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Replication of "Short Term Effects of the Kansas Income Tax Cuts on Unemployment"

Introduction

This paper re-examines and extends the findings of a 2017 study by Tracy M. Turner and Brandon Blagg (T&B) on the effects of Kanas' 2013 tax policy change on state level unemployment. Standard conservative theory argues that lowering taxes stimulates supply side growth. This logic was the impetus behind a sweeping reform measure passed by the state's legislature after conservative gains in the 2012 election. The new law allowed citizens to exempt unincorporated business profits from personal income statements, and lowered the state's top marginal corporate and individual income tax brackets (Turner and Blagg 2017, 1-4).

Originally billed as a job creator, Kansas governor Sam Brownback famously dubbed the policy a "real live experiment" (Rothschild 2012). Kansas has struggled in the years since the Great Recession, and intended for the policy to be a "shot of adrenaline" for its economy (Hobson 2017). Proponents of the bill have long argued that taxes are a barrier to economic growth, and that a reduction will allow firms to re-invest in themselves (Turner and Blagg 2017, 3). Indeed, famed supply-side economist Arthur Laffer was an advisor to Kansas governor Sam Brownback in the policy formulation process. At the outset of the policy, Brownback and his supporters anticipated that the bill would create 22,000 jobs over the next 5 years. (Hobson 2017).

The major component of the 2012 tax cuts is the elimination of taxation on pass-through income from unincorporated business profits. These provisions were intended to help small business owners by lowering barriers to entry (Brown 2018). However, as T&B note, the elimination of pass-through taxation also created a new opportunity for tax avoidance. For example, salaried employees could avoid paying income taxes by declaring themselves contract workers (Turner and Blagg 2017, 2). This provision was famously abused by citizens. For instance, a pair of state basketball coaches allegedly declared themselves independent contractors after the law had passed (Hobson 2017).

Likewise, opponents of the bill have maintained that claims that it will stimulate growth may be unfounded, and that the net loss in state revenue exceeds gains in employment. Five years after implementation, the latter scenario appears to be true. By May of 2017, the state budget gap reached $900 million. At the same time, Kansas has seen only a 3.3% increase in jobs since the policy went into effect, while the US average has reached 8.4% over the same period (Leachman, 2017). The policy has created 28,000 new non-farm jobs, exceeding Brownback's original prediction. However, this figure lags behind performance of economically similar neighboring states like Nebraska, who saw employment gains of 35,000 over the same period (Gleckman, 2017). One June 6, 2017 lawmakers formally repealed the tax code over the governor's veto just five years after signing (Gleckman, 2017).

Accordingly, T&B's original study attempts to explain the effect of the pass-through exemption net of state tax structure on job growth using difference in difference (DD) and matching analyses. Both specifications compare Kansas' relative gains to the performance of its border states over the period of 2004-2014, and lead the authors to find that the tax laws had little to no impact on statewide employment. In this paper, I attempt to replicate their results and extend their causal inference to look at the effect of the change in the tax base on state spending as well. Though I do not find statistically similar results as the authors, my findings do support their claim that the policy change was an ineffective job creator in the short term.

The proceeding sections are organized as follows. First, I discuss how I assembled and replicated the data used in the original analysis before outlining the empirical techniques I replicate. Second, I compare my findings to the authors, discuss possible sources of bias in the replication, and interpret both sets of results from a statistical and economic standpoint. Finally, I conclude the paper by extending T&B's DD model to look at the effects of the change in Kansas' tax base on various state expenditure programs.

Data and Empirical Methods

The outcome variable of interest in this study is county-level private sector employment extracted from the US Census' Quarterly Workforce Indicator (QWI). It is measured from the first quarter of 2004 to the fourth quarter of 2014. As the author's note in the original paper, the QWI data include all jobs covered by unemployment insurance. Individuals who are self-employed are excluded from the QWI, as enrollment in unemployment insurance is not mandatory. To account for self-employed individuals, the paper also includes county-level data on the number of sole proprietorships as reported by the US Bureau of Economic Analysis (BEA). The BEA data is only available at the annual level, and is linearly interpolated across each quarter-year observation included in the panel.

Several financial and economic control variables are also included in the panel, as this paper's identification strategy and its associated methods are concerned with testing the effects of state-level tax policy on employment. State sales rates, top corporate marginal tax rates, and top individual marginal tax rates are taken from the tax foundation. The original authors do not cite which specific tax foundation dataset they reference, and I had some difficulty matching their summary statistics exactly. For the regressions in this paper, I ended up using the tax data the authors present in table 1 of the original publication to ensure similarity between our two panels. T&B also use fixed effects to control for the unobservable heterogeneity between counties. They do the same for each quarter year included in their regressions, and because this is an important practice, I also include both sets of fixed effects in my replication. The authors and I also include state linear time trends for each year to control for other unobservable differences in the functional form. Finally, county population is also included in all of these regressions to account for heterogeneous population sizes and density. These data also come from the BEA, and are linearly interpolated from the annual to quarterly level across the entire sample.

The original paper uses two main identification strategies, a Difference in Difference (DD) estimation and a Border-County pairing specification, to test the effects of the Kansas Tax cuts on employment. For both techniques, the unit of analysis is a county in a state in a given quarter of the year (i.e. Cheyenne County, Kansas in the first quarter of 2004, etc.). The DD specification looks at the estimated differences between counties before and after the policy is in place, and between Kansas and its bordering states, Missouri, Oklahoma, Colorado, and Nebraska (where the policy does not have any effect). Hence, the treated group is made up of observations in the state of Kansas in the years when the tax policy is in place (2013 and 2014), while all other observations make up the control group. The DD specification is,

$$\mathbf{Yijt} = \beta 0 + \beta 1 \mathbf{KSpost2012}\mathit{jt} + \beta 2 \mathbf{Pop}\mathit{ijt} + \beta 3 \mathbf{Corp}\mathit{jt} + \beta 4 \mathbf{MTR}\mathit{jt} + \beta 5 \mathbf{Sales}\mathit{jt} + \varphi \mathrm{i} + \lambda t + \varepsilon ijt \quad (1),$$

where Yijt is an employment measurement of interest in county i in state j at time t. As T&B point out, KSpost2012jt is the DD estimator separating the treatment and control groups. It is equal to 1 if an observation is in Kansas and in either 2013 or 2014 and 0 otherwise (Turner and Blagg 2017, 8). The specification includes controls for county population (in 10,000s), the states' top corporate marginal tax rates, top individual marginal tax rates, and sales tax rates. The DD also controls for quarter-year and county level fixed effects to account for unobservable heterogeneity between our geographic and time units of analysis. As the authors note, controlling for the state tax structure and the heterogeneity across time and geography allows us to capture the net effect of changing the tax base on employment in the coefficient of our DD estimator.

Additionally, T&B break up their DD specification across two time periods and three measures of employment for a total of six specifications. They test the DD across the entire time frame from 2004 to 2014, and they also restrict the panel to 2010 to 2014 to better account for any post recessionary effects. Both their study and my replication present regression results for a log-linear, per capita level, and growth rate measure of employment in each period. Standard errors are clustered at the state level, as that is where the variation in tax policy occurs (Turner and Blagg, 8-9).

The Border-County match sample uses a similar differencing model restricted to the 40 Kansas border counties and adjacent counties in Oklahoma, Colorado, Nebraska and Missouri. As T&B note, "the identification strategy of using cross-state border county pairs differences out the potential bias as long as the cross-border match is a useful counterfactual." This counterfactual equation follows,

$$Y_{kmt} = \gamma_0 + \gamma_2 Pop_{kmt} + \beta_3 Corp_{mt} + \beta_4 MTR_{mt} + \beta_5 Sales_{mt} + \varphi_k + \lambda_t + \varepsilon_{kmt} \quad (2)$$

where, Ykmt is the employment measure of interest in a "neighboring state's matching county" in a given point in time in our sample. The 40 Kansas border counties in equation (1) serve as the treatment group, while equation (2) is the control. As such, the authors subtract equation (2) from equation (1) to do their analysis, giving,

$$\Delta Ykmt = \Pi_0 + \beta_1 KSpost2012_{jt}\ \Pi_1 \Delta Pop_{pt} + \Pi_2 \Delta Corp_{pt} + \Pi_3 \Delta MTR_{pt} + \Pi_4 \Delta Sales_{pt} + + \varepsilon_{pt}$$

(3).

The authors note that each coefficient Π represents the "difference in parameters across the border pair." Moreover, we expect the county fixed effects to be nearly zero because they represent the difference between unobserved heterogeneity of counties that are very close to one another. This also means the specification may have "contemporaneously correlated errors due to close geographic proximity and exposure to local shocks," so standard errors are clustered on county (Turner and Blagg 2017, 11).

T&B create their border-county pairs using a "convex combination of the matching counties' characteristics" because there is not always a one to one border match between counties. They weight these estimates by percent of the border distance shared (Turner and Blagg 2017, 11). I was unable to replicate this exactly as T&B due to a lack of familiarity with GIS software. To compensate, I measured the border-distance weights in google maps. This is not as precise as the GIS mapping, but provides at least a rough estimate to construct the border-county panel.

Table 1 compares my summary statistic to T&B's. While not an exact match, they are very close to the original data. I suspect some of this variation could be explained by updates to the BEA or QWI data since the original study was completed, or from using slightly different samples. For example, though I used the CA4 (personal income and employment by major component), I am not sure which BEA sample T&B used in their original analysis. I check for any statistical discrepancy between my sample and T&B's by comparing the sample means of each variable's sample mean. Both the pooled standard errors and the pertinent t-statistics are shown to the right of the main comparison in table. They show no statistically significant difference between my sample and the authors'.

Comparison and Discussion of Results

The main coefficient of interest in both analyses is $\beta 1$, which measures the impact of the change in tax policy on employment in Kansas relative to its border states. T&B point out that because the specifications control for the state's tax structure, the policy variable we are using captures the effects of eliminating pass through income taxes and thereby reducing the overall tax base on private sector employment (Turner and Blagg 2017, 8). The original paper gives a thorough overview of potential sources of bias in our estimates of the policy's causal impact. As previously discussed, they note the importance of controlling for county and time based fixed effects, as well as state time trends in the case that there are unobservable differences between the treatment and control group. They carry this idea further by checking for differences in long term employment trends with a trend test regressing private sector unemployment on year dummies, state fixed effects, and the interaction between the Kansas indicator and year dummies. They do not present these results, but note that if the coefficient of interaction terms is insignificant then we conclude that there is no difference in long term trends between states in our sample. My trend test verifies this assumption, and is presented in Table 2.

More importantly, T&B also point out the importance of considering the impact of balanced budget legislation and immigration policy on our interpretations of the policy differencing variable. Balanced-budget tax cuts stipulate that government spending should be reduced dollar for dollar. However, this has not been the case in Kansas. Despite the state's balanced budget laws, they have offset a dollar for dollar reduction with an increase in the state sales tax, have shifted funds around in the budget and have relied on the state "rainy day fund" to compensate for these reductions. In year 2014, they note that each dollar reduction accounted for a 22% fall in state general expenditure. In my extension in the following section, I examine the impact of these tax cuts on a few different spending components to further illuminate the impact of the tax policy on the state's finances (Turner and Blagg 2017, 9).

As far as immigration goes, T&B rightly point out that the DD analysis counts migratory effects because they would show up as a net loss for one state and a net gain for the other as a result of the policy. This biases the DD estimates upwards, unless there is no state-to-state migration in our sample. T&B underscore that if migration accounts for all job gains and losses, then results are cut in half. The border-county differencing panel is included to help account for these effects (Turner and Blagg 2017, 2010).

I have been largely unsuccessful in replicating the authors' findings in both the DD and matching specifications. In the DD sample, my results differ substantially across the full-time period of 2004 to 2014, and less so for the post-recessionary period from 2010 to 2014, as shown in table 3. This is despite the fact that our panels have the same number of observations and are not significantly different from one another from a statistical standpoint. For example, T&B find a -5.34 percentage point impact of the tax policy change on private sector employment in the full time period log-linear model. This estimate is statistically significant at the 95% confidence level. In the same model, I find only a -1.42 percentage point impact that is statistically insignificant. Similarly, my estimate of the impact of the tax policy change on per capita employment across the full sample is less than half of T&B's. They find roughly 13 jobs lost per 1000 people, while I find just six jobs lost per 1000 in the per capita model, even though both of our estimates are significant at the 95% level. With the exception of the corporate marginal tax rate, I find the same wide level of disparity between the coefficients I report and those reported by the authors in the 2004-2014 sample. This is particularly odd, as the tax data I use for my covariates are taken directly from the findings that the authors present in table 1 of their paper.

My results are much closer in the post-recession DD sample. I find a -4.73 percentage point effect of the Kansas tax policy on private sector employment as compared to T&B's 5.35 percentage point effect. The same is true for the per capita and growth rate models. I find 6 jobs lost per 1000, while T&B find 7 jobs lost per 1000 in the per capita model. Similarly, I find a .0135 increase in the employment growth rate, compared to the author's .0116. It's also worth noting that T&B and my replication of their work do not find any statistically significant impact of the tax policy change in the log-linear, per capita, and employment growth rate models in the post-recessionary sample. This leads us to conclude that that the tax policy did not have a meaningful impact in increasing or decreasing employment when comparing years from 2010 to 2014. However, I suspect that this could also be due to the fact that we are only observing the changes over a short-term period. Gains or losses to employment may be lagged relative to the change in tax base by a year or two as more workers shift their incomes to unincorporated pass-through occupations, making it difficult for us to measure exit and entry.

I suspect that a potential source of error in my replication of T&B's DD analysis has something to do with some difference in the fixed effects in our regressions. There are roughly 500 counties in Kansas and its border states and 400 quarter years across the full-time period (four quarters per year over 10 years). I find these coefficients to be highly statistically significant in each model, particularly at the county level. This is somewhat intuitive, because the reason we control for these fixed effects is that we expect the unobservable heterogeneity between counties and quarter years to have an impact on employment. However, in each specification we drop one fixed time and one county fixed effect to account for the collinearity between these covariates. Dropping a different county fixed effect or time fixed effect as the author could alter the results of my findings from the original paper's. A loss of a particularly statistically significant variable could shift around some of the model's explainable variance, especially in small samples of only a few hundred. I am currently in the process of developing a grid search function to find the least statistically significant fixed effects to see if dropping one of these would have an impact in biasing my results. I have also emailed the authors directly about this issue.

My replication of T&B's matching specification was also largely unsuccessful, and is presented in table 4. Indeed, my estimates of the impact of the tax policy change on employment or the number of sole proprietors sometimes differs by a factor of 100 or more, and are also sometimes the opposite direction as the authors'. Moreover, while T&B find no statistically significant impact of the DD estimator on any of their specifications, I find statistically significant results in the employment growth rate, per capita proprietor, and proprietor growth rate models with greater than 95% confidence. There also does not appear to be a consistent deviation between reported coefficients in each model. For example, I find my estimate of the DD coefficient to be one tenth of the authors' finding in the employment log-linear model, and twenty times larger than the authors' estimate of this same coefficient in the per capita employment growth model. A possible explanation for this is that the same sampling error in my DD analyses is also likely to be present in the matching specification. If these effects are compounded with the my less precise border-weight matching, this could be what is biasing my estimates.

Despite the large disparities between T&B's original findings and my replication of their work, it is interesting to note that the results of both the DD and border-county specifications. lead us to similar economic conclusions about the reasonableness of the change in Kansas' tax base. In four of the six DD models, neither mine nor the authors' analyses find a statistically significant effect of changing Kansas' tax base on raising or lowering employment relative to the control group states. In the full-time period per capita specification, I find that the tax policy leads to 6 jobs lost per 1000, while T&B find that it leads to 13 jobs lost per 1000. Similarly, in the same period's log-linear specification, T&B find that the policy leads to a 5.34 percentage point decrease in overall employment, while I find that it had no statistically significant impact. If totally unbiased, my findings imply that the tax policy had a less severe impact on employment than T&B's estimates. Still, neither of our empirical results in the DD sample support the notion that the Kansas Tax policy was a job creator, at least in the short term. This suggests that losses in state revenue due to the narrowing of the tax base may not have been compensated by lawmakers' expectations of statewide growth.

My replication of T&B's matching sample also implies that the tax policy was ineffective, even though it varies substantially from the original findings. I find statistically significant results in three of my border-county matching models. First, I show that a change in the tax base causes the employment growth rate to rise by .23 percent and the proprietorship growth rate to rise by only .02 percent. Similarly, I find that the number of sole proprietorships per capita falls by 8 per 1000 as result of the change in the tax base. This last result is particularly surprising, as the law should incent growth in proprietorship, as independent workers could take advantage of the unincorporated business income exemption. As such, my replication still supports T&B's conclusion that there is "no evidence that the policy has yielded short-run employment gains in the two years since enactment."

Extension: The Effects of Narrowing Kansas' Tax Base on Expenditure

Changes in the tax base have interesting implications for Kansas' public expenditure due to its balanced budget act, which stipulates that spending be reduced dollar for dollar if revenue fall. However, T&B point out that this didn't actually happen in the years immediately after Kansas' new tax law went into effect because of a slight increase in the state sales tax (and associated revenues) and the state spending of its "rainy day fund." Moreover, they note that funds were re-allocated from certain expenditure segments to more critical budget areas, so that the state only saw a modest decrease in its general fund (Turner and Blagg 2017, 10). As such, my extension evaluates what happened to state level expenditures to education, hospital, housing and community development, police, and fire departments over this same period.

Interestingly, Kansas' spending patterns have varied substantially over the five years that the change in tax policy was in place. In the first year the bill was introduced, the legislature actually increased spending by $400 million (Brown 2018). This occurred even as state revenues fell by $700 million, which is roughly 10% of the state's $6 billion general fund. This could have a particularly profound impact, as the state already spends slightly below the national average on several public expenditures (Gleckman 2017). For instance, Kansas ranks 42nd in housing expenditures, 40th in fire department funding, and 24th in education spending (Urban Institute 2018). The state took several measures to rectify this gap in spending after the policy was introduced, including depleting the state highway fund, halting construction projects, and cutting pensions and Medicaid. The state also consolidated several school districts and changed the ways it funds education in the wakes of these cuts (Hobson 2017). This would cause several school districts to close early in 2015 after K-12 funding fell by 13% relative to 2008 levels (Leachman, 2017). The Kansas Supreme Court later ruled that the legislature fell "short of its constitutional duties to provide enough money for K-12 funding and divide the money fairly" (Brown 2018).

I use the same DD framework (equation (1)) as T&B to test the causal impact of the change in Kansas' tax base on public expenditures from 2010-2014. Here, I extract annual expenditure data from the Census' annual State and Local Government Finance (S&L) survey and use that as my key outcome variable of interest. From this dataset, I select funding allocated to K-12 education, housing and community development, hospitals, local police and fire departments, measured in thousands of dollars. However, there are some important caveats to note in the construction of this panel. First, component expenditure does not always occur at the county level. For example, education spending goes through school districts, rather than individual townships, cities, or county governments. To account for this, I group each observation by county and total their expenditures to get a sense of the level of spending per each county. Second, the S&L sometimes misses observations in a given year or county, leading some of these samples to be rather small and incomplete. This is made worse by the fact that the S&L did not report county level data, only local totals in years 2010 and 2011. So, my sample is restricted further to 2012-2014, only one year before the policy change and two years after it. This causes the sample sizes I use in each specification to vary widely from one another. The sample I use to measure the effect of the Kansas tax law on housing and community spending has 1,752 observations, while my hospital expenditure sample has just 480 observations. These low sample sizes widen the confidence intervals of my estimates, and may bias the results in some cases.

Table 5 presents my findings from the census expenditure studies across a linear model and a log-linear model for each outcome variable of interest. In the linear model, only educational and hospital expenditures exhibit statistically significant results, albeit at the 99% confidence level. However, these numbers are quite high, which leads me to believe that they may be biased upwards due to the sample size issue. The education model shows that the change in tax base caused state level education expenditure to decrease by over $240 million.

Conversely, the data shows that hospital expenditures rose by $270 million dollars as a result of the tax cuts. These results could be plausible, as the state could redirect funds and increase or decrease component expenditure as taxable revenue falls. However, the large scale of magnitudes in my results relative to the small sample size makes me somewhat unconvinced of their validity. Indeed, the directionality of my specifications are not always consistent. My log-linear model shows that education funding should have increased by 58 percentage points due to the tax policy even though my linear specification shows it causes a $240 million decrease. Similar counterintuitive relationships appear when examining the impact of the various tax rates on education and hospital spending. For example, the results show that decreasing the marginal tax rate by a percentage-point leads to a $271 million increase in educational expenditure, while a percentage-point increase in the marginal tax rate leads to a $187 million decrease. By contrast, the model also shows that a percentage point increase in the corporate tax rate decreases hospital expenditures by $77 million, while a percentage point increase in the individual marginal tax leads to a $206 million increase. To interpreting these results in context, consider that Kansas' total expenditure is around $5-$6 billion per year (Turner and Blagg, 2017). So, these changes of $200-$300 million correspond to a 3-4% swings in expenditure over this period.

Because of the odd results I found in the S&L sample, I also run an additional extension focused just on examining the causal effect of restricting the Kansas tax base on different types of educational expenditure. Understanding this relationship in the short term is particularly important, due to the fact that the state Supreme Court ruled that later funding cuts to schools were unconstitutional (Brown 2018). For these specifications, I use the Public Elementary-Secondary Education Finance data taken from Census' Annual Survey of School System Finances. These data are available across my entire time frame from 2010-2014. I merged them with the S&L metadata by census id code to obtain the relevant county identifiers for each observation, and then merged with my main DD sample. I use total expenditure, total current spending, and total current spending per pupil (all measured in thousands of dollars) as my outcome variables of interest, and employ both a linear and log-linear specification to test for causality.

The results of my education specific regressions are presented in table 6, where they show that the state had a statistically significant increase in per pupil and current spending, but not in total educational expenditure. Both log-linear estimates show that per pupil and current spending in Kansas increased by about 9 percentage points due to the change in the state's tax law. Because education is an essential public service, this could suggest that the increase in spending was the result of Kansas quickly reallocating funds from other areas as state revenue fell. Results in the linear model demonstrate similar results. The tax policy induces a $3.4 million increase in total per pupil spending and a $9 million increase in total current education spending relative to other states in the years 2011-2014.

Conclusion

My replication of Turner and Blagg's study of the effect of Kansas' tax cuts on short term employment generally fails to replicate the authors' findings. Still, low levels of statistical significance across both of our analyses lead us to similar conclusions that restricting the state tax base had a negligible impact in increasing employment in both the difference in difference and border-county matching samples. Moreover, my extension of the author's difference in difference model finds that at least in the short term, the policy did have an impact on increasing and decreasing certain state expenditures.

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Tables and Findings

Table 1: Comparison of Sample Characteristics

	Mean		Standard Deviation		Minimum		Maximum			
	Author	Replication	Author	Replication	Author	Replication	Author	Replication	Pooled SE	T-Test
Panel A: All counties in five state panel data sample										
(a) Years 2004-2014										
Private Employment	15,654	15,646	51,594	51,722	10	10	577,428	585,045	51,658	0
Population	42,287	42,384	106,346	106,598	428	428	1,010,712	1,007,813	106,472	0
Sales Tax Rate	4.67	4.67	0.95	0.95	2.9	2.9	6.3	6.3	0.95	0.3916
Individual top MTR	5.99	6	0.76	0.76	4.63	4.63	7	7	0.76	1.0527
Corporate top MTR	6.51	6.51	1.01	1.01	4.63	4.63	7.81	7.81	1.01	0.1134
(b) Years 2010-2014										
Private Employment	15,655	15,650	51,357	51,595	12	12	559,478	569,369	51,476	0
Population	43,531	43,562	109,941	110,073	451	454	1,001,972	1,001,207	110,007	0
Sales Tax Rate	4.77	4.77	1.05	1.05	2.9	2.9	6.3	6.3	1.05	0.1883
Individual top MTR	5.82	5.82	0.78	0.78	4.63	4.63	6.84	6.84	0.78	0.4118
Corporate top MTR	6.47	6.47	0.98	0.98	4.63	4.63	7.81	7.81	0.98	0.3133
Number of Proprietors	5,746	6,190	13,655	15,390	141	113	140,075	136,844	22,187	1
Panel B: Kansas county-border match sample 2010-2014										
Private Employment	12,146	12,095	46,335	46,108	230	229	311,161	311,953	46,221	0
Population	29,750	29,753	89,110	89,171	1,234	1,254	576,009	573,024	89,141	0
Sales Tax Rate	6.04	6.04	0.37	0.38	5.3	5.3	6.3	6.3	0.37	0
Individual top MTR	5.81	5.81	0.78	0.78	4.8	4.8	6.45	6.45	0.78	0
Number of Proprietors	4366	4243	13274	13537	639	448	89461	91695	13405.98	0.0917

Table 2: Trend Test

Interaction	Coefficient (Standard Err)	Interaction	Coefficient (Standard Err)	Interaction	Coefficient (Standard Err)	Interaction	Coefficient (Standard Err)
2005*KS	-534.3137 (5755.1715)	2005*MO	-332.3442 (5659.6186)	2005*NE	-501.9070 (5894.1057)	2005*OK	-301.8301 (6138.6626)
2006*KS	-1091.9374 (5755.1715)	2006*MO	-781.7291 (5659.6186)	2006*NE	-1139.1535 (5894.1057)	2006*OK	-581.1578 (6138.6626)
2007*KS	-1525.5586 (5755.1715)	2007*MO	-1351.4892 (5659.6186)	2007*NE	-1697.3776 (5894.1057)	2007*OK	-961.5421 (6138.6626)
2008*KS	-1723.5733 (5755.1715)	2008*MO	-1664.4632 (5659.6186)	2008*NE	-1908.6973 (5894.1057)	2008*OK	-923.2302 (6138.6626)
2009*KS	-516.3096 (5755.1715)	2009*MO	-943.2237 (5659.6186)	2009*NE	-476.9874 (5894.1057)	2009*OK	-22.2340 (6138.6626)
2010*KS	-168.8250 (5755.1715)	2010*MO	-710.0252 (5659.6186)	2010*NE	-3.7101 (5894.1057)	2010*OK	278.2811 (6138.6626)
2011*KS	-575.8681 (5755.1715)	2011*MO	-1065.9175 (5659.6186)	2011*NE	-453.6142 (5894.1057)	2011*OK	149.1559 (6138.6626)
2012*KS	-1290.3292 (5755.1715)	2012*MO	-1702.5842 (5659.6186)	2012*NE	-1096.2238 (5894.1057)	2012*OK	-304.2086 (6138.6626)
2013*KS	-2035.2515 (5755.1715)	2013*MO	-2332.0104 (5659.6186)	2013*NE	-1875.0980 (5894.1057)	2013*OK	-955.0266 (6138.6626)
2014*KS	-3069.5629 (5755.1715)	2014*MO	-3249.0889 (5659.6186)	2014*NE	-2970.6394 (5894.1057)	2014*OK	-1892.4744 (6138.6626)

Note: *** p<.01, ** p <.05, *p<.1

Table 3: Difference in Difference Estimates

	All Years 2004-2014						All Years 2010-2014					
	Log -Linear (1)		Per Capita (2)		Growth Rate (3)		Log-Linear (4)		Per Capita (5)		Growth Rate (6)	
	Author	Replication	Author	Replication	Author	Replication	Author	Replication	Author	Replication	Author	Replication
KPOST2012	-.0534**	-0.0142	-.0133**	-0.0061**	0.0028	0.0041	-0.0535	-0.0473	-0.0074	-0.0061	0.0116	-0.0140
	(0.0156)	(0.0097)	(0.0047)	(0.0025)	(0.0075)	(0.0075)	(0.0341)	(0.0371)	(0.0051)	(0.0054)	(0.0076)	(0.014)
Corporate Tax Rate	-.0909*	-0.0931*	-0.0131	-0.0129	0.0094	0.0129						
	(0.0532)	(0.0561)	(-0.0134)	(0.0136)	(0.0158)	(0.0129)						
MTR	-.0283***	-0.0078**	-.0058**	-0.0024	0.0049	0.0050	-.0365*	-0.0332	-0.0048	-0.0042	.0102**	0.0110
	(0.007)	(0.0037)	(0.0022)	(0.0015)	(0.0032)	(0.0038)	(0.0211)	(0.0228)	(0.0031)	(0.0034)	(0.0051)	(0.0095)
Sales Tax	-.0099**	-0.0085	-.0044*	-0.0034***	-.0076*	-0.0047**	0.0040	-0.0024	-0.0009	-0.0003	-.0077**	0.0034***
	(0.0045)	(0.0058)	(0.0008)	(0.0006)	(0.0019)	(0.0021)	(0.006)	(0.0086)	(0.0011)	(0.0017)	(0.0034)	(0.0004)
Population	.011**	0.0139***			0.0001	0.0005	.014***	0.0183***			0.0006	-0.0013
	(0.004)	(0.0037)			(0.0004)	(0.0005)	(0.003)	(0.0037)			(0.0007)	(0.0023)
Observations	19,976	19,976	19,976	19,976	19,522	19,522	9,080	9,080	9,080	9,080	8,172	8,626
County FE	X	X	X	X	X	X	X	X	X	X	X	X
Quarter Year FE	X	X	X	X	X	X	X	X	X	X	X	X
State Time Trends	X	X	X	X	X	X	X	X	X	X	X	X

Table 4: County-border Match Sample, 2010-2014

	Establishment Employment						Number of Proprietors					
	Employment (1)		Per Capita (2)		Growth Rate (3)		Proprietors (4)		Per Capita (5)		Growth Rate (6)	
	Author	Replication	Author	Replication	Author	Replication	Author	Replication	Author	Replication	Author	Replication
KSPost2012	1431.27	-156.39	-.221	-4	.0179	23.39**	-270.52	-43.81	-.0150	-0.81**	.0496	1.81***
	(1241.47)	(2.7363)	(.0567)	(0.1038)	(.0412)	(0.1080)	(336.99)	(0.8815)	(.0324)	(0.0033)	(.1150)	(0.0068)
Individual Top MTR	803.2	-71.53	-.148	-1.18	.0128	10.46**	-213.56	-39.23	-.0118	-0.0023	.0256	02.04*
	(797.57)	(1.2334)	(.0350)	(0.0474)	(.0224)	(0.0449)	(206.27)	(0.6434)	(.0199)	(0.0029)	(.0712)	(0.0112)
Sales Tax Rate	10.16	92.83	-.00003	4.48	.0143	-10.68**	-38.53		-.0013	1.91***		
	(217.06)	(1.3956)	(.0059)	(0.0502)	(.0139)	(0.0469)	67.66		(.003)	(0.0032)		
Population	75.6*	0			-.000005	0.0000			23.68*	-0.0000	-.07	-0.16*
	(3.4)	(0.0000)			(.00003)	(0.0000)			(1.26)	(0.0001)	(.0001)	(0.0009)
County FE	x	x	x	x	x	x	x	x	x	x	x	x
Quarter-year FE	x	x	x	x	x	x	x	x	x	x	x	x
Sample Size	800	800	800	800	760	760	200	200	200	200	160	160

*Note: Coefficients Multiplied by 100. Errors Clustered at the County Level. *** p<.01, ** p< .05, * p< .01*

Table 5: Effects of Tax Change on Expendiutres

	Education		Housing		Hospitals		Fire Departments		Police	
	Linear	Log-Linear	Linear	Log-Linear	Linear	Log-Linear	Linear	Log-Linear	Linear	Log-Linear
KSPost2012	-241591.2130***	0.5816***	-7919.1150	-7.6475***	276263.9868***	3.7757**	70723.2772	-6.2134***	18931.8204	-1.8666
	(23202.5862)	(0.0598)	(12624.1259)	(1.4439)	(106699.5149)	(1.8320)	(52510.0136)	(1.2286)	(53780.5174)	(2.3578)
Corporate Top Marginal Tax Rate	271037.1133***	-0.9545***	10253.4991	6.4334***	-76672.9845**	-1.9391***	-61621.6008	5.1817***	-4638.3403	2.7831
	(24813.1455)	(0.0699)	(10580.3331)	(1.2021)	(34492.3874)	(0.3865)	(54568.3990)	(1.3300)	(57992.1920)	(2.6104)
Individual Top Marginal Tax Rate	-187272.1629***	0.4459***	-6053.7762	-4.9765***	206130.0678**	2.4461	51799.2611	-5.0550***	12614.9333	-1.6884
	(17624.4741)	(0.0511)	(9846.6512)	(1.0926)	(83857.3163)	(1.6486)	(38787.8801)	(0.9781)	(40510.6684)	(1.8724)
Population	32459.5829***	0.0252***	835.0495	0.4177***	53828.5089***	0.5830	-2157.6537	0.5396***	2113.9920	0.3070*
	(1555.7505)	(0.0053)	(800.2403)	(0.0877)	(17501.6588)	(0.3869)	(3315.2166)	(0.0937)	(3546.6561)	(0.1747)
State Sales Tax	194671.8350***	-0.4772***	6197.4004	3.3485***	-116826.2468*	-0.8881	-45298.7352	5.3736***	-6186.7035	2.8968
	(17536.8590)	(0.0696)	(10512.8099)	(1.1861)	(68683.1739)	(1.9543)	(34420.8988)	(1.1598)	(38358.2486)	(2.0662)
Observations	1359	1359	1752	1752	440	440	980	980	1108	1108
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Error's are clustered on state. *** p< .01, ** p<.05 , and *p <.1

Table 6: Effects of Tax Policy Change on Educational Expendiutre Variables

	Total Educational Expenditure (1)		Total Per Pupil Current Spending (2)		Total Current Education Spending (3)	
	Log-Linear	Linear Model	Log-Linear	Linear Model	Log-Linear	Linear Model
KSPost2012	-0.0278	6876.8844	3420.3287***	0.0933**	0.0995***	8738.6920***
	(0.1556)	(18117.3609)	(1194.2962)	(0.0467)	(0.0193)	(2242.9269)
Population	0.0297***	19492.3883***	-30.3790	-0.0087**	0.0262***	18697.2504***
	(0.0062)	(3098.1233)	(207.9145)	(0.0044)	(0.0072)	(2995.8096)
Sales Tax Rate	-0.0772***	-4505.2784**	-1174.6840***	-0.0311***	-0.0716***	734.8672
	(0.0134)	(1838.7558)	(236.6130)	(0.0063)	(0.0059)	(2565.5488)
Top Individual Marginal Tax Rate	-0.0083	5879.1983	2549.4552***	0.0723**	0.0832***	6162.8107***
	(0.1050)	(12016.7044)	(815.4394)	(0.0317)	(0.0128)	(1605.4751)
Top Corporate Marginal Tax Rate	-0.9781***	24669.5004	-10629.6389***	-0.4440***	-1.1091***	48232.7184
	(0.1360)	(45648.1557)	(3821.3546)	(0.0876)	(0.1186)	(50284.3464)
Observations	2260	2260	2260	2260	2260	2260
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Time Trends	Yes	Yes	Yes	Yes	Yes	Yes