THE LABOR INCIDENCE OF CAPITAL TAXATION: NEW EVIDENCE FROM THE RETAIL SALES TAXATION OF MANUFACTURING MACHINERY AND EQUIPMENT

John L. Mikesell and Justin M. Ross

This paper seeks to produce evidence on the labor incidence of the taxation of machinery and equipment purchases by manufacturers under the state general sales tax. For the identification strategy, we exploit tax policy discontinuities among adjacent counties along state borders. The main results demonstrate that, on average, there are no significant losses or gains to manufacturing labor from adjusting this tax. The main results are robust to specifications of controls and state specific time trends. The identification strategy also passes a falsification test where counties are differed from a randomly selected county.

Keywords: sales tax, capital income tax, tax incidence
JEL Codes: H25, H71

1. INTRODUCTION

There has long been substantial interest in the labor incidence of business tax instruments, both from economists in the study of economic incidence of partial factor taxes and from policy makers who worry these taxes are “job killers.” In the case of taxes on the formation of capital inputs, previous literature has mostly been limited to the study of corporate income taxes (Gravelle, 2013). This literature follows in the tradition of Harberger (1962) heavily cited general equilibrium model, “The Incidence

1 See a recent symposium review of the incidence of CIT in the March 2013 issue of the National Tax Journal. For distributional analyses of proposed tax reforms, both the Joint Committee on Taxation (2013) and the Congressional Budget Office assume that 25 percent of the CIT burden will fall upon labor, whereas the Department of Treasury on average allocates about 18 percent of the burden to labor.

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of the Corporate Income Tax,” which treats capital and labor as the only two factors of production, so that income could only flow to the non-labor sector through a return on capital, with the consequence that a tax upon capital in the corporate sector could be viewed equivalently as a single-sector capital income tax. However, the corporate income tax (CIT) is simply a tax on the value-added of production (i.e., profit) rather than a literal tax on a single factor of production used within an industrial sector.\(^2\) A firm may increase its profits (and, therefore, CIT liability) by investing in worker training, using resources more productively, or successfully investing in the discovery of new consumer markets for their product. Representing these distinctions in the original Harberger model can be illustrated in the stylized expression of after-tax profit (\(\pi\)) for a firm in sector \(c\):

\[
\pi_c = (p_c F(L_c, K_c) - w L_c - r(1 + \tau_K)K_c)(1 - \tau_c).
\]

The Harberger model (and subsequent literature) discusses the intuition of the CIT (\(\tau_c\)) as if it is tax \(\tau_K\) applied to the acquisition of capital (\(K\)). While both taxes can raise the firm’s cost of production and discourage corporate investment, the partial input tax \(\tau_K\) induces a relative price differential with labor (\(L\)) that potentially causes it to become cheaper relative to capital. While the CIT is a favored instrument for adjustment by policy makers in developed countries and states, and for that reason alone the policy instrument is worthy of the scholarly research it has attracted, the empirical evidence on CIT does not readily extend to other taxes that more literally apply to the factors of production.

This paper joins a much smaller literature by investigating a sector-specific tax on capital acquisition that is much closer to the theoretical models in the Harberger tradition than the CIT. Specifically, this paper studies state taxation of manufacturing machinery and equipment (MME) purchases on manufacturing labor employment and wages from 2001 through 2011. Unlike the CIT that is applied to firm profits, the MME is a tax applied to the purchase of capital inputs used in production. The empirical strategy estimates the causal effect of the MME tax by exploiting policy discontinuities in counties along state borders. The border differencing strategy robustly eliminates strong negative labor market impacts found in a panel fixed effects approach. A series of careful falsification checks and other tests are performed to provide evidence that the border differencing strategy is eliminating the predicted endogeneity bias, as opposed to being driven by reconstructions of the sample.

The subject of this study is also of direct relevance to policy debates on the inclusion of business inputs in the state general sales tax. Contrary to the common perception in economics as a tax on household consumption, a sales tax administered at the retail level emphasizes “finished” rather than “final” goods.\(^3\) Cash registers, fuel, hammers,

\(^2\) This is a distinction that Harberger (1962, 2006) has made repeatedly in his work.

\(^3\) By providing a tax credit to sellers engaged in business-to-business sales, the administration of an invoice credit VAT is widely considered superior to the RST for its ability to avoid business inputs in the effective tax base (Zodrow, 1999).
production equipment and machinery, and electricity are all examples of goods that may be “finished” and, therefore, expected to be subject to the retail sales tax (RST) even when they are acquired by businesses in further pursuit of profits. It is of no small concern because the RST yields more than 40 percent of tax revenue in ten states and average 32 percent of tax revenue among the 45 states that levy such a tax (Mikesell, 2012), in part because about 40 percent of expenditures subjected to the RST consist of business purchases — equipment, machinery, consumables, fuel, etc. (Ring, 1989, 1999; Mikesell, 2005. While the empirical literature on state business taxation has mostly investigated those on corporate net income, taxes on business inputs typically to firms of all types and, in 2014, accounted for almost twice as much state revenue.\(^4\) Figure 1 demonstrates this by reporting the split of about $373 billion in state business

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\(^4\) Based on authors calculations from data provided in Ernst & Young (2015).
tax revenues according to their source with 38.6 percent coming from general sales and use taxes on business inputs, compared to just 15.1 percent from corporate income. In fact, estimates from 2013 indicate that the MME tax accounted for $65.7 billion in state tax revenue in the 13 states still levying that tax, which significantly exceeded the $53.3 billion in combined state and local CIT revenues from the entire nation.5

The tax emphasized in this policy is of particular interest to policy makers, in part because of the attention paid to manufacturing among the states. Manufacturing machinery and equipment is a typical example of the “finished goods” that, by default, are included as part of the sales tax base unless otherwise specified. By increasing the required rate of return on a marginal investment in the amount of the tax rate, the MME tax is frequently accused of discouraging manufacturing investment and firm location. In 2013, the Florida Chamber of Commerce and the Manufacturing Association of Florida issued statements about eliminating the extension of the state sales tax to machinery and equipment. Gubernatorial candidates for 2014 in both Georgia and Alabama made an exemption of MME from the sales tax a policy platform in their campaign (see Oliver, 2013; Brunori, 2014).6

In the section that follows, we review the literature on capital income tax incidence and the expansion of the sales tax onto business inputs. In Section III we discuss the MME tax and describe the theoretical basis for the econometric model. It is also in this section that we propose a border discontinuity approach to identify the causal effect of the tax. Section IV presents the results and interpretation, with concluding discussion appearing in Section V.

II. RELEVANT LITERATURE

By using the state general consumption tax’s application to machinery and equipment purchases, this paper touches upon two seemingly disparate literatures — the incidence of the CIT and the taxation of business inputs under the sales tax. The contemporary literature on the labor incidence begins with Harberger (1962) general equilibrium model of a tax on capital in the corporate sector. This static model used two-sectors, corporate and non-corporate, with two output goods and two product inputs in a closed economy operating in perfect competition. The results demonstrated the incidence of

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5 This estimate was shared by Daniel Mullins, National Director, State and Local Tax Policy Economics, Ernst & Young, in email correspondence (June 28, 2015). The estimates imply 9.8 percent of all business tax revenue in 2013 came from the MME tax in the United States, and 24.3 percent of all business taxes in the 13 states still levying the tax as of 2013. See Phillips et al. (2012) for discussions of methodology.

6 The MME exemption typically works through a suspension certificate mechanism. The purchasing manufacturer provides the vendor with a certificate indicating that the item being purchased is, using the language from the Connecticut system, “Machinery to be used directly in the manufacturing production process” (Connecticut CERT-101) and the vendor then does not collect tax. On a similar purchase in a state without the MME exemption, the vendor would collect the tax at the prevailing statutory rate. Similar suspension procedures apply for other business input purchases, like ingredients, component parts, utilities, and so on that would otherwise be taxable for any purchaser.
the tax to depend upon the choice of assumptions over elasticities and factor shares, but that the most realistic assumptions would likely be that capital would bear the majority of the burden. The theoretical literature to follow Harberger (1962) largely modified assumptions that consequently resulted in the burden being shifted back to labor, particularly by expanding the number of sectors and moving to an open-economy model with tradable and non-tradable sectors (e.g., Harberger, 1995). Dynamic models with intertemporal models with neoclassical growth functions similarly tended to reinforce the perspective of partial factor taxes on capital being shifted to labor, particularly by reducing capital to labor ratios that lowered the marginal product of labor and wages in turn (e.g., Feldstein, 1974a; Feldstein, 1975b). Models of imperfect competition tended to similarly find means for partial factor inputs to escape the burden, such as Katz and Rosen (1985) implications of an oligopoly model that resulted in overshifting onto consumers and increases in firm profits. More recent theoretical work combining open borders with imperfect markets has demonstrated something of a comeback for the expectation that the tax will be borne by capital, either foreign or domestic (Gravelle and Smetters, 2006).

The empirical literature on subnational CIT incidence largely finds evidence consistent with labor sharing in the CIT burden, but not exclusively so. Using a pooled cross section of state data from 1970 to 2007, Carroll (2009) finds a one dollar increase in state and local revenue from corporate taxes is associated with a $2.5 decline in total real wages. Felix and Hines Jr. (2009) find firms with unionized labor captured about 54 percent of the reduced CIT burdens through lower wages in a cross section regression from 2000. Liu and Altshuler (2013) use individual level data in a two stage selection model for employment participation that uses nonwork income as an identifying instrument and finds that the average labor share of the corporate tax burden was about 60–80 percent. Outside the United States, Fuest, Peichl, and Sieloch (2013) use firm level panel data across more than 11,000 German municipalities from 1998 to 2008 and find a 1 euro increase in the effective corporate tax causes 77 cents decrease in the firm’s total wage bill. Using firm level panel data in nine European countries from 1996 to 2003, Arulampalam, Devereux, and Maffini (2012) find that a $1 increase in the CIT would reduce the total wage bill by 49 cents. By contrast, Dwenger, Rattenhuber, and Steiner (2013) study firm level data from 1998 to 2006 across German municipalities and find evidence that a 1 euro decrease in the CIT burden to reduce the union bargained corporate wage bill by 19 to 29 cents.

The other strand of relevant applied empirical work on input taxes stems from the literature on consumption taxes. Economists have long advanced that a universal single rate tax on household consumption of goods and services will have high potential effi-

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7 Although not fully reviewed here, it can also be argued that research on the deadweight loss of corporate taxation is implicitly evidence of incomplete tax shifting. For example, a recent study by Devereux, Liu, and Loretz (2014) finds that the marginal deadweight loss of the CIT near a significant kink point in the UK tax code is about 29 percent of CIT revenue; since the deadweight loss is neither zero nor 100 percent, it is likely that the corporate capital incidence is positive but not exhaustive.
ciency due to undistorted preferences for savings and production input choice, with
deadweight loss emerging only from income effects in the consumer’s labor-leisure
choice problem. The idea that consumption expenditure represents a nearly-ideal
basis for distribution of the cost of government emerges from Kaldor (1955, p. 47)
observation that consumption measures the household’s own assessment of its capac-
ity to afford goods and services sold by the private sector and, thus, should similarly
measure its capacity to afford services from the public sector. Critically, economists
have continuously emphasized the importance of taxing final household consumption
only, and not to extend the base of the consumption tax to include business purchases
out of concern for the compounding inefficiencies associated with tax pyramiding.8 Tax
pyramiding inefficiencies provide the prima facie case against the taxation of capital
(Mankiw, Weinzierl, and Yagan, 2009). Furthermore, the more complete exclusion of
production inputs with the value added tax (VAT) has been influential in the choice of
this tax rather than a RST by national governments. The underlying presumption of
these debates is that taxing business inputs as part of consumption substantively re-
allocates resources within the production process, subsequently generating inefficiencies
through tax pyramiding and business restructuring to gain price advantage. This turnover
component — widely criticized in the gross receipts tax literature — creates problems
for efficiency and equity of the tax. Applying the tax to pre-retail purchases causes
the tax to apply at varying rates to consumption expenditure (rates being dependent
on the number of pre-retail transactions undergone before the final purchase and the
degree of forward shifting), creates an economic advantage for integrated businesses,
and discourages economic development by adding tax burden to business purchases of
taxed inputs, including equipment and machinery.

Despite their policy and theoretical importance, the empirical evidence on the eco-
nomic consequences of a consumption tax that deviates from the economist’s recom-
modation to avoid taxing business purchases is much less prevalent than research on
corporate income taxation. This is certainly a consequence of the near impossibility
of determining how significant a state’s actual tax policy meaningfully deviates from
the ideal tax base, and then plausibly linking it to the corresponding sectors where the
resource distortions would occur.9 One effort is found in an analysis of the impact of
state business climate on industrial growth by Plaut and Pluta (1983). This study con-
sidered how the effective sales tax rate, as measured by state sales and gross receipts
tax collections as a percentage of gross retail sales (measured by Census of Retail Trade
data), impacted percentage change in real value added, employment, and real capital
stock from 1967 to 1972 and from 1972 to 1977, running pooled ordinary least squares

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8 See Ufier (2014) for a recent study of the effect of VAT on a variety of macroeconomic indicators in a panel
of countries. By and large, the reported effects are found to be consistent with economic theory’s predicted
effects on economic growth and private investment after correcting for endogeneity via propensity score
matching.

9 The sales taxes also exclude a wide array of household consumption expenditures but these impacts are a
topic that is outside the scope of the current research.
(OLS) regressions for the two observation periods. Although they noted that capital goods purchases are subject to tax in some states, they did not attempt to isolate any such impacts or distinguish tax initially paid by business from the tax initially paid by households but used total collections. Their sales tax variable was never significant at normal confidence levels.

Bartik (1989) analyzed the impact of state sales taxes on business purchases on small business start-ups. He focused on the sales tax differential for equipment purchases, measured by \( \ln(1 + t_m) - \ln(1 + t_g) \) where \( t_m = \) tax rate on machinery and equipment and \( t_g = \) standard sales tax rate. His investigation covered three periods: 1976–1978, 1978–1980, and 1980–1982. In pooled estimates, the rate differential was not significantly related to small business start-ups, but panel estimates showed a statistically significant negative relationship. A general sales tax variable \( \ln(1 + t_g) \) was not significant in either estimates. It is not clear, however, why a differential within a state between business purchases and general purchases would influence business activities.

Bird and Smart (2008) examine the investment impact when a number of Canadian provinces switched from retail sales taxes (with an estimated 43 percent of revenue coming from business purchases) to the harmonized sales tax (a VAT linked to the national Goods and Services Tax with virtually full exclusion of business purchases). They found that removing the business purchase burden with the 1997 change in taxes was associated with a 12.2 percent increase in annual machinery and equipment investment above their trend levels, consistent with the view that the RST was distorting capital choices.

While there are number of studies interested in various state and local tax policy effects on manufacturing sector growth and location (e.g., Bartik, 1985; Chirinko and Wilson, 2008; Duranton, Gobillon, and Overman, 2011) with mixed findings, the most directly comparable previous empirical work to this paper comes from another study of the MME tax by Hageman, Bobek, and Luna (2015). They use state level data 1983–2006 on capital expenditures and manufacturing employment, with both OLS and IV/2SLS results indicating that there is a statistically significant and economically modest negative effect of the tax on employment and capital expenditures. They use per capita personal income and the average tax rate in contiguous states as instruments for the MME tax. Similarly, Papke (1991) studies the effect of state subsidies on the purchase of plant and equipment on manufacturing firm births and deaths. Exploiting this variation, their estimates imply that there is a significant effect of high state marginal tax rates.

III. THE TAXATION OF MANUFACTURING CAPITAL PURCHASES AND EMPIRICAL STRATEGY

Table 1 lists the state tax rate applicable to the purchase of machinery and equipment by manufacturers from 2000 to 2011.\(^{10}\) During this period, 16 states levied the MME tax for at least some amount of time, and seven of these states made revisions to these tax

\(^{10}\) The rates are those for the RST plus any gross receipts tax that the state might levy with application to manufacturer purchases of machinery and equipment (Ohio, Washington, and Delaware).
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Note: All rates are state tax rates only, with the exception of California which has mandatory local add-on rate.
Not Listed: States without tax during time period.
rates 28 times. The common history of this tax is that machinery and equipment were part of the “finished” base of taxable goods, and over time some states have targeted that activity for selective treatment with full or partial exemptions in selected industries in selected instances, such as equipment that is used for research and development, or machinery and equipment purchased for a new or expanding plant. For example, Florida Chamber of Commerce in 2013 approved full exemption of machinery and equipment purchased by manufacturing and process companies (but only those in NAICS codes 31–33). Because of the selective treatment, states experience the administrative difficulty of determining whether or not the policy actually applies to a specific producer or producer’s purchase. For instance, equipment exemptions might only be applied to purchases where the goods are “directly used” in “manufacturing,” with some element of ambiguity involved in determining whether or not an entity is considered to be engaged in “manufacturing” and whether or not a use is considered “direct.”

Both statutory terms and judicial interpretations of this language can vary by state (Hellerstein et al., 2009). Some states allow for machinery and equipment to be exempt as long as the property is “integral or necessary” to the manufacturing process, and the capital’s application to the manufactured material is direct (Palms, Pope, and Todorova, 2013). Even with these considerations, the MME tax is as clear a tax on capital purchases as can be found in the tax system, and is much closer to the input taxes around which theoretical tax incidence models are built. This also allows for a more precise determination of the amount of temporal and cross-sectional variation in the tax code that will less likely be prone to measurement error.

A. Econometric Model for Tax Incidence on Labor

The expected theoretical labor incidence of a capital tax depends upon many of the assumptions of the model, but the main elements relevant to the labor market are qualitatively similar to the Harberger (1962) model with intuition that can be applied to the manufacturing labor market. These models generally decompose a change in price of good \(x\) as resulting from the change in price of its factor inputs weighted by their share in production. If the tax on manufacturing capital can be fully shifted forward onto consumers, then the owners of the factor inputs only pay the tax in proportion to their own individual consumption of the good and there will be no observable effect in the manufacturing labor market specifically; labor’s incidence of the tax on capital will then depend on the inability of producers to shift the tax forward onto consumers, and the producer’s share attributed to labor will depend on the elasticity of substitution between product factors and the underlying supply and demand forces for the inputs.

Note that this is different in structure from a VAT, where tax paid on inputs paid by a business represent a credit against tax paid to the government from tax the business collects on sales of its outputs, thus freeing the business of statutory tax burden. The relevant VAT applicability question is whether the purchase was of a business input, not the nature or use of the input. That treatment makes the MME tax rate equal to zero in the VAT structure.
A tax on capital renders substitute inputs cheaper by comparison, which if these are to include labor could result in workers of this sector experiencing higher wages and employment. Such an outcome would suggest that the full burden of the tax would be distributed to capital or perhaps consumers. If the machinery and equipment targeted by the MME tax were labor enhancing complements to production, however, then this would lower the demand for labor and cause workers to share in the incidence of the tax to the extent their labor supply is relatively inelastic. This would predict a negative effect on labor compensation. In the circumstance of a perfectly inelastic supply of labor, then workers would bear the full burden of the tax through lower wages and employment would be unchanged. By contrast, the observation of declining wages and employment would suggest that labor was sharing in the burden along with other factors of production. Declining employment with no effect on wages would suggest the negative demand shock was met with a perfectly elastic labor supply curve, so that labor flows out of manufacturing in the taxed area into other sectors and fully escapes the tax.

The intention here is to estimate the effect of the MME tax on manufacturing labor markets, rather than to specify a model that can determine where the burden does rest. The latter requires numerous identification strategies and an agreed upon general equilibrium model to parameterize. To explore the main elements of taxing capital in manufacturing labor market effects only, we depict a representative firm with production facilities in state $s$ whose output is sold on a national or global market. Their after tax income can be written as

$$\pi = [pF(L, K) - w(1 + \tau_w)L - r(1 + \tau_k)K](1 - \tau_f - \varphi \tau_s + \varphi \tau_s \tau_f),$$

with taxes on labor ($\tau_w$), capital ($\tau_k$), federal income ($\tau_f$), and state income ($\tau_s$) as determined by apportionment rules ($\varphi$). The first order conditions with respect to the choice variables are

$$\frac{\partial \pi}{\partial K} : (pF_K - r(1 + \tau_k))(1 - \tau_f - \varphi \tau_s + \varphi \tau_s \tau_f) = 0;$$

and

$$\frac{\partial \pi}{\partial L} : (pF_L - w(1 + \tau_w))(1 - \tau_f - \varphi \tau_s + \varphi \tau_s \tau_f) = 0.$$  

After rearrangement, these expressions yield the familiar $F_K = \frac{F_L}{w(1 + \tau_w)}$ implicitly defining the optimal input mix of labor and capital, with the overall profit function serving as a participation constraint. This reveals the substitution effect, as an increase in capital's relative price leads to a substitution of capital for labor.

---

12 State income apportionment formulas depend on the geographic location of the products’ sales, the workers, and capital assets with differing weights. If $\varphi$ is taken as a function of capital and labor, then application of the product rule in the derivative causes the expression to become

$$\frac{\partial F}{\partial K} = \frac{\partial pF_K}{\partial K} = \frac{\partial p}{\partial K} \frac{\partial F_K}{\partial K} + \frac{\partial p}{\partial \tau_s} \frac{\partial F_K}{\partial \tau_s} + \frac{\partial p}{\partial \tau_f} \frac{\partial F_K}{\partial \tau_f},$$

which reveals the same intuition on substitution effects as when apportionment is a simple constant.
in \( \tau \) (which corresponds to the MME tax) will make capital more expensive relative to labor and result in an optimal input mix that is more labor intensive. The additional cost from an increase in \( \tau \) could also cause the firm to cross the participation threshold and shut-down so that the observed effect on labor is to fall to zero. In summary, even at the firm level the effect of the tax is theoretically ambiguous.

Unfortunately, the functional form of production is unknown, and the optimal labor and capital equations will not feature a closed form solution. Instead, the generalized linear approximation of the labor demand equation is adopted and transcribed to fit the pattern of the data. The level of employment in manufacturing (\( EMP \)) and per worker wages (\( Wages \)) is observed in county \( (i) \) in quarterly periods from 2001 to 2011 \( (t) \). The objective of this paper is to obtain an unbiased estimate of \( \gamma \) of the MME tax rate \( (T) \) on these observable labor market outcomes in manufacturing:

\[
\begin{align*}
\ln(EMP_{iqt}) &= \beta_E X_{iqt} + \gamma_E T_{iqt} + \theta_i + \pi_q + \sigma_q + \varepsilon_{iqt}, \\
\ln(Wages_{iqt}) &= \beta_W X_{iqt} + \gamma_W T_{iqt} + \theta_i + \pi_q + \sigma_q + \nu_{iqt}.
\end{align*}
\]

Equations (1) and (2) represent reduced form of supply and demand equations which, by construction, will yield smaller estimates of elasticities than what would be found if the specific supply and demand elasticities were individually disentangled through an alternative identification strategy.\(^{13}\) As such, the point estimates of \( \gamma \) do not directly represent the labor demand or supply elasticity, but rather a market equilibrium response to the tax policy that is presumably the interest to the policy maker concerned with overall manufacturing employment in their state. After inclusion of fixed effects for county \( (\theta_i) \), year \( (\pi_q) \), and quarter \( (\sigma_q) \), any remaining variation that may be correlated with changes in \( T \) should be captured in a vector of controls \( (X_{iqt}) \) that carry non-zero coefficients for obtaining unbiased estimates of \( \gamma \). Certainly, states which introduced changes in the tax rate applicable to MME likely made other policy changes. One possibility is that states which tie the MME tax to their general sales tax rate might experience variation because of a policy choice to rely more/less upon another tax instrument. Individual and CIT rates are, therefore, potential policy changes that occurred among states during the decade of interest which might also affect manufacturing labor markets. As such, the marginal tax rate on individual income and the top marginal rate on corporate income are included as control variables.\(^{14}\) Relatedly, states might alter their rates in response to surplus or deficits in their public finances, which themselves might be related to economic performance and labor markets. Furthermore, over the last 10–15 years states have increasingly increased the weight on sales for the apportionment of corporate income for domestic firms. Finally, as a general control for economic size we include the log of population.

\(^{13}\) See Duncan and Peter (2010) for a representative derivation.

\(^{14}\) For the marginal individual income tax rate, we calculated the state and federal income tax rates that applied to the average observed local manufacturing wages using the National Bureau of Economic Research Taxsim model.
B. Identification Strategy

The observed variation in the treatment effect of interest, the MME tax \( T_{it} \), is not likely to be random with respect to the outcome. Although the specification attempts to include other covariates in \( X \) that partial out the influence of specific ways in which assignment into the treatment was violated, unobserved time-variant influences that determine tax policy and manufacturing outcomes remain a concern. For example, stakeholders within the state may form expectations about the future performance in manufacturing when considering tax policy in the current period, but these expectations are not easily observed or measured. One possibility is to think that local policy makers across the state form expectations of the responsiveness of their local manufacturing sector to a proposed state tax policy change. A similar form of expectation induced bias would occur when a manufacturer would be willing to pay as much as the present value of their expected tax liability to prevent a tax policy change that is unfavorable to their position. In that case, the expectations form from an unobserved tax tolerance function dependent on the local manufacturers’ expectations about the future. Compared to a manufacturer with plans to expand, a manufacturer who expects to discontinue capital acquisition or leave the state entirely in the near future will be less likely to oppose the tax because there is little saved profit from doing so. Either way, it is useful to think of an unobserved term \( \vartheta_{it} \) that is the result of a tax change tolerance function dependent on the expectations in county \( i \) at time \( t \) of future manufacturing activity in the state. Although we can consider this bias a concern for both employment and wages, for brevity we will proceed by examining only the employment specification in Equation (1) by adding the additional unobserved expectations term \( \vartheta_{it} \):

\[
\ln(EMP_{iqt}) = X_{iqt} + T_{iqt} + \vartheta_{it} + \sigma_i + \pi_q + \epsilon_{iqt}.
\]

Because the \( \vartheta_{it} \) in Equation (3) is unobservable it becomes part of the error term. County, year, and quarter fixed effects remove omitted invariant characteristics that might induce selection into the treatment and set the counterfactual to be based upon within county changes in the MME tax.\(^{15}\) These fixed effects, however, cannot produce unbiased estimates of \( \gamma \) if the time varying omitted factor is correlated with policy adoption in \( T \).\(^{16}\)

Any time-varying influence that is correlated with the tax policy changes that is not mitigated by conditioning on \( X \) will bias \( \gamma \). For instance, if states pass MME tax rate

\(^{15}\) Since the state is a time invariant attribute of the counties, any statewide policies or preferences that do not change over time are subsumed by the county fixed effects.

\(^{16}\) Note that cross-state manufacturing sales do not have a cross-border shopping problem in the same way that retail households experience it, and as a result it is the in-state tax rate that matters. Should a manufacturer make a purchase from an out-of-state vendor in a location levying a lower than home-state rate, that purchase would be exempt from the out-of-state tax (assuming the purchase was made in an appropriate fashion and delivery was made by common carrier) and would be subject to the compensating use tax in the home state at the home-state rate. If the lower out-of-state rate were paid for some reason, then the use tax would apply at a rate compensating for the difference between out-of-state and home-state rates; if the out-of-state rate is higher than in-state, then the manufacturers will arrange to take possession within their own lower tax state. In summary, the home-state MME rate is the relevant rate on all transactions.
increases in response to fiscal stress from a declining manufacturing sector and this effect is not adequately captured by conditioning on the observed fiscal surplus variable, then $\gamma$ will carry a negative bias in the reported effect of the MME tax on manufacturing labor demand. Similarly, if statewide there is little political opposition to tax rate increases because localities have negative expectations of future profits, then the resulting increases in state MME tax rates will be observed when expectations about future local profits are likely to be similarly negative, and the persistence of this correlation into labor markets will tend to find associations between increasing tax rates and decreasing employment. In either case the unobserved influence of $\vartheta$ seems likely to introduce a negative bias in the coefficient for the MME tax.

One indirect way to explore the plausible direction of this bias is to examine trends of lagged changes in the dependent variables as determinants of the MME tax. While it does not expose a causal relationship, if rising manufacturing wages and employment at the state level predict a negative change in the MME tax, it would be consistent with a growing industry investing in future protection of profits from local policy changes. We model changes in the MME tax as a function as a series of two quarter differences ($\Delta$), with lag variables appearing for every other quarter back to two years:

$$\Delta_2 T_{sqt} = b_1 \Delta_2 \ln(EMP_{sqt}) + b_2 \Delta_2 \ln(EMP_{sq,t-2}) + b_3 \Delta_2 \ln(EMP_{sq,t-4}) + b_4 \Delta_2 \ln(EMP_{sq,t-6}) + b_5 \Delta_2 \ln(EMP_{sq,t-8}) + \theta_q + \sigma_q + \varepsilon_{sqt}.$$  

Equation (4) is also estimated for wages rather than employment. Although lagged rates of change are not necessarily indicative of future expectations, it is at least an interesting proxy to observe in the trends around these policy discontinuities. Note that seasonality is stripped away with quarter fixed effects, but there is no year fixed effects or trend controls to allow for the purposed correlation to be observable. The coefficients for $b_1 - b_5$ are displayed along with their 90 percent confidence intervals in Figure 2, with Panel A representing the plot for employment and wages in Panel B. Panel A of Figure 2 demonstrates that an increase in manufacturing employment growth in the previous 6 to 18 months (2–6 quarters) reduces the likelihood of observing a change in the MME tax. Alternatively stated, those states which increased their MME tax experienced unusually negative periods of manufacturing employment growth during the previous two years. The point estimates for wages per worker, which can be seen in Panel B of Figure 2, reveal a similar pattern with a lower level of precision. This is evidence that the OLS estimation of labor incidence equations are likely to find the effects of the tax rate that are more negative than the true average treatment effect if the conditioning variables do not remove their influence.

Ultimately, the identification strategy used in this paper will be to exploit local variation because there are good reasons to expect $\vartheta_t$ to vary geographically within the state for reasons related to policy adoption. For example, although the tax is a state policy, policy changes result from a collection of representatives across the state from localized

17 Note that this is a statement about the overall industry, and is not a predictive statement that can be applied to individual firms or subsectors of the industry.
**Figure 2**

Employment and Wages as a Lagging Indicators of MME Tax Changes

**Panel A: Employment**

![Graph showing employment data with points and lines indicating changes over time.](image1)

**Panel B: Wages**

![Graph showing wages data with points and lines indicating changes over time.](image2)

Notes: Point estimate on the coefficient for lagged variables is solid black, with upper and lower bounds of the 90 percent confidence interval reported in dashed black.
legislative districts. A proposed tax increase is likely to be met differently across the state by representatives whose geographically defined districts contain constituent manufacturing interests. An area where no manufacturing base existed would lack an obvious special interest group to encourage their local representative to oppose a tax increase. The approach of this paper to explore manufacturing employment and wage divergences in counties that are along state borders, counties which are depicted with a dark shade in Figure 3. Specifically, a dataset of county pairs that are contiguous along state borders is created, allowing for Equations (1) and (2) to be estimated in terms of differences from a corresponding county in a differing state. Figure 4 provides a stylized example of this contiguous pair creation, where three hypothetical states are divided by a thick bold line with counties identified using alphanumeric characters separated by dashed borders.

There are a handful of other studies which have used state borders as an identification strategy. Border discontinuities have been the basis of studying the effect of Right-to-Work laws on manufacturing employment (Holmes, 1998), manufacturing establishments from local taxes in the United Kingdom (Duranton, Gobillon, and Overman, 2011), American state tax credits (Chirinko and Wilson, 2008), French corporate tax rates on business formation (Rathelot and Sillard, 2008), banking deregulation on economic growth (Huang, 2007), and state minimum wages on labor markets (Dube, Lester, and Reich, 2010; Rohlin, 2011). As each study points out, the identifying assumption for border differencing pairs is that the within-pair difference in the variables of interest is uncorrelated with the error term in expectation. To consider the effect border differencing could have on heterogeneous response bias, consider two counties (i and iN) with contiguous boundaries along a state border, such as counties 1 and A in the stylized diagram of Figure 4. Considering only the tax rate variable and idiosyncratic error from Equation (3) for brevity, this border differencing results in

\[
\ln(EMP_{iqt}) - \ln(EMP_{iN qt}) = \gamma(T_{iqt} - T_{iN qt}) + (\vartheta - \vartheta_{iN}) + (\varepsilon_{iqt} - \varepsilon_{iN qt}).
\]

If the unobservable bias is the same in areas along state borders (\(\vartheta = \vartheta_{iN}\)), then the middle term of the right hand side disappears and the equation reduces to only the local average treatment effect as

\[
(5) \quad \ln(EMP_{iqt}) - \ln(EMP_{iN qt}) = \gamma(T_{iqt} - T_{iN qt}) + (\varepsilon_{iqt} - \varepsilon_{iN qt}).
\]

In essence, the remaining identification assumption is that changes in the state MME tax are conditionally uncorrelated with the unobservable time varying differences that become relegated to the error term. The assumption that the unobserved bias is eliminated with border differencing requires defense for why these areas serve as useful counter-

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18 It is important to note that border discontinuities for the purpose of identification represent a characteristically different form of research from studies where the borders are uniquely interesting, which is extremely common in tax research. The tax induced cross-border shopping and tax avoidance literatures are examples where the bordering counties are uniquely interesting relative to all other non-border counties due to tax discontinuities (e.g., Fox, 1986; Luna, 2004; Tosun and Skidmore, 2007; Agrawal, 2012; Thompson and Rohlin, 2012; Rohlin, Rosenthal, and Ross, 2014).
factuals. The presence of agglomeration economies in manufacturing provides a strong theoretical justification for the assumption that time-varying unobservables are likely to be similar among neighboring counties. Agglomeration economies arise from industries relying on similar factor production needs, knowledge spillovers, and localized externalities that result in increasing returns to scale. There is also an extremely robust empirical literature that supports the theoretical foundations of agglomeration economies that result in industry concentration at localized levels (Rosenthal and Strange, 2004). The empirical literature extends to manufacturing at local levels, which is directly relevant to the purpose of this paper. For example, Rosenthal and Strange (2001) find empirical support for the importance of labor market pooling, knowledge spillovers, and reliance on manufactured inputs as important determinants of agglomeration at low levels of geography for manufacturing industries. Smith, Jr. and Florida (1994) find that Japanese automotive related manufacturing establishments reveal a preference for locations in close proximity to other Japanese automotive assemblers, as well as those areas with a high manufacturing density and educated work force. Using a site selection conditional logit model of U.S. counties, Woodword (1992) finds that a variety

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19 Previous literature employing instrument variables for identification produces coefficients that differ in interpretation and can make cross-study comparisons difficult. For example, Hageman, Bobek, and Luna (2015) use per capita income and average contiguous state tax rates while Liu and Alshuler (2013) use nonwork income as instruments, which results in estimates of how states induced into rate changes by those attributes are affected.
of agglomeration indicators vary in strength and importance depending on the type of local areas for the establishment of automotive manufacturers. For instance, they find that in rural areas highways were particularly important for establishment, while within the automotive corridor, avoiding distressed and urban areas was important. In more developed counties, Woodword (1992) found these manufacturers tended to favor those characterized by industry agglomeration and high concentrations of educated labor. This provides a theoretical a priori support for the assumption that cross-border differencing would eliminate heterogeneous response bias, but for empirical evidence the next section shows that differencing between two randomly selected border counties produces results approximating biased OLS estimates.

The control variables for other potential policy differences that could be correlated with divergences in the MME tax can be returned to Equation (5), as well as quarter, year, and county-pair fixed effects. Using $p$ to represent an identifier for each unique difference of cross-state adjacent counties, the following equation can be estimated:

$$\ln(EMP_{pq}) = X_{pq} \beta + \gamma T_{pq} + \Theta_p + \sigma_q + \pi_t + \epsilon_{pt}. $$

Equation (6) is analogous to Equation (3) except that every variable now represents a cross-border difference from an adjacent neighbor. It is also clear that this process will produce standard errors which are not independent by construction of the pairs. This is demonstrated in Figure 4, where pair 1-A, 1-B, and 2-B each appear as unique observations but share common information owing to the fact that each have multiple opportunities to appear in a pair. For this reason, the estimation of Equation (6) will undergo a bootstrapping procedure on both counties in the pair to account for this mechanical non-independence, as well as for time series autocorrelation.

C. Data

The Quarterly Census of Employment and Wages produced by the Bureau of Labor Statistics serves as the source for the dependent variable data. This data is generated from unemployment insurance reports filed by employers, and as a result it represents a count of employment occurring in county $i$ rather than a count of employed residents in county $i$. The $Wages$ variable represents the average weekly wages paid to an employee in the county. This is a cruder than ideal measure of worker compensation, because observable wage changes might depend on whether or not jobs created or destroyed were above or below the average. The MME tax rates were taken from the Commerce Clearing House Internet Tax Network descriptions of state sales and use tax structures, along with information collected from statutes and regulations of individual states.

The dataset employs counties which were observed in all quarters from quarter 1 of 2001 to quarter 4 of 2011 from all states and Washington, DC. Louisiana is excluded because Hurricane Katrina significantly impacted the observed data while also gradually phased out the MME tax during this period. Alaska and Hawaii are also dropped for the border differencing because they have no cross-state neighbors. Table 2 demonstrates
## Table 2

Selected Indicators for Counties on State Borders

<table>
<thead>
<tr>
<th>Variable</th>
<th>Non-Border Mean</th>
<th>Non-Border Standard Deviation</th>
<th>Border Mean</th>
<th>Border Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Manufacturing sales ($Million)</td>
<td>328.96</td>
<td>254.76</td>
<td>313.80</td>
<td>252.01</td>
</tr>
<tr>
<td>Manufacturing payroll ($Million)</td>
<td>29.87</td>
<td>25.28</td>
<td>29.73</td>
<td>25.53</td>
</tr>
<tr>
<td>Manufacturing establishments</td>
<td>108.70</td>
<td>403.69</td>
<td>90.71</td>
<td>239.85</td>
</tr>
<tr>
<td>Manufacturing employees / EST</td>
<td>35.23</td>
<td>30.00</td>
<td>35.45</td>
<td>29.81</td>
</tr>
<tr>
<td>Employment share (%) in agriculture, forestry, fishing, and hunting</td>
<td>3.93</td>
<td>6.52</td>
<td>3.90</td>
<td>7.53</td>
</tr>
<tr>
<td>Employment share (%) in mining, quarrying, and oil and gas extraction</td>
<td>4.39</td>
<td>9.11</td>
<td>4.59</td>
<td>8.28</td>
</tr>
<tr>
<td>Employment share (%) in utilities</td>
<td>0.96</td>
<td>1.67</td>
<td>1.02</td>
<td>1.32</td>
</tr>
<tr>
<td>Employment share (%) in construction</td>
<td>6.01</td>
<td>3.87</td>
<td>5.82</td>
<td>3.39</td>
</tr>
<tr>
<td>Employment share (%) in manufacturing</td>
<td>16.07</td>
<td>11.52</td>
<td>16.12</td>
<td>11.36</td>
</tr>
<tr>
<td>Employment share (%) in wholesale trade</td>
<td>5.13</td>
<td>3.67</td>
<td>5.05</td>
<td>3.82</td>
</tr>
<tr>
<td>Employment share (%) in retail trade</td>
<td>15.78</td>
<td>4.70</td>
<td>15.88</td>
<td>4.64</td>
</tr>
<tr>
<td>Employment share (%) in transportation and warehousing</td>
<td>4.48</td>
<td>4.20</td>
<td>4.49</td>
<td>3.69</td>
</tr>
<tr>
<td>Employment share (%) in information</td>
<td>1.57</td>
<td>1.23</td>
<td>1.50</td>
<td>1.05</td>
</tr>
<tr>
<td>Employment share (%) in finance and insurance</td>
<td>3.82</td>
<td>2.06</td>
<td>3.86</td>
<td>2.02</td>
</tr>
<tr>
<td>Employment share (%) in real estate and rental and leasing</td>
<td>1.19</td>
<td>0.91</td>
<td>1.20</td>
<td>0.96</td>
</tr>
<tr>
<td>Employment share (%) in professional, scientific, and technical services</td>
<td>3.83</td>
<td>3.78</td>
<td>3.71</td>
<td>3.26</td>
</tr>
<tr>
<td>Employment share (%) in management of companies and enterprises</td>
<td>1.19</td>
<td>1.38</td>
<td>1.18</td>
<td>1.21</td>
</tr>
<tr>
<td>Employment share (%) in administrative and support and waste management and remediation services</td>
<td>4.63</td>
<td>2.89</td>
<td>4.56</td>
<td>2.84</td>
</tr>
<tr>
<td>Employment share (%) in educational services</td>
<td>1.53</td>
<td>1.71</td>
<td>1.69</td>
<td>2.02</td>
</tr>
<tr>
<td>Employment share (%) in health care and social assistance</td>
<td>16.07</td>
<td>7.14</td>
<td>15.92</td>
<td>6.79</td>
</tr>
<tr>
<td>Employment share (%) in arts, entertainment, and recreation</td>
<td>1.66</td>
<td>2.31</td>
<td>1.72</td>
<td>1.98</td>
</tr>
<tr>
<td>Employment share (%) in accommodation and food services</td>
<td>11.25</td>
<td>5.48</td>
<td>11.47</td>
<td>5.96</td>
</tr>
<tr>
<td>Employment share (%) in other services (except public administration)</td>
<td>3.67</td>
<td>2.13</td>
<td>3.60</td>
<td>1.87</td>
</tr>
<tr>
<td>Employment share (%) in public administration</td>
<td>0.10</td>
<td>0.20</td>
<td>0.09</td>
<td>0.21</td>
</tr>
</tbody>
</table>

Sources: Employment share data is based on author’s calculations from the Bureau of Labor Statistics’ *Quarterly Census of Employment and Wages*; other variables computed from the United States Census Bureau’s *2012 Economic Census*. 
some general indicators of industrial mix and manufacturer size for border and non-border counties. The 2012 Economic Census provides the data on the additional manufacturing indicators, and these suggest that border and non-border counties are statistically similar to one another in terms of payroll, sales, and employees per firm in that survey. The Bureau of Economic Analysis (BEA) data for proportion of employment in each of the economic sectors defined at the two digit North American Industry Classification System (NAICS) code level is from 2011, and similarly demonstrates that border counties are very similar to non-border counties in their industrial mix. Table 3 provides the summary statistics for the variables employed in regression analysis by sample group regressed for the full period of study, as well as data sources and variable definitions. Across both tables, the observable indicators suggest that border and non-border counties are statistically similar to one another. The full sample includes all 2,420 counties that met the previously described criteria. Because the main results use the border differenced county pairs, Table 3 also provides these descriptive statistics for the 880 observations that are adjacent to state boundaries as well. Overall, the descriptive statistics suggest that border counties are representative of the full population, and this is similarly observed in both Tables 2 and 3. If every county had just one cross-state neighbor, this would reduce the border differenced sample size to 440 county pairs. However, the final sample is 793 due to some counties having multiple contiguous neighbors.

IV. RESULTS

As a prelude to the main results, Table 4 reports the employment estimates for Equations (1) and (2) on the full sample of counties as well as a sample limited to only those along state borders, and the same is presented in Table 5 for wages. All specifications include quarter, year, and county fixed effects, with standard errors clustered at the state level. In order to infer some sense of how important model specification is to the estimation of the MME tax, specifications differ by inclusion of control variables and state specific time trends. As discussed in Section III, the other control variables are introduced because they represent other important tax policies that were likely to have changed along with the MME tax during the period, so nothing in the empirical design of this paper allows for their direct causal interpretation. The results of Table 4 demonstrate that the point estimates for employment become more negative with the inclusion of control variables, whereas the effect on wages becomes less negative in Table 5. All point estimates are statistically significant at the 1 percent level. These negative effects on labor findings are consistent with the previous literature’s findings on this tax (see Hageman, Bobek, and Luna, 2015).\footnote{If aggregate state values are used instead of the county data, the pattern of findings are similar to those in Table 4 and 5. Without time trends, the point estimate on the effect of the tax on employment is $-1.83$ to $-2.17$ and statistically significant at the 1 percent level without time trends, but just slightly negative and statistically insignificant when state specific time trends are included. For wages, all of the coefficients across specifications are statistically insignificant and fluctuate in sign.} However, the inclusion of state-
<table>
<thead>
<tr>
<th>Variable</th>
<th>All Counties</th>
<th>Border Counties Only</th>
<th>Border Differenced</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Overall Mean</td>
<td>Within Variation</td>
<td>Overall Mean</td>
</tr>
<tr>
<td>Manufacturing employment¹</td>
<td>5,445</td>
<td>2,237</td>
<td>4,922</td>
</tr>
<tr>
<td>Wages per manufacturing worker¹</td>
<td>38,608</td>
<td>5,982</td>
<td>38,639</td>
</tr>
<tr>
<td>MME tax</td>
<td>1.00</td>
<td>0.45</td>
<td>0.98</td>
</tr>
<tr>
<td>Income tax rate</td>
<td>19.63</td>
<td>1.41</td>
<td>19.77</td>
</tr>
<tr>
<td>CIT rate</td>
<td>6.25</td>
<td>1.06</td>
<td>6.51</td>
</tr>
<tr>
<td>Sales apportionment weight</td>
<td>60.68</td>
<td>10.81</td>
<td>56.5</td>
</tr>
<tr>
<td>State surplus</td>
<td>–0.87</td>
<td>5.47</td>
<td>–0.77</td>
</tr>
<tr>
<td>Population¹</td>
<td>116,519</td>
<td>13,492</td>
<td>102,859</td>
</tr>
</tbody>
</table>

Notes: (1) For border differenced sample it is the logged difference of the actual numbers reported, whereas all other figures are untransformed.

**Variable Definitions and Sources:** Manufacturing employment is total employment in NAICS Industry Code 31-33 (Source: QCEW from BLS); Wages per manufacturing worker is average compensation in NAICS 31-33 (Source: QCEW from BLS); MME tax is the rate of taxation on Manufacturing Machinery and Equipment (Source: Authors’ research); “Income Tax Rate” is (federal marginal tax rate)+(state marginal tax rate)-(state x federal rates), where the applicable rates were determined from NBER Taxsim using Wages per Manufacturing Worker Data and assuming head of household tax status with no deductions. CIT rate is the highest marginal tax rate on corporate net income (Source: Tax Foundation); Sales Apportionment Weight is the share of corporate net income in the state determined by their nation-wide sales (Source: Bernthal et al., 2012); Population is county population (Source: U.S. Census). Surplus is the difference in the state government’s revenues minus expenditures, divided by revenues (Source: National Association of State Budget Officers Annual Fiscal Survey of States).
<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Border Counties</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>MME Tax</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-2.86***</td>
<td>-3.30***</td>
</tr>
<tr>
<td></td>
<td>(0.61)</td>
<td>(0.93)</td>
</tr>
<tr>
<td></td>
<td>0.27</td>
<td>0.09</td>
</tr>
<tr>
<td></td>
<td>(0.22)</td>
<td>(0.24)</td>
</tr>
<tr>
<td><strong>Income Tax Rate</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.83***</td>
<td>0.80***</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.14)</td>
</tr>
<tr>
<td></td>
<td>0.00</td>
<td>0.03</td>
</tr>
<tr>
<td></td>
<td>(0.36)</td>
<td>(0.15)</td>
</tr>
<tr>
<td><strong>CIT Rate</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-0.03</td>
<td>-0.03</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.02)</td>
</tr>
<tr>
<td><strong>Sales Apportionment Weight</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-0.03</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.02)</td>
</tr>
<tr>
<td><strong>ln(Population)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.57***</td>
<td>0.62***</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.15)</td>
</tr>
<tr>
<td></td>
<td>0.01</td>
<td>0.12*</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
</tr>
<tr>
<td><strong>State Surplus</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.10**</td>
<td>0.12*</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
</tr>
<tr>
<td><strong>State Specific Time Trend?</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>0.25</td>
<td>0.27</td>
</tr>
<tr>
<td>Yes</td>
<td>0.31</td>
<td>0.34</td>
</tr>
<tr>
<td>No</td>
<td>0.26</td>
<td>0.29</td>
</tr>
<tr>
<td>Yes</td>
<td>0.32</td>
<td>0.36</td>
</tr>
<tr>
<td>Within-R^2</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.00</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>0.71</td>
<td>0.69</td>
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<td></td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>0.73</td>
<td>0.71</td>
</tr>
<tr>
<td>Overall-R^2</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of groups</td>
<td>2,420</td>
<td>880</td>
</tr>
<tr>
<td>Sample size</td>
<td>106,480</td>
<td>38,720</td>
</tr>
</tbody>
</table>

Note: All specifications include county, year, and quarter fixed effects. Standard errors appearing in parentheses are clustered at the state level. Statistical significance reported at the 1% (***) , 5% (**), and 10% (*) level.
Table 5
Regression Results for County Manufacturing Wages, 2001–2012

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Border Counties</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>MME Tax</td>
<td></td>
</tr>
<tr>
<td></td>
<td>–0.65*** (0.13)</td>
<td>–0.50*** (0.08)</td>
</tr>
<tr>
<td></td>
<td>0.40*** (0.11)</td>
<td>0.80*** (0.09)</td>
</tr>
<tr>
<td></td>
<td>–0.54*** (0.10)</td>
<td>–0.36*** (0.09)</td>
</tr>
<tr>
<td></td>
<td>0.31*** (0.14)</td>
<td>0.65*** (0.09)</td>
</tr>
<tr>
<td>Income Tax Rate</td>
<td>1.93*** (0.15)</td>
<td>1.89*** (0.18)</td>
</tr>
<tr>
<td></td>
<td>1.88*** (0.15)</td>
<td>1.81*** (0.18)</td>
</tr>
<tr>
<td>CIT Rate</td>
<td>–0.09 (0.18)</td>
<td>–0.24 (0.21)</td>
</tr>
<tr>
<td></td>
<td>0.05 (0.06)</td>
<td>0.07 (0.11)</td>
</tr>
<tr>
<td>Sales Apportionment Weight</td>
<td>–0.04*** (0.01)</td>
<td>–0.030* (0.02)</td>
</tr>
<tr>
<td></td>
<td>–0.00 (0.01)</td>
<td>0.00 (0.02)</td>
</tr>
<tr>
<td>ln(Population)</td>
<td>0.05 (0.04)</td>
<td>0.07 (0.05)</td>
</tr>
<tr>
<td></td>
<td>0.03 (0.03)</td>
<td>0.08 (0.06)</td>
</tr>
<tr>
<td>State Surplus</td>
<td>–0.03 (0.02)</td>
<td>–0.05 (0.03)</td>
</tr>
<tr>
<td></td>
<td>–0.00 (0.01)</td>
<td>–0.01 (0.02)</td>
</tr>
<tr>
<td>State Specific Time Trend?</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Within-R²</td>
<td>0.52</td>
<td>0.53</td>
</tr>
<tr>
<td>Overall-R²</td>
<td>0.11</td>
<td>0.09</td>
</tr>
<tr>
<td>Number of groups</td>
<td>2,420</td>
<td>2,420</td>
</tr>
<tr>
<td>Sample size</td>
<td>106,480</td>
<td>106,480</td>
</tr>
</tbody>
</table>

Note: All specifications include county, year, and quarter fixed effects. Standard errors appearing in parentheses are clustered at the state level. Statistical significance reported at the 1% (***) , 5% (**), and 10% (*) level.
specific time trends have qualitatively important effects on the results. In the case of employment, the effect of the MME tax goes from being negative and statistically significant at the 1 percent level to being small, positive, and statistically insignificant. State specific time trends also fully reverse the sign on the effect of the MME tax from negative to positive at statistically significant levels. The inclusion or exclusion of the control variables continue to have very little substantive effect across these specifications. The sensitivity of the point estimates for MME tax are consistent with the concern that the selection into varying the tax rate is an important source of bias whose correlation is not being captured by other public finance policy controls. The direction of their swing with the inclusion of trends is also consistent with the theory that pre-adoption expectations influence the persistence of the tax in such a way that the OLS results were biased towards finding a negative impact on the labor market.

Tables 4 and 5 also demonstrate that the same patterns of results appear when regressing these specifications on only those counties which appear along the state border. With employment, the point estimates become a little more negative with the inclusion of control variables, while inclusion of state specific time trends captures some omitted effect. For wages, the inclusion of state specific time trends once again shifts point estimates from negative to positive while retaining statistical significance. The nature of bias that exists in the full sample continues to indicate a presence in border counties.

Next we turn to the main results in Table 6, in which the same specifications are applied to the border-differenced sample. Unlike the previous results of Tables 4 and 5 that were sensitive to specification, the border differenced strategy is remarkably robust to both the inclusion of control variables and the use of state specific time trends. All the results indicate that a 1 percentage point increase in the MME tax is estimated to cause a 1.1 to 1.3 percent increase in manufacturing employment, and the effect lacks statistical significance. The wage elasticity is also negative in all specifications of Table 6 and much closer to zero, ranging from \(-0.15\) to \(-0.25\) in the point estimates. In summary, the results suggest that endogeneity bias in the decision for states to revise rates drives the observed negative correlation between MME tax rates and the labor market outcomes, and that on average manufacturing labor experiences no negative impacts from the tax.\(^{21}\) Because of this rather significant change in results, the rest of this section will propose a series of falsification and robustness tests to determine an appropriate inference of this finding, particularly as it concerns whether the border differencing strategy is eliminating the proposed bias specified in Equation (3) as opposed to reconstructing the sample in some undesirable way.

\(^{21}\) If the sample is reduced to the period prior to the Great Recession (2000 to 2007), the result is qualitatively the same in both the border differenced and full sample specifications. In the main results that employ border differencing, point estimates for the effect of the MME tax are 0.62 to 0.83 for employment and 0.082 to 0.136 for wages, and all lack statistical significance. In the post-recession period (2008 to 2011), the effect ranges from \(-0.44\) to \(-0.73\) for employment and 0.51 to 0.70 for wages, again without statistical significance.
Table 6
Manufacturing Employment and Wage Regression Results for Border-Differenced Sample

<table>
<thead>
<tr>
<th></th>
<th>ln(Employment)</th>
<th>ln(Wages)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>MME Tax</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1.324</td>
<td>1.317</td>
</tr>
<tr>
<td></td>
<td>(0.983)</td>
<td>(0.966)</td>
</tr>
<tr>
<td></td>
<td>1.158</td>
<td>1.152</td>
</tr>
<tr>
<td></td>
<td>(0.897)</td>
<td>(0.847)</td>
</tr>
<tr>
<td></td>
<td>–0.258</td>
<td>–0.232</td>
</tr>
<tr>
<td></td>
<td>(0.377)</td>
<td>(0.405)</td>
</tr>
<tr>
<td></td>
<td>–0.175</td>
<td>–0.151</td>
</tr>
<tr>
<td></td>
<td>(0.329)</td>
<td>(0.329)</td>
</tr>
<tr>
<td></td>
<td>Income Tax Rate</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.747***</td>
<td>0.745**</td>
</tr>
<tr>
<td></td>
<td>(0.250)</td>
<td>(0.355)</td>
</tr>
<tr>
<td></td>
<td>1.854***</td>
<td>1.852***</td>
</tr>
<tr>
<td></td>
<td>(0.132)</td>
<td>(0.127)</td>
</tr>
<tr>
<td></td>
<td>CIT Rate</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.022</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(0.306)</td>
<td>(0.391)</td>
</tr>
<tr>
<td></td>
<td>–0.083</td>
<td>–0.080</td>
</tr>
<tr>
<td></td>
<td>(0.123)</td>
<td>(0.112)</td>
</tr>
<tr>
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<td>Sales Apportionment Weight</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.944***</td>
<td>0.941***</td>
</tr>
<tr>
<td></td>
<td>(0.163)</td>
<td>(0.165)</td>
</tr>
<tr>
<td></td>
<td>0.046</td>
<td>0.045</td>
</tr>
<tr>
<td></td>
<td>(0.056)</td>
<td>(0.074)</td>
</tr>
<tr>
<td></td>
<td>ln(Population)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.067*</td>
<td>0.067*</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.039)</td>
</tr>
<tr>
<td></td>
<td>–0.018</td>
<td>–0.018</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.018)</td>
</tr>
<tr>
<td></td>
<td>State Surplus</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.075</td>
<td>0.073</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.051)</td>
</tr>
<tr>
<td></td>
<td>–0.002</td>
<td>–0.003</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.016)</td>
</tr>
<tr>
<td></td>
<td>State Pair Specific Time Trend?</td>
<td></td>
</tr>
<tr>
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<td>Yes</td>
</tr>
<tr>
<td></td>
<td>0.004</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>0.031</td>
<td>0.034</td>
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<tr>
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<td>Yes</td>
</tr>
<tr>
<td></td>
<td>0.001</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td></td>
<td>0.078</td>
<td>0.082</td>
</tr>
<tr>
<td></td>
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<td></td>
</tr>
<tr>
<td></td>
<td>0.002</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>0.673</td>
<td>0.672</td>
</tr>
<tr>
<td></td>
<td>Overall-R²</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.000</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>0.342</td>
<td>0.342</td>
</tr>
<tr>
<td></td>
<td>Number of groups</td>
<td></td>
</tr>
<tr>
<td></td>
<td>815</td>
<td>815</td>
</tr>
<tr>
<td></td>
<td>793</td>
<td>793</td>
</tr>
<tr>
<td></td>
<td>Sample size</td>
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</tr>
<tr>
<td></td>
<td>35,860</td>
<td>35,860</td>
</tr>
<tr>
<td></td>
<td>34,892</td>
<td>34,892</td>
</tr>
<tr>
<td></td>
<td>35,860</td>
<td>35,860</td>
</tr>
<tr>
<td></td>
<td>34,892</td>
<td>34,892</td>
</tr>
</tbody>
</table>

Note: All specifications include pair, year, and quarter fixed effects. Bootstrapped standard errors appear in parentheses and are clustered on both counties appearing in the pair. Statistical significance reported at the 1% (***) , 5% (**), and 10% (*) level.
A. Subsector Analysis as a Check for Cross-State Wage Equalization

One of the concerns with the findings that there is no effect on overall manufacturing wages is that local labor markets might cross state lines to equalize wages. This is difficult to definitively rule out, but some evidence may be found in the analysis of manufacturing subsectors. The previous results differenced total manufacturing employment between the border paired counties, whereas this differences subsector employment. If it is the case that workers cross state borders for employment, this presumably would be strongest when the employing manufacturers are the most similar. For example, if chemical manufacturers are more similar in labor preferences to other chemical manufacturers than they are to fabricated metal product manufacturing, then wages effects might be especially muted.

Among the 22 three digit subsectors of manufacturing, there were ten subsectors that had at least 70 border pairs that were observed in both industries every year. Table 7 presents the parameter estimate information for the MME tax and the sample size information from each of the regressions, for brevity the results for the other control variables are omitted but available upon request. Since this is exploration of whether the previous findings of no wage effect were driven by border-equalization, the results for wages are of greater interest. The results indicate that some subsectors experience not just statistically significant, but qualitatively large effects on wages that differ in their direction. Food, furniture and related products, and miscellaneous manufacturing subsectors saw increases ranging from 2.18 to 5.73 percent in response to a 1 percent increase in the MME tax; fabricated metal product and printing and related support activities saw comparably large negative effects on their labor markets. Assuming that local labor markets would be most substitutable within subsectors, the collective findings of Table 7 indicate that wage equalization is not occurring so quickly that potential impacts of the tax are capitalized away to prevent the main results of Table 6 from detecting effects. Instead, Table 7 suggests that the policy lever that can be pulled by the state has heterogeneous effects that, on average, are as likely to favor as they are to harm workers.

It is worth noting that finding the results in Table 7 to be heterogeneous across subsectors is not surprising as the effect of the tax is theoretically ambiguous. All theoretical models depend heavily on relative elasticities, and these subsectors might differ in the ability to shift the tax onto consumers or substitute between inputs. Another important parameter in most of the theoretical work is the capital to labor intensity, but there seems to be no discernable pattern at least as measured by the industry ratio of capital expenditures to annual payrolls that is provided in the last column of Table 7.

22 We also attempted to split the sample into a couple of subsamples in order to examine some possible common labor market tests. Samples included (1) both counties in the border pair were in the same Metropolitan Statistical Area (MSA), (2) the counties in the pair were in different MSAs, (3) one county was in a MSA but the other was not, and (4) neither counties were in an MSA. Presumably, sample 1 would be closest to zero while the others would demonstrate a larger difference, but the data were too noisy to generate statistical significance on any variables, including the variable of interest.
<table>
<thead>
<tr>
<th>Industry</th>
<th>Wages</th>
<th>Employment</th>
<th>Capital to Labor Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Point Estimate</td>
<td>Point Estimate</td>
<td>N</td>
</tr>
<tr>
<td></td>
<td>Standard Error</td>
<td>Standard Error</td>
<td></td>
</tr>
<tr>
<td>Chemical manufacturing</td>
<td>1.016 (4.608)</td>
<td>15.984 (11.312)</td>
<td>73</td>
</tr>
<tr>
<td>Food manufacturing</td>
<td>3.411* (1.949)</td>
<td>6.627 (6.806)</td>
<td>138</td>
</tr>
<tr>
<td>Nonmetallic mineral product manufacturing</td>
<td>-0.528 (1.944)</td>
<td>16.060** (6.574)</td>
<td>132</td>
</tr>
<tr>
<td>Plastics and rubber products manufacturing</td>
<td>0.839* (0.488)</td>
<td>-1.263 (3.915)</td>
<td>85</td>
</tr>
<tr>
<td>Machinery manufacturing</td>
<td>-0.199 (2.763)</td>
<td>-19.703** (9.257)</td>
<td>155</td>
</tr>
<tr>
<td>Wood product manufacturing</td>
<td>-0.677 (1.206)</td>
<td>-4.131 (4.592)</td>
<td>146</td>
</tr>
<tr>
<td>Miscellaneous manufacturing</td>
<td>5.733*** (2.189)</td>
<td>12.446 (8.784)</td>
<td>116</td>
</tr>
<tr>
<td>Fabricated metal product manufacturing</td>
<td>-2.360*** (0.737)</td>
<td>-1.581 (2.547)</td>
<td>288</td>
</tr>
<tr>
<td>Printing and related support activities</td>
<td>-4.812*** (1.668)</td>
<td>-0.617 (4.529)</td>
<td>113</td>
</tr>
<tr>
<td>Furniture and related product manufacturing</td>
<td>2.181*** (0.698)</td>
<td>2.351 (4.046)</td>
<td>90</td>
</tr>
</tbody>
</table>

Table 7
Border Differenced Regression Results by Manufacturing Subsector

All regressions employ area fixed effects, year fixed effects, and all independent regressors from Tables 4 to 6, but only the coefficient on the MME tax is reported here. Statistical significance is reported at the 1% (***), 5% (**), and 10% (*) level. Standard errors are clustered by border pair and reported in parentheses. Industry level capital to labor expenditure ratios calculated from 2011 Annual Survey of Manufactures (U.S. Census Bureau) by dividing total capital expenditures by annual payroll.
B. Direction of Bias Check

The findings of Table 6 and the influence of state specific time trends that mitigate the negative OLS results in Tables 4 and 5 of the MME tax on labor is driven by endogeneity in the state policy choice to revise rates. It was posited that the tax policy is only revised in the case when the cumulative resistance exceeds an unobserved threshold that results in a negative bias. In this case, states which actually revised their MME tax during the period would presumably carry a larger latent bias during the sample period. If the sample is reduced to only those 13 cases which changed their tax policy during the period, the OLS estimation would be more negative and so the border differencing would produce a larger swing towards the average treatment effect.\(^\text{23}\) Those results are presented in Table 8 and demonstrate exactly this finding, although statistically they have overlapping confidence intervals with the full sample point estimates. For employment, the full sample with all controls had a \(-3.765\) effect in Table 8 as compared to \(-3.076\) in Table 4. The effect of the MME tax on wages for the full sample in Table 5 was \(-0.538\) but \(-0.667\) in Table 8. After cross-state neighbor differencing, the employment effect is \(2.940\) percent and statistically significant at the 1 percent level in Table 8, larger than the \(1.152\) that was not statistically different from zero in Table 6. The wage effect was statistically insignificant in both Table 6 and Table 8, but the sign has turned from a negative \(0.175\) to a positive \(0.122\).\(^\text{24}\)

C. Falsification Test for Border Differencing

The main results of this paper differed each county by its cross-state contiguous neighbor. The central concern to the results is whether or not the difference in point estimates between Table 6 and Tables 4 to 5 is due to the elimination of a predicted bias, or whether the differencing produced a spurious result. Here a falsification test is considered where each border county is differenced from another county selected at random. As demonstrated in Equation (5), the border differencing has the potential to produce an unbiased estimate of the tax effect if \((\vartheta_i = \vartheta_{iN})\). An abundance of literature on agglomeration economies and spatial competition provide some theoretical support for such an assumption. If, on the other hand, the county was differenced from another randomly selected county instead of its cross-state contiguous county, then this cancellation would not hold. In expectation, \(\mathbb{E}[\vartheta_i - \vartheta_{iN}] = \vartheta_{\text{eq}}\) equation Equation (5) would become

\[
\ln(EMP_{iqt}) - \ln(EMP_{iNqt}) = \gamma(T_{iqt} - T_{iNqt}) + \vartheta_{\text{eq}} + (\varepsilon_{iqt} - \varepsilon_{iNqt}).
\]

\(^{23}\) In the full sample and border county results, these estimates are based on just the states that altered their rates during the period, while the border difference estimates include the variation from counties that bordered those states offering within variation in MME tax rates.

\(^{24}\) To highlight the role of bias from a latent propensity and economize on space, Table 8 presents only the results without the state specific time trends. However, the pattern described continues to fit when comparing the specifications. Also of note is that in this reduced sample, the inclusion of state time trends continues to have large effects on the full sample but no effect on the border difference sample.
Table 8
Regression Results for Counties with Within Variation

<table>
<thead>
<tr>
<th></th>
<th>ln(Employment)</th>
<th></th>
<th>ln(Wages)</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Full</td>
<td>Border</td>
<td>Border Diff</td>
<td>Full</td>
<td>Border</td>
</tr>
<tr>
<td>MME tax</td>
<td>−3.765***</td>
<td>−4.698**</td>
<td>2.940***</td>
<td>−0.667***</td>
<td>−0.391</td>
</tr>
<tr>
<td></td>
<td>(0.966)</td>
<td>(1.840)</td>
<td>(0.983)</td>
<td>(0.153)</td>
<td>(0.279)</td>
</tr>
<tr>
<td>Income tax rate</td>
<td>0.502</td>
<td>1.198***</td>
<td>1.296***</td>
<td>1.884***</td>
<td>2.082***</td>
</tr>
<tr>
<td></td>
<td>(0.505)</td>
<td>(0.439)</td>
<td>(0.323)</td>
<td>(0.370)</td>
<td>(0.375)</td>
</tr>
<tr>
<td>CIT rate</td>
<td>0.071</td>
<td>−0.893</td>
<td>0.615**</td>
<td>0.204*</td>
<td>0.141</td>
</tr>
<tr>
<td></td>
<td>(0.679)</td>
<td>(1.043)</td>
<td>(0.312)</td>
<td>(0.118)</td>
<td>(0.267)</td>
</tr>
<tr>
<td>Sales apportionment weight</td>
<td>0.086</td>
<td>0.155</td>
<td>0.586**</td>
<td>−0.019</td>
<td>−0.004</td>
</tr>
<tr>
<td></td>
<td>(0.100)</td>
<td>(0.129)</td>
<td>(0.286)</td>
<td>(0.020)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>ln(Population)</td>
<td>0.417*</td>
<td>0.485</td>
<td>−0.054</td>
<td>0.166</td>
<td>0.063</td>
</tr>
<tr>
<td></td>
<td>(0.249)</td>
<td>(0.391)</td>
<td>(0.071)</td>
<td>(0.127)</td>
<td>(0.124)</td>
</tr>
<tr>
<td>State surplus</td>
<td>0.220</td>
<td>0.327*</td>
<td>0.025</td>
<td>0.013</td>
<td>−0.041</td>
</tr>
<tr>
<td></td>
<td>(0.135)</td>
<td>(0.190)</td>
<td>(0.095)</td>
<td>(0.075)</td>
<td>(0.076)</td>
</tr>
<tr>
<td>Within-R²</td>
<td>0.345</td>
<td>0.360</td>
<td>0.079</td>
<td>0.590</td>
<td>0.569</td>
</tr>
<tr>
<td>Overall-R²</td>
<td>0.799</td>
<td>0.773</td>
<td>0.681</td>
<td>0.538</td>
<td>0.525</td>
</tr>
<tr>
<td>Number of groups</td>
<td>312.0</td>
<td>114.0</td>
<td>190.0</td>
<td>312.0</td>
<td>114.0</td>
</tr>
<tr>
<td>Sample size</td>
<td>13,728</td>
<td>5,016</td>
<td>8,360</td>
<td>13,728</td>
<td>5,016</td>
</tr>
</tbody>
</table>

Note: All specifications include pair, year, and quarter fixed effects. Bootstrapped standard errors appear in parentheses and are clustered on both counties appearing in the pair. Statistical significance reported at the 1% (***) , 5% (**) , and 10% (*) level.
In expectation, this becomes equivalent to the biased OLS estimates of Equation (1). In other words, differencing from a random county instead of the cross-contiguous neighbor should replicate the results of Tables 4 and 5. If differencing from a randomly selected neighbor replicates the cross-state neighbor differenced results of Table 6, then it would suggest that the motivating intuition was incorrect. Operationally, subsamples of half the border counties are selected at random without replacement. The remaining half are then randomly assigned as pseudo-neighbors and differenced to produce a new pseudo-border differenced data set. A regression analysis using the full specification of variables is estimated, the results saved, and the process repeated 1,000 times.

Figure 5 presents kernel density plots of the MME tax coefficients from the 1,000 regressions over the random border differencing approach. For quick comparison, a vertical line indicating the effect size from Table 4 to see how close the trials came to replicating OLS. Overall, the findings are supportive of the border-differenced results.

![Figure 5](image)

**Figure 5**

Kernel Density Plots of Random Border Differencing Results for MME Tax Effect on Manufacturing Employment and Wages

Notes: Dashed grey vertical lines indicate Tables 4 and 5 border difference effect size ($\gamma$) of $-3.076$ and $-0.538$ for employment and wages, respectively. The black vertical lines show the equivalent Table 6 point estimates of $1.158$ and $-0.175$. Number of Trials = 1,000.

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In another border-differencing study, Huang (2007) created a fictitious placebo policy on additional randomly selected non-event borders to determine the threshold at which the effect sizes disappeared. In this study, the main finding was the elimination of a statistically significant effect, so the most analogous placebo approach would have been to non-randomly select counties and change their tax rates by a randomly chosen continuous number. This would have been difficult to meaningfully interpret relative to our random border reassignment approach.
in Table 6 for eliminating unobservable omitted variable bias. The mean MME tax coefficient is \(-3.71\) for employment with a standard deviation of 1.37, which is indistinguishable from the results of Table 4 at \(-3.076\). For average wages, the pseudo-neighbor differenced sample has a mean of \(-0.11\) with a standard deviation of 0.30, so that it is statistically indistinguishable from the results of Table 5 or Table 6.

D. Implications for Policy and Research

Taking these results as a collective, what is the economist to advise policy makers in the formation of tax policy regarding the inclusion of MME purchases in what purports to be a general consumption tax? Our results suggest that on average manufacturing labor will neither significantly gain nor be displaced by the extension of the RST to manufacturing. Alternatively, it could be said that the incidence effect on manufacturing labor overall is negligible enough to not be a particularly important consideration. At the state level, the mix of findings across subsectors arguably supports a \textit{prima facie} case for applied theory and formal Computable General Equilibrium (CGE) incidence models. In practice, this paper functionally suggests that the economist advisor should start with the assumption that the effect on labor is zero, rather than largely negative or positive, and then apply the state’s manufacturing traits to influence their judgment towards either a positive or negative position. Are the manufacturers within the state price takers or price setters in their markets? Are they capable of significant substitution between capital and labor? Finally, efficiency matters in addition to incidence, and the subsector differentials indicate that the tax remains distortive, just not in a systematic fashion that continuously favors labor. If the United States were to switch to the VAT, the evidence here implies that it would reduce these distortions but would not offer labor a relief in tax incidence, at least in manufacturing.

The finding of this paper is that, on average, an exogenous increase in a partial factor state tax on capital acquisition in the United States is not borne by labor offers some inferences for both our understanding of the theoretical incidence and the connection to its associated empirical work. First, most of the empirical work motivated by Harberger-style capital tax theoretical work has found labor to at least share in the burden, but largely draws upon examinations of corporate net income taxes that have only tenuous connections to a partial factor input tax. The corporate net income tax in a state may reduce the accounting profit of a corporation’s purchase of a forklift and hire an operator below their economic hurdle rate, but the corporate net income tax is indifferent to how much of the firm’s cost is allocated to the forklift relative to its operator. In representing a clear sector specific input tax, the findings here on the MME tax do not contradict the empirical research on corporate income taxes. Furthermore, the findings seem to be in line with both recent theoretical work and the original Harberger (1962) model that provided multiple routes for labor to avoid the tax.\textsuperscript{26}

More recently,

\textsuperscript{26} The most notable condition from Harberger (1962) was the case where input elasticity of substitution in both the taxed and untaxed sectors were unitary. However, the model also yielded full capital burden when the taxed and untaxed sectors were equal in both input elasticity of substitution and in their capital-to-labor intensity.
Gravelle and Smetters (2006) extend the Harberger model to the open economy case with consumers that have imperfect substitution among products, an assumption that carries empirical support and pushes the conditions back in favor of expecting the burden to shift away from labor in the local taxed sector. Finally, the empirical research on local business input taxes more generally has rather consistently found taxes to be shifted forward onto consumers.27

V. CONCLUSION

Using data from the Quarterly Census of Employment and Wages from 2001 to 2011, we estimate the impact of state taxes of machinery and equipment purchases by manufacturers on the county level average wages and employment of manufacturing workers. A two-way fixed effects regression produces estimates that are highly sensitive to model specification, particularly with respect to the inclusion of state specific time trends. We articulate a case for an alternative empirical strategy of differencing adjacent county levels across state borders. The results of the border differencing approach overall find no effect of the tax on manufacturing labor employment or wages that is highly robust to specification, and some limited evidence that the effect may be positive rather than negative. Far from the concern that the tax is a “job killer” as it has been described by political opponents, the evidence here is consistent with the tax causing a substitution from capital to labor to mitigate any reductions in overall manufacturing for which the tax might be responsible. It is also demonstrated that differencing from a randomly selected county produces estimates comparable to the original fixed effects without border differencing, a falsification test that provides support for our identification strategy. Furthermore, evidence is presented that this finding is not driven by rapid wage equalization in local labor markets.

Importantly, this is the study of a particular capital input tax, which most of the literature has examined only by inferring from the CIT. While the capital tax of this paper finds no evidence that labor bears the burden of the tax, it still might be the case that labor would share in the incidence of the CIT. This paper was also limited in the sense that it identifies where the incidence is not rather than where it finally falls (i.e., consumers or capital owners). Also, applying the approach to manufacturing subsectors reveals considerable heterogeneity in the direction of the effect of the tax on labor, which is consistent with an average response being zero rather than the complete absence of an effect on manufacturing labor. It is not that the findings indicate that the tax is not distortive, just that it is not distortive in a manner that is on average burdensome to labor. Future research may be able to empirically discern why the different subsectors respond differently to the tax. Furthermore, if the burden of the MME tax is not on labor it must be on capital or consumers, and that remains a topic for future investigation. Finally, incorporating local taxes into the analysis is a potential area of further research.

27 See Ross (2016) for a review of the empirical literature on tax shifting for business input taxes.
especially considering that localities frequently provide special privileges to large local employers that might offset unfavorable state policies.

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DISCLOSURES

The authors have no financial arrangements that might give rise to conflicts of interest with respect to the research reported in this paper.

REFERENCES


