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The Missouri Quality Jobs Program: Rearranging the Deck Chairs (And Throwing Some Overboard)

By Howard J. Wall

EXECUTIVE SUMMARY

According to the Missouri Department of Economic Development (DED), the Missouri Quality Jobs Program (MQJP) will create 118 new jobs by 2020 for each \$1 million dollars in tax credits awarded under the program. The claimed sources of these job gains are the direct increase in employment at the firms receiving the credits, and indirect increases at other firms due to spinoff and multiplier effects. Unfortunately, the DED's estimates for these effects are based more on faith than on evidence. First, the DED rather naively assumes that all of the job gains at the firms receiving tax credits occur only because of the credits. Second, the DED's projections of spinoff and multiplier effects are generated with a forecasting model that is incapable of an accurate accounting of negative substitution effects, such as the fact that many of the new jobs will be filled by people who are already employed. This paper summarizes new estimates of the employment effects of the MQJP using the actual, rather than the assumed, experience of local economies. What these estimates show is that after an initial net increase in employment following the authorization of tax credits, the net effect on employment becomes negative by the second year after authorization: Job gains in the county receiving the tax credits simply came at the expense of neighboring counties, which tend to

lose more jobs than a recipient county gains. Finally, by the fourth year after authorization, the only statistically significant effects of the tax credits are job losses in neighboring counties.

1. INTRODUCTION

This paper examines the effectiveness the Missouri Quality Jobs Program (MQJP), the declared purpose of which is to “(f)acilitate the creation of quality jobs by targeted business projects” by awarding tax credits in support of qualifying projects.¹ Tax credit programs such as the MQJP are quite common around the country and are touted by state economic development agencies as important components of their development efforts. Nonetheless, there is little evidence that targeted tax credits and similar policies are effective in spurring economic development and employment.² In fact, one recent study of employment tax credits in Michigan found that the state's MEGA tax credits were sometimes responsible for *losses* in overall employment.³

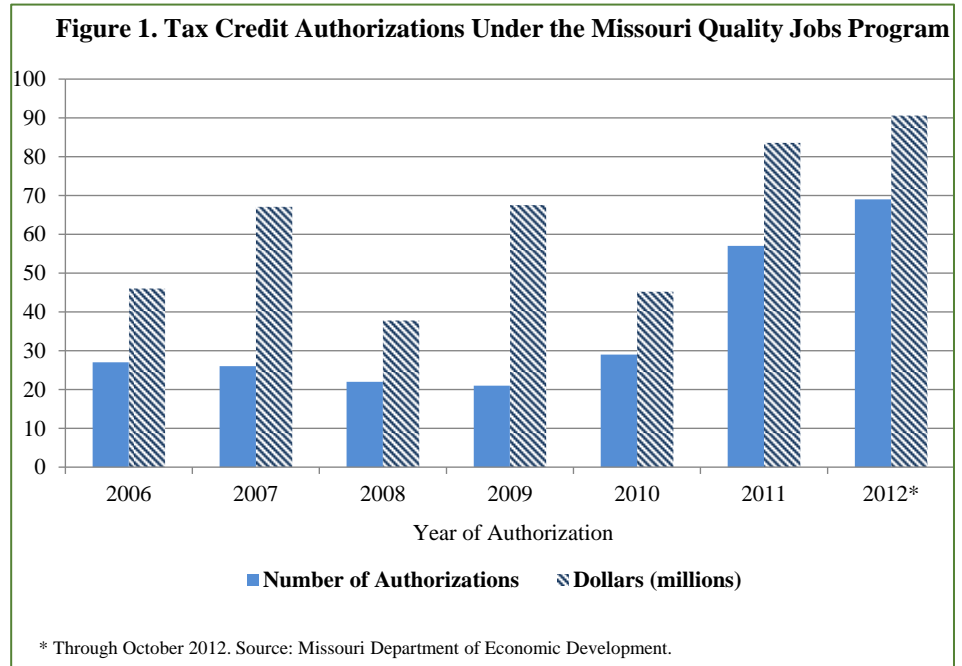
For development tax credits to work there must be some market failures, such as imperfect capital markets or agglomeration economies, that create a gap between the actual and efficient levels of local employment. If there are such market failures, the argument goes, then there might be room for a properly structured program that would use state money to direct resources to close the

employment gaps. Broadly speaking, therefore, if a tax credit program fails to deliver on promised jobs, it was either because market failures were not significant drags on employment or because the program was not structured properly. On the heels of the aforementioned history of failure of these programs, significant improvements have been made in how they are administered.⁴ Most notably, recent incarnations of state tax credit programs are designed with much greater accountability to ensure a closer link between promised and realized new jobs at firms receiving the tax credits.

In many respects, the MQJP has been ahead of the curve in terms of accountability in that it includes provisions for cancelling tax credits in the event that job-creation thresholds are not met, which it did for 33 projects in 2012.⁵ In addition, despite the extremely weak national economy following the launch of the MQJP, Missouri has so far maintained program accountability, thereby bucking the tendency for governments to erode accountability during difficult economic times.⁶ Given its relatively sound structure, therefore, the success or failure of the MQJP in delivering on employment creation is likely attributable to the extent to which it is based on solid economic efficiency grounds rather than on the soundness of its administration.⁷

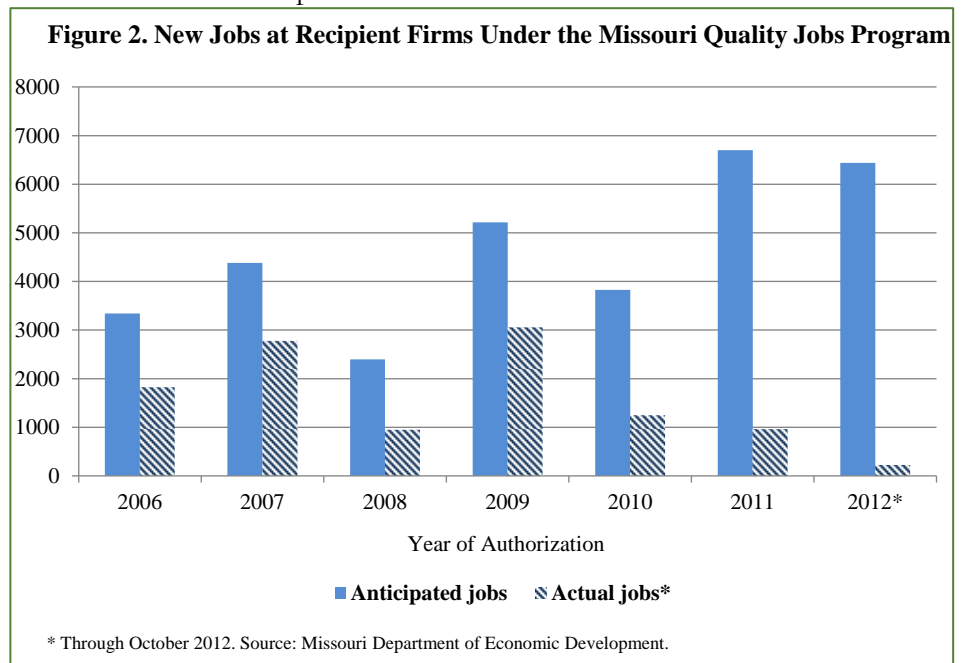
2. THE PROGRAM AND ITS PROMISES

Tax credits have been awarded under the MQJP since 2006 and are distributed under three business sub-categories—small/expanding, technology, and high-impact—each with its own set of eligibility criteria and program benefits. By 2012, the number and total value of tax-credit



authorizations were both more than double their 2006 levels, although this trend was interrupted a great deal by the national recession of 2008-09 (Figure 1).⁸ The increase in the anticipated number of new jobs at recipient firms roughly doubled between 2006 and 2012, although, as shown in Figure 2, the number of actual new jobs is, so far, well short of what had been anticipated when

the credits were authorized. Obviously, the lag between the date of authorization and the actualization of new jobs accounts for most of the shortfall for 2010-12, but even credits authorized in 2006-08 have fallen well short of their promise. Perhaps the credits from those years would look more successful if it weren't for the recession of 2008-09.



The most recent claims made by the Missouri Department of Economic Development (DED) about the direct effects (new jobs at firms that were awarded tax credits) and indirect effects (spinoff and multiplier effects) of the MQJP are contained in the program's 2012 annual report.⁹ At the end of 2012, there were 220 active supported projects, 73 of which were newly authorized in 2012. The DED claims that projects authorized through 2011 were directly responsible for 10,137 actual new jobs by the end of 2012—with more to come as the projects progress—and that the 73 new projects are anticipated to directly generate another 7,054 new jobs in five years time. After plugging their estimates of direct job growth into their forecasting model, DED arrives at the claim that the tax credits awarded through 2012 will have created 50,096 jobs (directly and indirectly) by 2020, or 118 jobs for each million dollars in tax credits. There are a number of reasons to doubt the DED's claims about the effects of the MQJP. With regard to direct job creation, the DED is being naïve, or perhaps narcissistic, in assuming that every new job supported by the program exists only because of the program and that the eventual number of direct jobs created is the same as the number claimed when the tax credits were authorized. These assumptions fly in the face of logic and the evidence for similar programs.¹⁰ Perhaps even more absurd is how the DED presumes that none of the new jobs are filled by workers who were already employed elsewhere in Missouri.¹¹

As for the broader indirect effects, the DED relies on the belief that the reshuffling of employment that occurs between subsidized and unsubsidized firms must be greatly

outweighed by large spinoff and multiplier effects. This belief is embedded into the DED's Regional Economic Models, Inc. (REMI) forecasting model which, despite a veneer of quantitative detachment, is simply a mathematical specification of the DED's prior beliefs about how the economy works.¹² More precisely, the primary sources of the indirect gains predicted by the REMI forecasting model are illusive multiplier effects that are believed to dominate the substitution effects across firms and communities.¹³ This notion is, to say the least, extremely controversial among economists in that regional forecasting models are afflicted with many of the same problems as the outdated national forecasting models from the 1960s and 1970s they are based on.¹⁴

To illustrate the difficulty, if not the impossibility, of modeling the indirect effects of tax credits, consider a firm that receives a \$1 million credit to support a new factory that will eventually employ 50 workers. Even if we accept that all 50 of the jobs at the recipient firm would not have existed without the tax credit, it's not possible say anything useful without knowing where the workers came from to fill the new jobs. Unless they all came from the ranks of the nonemployed or from out of state, some of the 50 new jobs are simply substitutes for jobs that already existed. If the jobs were simply shifted from other Missouri employers, then it is necessary to know what happened to those firms. Because the subsidy to one employer makes it difficult for unsubsidized employers to compete for local workers, these unsubsidized firms might downsize, shut down, or relocate, thereby further eroding the alleged direct job gains.

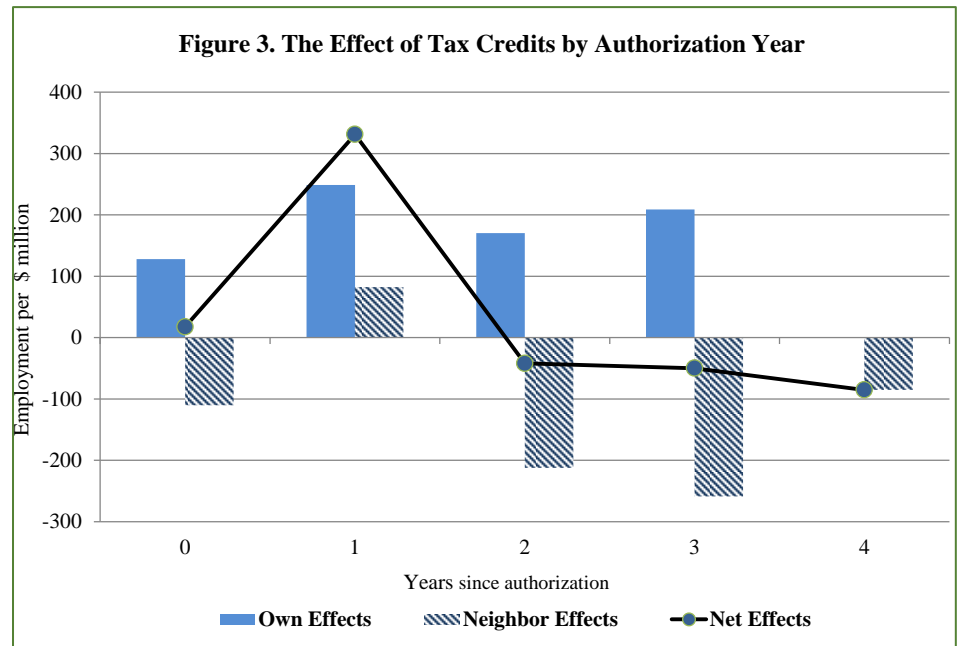
These substitution effects are not captured very well, if at all, by the DED's forecasting model. According to the DED's model, however, these unknown and unaccounted for substitution effects will be more than offset by spinoff and multiplier effects. Fortunately, it is no longer necessary to rely on the DED's claims about the current and future effects of the program because the MQJP has been in place for several years. It is, therefore, possible to compare actual employment outcomes in Missouri against those promised by the MQJP.

3. EMPIRICAL ESTIMATES

As a practical matter, it is not possible to trace the various employment effects of a tax credit authorization back their source, so it is necessary to instead look at aggregate employment. Therefore, I used data on county-level private employment for Missouri counties for 1998-2011, with the objective of identifying statistical patterns between levels of employment and the amount of tax credits received by firms in the counties.¹⁵ To detect these patterns, I estimated baseline levels of employment, controlling for the business cycle and county-level trends. Any deviations from these baselines that are related to the receipt of MQJP tax credits might then be attributed to the program. County economies do not operate in isolation, so I also looked for the effects that a county might experience because firms in neighboring counties received tax credits, and whether a county is in a broader metropolitan area. Note that my estimates are of the net effects of tax credits and do not distinguish between direct, indirect, spinoff and multiplier effects.

My estimation results indicate that tax credits had positive and statistically significant effects on employment in counties whose firms received tax credits under the MQJP. These effects were significant only through the third year after the tax credits were authorized, however, and were typically offset by negative and statistically significant effects on neighboring counties. The picture is complicated somewhat when looking at counties within metro areas because these counties' labor markets are closely integrated. As a result of this integration, the short-run employment gains from tax credits can also be felt by neighboring counties, although the negative longer-run effects on neighbors are amplified.

Figure 3 illustrates the average five-year effects of tax credits under the MQJP. These effects were obtained by applying the estimated effects described above to the actual allocation of tax credits across Missouri counties.¹⁶ In the figure, the solid bars are the employment effects on counties whose firms received the credits, the dashed bars are the effects on the receiving counties' neighbors, and the solid line is the net effect. Each of these is measured as the average effect of \$1 million dollars in tax credits. According to Figure 3, tax credits led to a net increase in state employment only during the year of authorization and the following year. Specifically, in the year of authorization, tax credits led to 128 more jobs per \$1 million in the recipient counties, but a loss of 110 jobs per \$1 million in neighboring counties. In the year following authorization, recipient counties and their neighbors both tended to see increased employment: 249 and 82 jobs per \$1 million, respectively. Beyond this initial start-up period,



however, average job gains in receiving counties were more than offset by job losses in neighboring counties; the net effects were losses of 42 and 50 jobs per \$1 million in tax credits during the second and third years after authorization. By the fourth year after authorization, there were no statistically significant effects on the recipient counties' employment, but neighbors tended to have lost 85 jobs per \$1 million in tax credits.

4. CONCLUSIONS

The MQJP has been in place long enough to obtain statistical evidence of its effects on the communities with firms receiving tax credits under the program. In the short run—the first two years—tax credits are associated with job gains in the recipient county and its neighbors. Over the medium run (the next two years), however, the recipient county gains employment only at the expense of its neighbors, and there is a net loss of jobs. At the beginning of the long run—the fourth year after authorization—there are no longer any significant job gains in the recipient county, but

the market distortions created by the tax credits mean that there are still significant job losses in neighboring counties.

It's not possible given the data available to estimate what happens beyond this early stage of the long run, but it is difficult to imagine that the trend reverses itself to result in anything close to the DED's projection of 118 new jobs per million dollars of tax credits. The more likely best-case scenario is that the employment distortions eventually work themselves out and the net effect of the tax credits approximates zero.

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TECHNICAL APPENDIX

EMPIRICAL MODEL

My data set uses annual private employment for 1998-2011 for all counties in Missouri from the Census Bureau's County Business Patterns. It is a balanced panel of 114 counties in which the independent city of St. Louis is included as a county and Worth County is excluded because of missing data. Tax credit data are converted to 2011 dollars using the CPI deflator from the Bureau of Labor Statistics and include all tax credits that were authorized, including those that were subsequently canceled for failing to meet the program's performance criteria.

My primary interest is in deviations from baseline employment that are due to the effects of tax credits, which can affect employment over many years and have different effects over time. To account for this, the specification allows for employment in a given year to be related to the value of the tax credit awarded during that year and during each of the previous five years (which, given the data set, is the maximum lag). Analogous variables are also included to capture the effects of tax credits received by neighboring counties. To eliminate the effects of fluctuations in employment due to the business cycle, my dependent variable is county-level shares of state employment. To control for changes in county employment shares that are unrelated to the MQJP, I assume that each county has its own quadratic trend. Note that my estimates look at the net effects of tax credits and do not distinguish between direct, indirect, spillover, or multiplier effects.

The effectiveness of tax credits might differ a great deal on the extent to which the labor markets in neighboring counties are integrated with one another. Many Missouri counties are part of larger economically integrated entities: 17 are in one of two large metropolitan statistical areas (MSAs), another 17 are in one of six small MSAs, and ten are in micropolitan areas (μ SAs) with more than one county. Counties within these entities are, by definition, economically integrated, so I account for the possibility that tax credits have different effects on them than on nonmetro counties. Finally, the specification also accounts for border effects to control for the fact that Missouri's two large MSAs contain substantial areas in other states and that 46 of Missouri's counties are on the state border.

Given the considerations outlined above, I specify E_{it} , county i 's relative employment at time t a

$$E_{it} = \alpha_{0i} + \beta_{1i}t + \beta_{2i}t^2 + \sum_{j=0}^5 (\theta_j + \omega_j S_i + \lambda_j L_i + \kappa_j B_i) C_{it-j} + \sum_{j=0}^5 (\gamma_j + \nu_j S_i + \delta_j L_i + \pi_j B_i) N_{it-j} + \mu_{it}. \quad (1)$$

In (1), C_{it} is the real amount of tax credits authorized for firms in county i in year t and N_{it} is the corresponding measure for county i 's neighbors. To differentiate the effects of tax credits across types of counties, there are three interaction dummies for each of C_{it} and N_{it} : S_i equals one if the county is in a small MSA or a μ SA composed of more than one county, L_i equals one if the county is in one of the state's two large MSAs, and B_i equals one if the county borders another state. The possible lags in the effects of tax

credits are captured by including the levels of tax credits over a six-year period, with the year of authorization denoted as $j = 0$. Because (1) includes lags in the effects of tax credits, and because of the wide disparity in employment shares across counties, the estimation allows for autocorrelated errors and is corrected for heteroskedasticity, respectively. To obtain estimates with these corrections, I estimate expression (1) using Feasible Generalized Least Squares.

ESTIMATION RESULTS

The estimation results for the unrestricted version of equation (1) are provided in Table 1, which, for space considerations, does not include the estimates of the 228 county-level trend coefficients or the 114 county-specific intercepts. A set of alternative results under various restrictions on the interaction terms in (1) are provided in the appendix. It should be noted at the outset that all of the estimated effects for the fifth year after authorization are based on very few observations: Only nine counties received tax credits in 2006, so there are only nine observations of the effects of tax credits in the fifth year after authorization. For the most part, therefore, these estimates can be safely ignored, although they need to be obtained to guard against estimation bias.

The effects in the first column of Table 1 are for a baseline county that is not in a metro area and does not border another state. For these counties, tax credits have positive and statistically significant effects on employment in the year of authorization through the third year after authorization. In subsequent years, however, the effects are statistically no different from zero.

Table 1. Base Estimation: Dependent Variable = County Relative Employment

	Non-Metro	Small Metro	Large Metro	Border
<i>Effects of Own Credits</i>	$\theta_0, \dots, \theta_5$	$\omega_0, \dots, \omega_5$	$\lambda_0, \dots, \lambda_5$	$\varkappa_0, \dots, \varkappa_5$
Authorization year	0.0046 * (0.0020)	-0.0082 (0.0068)	-0.0017 (0.0053)	-0.0056 (0.0036)
First year after authorization	0.0090 * (0.0028)	0.0049 (0.0104)	-0.0091 (0.0068)	-0.0047 (0.0047)
Second year after	0.0062 † (0.0034)	0.0011 (0.0139)	-0.0048 (0.0070)	-0.0036 (0.0058)
Third year after	0.0099 * (0.0043)	-0.0259 † (0.0157)	0.0008 (0.0087)	-0.0114 (0.0073)
Fourth year after	0.0050 (0.0042)	-0.0181 (0.0160)	-0.0011 (0.0098)	-0.0078 (0.0088)
Fifth year after	-0.0014 (0.0659)	-0.1152 (0.1371)	-0.0005 (0.0655)	0.0165 (0.0166)
<i>Effects of Neighbors' Credits</i>	$\gamma_0, \dots, \gamma_5$	$\delta_0, \dots, \delta_5$	$\upsilon_0, \dots, \upsilon_5$	π_0, \dots, π_5
Authorization year	-0.0013 * (0.0003)	0.0024* (0.0007)	0.0000 (0.0010)	0.0020 (0.0017)
First year after authorization	-0.0023 * (0.0004)	0.0017† (0.0010)	0.0030 * (0.0011)	0.0044 * (0.0021)
Second year after	-0.0022 * (0.0005)	-0.0034* (0.0014)	0.0001 (0.0018)	0.0016 (0.0025)
Third year after	-0.0023 * (0.0006)	-0.0065* (0.0017)	0.0027 (0.0022)	0.0026 (0.0030)
Fourth year after	-0.0002 (0.0006)	-0.0088* (0.0017)	-0.0023 (0.0033)	-0.0015 (0.0041)
Fifth year after	-0.0029 (0.0018)	0.0320* (0.0103)	0.0063 (0.0049)	-0.0027 (0.0062)

The estimation includes county fixed effects and county-specific quadratic trends, which are not reported here. Estimation is performed using Feasible Generalized Least Squares with corrections for heteroskedasticity and autocorrelation. Statistical significance at the 5 percent and 10 percent levels are indicated by “*” and “†”, respectively. Data are annual for 114 Missouri counties covering 1998-2011.

In contrast, employment in non-metro counties tends to be reduced by the tax credits received by neighboring counties. The neighbor effects for the fourth and fifth year after authorization are not statistically significant, however.

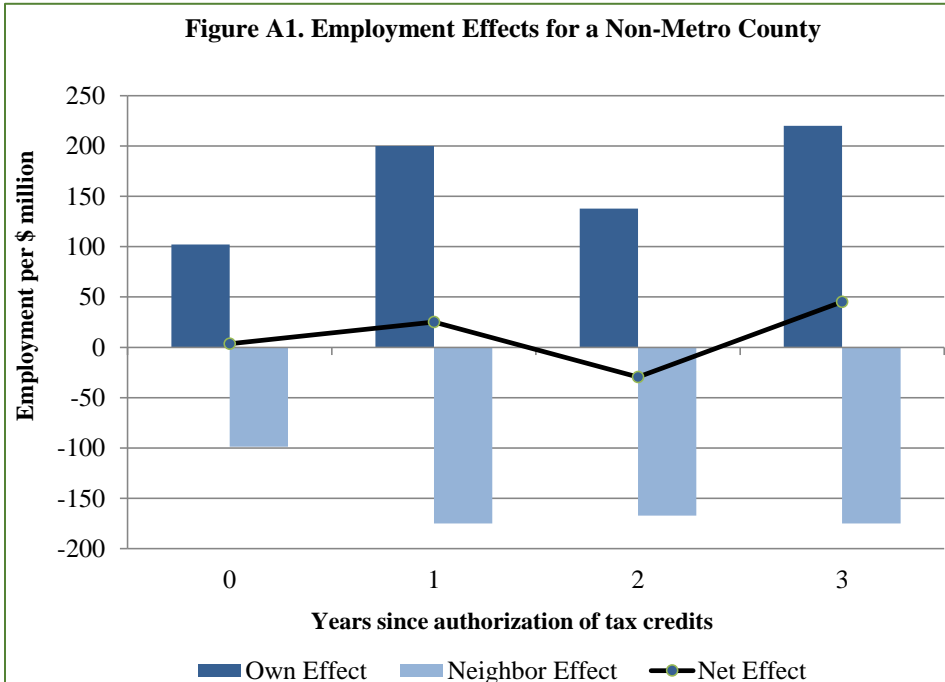
Figure A1 illustrates the effects of one million dollars in tax credits on a non-metro county and its neighbors. To calculate the effects illustrated by Figure A1, recall that county employment is measured as a fraction of state employment and note that over the period 2006-11 a percentage point of employment was, on average, 22,232 jobs. The

marginal effect of one million dollars of tax credits is, therefore, 22,232 times the estimated coefficient for the relevant variable. Also note that the neighbor effects reported in Table 1 indicate the marginal effects of neighbors' tax credits on only one county, so the full effect on neighbors is the relevant coefficient times 3.42, the average number of neighbors for counties that received tax credits. For the year of authorization and the following three years, the effects on a county and its neighbors are statistically significant although, as is apparent from the figure, the large positive effects on the recipient

county is usually cancelled out by roughly comparable job losses in neighboring counties. The solid line in the figure represents the net employment effect for each year, and is small and positive for three of them, rising to 45 jobs by the third year after authorization. By the fourth year after authorization neither the recipient county nor its neighbors see a statistically significant effect on employment because of the tax credits.

It's not possible to know the precise sources of the job gains and losses illustrated in Figure A1 because they include the direct gains at recipient firms, substitution effects on other firms, and spinoff and multiplier effects. The results do, however, illustrate that one county's gains are likely at the expense of other local economies, and not by attracting workers out of unemployment or by generating large multiplier effects. The importance of these cross-county effects is highlighted further by the large and statistically significant negative neighbor effects for counties in μ SAs and small MSAs.

The closer integration of counties in μ SAs and small MSAs mean that the negative effects of tax credits on a county's neighbors are significantly larger than are illustrated in Figure A1. During the authorization year and the year following it, a county within a small metro area tends to see an additional boost in employment when firms in one of its neighbors receive tax credits. Perhaps this is because the entire metro area is participating in construction-related activities for the supported projects. Subsequently, however, the neighbor sees increasingly large negative effects that are in addition to the non-metro neighbor effects already described. In contrast, there



is little additional neighbor effect when the county is in a large MSA other than in the year after authorization. Perhaps this should be expected because the smallness of μ SAs and MSAs means that neighbor effects will be more obvious statistically. That is, the metro-area neighbor effect might be highly diluted in large metro areas such as Kansas City and St. Louis, which have 16 and 17 counties, respectively.

The total effects in terms of employment per million dollars of tax credits are illustrated by Figure 3. The figure shows the own effects, the neighbor effects, and the net effects by the number of years since the tax credits were authorized. To obtain it, I applied the marginal effects described above to the tax credits that were authorized through 2011. Note that I used only the coefficients that were statistically significant at the 10 percent level or better. I then aggregated the estimated effects on 2011 employment according to the year in which the credits were authorized

and whether the credits were received by a county or by its neighbors.

ALTERNATIVE SPECIFICATIONS

Equation (1) allows for a variety of employment effects in addition to those on the counties receiving tax credits: (i) neighbor effects, (ii) metro effects, and (iii) border effects. I tested alternative specifications that restrict the effects of these categories to zero. The results of three restricted estimations, each of which excludes one category of variables, are summarized in Table 2.

The first restricted model assumes that there are no neighbor effects and this restriction has little effect on the rest of the estimates, even though neighbor effects were found to be statistically significant in the unrestricted estimation. This result is readily apparent from a comparison of Table 1 to Table 2 and indicates there was no localized spatial correlation in the allocation

of tax credits under the program. The second set of restricted results show the statistical importance of including metro effects to avoid biasing the estimates of the rest of the model. Specifically, the positive and statistically significant effects of counties' own credits for the year of authorization and the third year following authorization would not be obtained, and the coefficients on the neighbor effects would be reduced by one third to one half. Finally, even though only one of the border coefficients in the unrestricted estimation was statistically significant, if it were assumed that the effect of being on the border was zero, the positive effect found for counties' own tax credits would be reduced for all years of authorization that are statistically significant in the unrestricted estimation.

Table 2. Alternative Estimation Results: Dependent Variable = County Relative Employment

	No Neighbor Effects			No Metro Area Effects			No Border Effects		
	Non-Metro	Small	Large	Non-Metro	Border	Border	Non-Metro	Small	Large
<i>Effects of Own Credits</i>	$\theta_{b,\dots,0_5}$	$\omega_{b,\dots,0_5}$	$\lambda_{b,\dots,\lambda_5}$	π_{b,\dots,π_5}	π_{b,\dots,π_5}	π_{b,\dots,π_5}	$\theta_{b,\dots,0_5}$	$\omega_{b,\dots,\omega_5}$	$\lambda_{b,\dots,\lambda_5}$
Authorization year	0.0045* (0.0021)	-0.0047 (0.0053)	-0.0018 (0.0042)	-0.0056 (0.0035)	-0.0041 (0.0055)	-0.0041 (0.0055)	0.0023 (0.0015)	-0.0057 (0.0068)	-0.0042 (0.0061)
Year after authorization	0.0092* (0.0030)	0.0073 (0.0101)	-0.0094† (0.0055)	-0.0055 (0.0046)	-0.0057 (0.0067)	-0.0057 (0.0067)	0.0073* (0.0021)	0.0065 (0.0104)	-0.0116 (0.0077)
Second year after	0.0067† (0.0035)	0.0042 (0.0134)	-0.0077 (0.0056)	-0.0048 (0.0056)	-0.0015 (0.0080)	-0.0015 (0.0080)	0.0055* (0.0024)	0.0011 (0.0140)	-0.0051 (0.0079)
Third year after	0.0096* (0.0044)	-0.0242 (0.0159)	-0.0013 (0.0072)	-0.0113 (0.0069)	-0.0031 (0.0092)	-0.0031 (0.0092)	0.0056† (0.0031)	-0.0228 (0.0158)	-0.0042 (0.0096)
Fourth year after	0.0045 (0.0043)	-0.0164 (0.0162)	-0.0025 (0.0079)	-0.0086 (0.0083)	0.0012 (0.0111)	0.0012 (0.0111)	0.0033 (0.0035)	-0.0177 (0.0161)	-0.0005 (0.0104)
Fifth year after	-0.0094 (0.0625)	-0.1112 (0.1358)	0.0072 (0.0623)	0.0129 (0.0144)	0.0116 (0.0149)	0.0116 (0.0149)	0.0195 (0.0649)	-0.1359 (0.1369)	-0.0123 (0.0660)
<i>Effects of Neighbors' Credits</i>	$\gamma_{b,\dots,\gamma_5}$	π_{b,\dots,π_5}	π_{b,\dots,π_5}	π_{b,\dots,π_5}	π_{b,\dots,π_5}	π_{b,\dots,π_5}	$\gamma_{b,\dots,\gamma_5}$	$\delta_{b,\dots,\delta_5}$	ν_{b,\dots,ν_5}
Authorization year	-0.0005* (0.0002)	0.0018 (0.0019)	0.0018 (0.0019)	0.0018 (0.0019)	0.0018 (0.0019)	0.0018 (0.0019)	-0.0012* (0.0003)	0.0023* (0.0007)	0.0021 (0.0018)
Year after authorization	-0.0007* (0.0003)	0.0042† (0.0023)	0.0042† (0.0023)	0.0042† (0.0023)	0.0042† (0.0023)	0.0042† (0.0023)	-0.0020* (0.0004)	0.0016 (0.0010)	0.0029 (0.0022)
Second year after	-0.0010* (0.0003)	0.0004 (0.0028)	0.0004 (0.0028)	0.0004 (0.0028)	0.0004 (0.0028)	0.0004 (0.0028)	-0.0022* (0.0004)	-0.0033* (0.0014)	0.0013 (0.0030)
Third year after	-0.0016* (0.0004)	0.0021 (0.0033)	0.0021 (0.0033)	0.0021 (0.0033)	0.0021 (0.0033)	0.0021 (0.0033)	-0.0022* (0.0005)	-0.0065* (0.0016)	0.0022 (0.0033)
Fourth year after	-0.0003 (0.0004)	-0.0043 (0.0043)	-0.0043 (0.0043)	-0.0043 (0.0043)	-0.0043 (0.0043)	-0.0043 (0.0043)	-0.0003 (0.0005)	-0.0087* (0.0016)	-0.0027 (0.0044)
Fifth year after	-0.0006 (0.0010)	-0.0008 (0.0061)	-0.0008 (0.0061)	-0.0008 (0.0061)	-0.0008 (0.0061)	-0.0008 (0.0061)	-0.0028† (0.0017)	0.0308* (0.0102)	0.0012 (0.0061)

The estimation includes county fixed effects and county-specific quadratic trends, which are not reported here. Estimation is performed using Feasible Generalized Least Squares with corrections for heteroskedasticity and autocorrelation. Statistical significance at the 5 percent and 10 percent levels are indicated by “*”, and “†”, respectively. Data are annual for 114 Missouri counties covering 1998-2011.

NOTES

¹ This paper is an abridged version of a working paper: Howard J. Wall, "[Robbing Peter to Pay Paul: The Employment Effects of the Missouri Quality Jobs Program](#)," (2013), Munich Personal RePEc Archive, paper no. 50605. The working paper contains the technical details of the econometric estimation summarized here.

² Howard J. Wall, "[Tax Credits as a Tool of State Economic Development Policy](#)," Show-Me Institute, public policy study no. 30, published online November 1, 2011,.

³ Michael D. LaFaive and Michael Hicks, "The Influence of Targeted Economic Development Tax Incentives on County Economic Growth: Evidence from Michigan's MEGA Credits," *Economic Development Quarterly*, 25, no. 2, (2011): 193-205.

⁴ For a discussion of the types of policies employed and the reasons they have persisted despite the lack of evidence of their effectiveness, see Lingwen Zheng and Mildred Warner, "Business Incentive Use Among U.S. Local Governments: A Story of Accountability and Policy Learning," *Economic Development Quarterly*, 24, no. 4 (November 2010): 325-36.

⁵ By 2009, only 23.1 percent of programs included such clawbacks, as noted in: Mildred Warner and Lingwen Zheng, "Business Incentive Adoption in the Recession," *Economic Development Quarterly*, (2013), 27, no. 2, (November 18, 2013): 90-101.

⁶ Zheng and Warner, "Business Incentive Use Among U.S. Local Governments," 325-36.

⁷ I should note that the Missouri state auditor issued [a report](#) chastising the state's Department of Economic Development over its administration of the MQJP. Most of the report had to do with the methods used to calculate job gains at recipient firms, which is largely beside the point in determining the actual effectiveness of the program.

⁸ These numbers are summed across the three sub-programs and include all authorizations that were not disqualified.

⁹ Missouri Department of Economic Development, Missouri Quality Jobs Program, [Annual Report for 2012](#).

¹⁰ For the experience in Ohio, see Todd M. Gabe and David S. Kraybill, "The Effect of State Economic Development Incentives

on Employment Growth of Establishments." *Journal of Regional Science*, 42, no. 4 (November 2002): 703-30. For the experience in Georgia, see Dagny Faulk, "Do State Economic Development Incentives Create Jobs? An Analysis of State Employment Tax Credits." *National Tax Journal*, 42, no. 4: 703-30.

¹¹ Nationally, about one-third of all new jobs in the United States are filled by job switchers and there are large differences in job-switching rates across industries. See Henry Hyatt and Erika McEntarfer, "[Job to Job Flows and the Business Cycle](#)," (March 2012), U.S. Census Bureau, Center for Economic Studies Discussion Series 12-04.

¹² The DED's model of the Missouri economy is a version of the widely used regional forecasting models produced by Regional Economic Models, Inc.

¹³ Edwin S. Mills, "The Misuse of Regional Economic Models." *Cato Journal*, 13, no. 1 (May 31, 1993): 29-39.

¹⁴ Regional forecasting models "suffer from the Lucas critique, equation parameters may be unstable over time, and their lack of deep structure confounds interpretation of estimated parameters," [Dan S. Rickman "Modern Macroeconomics and Regional Economic Modeling," *Journal of Regional Science*, 50, no. 1, (February 2010): 23-41].

¹⁵ For details and complete estimation results, see Wall, "Robbing Peter to Pay Paul," MPRA paper.

¹⁶ Note that only statistically significant effects were used to construct Figure 3