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Appendix K: Quasi experimental statistical analysis and economic impact modeling

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

Entropy balanced panel regression

Weldon Cooper Center staff estimated a series of staggered difference in differences models using entropy balance weights to assess the employment effects of the incentives in this report (except for the two tax credits).

The difference in differences method calculated the treatment effect (received incentive) for an outcome variable (employment) by comparing the change over time between treatment (businesses receiving incentives) and control groups (similar businesses that did not receive incentive) with respect to a pretreatment baseline period. Difference in differences can also be implemented for multiple time periods through a panel data fixed effects model. Staggered difference in differences accounts for the different timing of incentive awarding across the universe of firms receiving economic incentives and has been used in other policy effectiveness studies such as Stevenson and Wolfers (2006) and a recent study of North Carolina economic incentives by Lester, Lowe and Frey (2014). It is estimated here using a panel data fixed effects model that takes the following form:

$$(1)\ y_{it} = \beta TRT_{it} + \gamma_t + \tau_i + \epsilon_{it}$$

where *i* represents the establishment (1, . . ., N) and *t* is the year (t=1, . . .,10). The relevant parameter for assessing the policy treatment (TRT) impact is β. TRT_{it} is an indicator that equals one for each establishment i receiving an economic incentive in all $t>k_i$ periods. τ_i represents individual fixed effects parameters while γ_t are time fixed effects parameters. The individual fixed effects capture unobserved establishment heterogeneity that explains employment changes.

This fixed effects model was estimated with entropy balance weights. Entropy balance reweighting has been recommended in the policy literature for balancing the characteristics of treatment and control group observations so that their distributional characteristics (means and higher order moments if needed) are similar (Hainmuller 2012). Its motivation is similar to other policy treatment assessment methods, including weighting and control group selection methods implemented through propensity scoring and nearest available matching in selecting on observable characteristics. Neither the fixed effects panel model or entropy balance weights account for potential unobservable characteristics that might be correlated with both treatment and outcome and thereby hinder properly identifying the policy effect.

Entropy balance weighted regression has been used in recent policy applications, including a study of Appalachian Regional Commission community development spending on household sanitary conditions (Grossman, Humphreys and Ruseski 2017) and a study of the employment effects of a Kansas economic development incentive program (Jensen 2017a). Unlike more common weighting and control group selection methods implemented through propensity scoring and nearest available matching, it does not require the user to monitor and continuously recalibrate selection and matching models to achieve covariate balance. Entropy balancing generally also performs better than competing matching techniques in terms of certain statistical metrics of accuracy such as Mean Square Error and bias and reduces model dependency (Hainmuller 2012). Entropy weights (w_i) are estimated using an entropy balancing scheme that minimizes an entropy distance metric subject to balance constraints that cause treated and control covariates means (and higher order moments if needed) to be the equal and a constraint that the weights add up to one. The solution weights are found using a computational algorithm.

In addition to the entropy balance method, a propensity score matching method was used to construct control groups similar to the treatment group for double difference estimation. The control group was winnowed to those observations (i.e., establishments) that met the propensity score balancing property (i.e., region of common support). The technique did not appreciably change the statistical significance results (not reported here) for incentive effects but did result in a lower treatment effect estimates and worse overall model fit (within and total variation explained). Also, a more conventional difference in difference fixed effects model with a treatment uniformly commencing in 2010 without time fixed effects was also estimated, which tended to produce lower treatment effects.

Three Stata programs were used in the estimation process: (1) *xtreg*, which is the standard Stata procedure for implementing fixed effects regression; (2) *ebalance*, a user-written Stata package for entropy balancing (Hainmueller and Xu 2013), that was used to generate the entropy balanced regression weights, and (3) *pscore*, a user-written Stata package for propensity scoring that was used for control group selection in an alternative estimation procedure (Becker and Ichino 2002).

Two covariates were used for entropy balancing: the natural log of establishment employment (lemp) and estimated sales outside Virginia as a percentage of total output for the establishment's industry. Employment was used as a covariate to represent firm tendency to self-select into economic incentive programs based on firm employment size. Larger firms are more likely to take advantage of incentives because of their lower administrative costs and greater program awareness, a fact well documented in the incentive policy literature (Neumark 2013; Faulk 2002). It was also used because small business economic incentive programs impose size eligibility criteria, with an employment ceiling of 250 employees.

The export variable was used to account for industry-level differences in establishments and account for program selection in favor or export-base sectors. In particular, three programs here (VJIP, Small Business Jobs Grant Fund, and Economic Development Loan Fund) explicitly target export-base sectors. The percentage of firm output sold to out-of-state domestic and international export markets (pexport) was estimated based on joining establishment industry NAICS codes with an industry-level crosswalk to IMPLAN sector export sales and output estimates. Other economics incentive studies have used other selection variables collected through surveys and available through NETS such as firm age, branch plant indicators, and other firm-specific information. However, such detailed information was not available for this study.

The natural log of employment (*lemp*) was the outcome variable. The treatment variable indicates whether the establishment received an incentive or not during the treatment period 2010-2016. Only projects that had a legally binding agreement or contractual obligation that was signed or took effect between FY10 and FY16 were included in the treatment group. The FY16 cutoff date was imposed because annual employment data was available only for 2007-2016 and treatment effects for FY17 awards would not yet be observed. Since the award process works differently for each of the programs, incentive treatment timing was treated differently. For VJIP and the Small Business Jobs Grant, which provide reimbursement only after all performance obligations (i.e., employment, wages, and capital investment) have been met, the treatment year is considered the year of project payment or completion. For the small business loan and equity investment (Small Business Investment Grant) programs, which provide funds up-front, the treatment year corresponds to the year the award or loan was approved.

Table K-1 summarizes pertinent information from the weighted regressions by program. The middle portion of the table shows covariate means for the treated units by program compared to the unweighted control units and the entropy balanced weighted sample. For each program, the weighted control units are reweighted to look more like the treated units did in 2007. Regression results are reported only for the treatment effect. Results indicate that each of the programs has a statistically significant effect on employment for α=.05. However, with the more demanding standard of α<.0001 for large samples only four programs show evidence of program impacts (Loan Guaranty Program, Small Business Investment Grant, Small Business Jobs Grant, and VJIP).

TABLE K-1
Entropy balanced weighting fixed effects regression results by program

	EDLF	LGP	SCCP	SWMLF	SBIGF	SBJGF	VJIP
Regression results							
β	0.8915895	1.0183120	0.9878078	0.2291378	1.4954400	0.7380750	1.4407970
SE	(0.4412923)	(0.1517618)	(.3722698)	(.0910727)	(0.2897905)	(0.2035651)	(0.0907474)
T	2	7	2.7	2.52	5	4	15.9
p value	0.0430	<0.0001	0.0080	0.0120	<0.0001	<0.0001	<0.0001
Treated units	4	46	5	68	7	42	302
Control units	461,545	461,503	461,544	461,481	461,542	461,481	461,197
Time periods	10	10	10	10	10	10	10
Adjusted R2	0.40	0.69	0.72	0.70	0.57	0.71	0.74
Mean values from entropy balancing							
Treatment							
lemp	0.00	0.66	1.55	0.81	0.00	1.82	2.41
pexport	62.7	18.1	39.0	9.8	45.0	44.8	50.8
Control pre-balance							
lemp	0.81	0.81	0.81	0.81	0.81	0.81	0.81
pexport	16.1	16.1	16.1	16.1	16.1	16.1	16.1
Control post-balance							
lemp	0.01	0.66	1.55	0.81	0.00	1.82	2.41
pexport	62.7	18.0	39.0	9.8	45.0	44.7	50.8

SOURCE: Weldon Cooper Center.
NOTE: EDLF, Economic Development Loan Fund, LGP, Loan Guaranty Program, SCCP, State Cash Collateral Program, SWMLF, SWaM Business Microloan Fund, SBIGF, Small Business Investment Grant Fund, SBJGF, Small Business Jobs Grant Fund, VJIP, Virginia Jobs Investment Program.

The magnitudes of the treatment policy variables in all cases are relatively small (from one to five jobs) (Table K-2). This magnitude of estimated job creation is similar to other recent studies using similar statistical methods such as Jensen's (2017b) estimate of 0.37-1.66 jobs for Virginia's Commonwealth Opportunity Fund and the Maryland Economic Development Assistance Authority Fund. These job creation effect estimates are generally much smaller than program reported average job creation goals and completions, which range from 2.4 (SWaM Business Microloan) to 96 jobs (VJIP). As a result, using this technique the incentives generally are computed to have relatively high "deadweight" with an average incentive effect across all programs of 14.3 percent (ranging from a low of 48 percent percent for the SWaM Business Microloan to a high of 97 percent for the Economic Development Loan Fund).

TABLE K-2
Treatment effect and deadweight estimates by program (average per project)

	EDLF	LGP	SCCP	SWMLF	SBIGF	SBJGF	VJIP
Estimated impact of incentive on job creation	2.4	2.8	2.7	1.3	4.5	2.1	4.2
Program reported job creation	71.0	9.6	33.5	2.4	11.8	28.7	95.7
Estimated impact of incentive	3.4%	28.7%	8.0%	51.6%	37.8%	7.3%	4.4%
Deadweight	96.6%	71.3%	92.0%	48.4%	62.2%	92.7%	95.6%

SOURCE: Weldon Cooper Center.
NOTE: EDLF, Economic Development Loan Fund, LGP, Loan Guaranty Program, SCCP, State Cash Collateral Program, SWMLF, SWaM Business Microloan Fund, SBIGF, Small Business Investment Grant Fund, SBJGF, Small Business Jobs Grant Fund, VJIP, Virginia Jobs Investment Program.

In general, these results conform to expectations. The deadweight estimate 85.7 percent is close to the 85 percent to 90 percent deadweight consensus range commonly recommended for use in economic incentive evaluation (Bartik 2018; Peters and Fisher 2004) and slightly lower than the 90 percent value used in the 2012 JLARC report (*Review of State Economic Development Incentive Grants*, 2012, JLARC).

Inter-program deadweight variation also conforms to expectations. Less discretionary (or automatic) grant programs such as VJIP and the Small Business Jobs Grant are anticipated to have higher deadweight effects than more discretionary programs (such as loan programs) (Bartik 2005). Programs that discriminate in favor of firms that were less likely to undertake the economic activity without the assistance based on documented appraisals of project viability should have smaller deadweight effects. In the case of loan programs, there is substantial evidence that some categories of small firms face credit constraints. The bank and agency underwriting process requires careful examination of firm financial records and determination of their ability to repay the loan. Thus, the discretionary decision process and due diligence involved lead to more favorable (smaller) deadweight estimates. Their average deadweight effect for loan programs is computed here as 69 percent. In contrast, VJIP/Small Business Jobs Grant awards are largely nondiscretionary due to the fact that they are generally available to any industry-conforming firm meeting numerical targets of job creation and capital investment and have an estimated average deadweight of 95.2 percent. Small businesses are also likely to be more sensitive to economic development incentives than large firms. Employment tax elasticities reported for small firms have often been found to be higher than large firms (Borchers, Deskins, and Ross

2016), implying lower deadweight. Smaller firms are also likely to face operating liquidity constraints because they are less able to access internal resources and capital markets. Firm size and the level of program discretion have been found to be important determinants of economic incentive deadweight based on firm survey assessments of project viability (Lenihan 2004).

Synthetic control method analysis

Weldon Cooper Center staff conducted a synthetic control method analysis of Virginia's Telework Expenses Tax Credit. The method is a quasi-experimental case study method developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). The purpose of the analysis is to identify the change in telecommuting rates after the introduction of the tax credit program. The synthetic control method analysis compares a treatment unit (Virginia) affected by a particular policy (tax credit) to a synthetic control constructed from weighted units (other states) unaffected by the policy. The synthetic control group represents the counterfactual of what would have happened to the treated unit (Virginia) without the policy (tax credit).

Statistical analysis was conducted to develop the synthetic control group. The analysis used the percentage of private employees that worked at home as a measure of the telework use (TELECOMM), the outcome variable of interest. The predictor variables include

- the percentage of private non-farm employment in selected service industries (PSERVICES) (i.e., information, finance and insurance, real estate and rental and leasing, professional, scientific, and technical services, educational services, and health care and social assistance),
- the percentage of population 25 years and older with a college degree (PCOLLEGE), and
- mean commuting time (COMMTIME) in minutes.

These predictor variables have been identified as explanatory variables for telecommuting adoption in other research. Two lagged variables for telecommuting adoption (2005 and 2011) were also included to improve control group fit. This information was obtained from the U.S. Census Bureau and Bureau of Economic Analysis. The three states that had adopted telework tax incentives during the period (i.e., Virginia, Georgia, and Oregon) were identified from internet research. The data was input into the Stata analysis software to perform the analysis using the "synth_runner" procedure (Galiani and Quistorff 2016), which is a data-driven procedure for constructing a synthetic control unit and conducting diagnostic tests. The pre-treatment period, over which predictor variables are averaged, was 2001-2011. The treatment period, which represents the period when the incentive was in force, was 2012-2016.

The synth procedure selected a weighted average of Colorado, Hawaii, Maryland, Massachusetts, and New Hampshire as the synthetic control group (Table K-3). This group was constructed by selecting weights that minimize the mean squared prediction errors of the predictor variables during the pre-treatment period. The lower the mean squared prediction errors, the closer the "fit" of the synthetic control group to the treated unit (Virginia). The suitability of the synthetic group was evaluated by several diagnostics. Synthetic control predictor values for the pre-treatment period are always closer to Virginia values than all untreated states, as the method ensures (Table K-4).

TABLE K-3
Five out of 47 states that had not established telework incentives were selected for synthetic control group

Control state	Weights for synthetic control
Colorado	0.11
Hawaii	0.37
Maryland	0.23
Massachusetts	0.15
New Hampshire	0.14

SOURCE: Weldon Cooper Center.
NOTE: Weight reflects the proportion of the control group that is represented by that state. Forty-two of the states without incentives were excluded because they did not improve the fit of the control group.

TABLE K-4
Predictor variables of the synthetic control are closer to Virginia values than all untreated states prior to establishing the tax credit

Predictor variable	Virginia	Synthetic control	All untreated states
PSERVICE	38.05	38.31	34.88
PCOLLEGE	33.79	33.60	26.96
COMMTIME	27.15	27.12	23.28
TELECOMM(2005)	1.81	1.81	1.64
TELECOMM(2011)	2.92	2.92	2.49

SOURCE: Weldon Cooper Center analysis of Virginia Telework Expenses Tax Credit.

Informal statistical inference of the causal relationship occurs by conducting "placebo" comparisons and mean square prediction error tests. In the placebo comparisons, the units (states) eligible for the synthetic control are regarded as treatment units and synthetic controls constructed. The paths of the differences between the eligible control units and their corresponding synthetic controls are compared with the differences between the treatment unit (Virginia) and its synthetic control. If the difference between Virginia and its synthetic control is an outlier during the post-treatment period, this provides evidence that the difference is casual. The placebo test results (not shown) were not supportive of causative effect because Virginia was not an outlier during the treatment period. Secondly, mean square prediction error tests are conducted by calculating ratios of post-treatment period to pre-treatment period. A relatively high ratio for the treatment unit compared to the eligible control units provides another informal test of causal relationship. Ratios of post/pre mean square prediction errors indicate that the Virginia value is lower than most states, which provides additional evidence that the Virginia result is not causal.

Random effects panel regression

In order to determine the effect of state tax credits on apprenticeship training, a series of panel data regressions was performed using data from the 50 states and the District of Columbia for the years 2011-2017. The dependent variable (*APPRENT*) was the number of persons entering apprenticeship programs during the federal fiscal year (obtained from the Department of Labor, Employment and

Training Administration) divided by the civilian labor force for the corresponding calendar year (from the Bureau of Labor Statistics Local Area Unemployment Statistics). The panel estimates control for time-invariant characteristics of states that might increase the number of new apprentices.

The independent variables included the state unemployment rates (*UNEMP*) (from the Bureau of Labor Statistics Local Area Unemployment Statistics) to represent state labor market conditions with increases in the unemployment rate hypothesized to be associated with fewer new entrants into apprenticeship training. Unionization levels of the private workforce (*PUNION*) was hypothesized to be associated with greater apprenticeship activity because unions have traditionally used apprenticeship as part of collective bargaining agreements to advance wage standards and obtain more secure employment. This variable was obtained from the Unionstats.com website and is based on computations using U.S. Census Current Population Survey microdata (Hirsch and Macpherson 2003).

Two state policy variables were used, dummy variables representing the presence of state tax credits (*TAXCRED*) and presence of state prevailing wage policies (*PREVWG*). Eleven states offered apprenticeship tax credits over the period, of which three (Alabama in 2016 and Maryland and Montana in 2017) introduced them during the period and eight had offered them before 2011. Thirty-three states had prevailing wage laws in 2011 but only 29 had them in 2017. Information on the availability and timing of the tax credits was assembled from information from the United States Department of Labor, including a list of states which currently offer tax credits to employers for hiring apprentices and list of states that have prevailing wage laws and list of those that have repealed them by year.

Two regressions were performed: a fixed effects panel model using the variables listed above and a random effects panel model using the same variables. The random effects model assumes that the individual effect is a random variable and is the preferred model if the intercept is not correlated with the explanatory variables. Both models used cluster-robust standard errors on states to permit error correlation within states across years and reduce the possibility of making false inferences of statistical significance. A robust Hausman test was run using the user written Stata command *xtoverid* to determine if the individual effects are random and choose between the two models (Schaffer and Stillman 2006). The test fails to reject the null hypothesis that the random effects is the appropriate model with a p value of .1541. Thus, only the random effects model results are shown in Table K-5 below.

TABLE K-5
Random effects panel regression of new apprentices as percentage of the labor force

Variable	Coefficient	Standard error	Z	P>|z|
URRATE	−.0126733	.0044015	−2.88	0.00
PUNION	.0042068	.0043943	0.96	0.34
TAXCRED	.0253564	.020153	1.26	0.21
PREVWG	.0384177	.0201469	1.91	0.06
Constant	.1355071	.0265874	5.10	0.00
Clusters	51			
Observations	352			
R2	0.07			

SOURCE: Weldon Cooper Center analysis of state apprenticeship data.

The results indicate that apprenticeship training is sensitive to state unemployment rates, with the relative number of apprentices increasing when unemployment rates decrease. Also, state prevailing wage policies are associated with a higher new apprentice training rates. The coefficient for state apprenticeship tax credits is positive but not statistically significant at the α=.10 level. The model explains only a small amount of variation in the data as indicated by the R^2 value of just .07.

It is possible that the state tax credits were adopted in response to low levels of new apprentices. If so, the coefficient on the tax credit variable would be biased downward. To test whether state tax credits are endogenous, the regression model was re-estimated with the state tax credit as the dependent variable and lags of various lengths (1, 2, and 3) of the new apprenticeship variables used as an independent variable along with the previously used independent variables. These regressions did not show that the lagged *APPRET* variables were statistically significant. Thus, we conclude that endogeneity may not seriously affect the results.

Economic impact modeling

Weldon Cooper Center staff conducted ex-post 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. The model used for this analysis was customized for the state of 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. Two estimates were calculated: (a) very conservative low-end estimates based on REMI policy capital cost, production cost, and spending policy variables and (b) high-end estimates created by varying deadweight employment calculations based on econometric estimates of program employment effect. The low-end estimate generally assumes that the financial assistance affects firm economic behavior like a business tax decrease. The high-end estimate generally assumes that the incentive induced firms to change their employment activity by a considerably larger amount because it influenced firm decisions at the margin to expand or relocate in Virginia.

The high-end estimates are the only estimates reflected in the main report, but both the low-end and high-end estimates are shown in Appendix K. High-end estimates are shown because, with the exception of the Small Business Investment Grant where there is not enough history to accurately evaluate deadweight effects, we view the higher estimates as the most likely scenario. The high-end estimate for the Small Business Investment Grant is also used in the body of the report to be consistent with the other incentives, even though the estimate is less reliable.

In no instance are estimated program employment levels equal to program reported job creation. In the case of the SWaM Business Microloan Fund, approximately half of stated job creation goals were related to the program, while only 3.2 percent of Economic Development Loan Fund job creation goals were related to the program. Employment effects could not be estimated for the Telework Expenses Tax Credit and the Worker Retraining Tax Credit programs. These programs serve mainly to reduce training and equipment costs of employers; employment creation is not the goal of the programs and no agency employment performance metrics are collected. Moreover, econometric estimates of employment impacts could not be made because of tax credit data limitations and preponderance of pass-through entities rather than FEIN-identifiable business entities receiving the credits.

Table K-6 describes the REMI modeling inputs by program using information on REMI modeling blocks and policy variables and how low-end and high-end scenarios were mapped.

TABLE K-6
REMI policy variables and modeling description by incentive

Name of incentive	REMI Model policy variables	Modeling description
Telework Expenses Tax Credit	(1) Compensation and Prices->Production Costs->Capital Costs, (2) Output and Demand>-Industry Sales (Exogenous Production)>-Computer and electronic products	Assign REMI industry based on NAICS industry information available from associating tax credit utilization files with VEC ES202 establishment payroll records that contain NAICS codes. Model low economic impact estimate based on reduced capital cost equal to tax credit amount (1). Model high economic impact on (1) and purchase of computer and electronic products (2) equal to tax credit amount.
Worker Retraining Tax Credit	(1) Compensation and Prices->Production Costs->Production Costs, (2) Output and Demand>-Industry Sales (Exogenous Production)>-Educational services	Assign REMI industry based on NAICS industry information available from associating tax credit utilization files with VEC ES202 establishment payroll records that contain NAICS codes. Model low economic impact estimate based on reduced capital cost equal to tax credit amount (1). Model high economic impact on (1) and purchase of educational services (2) equal to tax credit amount.
Small Business Investment Grant Fund	Low: Compensation and Prices->Production Costs->Capital Costs, High: Labor and Capital Demand>-Employment>-Firm >-Industry	Model low economic impact estimate based on reduced capital cost. Model high economic impact estimate based on econometric deadweight analysis and employment increase.
Small Business Jobs Grant Fund	Low: Compensation and Prices->Production Costs->Production Costs; High:(1) Labor and Capital Demand>-Employment>-Firm >-Industry; (2) Output and Demand>-Real Disposable Income>-Compensation (Adjust compensation by difference from industry average compensation)	Model low economic impact estimate based on reduced production cost. Model high economic impact estimate based on econometric deadweight analysis and employment increase.
Virginia Jobs Investment Program (VJIP)--Job Creation	Low: Compensation and Prices->Production Costs->Production Costs;	Model low economic impact estimate based on reduced production cost. Model high economic

Name of incentive	REMI Model policy variables	Modeling description
	High: (1) Labor and Capital Demand>-Employment>-Firm >-Industry; (2) Output and Demand>-Real Disposable Income>-Compensation (Adjust compensation by difference from industry average compensation).	impact estimate based on econometric deadweight analysis and employment increase.
Virginia Jobs Investment Program (VJIP)--Training	Low: Compensation and Prices->Production Costs->Production Costs; High: (1) Output and Demand>-Industry Sales (Exogenous Production)>-Educational services; (2) Output and Demand>-Investment Spending>-Equipment, (3) Output and Demand>-Real Disposable Income>-Compensation (Adjust compensation by amount of training related wage increase). (4) Output and Demand>-Output (Adjust by ratio of value-added to training related wage increase)	Model low economic impact estimate based on reduced production cost. Model high economic impact estimate based on econometric deadweight results and 1.5% wage and salary increase with associated output increase. Also, model educational services purchases and capital investment.
Economic Development Loan Fund	Low: Output and Demand>-Industry Sales (Exogenous Production)>-Investment Spending)>-Equipment; High: Labor and Capital Demand>-Employment>-Firm >-Industry	Model low economic impact estimate of loan activity as investment spending only. Model high economic impact estimate based on econometric deadweight analysis and employment increase.
Loan Guarantee Program	Low: Output and Demand>-Industry Sales (Exogenous Production)>-Investment Spending)>-Equipment; High: Labor and Capital Demand>-Employment>-Firm >-Industry	Model low economic impact estimate of loan activity as investment spending only. Model high economic impact estimate based on econometric deadweight analysis and employment increase.
State Cash Collateral Program	Low: Output and Demand>-Industry Sales (Exogenous Production)>-Investment Spending)>-Equipment; High: Labor and Capital Demand>-Employment>-Firm >-Industry	Model low economic impact estimate of loan activity as investment spending only. Model high economic impact estimate based on econometric deadweight analysis and employment increase.
SWaM Business Microloan Fund	Low: Output and Demand>-Industry Sales (Exogenous Production)>-Investment Spending)>-Equipment; High: Labor and Capital Demand>-Employment>-Firm >-Industry	Model low economic impact estimate of loan activity as investment spending only. Model high economic impact estimate based on econometric deadweight analysis and employment increase.

SOURCE: Weldon Cooper Center.

When modeling firm employment increases, the REMI firm employment option is used. This option assumes that firm sales associated with incentivized job creation may displace to various degrees the sales of other Virginia-based firms in the same industry based on industry market-area characteristics.

In order to generate input data to capture the effect of firm retraining, data for wages and salaries and value-added by industry were obtained from the Virginia REMI PI+ model. Ratios of value-added per wages were formed for each REMI industry. These ratios were then multiplied by the estimated wage increases (1.5 percent) that resulted from firm retraining for the 4.4 percent of firms that were assumed to have been incentivized by the VJIP retraining grant. The results by year were assigned to the REMI policy variables "Industry Sales/Exogenous Production" by REMI industry. Estimated wage increases were assigned to the compensation policy variable. This method is similar to that used to capture wage/productivity increases of degree completion in study of Virginia public higher education study (Rephann 2017).

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. In order 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.