degree.
Racial composition (blk)	American Community Survey, 5-year estimates	Share of population in the locality who are Black/African-American.
Population (pop)	Census Bureau Population Estimates Program	Natural log of locality population.
Industrial composition (ind)	US Census Bureau County Business Patterns	Share of total local employment in an industry division (nine industry division variables, except for public administration and nonclassified establishments).
Enterprise zone (EZ)	Department of Housing and Community Development	Dummy variable with 1 = locality has EZ, 0 = locality does not have EZ.
Urban influence (UI)	U.S. Department of Agriculture, Economic Research Service, Urban Influence Codes	A ranking on a scale of 1–12 that describes to what degree an area is both urbanized and is under the influence of other nearby urban areas.

Table O-12 shows the results from the regression in equation (3). Columns 1 and 2 include all counties in Virginia. Columns 3 and 4 drop the counties in vicinity of the Washington, D.C., metro area, and columns 5 and 6 control for the influence urban influence on the area. Through all the cases the sign on the coefficient is negative, meaning the tobacco region, after controlling for other factors, had slower employment and wage growth than the rest of the state. However, the estimates are not statistically significant.

TABLE O-12
Linear regression estimates for the effect of the Tobacco Region Revitalization Commission

	Dependent variables					
	emp_growth	**inc_growth**	**emp_growth**	**inc_growth**	**emp_growth**	**inc_growth**
	(1)	(2)	(3)	(4)	(5)	(6)
TR	-0.034	-0.02	-0.027	-0.025	0.005	-0.016
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
Observations	129	129	118	118	118	118
R^2	0.092	0.178	0.088	0.175	0.229	0.28

***p<0.01; **p<0.05; *p<0.1

Figure O-2 shows the results letting each locality be its own dummy variable in the regression. After running this regression the counties were divided into those in the tobacco region and those outside, and kernel density estimates were obtained for each group (i.e., frequency distribution of the dummy variable estimates). The mass of the tobacco region is clearly to the left in both panels in the figure indicating that, on average, it has had lower wage and employment growth over the period from FY11–FY18. However, the distributions heavily overlap in both panels meaning that there are many localities in the tobacco region that grew faster than the rest of the state and vice versa.

FIGURE O-2:
Density of growth rates after controlling for baseline factors, tobacco region and other Virginia

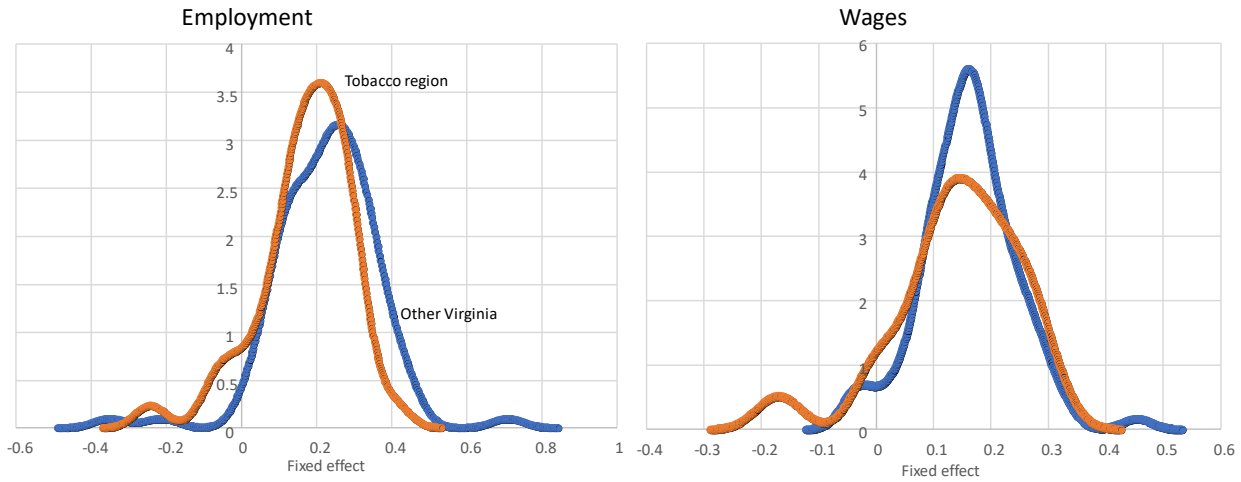


Figure O-3 replicates Figure O-2 but divides the tobacco region localities into the Southside and the Southwest regions. It conveys the same basic information as Figure O-2, showing that both of the areas within the tobacco region generally grew at slower rates than the rest of the state, but there are exceptions. Generally speaking, it also seems to show Southside has fared slightly worse than Southwest in the same period. Recall that these estimates control for education levels, share of black residents, and population. So, even though Southwest is generally considered as the most economically distressed region of Virginia, when its initial conditions are controlled for it has actually outperformed the neighboring Southside counties.

FIGURE O-3:
Density of growth rates after controlling for baseline factors, Southwest, Southside and other Virginia

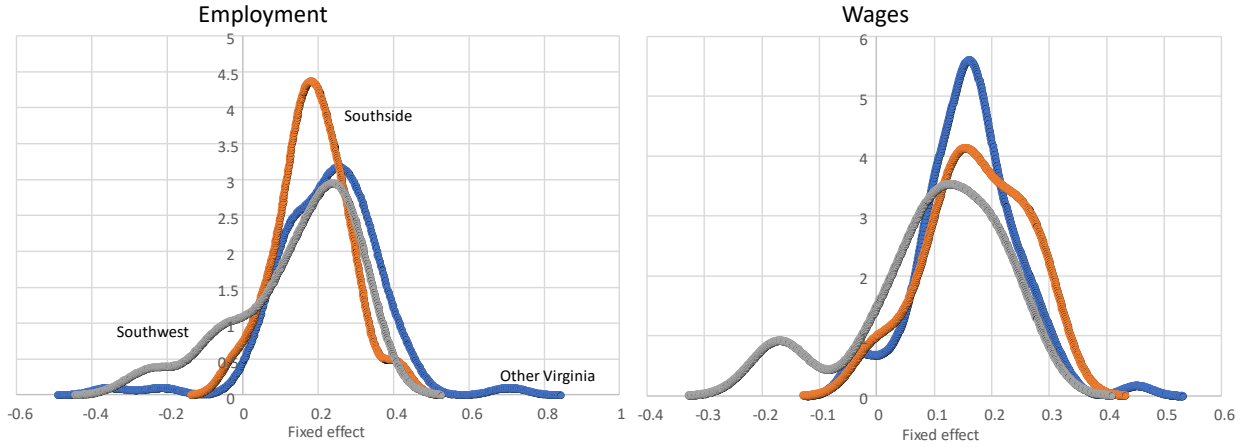


Table O-13 shows the results from equation (1). Columns 1 and 2 show the coefficients from the regression shown; columns 3 and 4 show the results from when it is instrumented for using historical

industry compositions in the area. The fixed effects estimates in column 1 and 2 show statistically significant results. Column 1 indicates that a 1 percent increase in TROF funds awarded, on average, results in a 2.6 percent increase in employment. Column 2 actually indicates that wages decreased in response to increases in TROF funds, though the estimate is very small, indicating that the average response to a 1 percent increase in TROF funds is a decrease in wages of 0.03 percent.

TABLE O-13
Linear regression and historical instrumental variable estimates for the effect of TROF funds completed

	Dependent variables			
	emp	**inc**	**emp**	**inc**
	(1)	(2)	(3)	(4)
TROF	0.026***	-0.003**	0.012	-0.005
	(0.007)	(0.002)	(0.025)	(0.005)
Observations	900	900	900	900
R2	0.639	0.49	0.637	0.489
First- stage F-stat			70.515	38.34

***p<0.01; **p<0.05; *p<0.1

The fixed effects results do not remain significant when instruments are used, however. This is partially because the instruments seem to introduce some noise, so the standard errors are larger, but both the point estimates of the coefficients are reduced too. So the fixed effect results could be overestimates. This indicates that, within the tobacco region, funds are disproportionately allocated to faster-growing areas.

Table O-14 shows the results when the presence of an enterprise zone in the locality is used as an instrument. The first two columns show simply using a dummy variable that takes the value 1 as the instrument and the final two columns show the coefficients when the procedure is carried out by estimating each regression in separate strata based on the propensity scores calculated for enterprise zone designation.

TABLE O-14
Enterprise zone instrumental variable estimates for the effect of TROF funds completed

	Dependent variables			
	emp	**inc**	**emp**	**inc**
	(1)	(2)	(3)	(4)
TROF	-0.004	-0.033**	0.008	0.019
	(0.025)	(0.014)	(0.176)	(0.020)
Observations	274	274	274	274
R^2	0.729	-0.999		

***p<0.01; **p<0.05; *p<0.1

Simply using an enterprise zone as a dummy variable results in substantially negative estimates for the effect of TROF funds on average wages (inc), and an insignificant decline in employment (emp). This could be due to two factors. First, though having an enterprise zone does bring more funds to an area, preference is given to projects with matches, and enterprise zones are given priority if they pay higher wages in the same way that TROF funds are; so the preference for a match could be outweighing the decline in the average wage for jobs funded by TROF in enterprise zones. The second is that enterprise zones are located in distressed economic areas to begin with, and that the instrument is correlated with some omitted variable that is actually dragging down the estimates. The final two columns show no statistically significant effect when the results designated are estimated in strata. That is, when compared to places that were equally likely to be as an enterprise zone, the additional funds that come from having another source for matches to TROF do not appear to any effect on wages.

Tables O-15 and O-16 show the estimates from equation (2), that regresses the growth rates from 2011–2018 on the total amount of TROF funds disbursed in those periods. In these regressions, the sample drops to only 40 observations, which limits statistical power and makes it less likely to obtain precise estimates. None of the estimates are significant, but they do indicate that places that received more TROF funds did, on average, grow more. These estimates are fairly small, indicating that a 1 percent increase in TROF funds is associated with increases in employment growth of 0.04 percent to 0.16 percent and increases in wage growth of between 0.05 percent to 0.07 percent.

TABLE O-15
Linear regression and historical instrumental variable estimates for effect of TROF funds

	Dependent variables			
	emp	**inc**	**emp**	**inc**
	(1)	(2)	(3)	(4)
TROF	0.004	0.005	0.006	0.006
	(0.004)	(0.004)	(0.009)	(0.010)
Observations	40	40	39	39
R^2	0.251	0.53	0.524	0.233
First stage F-stat			12.283	7.653

***p<0.01; **p<0.05; *p<0.1

TABLE O-16
Enterprise zone instrumental variable estimates for effect of TROF funds disbursed

	Dependent variables			
	emp	**inc**	**emp**	**inc**
	(1)	(2)	(3)	(4)
TROF	0.014	0.004	0.016	0.007
	(0.015)	(0.015)	(0.035)	(0.009)
Observations	39	39	39	39
R^2	0.489	0.267		
First stage F-stat	35.2	0.072		

***p<0.01; **p<0.05; *p<0.1

Though no prevalent effect of the Tobacco Commission or TROF is detected; there is insufficient evidence to prove either that it had no effect. The variations on the coefficients are such that large positive effects are possible, but a more conservative conclusion is appropriate here. TROF awarded $147 million over the period in the sample, and it seems more likely that the effects of such an amount spread over 40 counties cannot be clearly identified for a time frame of only 5–10 years. There are far too many factors affecting regional growth at the same time for a relatively modest program to have an effect that can be detected amongst the noise. Macroeconomic trends are likely to interact with the local conditions in the tobacco region. These effects are much larger in magnitude than the funds the Tobacco Commission can employ. TROF may or may not be an effective program and may have very localized effects that are not detectable at the county-level, but its effectiveness or ineffectiveness cannot be shown at the county-level without a significantly longer time series.

Estimation of "but for" effect of selected incentives

The "but for" effect of an incentive is the percentage of firm activity or growth that can be attributed to the incentive. Trying to determine this effect with precision is difficult. Site selection decisions are based on a variety of factors that affect businesses' operations and employees. While the importance of individual factors varies based on the requirements of each business and project, factors affecting long-term costs—such as transportation infrastructure and labor availability and costs—are typically most important. Incentives, if considered, often become more important toward the end of the site selection process, after a few sites meeting the fundamental business requirements have been selected. However, some businesses may rule out sites early on if incentives are not available. Site selection decisions are ultimately made by business executives whose motivations are hard to anticipate and impossible to verify after the fact. (See *Review of State Economic Development Incentive Grants*, JLARC 2012).

Weldon Cooper Center estimated the "but for" effect of seven Virginia incentives for this report:

- Economic Development Access Program (road access grant),
- Job Creation Grant,

- Rail Industrial Access Program (rail access grant),
- Real Property Investment Grant,
- Tobacco Region Megasite Grant,
- Tobacco Region Opportunity Fund, and the
- Virginia Business Ready Site Program.

The estimation relies on recent research by Bartik (2018) on the role of the relative intensity or size of the incentive relative to locating or expanding firm cost of operations in influencing company site decisions. The "but for" effect is the percentage of firm growth during the period that can be attributed to the incentive and is determined by a tax-elasticity-based formula. The intuition behind the formula is that smaller incentives relative to the firm's expanded or newly relocated operations are less likely to "tip the balance" in a firm's location decision than larger incentives. For instance, Bartik estimates that the recent Wisconsin Foxconn incentive deal (approximately $230,000 per job) reduces operating costs for the firm on a discounted basis over time by 30 percent. This 30 percent cost reduction would influence firm location and expansion decisions 97 percent of the time, on average. In contrast, an incentive that constitutes just .1 percent of the amount would affect only 1 percent of the location/expansion decisions.

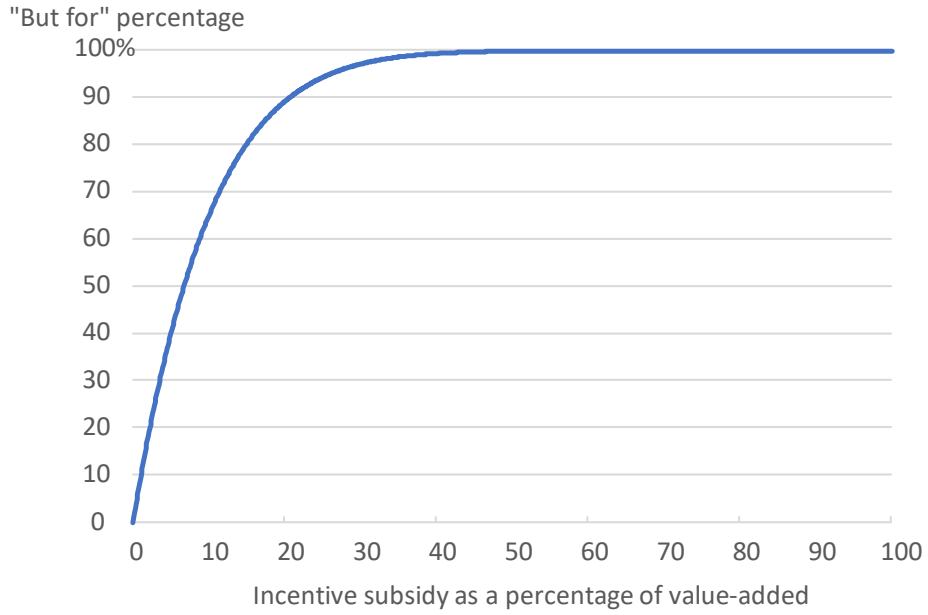
The formula (derivation which is explained in Appendix D of Bartik [2018]) is as follows:

$(E_a-E_b)/E_a=(1-(1-s)(-R)$

- E_a is the employment before the incentive,
- E_b is the employment after the incentive,
- R is the elasticity of long-run business activity for business costs (and assumed to be equivalent to -10 in line with business activity tax elasticities of -0.5 and the finding that business taxes represent about 5 percent of value-added or R=-.5/.05=-10), and
- s is the relative incentive size (i.e., present value of incentives as a proportion of present value of stream of company value added over the 20-year period).

For grants with job creation information, it was necessary to convert job creation into dollar values. This was done by computing the incentive award value as a percentage of the discounted stream of production costs for a 20-year project lifespan, using a 12 percent real discount rate as outlined by Bartik (2018). Production costs are proxied by value-added, which are payments made to capital and labor. Value-added per employee by industry was obtained from REMI and merged with the grant records using a REMI to NAICS bridge to compute value-added equivalents. The stream of value-added and incentives are discounted over time to determine the present value of costs and cost savings. Bartik recommends using 12 percent as the discount rate because it best represents the time value of money for private companies. Investment grant (Real Property Investment Grant) relative sizes were computed as the time value of an investment grant award as a percentage of total project capital investment spending to determine the same "but for" percentage. Figure O-4 illustrates the effect of the incentive subsidy as percentage of value-added on estimated "but for" percentage.

FIGURE O-4
The "but for" percentage increases with relative size of the incentive



SOURCE: Bartik (2018) *Who Benefits from Economic Development Incentives,* using state economic activity tax elasticity of -.5
NOTE: Program estimates based on actual and estimated employment and industry value-added per employee from REMI; actual and estimated incentive amounts; assumes discount rate of 12 percent.

The incentive share of project costs for the road access program, rail access program, TROF, and Job Creation Grant grants was estimated using actual and anticipated employment increase for each project over a 20-year time horizon. Value-added per employee over the period is estimated using information from REMI for the industry beneficiaries using NAICS codes from program records. Estimates for incentive share of project costs and the corresponding "but for" estimates are shown in Table O-17.

TABLE O-17
Estimates for incentive share of project costs and "but for" estimates

Incentive	Incentive share of project costs	"But for" estimate
Business Ready Sites Program	0.02%	0.1%
Economic Development Access Program (road access program)	1.34	12.6
Job Creation Grant	0.12	1.2
Megasite Grant	1.1	10.7
Rail Industrial Access Program (rail access program)	0.70	6.8
Real Property Investment Grant	n.a.	22.0
TROF	0.62	6.0

n.a., not applicable.

Analogously, the Real Property Investment Grant share of commercial and industrial non-residential capital investment is estimated to be 24.5 percent. The housing component of mixed use development (estimated at approximately 10 percent of total capital investment) is not counted as capital investment. So, only 22 percent of total capital investment is estimated to be attributable to the Real Property Investment Grant program.

The "but for" effect for the industrial park programs (Tobacco Commission Megasite Grant program and Virginia Business Ready Sites Program) was estimated by assuming that land would be provided to new or relocating establishments at reduced or no cost as an economic incentive. Interviewees in the tobacco region indicated that they commonly use such firm incentives to entice firms to locate and expand in their communities. Furthermore, the discounted land value can serve as a local match for Virginia economic development incentive programs (e.g., road access program, Commonwealth's Opportunity Fund) that require one. Based on funds attracted to megasite projects over FY08–FY18, the average spent in park development is approximately $39,500 per acre. The cost of park development was covered by the Tobacco Commission, localities, and other state, federal, and private sources. For analytical purposes, it is assumed that the industrial parks would not have been built without Tobacco Commission funding and that the entire incentive per acre (and ability to leverage other funding) was due to the program. This estimated size of the free land incentive is $14,545 per job. This figure was used to develop an estimate of the "but for" based on the Bartik methodology described further below which compares the size of the incentive to total project costs (i.e., value added of projected new employment in the state by industry). Based on these calculations, approximately 10.7 percent of the total employment creation was attributed to the program. This percentage of projected net new incentivized park employment was assigned to manufacturing and warehousing and storage industries based on their proportions of total state employment in these industries.

For the Virginia Business Ready Sites Program, only program funding and local match amount is counted toward the incentive costs. In contrast to the megasite grant, the Business Ready Sites Program accounts for only a very small share of park funding and is not assumed to be the catalyst for park completion but merely a facilitator. The estimated incentive per acre is $353 from the Virginia Economic Development Partnership and $353 from local sources. In addition, the land assembly is assumed to offer a $50 per search cost savings per acre value to firms (based on estimated cost savings to a firm of $50,000 for a 1,000-acre park). The average incentive per job is estimated at $279, which represents approximately 0.022 percent of total project costs. This equates to a "but for" estimate of 0.10 percent using the Bartik methodology.

Since the "but for" effect formula is based on firm reactions to business cost changes due to tax changes, it more typifies the likely firm response to a typical by-right tax cut rather than discretionary incentive. Ordinarily, greater discretion and agency due diligence might be expected to improve the likelihood of an incentive of a given size to move the needle by selecting only those projects most at risk of moving or expanding elsewhere rather than providing the incentive across the board. No adjustments were made for programs that had these elements; thus, they represent conservative "but for" assumptions.

Economic impact modeling

Weldon Cooper Center staff conducted economic impact analyses of Virginia economic incentives using REMI PI+ (Policy Insight Plus) software. REMI PI+ is a dynamic, multi-sector regional economic simulation model used for economic forecasting and measuring the impact of public policy changes on local economies. The model combines different contemporary regional economic modeling methods such as input-output analysis, econometric forecasting, and computable general equilibrium to characterize the mechanics and path of a regional economy. The model has been extensively peer-reviewed and is widely used by state agencies elsewhere in the nation to model economic and tax revenue impacts of economic development incentive programs, including economic development incentives. The model used for this analysis was customized for Virginia and includes 70 industry sectors. Outcome variables examined include total employment, state GDP, and personal income. In addition, a state tax revenue impact analysis was conducted based on a methodology described further below.

The modeling of each program was conducted differently depending on the type of economic stimulus provided by the program. Table O-18 describes the REMI modeling inputs by program using information on REMI modeling blocks and policy variables. Three basic approaches were used. When the only information for the program available was the effect of the program on firm costs, program cost savings (state revenue impacts) were modeled as reductions in firm production costs for the industries that were affected.

When information on program private employment and capital investment impacts are available from program documents and employment records, firm employment increases and capital investment expenditures were modeled. However, not all of the job creation or capital investment was attributed to the receipt of the incentive. Instead, the portion that could reasonably be attributed to the incentive based on its share of additional firm operational costs was estimated using the procedure described earlier. In the case of TPOF, employment goal information was available, and it appeared that employment levels were not maintained. This could not be verified from program or VEC employment payroll records. Thus, it was assumed that the grant did not support direct job creation for completed projects during the period.

When information on spending on public improvements such as roads and park infrastructure were available, the funds were represented as an increase of sales to the construction industry (or in the case of planning services, an increase in the sales of professional, scientific, and technical services). If information on local, other state, federal, or other funding was available, these funds were incorporated into the increased sales total.

For each economic impact analysis, the opportunity cost of state funds was accounted for by raising personal income taxes. Personal income taxes are the largest source of tax revenue for the general fund, and thus seemed appropriate as a source for offsetting the cost of the incentive programs.

REMI PI+ discontinued tax revenue estimation as part of its base package beginning with the 2.0 version and moved improved revenue modeling capabilities into its new REMI Tax PI model. To conduct tax revenue analysis, this study scaled revenues to economic outputs using the procedure described in Regional Economic Models, Inc. (2012). State tax revenues were derived from the Census of Government's *State and Local Government Finance* and *Annual Survey of State Tax Collections*. Revenue estimates are calculated by multiplying state revenue rates by the corresponding base quantity, which

included state-level demand for selected industries (general sales tax, selective sales tax, license taxes), state-level personal income less transfer payments (individual income tax), corporate income tax (gross domestic product), and personal income (other taxes). The tax revenue impact analysis does not include the effect of economic development incentives on other revenues, including non-general revenues. Nor does it estimate the effect on local tax revenues. Lastly, it does not estimate the effect of economic development incentives on government expenditures at the state or local level.

TABLE O-18
REMI policy variables

Name of incentive	REMI model policy variables	Modeling description	REMI industry
Economic Development Access Program (road access program)	(1) Output and Demand>-Investment Spending>-Non-residential. (2) Labor and Capital Demand>-Employment>-Firm >-Industry, (3) Output and Demand>-Industry Sales (Exogenous Production)>-Construction	Model capital investment for speculative projects as investment spending. Model job creation by industry for regular projects. Use 12.6% “but for” estimate. Account for EDAP financed portion of road investment as increase in construction sales.	Employment assigned to REMI industries based on NAICS codes of awarded employment for completed grants.
Rail Industrial Access Program (rail access program)	(1) Labor and Capital Demand>-Employment>-Firm >-Industry, (2) Output and Demand>-Industry Sales (Exogenous Production)>-Construction	Model employment by industry. Use 6.7% "but for" estimate. Account for program financed portion of transportation investment as increase in construction sales.	Employment assigned to REMI industries based on NAICS codes of awarded employment for completed grants.
Transportation Partnership Opportunity Fund	Output and Demand>-Industry Sales (Exogenous Production)>-Construction	Account for TPOF transportation investment as increase in construction sales.	
Virginia Business Ready Sites Program (VBRSP)	(1) Output and Demand>-Industry Sales (Exogenous Production)>Professional, scientific, and technical services (2) Labor and Capital Demand>-Employment>-Firm >-Industry	Model VBRSP grant and local match for soft costs as increase in sales of professional services. Model projected job creation after park opening as described elsewhere in the appendix.	Assign employment to manufacturing and warehousing and storage as described elsewhere in the appendix.

Name of incentive	REMI model policy variables	Modeling description	REMI industry
Tobacco Commission Megasite Grant	(1) Output and Demand>-Industry Sales (Exogenous Production)>Professional, scientific, and technical services and Construction (2) Labor and Capital Demand>-Employment>-Firm>-Industry,	Model grant and matched funds for park development as professional services and construction sales. Model projected job creation after park opening as described elsewhere in the appendix.	Assign projected job creation to manufacturing and warehousing and storage as described elsewhere in the appendix.
Real Property Improvement Grant	(1) Output and Demand>-Investment Spending>-Nonresidential.	Model completed capital investment as investment spending for nonresidential building. Use 22.0% "but for" estimate.	
Jobs Creation Grant (JCG) and Tobacco Region Opportunity Fund (TROF)	(1) Labor and Capital Demand>-Employment>-Firm>-Industry	Model job creation by industry based on 1.22% "but for" assumption for JCG and 6.01% for TROF.	Employment assigned to REMI industries based on NAICS codes of job creation for completed grants.
Coalfield Employment Enhancement Tax Credit	Compensation and Prices->Production Costs->Production Costs	Model economic impact based on reduced production cost equal to tax credit.	REMI industry "mining"
Virginia Coal Employment and Production Incentive Tax Credit			Distributed to utilities and paper manufacturing REMI industries based on tax credit use.