Refusing Refuse: The Impact of State Tax Policy on Hazardous Waste Flow and Environmental Justice

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Abstract

Following the development of hazardous waste regulations in the 1980s, disposal tax gained popularity among state governments as a means to mitigate against a spillover of negative externalities disguised as shipments. Today, more than half of hazardous waste shipments still cross state borders. In this study, I evaluate the extent to which Missouri's introduction of an out-of-state hazardous waste tax in 2005 affects quantity shipped from adjacent states. I estimate a difference-in-difference model using facility-level shipment data between 2001 and 2017. Results show that the tax introduction decreased waste inflow by 45% to 48%. To investigate the policy's implication on environmental justice, I estimate the pre-tax correlation between shipment quantity and the share of non-white residents at destinations and how it changes after the tax introduction. Results suggest that the correlation is positive but weakened after the tax introduction, corresponding to an improvement in environmental justice. These findings add to the environmental justice literature, which focuses almost exclusively on the siting decision of hazardous waste facilities.

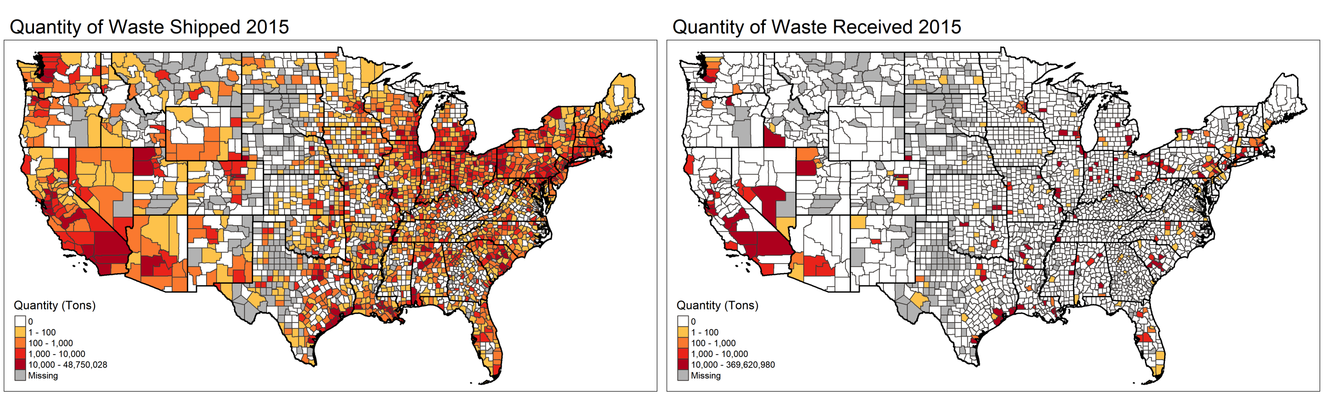
1 Introduction

Modern humans are addicted to convenience. We are careless about the negative externalities of our actions because we can afford to be so: out-of-sight, out-of-mind. This unfortunate attribute is apparent in the way we handle hazardous waste. Most people are not aware of how their livelihood depends on hazardous waste generating activities. Consider plastics, for example. If they were to disappear tomorrow, human civilization would collapse. Plastics are hazardous-waste-intensive as they are produced jointly by the chemical product manufacturing and the petroleum industries – the top two industries responsible for over 80% of total hazardous waste generation in 2007 (Jensen, 2012).

One curious feature of hazardous waste that sets it apart from other types of environmental pollution is the geographical disconnection between generation and disposal. Air emissions, for example, disperse from the point of release according to wind direction. The dispersion of hazardous waste, on the other hand, is determined by the location of management facilities and the shipping decision of generators.

A comparison of the outflow (left) and inflow (right) quantity of hazardous waste at the county level in figure 1 gives insight into how hazardous waste is shipped away from generating locations to a much smaller set of destinations. While generating activities can bring economic benefits to the host community and local government, the management and disposal impose high environmental costs but offer negligible benefits (Levinson, 1996). For this reason, shipments disproportionately relocate the cost of hazardous waste from origins to destinations and behave like a pollution spillover when crossing jurisdictional borders.

Figure 1: Quantity of hazardous waste shipped from (left) and received by (right) counties in 2015



State governments have used hazardous waste taxes to mitigate this type of spillover. In this paper, I present first evidence on how an introduction of a hazardous waste tax can discourage waste inflow and alter the environmental justice landscape of hazardous waste shipment. More specifically, I examine the significance of Missouri's introduction of a disposal tax of \$2 per ton on out-of-state hazardous waste in 2005 by addressing two questions: (i) To what extent does the tax introduction reduce the quantity of out-of-state waste inflow to Missouri? (ii) How does the correlation between shipment quantity and the share of non-white residents at destination change after the tax introduction?

To answer the first question, I employ a difference in differences methodology. Because the tax is specific to out-of-state waste, shipments within Missouri are not affected by the policy change. Generators in Missouri serve as a control group in my analysis. In the preferred sample, the treated group consists of facilities in Missouri's adjacent states that shipped to Missouri before the tax introduction. For robustness checks, I construct matched samples based on various combinations of generation quantity, shipment quantity, industry group, population density, and share of manufacturing employment.

To estimate the model, I use shipment data between 2001 and 2017 from the Environmental Protection Agency's (EPA) Biennial Report. The model includes state disposal tax as a control variable, in addition to total shipment quantity and demographic characteristics of facilities' census tracts. I gathered tax data for the study period from regulatory documents and communications with state government agencies. Results show that the tax introduction effectively reduced hazardous waste inflow: shipment quantity from adjacent states into Missouri decreased by approximately 45% to 48%. This suggests that an out-of-state tax, even a small amount, can effectively divert hazardous waste inflow. Results from these matched samples confirm the main finding.

The literature on hazardous waste tax and shipment is thin[1]. Levinson single-handedly studied the causal relationship between hazardous waste tax and shipment in a series of three published papers in which he conducted analyses using a state-pair panel data of shipment and disposal tax rates between 1989 and 1995 (Levinson 1996, Levinson 1999a, Levinson 1999b). Results show that hazardous waste tax discourages shipment. Levinson emphasizes endogenous tax as a major identification challenge in his studies.

Taxes are endogenous: states that import more waste impose higher tax rates. Without accounting for this reversed causality, as shown in his pooled OLS results, an estimate appears as if high tax rates attract more waste flow (Levinson 1999a, Levinson 1999b). Levinson uses three strategies to handle this issue. First, he runs OLS regressions using only observations that involve a "retaliatory tax" – the destination state charges the maximum between its tax rate and the tax rate of the origin state. This method excludes many state pairs and yields negative but

[1]Partially relevant studies are such as (1) Sigman (1996) on how the tax imposed on generators affects their choice of generation quantity and management method, (2) Alberini and Frost (2007) on liability regulations, (3) Deyle and Bretschneider (1995) on two policy interventions in New York: the state Superfund tax imposed on generators and the land disposal restriction, and (4) Jenkins and Maguire (2009) on how the racial composition of affected communities influences state hazardous waste tax

statistically insignificant coefficient estimates (Levinson 1996, Levinson 1999b). Second, he incorporates destination state fixed effects which yield negative and statistically significant estimates in all three studies. This method, however, relies on the assumption that the omitted variables are time-invariant. Third, he uses an instrumental variable for tax rates[2] (Levinson, 1999b). The resulting estimate is negative and larger in magnitude than the estimates from the fixed-effects model.

My study takes a different approach in addressing the endogenous tax issue. I make use of a specific policy intervention – the tax introduction by Missouri – to estimate a difference in differences model. While both cases concern the causal effect of tax on shipment, their questions are slightly different but complementary. Levinson asks: what is the extent to which a marginal increase in disposal tax decreases waste inflow? On the other hand, my study asks: to what extent does an introduction of an out-of-state tax discourages waste inflow? Another important distinction is that taxes in Levinson's studies are not specific to out-of-state waste.

In a broader literature of environmental federalism, economists have voiced concern that the combination of fragmented governing system and the imbalance in the distribution of the costs and benefits of hazardous waste is inducive to a "race to the top" among state governments causing an inefficient welfare outcome (Oates et al. 1972, Levinson 2003, Sigman 2003). This inefficiency is when the realized welfare is lower than the "optimal" scenario in which a social planner operates to maximize the whole nation's welfare. However, the decentralized political structure of the United States begs the question of why a centralized solution should be applied when it comes to hazardous waste.

Taking the larger governing structure as given, state governments have justifiable reasons to optimize at the state level when designing hazardous waste policies. Previous studies consistently document environmental injustice in the hazardous waste industry. Siting of treatment, storage, and disposal facilities (TSDFs) is positively correlated with the degree of marginalization of host communities, commonly measured by the share of residents of minority races (Banzhaf et al. (2019) provide an extensive review of this literature).

Scholars are still debating whether this pattern is due to direct racism, indirect racism in the form of firms following the "path of least resistance," confounding economic factors, or the reversed causality of "minority move-in" (Hamilton 1995, Saha and Mohai 2005, Zahran et al. 2008, Anderton et al. 1994, Pastor et al. 2001). In terms of policy-making, however, the most pressing issue is that marginalized population subgroups are vulnerable to falling victims to having these negative externalities pushed upon them.

[2] He constructs the instrument from three variables: the log of hazardous waste generation of the destination state, the percent of the gross state product attributed to the manufacturing sector, and the percent of voters voting Republican in the 1988 presidential election.

Relocating existing facilities to less marginalized neighborhoods to dissolve the correlation is likely not an executable solution. The role of siting has diminished over time since establishing a new TSDF has become uncommon after the early 1990s[3]. In contrast, the shipment of hazardous waste continues. In the past two decades, the overall prevalence of interstate shipment has remained consistent, but individual states have experienced different trends. Missouri, for example, appears to be on the trajectory to become a hazardous waste hub, whereas Arizona is retiring[4]. The pattern of hazardous waste shipment suggests that management and disposal activities are getting more geographically concentrated[5] , and state taxes play a role in slowing down this concentration (Levinson, 1996). Given this dynamic, state taxes may have a role to play in shaping the environmental justice landscape of hazardous waste shipment, and evaluating that relationship can have policy merit.

Given that the tax introduction effectively discouraged waste inflow, as the difference in differences analysis suggests, it follows that the policy change must have displaced some waste to other locations. The second question of this paper evaluates how the change in shipping behavior of affected generators may have had an environmental justice implication. To this end, I conducted regression analyses with origin, destination, and year fixed effects to estimate the pre-tax correlation between the pair-specific shipment quantity and the share of non-white residents at destination and the change in the correlation in the post-period. Results suggest that the pre-ban correlation is positive and diminished after the tax introduction. This corresponds to a case of environmental injustice in the pre-ban period – generators ship more to places with non-white residents – and an improvement after the tax introduction. Results from matched samples confirm this finding. This analysis contributes to the literature on environmental justice of hazardous waste, which focuses exclusively on siting of TSDF.

The rest of the paper is organized as follows: section two provides a regulatory background, section three describes the data, section four presents the difference-in-difference analysis of the impact of the tax introduction on waste flow, section five presents an empirical analysis of the

[3]According to Stafford (2000), approximately 500 commercial TSDFs were operating in the late 1990s. These were largely a subset of the TSDFs that were built in the late 1980s and early 1990s. During those years, the largest TSDFs consolidated the market by buying competitors and building new facilities. In addition, cleanup liability requirements make closure costs for a TSDF very expensive. For this reason, these facilities tend to remain in the industry even when being underutilized.

[4]The quantity of waste that Missouri received from other states is larger than its generation in all years, except for 2009. The quantity received from other states was decreasing between 2001 and 2009 and increasing afterward. On the other hand, Arizona currently has the highest HW treatment and disposal tax of $270 per ton, which applies to in-state and out-of-state waste. Back in 2010, this was only $40 per ton. Between 2001 and 2017, its generation quantity exhibited no clear trend. Its out-of-state shipping quantity fluctuated with generation quantity between 2001 and 2007 and increased steadily between 2009 and 2017. Its quantity received from other states was smaller than its generation every year except for 2005 and exhibited a decreasing trend. According to my conversation with a staff member of Arizona's Department of Environmental Quality, Arizona only has TSDFs in a post-closure stage. Facilities in Arizona only do preliminary processing before sending waste to other states for treatment and disposal.

[5]According to Levinson (1999b) and my analysis of shipment data between 2001 and 2017.

environmental justice implication of the policy, and section six concludes the paper. Through this paper, I occasionally refer to hazardous waste as HW and difference in differences as DID.

2 Background

Generation and management of hazardous waste were not always geographically disconnected. In 1976, Congress established the Resource Conservation and Recovery Act (RCRA), which comprises federal regulations that govern hazardous waste. Before that, generators regularly dumped hazardous waste on open land or water bodies at their convenience (Canter, 1981). The most notorious of all is the Love Canal incident. Hooker Chemical and Plastics Corp. used the canal as an industrial dump for a few decades before selling it to the Niagara Falls Board of Education for $1 in 1953 (Williams, 1993). The chemical seepage became so severe that President Carter declared it a national emergency. Over 700 families were relocated from the area (Holden, 1980). The public was horrified.

As the RCRA solidified in the early 1980s, regulatory costs skyrocketed. Many firms went belly up (Stafford 2000, Eichler 1984). The RCRA imposed strict standards in all stages of hazardous waste handling and only allowed management and disposal at facilities with a Treated Storage and Disposal Facility (TSDF) permit. Demand for TSDF services surged, and the lack of proper management capacity became a national crisis (Szasz, 1986). The government and the industry needed more TSDFs. But where to build them?

"Well, not in my backyard!" answered the public. Public opposition was a critical obstacle to TSDF capacity expansion (Saha and Mohai, 2005). The ability to oppose siting, however, was not uniform. Siting was influenced by the pre-existing social structure, which paved out the "path of least resistance." Early resistance against siting started among the white middle class, followed by the white working class, leaving minority and poor communities more vulnerable in that transition (Taylor 1997, Taylor 2000).

The struggle to keep hazardous waste out of one's yard was not limited to the general public. Similar tension pervaded state governments. In addition to an overall capacity shortage, the distribution of management infrastructure relative to generation was disproportional across states. The federal government pressed all states to cooperate in the National Capacity Assurance Program, which required them to build sufficient management capacity to accommodate local generation. However, the program was unsuccessful and the imbalance in hazardous waste burden persisted, causing interstate political conflict to intensify. The Carolinas, for example, were at a "hazardous waste border war" in the late 1980s (Wynne III and Hamby, 1990).

The incentive to expand capacity was weak among net-exporting states (Wynne III and Hamby, 1990). Siting would upset the citizen, and any contamination issues in the future would erode their budget, not to mention the pre-existing contaminated sites waiting for remediation. High-capacity states wanted to unload the unjust burden and began taking various regulatory measures to minimize waste inflow. Some states attempted a total ban on out-of-state waste but were ruled unconstitutional by the Supreme Court (Wynne III and Hamby, 1990). Other efforts included reciprocal waste reduction requirements, pre-approval of waste streams, and agreements with commercial facilities to prioritize local waste.

Hazardous waste taxes began to gain popularity in the late 1980s. Some states charged extra on waste from other states. In 1989, Alabama added a $42 per ton tariff on out-of-state waste on top of the local fee of $40 per ton. The policy generated $30 million in revenues and reduced interstate inflow by more than half (Levinson, 1996). The Supreme Court ruled the tariff as a violation of the Interstate Commerce Clause, however, forcing Alabama to discontinue the policy in 1992 (Levinson 1999a, Levinson 1999b, Sigman 2000).

The Supreme Court tolerates subtler forms of out-of-state taxes as long as they only "serve as a means to recover the regulatory costs" (Wynne III and Hamby, 1990). Today, South Carolina, Louisiana, and Mississippi employ "retaliatory" taxes: each charges the maximum between the tax rate of the state where the waste was generated and its local tax rate. Georgia, New Hampshire, Texas, and Vermont charge a single higher fee on waste from other states. Missouri joined this group in 2005, imposing a $2 per ton tax on the management and disposal of out-of-state waste while local waste remains tax-free.

3 Data

This section describes three sets of data used in this study, including HW shipments, census tract demographic characteristics, and state HW taxes.

3.1 Hazardous Waste Shipment Data

Data on waste shipments are from the RCRA's Biennial Report (BR) database, accessible through RCRAInfo Public Extract[6]. As of March of 2019, there were nine BR data tables, corresponding to nine biannual report surveys between 2001 and 2017[7].

There are three main types of HW facilities: generators, transporters, and treatment, storage, and disposal facilities (TSDFs). There are three subgroups of generators, including large quantity generators (LQG), small quantity generators (SQG), and very small quantity generators (VSQG). Only large quantity generators and TSDFs are required to submit the Biennial Report. I refer to them as "reporting facilities."

[6]Source: https://rcrapublic.epa.gov/rcra-public-export/

[7]The BR report dates back further than 2001, but 2001 is the first year that data are available on RCRAInfo.

The BR survey only covers shipments that involve a reporting facility. Shipments between non-reporting facilities are not recorded in this database. To clarify, three types of shipments are included: (1) shipments from a reporting facility to another reporting facility, (2) shipments from a reporting facility to a non-reporting facility, and (3) shipments from a non-reporting facility to a reporting facility. The information on non-reporting facilities is limited only to facility IDs and their states. As for reporting facilities, the BR data includes facility name, street address, total quantity generated, total quantity treated, and the industry to which they belong, and other information.

Because locations of origin and destination facilities are necessary for assigning demographic characteristics information, I only include shipments between reporting facilities in my analyses. This subset of data covers shipments to the final destinations of the waste's life cycle because TSDFs are required to submit the BR report. There are 78,754 reporting facilities that made at least one transaction with another reporting facility between 2001 and 2017, which accounts for approximately 14% of all facilities. Among these, 77,677 facilities are located in the 48 mainland states of the US, which is the geographical scope of this study.

The BR data provides street addresses of the reporting facilities but not the geo-coordinates. I obtain the geo-coordinates of these facilities in two ways. First, I use facility IDs to search for their geo-coordinates in the EPA's Facility Registry Service (FRS) and the RCRA's GIS databases. For facilities that do not appear in these databases, I use facility names and street addresses to obtain their geo-coordinates from the geocoding API provided by Google Map. I was able to obtain geo-coordinates of 76,792 facilities: 64,111 from the FRS, 1,149 from the RCRA GIS, and 11,532 from geocoding. This is approximately 99% of all reporting facilities in 48 states.

The BR data reports two types of waste flow quantity: quantity received and quantity shipped. Ideally, for each pair of reporting facilities that transact with one another, the quantity shipped reported by the shipping facility should be equal to the quantity received reported by the receiving facility. Unfortunately, I found that there are some disparities in these two quantities. I use the quantity reported by receiving facilities, assuming that it provides a more accurate measure of the welfare implications at destinations.

3.2 Census Tract Demographic Data

I use census tract boundaries to define local communities of facilities from the BR data. Data on demographic characteristics of census tracts are obtained from the 5-Year American Community Survey (ACS)[8]. The earliest set of available data is from the survey of 2005-2009. I use data

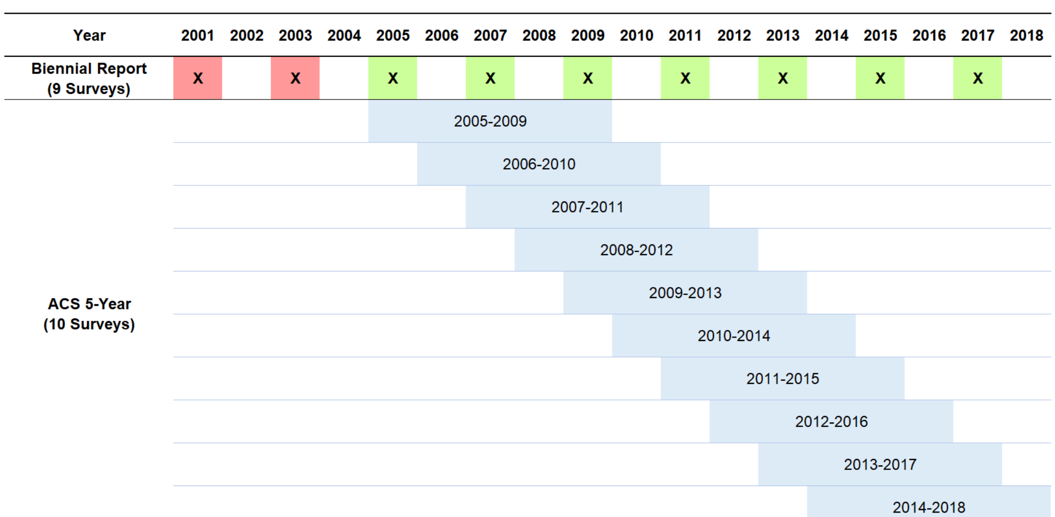
[8]The 1-Year ACS is suitable only for geographic areas with populations of 65,000 or more, and thus it is not suitable for analysis at the census tract level.

from the surveys of 2005-2009 through 2014-2018[9] to compute the following variables for each census tract: the share of non-white residents, the share of manufacturing employment, the share of residents with educational attainment beyond high school, median household income, median home value, and population density.

According to the ACS user manual (US Census Bureau, 2020), 5-year ACS data provide "period estimates." That is, the estimates are specific to the period that the data collection process occurs. It is not appropriate to interpret an estimate from the 5-year ACS survey as an estimate of a specific point in time. For example, an estimate of the share of non-white residents from the 2005-2009 survey should not be used as an estimate for 2007 or any of the specific years within the year range. It is intended to be interpreted as an estimate of the share of non-white residents between 2005 and 2009.

The temporal misalignment poses two challenges to my analysis with regards to assigning demographic data from the ACS to facilities in the BR data for each year. Figure 2 provides a visualization of how the BR data and the 5-year ACS data overlap. The first challenge is that the BR data from 2001 and 2003 do not overlap with any of the available 5-year ACS surveys. The second challenge is that the mapping between BR to ACS is not unique for the BR years that overlap with multiple ACS year ranges. For example, BR year 2005, 2007, and 2009 overlap with the ACS survey of 2005-2009. Similarly, BR year 2009 is contained in five ACS surveys (2005-2009, 2006-2010, 2007-2011, 2008-2012, and 2009-2013). For this reason, there is no single appropriate way to match the BR and ACS data.

Figure 2: Temporal overlap of BR and ACS Data

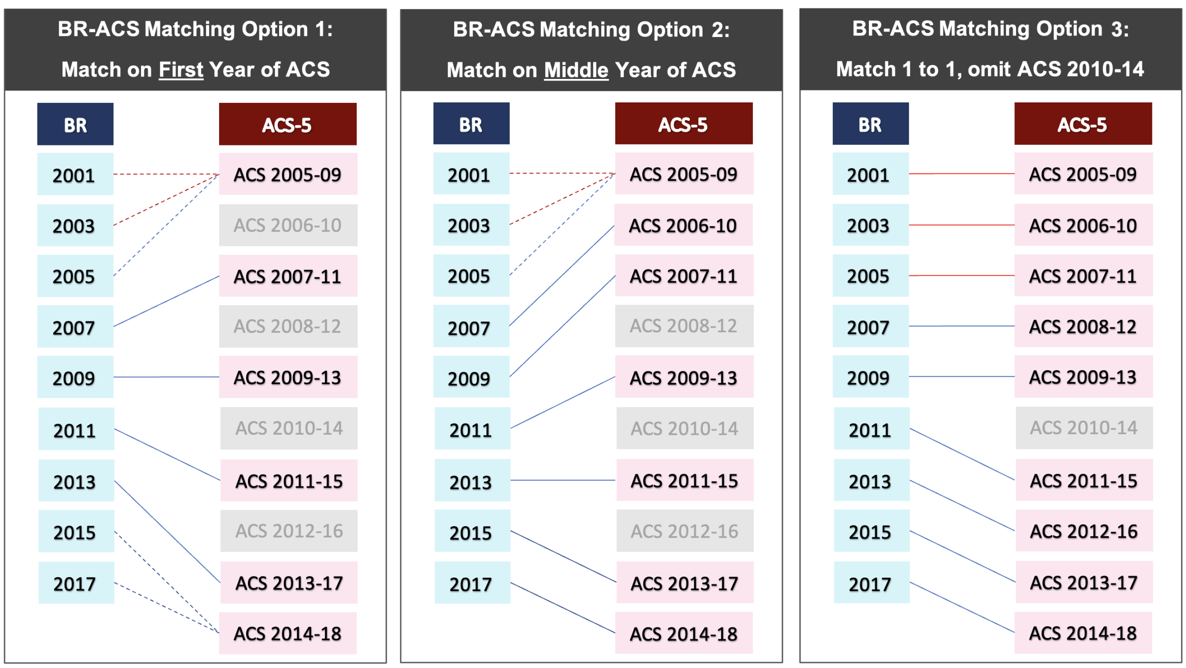


In this study, I consider three alternatives of BR-ACS assignment, which are shown in figure 3.

[9]The latest survey, published in December of 2020, covers the year range 2015-2019. I plan to incorporate this new set of data in my analysis in the future.

In each diagram, a red line indicates that the BR year falls outside of the matched ACS year range (for example, when BR 2001 is matched with ACS 2005-2009). A dashed line indicates that the ACS survey is matched with multiple BR years. Option 1 matches each BR year to the first year of the ACS 5-year range when possible. Two ACS surveys are matched with multiple BR years: ACS 2005-2009 is matched with 2001, 2003, and 2005, and ACS 2014-2018 is matched with 2015 and 2017. Option 2 prioritizes matching each BR year to the middle year of the ACS 5-year range. For example, BR 2009 is matched with ACS 2007-11. In this case, ACS 2005-2009 is matched with 3 BR years: 2001, 2003, and 2007. Option 3 matches one BR year to one ACS survey. The caveat is that there are three matches in which the BR year is not within the ACS survey's year range. These are BR 2001 and ACS 2005-2009, BR 2003 and ACS 2006-2010, and BR 2005 and ACS 2007-2009. Note that ACS 2010-2014 is omitted.

Figure 3: BR-ACS assignment options



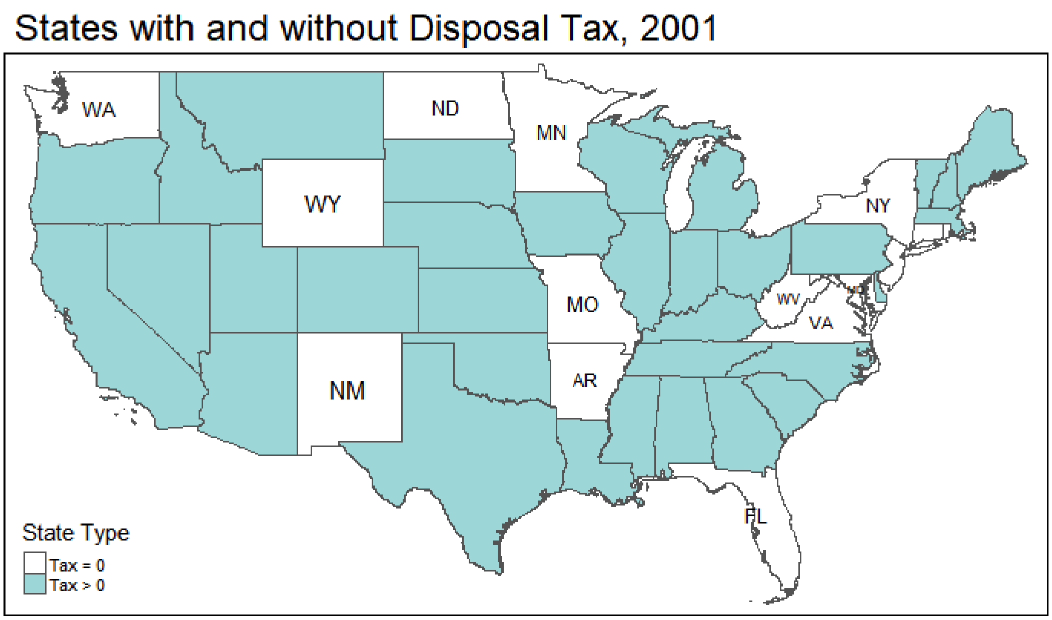
The choice of BR-ACS assignment does not have a meaningful impact on the estimates of the coefficient of interest in the first analysis which evaluates the impact of Missouri's tax introduction on the quantity of waste flow into Missouri. This is because data from the ACS are used as control variables in this analysis. The coefficient of interest does not involve a demographic variable. For the second analysis which evaluates the environmental justice implications of the tax introduction, however, the choice of BR-ACS assignment matters. This is because the explanatory variable of interest, the share of non-white residents at the destination, is derived from the ACS survey. In the section where I present this analysis, I discuss how the results vary when using each of these matching options.

3.3 Hazardous Waste Tax Data

I collected the per-ton tax rates that each state charges TSDFs for the management and disposal of HW from codes, statutes, and regulations[10]. In addition, I contacted state agencies that oversee HW management via e-mails and phone calls to verify the information I obtained from these regulatory documents and clarify any confusion.

There are three broad categories of activities that involve HW, including generation, transportation, and management. The amount of regulatory cost of these activities vary across states. Tax rates of management may be differentiated by characteristics of HW such as liquid or solid and acute and non-acute HW. They may also be differentiated by management methods or management location (on-site or off-site). The four main categories of management methods include disposal, treatment, storage, and recycling/energy recovery. In this study, I focus on the per-ton tax on the disposal of HW[11]. Figure 4 shows states with at least one type of per-ton HW tax in 2001.

Figure 4: States with and without hazardous waste disposal tax in 2001



Some states have HW tax systems that are more complex than others. Texas, for example, distinguishes between the following 8 management methods: landfill, land treatment, underground injection, incineration, processing, storage, energy recovery, and fuel processing. Each of these management methods has four different fees, depending on whether the facility is commercial or non-commercial and whether the waste is generated in Texas or imported from other states. In

[10] I accessed these documents via WestLaw.

[11] Other fees are such as per-ton fees on generation paid by generators. Fees that do not vary with quantity are such as various types of annual operation fees and permit fees.

total, Texas specifies $8 \times 4 = 32$ tax rates. Louisiana, on the other hand, only specifies three different fees: \$30 for on-site management of regular HW, \$40 for off-site management, and \$100 for extremely HW, on-site or off-site. These fees are specific to waste generated in Louisiana. For out-of-state waste, Louisiana charges the maximum between its off-site rate and the rate of the origin state. Missouri, which is the focus of this study, currently charges no tax on the management of waste generated in Missouri. Starting in 2005, it charges a flat fee of \$2/ton on the management and disposal of out-of-state waste.

In this study, HW taxes are used as a control variable in analyses that evaluate the impact of the out-of-state tax introduction by Missouri. In the case where a state specifies multiple tax rates, I use the maximum rate imposed on the disposal of regular solid HW. Note that in all cases, disposal rates are greater or equal to the rates for treatment, storage, and recycling/energy recovery.

Table 1 below shows summary statistics of these tax rates for each of the BR years. The only year with an increase in the number of states with per-ton HW tax is 2005, which corresponds to Missouri's tax introduction. Notice that Arizona's tax was as high as \$280/ton in 2011 and \$270 in 2013, almost ten times the average rate. This rate was only \$40/ton before 2010. From my conversation with an employee of Arizona's Department of Environmental Quality, even though this is the rate for disposal, Arizona currently has no disposal facilities. There are 13 TSDFs in Arizona currently, but most of them are in a post-closure phase. According to the agency, almost all waste is shipped to other states after some preliminary management process, such as bulking or combining waste with chemicals that would allow it to be burned for energy.

Table 1: Summary statistics of hazardous waste disposal tax (\$/Ton)

Year	**Number of States With Tax**	**Average**	**Minimum**	**Maximum**	**Min/Max State**
2001	33	24.3	3	80	NC/VT
2003	33	25.3	3	80	NC/VT
2005	34	24.9	2	80	MO/VT
2007	34	25.0	2	80	MO/VT
2009	34	25.1	2	80	MO/VT
2011	34	32.2	2	280	MO/AZ
2013	34	31.7	2	270	MO/AZ
2015	34	31.8	2	270	MO/AZ
2017	34	31.8	2	270	MO/AZ

4 The Impact of Tax Introduction on Shipment

In 2005, Missouri introduced an "out-of-state waste fee" that requires treatment storage and disposal facilities (TSDFs) to pay $2 per ton on the management and disposal of out-of-state waste. Waste generated in Missouri remains tax-free[12]. Assuming that TSDFs pass down at least some of this cost to their customers, the tax introduction may have increased the cost of managing waste in Missouri for generators outside Missouri. Consequently, these affected facilities may reduce shipment to Missouri. I use a difference-in-differences (DID) approach to evaluate the extent to which this tax introduction reduces the shipment of HW into Missouri.

4.1 Difference-in-Differences Analysis Design

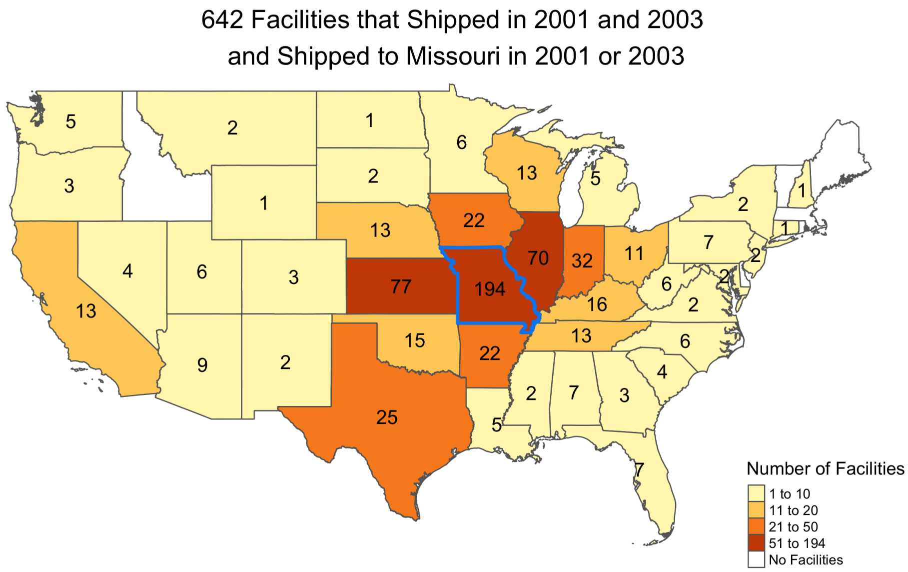
Sample Construction

The broadest set of affected facilities includes those located outside Missouri and shipped to Missouri before the tax introduction. Because the HW shipment data are available between 2001 and 2017 biannually, the pre-treatment period within the scope of this empirical analysis includes 2001 and 2003, and the post-treatment period includes 2005 through 2017.

Using this shipment data, I define the set of facilities relevant to the tax introduction to include facilities that shipped waste in 2001 and 2003 and shipped to Missouri in 2001 or 2003. Within 48 mainland states, 642 facilities satisfy this "relevance criterion." Figure 5 shows the number of these facilities in each state. These facilities make up the most inclusive sample used in this analysis, in which 194 facilities in Missouri are the control group, and 448 facilities outside of Missouri are the treated group.

[12]Generators in Missouri are required to pay a tax of $6.1 per ton of waste generated. They also pay a land disposal fee of $29.50 per ton. These taxes apply regardless of whether the management and disposal occur in Missouri or other states.

Figure 5: Facilities that are potentially affected by Missouri's tax introduction

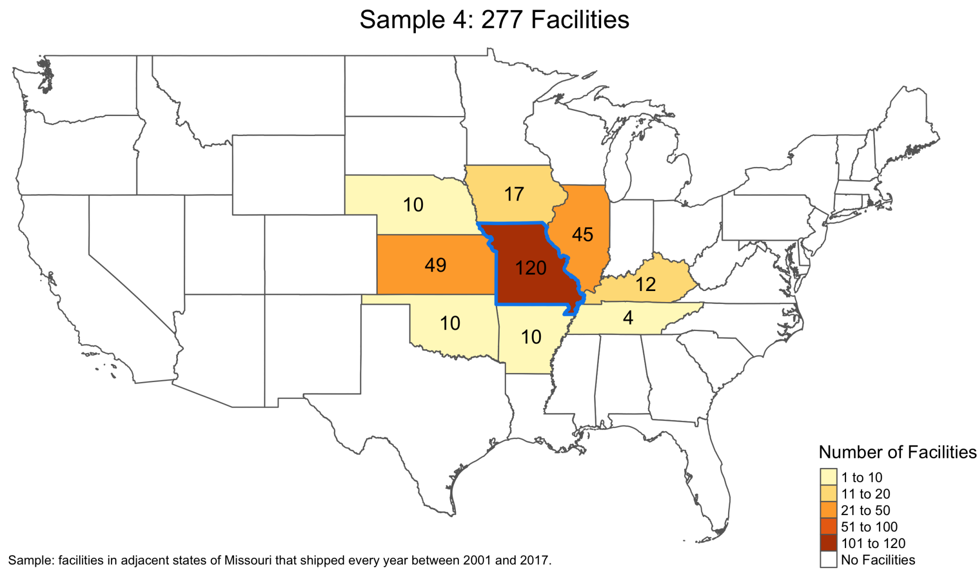


To construct three alternative samples, I specify two additional criteria. First, the "post-tax shipping criterion" requires that a facility shipped HW every year between 2005 and 2017. Second, the "adjacent state criterion" requires that a facility is in Missouri or adjacent states of Missouri, including Arkansas, Illinois, Iowa, Kansas, Kentucky, Nebraska, Oklahoma, and Tennessee. Table 2 shows the number of facilities in four sample sets. The main results are based on sample 4, which satisfies all three criteria. Results from three other samples are discussed in the robustness check section. Sample 4 consists of 277 facilities: 157 outside Missouri (treated) and 120 in Missouri (control), as shown in figure 6. Using this sample, I construct a balanced panel of 9 periods, which includes 2,493 units of observation, 42% of which have zero quantity shipped to Missouri (the outcome variable).

Table 2: Number of facilities in each sample set

Sample	**Sample Criteria**			**Number of Facilities**		
	Relevance	**Post-Tax Shipping**	**Adjacent State**	**Total**	**MO**	**Non-MO**
1	Yes	No	No	642	194	448
2	Yes	Yes	No	411	120	291
3	Yes	No	Yes	442	194	248
4	Yes	Yes	Yes	277	120	157

Figure 6: Facilities in sample 4



Facilities affected by the tax introduction may respond by stopping shipments to Missouri (external-margin adjustment) or by decreasing the quantity shipped to Missouri (internal-margin adjustment). Zero values are meaningful in the context of this research question because they may reflect the external-margin adjustment. Recall that sample 4 includes facilities that ship to at least one destination in every period. Using this sample rules out the scenario where zero quantity shipped to Missouri results from a facility shipping no waste at all.

Figure 7 shows that the number of facilities that ship to Missouri decreases over time for both the treated and the control groups. The treated group (dashed line), however, has a steeper decline between 2003 and 2009. This trend may reflect an external margin adjustment. Figure 8 shows that the share of quantity shipped to Missouri decreases over time for both groups, but the decrease of the treated group is noticeably steeper between 2003 and 2009. This trend may reflect an internal margin adjustment.

Figure 7: Time trend of the number of facilities that shipped to Missouri in treated and control groups

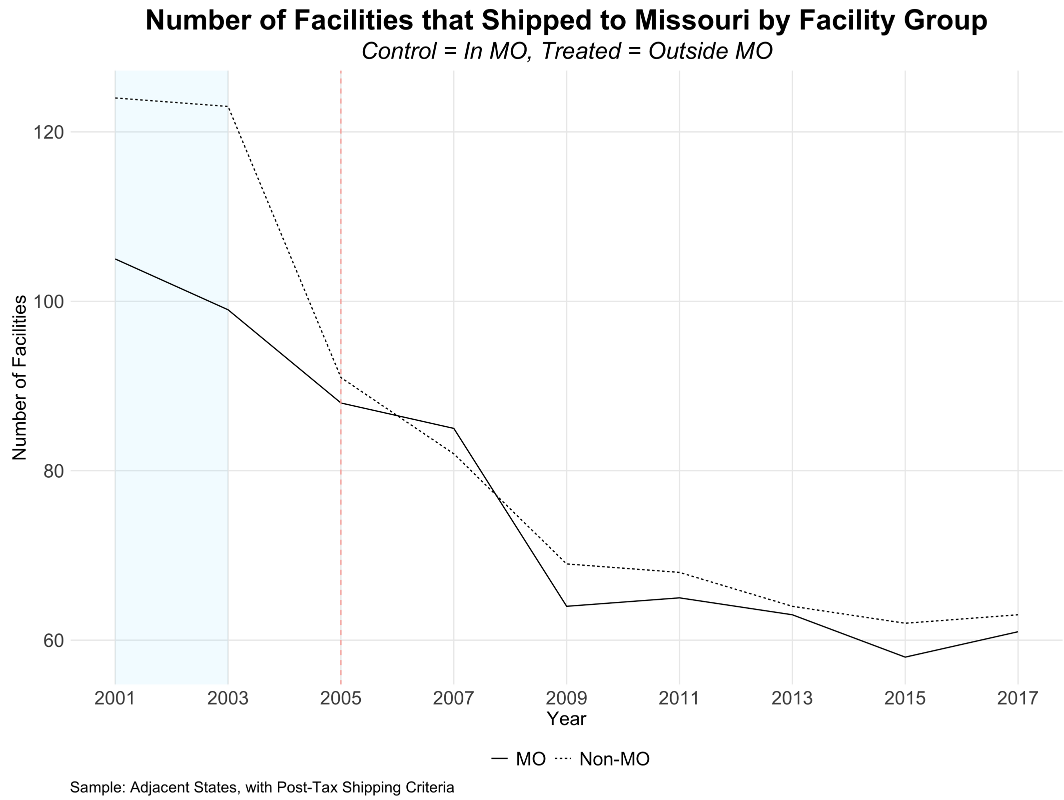
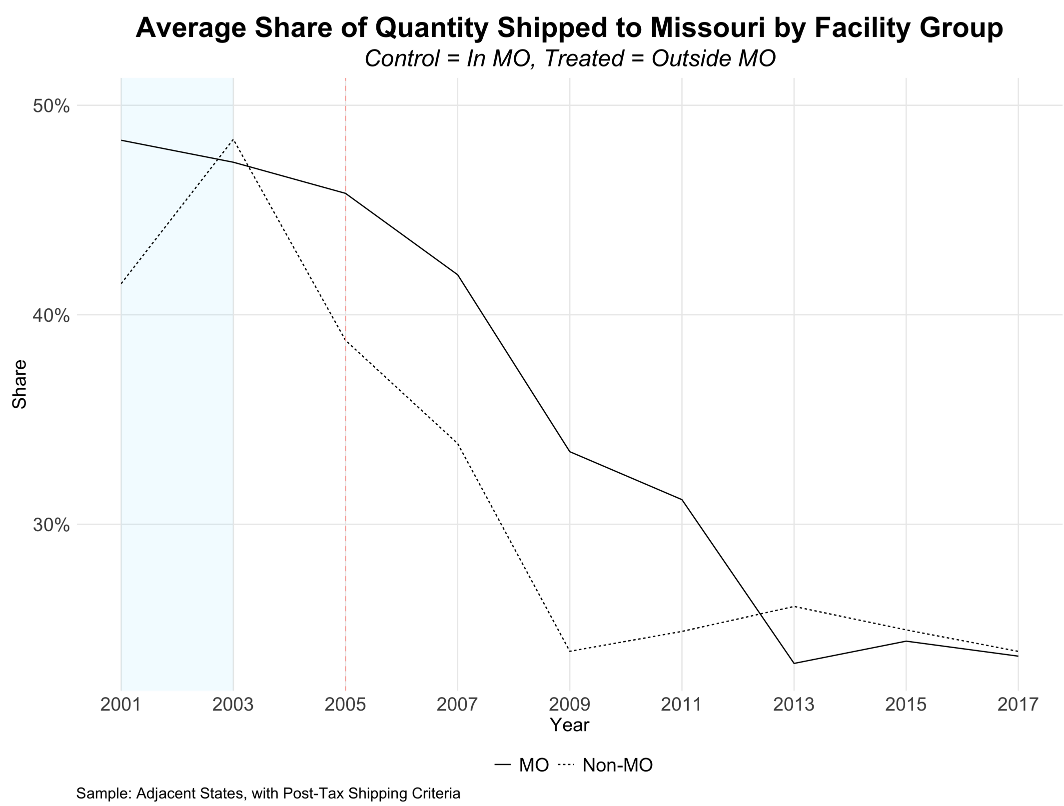


Figure 8: Time trend of the share of quantity shipped to Missouri of treated and control groups



Regression Equation

The quantity shipped to Missouri is heavily right-skewed (see figure 25 and 26 in the appendix). Given this shape of the distribution and a relatively high share of observations with zero value, I use the inverse hyperbolic sine (IHS) transformation of quantity shipped to Missouri as the dependent variable in the linear estimation of the DID model shown below. Similarly, I use the IHS transformation of total shipment quantity as a control variable.

Let $IHS(q)_{kt}$ denote the IHS transformation of quantity shipped from facility k to Missouri in year t. Let $treat_k$ be an indicator for a facility in the treated group: $treat_k = 1$ if facility k is outside Missouri. Let $post_t$ be an indicator for the post-treatment period: $post_t = 1$ for all $t \geq 2005$. Let $IHS(s)_{kt}$ denote the IHS transformation of total quantity shipped by facility k in year t to any location in 48 states. Let $\boldsymbol{T}$ denote two tax variables: (1) the average of the tax rate of the state where facility k is located and the tax rates of its neighboring states; (2) the interaction of the average tax variable with $post_t$. Let $\boldsymbol{D}$ denote a set of demographic variables of the census tract where facility k is located. This includes the share of non-white residents, share of employment in manufacturing, share of residents with education attainment beyond high school, share of residents with age over 65, median household income, median home value, and population density. Let α_k denote facility fixed effects and y_t denote year fixed effects. The DID regression equation is as follows:

$$IHS(q)_{kt} = \alpha_k + y_t + \beta treat_k \times post_t + \eta IHS(s)_{kt} + \boldsymbol{\tau' T} + \boldsymbol{\gamma' D} + \epsilon_{kt} \tag{1}$$

The coefficient of interest is β which measures the impact of the tax introduction on the quantity of waste flow from other states into Missouri. For β to be an unbiased estimator, three assumptions that underlie a DID analysis design must hold. First, the control group must be a "good" counterfactual of the treated group. Second, the control group's outcome must not be affected by the treatment. Third, any confounding factors left in the error term must not be correlated with the treatment and must not have a time-varying impact on the outcome variable (Wing et al., 2018).

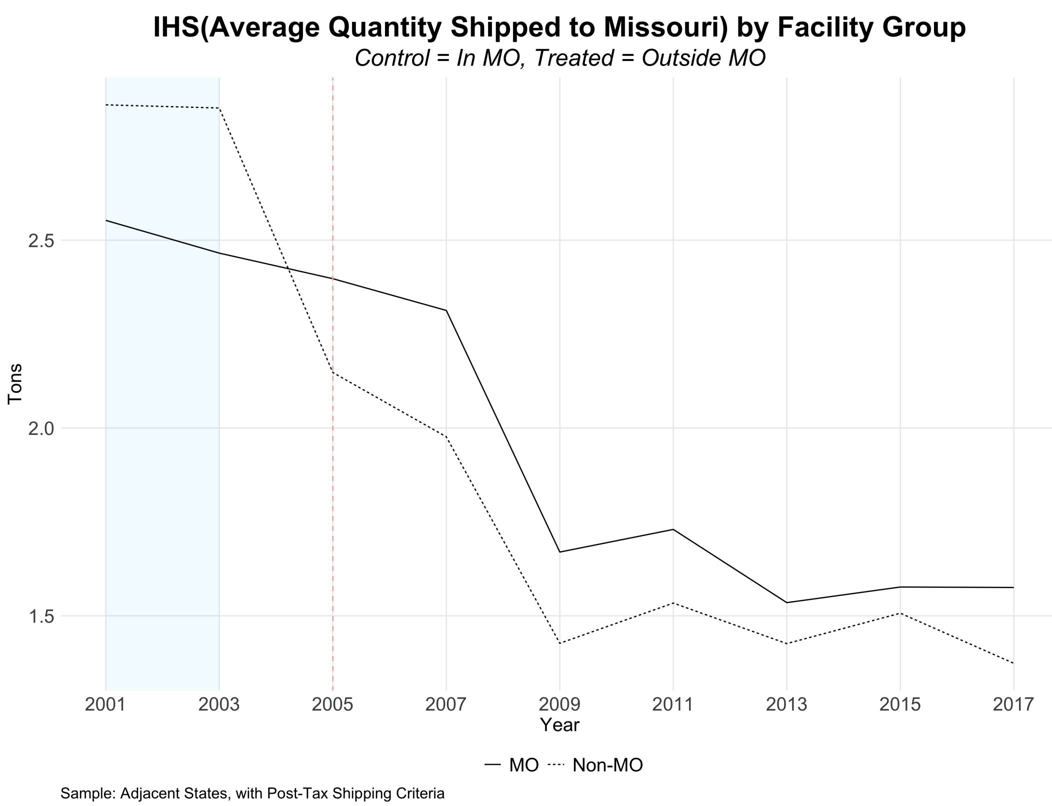
Identification Challenges

1. Parallel Pre-Trend

To state simply, a DID analysis requires that the control group is a "good" no-treatment counterfactual of the treated group. Since the assumption involves counterfactual quantities, it is not empirically testable. One common practice to speculate on the validity of this assumption is to evaluate whether the outcome variable of the treated and the control groups have parallel trends in the pre-treatment periods. An ideal case of parallel pre-trends shows that the treated and control trends co-fluctuate over many periods[13]. This pattern suggests that the outcome variable of the treated and the control groups responded to changes in a similar manner before the treatment takes place. For this reason, variation in the outcome variable of the control group in the post-treatment periods likely serves well as the counterfactual of variation in the treated group's outcome variable under a no-treatment scenario.

Due to data limitation, there are only two pre-treatment periods in this analysis, which are 2001 and 2003. Figure 9 shows group-specific averages of IHS-transformed quantity shipped to Missouri over time. Time trends of the original quantity in tons can be found in figure 24 in the appendix. The averages are decreasing over time for both treated and control groups, but the decrease of the treated trend (dashed) is steeper between 2003 and 2009. In the pre-period, treated averages are higher than control averages. Starting in 2005, however, the treated average fall below the control average and remains so until 2017.

Figure 9: Time trend of average IHS(quantity shipped to Missouri) by treated and control groups



The pre-trends in figure 9 are not perfectly parallel: the decrease in the control pre-trend is slightly steeper than the decrease in the treated pre-trend. This pattern suggests that the observed

[13]That is, the width of the gap between the two trends is constant even with period-to-period fluctuation in both trends. Fluctuating parallel trends are a better indicator of a valid counterfactual than two straight parallel trends without co-movements.

post-treatment trend of the control group might be steeper than the no-treatment counterfactual of the treated group in the post-treatment period. In this case, the data may produce an estimate of β that understates the true magnitude of the impact of the tax introduction. I address the nonparallel pre-trend concern by using matched samples, some of which produce improved parallel pre-trends. Results from matched samples are discussed in the robustness check section.

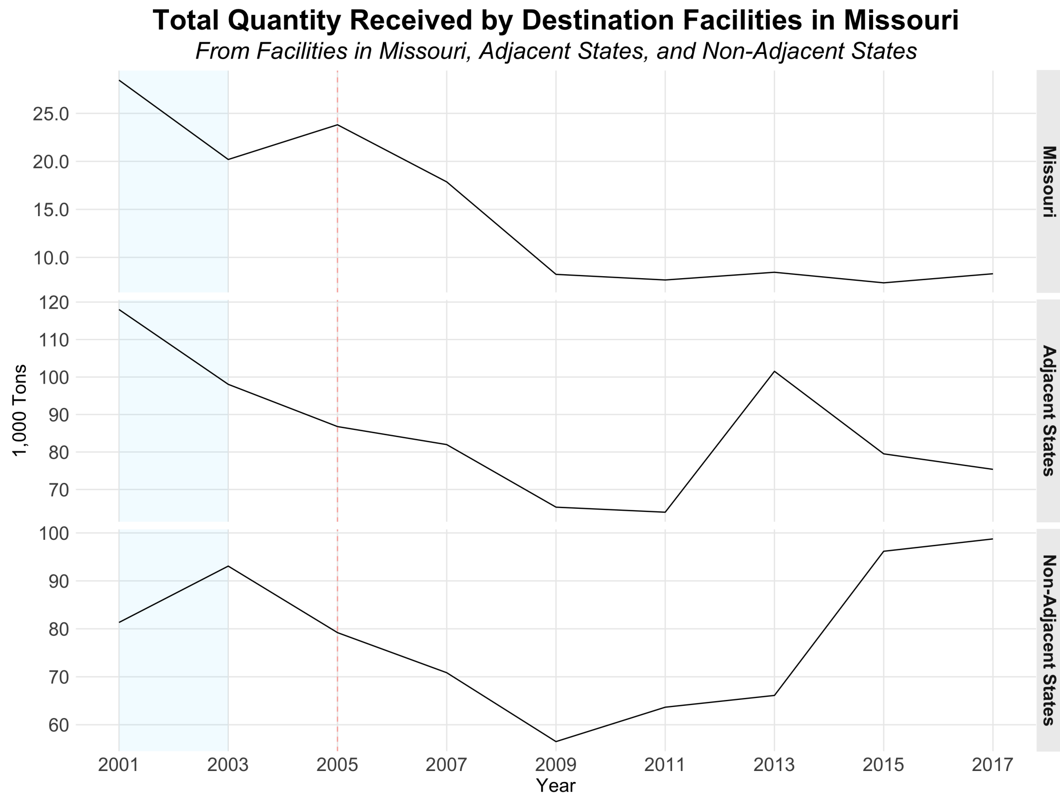
2. The Exclusive Treatment Assumption

Parallel pre-trends alone do not guarantee the validity of a DID estimate. For the control group to be an appropriate counterfactual, the treatment must only affect the treated group without influencing the outcome variable of the control group. In this study, this translates to a requirement that the tax introduction has no impact on the quantity that facilities in Missouri ship to destinations in the same state. Since the tax explicitly targets waste generated outside Missouri, it should not directly impact the quantity shipped within Missouri. However, indirect impacts are possible if TSDFs in Missouri adjust the service fee they charge on in-state waste in response to the tax introduction. TSDFs may increase the fee charged for smoothing out the tax-induced increase in the cost of operation, which can discourage shipment from the control group. On the other hand, TSDFs may decrease the fee to attract more in-state waste, which has a relatively lower regulatory cost, to fill the vacant capacity that follows a decrease in out-of-state quantity.

Time trends of the outcome variable in figure 9 show that the slope of the control group's trend is relatively stable between 2001 and 2007. Similarly, the slope of the trend of the share of intra-state shipment of the control group in 8 is relatively stable between 2001 and 2005. These patterns suggest that the tax introduction may not have an indirect impact on the control group.

An evaluation of how TSDFs in Missouri may have responded to the tax introduction *in general*, however, suggests a possibility of an indirect impact. Figure 10 shows the time trend of the total shipment quantity of non-wastewater hazardous waste received by destination facilities in Missouri. This figure pertains to 41 facilities in Missouri that were recipients of hazardous waste shipment in the BR data between 2001 and 2017 (not restricted to recipients of facilities in the sample of this analysis). Three horizontal panels correspond to three types of origins: Missouri, adjacent states, and non-adjacent states. Between 2001 and 2003, the quantity of shipment from Missouri and adjacent states decreased, whereas the quantity from non-adjacent states increased. Between 2003 and 2005, the quantity from other states, both adjacent and non-adjacent, shows a decreasing trend. Quantity from Missouri, on the other hand, slightly increased. This pattern reflects the possibility that TSDFs in Missouri may have made adjustments to attract more in-state waste in response to the tax introduction. Under that scenario, the estimate of the coefficient of interest will overstate the magnitude of the tax introduction.

Figure 10: Time trend of the total quantity received by facilities in Missouri by location of origins



3. The Confounding Assumption

In an ideal scenario, the only difference between the treated and the control group is that treated units receive the treatment whereas the controls do not. This condition does not hold for the sample of this analysis. In this case, facility fixed effects take care of differences in time-invariant facility characteristics, and control variables help mitigate against differences in the observable time-variant attributes. Nevertheless, some unobserved factors of the outcome variable may remain the error term. If these omitted factors correlate with the treatment and have a time-varying impact on the outcome variable, then the estimate of β will be biased. While it is not possible to empirically check for confounding factors, a comparison of group-specific averages of the observable covariates may provide some clue.

Table 3 compares the pre-treatment averages of covariates of the treated and control groups. Except for the averages of neighboring states' tax rates and the share of residents with education attainment beyond high school, t-statistics suggest that the pre-treatment averages of the two groups are different in a statistically significant manner. This raises a concern that there may exist an omitted variable in the error term that is correlated with treatment. If the omitted variable has a time-variant effect on the outcome variable, then the estimate of β will be biased. To address this concern, I re-estimate the coefficient of interest using matched samples. I present matched results after the discussion of the main results.

Table 3: Pre-treatment averages of control and treatment groups

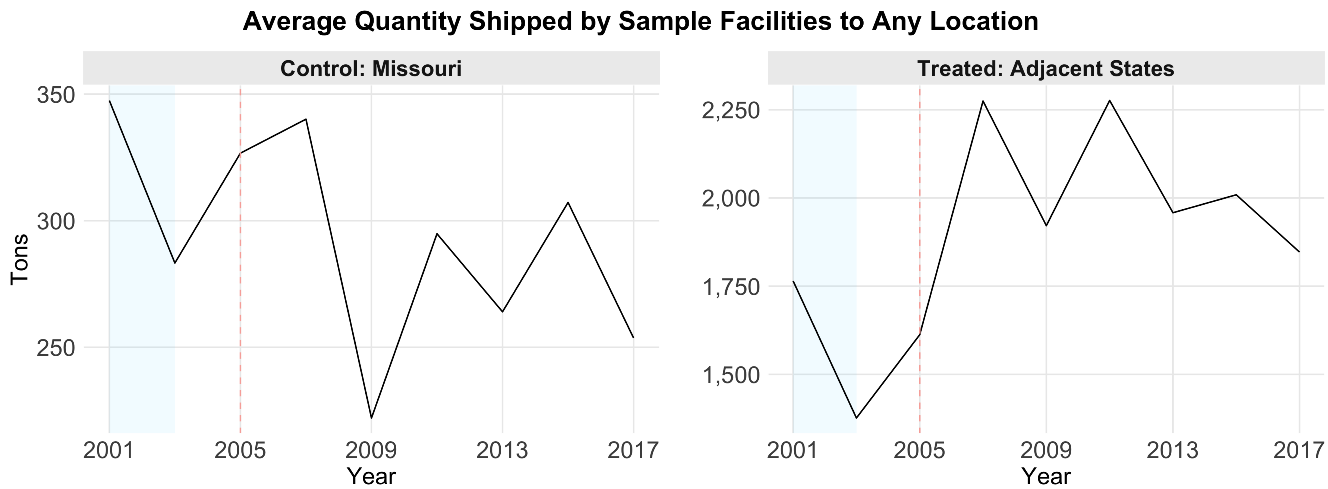
Pre-Treatment Averages (2001 and 2003)					
	Control **120 Facilities**	**Treatment** **157 Facilities**	**Difference**	**t-statistics**	
Quantity Shipped to MO (Tons)	186	615	429	-2.59	**
Total Quantity Shipped (Tons)	315	1,571	1,255	-3.62	**
In-State Disposal Tax ($/Ton)	0.00	21.91	21.91	-27.56	***
Average Tax of Neighbor States ($/Ton)	17.32	17.51	0.19	-0.50	
Share Non-White	21.93%	18.10%	-3.83%	2.55	**
Share Mfg. Employment	13.99%	15.52%	1.53%	-2.43	**
Share Over High-School Education	48.92%	47.83%	-1.09%	0.87	
Share Age 65 and Above	11.08%	12.97%	1.89%	-4.24	***
Population Density (per sq.mile)	1,334	903	-431	4.18	***
Median Income ($1k)	43.57	46.82	3.25	-2.28	**
Median Home Value ($1k)	126.23	112.27	-13.97	2.15	**

In addition to differences in unobserved characteristics of facilities addressed in table 3, other concurrent regulatory changes in Missouri may be another source of omitted variable bias. Regulations of Missouri can only impose costs on activities in Missouri. Consider two types of regulatory costs concerning hazardous waste handling: regulatory costs imposed on TSDFs and regulatory costs imposed on generators. The tax introduction, for example, imposes an additional regulatory cost on TSDFs in Missouri, but only for the management of waste from other states. Other regulatory changes that affect TSDFs' costs in other ways could have been implemented around the same time. Recall from figure 8 that the share of quantity shipped to Missouri decreases sharply around the time of tax introduction for both the control and treated groups. This pattern suggests that both types of facilities become less reliant on management and disposal services provided by TSDFs in Missouri over time. Such adjustment may result from policy changes in Missouri that cause an increase in service fees of TSDFs. This type of policy change does not pose an identification concern if it raises the cost uniformly for in-state and out-of-state waste, thereby creating no cross-group heterogeneity.

Policy changes that alter the regulatory cost faced by generators are also a potential concern for identification problems. An increase in regulatory cost may drive inefficient generators out of business, for example, causing a change in the composition of facilities in the control group over time. This does not pose a threat to the main analysis because the sample includes facilities that ship every year during the whole period of study. On the other hand, an increase in regulatory costs may incentivize facilities in Missouri to produce less hazardous waste and thereby ship less waste in general.

Figure 11 shows group-specific time trends of average shipment quantity to any location. The treated group (right) has an increasing trend, with a sharp increase between 2003 and 2007. The control group has a slightly decreasing trend with a significant drop between 2007 and 2009. Comparing these two trends may point to the concern that generators in Missouri were affected by a different set of policy changes during that time. To mitigate this concern, I include total waste shipment quantity as a control variable. In addition, notice that the average values of the treated group are significantly greater than the control group's averages. I address this disparity by matching on total shipment quantity.

Figure 11: Time trend of total quantity shipped by sample facilities to any location



Event Study

To evaluate how the effect of the tax introduction on quantity shipped to Missouri varies over time, I conducted an event study. Let y be the number of bi-annual periods since the treatment took place. $y = \{-2, -1, 0, 1, ..., 6\}$. The event study regression equation that corresponds to the model equation (1) is:

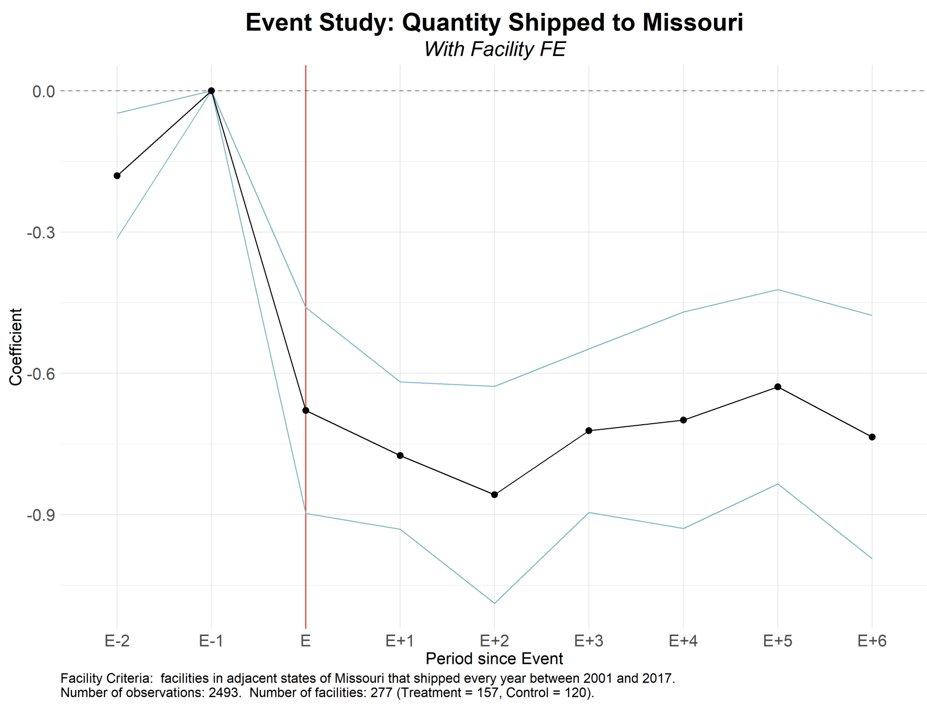
$$IHS(q)_{kt} = \alpha_k + y_t + \sum_{y=-1}^{y=6} \gamma_y \mathbf{1}\{t = 2003 + treat_k 2(y+1)\} + \eta IHS(s)_{kt} + \boldsymbol{\tau' T} + \boldsymbol{\gamma' D} + \epsilon_{kt} \quad (2)$$

Note that for the control group, the event indicator $\mathbf{1}\{t = 2003 + 2treat_k(y+1)\} = 1$ only for $y = -1$. The coefficient on the period before the event $(E-1)$, which corresponds to 2003, is omitted.

Figure 12 below shows the estimates of event study coefficients and their standard errors.

The estimates of the event period (E) and post-event periods ($E+1, ..., E+6$) are negative and statistically significant. This pattern suggests that the tax introduction reduces the quantity shipped to Missouri immediately after its implementation. The impact remains relatively stable over time, decreasing only slightly after $E-3$. The estimate of the coefficient of the indicator of two periods before the event ($E-2$), however, is also negative and statistically significant. Even though its magnitude is substantially smaller than the estimates for the event and post-event periods, this still raises a concern that the control facilities may not serve well as a counterfactual for the treated group even after controlling for total quantity shipped, taxes, and demographic variables.

Figure 12: An event study that corresponds to model 4



4.2 Main Results

Table 4 below shows estimates of the coefficients in equation (1). Four columns correspond to four models, each of which includes a different set of control variables. Model 1 only includes the total quantity shipped (s_{kt}). Model 2 adds demographic variables to model 1 ($\boldsymbol{D}$). Model 3 adds tax variables to model 1 ($\boldsymbol{T}$). Model 4 adds both the tax and the demographic variables to model 1. All models are estimated with facility fixed effects and year fixed effects. Standard errors are clustered at the state level.

The estimates of the coefficient on $post \times treat$ are negative and statistically significant in all four models. They suggest that the tax introduction decreased the quantity shipped to Missouri by 45% to 48%. As expected, the estimates of the coefficient on total quantity shipped are positive and statistically significant. This suggests that facilities that ship more waste overall also ship more waste to Missouri.

Table 4: Main Results: Coefficient estimates of the difference-in-difference analysis

Variable	**IHS(Quantity Shipped to Missouri in Tons)**			
	Model			
	(1)	**(2)**	**(3)**	**(4)**
Post × Treat (Outside MO)	-0.568^{**}	-0.569^{**}	-0.629^{**}	-0.633^{***}
	(0.207)	(0.204)	(0.188)	(0.188)
IHS(Total Quantity Shipped)	0.445^{***}	0.448^{***}	0.444^{***}	0.447^{***}
	(0.048)	(0.05)	(0.051)	(0.053)
Average Tax of Neighboring States ($/Ton)			0.228	0.252
			(0.3)	(0.296)
Post x Neighbor Tax			−0.012	−0.009
			(0.03)	(0.03)
Share Non-White		0.996		0.973
		(1.452)		(1.429)
Median Income ($1k)		−0.011		−0.011
		(0.008)		(0.008)
Median Home Value ($1k)		0.005^{***}		0.005^{***}
		(0.001)		(0.001)
Share Mfg. Employment		0.291		0.248
		(1.086)		(1.068)
Share Over High-School Education		0.102		0.189
		(1.28)		(1.203)
Share Age 65 and Above		-2.102^{***}		-2.178^{***}
		(0.489)		(0.504)
Population Density (1k/sq.mile)		0.043		0.049
		(0.248)		(0.251)
Observations	2,493	2,493	2,493	2,493

*p<0.1; **p<0.05; ***p<0.01.
Facility fixed effects and year fixed effects are included; cluster SE at state level.

4.3 Robustness Check

As discussed previously, I used three sample criteria to construct four samples as shown in table 2. In addition, I work around the temporal misalignment of the BR and ACS data by specifying three ways of assigning the ACS demographic data to facilities in the BR data, as shown in the diagram of figure 3. Estimates from the main analysis in table 4 above correspond to sample 4 and BR-ACS assignment option 1. In this section, I discuss results from two sets of robustness analyses that re-estimate the impact of the tax introduction using alternative samples. First, I present a comparison of results from the four sample alternatives and three BR-ACS assignment options. Second, I present results from 78 matched samples that are derived from the sample that underlies the main analysis.

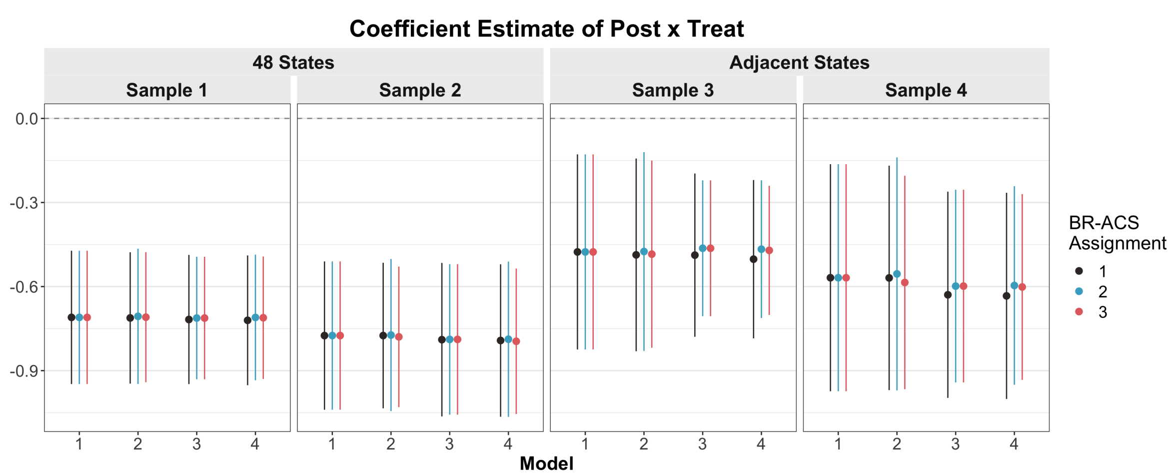
Alternative Samples and BR-ACS Assignment Options

Figure 13 shows 48 estimates of the coefficient on post×treat form four samples (vertical panels), four regression models (dot clusters), and three BR-ACS assignment options (dot colors). Results from table 4 correspond to the black dots in the right-most panel (sample 4). Sample 1 and 2 include facilities in Missouri and other 47 mainland states. Sample 3 and 4 include facilities in Missouri and its adjacent states. Sample 2 and 4 satisfy the post-tax shipping criterion.

All estimates are negative and statistically significant. The choice of BR-ACS assignment only slightly changes the magnitude of the estimates. Estimates from adjacent-state samples are noticeably smaller in magnitude than estimates from 48-state samples. Overall, these results support the conclusion from the main analysis that the tax introduction reduces hazardous waste inflow to Missouri.

Recall that elasticity estimates from the main result range between a reduction of 45% and 48% in shipment to Missouri. This is an overestimate in comparison to results from sample 3, and is an underestimate in comparison to results from sample 1 and 2. While the estimated magnitude of the effect varies across sample options, the sign and statistical significant are robust.

Figure 13: Estimates of coefficient on post×treat using alternative samples and BR-ACS assignment options



Matched Samples

Matched Sample Construction

I repeated the difference in difference analysis using matched samples that are derived from the sample that underlies the main analysis. These matched samples differ by 3 matching attributes: matching method, facility characteristics, and demographic characteristics. I employ two matching methods: coarsened exact matching (CEM) and 1-to-1 propensity score matching (PSM). For CEM matching, I divide each continuous variable into 4 bins using the 25th, 50th, and 75th percentile values as cutoffs. I specify 13 sets of facility characteristics that are used in matching (see table 5), all of which include industry group but differ by the type of generation and/or shipping quantity it includes (all waste, non-wastewater only, or both). There are five categories of industry groups: manufacturing, waste management, transportation and warehouse, public administration, and other industry. I specify three sets of demographic characteristics that are used in matching. I refer to these as matching group A, B, and C. The set of demographic characteristics of group A is an empty set, that is, the matching procedure of this group uses no demographic characteristics. Group B uses population density, and group C uses both population density and the share of manufacturing employment.

These three matching attributes produce $2 \times 13 \times 3 = 78$ matched samples in total. The last two columns of table 5 specify matched samples with improved pre-trend of the IHS-transformed quantity shipped to Missouri[14] (corresponds to figure 9). Out of 78 matched samples, 27 have improved pre-trend. For example, using all and non-wastewater shipping quantity without using generation quantity in matching (facility characteristic set S3) results in improved pre-trend for all CEM matching regardless of the set of demographic characteristics and results in improved pre-trend for the PSM matching that does not use any demographic characteristic (group A).

All variables used in matching are from 2001, which is the first pre-treatment year in the data. The pool of facilities from which matched samples are selected includes 210 facilities: 82 are in Missouri and 128 are outside Missouri. Note that this is a subset of the original sample, which has 227 facilities. Seventeen facilities are excluded from the matching pool because they did not directly submit the BR report as a handler in the pre-treatment period (but appear in the data because other facilities reported transactions with them). I do not have pre-tax information necessary for matching these facilities.

Figure 14 shows the number of facilities in each matched sample. The left-most bar corresponds to the original sample: 277 total, 120 in Missouri, and 157 outside Missouri. The second-left-most

[14] I specify a matched sample as having an improved pre-trend if its the absolute value of difference in group-specific slopes (treated vs. control) of the line that connects 2001 and 2003 values in the time trend plot (analogous to 9) is smaller than that of the original sample.

bar corresponds to one-to-one PSM matched samples, all of which have the same number of facilities (162 total, 81 in Missouri, and 81 outside Missouri). For CEM matched samples, the number of facilities in group A is between 175 and 203, the number of facilities in group B is between 130 and 190, and the number of facilities in group C is between 55 and 119.

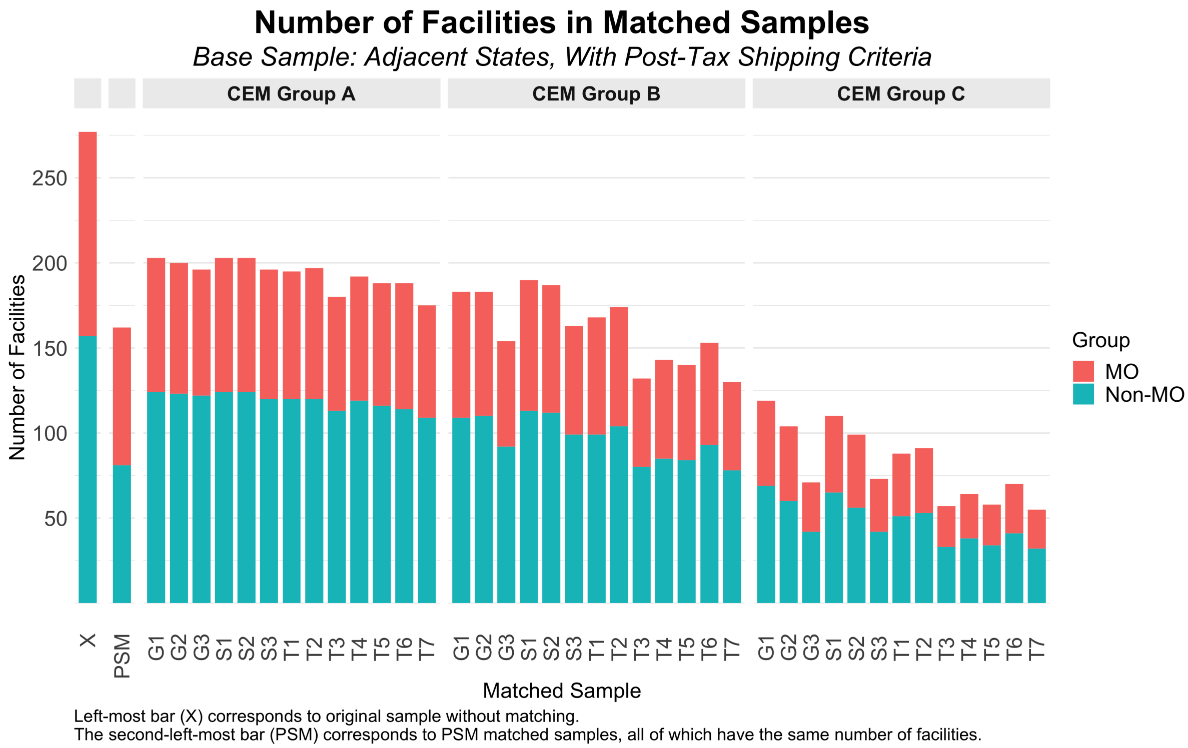
Recall that, as shown in table 3, the pre-treatment averages of the treated and control groups are imbalanced for some observable characteristics. Matching resolves this issue for variables that are used in matching. While some demographic characteristics exhibit imbalanced pre-trend averages, I believe that it is important to prioritize matching on facility characteristics.

Figure 27 in the appendix shows the cross-group difference of pre-treatment averages of the quantity of non-wastewater shipment to Missouri (dependent variable) of the matched samples relative to the original sample. Similarly, figure 28 shows a plot for the difference in total shipping quantity of non-wastewater, and figure 29 shows a plot for the difference in population density. PSM matching significantly improves the similarity in total quantity shipped across all three matching groups.

Table 5: 13 combinations of generation and shipping quantities used in matching

Set of Facility Characteristics		**Matched Quantity**		**Improved Pre-Trend**	
		Generation	**Shipping**	**CEM**	**PSM**
1	G1	All	-	A	C
2	G2	Non-Wastewater	-	A	A, B
3	G3	Both	-	A, B	
4	S1	-	All	A, C	
5	S2	-	Non-Wastewater	A, C	
6	S3	-	Both	A, B, C	A
7	T1	All	All	A	
8	T2	Non-Wastewater	Non-Wastewater	A	
9	T3	Both	All	A, B	
10	T4	Both	Non-Wastewater	A	
11	T5	All	Both	B	A
12	T6	Non-Wastewater	Both	A, C	A
13	T7	Both	Both	A	A

Figure 14: Number of facilities in matched samples



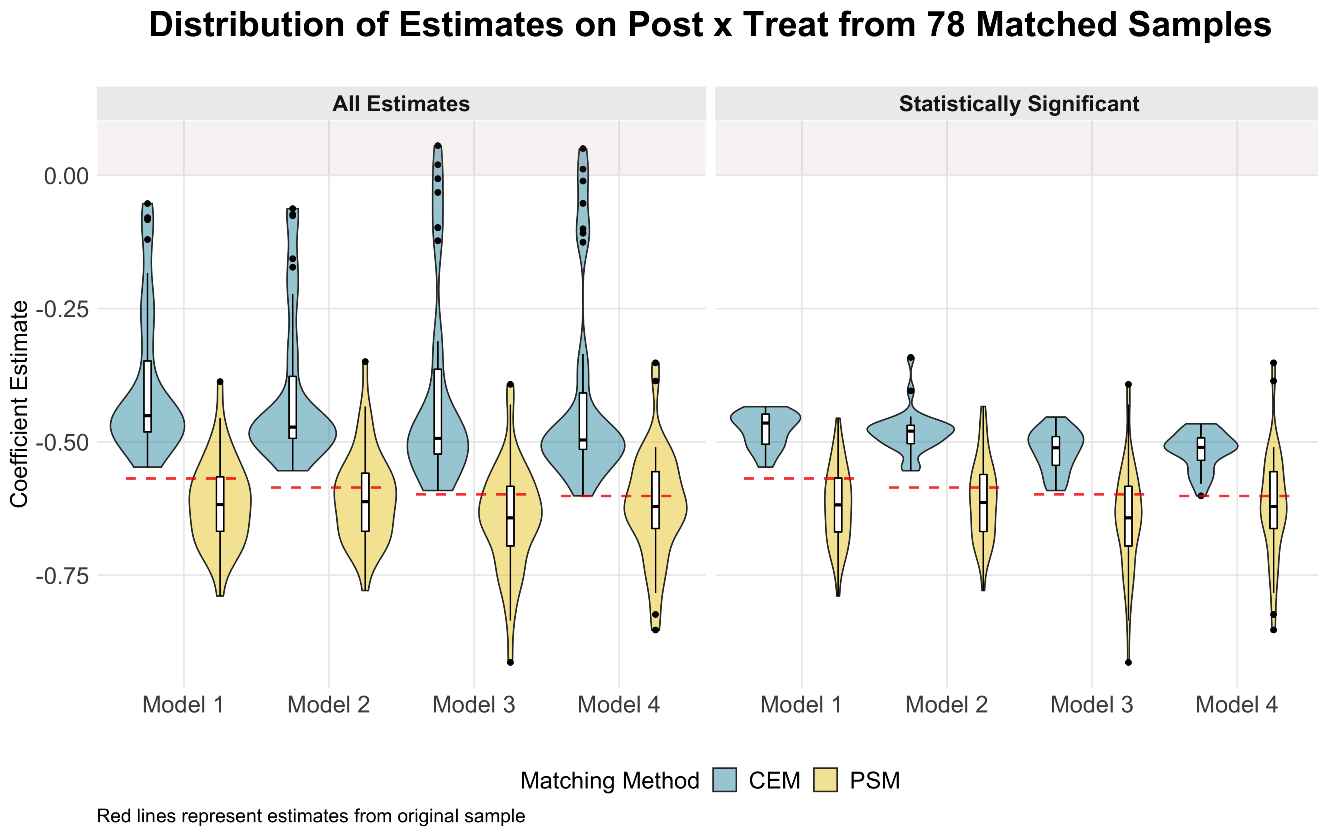
Matched Results

Figure 15 shows distributions of coefficient estimates on post×treat from all 78 samples in the form of violin plots and box plots.[15] The left panel shows the distribution of all estimates, and the right panel shows the distribution of statistically significant estimates. For each model, the left violin plot (blue) is the distribution of estimates from CEM matched samples, and the right violin plot (yellow) is the distribution of the estimates from the PSM matched samples. The red horizontal lines represent estimates of the original sample from each model.

All estimates are negative, with a few exceptions of estimates of model 3 and 4 of the CEM matched samples. These estimates, however, are not statistically significant. For all models, the entire distribution of CEM estimates (blue) lies above the original estimate, with an exception of one statistically significant estimate from model 4 (see the right panel). PSM matched samples (yellow), on the other hand, produce more estimates that are greater in magnitude than the original estimates. Among statistically significant results, CEM estimates have a lower variance than PSM estimates. This is because many of the CEM estimates, particularly those in group C, have relatively small sample sizes which produce statistically insignificant estimates.

[15]Each of the figure is a violin plot with corresponding box plot on the inside. The outline of a violin plot is the kernel density of the data, mirrored across a vertical axis, giving a violin-like shape.

Figure 15: Coefficient estimates on post×treat from matched samples

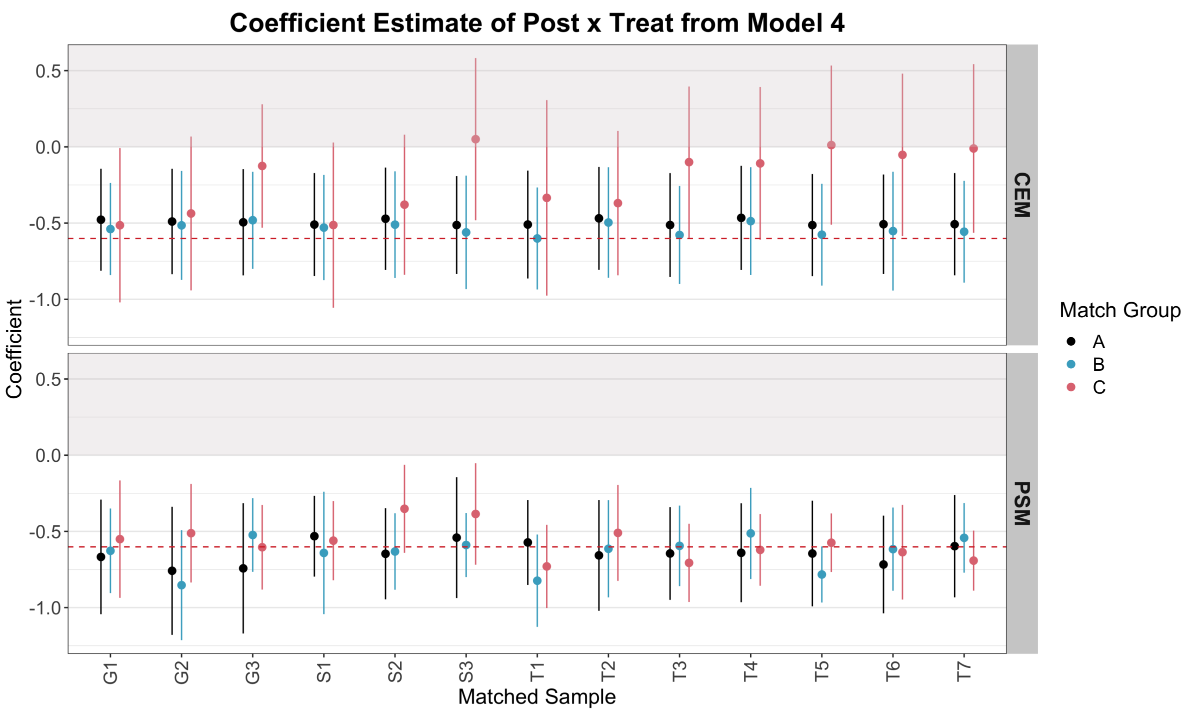


Model 4 is the preferred specification since it includes a full set of control variables (total quantity shipped, tax, and demographics). Figure 16 shows the coefficient estimates on post×treat of model 4 of 78 matched samples. The upper panel contains estimates from CEM matched samples. The lower panel contains estimates from PSM matched samples. These correspond to the rightmost violin plots in each panel of figure 15. Each group of 3 dots are estimates from the matched samples with the same set of facility characteristics used in matching (G1, G2, ..., T7), and each dot correspond to each set of demographic characteristics used in matching: group A (left-black), B (middle-blue), and C (right-pink), respectively. The red horizontal line shows the estimate of the original sample.

All CEM estimates (upper panel) are greater than the original estimate except for matched sample T1B. Two CEM estimates are positive (S3C and T5C), both of which are not statistically significant. All of the CEM estimates from group C are not statistically significant, except for the estimate from matched sample G1. All PSM estimates (lower panel), on the other hand, are negative and statistically significant. The magnitudes are quite similar to the original estimate (-0.60), except for estimates from matched sample S2C (-0.35) and S3C (-0.39) which are noticeably smaller in magnitude.

Overall, results from matched samples suggest that the estimates from the original sample is likely robust at least in terms of its negative sign. That is, the tax introduction effectively discouraged waste flow into Missouri from facilities in adjacent states. The magnitude of the original sample may be an over estimate, however. The smallest statistically significant estimate among the matched samples is -0.352 from matched sample PSM S2C. This translates to an elasticity of -0.304. That is, it suggests that the tax introduction reduces the quantity of waste shipped into Missouri by approximately 30% in comparison to 48% according to the original sample.

Figure 16: Coefficient estimates of post×treat in model 4 from regressions of matched samples



Estimated with facility FE. Model 4: RHS variables include total quantity shipped, demographics, and disposal tax.
Sample: Adjacent States, With Post-Tax Shipping Criteria. Red line represents estimate using original sample.

5 The Environmental Justice Implication of the Tax Introduction

Results from section 4 show that the tax introduction led to a decrease in waste flow from other states into Missouri. In this section, I present an analysis that investigates whether the diversion of HW flow has environmental justice implications. More specifically, I evaluate whether the correlation between shipment quantity and the share of non-white residents of the destination facility's census tract changed after the tax incidence.

5.1 Analysis Design

The outcome variable of interest is the quantity of waste flow from origin facility k to destination facility j in year t. The explanatory variables of interest are the share of non-white residents at the destination, which is assumed to reflect the degree of marginalization of affected communities.

Since the objective of this analysis is to evaluate the environmental justice implications of the "diversion" of HW that follows Missouri's tax introduction, I focus on shipments from affected origins to destinations outside of Missouri. These affected origins include 153 facilities in the treated group of the analysis in section 4. Four facilities are excluded because they only ship to destinations in Missouri.

For each origin facility k in year t, I assign a set of potential destination facilities using the following criteria. Facility j is considered a potential destination of origin k in year t if (1) it received waste from origin k at least once between 2001 and 2017 and (2) it received some waste in year t (i.e. was operating). There are 288 destination facilities that satisfy these criteria. Figure 17 shows the number of origin facilities in each state, and figure 18 shows the number of destination facilities.

The resulting origin-destination-year panel consists of 12,611 observations, including 1,624 unique origin-destination pairs. Approximately 66% of the observations have zero shipment quantity. The distribution of shipment quantity is very right-skewed (see figure 30 in the appendix). In the regression equation below, I use the IHS transformation of the shipment quantity as the dependent variable, denoted by $IHS(q)_{kjt}$. $Post_t$ indicates observations from 2005 and after. $NonWhite_{jt}$ denotes the share of non-white residents in the census tract of destination j in year t. I include the interaction term of $NonWhite_{jt}$ with $Post_t$ to estimate the change in their correlations with the waste flow after the tax introduction.

Figure 17: Origin facilities in the environmental justice analysis (affected origins)

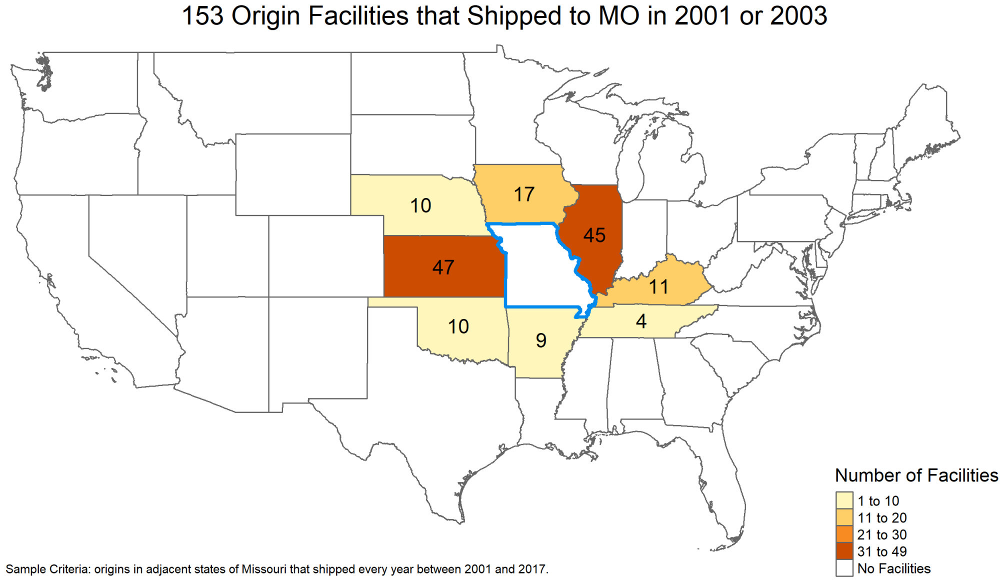
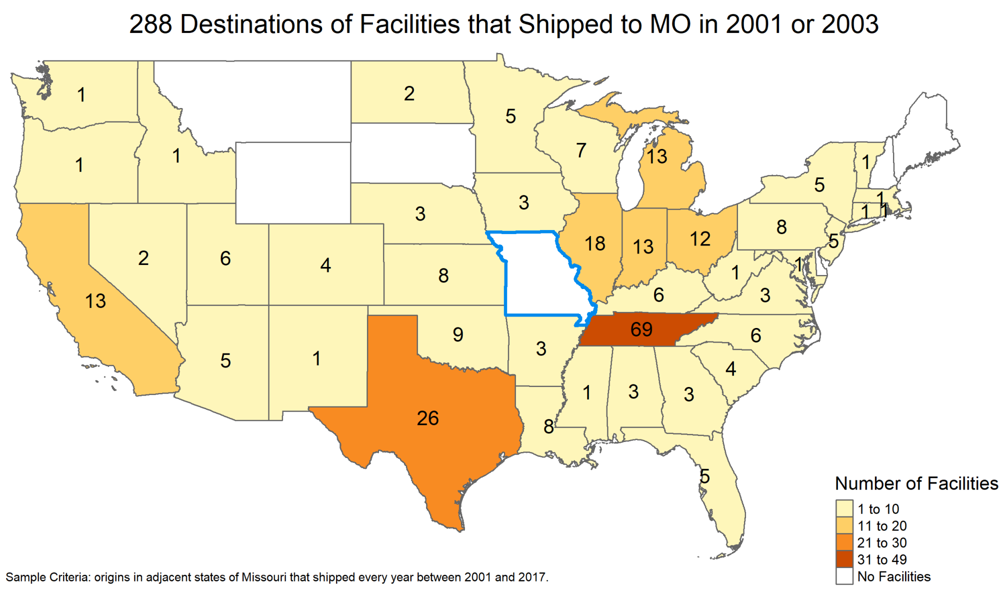


Figure 18: Number of destination facilities that received waste from affected origin facilities



There are six groups of control variables. The first group includes two types of facility-specific waste quantity: $IHS(s)_{kt}$ denotes the IHS-transformed total quantity shipped by origin k in year t and $IHS(r)_{jt}$ denotes the IHS-transformed total quantity received by destination j in year t. The second group includes two proximity variables: distance between origin and destination facilities ($Distance_{kj}$) and an indicator of origin and destination being in the same state $SameState_{kj}$.

The third group is a set of other demographic characteristics of destinations, denoted by $\boldsymbol{D}$, which includes median income, the share of manufacturing employment, the share of residents with education attainment beyond high school, the share of population age over 65, median home value, and population density. The fourth group is a set of demographic characteristics of origins, denoted by $\boldsymbol{O}$. This includes all characteristics in $\boldsymbol{D}$ and the share of non-white residents of origin. The fifth group includes the per-ton disposal tax on waste shipment from origin k to destination j in year t (Tax_{kjt})[16]and its interaction with $Post_t$. The last group consists of one variable: the interaction of $Post_t$ and $SameState_{kj}$.

Year fixed effects (y_t), origin fixed effects (o_k), and destination fixed effects (d_j) are included. Standard errors are clustered at the origin state level. I also run an analysis with pair fixed effects added. This only marginally changes the magnitude of the estimate without meaningful implication on the sign or statistical significance. ϵ_{kjt} represents unexplained variation in $IHS(q)_{kjt}$.

$$\begin{aligned} IHS(q)_{kjt} = y_t &+ o_k + d_j \\ &+ \beta_1 NonWhite_{jt} + \beta_2 Post_t \times NonWhite_{jt} \\ &+ \eta_1 IHS(s)_{kt} + \eta_2 IHS(r)_{jt} \\ &+ Distance_{kj} + SameState_{kj} \\ &+ \boldsymbol{\gamma}'\boldsymbol{D} + \boldsymbol{\lambda}'\boldsymbol{O} \\ &+ \tau_1 Tax_{kjt} + \tau_2 Post_t \times Tax_{kjt} \\ &+ \psi Post_t \times SameState_{kj} + \epsilon_{kjt} \end{aligned} \tag{3}$$

There are two coefficients of interest. β_1 measures the pre-tax correlation between the quantity of waste flow and the share of non-white residents at the destination. β_2 measures the change in that correlation after the tax introduction. $\beta_1 > 0$ corresponds to a case of environmental *injustice* in the pre-tax period. They suggest that destinations with a higher share of non-white residents receive relatively more waste shipment and thereby bearing a larger burden of negative externalities of HW management. $\beta_2 < 0$ corresponds to an improvement in the post-tax period.

5.2 Summary Statistics

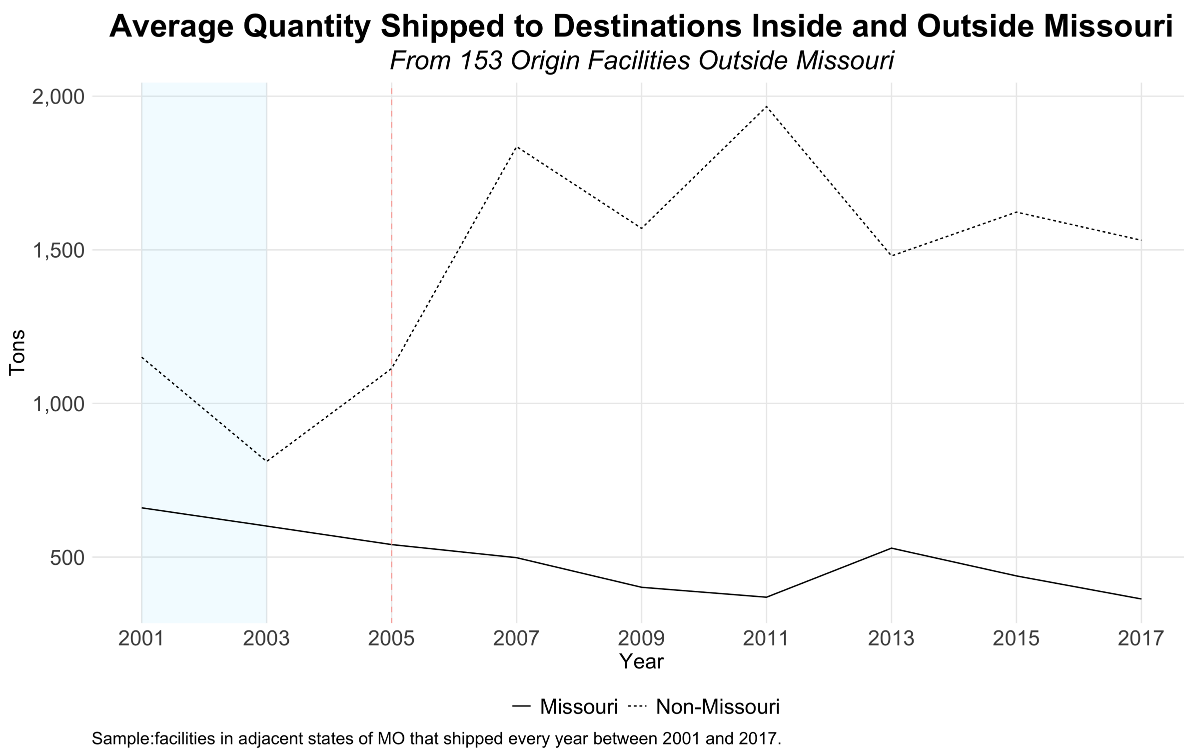
Quantity and Number of Destinations

Figure 19 shows the average quantity of non-wastewater shipment from affected origin facilities to destinations in Missouri and outside Missouri over time. The average quantity shipped to Missouri (blue) shows a decreasing trend, whereas the average quantity shipped to destinations

[16]Louisiana, Mississippi, and South Carolina impose "retaliatory" taxes. Each of these states charges the maximum between its out-of-state tax rate and the rate of the state where the waste is generated. In addition, some states, like Missouri, charge an additional flat fee for the management of waste from other states. For this reason, the tax rate is a time-varying pair-specific variable.

outside Missouri increased sharply between 2003 and 2005 and remained relatively high in the post-tax years relative to the averages in the pre-tax years. This suggests that some waste might have been diverted from Missouri to other states after the tax introduction in 2005.

Figure 19: Average quantity shipped to destinations inside and outside Missouri



Share of Non-White Residents

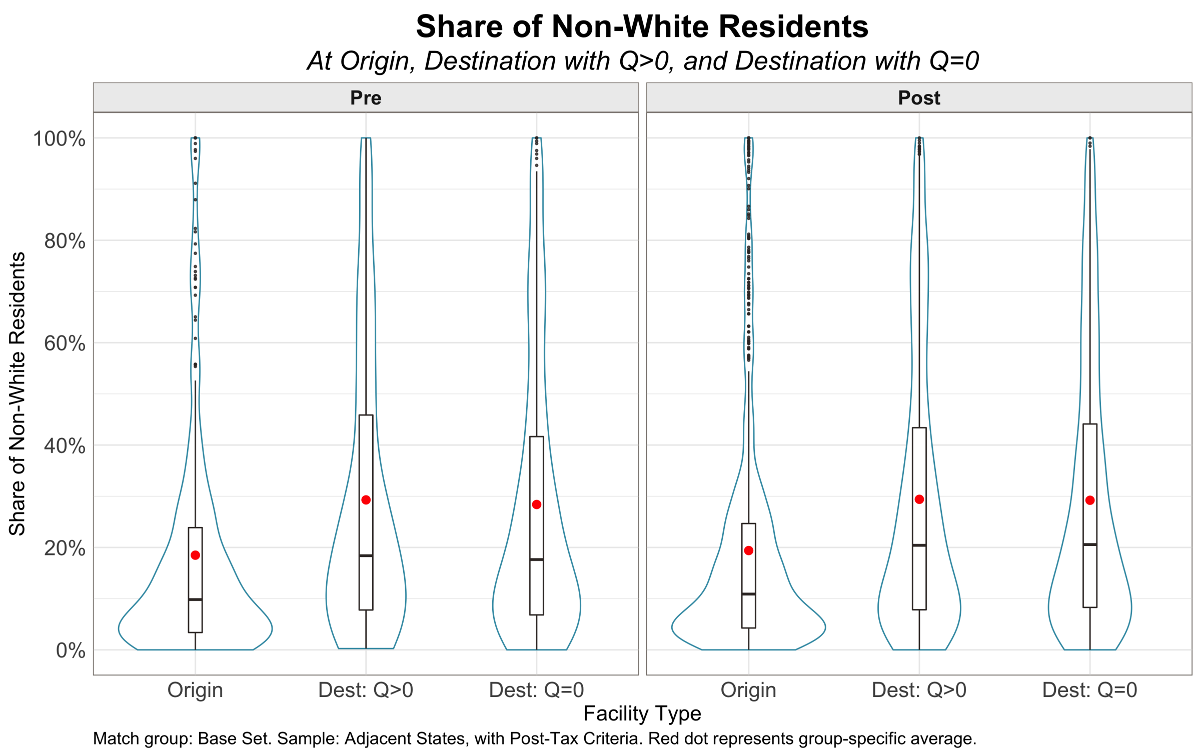
A consideration of the relative distribution of the share of non-white residents between the origin and destination facilities is necessary for providing an environmental justice interpretation to the coefficient estimates of interest. Recall that 66% of the observations have zero shipment quantity. By including them in the analysis, I assume that these observations are relevant to estimating the correlation between waste flow quantity and the degree of marginalization at destinations. More specifically, for each observation of origin k and destination j in year t, the destination is treated as if it is a "potential" destination of that origin in that year. In the case where shipment quantity is zero, that observation represents a case where origin k *chooses* not to ship to destination j in year t. Thus, zero quantity is meaningful in the context of environmental justice because it implies that the origin transfers no negative externalities to the destination, even though it could have done so.

In the case of HW shipment, environmental injustice is probable if the pattern of shipment exhibits the following characteristics. First, destinations are more marginalized than origins. Second, chosen destinations (positive quantity) are more marginalized than non-chosen destinations (zero quantity). Third, among chosen destinations, the degree of marginalization positively correlates with shipment quantity. The tax introduction likely has environmental justice implications if these characteristics of the shipment pattern in the pre-tax period are different from the characteristics of the shipment pattern in the post-tax period.

In this section, I evaluate the first and second characteristics of shipment patterns by comparing the pre-tax and post-tax averages and distributions of the share of non-white residents at origins and destinations as well as the pair-specific disparity between observations with zero quantity and observations with positive quantity.

Figure 20 shows pre-tax and post-tax distributions of the share of non-white residents of origin facilities and two types of destination facilities: actual destinations that received waste flow from the origins ($Q > 0$) and potential destinations that are within the choice set but did not receive any waste in a particular year ($Q = 0$). The left panel shows the pre-tax distributions, whereas the right panel shows the post-tax distributions. It is apparent that, overall, origin facilities have a lower share of non-white residents than both types of destination facilities.

Figure 20: Distribution of the share of non-white residents at origin and destination



Panel A of table 6 below shows the pre-tax averages and post-tax averages of share of non-white residents at the destination as well as their difference and the corresponding t-test for three groups of observations: all observation, observations with positive shipping quantity, and observations with zero shipping quantity. The last two rows correspond to the averages of the two destination groups in figure 20 (represented as red dots). Panel B shows these statistics for destination-origin difference (disparity) in the share of non-white residents. This type of statistics for other variables including destination demographic variables, pair-specific disparity of each demographic variable, pair-specific disposal tax, total quantity received by destination, total quantity shipped by origin, and distance can be found in table 9, 10, and 11 in the appendix.

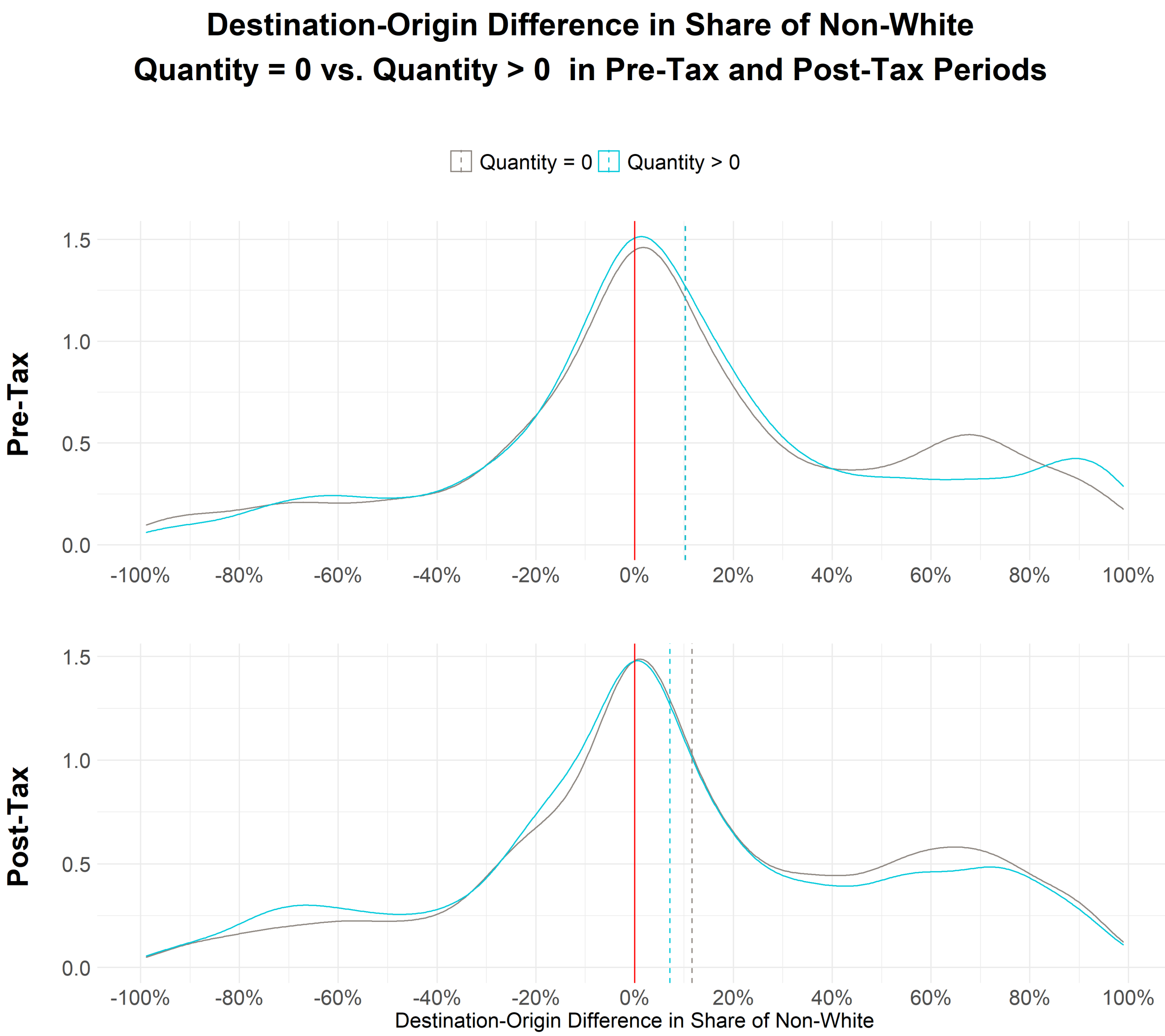
Statistics in panel B of table 6 shows that, for all observations, the average disparity in the share of non-white residents across all observations is positive in both the pre-tax and post-tax periods. The share of non-white residents at destinations is 10% higher than the share at origins which corresponds to a case of environmental injustice. The t-test suggests that the pre-tax and post-tax averages are not statistically significantly different. For observations with zero quantity, the pre-tax average (10.30%) is slightly smaller than the post-tax average (11.63%), but they are not statistically significantly different. For observations with positive quantity, the pre-tax average (10.22%) is larger than the post-tax average (7.16%). The t-test suggests that these averages are statistically significantly different, implying that the disparity in the share of non-white residents between chosen destinations and origins decreased after the tax.

Table 6: Pre-tax vs. post-tax averages of share of non-white residents

Variable	**Pre-Tax**	**Post-Tax**	**Difference**	**t-statistic**
(A) Share of Non-White Residents at Destination				
All Observations	34.74%	35.70%	0.96%	-1.38
Observations with $Q > 0$	35.29%	34.48%	-0.81%	0.65
Observations with $Q = 0$	34.48%	36.34%	1.87%	-2.20^{**}
(B) Destination-Origin Difference in Share of Non-White Residents				
All Observations	10.27%	10.09%	-0.18%	0.20
Observations with $Q > 0$	10.22%	7.16%	-3.06%	1.89^{*}
Observations with $Q = 0$	10.30%	11.63%	1.33%	-1.16

Figure 21 shows four kernel densities of the disparity in the share of non-white residents. A visual inspection of these densities provides a similar conclusion. The top panel is an overlay of two pre-tax densities: the grey density corresponds to observations with zero quantity, and the blue density corresponds to observations with positive quantities. Similarly, the lower panel shows an overlay of two densities from the post-tax periods. The vertical dashed lines are the averages that are specific to each type of observation which corresponds to the averages in table 10 and 11. Notice that in the pre-tax period, the averages of the disparity in the share of non-white residents overlap almost perfectly. In the post-tax period, however, the positive-quantity average is noticeably lower than the zero-quantity average.

Figure 21: Kernel density of destination-origin difference in share of non-white residents



5.3 Main Results

Estimates of the coefficients on the share of non-white residents of destinations and their interactions with the post-tax indicator are shown in table 7, together with corresponding elasticities. The elasticities are computed using the pre-tax averages of all observations. Five columns correspond to five models. Each model includes a different set of control variables in equation (3).

Model 1 includes the quantity controls (total quantity shipped by origins and total quantity received by destinations) and proximity variables (distance and same-state indicator). Model 2 adds demographic characteristics of destinations to model 1. Model 3 adds demographic characteristics of origins to model 2. Model 4 adds tax variables to model 3. Model 5 adds the interaction of the same-state dummy variable and post-tax indicator to model 4. The full result table with coefficient estimates of all variables on can be found in table 12 in the appendix.

Table 7 shows that the estimates of the coefficient on the share of non-white residents at destinations are positive and not statistically significant in all models. These estimates vary between 0.446 and 0.515, which correspond to elasticities of 0.16% to 0.18%. These can be interpreted as a 1% increase in the share of non-white residents is correlated with a 0.16% to 0.18% increase in shipment quantity in the pre-tax periods. The estimates of the coefficient on the interaction of the share of non-white residents and the post indicator, on the other hand, are negative and statistically significant in all models. These estimates range between -0.44 and -0.37, which suggests that the elasticities of shipment quantity with respect to the share of non-white residents at destinations decreased by 0.13 to 0.15 percentage points after the tax introduction.

To assign environmental justice interpretation to these results, it may be useful to consider the implications of the point estimates and their statistical inferences separately. In terms of point estimates, these results suggest that non-white communities were more exposed to HW shipments from these origin facilities and the reallocation that follows the tax introduction is associated with a reduction in that exposure. This can be interpreted as an alleviation in environmental injustice after the tax introduction.

In terms of statistical inference, however, these results suggest that non-white communities might have not been more exposed to HW relative to white communities because the correlations between the share of non-white residents at destinations and waste flow quantity are statistically insignificant in all models. As discussed previously, however, the pre-tax distribution of the share of non-white residents of origin facilities is visibly more left-skewed than destinations, as shown in figure 20. This invites us to refrain from concluding that the pre-tax shipping pattern is truly free of environmental injustice. As I will discuss further in the robustness section, results from matched samples indicate that the pre-tax correlation is likely statistically significantly greater than zero.

Table 7: Main Results: Coefficient estimates and elasticities of share of non-white at destination

Estimates	**Dependent Variable: IHS(Quantity Flow)**				
	Model				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Coefficients					
Share of Non-white at Dest	0.458	0.516	0.475	0.449	0.438
	(0.378)	(0.447)	(0.428)	(0.435)	(0.453)
Post × ...	-0.437^{***}	-0.408^{**}	-0.395^{**}	-0.368^{**}	-0.365^{**}
	(0.093)	(0.125)	(0.128)	(0.147)	(0.145)
Elastcities (%)					
Share of Non-white at Dest	0.159	0.179	0.165	0.156	0.152
Post × ...	-0.152^{***}	-0.142^{**}	-0.137^{**}	-0.128^{**}	-0.127^{**}
Control Variables					
Total Quantities	YES	YES	YES	YES	YES
Destination Demographics	NO	YES	YES	YES	YES
Origin Demographics	NO	NO	YES	YES	YES
Tax	NO	NO	NO	YES	YES
Same State × Post	NO	NO	NO	NO	YES

*p<0.1; **p<0.05; ***p<0.01. Number of observations = 12,611.
Include origin, destination, and year fixed effects; cluster SE at origin state level.
Elasticities are computed using pre-tax averages of all destinations.
These estimates are based on data from BR-ACS matching option 1 as in figure 3.

5.4 Robustness Check

I perform two sets of robustness checks for the environmental justice analysis analogously to the robustness check for the difference in difference analysis. First, I present results from alternative samples and BR-ACS assignment options. Second, I present results from matched samples of sample 4.

Alternative Samples and BR-ACS Assignment Options

As mentioned previously in the data description section, the choice of BR-ACS assignment option affects the estimates of the coefficient of interest in this analysis. Table 8 below presents estimates from model 5. Each of the three columns corresponds to the three BR-ACS assignment options as shown in figure 3 in the appendix. The first column of this table corresponds to the fifth column in table 7. Different BR-ACS assignment options produce estimates that have the same sign and statistical significance but vary in magnitudes.

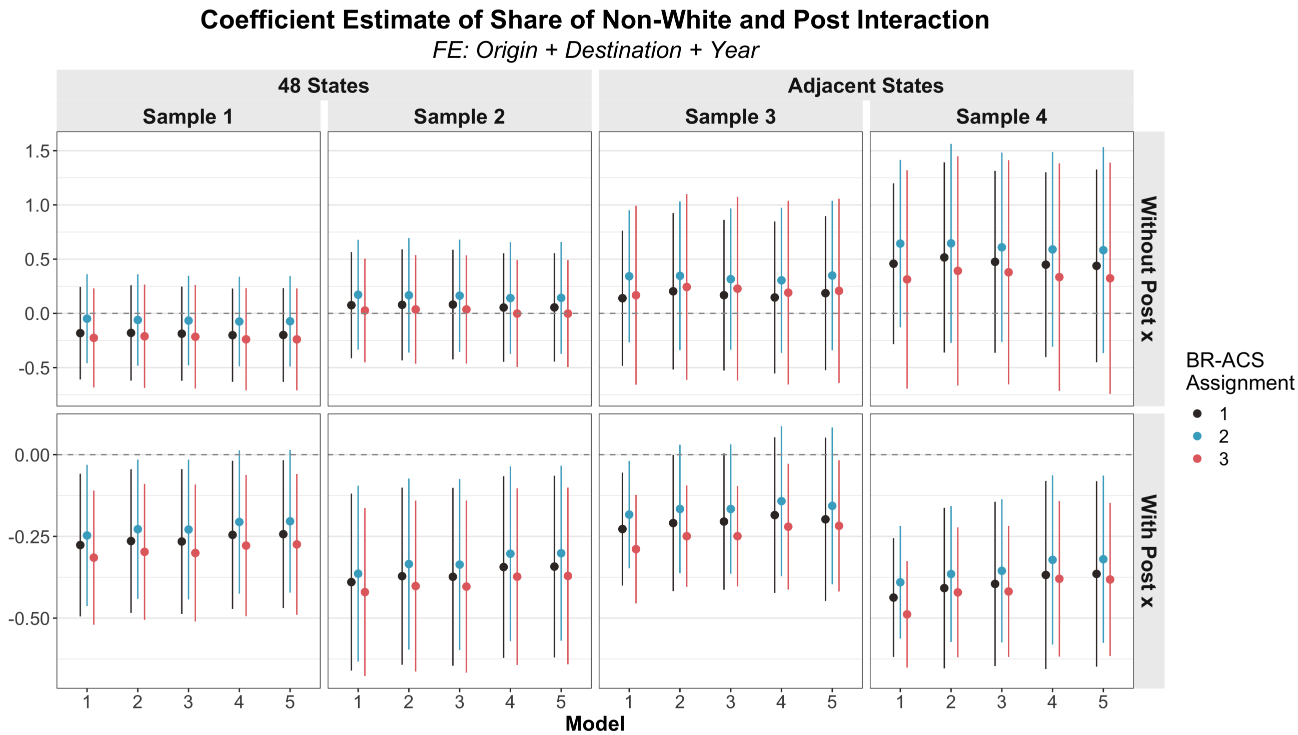
Table 8: Coefficient estimates and elasticities of share of non-white at destination

Dependent Variable: IHS(Quantity Flow)			
Model 5 Estimates	**BR-ACS Matching Option**		
	(1)	**(2)**	**(3)**
Coefficients			
Share of Non-white at Dest	0.438	0.583	0.324
	(0.453)	(0.485)	(0.544)
Post × ...	-0.365^{**}	-0.320^{**}	-0.382^{**}
	(0.145)	(0.131)	(0.120)
Elastcities (%)			
Share of Non-white at Dest	0.152	0.203	0.115
Post × ...	-0.127^{**}	-0.111^{**}	-0.136^{**}
Control Variables			
Total Quantities	YES	YES	YES
Destination Demographics	YES	YES	YES
Origin Demographics	YES	YES	YES
Tax	YES	YES	YES
Same State × Post	YES	YES	YES

*p<0.1; **p<0.05; ***p<0.01. Number of observations = 12,611.
Include origin, destination, and year fixed effects. Cluster SE at origin state level.
Elasticities are computed using pre-tax averages of all destinations.

Figure 22 shows 60 estimates of the coefficient on the share of non-white residents at destination (upper panel) and 60 estimates of the coefficient on post×share of non-white residents at destination (lower panel). Four vertical panels correspond to 4 samples in table 2, five dot clusters in each panel correspond to five regression models, and three colors correspond to three BR-ACS assignment options in figure 3. Results in table 7 correspond to the black dots in the right-most panel.

Figure 22: Estimates of the coefficient on share of non-white residents at destination and its post-interaction from alternative samples and BR-ACS assignment



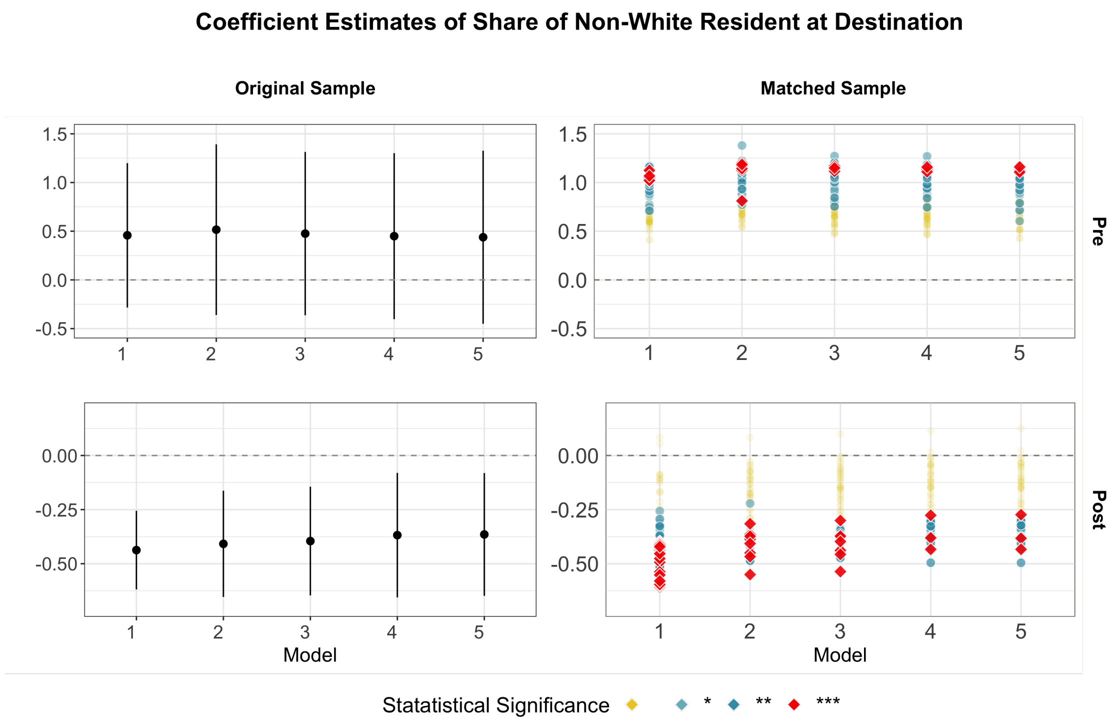
Estimates of pre-tax correlation between shipment quantity and the share of non-white residents at destination are not statistically significant in any case. Estimates of sample 1, which includes facilities in all 48 states and without post-tax shipping criterion, are all negative. Estimates of sample 2, which includes facilities in all 48 states that satisfy the post-tax shipping criterion, are positive, with the exceptions of estimates from models 4 and 5 using BR-ACS assignment option 3, which are negative but very small (-0.0016 and -0.0018 respectively). Estimates of samples 3 and 4, both of which only include facilities in Missouri and its adjacent states, are all positive.

As for the post-tax change in the correlation, all estimates are negative and the majority are statistically significant (54 out of 60). The only six estimates that are not statistically significant are from sample 3. These include estimates from models 4 and 5 that use BR-ACS assignment option 1 and 2, and an estimate from models 2 and 3 that uses BR-ACS assignment option 2. The magnitudes are larger for samples with post-tax shipping criteria (samples 2 and 4).

Matched Samples

I repeat the EJ analysis using observations in which origins are non-Missouri facilities that are in each of the matched samples. Figure 23 shows estimates of the coefficient on the share of non-white residents at the destination from 5 models that corresponds to 5 columns in the main result table. Top panels show estimates of the coefficient on the share of non-white residents and bottom panels show estimates of its interaction with the post-tax indicator. These are estimates from regression analyses that use BR-ACS assignment option 1. Figure 32 and 33 in the appendix shows estimates from BR-ACS assignmention option 2 and 3 respectively.

Figure 23: Estimates of the coefficient on share of non-white residents at destination and its post-interaction from original and matched samples



The left vertical panel displays estimates and standard errors from analyses that use the original sample. The right vertical panel displays estimates from 78 matched samples. Each dot represents an estimate from a matched sample. Yellow dots represent estimates that are not statistically significant. Light and medium blue dots represent estimates that are statistically significant at 10% and 5%, respectively. Red dots represent estimates that are statistically significant at 1%.

As discussed earlier original estimates of the pre-tax correlation are all positive, but none of them is statistically significant. The corresponding matched sample estimates are also all positive. Some of them are statistically significant with a larger magnitude, even in model 5, which includes a full set of control variables. These results suggest that the shipping pattern before the tax introduction from these origin facilities likely exhibits an environmental injustice pattern because shipment volume correlates with the share of non-white residents at the destination.

The original estimates of the change in the correlation are all negative and statistically significant. All estimates from matched samples are negative and a large share is statistically significant, many of which have a larger magnitude than the original estimates. These results suggest that there is an improvement in environmental justice of the shipping pattern after the tax introduction, confirming the main finding.

6 Conclusion

Shipment of hazardous waste allows for the distribution of the benefits of generation and the costs of disposal to be geographically disconnected. Negative externalities of hazardous waste are shipped away from a large number of origins to a much smaller set of destinations. The pattern of the shipment depends on the location of management facilities (TSDFs) and the shipping decisions of generators. In the past two decades, approximately 20% of HW generated is managed in another location, and over half of that amount crosses state borders. Moreover, over 95% of generators ship at least some waste off-site. Data show that the management and disposal of HW are becoming more geographically clustered in the United States. While the benefits of HW generation are geographically dispersed, the cost of management is localized. Given such an imbalance, individual states have taken regulatory measures to limit the burden of managing HW that was generated outside their borders.

In this study, I evaluate the extent to which Missouri's introduction of an out-of-state HW tax of $2 per ton affects the amount of HW shipments into Missouri from adjacent states. The difference-in-differences estimates suggest that the tax introduction decreased shipment quantity by 45% to 48%. Results from matched samples confirm this finding. In addition, I evaluate the environmental justice implication of the policy by estimating the change in the correlation between shipment quantity and the share of non-white residents. Point estimates from the main sample suggest an improvement in environmental justice with regards to the share of non-white residents: the positive correlation between shipment quantity and the share of non-white residents at destinations weakened after the tax introduction. In terms of statistical inference, even though the estimates of the change in the correlation are statistically significant, the estimates of the pre-tax correlations are not. Results from matched samples confirm the signs of main estimates. In addition, some of the matched estimates are statistically significant. This gives a reason to believe that the pre-tax shipment pattern may, in fact, be a case of environmental injustice.

The majority of the states do not impose an out-of-state HW tax currently. Even though there exists a concern among scholars that a "race to the top" in HW taxes may lead to an inefficient outcome, state governments operating under the limitation of the current governing system may have the incentive to consider such a policy tool to mitigate against uncompensated welfare loss from HW management. I hope that this study can provide a useful piece of information for policymakers who may consider this policy tool in the future.

There are many ways through which this study can be improved. First, with only two pre-event periods, it is difficult to evaluate how well the control group serves as a non-treated counterfactual of the treatment group. Data from earlier BR surveys exist (before 2001), but they are not readily accessible via an online platform[17]. I plan to request these data from the EPA and update these analyses in the future. Second, the share of observations with zero quantities is high in both analyses. Levinson encounters this issue in two of his papers (Levinson 1999b and Levinson 1999a) and addresses them by using the Tobit model. I plan to follow suit in the future. Lastly, a regression discontinuity analysis that makes use of Missouri's state border to identify the treatment effect may be a good alternative to the difference-in-differences design. I hope to experiment with this methodology in the future.

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[17] I found an EPA web page that provides links to these older datasets, though those links do not work: link to web

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Appendix

Difference in Difference Analysis

Main Analysis

Figure 24: Time trend of average quantity shipped to Missouri by treatment and control groups

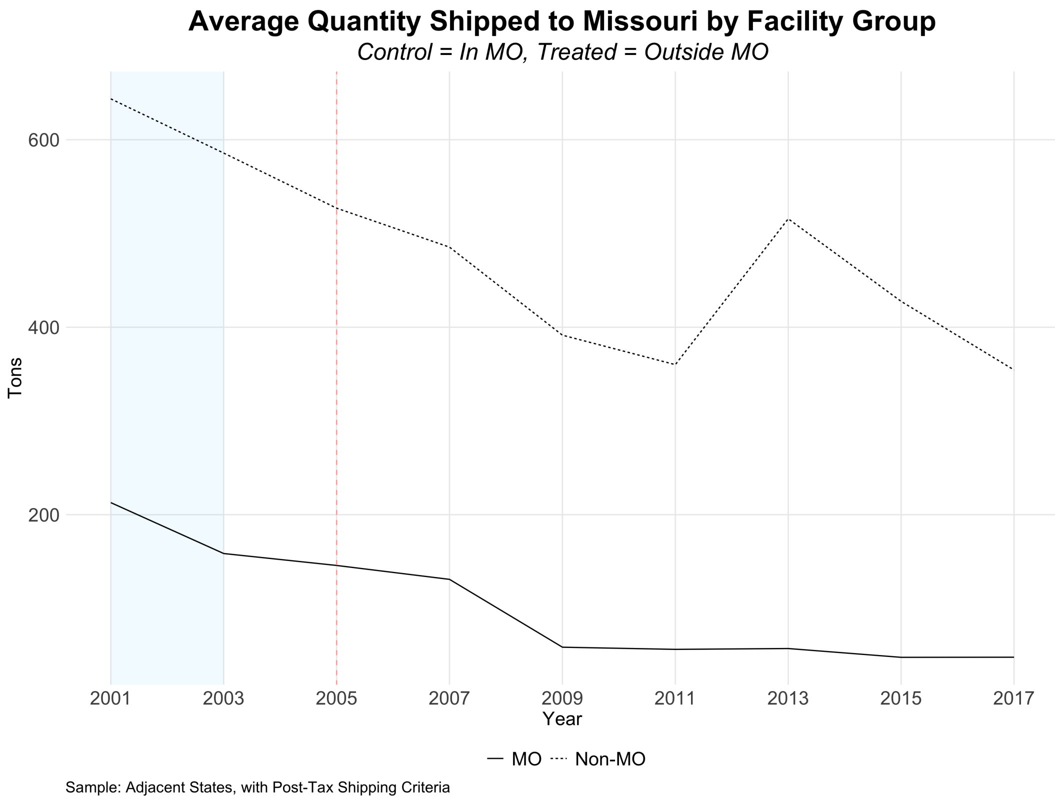


Figure 25: Kernel density of quantity shipped to Missouri by treatment and control groups

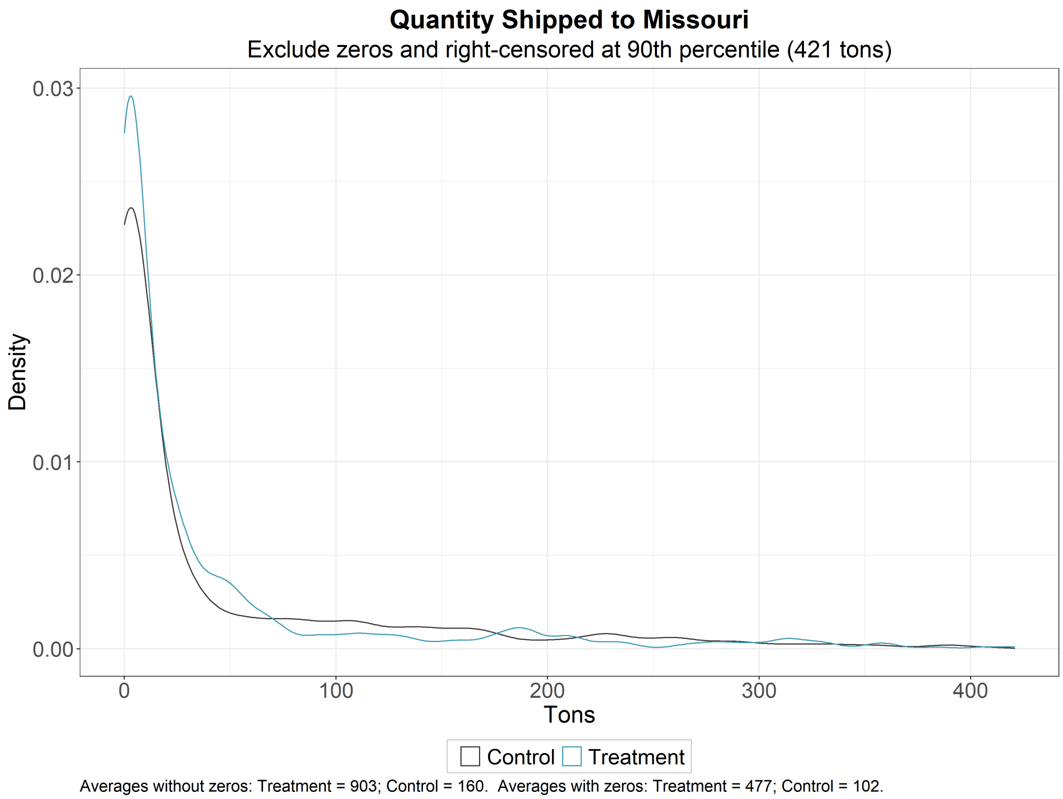
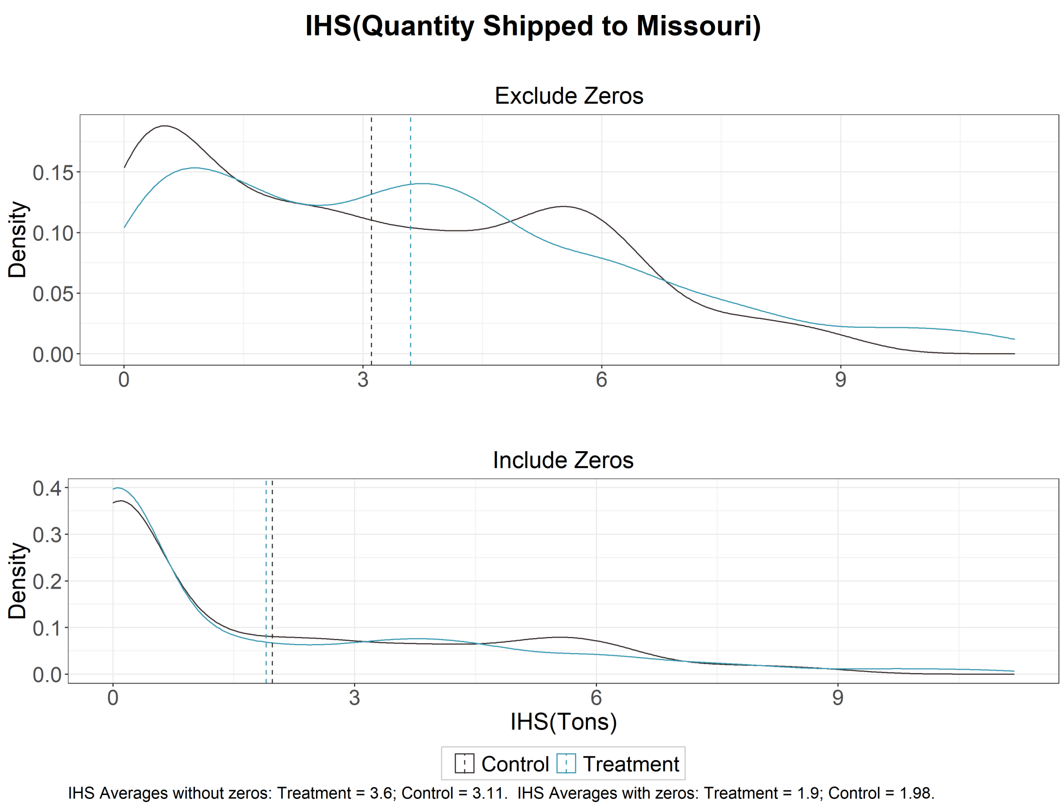


Figure 26: Kernel density of IHS(quantity shipped to Missouri) by treatment and control groups



Matched Sample

Figure 27: Difference in group-specific pre-tax averages of quantity shipped to Missouri

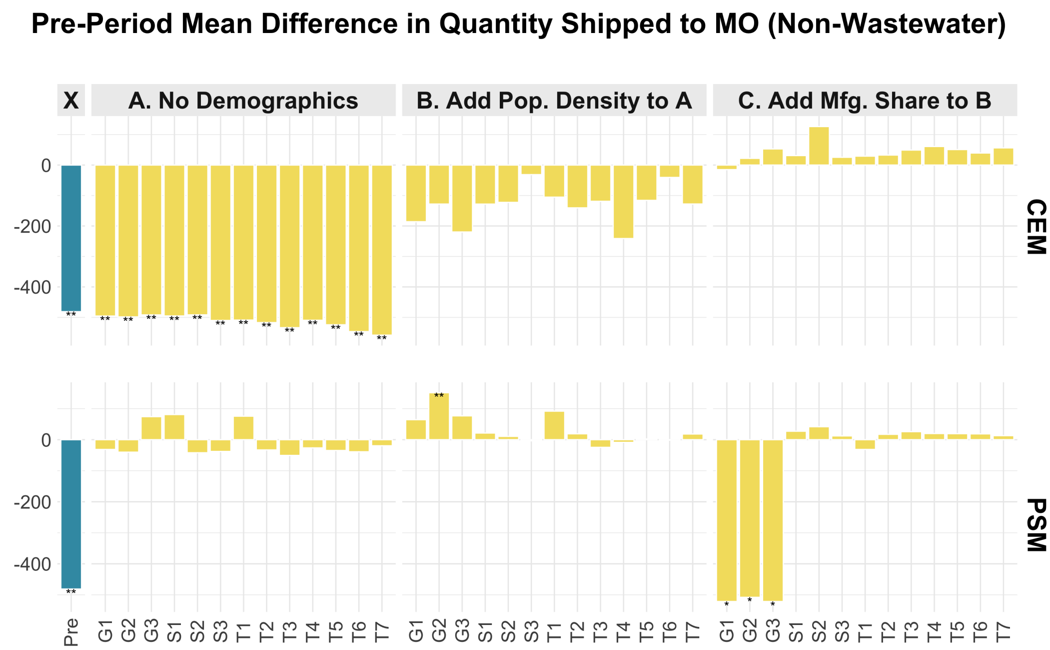


Figure 28: Difference in group-specific pre-tax averages of total shipping quantity

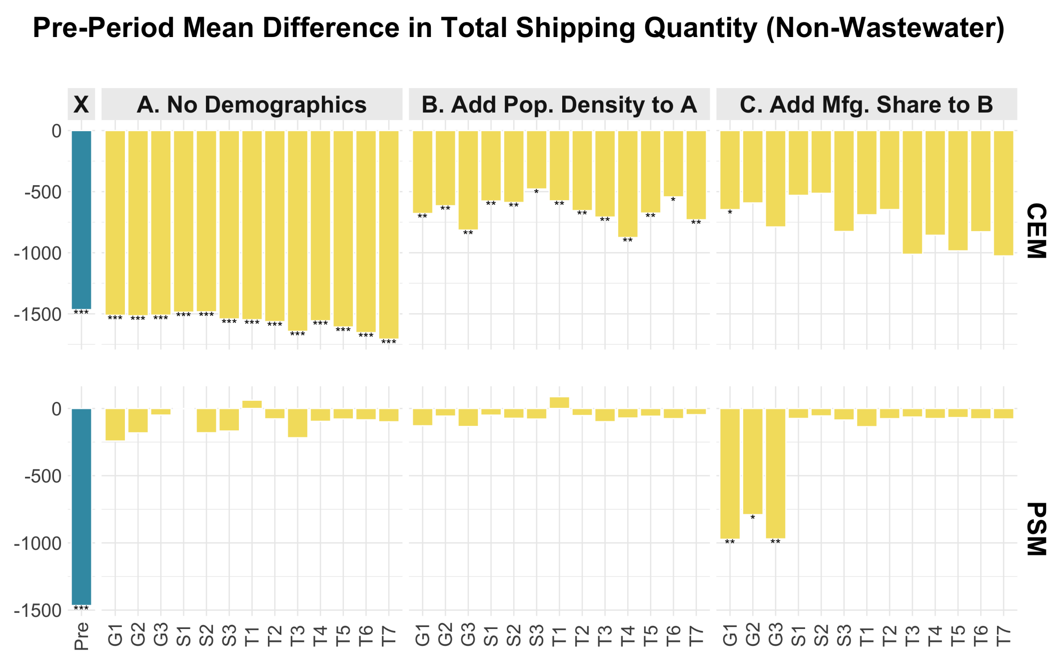
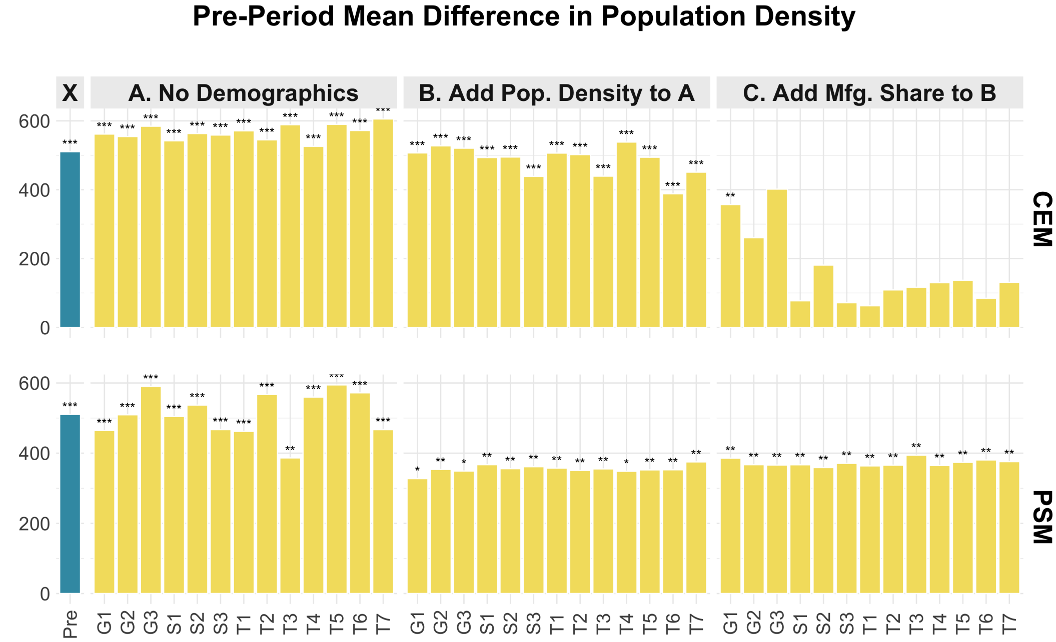


Figure 29: Difference in group-specific pre-tax averages of quantity shipped to Missouri



Environmental Justice Analysis

Summary Statistics Tables

Table 9: Pre-tax vs. post-tax averages of all origin-destination pairs

All Origin-Destination Pairs				
Variable	**Pre-Tax**	**Post-Tax**	**Difference**	**t-statistic**
(A) Destination Demographics				
Share of Non-white	34.74%	35.70%	0.96%	-1.38
Median Income ($1k)	44.18	45.07	0.89	-2.19^{**}
Share of Mfg. Employment	14.31%	13.81%	-0.50%	3.32^{***}
Share of Over HS Education	43.90%	45.45%	1.55%	-5.83^{***}
Share of Over 65-Year Old	13.31%	14.27%	0.97%	-6.45^{***}
Median Home Value ($1k)	113.77	111.74	-2.03	1.41
Population Density (per sq.mile)	806	881	75	-3.11^{***}
(B) Destination-Origin Difference in Demographics				
Share of Non-white	10.27%	10.09%	-0.18%	0.20
Median Income ($1k)	-1.82	-5.32	-3.50	6.14^{***}
Share of Mfg. Employment	-1.35%	-1.20%	0.15%	-0.66
Share of Over HS Education	-1.74%	-4.39%	-2.64%	6.86^{***}
Share of Over 65-Year Old	0.39%	-0.28%	-0.67%	3.50^{***}
Median Home Value ($1k)	0.17	-7.46	-7.63	3.99^{***}
Population Density (per sq.mile)	-108	-4	104	-2.59^{***}
(C) Quantities, Tax, and Proximity				
Quantity Flow (Tons)	108	173	66	-1.91^{*}
Total Quantity Received by Destination (Tons)	36,726	36,460	-266	0.33
Total Quantity Shipped by Origin (Tons)	8,512	7,913	-599	1.40
Same State Dummy	0.11	0.11	-0.01	1.21
Distance (100 miles)	4.51	4.55	0.05	-0.71
Disposal Tax ($/Ton)	19.05	19.73	0.68	-1.83^{*}
Number of Observations	2,789	9,822	7,033	
Number of Origin-Destination Pairs	1,413	1,590	177	
Number of Origin Facilities	153	153	0	
Number of Destination Facilities	186	273	87	

Table 10: Pre-tax vs. post-tax Averages of origin-destination pairs with quantity > 0

Origin-Destination Pairs with Quanaity > 0				
Variable	**Pre-Tax**	**Post-Tax**	**Difference**	**t-statistic**
(A) Destination Demographics				
Share of Non-white	35.29%	34.48%	-0.81%	0.65
Median Income ($1k)	44.75	44.90	0.15	−0.20
Share of Mfg. Employment	14.05%	13.91%	-0.14%	0.49
Share of Over HS Education	44.85%	45.63%	0.78%	−1.66*
Share of Over 65-Year Old	12.59%	14.51%	1.92%	−7.24***
Median Home Value ($1k)	117.86	109.79	-8.07	3.20***
Population Density (per sq.mile)	834	818	-15	0.36
(B) Destination-Origin Difference in Demographics				
Share of Non-white	10.22%	7.16%	-3.06%	1.89*
Median Income ($1k)	-1.67	-4.97	-3.30	3.23***
Share of Mfg. Employment	-1.69%	-0.80%	0.88%	−2.21**
Share of Over HS Education	-1.01%	-3.73%	-2.72%	4.17***
Share of Over 65-Year Old	-0.25%	-0.22%	0.03%	−0.10
Median Home Value ($1k)	4.73	-8.14	-12.87	3.92***
Population Density (per sq.mile)	-15	-80	-64	0.92
(C) Quantities, Tax, and Proximity				
Quantity Flow (Tons)	333	504	171	−1.67*
Total Quantity Received by Destination (Tons)	38,787	40,637	1,849	−1.29
Total Quantity Shipped by Origin (Tons)	10,527	9,971	-556	0.68
Same State Dummy	20.49	18.45	-2.04	3.15***
Distance (100 miles)	0.17	0.14	-0.02	1.52
Disposal Tax ($/Ton)	4.29	4.10	-0.19	1.67*
Number of Observations	902	3,376	2,474	
Number of Origin-Destination Pairs	671	1,286	615	
Number of Origin Facilities	146	151	5	
Number of Destination Facilities	140	262	122	

Table 11: Pre-tax vs. post-tax Averages of origin-destination pairs with quantity = 0

	Origin-Destination Pairs with Quanaity = 0			
Variable	**Pre-Tax**	**Post-Tax**	**Difference**	**t-statistic**
(A) Destination Demographics				
Share of Non-white	34.48%	36.34%	1.87%	-2.20^{**}
Median Income ($1k)	43.91	45.16	1.25	-2.56^{**}
Share of Mfg. Employment	14.44%	13.75%	-0.68%	3.78^{***}
Share of Over HS Education	43.45%	45.35%	1.91%	-5.94^{***}
Share of Over 65-Year Old	13.65%	14.15%	0.50%	-2.76^{***}
Median Home Value ($1k)	111.82	112.77	0.95	-0.54
Population Density (per sq.mile)	793	914	121	-4.15^{***}
(B) Destination-Origin Difference in Demographics				
Share of Non-white	10.30%	11.63%	1.33%	-1.16
Median Income ($1k)	-1.88	-5.50	-3.61	5.25^{***}
Share of Mfg. Employment	-1.19%	-1.41%	-0.22%	0.78
Share of Over HS Education	-2.09%	-4.73%	-2.64%	5.53^{***}
Share of Over 65-Year Old	0.69%	-0.32%	-1.01%	4.30^{***}
Median Home Value ($1k)	-2.01	-7.11	-5.10	2.17^{**}
Population Density (per sq.mile)	-152	36	188	-3.83^{***}
(C) Quantities, Tax, and Proximity				
Quantity Flow (Tons)	0	0		
Total Quantity Received by Destination (Tons)	35,740	34,272	-1,468	1.51
Total Quantity Shipped by Origin (Tons)	7,549	6,835	-713	1.45
Same State Dummy	18.37	20.41	2.04	-4.49^{***}
Distance (100 miles)	9.01%	8.63%	-0.38%	0.51
Disposal Tax ($/Ton)	4.61	4.79	0.18	-2.29^{**}
Number of Observations	1,887	6,446	4,559	
Number of Origin-Destination Pairs	1,154	1,385	231	
Number of Origin Facilities	150	151	1	
Number of Destination Facilities	168	187	19	

Regression Results

Table 12: Coefficient estimates from the model equation (3)

(all variables)

Variable	Dependent Variable: IHS(Quantity Flow in Tons)				
	Model				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
DOM Variables					
Share of Non-white at Dest	0.457	0.515	0.475	0.448	0.450
	(0.379)	(0.448)	(0.426)	(0.433)	(0.451)
Post × Share of Non-white at Dest	−0.437***	−0.408**	−0.397**	−0.369**	−0.369**
	(0.093)	(0.125)	(0.128)	(0.147)	(0.146)
Median Income ($1k) at Dest	0.004	0.003	0.003	0.002	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Post × Median Income ($1k) at Dest	−0.007*	−0.007**	−0.007**	−0.006*	−0.006
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Control Variables					
IHS(Total Quantity Received by Dest)	0.217***	0.219***	0.219***	0.213***	0.213***
	(0.019)	(0.019)	(0.019)	(0.018)	(0.018)
IHS(Total Quantity Shipped by Orig)	0.116***	0.116***	0.116***	0.115***	0.115***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Share of Mfg. Employment at Dest		−0.593	−0.575	−0.589	−0.588
		(0.8)	(0.762)	(0.754)	(0.753)
Share of Over HS Education at Dest		−0.057	−0.072	−0.14	−0.139
		(0.593)	(0.602)	(0.619)	(0.632)
Median Home Value ($1k) at Dest		0.001	0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Population Density (1k/mile^2) at Dest		0.076	0.074	0.103	0.103
		(0.082)	(0.089)	(0.089)	(0.083)
Share of Over 65-Year Old at Dest		0.604	0.603	0.627	0.629
		(0.524)	(0.515)	(0.515)	(0.548)
Share of Non-white at Orig			−0.645**	−0.633*	−0.633*
			(0.268)	(0.273)	(0.269)
Median Income ($1k) at Orig			−0.002	−0.002	−0.002
			(0.004)	(0.004)	(0.004)
Origin Demographics	NO	NO	YES	YES	YES
Tax Variables	NO	NO	NO	YES	YES
Post × Same-State	NO	NO	NO	NO	YES
Observations	12,611	12,611	12,611	12,611	12,611

*p<0.1; **p<0.05; ***p<0.01.
Include origin-destination fixed effects and year fixed effects; cluster SE at origin state level.

Figures

Figure 30: Kernel density of shipment quantity in the pre-tax and post-tax periods, excluding zero.

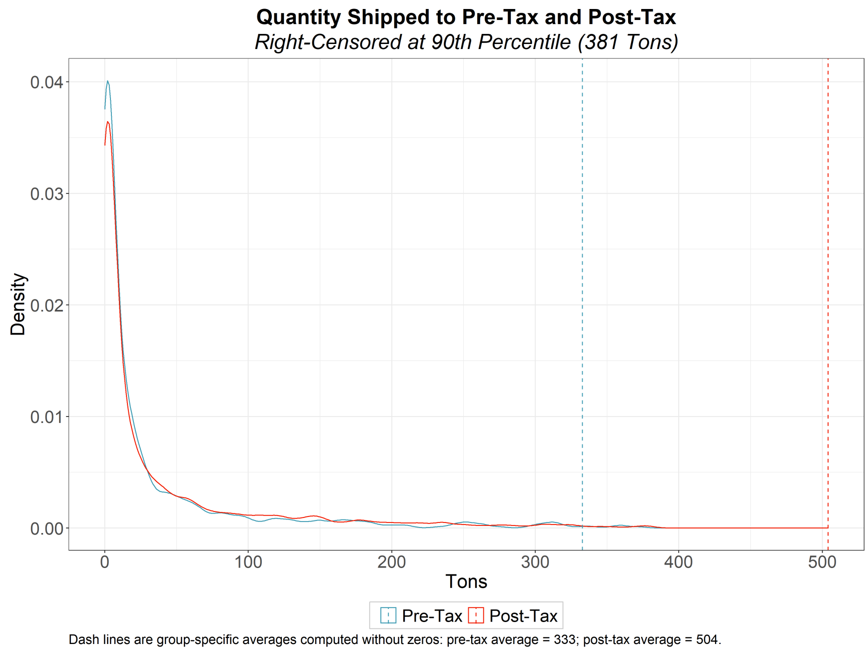


Figure 31: Kernel density of share of non-white residents at destination

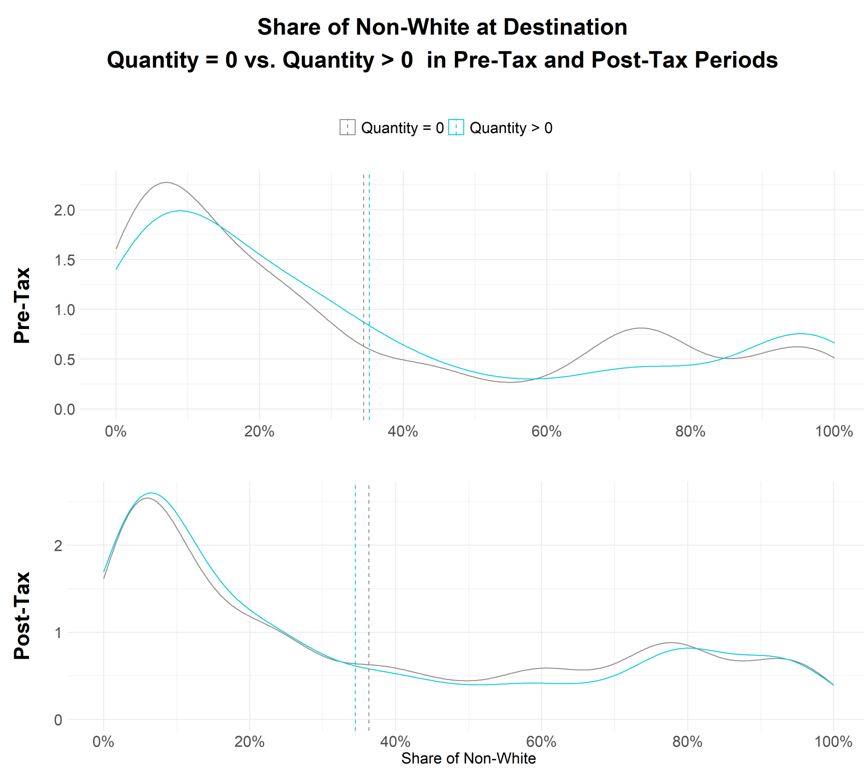


Figure 32: Environmental justice results from matched sample: BR-ACS assignment option 2

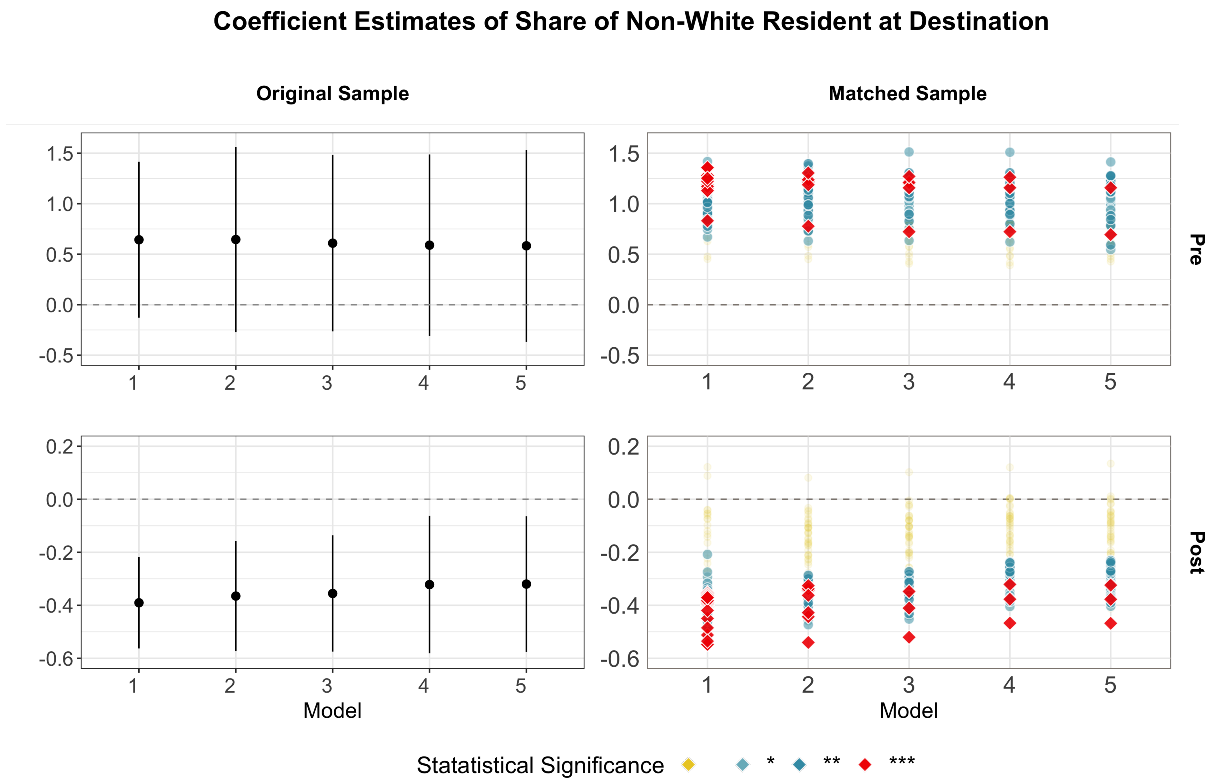


Figure 33: Environmental justice results from matched sample: BR-ACS assignment option 3