Do Incentives Programs Cause Growth? The Case of The Oklahoma Quality Jobs Program and Community-level Economic Growth

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Selected Paper prepared for presentation at the Agricultural & Applied Economics Association's 2013 AAEA & CAES Joint Annual Meeting, Washington, DC August 3 – 6, 2013

Introduction

The Great Recession encouraged state and local governments to increase their reliance on incentive programs that attempt to persuade businesses to locate or expand within their jurisdiction. Osgood et al. (2012) note that between 2004 and 2009 the percentage of local governments offering business incentives increased from 56 percent to 89 percent. Incentive programs are generally utilized by state and local governments for two reasons: 1) a belief that such programs motivate companies to move into their area, creating new jobs and business investments; and 2) a belief that the economic growth resulting from these new jobs and investment will increase public revenue which in turn improves public services and quality of life (Peters and Fisher, 2004; Osgood et al., 2012). However, valid assessments of the effectiveness of these incentive programs has declined, with only 31 percent of local governments attempting to measure this in 2009 versus 57 percent in 2004 (Osgood et al. 2012). Similarly, the Pew Center (2012) finds that only half of the states in the U.S. have taken basic steps to evaluate whether incentive programs deliver a strong return on taxpayer investments.

The consensus regarding the effectiveness of incentive programs has changed over time. Up until the 1980s, most research suggested incentive programs had no effect on economic growth. Studies conducted during the 1990s, however, reported significant and positive relationships between incentive programs and economic growth. More recently (early 2000s), researchers have returned to the finding of is no significant relationship between incentive programs and economic growth, but acknowledge some general positive outcomes of such programs.

Most studies before and during the 1980s suggested economic development incentives had minor to no impact on economic growth (Due, 1961; Eisinger, 1988; Oakland, 1978).

However, this began to change during the late 1980s and 1990s when several studies found that tax incentives did impact economic growth (Peters and Fisher, 2004). During the 1990s new data sets became available and econometric methods began to advance, creating doubt about conclusions that incentives had no effect on economic growth (Newman and Sullivan, 1988). Newman and Sullivan found a significant link between economic development incentives and economic growth, as did Bartik (1991). Bartik's comprehensive review found that job growth resulting from incentive programs created positive long-term effects for economies, such as lower unemployment, higher labor force participation, higher house values, and better occupational opportunities.

Currently, studies have found no real consensus between economic development incentives and economic growth. Some have found no impact (Hansen and Kalambokidis, 2010; Florida, 2012) while others have been slightly more positive (Luger and Bae, 2005). An argument exists that state policy makers are obliged to offer incentives to businesses out of fear that they may lose businesses already located in their state to other states with tax incentive programs. This study will add to the recent literature regarding incentive programs by evaluating the relatively unique Oklahoma Quality Jobs Program.

Oklahoma offers a distinct approach to economic development incentives through the Oklahoma Quality Jobs Program. In order to encourage firms to locate or expand within Oklahoma, the Quality Jobs Program provides cash payments, rather than tax incentives, to firms for up to five percent of newly created gross taxable payroll (Warner and Dauffenbach, 2004). Proponents of the Quality Jobs Program believe these cash payments help convince businesses to locate in Oklahoma, while opponents believe businesses receiving these cash payments would have located or expanded in Oklahoma regardless of the incentive program. Thus, opinions on the Quality Jobs program can vary from "one of the outstanding job recruitment programs in the country" (Krehbiel, 2011) to "I never really saw that it developed a lot of jobs" (Vieth, 2011).

Literature Review

Much of the recent research on economic development programs has found little evidence that incentive programs have any effect on an area's economic performance. Florida (2012) found no statistically significant association between a state's economic development incentives per capita and income, college graduates, or the state's unemployment rate. The only significant relationship Florida found was between incentives and the poverty rate, which agrees with Osgood et al.'s (2012) findings about economically distressed areas, commonly referred to as opportunity zones.

Osgood et al. (2012) discovered that areas with higher poverty rates, higher percentages of minority residents, lower percentage of residents with at least a bachelor's degree, lower median household incomes, and lower median home values rely heavily on firms to locate to their area to provide jobs, wealth, and tax revenues. These opportunity zones face more competition for economic development incentives but are provided less funding than their wealthier counterparts due to lack of capital and excessive labor costs (Osgood et al., 2012).

Hansen and Kalambokidis (2010) analyzed Minnesota's economic development program, Job Opportunity Building Zone (JOBZ) between 2004 and 2007. The JOBZ program, which is somewhat similar to Oklahoma's Quality Jobs Program, was implemented in 2004 in 10 zones located outside of the Minneapolis-St. Paul metropolitan ring. The zones were determined by socioeconomic need. Participating businesses in the JOBZ program reported creating many jobs and investing millions of dollars in each zone over the studied time period. However, Hansen and Kalambokidis' econometric analysis using a multivariate regression found little evidence that the JOBZ program impacted employment and income growth over the short time period in question. Hansen and Kalambokidis conclude with a number of explanations for their (lack of) results, including that their study was conducted too early or on too large a scale, that multiplier effects did not have time to occur, or that the link between population and income growth may be weak - meaning that an individual working in one county may not live in that same county. They propose future research might conduct evaluations at the community level rather than the county level, and be conducted over a longer time frame.

Reese and Ye (2011) offer an interesting approach to determining whether the health of an economy is more influenced by environmental or policy variables. Focusing on community-level data, Reese and Ye determined that a significant correlation exists between aspects of weather (an environmental variable) and economic growth. Reese and Ye found that lower amounts of precipitation and fewer days with low temperatures less than 32 degrees contributed to economic growth. Although Reese and Ye determined that climate plays a role in economic prosperity, they also found that effective public policy regarding quality of life (public services, investment in amenities) plays an even larger role. Reese and Ye emphatically find, however, that economic prosperity is more likely to stem from policies related to quality of life rather than incentive programs. Such findings provide additional support for the minimal economic impact of the Minnesota JOBZ program (Hansen and Kalambokidis, 2010). Similarly, Koven and Lyons (2010) suggest that state governments should focus more on basic services rather than expanding incentive programs.

Luger and Bae (2005) suggest that rather than using elegant modeling, researchers could be more effective by using a straightforward approach that simply calculates the potential savings from a specific type of incentive and converts it into jobs gained. Luger and Bae tested this approach on North Carolina's Lee Act, which offers 5 different categories of incentives (job creation, machinery and equipment, central administrative offices, research and development, and worker training) to qualified employers. One particularly interesting aspect of the Lee Act is that the level of incentive increased for more economically distressed areas (for example, a per-job credit of $12,500 in Tier 1 but only $500 in Tier 5).

Luger and Bae (2005) provide a multitude of tables detailing the potential savings from each of the tiered incentive programs for hypothetical average salaries or investments. They translate the savings into the number of jobs potentially created per firm, which for the job creation component varies from 0.07 in the higher tiers to 6.75 in the lower tiers. This approach differs from most econometric-oriented studies since it, in their words, "obviates the need for actual microlevel (or firm-level) data" (p. 331). In essence, they create a (hypothetical) range of potential savings for firms across different tiers and estimate the potential jobs gained from those savings. Not surprisingly, Luger and Bae note that the results of their simulation approach differ significantly from the estimated gross employment effects that the companies report. They also showed that the overall incentive program was not cost effective, with a cost per job induced of over $147,000 in 1999. They do note that this number was significantly lower in Tiers 1 and 2 (the most economically distressed tiers), with an average cost per job of $28,800. Luger and Bae conclude that about four percent of new jobs reportedly associated with the Lee Act would otherwise not have occurred, which they suggest is important information for legislators. The study was subject to several limitations (such as a paucity of data and assumptions made regarding wages / investment in the simulation analysis), but the primary point that Luger and Bae drive home is legislators need to understand the difference between net-induced effects and gross effects – and that they provide a relatively simple methodology for constructing those estimates.

The Pew Center (2012) reports that despite an increasing reliance on tax incentives, many states have not taken steps to include policy makers on whether there is a good return on their incentive programs. Few states are leading the way in providing answers to their tax incentive effectiveness and over half are falling behind. Billions of dollars are spent on tax incentive programs but there is little regular, in-depth testing of the economic effects of the program. The Pew Center (2012) reports that Minnesota and North Carolina are two states that are leading the way in providing answers for measuring tax incentive effectiveness. Both states ranked highly in terms of measuring economic impact and drawing clear conclusions. Oklahoma, meanwhile, is listed as "trailing behind" in their methodology.

Background on Oklahoma's Quality Jobs Program

The Oklahoma Quality Jobs Program was created in 1993 as an economic incentive program encouraging businesses to create jobs in Oklahoma (Blatt, 2010; Warner and Dauffenbach, 2004). The program makes quarterly payments of up to 5% of newly created payroll for companies that meet specific requirements. In order to qualify for these cash payments, a business must be relocating to or expanding in Oklahoma and meet standards of: type of industry, number of jobs, wage level, and health insurance coverage (Warner and Dauffenbach, 2004). Qualifying businesses include central administrative offices, manufacturers, and research and developers. There is a notable focus on "basic industries," which means that a significant share of the company's sales are expected to be made to out-of-state companies. This program is relatively unique since the state actually makes quarterly cash payments to companies with Quality Jobs contracts, rather than exempting them from certain types of taxes. As Warner and Dauffenbach (2004) indicate, few other states offer this type of direct payroll reimbursement. Only 4 out of 1,106 other state-level incentive programs included in the National Association of State Development Agencies' survey resembled the Oklahoma program as of 2004.

Each firm completing an application for the Quality Jobs program provides an estimate of the new direct jobs they will create. While there is some oversight from the Oklahoma Department of Commerce, these estimates are rarely contested. As a result, there is an incentive for the firms to overstate their estimates – and in fact, although the companies involved promised to create 237,000 jobs in aggregate, only about a third of those have actually been created (Cameron, 2012).

To determine the amount of cash payments going to each company, the Department of Commerce compares the benefits and costs associated with each claimed job. The new jobs should produce additional state government revenues, referred to as "estimated direct state benefits" such as state income, sales, and use taxes. The Department of Commerce also takes into account the increased cost of services supplied by the state government for these jobs. They estimate "direct state costs" comprised of education costs for children, public health and transportation costs that will be used by the new state residents. Net benefits are calculated by subtracting the estimated state costs from the estimated state benefits. These net benefits are then divided by the gross payroll of the new workers to derive a "net benefit rate." This net benefit rate is the percentage of new payroll paid back to companies on a quarterly basis, and is typically around four percent on most applications (Warner and Dauffenbach, 2004). By conducting the application process in this way, the program is attempting to guarantee that state-level benefits are exceeding state-level costs; however, companies have an incentive to exaggerate the number of jobs they claim will be created.

The Oklahoma Tax Commission is responsible for making the cash payments to qualifying businesses. In order to receive the payments, a business' annual payroll for new employees must total $2.5 million within three years (though lower thresholds apply for "high impact projects" or companies in certain targeted industries); the business must offer basic health insurance, and meet the average county wage for 2013 (or $31,297, whichever is lower). Companies are eligible to receive payments from the program for up to 10 years (Oklahoma Department of Commerce, 2013). According to Warner and Dauffenbach's 2004 report, almost four-fifths of the Quality Jobs Program cash payments went to businesses in metropolitan statistical area counties as of 2003; our updated analysis confirms this urban bias.

The first year after the Quality Jobs Program was implemented (1994), $239,000 was provided to qualifying firms. The cash payment has significantly increased since 1994, with a $54.2 million overall payment in 2003 and a $68.9 million overall payment in 2012 (Warner and Dauffenbach, 2004; Oklahoma Tax Commission, 2012). The Oklahoma Quality Jobs Program has not historically had a "clawback" provision, which means a firm was not required to pay back the funds received if they fail to meet the guidelines and goals of the Quality Jobs Program (Cameron, 2012; Blatt, 2010). Several companies did leave the program during our period of analysis after their estimated new jobs did not materialize, but the lack of a clawback provision prevented any fund reimbursement.

The Oklahoma Department of Commerce, along with many state officials, believes the Quality Jobs Program is making a positive impact in Oklahoma. There is some research to back up this position (see the next section of this paper); however, many residents and elected officials have been critical of the program (Vieth, 2011). Oklahoma state representative Eric Proctor cites the case of Imation, a company that received over $1.9 million from the QJ program but ultimately moved production overseas (Cameron, 2012). State representative Mike Reynolds also disagrees with the program calling it "a socialist-style redistribution of wealth" and stating that it is not an incentive program but rather corporate welfare (Cameron, 2012). Oklahoma Department of Commerce Secretary Dave Lopez disagrees. Even though he acknowledges that incentives like the QJ program are not usually the primary reason a company decides to relocate or add new positions, he argues that they do help by demonstrating that the economic climate of the state is supportive (Cameron, 2012).

Previous Research on the Quality Jobs Program

A 2004 study by Dauffenbach and Warner measured the economic impact of the Quality Jobs Program on Oklahoma's economy. Dauffenbach and Warner used a Regional Input / Output analysis, along with a macro approach, to assess the program. One notable assumption made in this analysis is that a company's location or expansion decision was based *solely* on the incentive program. The basis of their study is an expectation that since the QJ program has been focused on specific industries, higher rates of job and earnings growth are expected in those industries versus what has been observed nation-wide.

Using 2-digit industry codes, an IMPLAN model, and data on the QJ program from 1996 – 2003, Dauffenbach and Warner found that the industries where the QJ program is focused are substantially benefiting from the program. The newly created jobs in these industries caused direct employment, indirect employment, household spending, and labor income to increase.

Dauffenbach and Warner found outsized employment gains in the industries where the Quality Jobs Program is focused. They find a high correlation (0.60) between the share of QJ payments received for an industry and the differential growth rate (Oklahoma versus US) in that industry. Thus, they conclude that the Quality Jobs Program contributed to Oklahoma's employment base "beyond what would have occurred naturally". Even so, the authors conclude their findings with an "appeal to go back to the roots of the Quality Jobs program and examine how closely the intent of the program has been followed" (Dauffenbach and Warner, 2004).

However, the key assumption in the Dauffenbach and Warner study is that company-level location/expansion decisions were based solely on the incentive program. The current study takes an alternative approach and develops a "counterfactual" argument by matching similar communities that differ only by whether or not they participated in the Quality Jobs Program. In particular, the focus of this research is on community-level growth as opposed to industry-level growth. This research also addresses the limitations asserted by Hansen and Kalambokidis, since the analysis is performed over a longer period of time and the assessments are done at the community level.

Data and Methodology

The Oklahoma Department of Commerce, who runs the Quality Jobs (QJ) program, provided data on all companies (and the communities where they were located) that had been awarded QJ rebates since the program began in 1994. Although data are available up through 2013, only companies that received rebates prior to 2004 were included in our analysis. This allowed at least some time to pass before the post-treatment economic data was collected (as detailed below, we used pooled data from 2005-2009 as the post-treatment period). Over 600 companies across 70 communities in the state were listed in the final dataset. While approximately 41% of the communities receiving funding were located in metropolitan counties, the companies in these locations received almost 85% of the total funds awarded. While 14% of the recipient communities were towns of under 2,500 population, they received only 2.4% of the funds over this period. The QJ communities are displayed in Figure 1.

[Figure 1 about here]

To assess the impact of the Quality Jobs program, community-level Census data from 1990 and the 2005-2009 American Community Survey was used. Our interest is not with the actual businesses receiving QJ funds but rather in the economic development outcomes for the cities that are home to these businesses. The dependent variables are city-level growth rates over this time (1990 to 2005-09) of six social and economic measures: (1) median household income, (2) population, (3) percentage of residents in poverty, (4) median house value, (5) number of manufacturing jobs, and (6) the percentage of total jobs employed by manufacturing. In particular, we are interested in seeing whether these city-level growth rates are significantly impacted by participation in the QJ program. Other variables used in the analysis are education levels, racial characteristics, and 1980-90 growth rates for population, median household income, and the number of manufacturing jobs. Using 1980-90 values allows us to control for growth rates that occurred prior to the QJ program, so that we can spot trends in growth that are not related to the QJ implementation.

Table 1 displays summary statistics for the variables used in the analysis. The data is broken out into communities in OK that either did or did not receive at least one QJ rebate during the 1994 – 2003 period. After removing the two outliers of Oklahoma City and Tulsa (with over $100M in QJ rebates each), the average total payment to businesses in each recipient community over that time period was $3,154,000 but ranged from $9,408 to $38,500,000. We also include all Kansas communities; Kansas does not have a program similar to the QJ program and these communities will be used in the matching estimator analysis described below.

[Table 1 about here]

Note that while there are some statistically significant differences between communities in OK that received QJ funds and those that didn't (particularly in population, income, and education), several categories of interest do not display any differences (population growth, manufacturing job growth). In fact, the manufacturing percentage growth was actually lower in QJ recipient communities. When the QJ recipient communities are compared to KS cities, there are striking differences in poverty growth (poverty rates actually increased in KS over this time, compared to large reductions in OK), manufacturing job growth, and population growth. However, simple descriptive statistics such as these do not allow us to determine the role of the QJ program on any of the economic variables.

To accurately assess whether the QJ program had an impact on the communities where it was received, it is necessary to control for multiple demographics that could affect economic growth over that time. Two distinct econometric techniques are used to estimate community-level impacts of the QJ program – *multivariate regression* and *matching estimators*.

We follow Prieger (2013) in demonstrating the differences between the two techniques in terms of the potential biases that can exist. As Prieger notes, there are three potential sources of bias that exist when comparing a treatment group (in this case, the communities that received QJ funding) and a control group. The first (B1), known as the "common support" problem, exists when there are no members in the control group that have comparable observed characteristics as some members in the treatment group. [We have eliminated Oklahoma City and Tulsa for this reason, but there may be others] The second type of bias (B2) occurs when the treatment and control group have significantly different distributions of covariates. The third type (B3) happens when unobserved factors influence the outcomes between the treatment and control group. Generally, this occurs when those unobserved factors are not distributed equally between the two groups.

Multivariate Regression

Multivariate regression is a commonly applied technique used to address B2, but it does not correct for B1. Further, regression is dependent on the functional form chosen (namely, that the dependent variable is linear with respect to the regressors), and is correctly specified in terms of normality and heteroskedasticity. Our regression originates from a modified growth model:

$$Y_t = A{Y_{t-i}}^{\alpha}\, e^{\mathrm{ri}} \tag{1}$$

where: Y_t represents the economic level at time t,

A is a constant, α is a scaling parameter, and

e^{ri} is the formula for compounded growth at rate r for i periods.

The most important element in this approach is to determine the correct expected growth rate, *r*, between the two periods. Because of the importance of this step, the growth rate, *r*, is determined statistically using multivariate regression analysis. Transforming this growth equation using natural logarithms, assuming that A and α equal one (which are standard assumptions when empirically testing growth models), and defining time periods in such a way as to make i = 1, we derive the following equation:

$$ln(\frac{Y_t}{Y_{t-1}}) = r_t = \beta_1 X_{1t} + \cdots + \beta_n X_{nt} + \gamma QJ_t + \varepsilon_t \qquad (2)$$

Equation (2) states that the economic growth rate r_t for a community is a function of the explanatory variables (X_{1t} through X_{nt}, which could include growth rates during the 1980s), a quality job dummy variable QJ_t, and an error term ε. In this study, the dependent variable (Y) represents six distinct measures of economic growth between 1990 and the 2005-09 ACS, the explanatory variables (X) include a variety of socioeconomic factors and 1980s growth rates, and the error term ε is assumed to have a log-normal distribution. The QJ dummy variable (QJ_t) is created by assigning a one (1) to all communities with at least one business that received QJ funding from 1994 to 2004, and zero (0) otherwise. In particular, we are interested in whether the QJ program impacted growth, or whether $\gamma = 0$. Each of the six dependent variables is regressed in this manner.

Matching Estimators (Average Treatment Effects)

As opposed to multivariate regression, matching estimators enforce a "common support" requirement when comparing treatment and controls, effectively removing B1. Additionally, the need for a correct specification of the regression function is removed since the technique matches on the probability of being in the treatment group (this is why the technique is known as semi-parametric).

The matching technique is applied in several steps. Initially, the likelihood of being treated (in this case, obtaining QJ funding) is estimated via a logistic regression using observed covariates from previous time periods (i.e. 1980s growth or 1990 values). This results in a "propensity score" for each community. Each community in the treated group is then matched to a community in the control group by looking for similar propensity scores. Thus, each community that received QJ funding is linked to another community that is similar in observable characteristics (and thus has a similar propensity score), but did not receive any QJ funds. This matching can be done in a number of ways, including "nearest neighbor" (looking for the closest propensity score between groups) or "kernel matching" (weighting the difference between propensity scores in a group and matching each treated unit with a weighted counterpart)[1]. The difference in economic growth outcomes between the matched groups is then assessed using simple t-tests.

More formally, we let ΔY_1 and ΔY_0 be the economic growth indicators of areas with and without QJ funding, respectively. The average treatment effect can be presented as:

$$ATE = E(\Delta Y_1 |\ QJ_t = 1) - E(\Delta Y_0 |\ QJ_t = 1) \tag{3}$$

where QJ_t equals 1 for communities that received QJ funding (treated) and 0 for communities that did not (control). We can observe either ΔY_1 or ΔY_0 for a particular place, but not both, since each community has either participated or not participated in QJ program. The above matching technique is used to establish a comparable, non-treated counterpart to each treated community. Blocks of communities with similar propensity scores are developed, and a test developed by Becker and Ichino (2002) determines whether the treated and control communities in each block have the same distribution of covariates (thus addressing bias from B2). The use of this matching technique does lend itself to statements about causality, since by definition equation (3) attempts to account for covariates that predict the treatment effect (Rosenbaum and Rubin, 1983).

[1] This paper matches each treatment community with the 5 nearest neighbors (in terms of propensity score) from the control group. This helps remove the impacts of one-to-one matches that might not be quantitatively similar.

In this paper, we apply the matching technique both to other Oklahoma communities that did not receive any QJ funding, and also to KS communities that were not eligible for funding (and do not have a similar program). This step is taken in an attempt to control for the fact that if communities are similar, there would be little reason for any OK community not to pursue the QJ program. Thus, the difference between the matched communities in OK may be due to some unobservable characteristic, such as the presence of an economic development professional familiar with the workings of the QJ program in one community. Such unobservables would likely be present in any B3 bias that might exist. In KS, on the other hand, communities can be similar to OK QJ recipients, but have no incentive program to apply for.

Results

Multivariate Regression

Results from the multivariate regressions are displayed in Table 2. The parameter associated with the primary variable of interest, QJ funding, is never significant in any of the regressions.

[Table 2 about here]

Although none of the QJ parameters is significant, it is interesting to note that they all have negative signs. Several of the models suffer from exceedingly poor fits, such as the percentage change in the number of manufacturing jobs, with adjusted R^2 values of less than 0.01. Others, however, are more reasonable, with adjusted R^2 values between 0.06 and 0.33. Alternative specifications were attempted that used the logarithm of total QJ funds received instead of the QJ dummy variable, and again obtained no significant parameters. These regression results do not provide any evidence that obtaining QJ funding had any impact on community-level economic changes between 1990 and 2005-09.

Matching Estimators (Average Treatment Effects)

One important point of discussion when using matching estimators is the fit of the logistic model that estimates the likelihood of receiving funding from the QJ program. As Appendix A details, the logistic model run here has a relatively high pseudo-R2 measure (0.571) and has several significant predictor variables, including manufacturing job growth during the 1980s and population / manufacturing jobs as of 1990. The model correctly identifies about 2/3 (65%) of all communities that did receive funding, and about 89% of communities that did not. Thus, the propensity scores used to develop the matching estimators seem to come from a reasonably well-specified model. It is also important to note that the coefficients resulting from this model are applied to the KS communities to estimate their propensity scores (since KS does not have any QJ recipients). Table 3 below provides the results of the matching estimators (both nearest neighbor and kernel) when the treatment group is compared to matched communities in both OK and KS.

[Table 3 about here]

In terms of the matching estimator outcomes, the first result of interest is that there are no statistically significant differences between treatment and control groups when the analysis is limited to OK communities. This holds true regardless of whether nearest neighbor or kernel matching methods are used. Note that 10 observations are removed from the control group due to the “common support” imposed, which is why the treated averages displayed do not exactly match those from Table 1. In this case, 10 treated communities had propensity scores that were higher than the maximum propensity score for the control group, which is why they were eliminated from the analysis. The main takeaway, however, is the lack of result for any of the variables of interest.

The second part of Table 3 compares the OK QJ communities with otherwise similar KS communities. Interestingly, several results are found. The OK QJ communities were found to have statistically higher levels of median household income (85% vs. 75%) growth from 1990 - 2005 under the nearest neighbor technique. The statistical significance disappears once the kernel technique is used, but the control communities still demonstrate lower growth in median household income (which did not occur in the OK sample). The matching estimators also find significant differences in poverty growth and growth in manufacturing's percentage of all jobs; however, poverty growth showed dramatic differences between states before any matching took place (Table 1). The decline in the percentage of jobs contributed by manufacturing in the QJ communities is noteworthy, particularly since comparable communities in OK and KS had slower declines (though the results for OK were not statistically significant) and the QJ program was supposed to be heavily focused on manufacturing jobs. Note that no treatment group observations were removed from this analysis, since the propensity scores for the KS communities had ranges exceeding the minimum and maximum values for the treated OK cities.

Conclusion

The results of this analysis generally suggest that the provision of quality job program funding to a business within a particular community has limited long-term impacts on income, population, house value, or manufacturing job growth. Multivariate regression and matching estimators for communities in Oklahoma provide no evidence that cities with at least one business that obtained QJ funding grew any faster in terms of income, population, or house value than did non-recipient communities.

We do find limited evidence that the QJ program increased median household income when treated communities in OK are compared to similar communities in KS, which does not have a comparable program. Using the nearest neighbor matching method, obtaining QJ funding (arguably) causes a 10% higher growth rate in median household income over the longer term. When the kernel matching method is used, we still find evidence of a roughly 9 percentage point higher median household income, but the difference is no longer statistically significant due to higher standard deviations around the estimates of the matched communities. We also document significant differences in poverty growth rates and job composition (the percentage of manufacturing jobs actually declined more in QJ communities), but large (unmatched) state-level differences in these variables diminish the causal claims that can be made in these cases.

We specifically note that our results are different from Dauffenbauch and Warner (2004), who were much more positive in their review of the QJ program nearly a decade ago. The differences lie in the assumptions made (Dauffenbach and Warner assumed that all location or expansion decisions made were based solely on the incentive program, while the analysis here uses counterfactuals instead of making that assumption); the techniques utilized (Dauffenbach and Warner use an IMPLAN and shift-share models at the aggregate, state-level, while the analysis here uses community-level data and regression / matching models); and the time frame of analysis (Dauffenbach and Warner have the year 2000 as their post-treatment period, while the analysis here uses data from the 2005-09 ACS).

Our results are generally similar to those of Hansen and Kalambokidis (2010), who found little evidence of a specific incentive program (JOBZ) in Minnesota. Their analysis, however, was performed at the county level, and done over a relatively short period of time (2004-2007). The analysis here demonstrates that even over the longer term, and at the city level, impacts of specific economic development incentive programs can be difficult to document.

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Figure 1. Location of Quality Job Community Recipients in Oklahoma, 1994 – 2003.

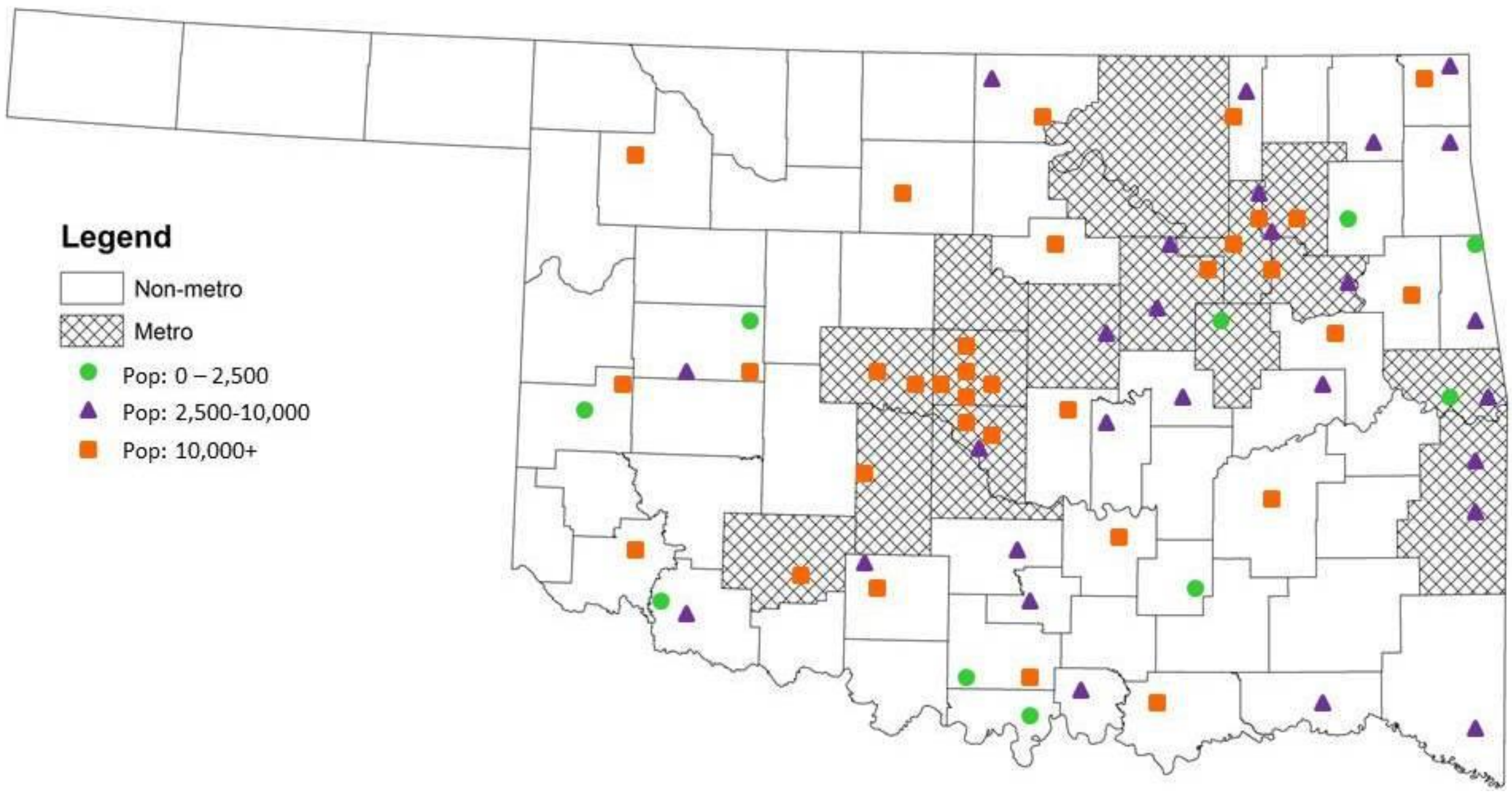

Table 1. Summary Statistics by Participation in the QJ Program

	OK Cities			KS Cities	
	QJ = 1	QJ = 0			
MHI Growth (90-05)	0.8465	1.1437	**	0.9389	
POP Growth (90-05)	0.0860	0.1318		0.1718	
POV Growth (90-05)	-0.3986	-0.4303		0.2004	***
MHV Growth (90-05)	1.1230	1.2630		1.1826	
MFG job Growth (90-05)	0.0163	0.1864		0.2559	***
MFG pct Growth (90-05)	-0.2733	-0.1224	*	0.1172	***
MFG job Growth 80s	0.1007	0.0825		0.0593	
MFG pct Growth 80s	-0.0263	0.0006		0.0519	
Pop Growth 80s	0.1361	0.0105		-0.0817	***
MHI Growth 80s	0.5636	0.5699		0.6316	
Pop 1990	15,033	1,088	***	3,151	***
MHI 1990	20,163	17,967	**	21,795	
Pct Bach 1990	0.1508	0.0925	***	0.1202	**
Pct <14 min 1990	0.5389	0.3846	***	0.4654	***
Pct Vacant 1990	0.1336	0.1896	***	0.1509	
Pct Black 1990	0.0491	0.0407		0.0080	***
# Obs	68	516		628	

*, **, and *** denote statistically significant differences from the QJ = 1 category at the p=0.10, 0.05, and .01 levels, respectively

Table 2. Multivariate Regression Results

DV	lnmhigrowth90s		lnpopgrowth90s		lnpovgrowth90s		lnmhvgrowth90s		lnmfgjobs90s		lnmfgpct90s	
lnmhigrowth80s	-0.378	***	-0.116		0.551	***	-0.106		-0.163		-0.202	*
lnpopgrowth80s	0.073	**	-0.205	***	-0.084		-0.073	*	0.070		0.029	
lnpop1990	-0.032	**	0.010		-0.011		0.038	**	-0.013		-0.010	
lnmhi1990	-0.683	***	-0.307	**	-0.593	***	0.176	*	0.187		0.260	*
less1pov1990	-0.160		-0.442		-3.289	***	0.427	**	0.020		0.114	
lnmhv1990	0.182	***	0.502	***	0.246	*	-0.417	***	0.007		-0.128	
pctblack1990	-0.166		-0.055		0.000		0.114		-0.040		-0.056	
pctother1990	-0.170		0.263		-0.065		0.084		-0.061		-0.214	
pctbach1990	0.772	***	-0.313		0.143		0.361		0.037		0.376	
pctvacant1990	-0.204		0.743	***	0.329		-0.228		-0.181		-0.191	
qjfunding	-0.033		-0.067		-0.034		-0.019		-0.049		-0.068	
_cons	5.869	***	-2.264	*	3.061	*	2.979	***	-1.770		-1.409	
Adjusted R2	0.3259		0.0616		0.136		0.1237		-0.0074		0.0004	
Number Obs	558		558		513		548		525		525	

*, **, and *** denote statistically significant differences from 0 at the *p*=0.10, 0.05, and 0.01 levels, respectively

Table 3. Matching Estimator Results

OK Comparison

		Nearest Neighbor					Kernel			Observations Removed
	Treated	Control	Difference	T-stat		Control	Difference	T-stat		From Treated group
mhigrowth90s	0.870	0.888	-0.018	-0.190		0.906	-0.036	-0.160		10
popgrowth90s	0.071	0.116	-0.045	-0.500		0.062	0.009	0.020		10
povgrowth90s	-0.403	-0.308	-0.095	-0.970		-0.289	-0.114	-0.770		10
mhvgrowth90s	1.146	1.241	-0.095	-0.470		1.181	-0.034	-0.120		10
mfgjobgrowth90s	0.012	0.251	-0.239	-1.010		0.134	-0.122	-0.400		10
mfgpctgrowth90s	-0.277	-0.116	-0.161	-1.060		-0.174	-0.103	-0.520		10

KS Comparison

		Nearest Neighbor					Kernel			Observations Removed
	Treated	Control	Difference	T-stat		Control	Difference	T-stat		From Treated group
mhigrowth90s	0.846	0.745	0.101	2.020	**	0.760	0.086	1.090		0
popgrowth90s	0.086	0.086	0.000	-0.010		0.090	-0.004	-0.010		0
povgrowth90s	-0.399	0.127	-0.526	-6.140	***	0.145	-0.543	-2.520	***	0
mhvgrowth90s	1.123	1.135	-0.012	-0.120		1.134	-0.011	-0.090		0
mfgjobgrowth90s	0.016	0.141	-0.125	-1.540		0.153	-0.137	-1.530		0
mfgpctgrowth90s	-0.273	-0.071	-0.202	-3.170	***	-0.060	-0.214	-2.950	***	0

*, **, and *** denote statistically significant differences from 0 at the *p*=0.10, 0.05, and 0.01 levels, respectively

Appendix A. Logistic Regression Output for Propensity Score Specification

DV: QJ Funding		
lnpopgrowth1980s	-0.1579	
lnmhigrowth1980s	0.4490	
lnmfgjobgrowth1980s	3.0149	*
lnmfgpctgrowth1980s	-3.4335	*
lnmfgjob1990	-0.4203	**
mfgpct1990	17.0406	***
lnpop1990	2.1899	***
lnmhi1990	-0.8531	
lnmhv1990	-0.3842	
_cons	-5.7122	
Psuedo R2	0.5710	
Number Obs	534	

*, **, and *** denote statistically significant differences from 0 at the *p*=0.10, 0.05, and 0.01 levels, respectively