



# Oil and Water: Essays on the Economics of Natural Resource Usage

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# **Oil and Water: Essays on the Economics of Natural Resource Usage**

A dissertation presented

by

**Samuel Stolper**

to

The Department of Public Policy

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

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Cambridge, Massachusetts

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## **Oil and Water: Essays on the Economics of Natural Resource Usage**

### **Abstract**

As the developing world continues its pace of rapid growth and the threat of climate change intensifies, the economics of natural resource usage become increasingly important. From the perspective of both economic efficiency and distributional equity, effective policy design is correspondingly urgent. Market failures such as imperfect competition, externalities, and incomplete information plague resource markets everywhere; and both initial endowments and policy interventions often have regressive incidence. I shed light on some of these issues by studying the economics of natural resource usage in two separate empirical contexts. The first is the market for automotive fuel in Spain; I measure pass-through – the degree to which retail fuel stations "pass through" diesel taxes to final consumer prices – and use it to assess the distributional impacts of energy policy. The second is the Ganga River Basin of India; I estimate the impacts of environmental regulation on river water quality and infant mortality. In both contexts, I utilize estimates of policy impacts to examine the underlying mechanisms by which affected consumers and suppliers of natural resources make decisions.

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In honor of my great-grandfather, Israel Stolper.

# Introduction

The field of economics holds great power to explain humans' interaction with natural resources. For millennia, such resources have intimately affected health and quality of life, welfare and the ability to grow, and the pursuit of democratic and equitable society – and they will continue to do so. In the age of climate change, countries face the imperative of changing how they use natural resources (like fossil fuels) before major, irreversible changes are made to the Earth's climate system. Sound economics and policy design have the potential to mitigate climate change and make natural resource usage more sustainable.

The outcomes of natural resource policy, as well as the mechanisms by which it affects those outcomes, are the focus of my doctoral dissertation. In the following three chapters, I study two different settings in which governments have used policy to improve natural resource usage. The first setting is the Spanish market for retail automotive fuel. Chapters 1 and 2 focus on this market, in which rising taxes have been used as a means to reduce the social costs of automotive fuel consumption and raise government revenue. The second setting is the Indian tanning industry, and more generally the river systems whose water it pollutes. Chapter 3 focuses on a landmark environmental regulation regulating this industry, and the water quality and public health outcomes that it sought to improve.

The point of Chapter 1 is to show how the economic incidence of energy taxes is intimately affected by imperfect competition, consumer wealth, and the shape of the energy demand curve. Existing estimates of energy tax incidence tend to assume that the pass-through of taxes to final consumer prices is uniform across the affected population. I show that, in fact, variation in local market conditions drives significant heterogeneity in

pass-through, and ignoring this can lead to mistaken conclusions about the distributional impacts of energy taxes. I use data from the Spanish retail automotive fuel market to estimate station-specific pass-through, focusing on the effects of competition and wealth. A novel informational mandate provides access to a national, station-daily panel of retail diesel prices and characteristics and allows me to investigate market composition at a fine level.

Event study and difference-in-differences regression reveal that, while retail prices rise nearly one-for-one (100%) with taxes on average, station-specific pass-through rates range from at least 70% to 120%. Greater market power – measured by brand concentration and spatial isolation – is strongly associated with higher pass-through, even after conditioning on detailed demand-side characteristics. Furthermore, pass-through rises monotonically in area-average house prices. While a conventional estimate of the Spanish diesel tax burden suggests roughly equivalent incidence across the wealth distribution, overlaying the effect of heterogeneous pass-through reveals the tax to be unambiguously progressive.

In Chapter 2, I retain my focus on pass-through of Spanish diesel taxes, but I look specifically at state borders for insight into the effect of competition on incidence. I begin with the observation that, in a long empirical literature on pass-through, most estimates capture the response of firms to an industry-wide cost shock. By leveraging administrative borders, I am able to estimate the pass-through of cost changes that specifically affect only part of a market. In Spain's automotive fuel market, retail taxes have a state-specific component. Thus, when a state raises its tax, gas stations facing competition from cross-border rivals are subjected to an own-cost shock, while cross-border rivals are subjected to a rival-cost shock.

Using event study and difference-in-differences, I show that own-tax pass-through is significantly reduced at state borders, while rival-tax pass-through is significantly greater than zero. The magnitudes of both of these changes rise in the number and proximity of cross-border rivals. These 'firm-specific' pass-through estimates are policy-relevant because they facilitate calibration of the full pass-through matrix – i.e., the response of each firm to each other firm – as a function of proximity to rivals. The estimates also document a strong

incentive to raise rivals' costs, by showing firms raising prices in response to others' cost shocks.

In Chapter 3, I switch gears completely, to an investigation of water pollution in the developing-country context of India. India's rivers are heavily polluted, and one of the most polluted sites is the city of Kanpur, situated along the banks of the Ganga River. The Ganga receives large amounts of toxic waste from the city's large and highly-concentrated tannery industry. I study the impact of a landmark piece of judicially-mandated environmental regulation in Kanpur: in September 1987, the Supreme Court of India ordered the city's tanneries to either clean their waste or shut down.

I explore the pollution and health effects of this ruling in Kanpur district, and find that it both improved surface water quality (as measured by Biochemical Oxygen Demand) and saved infant lives (as measured by neonatal mortality). I then explore the mechanisms of the policy's mortality impact using instrumental variables. My statistical test of mechanisms fails to reject the null hypothesis that pollution fully explains the policy's mortality impact but leaves open the possibility of an income channel reducing the net health benefits of the policy. In addition to providing evidence on mechanisms, the results establish a causal link between river pollution and infant mortality as well as a spillover of pollution-induced health costs well downstream of initial pollution measurement.

# Chapter 1

## Who Bears the Burden of Energy Taxes? The Critical Role of Pass-Through

### 1.1 Introduction

Energy taxes – and related market-based policies – are attractive because they have the potential to reduce negative externalities like pollution, traffic, and accident risk in a cost-effective manner, thereby raising social welfare. But what are the distributional impacts of these policies? Researchers (Morris and Munnings 2013), politicians (Metcalf, Mathur, and Hassett 2011), and popular media (New York Times 2009) alike have long debated the economic incidence of energy taxes - for example, how much of the tax burden is borne by consumers versus suppliers, and how taxes affect households of different wealth levels. Distributional outcomes are increasingly subject to scrutiny as the demand for climate policy grows, and as the scope and scale of household energy use continue to increase.

In this paper, I provide new insight into distributional questions about energy policy by estimating the pass-through of automotive fuel taxes to final, retail prices. Pass-through – the degree to which costs physically imposed on one segment of a market are “passed

through” to others – is a useful economic tool for at least two reasons. First, it is determined in equilibrium by supply, demand, and competition; thus, empirical pass-through patterns provide indirect insight into underlying market function. Second, pass-through measures the extra cost of maintaining consumption in the face of a tax hike, thereby providing *direct* insight into tax incidence. I make use of these attributes by studying how energy tax pass-through rates vary with local competition and consumer characteristics.

My focus is on the retail automotive fuel market of Spain, whose government provides access to daily gas-station prices and characteristics through a novel informational mandate issued in 2007. State-specific taxes on automotive fuel provide panel variation in tax levels. Cross-sectional variation in branding and location, as well as temporal variation in local competition generated by entry and exit of stations, allows me to estimate a relationship between tax pass-through and market power. Survey measures of population, property values, and education aid in the identification of that relationship and also facilitate a study of the relationship between pass-through and wealth.

I find that branding and location patterns in the Spanish market predict significant heterogeneity in pass-through. Moreover, pass-through exhibits a strong positive correlation with wealth, as measured by local house prices. These results challenge the wisdom of existing energy tax incidence analyses (e.g., West 2004; Bento et al. 2009; Grainger and Kolstad 2010), which consistently find that taxes on gasoline and carbon dioxide are regressive – i.e., relatively worse for poorer people than for richer ones – in industrialized countries. These analyses focus primarily on how differences in consumption (both before and after a tax change) across the wealth spectrum affect distributional equity, but they assume away corresponding differences in prices. My own analysis suggests that the price impacts of taxation (measured by pass-through) are not only non-uniform, but also systematically related to wealth. When I account for this in my own incidence calculation, the Spanish tax appears strongly progressive.

My empirical analysis is essentially a comparison of prices before and after tax hikes, at stations of different types. I begin with an event study of tax hikes, which provides a sense



of price trends at stations experiencing tax hikes relative to those not experiencing them. The results imply that “treatment” and “control” stations have parallel price trends before and after tax events. Motivated by this finding, I use difference-in-differences (DiD) regression to estimate an average pass-through rate of 95% for Spanish diesel taxes (diesel is the dominant automotive fuel in Spain). However, this average rate masks significant heterogeneity at the local level. I capture this heterogeneity by comparing prices before and after taxes among stations with (a) different brands, (b) facing different numbers and types of rivals, and (c) serving different consumer bases. Econometrically, I do this by re-estimating event study and DiD models while interacting my tax variable with characteristics of stations and their surroundings.

The results show that stations bearing the brand of a vertically-integrated refiner are associated with significantly higher tax pass-through, as are stations facing relatively less-dense spatial competition. In addition, pass-through rises in the local concentration of one’s own brand. While brand and location are likely endogenous due to station owners’ consideration of local demand in their decisions, the inclusion of a suite of detailed demand-side characteristics in regression analysis leaves my main estimates unchanged. Through both a branding channel and a spatial channel, market power appears to raise pass-through.

I also find that pass-through rises monotonically in area-average house prices. I cannot interpret this relationship as causal, but it is nonetheless the case that richer areas see, on average, higher price impacts of taxation, even conditional on local market structure. Together, competitive environment and local consumer characteristics predict a wide distribution of pass-through rates among Spanish gas stations, centered around 90% but ranging from approximately 70 to 115%. The existence of overfull (>100%) pass-through may seem surprising, but it has been found in other markets (Besley and Rosen 1999) and is the natural result of imperfect competition with sufficiently convex demand (Seade 1985). In Spain, 24% of gas stations have estimated pass-through rates in excess of 100% on the last day of observation in my sample period.

The combination of imperfect competition and convex demand has significant impli-

cations for tax incidence. *Perfect* competition, which is a standard assumption in energy tax incidence analysis, bounds pass-through between 0 and 100%; since empirical research shows that fuel tax pass-through is nearly 100% on average (see, e.g., Marion and Muehlegger 2011), the perfect-competition assumption implies 100% pass-through everywhere. This uniform, full pass-through rate is what is applied in nearly every incidence analysis to date. My results, in contrast, show that pass-through varies substantially across space.

Pass-through variation intimately affects distributional equity, by imposing larger price impacts on richer areas. Existing incidence analyses miss this effect by using a uniform pass-through rate. To show the consequences of this omission, I estimate the effect of a marginal tax hike on household tax burdens, before and after accounting for the relationship between pass-through and wealth. I obtain annual automotive fuel consumption totals for a sample of households from the Spanish Household Budget Survey. This quantity, multiplied by the pass-through rate, and divided by total household expenditure, gives an estimate of the marginal tax burden as a proportion of wealth. Existing analyses of this type assume a pass-through rate of 100%; replicating this assumption yields burden estimates that are roughly equivalent across wealth deciles. In contrast, using estimated pass-through rates specific to each house-price decile yields burden estimates that rise with wealth.

The conventional wisdom is that gasoline and diesel taxes are regressive in industrialized countries because the poor in those countries tend to spend a larger proportion of their wealth on energy than the rich. This presents a serious, oft-cited flaw in a policy instrument that is generally seen as good for overall social welfare. But it relies in part on an assumption of uniform pass-through that I here prove inaccurate. All else equal, a positive relationship between pass-through and wealth makes taxes more progressive. In Spain, it turns a tax with roughly flat incidence across the wealth distribution into a progressive policy. To the extent that the positive relationship between pass-through and wealth holds in other contexts, taxes on those energy products and in those locales become correspondingly more attractive from a distributional standpoint. More generally, the widespread heterogeneity that I identify due to variation in local competition and preferences suggests that analysts

should not assume away cross-sectional differences in the price impacts of energy regulation. Reduced-form pass-through estimation provides a tractable way of addressing this problem.

The rest of this paper is laid out as follows: Section 1.2 describes what is known, in theory and in empirics, about energy tax incidence; Section 1.3 provides a picture of the Spanish automotive fuel market and the relevant taxes and data; Section 1.4 describes my analysis of average tax pass-through; Section 1.5 details the corresponding estimation of local tax pass-through as a function of market structure and consumer makeup; Section 2.5 discusses the distributional implications of these results; and Section 3.6 concludes.

## **1.2 Pass-Through in the Existing Literature**

The term “pass-through” refers to what Alfred Marshall (1890) described as “the diffusion throughout the community of economic changes which primarily affect some particular branch of production or consumption.” Most commonly, these “economic changes” are costs, physically imposed on one part of a supply chain, and passed through to others. As Weyl and Fabinger (2013) have recently highlighted, pass-through has extraordinary potential as a tool of economic analysis. For this reason, several disciplines of economics feature the topic in research. International economists have long been concerned with exchange-rate pass-through, because of its role in explaining movements in relative prices and business cycles (Auer and Schoenle 2013). The field of industrial organization contains much research on pass-through because of the light it sheds on mergers (Jaffe and Weyl 2013) and price discrimination (Aguirre, Cowan, and Vickers 2010). In public finance, pass-through is important primarily because of its connection to tax incidence. This last application is the one on which I focus.

### **1.2.1 The use of pass-through in incidence analysis**

The change in consumer surplus elicited by a rise in energy taxes is naturally divided into two components: (a) the additional cost of energy consumption maintained in the face of

rising prices; and (b) the utility lost from reduced consumption (i.e., the *deadweight* loss).<sup>1</sup> Pass-through physically measures the former, per unit consumption. It is thus an integral part of incidence analysis, which generally focuses on estimating changes in surplus among different segments of society (e.g., consumers vs. producers, and richer vs. poorer). If the price impacts of rising taxes vary across geographic regions, firms, or individuals, then distributional welfare will vary accordingly.

Existing analyses of energy tax incidence, however, assume without exception that pass-through of taxes – whether on gasoline (West 2003; West and Williams 2004; Bento et al. 2005, 2009) or on carbon (Metcalf 2009; Grainger and Kolstad 2010; Metcalf, Mathur, and Hasset 2011; Mathur and Morris 2012) – is uniform across the affected population. With one exception (Metcalf, Mathur, and Hasset 2011), these analyses further assume that pass-through is fully 100% (one for one).

Why is pass-through assumed or expected to be uniformly 100%? The answer is a combination of theory, intuition, and empirics. The natural starting point in public finance is of tax incidence in perfect competition. In such a model, pass-through is entirely a function of the elasticities of supply and demand. Equation 1.2.1 provides the mathematical definition (see Appendix A.1 for the derivation):

$$\frac{dp_c}{dt} = \frac{\epsilon_S}{\epsilon_S - \epsilon_D} = \frac{1}{1 - \frac{\epsilon_D}{\epsilon_S}} \quad (1.1)$$

Pass-through of tax  $t$  to retail price  $p_c$  rises in the supply elasticity ( $\epsilon_S$ ) and falls in the absolute demand elasticity ( $\epsilon_D$ ). In the polar cases of either perfectly elastic supply ( $\epsilon_S \rightarrow +\infty$ ) or perfectly *inelastic* demand ( $\epsilon_D \rightarrow 0$ ), pass-through rates are identically 100%.

The consensus intuition about automotive fuel markets is that retail supply *is* very elastic – because of opportunities for storage and the ease of purchasing wholesale fuel for resale – and that retail demand *is* very inelastic – because driving is a fundamental input to so

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<sup>1</sup>Consumer surplus is also determined by (a) ownership of supply-side capital; (b) externalities like pollution, traffic, and vehicular safety; (c) other goods' prices that are affected by energy taxes in general equilibrium; and (d) the use of government revenues obtained through taxation. In this paper, however, I focus only on the utility derived directly from the purchase and consumption of energy. See Sterner (2012) for a fuller discussion of the various channels through which a tax affects welfare.

many daily activities. Empirical research suggests that at least the latter is true (Dahl 2012; Hughes, Knittel, and Sperling 2008). In perfect competition, the expected result is thus high (i.e., close to 100%) pass-through.

The empirical pass-through literature strongly supports the above intuition: estimated average pass-through rates in automotive fuel markets are consistently 100% or very nearly so (Alm, Sennoga, and Skidmore 2009; Marion and Muehlegger 2011; Bello and Contín-Pilart 2012). High pass-through has also been found for the cost of permits under the European Union Emissions Trading System (Fabra and Reguant 2014) and credits (“RINs”) under the U.S. Renewable Fuel Standard (Knittel, Meiselman, and Stock 2015). Pass-through is bounded above by 100% in perfect competition, as can be seen from Equation 1.2.1; in such a model, full pass-through *on average* therefore implies full pass-through everywhere. This, perhaps, is why researchers assume the latter in incidence analysis.

## 1.2.2 The logic of heterogeneous pass-through

Once the assumption of perfect competition is set aside, full pass-through on average no longer guarantees full pass-through locally. In *imperfect* competition, pass-through varies with not just the first derivative (elasticity), but also the second (convexity). Consider the formula for pass-through in monopoly with constant marginal costs  $c$ :

$$\frac{dp_m}{dt} = \frac{\frac{\partial p(q_m)}{\partial q_m}}{2 \frac{\partial p(q_m)}{\partial q_m} + q_m \frac{\partial^2 p(q_m)}{\partial q_m^2}} \quad (1.2)$$

The shape of demand – described by  $\frac{\partial p(q_m)}{\partial q_m}$  and  $\frac{\partial^2 p(q_m)}{\partial q_m^2}$  – is integral to the magnitude of monopoly pass-through. In oligopoly, the same holds true: tax pass-through depends on first and second derivatives of demand with respect to both one’s own prices *and* the prices of its competitors (see Equations A.3 and A.4 for derivations of pass-through in imperfect competition).

Anything that affects the shape of demand causes a change in the level of pass-through. For example, greater market power at some gas station  $i$ , due to either larger market

shares or greater spatial isolation, could reduce the magnitude of  $\frac{\partial q(p_i)}{\partial p_i}$ ; this would, in turn, lead to a different pass-through rate than at other stations. Along these lines, Doyle and Samphantharak (2008) find that pass-through of U.S. sales taxes into retail gasoline prices is lowest in areas with the lowest brand concentration.<sup>2</sup> At the same time, consumer preferences or budget constraints could also affect the shape of demand. Though the direct relationship between pass-through and wealth is undocumented, it is nonetheless clear that demand could be more or less elastic in richer areas, relative to poorer ones. Such variation would, in turn, drive differences in pass-through.<sup>3</sup>

One important observation from Equation 1.2 is that the sign of the relationship between pass-through and (absolute) demand elasticity is theoretically ambiguous. If demand is linear, the second derivative of demand is zero, and monopoly pass-through collapses to 50% regardless of the slope of demand. If demand is concave, then pass-through is below 50%, and more inelastic demand leads to lower pass-through, all else equal. If demand is convex, then pass-through is above 50%, and more inelastic demand leads to *higher* pass-through, all else equal. With prior knowledge of the second derivative of demand, this ambiguity is resolved. Without it, the relationship between market power and pass-through, or wealth and pass-through, becomes an empirical question.

### 1.2.3 Overfull pass-through

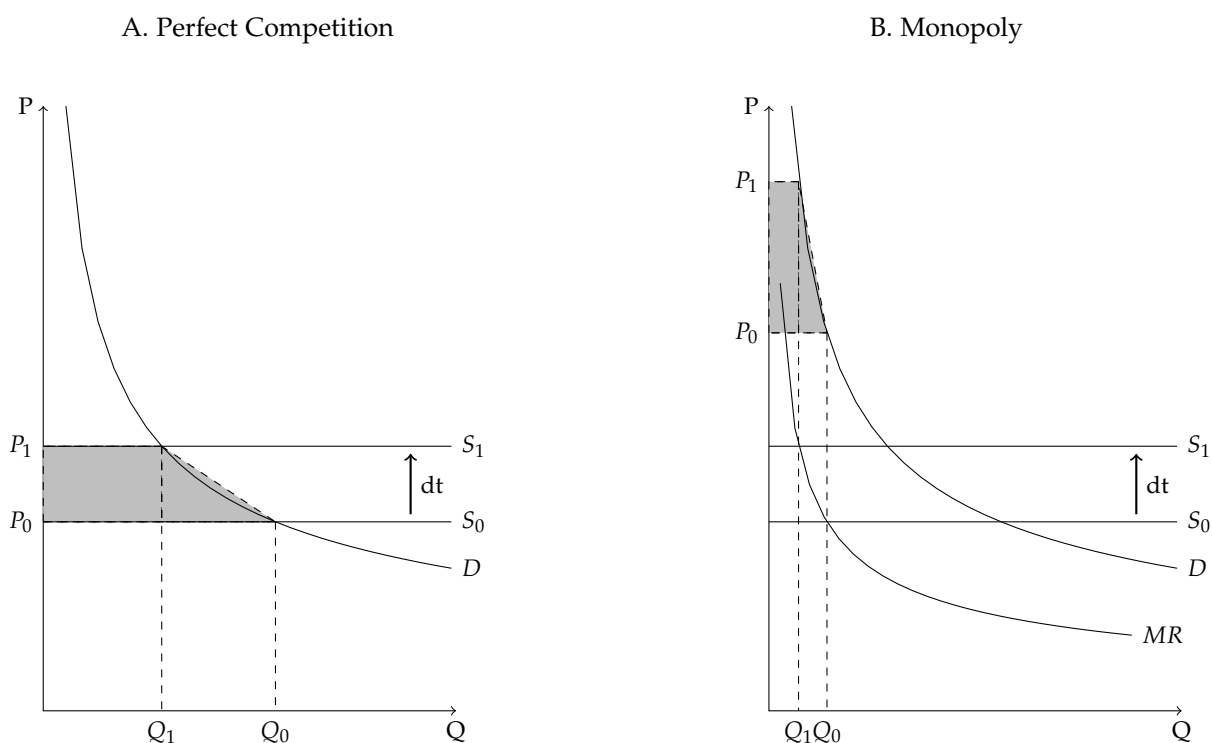
In certain circumstances, pass-through can even exceed 100% (Seade 1985). To see this point, consider the graphical depiction of (excise) tax pass-through in Figure 1.1. The two panels denote identical settings of linear supply, isoelastic demand, and a tax hike  $dt$  that shifts supply upwards from  $S_0$  to  $S_1$ .  $(P_1 - P_0)$  is thus the change in price due to the tax hike.

---

<sup>2</sup>Miller, Osborne, and Sheu (2015) investigate the effect of spatial isolation on fuel cost pass-through in the U.S. cement market. They find that increasing distance to competitors raises *own*-cost pass-through but reduces *rival*-cost pass-through; the two channels cancel each other out, so that pass-through is empirically insensitive to spatial competition.

<sup>3</sup>The shape of the supply curve is similarly relevant, though I assume it to be flat in Equation 1.2. Marion and Muehlegger (2011) identify a positive relationship between tax pass-through and the elasticity of supply in retail automotive fuel markets.

**Figure 1.1:** *Pass-Through with Isoelastic Demand*



There is only one difference between the two panels: in Panel A, competition is perfect, while in Panel B, supply is a monopoly. Panel A prices are simply set at the intersection of  $D$  and  $S$ . Panel B prices, in contrast, are set according to the marginal revenue curve  $MR$ . The monopolist first finds its optimal quantity at the intersection of  $MR$  and  $D$ , and then maps this quantity back to price using the demand curve.

Pass-through in Panel A is 100% because supply is perfectly elastic (i.e., flat); in Panel B, however, pass-through is *greater* than 100% ( $dp > dt$ ). This “overfull” pass-through is a result of the interaction between market power and sufficiently convex demand. Market power shifts the relevant quantity range, and demand convexity causes the slope of demand to be steeper over this new range. The slope is so steep that the resulting jump in prices exceeds the rise in taxes.

Overfull pass-through has been found in a variety of markets (see, e.g., Besley and

Rosen 1999), but in automotive fuel markets it has only been found in certain situations of abnormally high supply elasticity (Marion and Muehlegger 2011). To the extent that overfull pass-through is observed in energy markets, one plausible explanation is differential consumer search. If a fraction of consumers in a market are price-insensitive and always patronize the same gas station, while a fraction shop around much more, then demand may have the required convex shape. More generally, demand could be convex if those with the highest willingness to pay for energy are relatively richer, and if richer individuals are less price-sensitive than poorer ones. This latter pattern is a common result estimated in structural models of demand for a variety of goods (including, most relevantly, Houde 2012 for retail gasoline).

The preceding discussion serves to highlight the divergence between energy tax incidence analysis and its theoretical foundations. Pass-through need not be exactly 100%, and it can vary substantially at a local level due to the shape of demand and supply and the toughness of competition. In the next section, I introduce the data that I use to identify this *local* pass-through.

### 1.3 Background on Spain's Oil Markets

The Spanish retail automotive fuel market is an ideal setting for a study of the determinants of energy tax incidence: it appears highly imperfectly competitive;<sup>4</sup> it features panel variation in state-level taxes; and the government records very detailed price data in it. Three companies (Repsol, Cepsa, and BP) own the nine oil refineries operating in Spain (imports account for only 10% of refined diesel), and together they own a majority stake in the national pipeline distribution network. Most importantly, they are heavily forward-integrated into the retail market: 60% of retail gas stations in Spain bear the brand of a refiner. Not surprisingly, these companies face significant scrutiny from government and popular media alike, on the grounds of alleged collusion and some of the highest estimated

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<sup>4</sup>For background on the evolution of Spain's oil markets, see Contín-Pilart, Correljé, and Palacios (2009) and Perdiguero and Borrell (2007).



retail margins in all of Europe (see, for example, El País 2015).

One result of such scrutiny has been very close monitoring of pricing by gas stations. A government mandate which went into effect in January 2007 requires all stations across the country (more than 10,000 today) to send in their fuel prices to the Ministry of Energy whenever they change, and weekly regardless of any changes. These prices are then posted by the Ministry to a web page - called *Geoportal* - that is streamlined for consumer use; Figure 1.2 provides a representative screenshot. The objective of *Geoportal* is to help consumers optimize their choices of when and where to purchase automotive fuel, but it also provides rich data for analysis of retail fuel markets. I thus obtain daily price data for retail diesel (which has a 67% share of the retail automotive fuel market), as well as the location, amenities, brand, and wholesale contract type at all Spanish gas stations from January 2007 to June 2013. While my price data are therefore quite detailed, corresponding quantity (consumption) data are not collected with a frequency sufficient for use in my study of station-specific pass-through.<sup>5</sup>

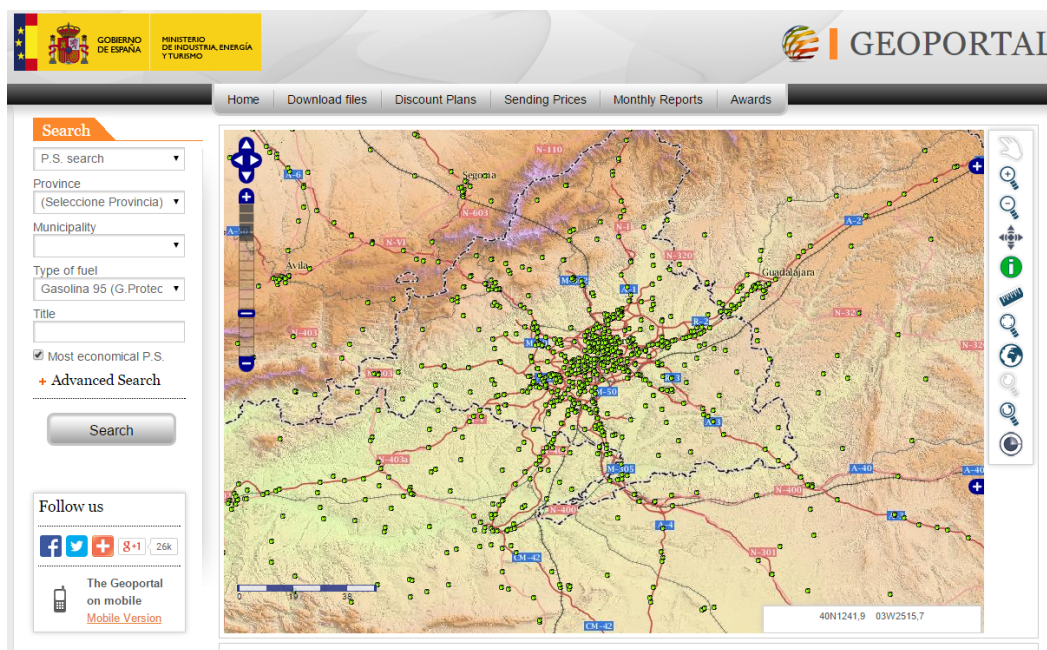
For each individual station, I calculate the overall concentration of stations in its vicinity, as well as brand-specific concentrations. My competition measures are an improvement over traditional indicators because they rely on driving times rather than administrative borders or straight-line distance. To start with, I compute the travel time by car between pairs of stations and define a station's competitors as all other stations within 5 minutes' drive. From the set of competitors within each station's 5-minute radius, I calculate two values. First, I tally the overall count of rival stations, weighted by inverse travel time. Second, I calculate the proportion of local stations within that radius that are under the same ownership as the reference station.<sup>6</sup> These values capture market power through a spatial channel and a branding channel, respectively. Neither of these measures is perfect; in

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<sup>5</sup>The government collects county-month consumption totals, but the county is too large a geographic area to be useful here. Station-year totals are also available, but many stations are missing values, so I do not use these data, either.

<sup>6</sup>I define two stations with the same brand as also being under the same *ownership* if each of them is (a) owned by that brand, or (b) operated either directly by that brand or under a "commission" contract, which ensures that the branded firm captures most of the profit from retailing.

Figure 1.2: Screenshot of Geoportal



Source: <<http://geoportalgasolineras.es/>>, accessed on February 15th, 2015.

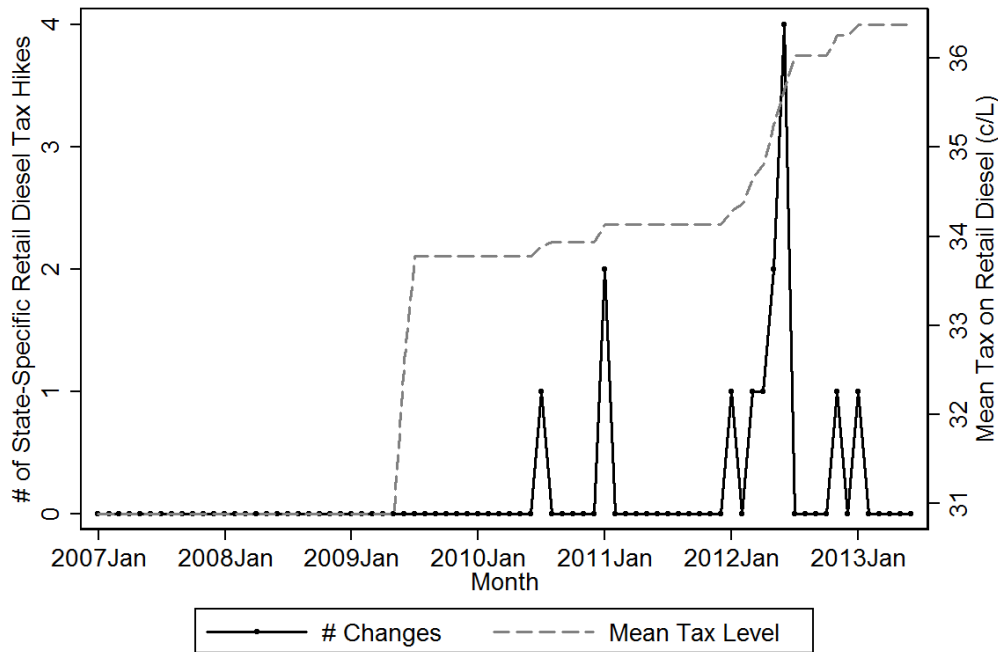
particular, they do not take into account the driving patterns of consumers, which are often a function of unobservables like place of work (Houde 2012). I cannot integrate commuting data into my analysis because no such dataset exists at the national level in Spain.

To these station-level data, I add information on a per-unit retail state diesel tax for 16 of the 17 Spanish states.<sup>7</sup> This tax, colloquially known as the 'centimo sanitario' ("public health" tax), has as its stated purpose the generation of revenues to be used for public health improvements. In my sample time period, it varies from 0 to 4.8 Eurocents/liter across states and discretely rises 14 times over my seven-year time period. This variation is plotted in Figure 1.3. While my data begin in January 2007, no state increases its diesel tax until early 2010. State-specific taxes are additional to federal excise taxes on retail diesel, which sum to 30.2 c/L at the start of my sample and increase once, to 33.1 c/L in June 2009. The total mean specific tax on diesel rises from just under 31 c/L at the start of my sample time

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<sup>7</sup>I was unable to obtain data on tax levels for the Canary Islands and for the two Spanish territories, Ceuta and Melilla. Stations in these areas are dropped from analysis.

**Figure 1.3: Tax Variation**



Note: The solid line plots state-specific tax hikes. The dashed line plots the national mean tax level; it rises discretely in June 2009 because the *national* component of the diesel tax rises in that month.

Source: Author’s calculation using data from the Spanish Ministry of Industry, Energy, and Tourism

period to to above 37 c/L at the end.<sup>8</sup>

Geographic and socioeconomic proxies for the demand side round out the list of variables which I use in my primary analysis. From the Spanish Statistical Institute, I collect annual population totals at all municipalities (there are 8,117 of these) and cross-sectional indicators of education level at 1-km<sup>2</sup> grid-squares (there are 79,858 of these). From the Spanish Ministry of Public Works, I obtain average house prices at the municipality-quarter level, for all municipalities with greater than 25,000 residents.

I calculate population density at the municipality-year level in order to proxy for the size of the consumption base and also the extent of public transit infrastructure (an alternative to

<sup>8</sup>There is additionally a national sales tax of 21% that applies to retail diesel sales. I remove the contribution of this tax from retail prices in all analyses.

driving which likely affects the elasticity of demand for diesel). Meanwhile, house-price data are useful as indicators of average lifetime wealth, an important determinant of automotive fuel demand that likely varies with brand and location choices. By the same token, education levels may be predictive of wealth and/or preferences for fuel consumption. All of these variables are doubly important: they allow me to better assess the causal link between competition and pass-through in their capacity as detailed proxies for the demand side; and they provide their own evidence of heterogeneity in pass-through, through non-competition channels.

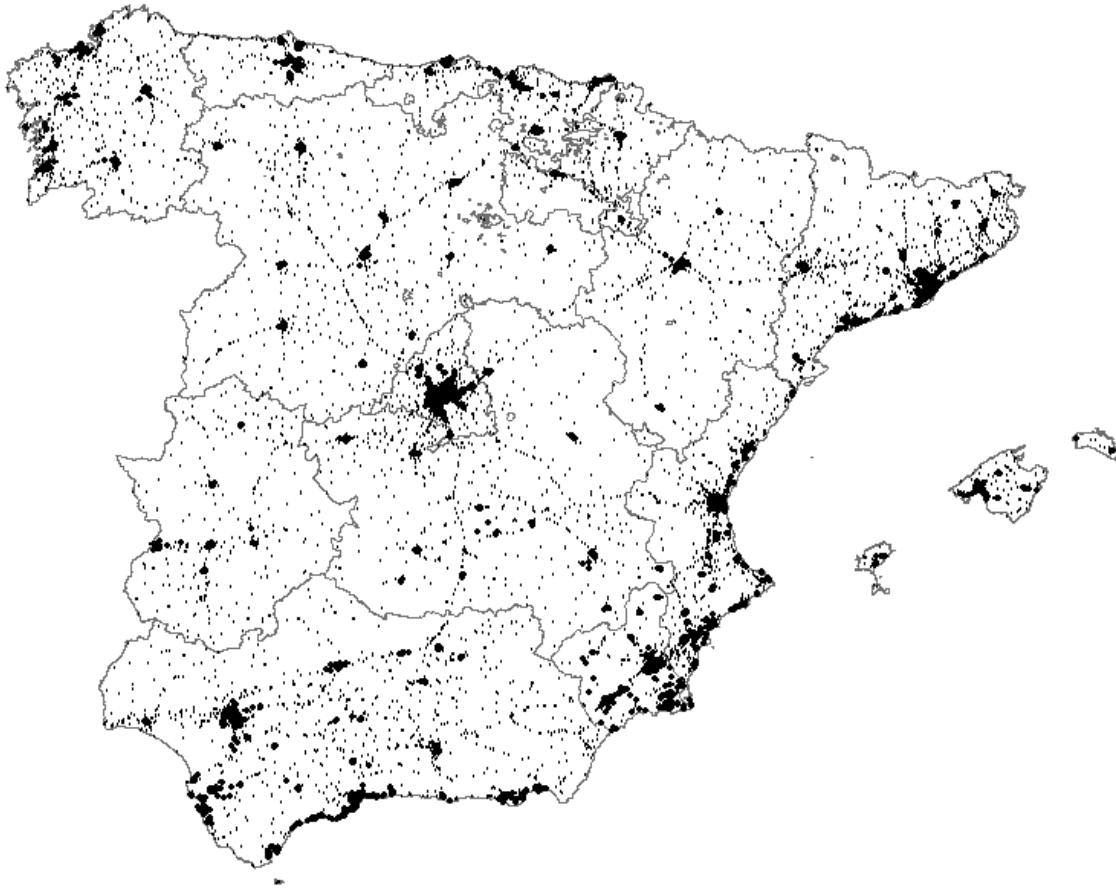
The raw *Geoportal* data contain 9,911 stations as of June 2013 (the end of my sample period). The total drops to 9,457 when I remove stations from the three areas with unknown tax levels. From this number, I select for analysis only those stations with non-missing demand-side indicators. The importance of these indicators to my empirical strategy justifies this cut. As I discuss below in Section 1.4, branding and location variables are endogenous - they are very likely determined with some knowledge of local wealth and driving preferences. Proxies for these characteristics are therefore integral to establishing a causal link between competition and pass-through. Moreover, given my intent to assess the degree of heterogeneity in pass-through, it is important to capture variation in both the toughness of competition and the makeup of the consumer base.

Because of the limited scope of house price measurement, as well as incomplete coverage by the survey on education, the effect of this sample restriction is to drop rural areas. In these areas, spatial competition is likely governed not by the local indicators that I am able to measure but by inter-city driving patterns. Indeed, a great many gas stations in Spain are situated along inter-city highways in unpopulated areas. Figure 1.4 illustrates exactly this fact, by mapping all stations and highlighting (with large dots) the stations in areas with non-missing demand-side characteristics. This “urban subsample” covers 26% of all Spanish gas stations (2,553 out of a possible 9,911) and will be my analytical sample for the remainder of the paper.<sup>9</sup>

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<sup>9</sup>I do, however, show results using these rural stations in the ensuing tables as a robustness check.

**Figure 1.4:** *Geography of Full and Restricted Samples*



Notes: All dots are Spanish retail gasoline stations. Large dots indicate the 2,553 stations included in my main analysis sample; small dots denote the remaining 7,358 stations used only in robustness checks. The analysis sample is chosen based on the availability of demand-side characteristics (population, house prices, and education levels).  
Source: Author's calculation, using data from the Ministries of Industry, Energy and Tourism (stations) and Public Works (house prices).

The price and non-price characteristics of the stations in my analysis sample are summarized in Table 1.1. The average, pre-sales-tax, retail diesel price is nearly 99 Eurocents per liter (c/L) during the sample period; this corresponds to a price of 4.70 \$/gallon at the end-of-sample exchange rate. While this mean price shows how much more expensive automotive fuel is in Spain relative to the U.S., it says nothing about the variation in prices over time and across space. Figure 1.5 gives a sense of this variation, by plotting time series of retail prices within and across counties of Andalucia state. I choose Andalucia arbitrarily because it is first alphabetically among Spanish states, but it is also the most populous state. The top panel of Figure 1.5 plots prices over time in the most expensive and least expensive counties of Andalucia (Málaga and Almería, respectively); there is essentially no difference in these county-average prices. The bottom panel, in contrast, plots prices at the most and least expensive municipalities *within* each of these counties. The cross-municipality range of prices is as much as 8 c/L (or  $\sim 38$  U.S. cents/gallon, as of June 2013) in a given week. This fact provides suggestive evidence that market conditions at the municipality level or finer do, in fact, matter for pricing decisions.

The rest of the statistics in Table 1.1, as well as those of Table 1.2, describe some of the factors that may contribute to the variation seen in Figure 1.5. Stations (and their retail fuel products) are differentiated by their brands, their contracts, their amenities, and their location with respect to rivals, allies, and consumers. As noted above, there are three companies in Spain that refine oil, sell wholesale refined fuel to retail operators, *and* own and/or operate retail stations themselves. Among the 2,553 stations in my analysis sample, 58% of them bear the brand of one of these three companies, referred to henceforth simply as 'refiners'. There are also 24 companies that engage only in wholesaling and retailing; 27% of stations bear one of these 'wholesaler' brands. The remaining 'independents' have no long-term contract (or branding agreement) with any of these companies, interacting with them only to purchase wholesale fuel on the spot market.

Any station that bears the brand of a refiner or wholesaler is further differentiated by its contractual arrangement, which describes the degree of vertical integration between the

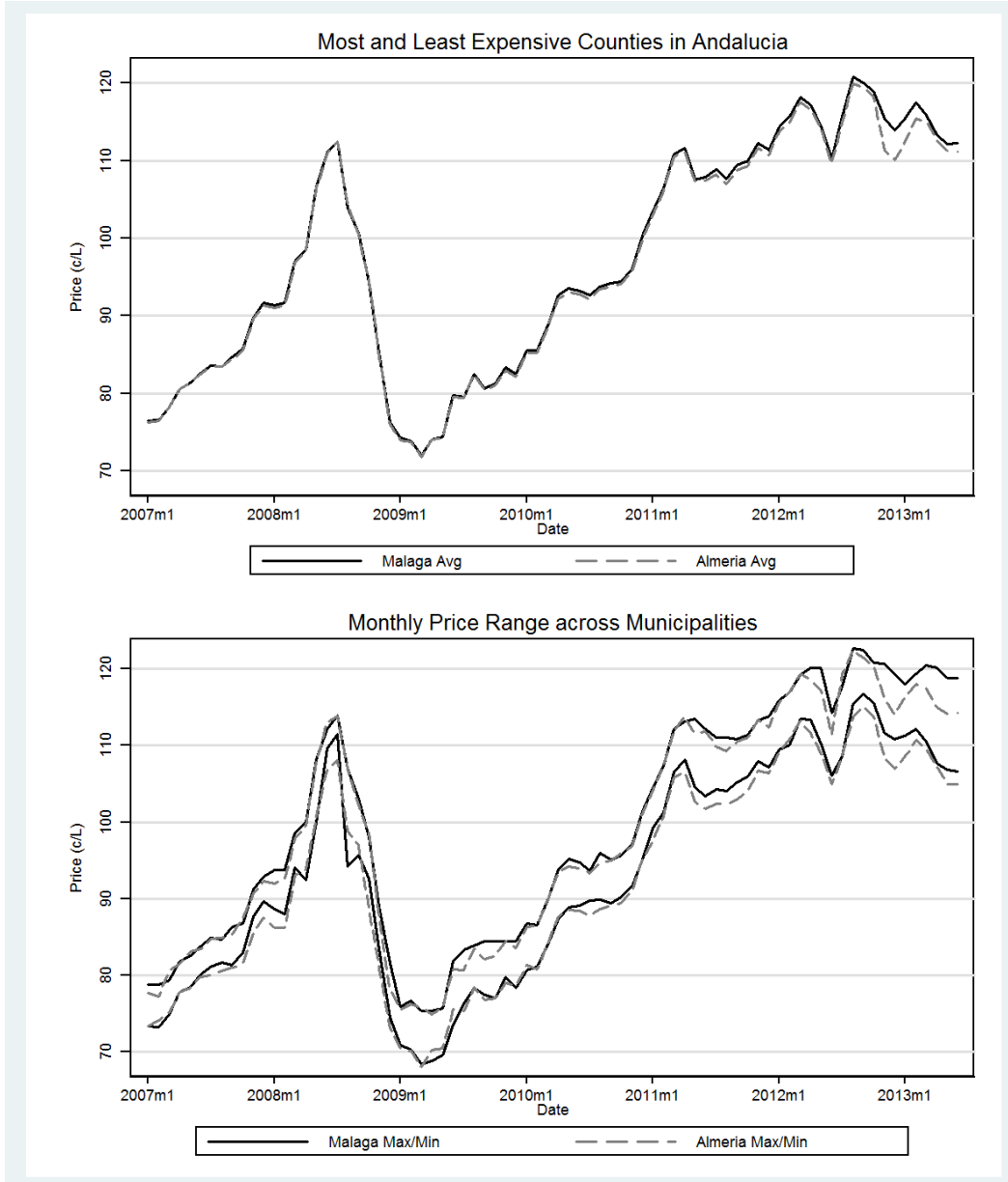
**Table 1.1:** *Characteristics of Spanish Retail Gas Stations*

	Mean	St. Dev.	Min.	Max.
Retail price (c/L)	98.59	4.84	73.54	117.64
Retail quantity (million L)	2.47	1.91	0.02	29.55
Brand				
Refiner	0.58	0.49	0	1
Wholesaler	0.27	0.45	0	1
Contract				
COCO	0.30	0.46	0	1
Commission contracted	0.30	0.46	0	1
Firm-sale contracted	0.19	0.39	0	1
Amenities				
Carwash	0.49	0.50	0	1
Tires and fluids	0.63	0.48	0	1
Convenience store	0.67	0.47	0	1
Cafeteria	0.13	0.34	0	1
N	2,553			

Notes: All statistics are calculated from station-level observations. Price and quantity vary over time and are first collapsed to station-specific means. Brand, contract, and amenities variables are cross-sectional dummies from the time of entry into Geoportal. 'Refiner' refers to any of the three brands with refining capacity in Spain (Repsol, BP, and Cepsa). 'Wholesaler' refers to all other brands (the omitted group is unbranded independents). COCO stands for 'Company-owned, company-operated' and indicates a fully vertically-integrated station. 'Commission' indicates a contract in which the station operator does not buy the wholesale fuel and thus makes only a small percentage commission on its sales. 'Firm-sale' indicates a contract in which the station buys the wholesale fuel and becomes the residual claimant. The sum of COCO, commission, and firm-sale contracts does not equal the sum of refiner and retailer brand counts because a small percentage of brand contracts remain unclassified in the data.

Source: Author's calculation using data from the Spanish Ministries of Industry, Energy, and Tourism.

**Figure 1.5: Price Variation Across and Within Counties**



Notes: The figure displays price trends calculated only with data from the state of Andalusia. Malaga and Almeria counties are the focus in both graphs because they are the counties with the (on average) cheapest and priciest diesel in the state, respectively. All data points are weekly, county-level measures (either mean, maximum, or minimum, as indicated by the legend).

Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.



**Table 1.2:** *Characteristics of Stations' Surroundings*

	Mean	St. Dev.	Min.	Max.
<i>Panel A. Competition measures</i>				
# of rival stations, weighted by inverse travel time (1/s)	0.47	0.14	0	2.13
Own-firm proportion	0.40	0.26	0.07	1
<i>Panel B. Demand-side characteristics</i>				
Municipal population density (1000s/km <sup>2</sup> )	2.89	3.81	0.03	20.56
Municipal mean house price (1000s of euros/m <sup>2</sup> )	1.99	0.64	0.83	3.86
Education: Some schooling, up to high school	0.11	0.05	0	0.35
Education: High school and/or professional degree	0.46	0.08	0.11	0.75
Education: Baccalaureate, master, or doctoral degree	0.17	0.11	0	0.66
N	2,553			

Notes: All statistics are calculated from station-level observations; if a variable exhibits panel variation, then I first collapse the variable to the station level. 'Rival stations' are those with a different brand than the reference station. 'Own-firm proportion' is the proportion of stations within five minutes' drive under the same ownership as the reference station. Shared ownership requires (a) shared brand, and (b) a commission or COCO contract at the reference station as well as the comparison station. Education-variable units are proportions (of a census-block population).

Source: Author's calculation using data from the Spanish Ministries of Industry, Energy, and Tourism (competition measures) and Public Works (house prices), and the National Statistical Institute (population density and education level).

station and its upstream supplier. There are a number of different contract classifications observed in Spain. For conciseness, I divide them into three categories. Company-owned, company-operated (COCO) stations are fully vertically integrated; the “company” is the upstream refiner or wholesaler. Commission-contracted stations are those in which the operator of the station does not buy wholesale fuel but rather sells it on behalf of the supplier, earning a commission. Finally, stations with firm-sale contracts physically purchase wholesale fuel and keep all profits from retailing.<sup>10</sup> These contracts are ordered from most to least vertically integrated. COCO and commission-contracted stations each account for 30% of all stations in my sample, while another 19% operate with firm-sale contracts. Unclassifiable contracts (‘Other’, in the data) account for the remainder of the 85% fraction of the sample that is branded.

Panel A of Table 1.2 provides a sense of the spatial and brand patterns in the Spanish retail automotive fuel market. Many stations have no competitors whatsoever within a five-minute drive, but some have quite a few - the maximum weighted rival count of 2.13 comes from a station with 22 competitors closer than five minutes away. However, the mean value of 0.47 indicates substantial skewing towards the bottom of the distribution. Most stations only have one or two neighbors, situated at least a minute away by car. For stations that do have nearby neighbors, the own-firm proportion indicator measures ownership concentration. The average station has shared ownership with 40% of other stations in its vicinity. The variable takes values of nearly 0 and identically 1 with some frequency, however, because some markets have almost no multi-station owners (own-firm proportion $\approx$ 0) and others are effective monopolies (own-firm proportion $\approx$ 1).

The final set of important variables is composed of demand-side characteristics: population density, house prices, and education levels summarized in Panel B of Table 1.2. The first of these variables exhibits an undeniably wide range of observed values. The sample average

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<sup>10</sup>Stations are additionally classified as company-owned, dealer-operated (CODO) and dealer-owned, dealer-operated (DODO) - where ‘dealer’ denotes a non-wholesaling entity - but I deem these classifications less important than the commission/firm-sale distinction. This conclusion is borne out by regression analysis, in which the type of sale has a larger and more statistically significant predictive effect on pass-through than the ownership-operation arrangement.

population density in this study is 2,890 people per square kilometer; the municipality of Jumilla in Murcia state has a mere 30 residents per km<sup>2</sup>, while Hospital L'lobregat – a section of Barcelona – has 20,560. Municipal-average house prices, meanwhile, vary around a mean of 1,990 Euros/m<sup>2</sup> from 830 at the cheapest to 3,860 at the most expensive. Finally, in the average neighborhood surveyed in the 2011 Census, 11% of residents' have some high school experience but did not graduate; 46% of residents have graduated high school and/or obtained a professional/technical degree; and 17% have baccalaureate, master's, or doctoral degrees. Spanish communities are thus characterized by sizeable variation in wealth, education, and urbanization – three characteristics of consumers that are likely to be closely related to driving preferences.

## 1.4 Average Pass-Through of the Spanish Diesel Tax

I begin my empirical analysis with a study of *average* diesel tax pass-through. Focusing on average pass-through allows me to explore the timing and location of tax variation in isolation, before moving on to a consideration of taxes and local market conditions jointly. Moreover, my estimates of this outcome are a logic test: if they differ substantially from the consensus of nearly 100% pass-through in the existing literature, then there must be some aspect of either my methods or my setting that explains this discrepancy.

Because I do not observe quantities sold by stations, I cannot estimate a demand curve structurally. Instead, I use a reduced-form model to linearly approximate prices at retail gas stations:<sup>11</sup>

$$P_{it} = \rho_{ii}C_{it} + \sum_{j \neq i} \rho_{ij}C_{jt} + X'_{it}\gamma + \lambda_i + \sigma_t + \varepsilon_{it}$$

In this formulation,  $P_{it}$  is the after-tax (but gross of sales tax) price of retail diesel at station  $i$  and week  $t$ ,  $C_{it}$  is station-specific costs, and  $X_{it}$  is a vector of observable demand and supply

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<sup>11</sup>Miller, Osborne, and Sheu (2015) start with the same model in their context of fuel cost pass-through by cement plants.

shifters.  $\lambda_i$  and  $\sigma_t$  are station and week fixed effects, respectively, and  $\varepsilon_{it}$  is a pricing residual that captures unobservable demand and cost conditions.

The cost terms illustrate the fact that prices are a function of both a station's own costs and its rivals' costs. Thus, pass-through can be divided into two channels: own-cost pass-through and rival-cost pass-through. I do not observe the  $C_{it}$  fully, so I cannot estimate these two parameters separately.<sup>12</sup> However, because my focus is on state-wide taxes, I am primarily interested in the aggregation of own- and rival-cost pass-through - what is called "industry cost" pass-through in the literature. I therefore replace the  $C_{it}$  with  $Tax_{it}$ , which measures the state-wide retail diesel tax. This yields the following estimating equation, common to most reduced-form pass-through analyses in the literature:

$$P_{it} = \alpha + \beta Tax_{it} + \delta \mathbf{X}_{it} + \lambda_i + \sigma_t + \varepsilon_{it} \quad (1.3)$$

$\mathbf{X}_{it}$  includes the panel-varying competition indicators and demand-side characteristics summarized in Table 1.2: number of rival stations and own-firm proportion (both defined for a five-minute radius); and population density and average house prices per unit area (both defined for a municipality). The week fixed effects  $\sigma_t$  capture national shocks to supply and demand in each week – such as changes in the price of crude oil or national weather trends that affect preferences for driving. The station fixed effects  $\lambda_i$ , meanwhile, capture permanent characteristics of stations – such as a negotiated price of wholesale fuel stipulated in a long-term supply contract, or the average income of a station's consumer base.

Equation 2.1 only identifies an average causal impact of taxes on retail prices if tax hikes are uncorrelated with unobservable determinants of prices (the  $\varepsilon_{it}$ ) after conditioning on the  $\mathbf{X}_{it}$  and station and week fixed effects. This, however, is far from obvious *ex ante*. According to correspondence with the Ministry of Industry, Energy, and Tourism, the state-level taxes in question have been raised in order to collect more revenue. States with relatively greater

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<sup>12</sup>It is possible that state borders could be leveraged to separate the two channels; a tax hike in one state affects a station in that state via the own-cost channel, while it affects a competitor across the border via the rival-cost channel.

need for revenue may have systematically different price trends from other states; this is one example of how pass-through estimation via the above equation could be invalidated. Moreover, even if treated states exhibit trends that are parallel to untreated ones, my analysis could be compromised if I do not account for potential anticipatory market responses to tax hikes. Coglianesse et al. (2015) show that U.S. consumers adjust their consumption of gasoline upwards one month in advance of tax hikes and downwards in the first month of the new tax level. While they fail to find corresponding adjustments in retail prices, the fact remains that tax hikes are anticipated.

### 1.4.1 Event study

To explore the viability of Equation 2.1 in identification of pass-through, I first estimate an event study model of price trends in the vicinity of tax changes. Event study provides a sense of pre-existing pricing patterns in locations experiencing a tax change, as well as the timing of a market's response to such a tax change. Its purpose is thus diagnostic - I use it only to assess the potential for endogeneity and anticipation, not to quantify pass-through.

#### Model

A natural starting point for event study of diesel tax hikes in Spain is the following model:

$$P_{it} = \alpha + \sum_{j=a}^b \pi^j D_{it}^j + \delta X_{it} + \lambda_i + \sigma_t + \varepsilon_{it} \quad (1.4)$$

This equation is identical to Equation 2.1 except that it parametrizes the role of taxes differently. Whereas before price was a function of taxes only in the current period, now price is allowed to move in advance of or in belated response to a change in taxes, through the set of terms  $D_{it}^j$ . The index  $j$  denotes a time period *relative* to the event of interest - a tax hike.  $D_{it}^j$  is thus a binary variable equalling one if an observation is both (a) in a state experiencing a tax hike and (b)  $j$  periods after (or before) that tax hike, where  $j \in [a, b]$ . Equation 1.4 is a conventional event study model, allowing prices to respond to an event flexibly over time. If prices respond either prematurely or with a lag relative to a tax hike,

that response will be captured by the coefficients  $\pi^j$ .

Several implementation details should be noted. First, and as suggested earlier, I choose the station-week as my baseline observation. Taxes themselves vary only at the state level; however, competition is a much more local phenomenon in retail automotive fuel markets. Meanwhile, the week level balances high resolution of analysis with computational tractability. Second, I choose  $[a, b]$  to be equal to  $[-12, 12]$ , which is an observation window of 6 months, and omit the term  $\pi^0 D_{it}^0$  so that the price impact in the week of the tax hike is normalized to zero. Third, I use all weeks from January 2007 through June 2013, regardless of their temporal proximity to tax hikes; this helps pin down my time fixed effects but necessitates the creation and inclusion of two dummy variables: one for an observation being from a period  $j < -12$ , and one for an observation being from a period  $j > 12$ . Fourth, I use all states, regardless of whether they are “treated” (with a tax hike) or “untreated”.<sup>13</sup> Fifth, and finally, I cluster standard errors at the state level.

## Findings

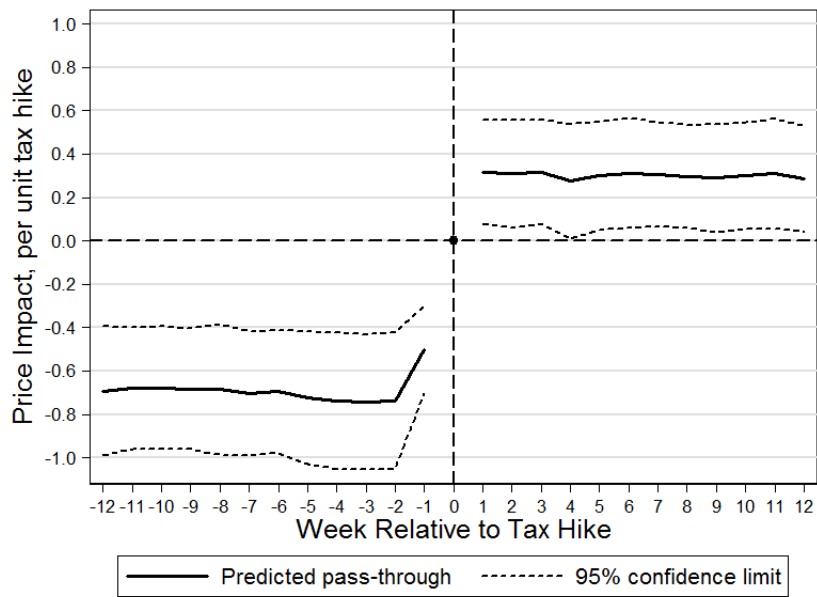
Figure 1.6 graphically depicts the results of the event study estimation of Equation 1.4. Each plotted y-value is the average value of  $\left( \frac{\pi^j D_{it}^j}{EventSize_{it}^j} \right)$ , which is the price predicted in a location  $i$  that has been (or is going to be) subject to a tax hike in period  $t + j$ . The y-value in week 0 is normalized to zero, so every other plotted point represents the predicted price relative to that initial week of the tax hike.

If there were observable trends or movements in the predicted price *before* tax changes take effect, these would raise concerns about the exogeneity of the tax changes. That is not the case here. Figure 1.6 exhibits extremely flat trends in prices both before and after the tax event. The only time period with any slope at all is a three-week period surrounding the tax event. Most of the price jump occurs in week 0 itself - right when the tax changes; however, there are rises in the week prior and the week after as well. I interpret these movements as

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<sup>13</sup>Estimation is also possible using *only* treated states, but this requires an additional parametric assumption (see McCrary 2007).

**Figure 1.6:** *Event Study of Tax Hikes: Overall Pass-Through*



Notes: Lines are constructed from coefficients estimated using Equation 1.4. The y-axis measures the average price associated with a given value of  $x$  (week), relative to Week 0, which is omitted from the regression. Mathematically,  $x = j$  and  $y = \frac{1}{N} \sum^i \left( \frac{\pi^j D_{it}^j}{EventSize_{it}^j} \right)$ , where  $i = 1, \dots, N$  indexes a station.

evidence that the market anticipates tax hikes by one week and takes one additional week after the hike itself to fully re-equilibrate.

The evidence strongly suggests that the retail price response to a tax hike is a mean shift. This observation, in turn, motivates a fixed effects regression model to identify the actual pass-through rate. Of course, Figure 1.6 does strongly hint at what this rate is: a comparison of the plotted price levels before the event with price levels after the event suggests a gap of at least 0.9 – i.e., average retail price rises 0.9 c/L for every 1 c/L of a tax hike – which translates directly to a pass-through rate of at least 90%. This estimate, as well as the pre- and post-trends estimated, is robust to a variety of specifications. The results hold for alternative event study models;<sup>14</sup> they hold at several different levels of observation;<sup>15</sup> and they hold with sample restrictions that exclude observations from outside of the six-month window of a local tax hike.

## 1.4.2 Difference-in-Difference Regression

Armed with the evidence provided by event study, I now return to Equation 2.1, reprinted below:

$$P_{it} = \alpha + \beta Tax_{it} + \delta \mathbf{X}_{it} + \lambda_i + \sigma_t + \varepsilon_{it}$$

Equation 2.1 identifies the average overall pass-through rate of diesel taxes in Spain. Single differences across time and across locations are captured by the corresponding fixed effects; the coefficient  $\beta$  then captures the difference-in-difference impact of a tax change. In estimating this equation, I make the exact same implementation choices as described above in Section 4.1 for the event study.

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<sup>14</sup>Equation 1.4 is, to my knowledge, consistent with all other published event studies in the economics literature, in that it parameterizes the event of interest as a dummy variable. This is equivalent to modeling only the extensive margin of the event. As a robustness check, I also estimate a model that captures the intensive margin, through a set of terms  $\sum_{j=a}^b (\theta^j D_{it}^j * EventSize_{it}^j)$ , as well as a model that captures both margins.

<sup>15</sup>I run event study regressions at the cross-sectional levels of station, municipality, and state, as well as the temporal units of week and month.



## Results

Table 1.3 displays the results of estimating Equation 2.1. Column 1 reflects the most sparse specification, in which prices are regressed on taxes and fixed effects only ( $X_{it}$  is empty); average pass-through here amounts to approximately 95%. Column 2 adds controls for my two local competition indicators, while column 3 adds the two panel-varying demand shifters – population density and house prices. Columns 4 through 6 test the robustness to three different adjustments: the addition of state-year fixed effects, the use of first (i.e., one-week) differences instead of fixed effects, and the inclusion of rural-station observations, respectively.

The estimated average pass-through rate is very robust to the specification adjustments in columns 2 through 6: the minimum estimate is 93.9% and the maximum is 95.2%. Importantly, none of these point estimates is statistically different from 100% at conventional (5%) significance levels. These results are very much in line with existing estimates of average pass-through; Chouinard and Perloff (2004), Alm, Sennoga, and Skidmore (2009), and Marion and Muehlegger (2011) all fail to reject the null hypothesis that state-level automotive fuel tax pass-through is fully 100%. The evidence in Table 1.3 thus corroborates the pattern of high pass-through in the existing literature.

### 1.5 Local Pass-Through

Having estimated the magnitude of average pass-through, I now investigate how applicable that average rate is to individual stations and communities. Is there even a reason to believe that pass-through varies at a local level? The mathematical and graphical examples of section ?? (and the derivations of Appendix A.1) imply that there is, but the empirical literatures on both pass-through and welfare impacts of energy taxes abstract away from the possibility. To assess the extent of heterogeneity in pass-through across stations of different types, I return to Equations 2.1 and 1.4 and add interaction terms between the key tax variable(s) and my indicators of local competition and preferences. As in Section 1.4, I begin

**Table 1.3: Overall Pass-Through**

	(1)	(2)	(3)	(4)	(5)	(6)
Mean Tax Level (c/L)	0.952*** (0.036)	0.952*** (0.036)	0.938*** (0.036)	0.946*** (0.027)	0.939*** (0.039)	0.940*** (0.035)
Own-firm proportion		-0.025 (0.273)	0.021 (0.252)	0.026 (0.225)	-0.200 (0.178)	-0.065 (0.218)
# of Stations w/in 1 km		-0.398*** (0.102)	-0.353*** (0.108)	-0.276** (0.125)	-0.044 (0.202)	-0.367** (0.168)
Demand shifters			X	X		
State-year FE				X		
First differences					X	
Full sample						X
R-Squared	0.996	0.996	0.996	0.996	0.780	0.995
N	730,146	730,146	730,146	730,146	415,155	2,622,632

Notes: Dependent variable is retail price (c/L), except in column (5), where it is the one-week change in that price. An observation is a station-week. 'Demand shifters' are municipal average house price and municipal population density. 'Full sample' refers to the complete national dataset, as opposed to the default urban subsample I use in analysis. All specifications are estimated via OLS with station and week fixed effects. Standard errors, clustered at the state level, are in parentheses.  
 Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

with an event study.

### 1.5.1 Event study

I augment Equation 1.4 by creating interaction terms between the event study variables and the competition indicators in  $\mathbf{X}_{it}$ . Adding these terms, either separately or simultaneously, yields the following event study model:

$$P_{it} = \alpha + \sum_{j=a}^b \pi_0^j D_{it}^j + \delta \mathbf{X}_{it} + \sum_{k=1}^K \left[ \sum_{j=a}^b \left( \pi_k^j D_{it}^j * X_{it}^k \right) \right] + \lambda_i + \sigma_t + \varepsilon_{it} \quad (1.5)$$

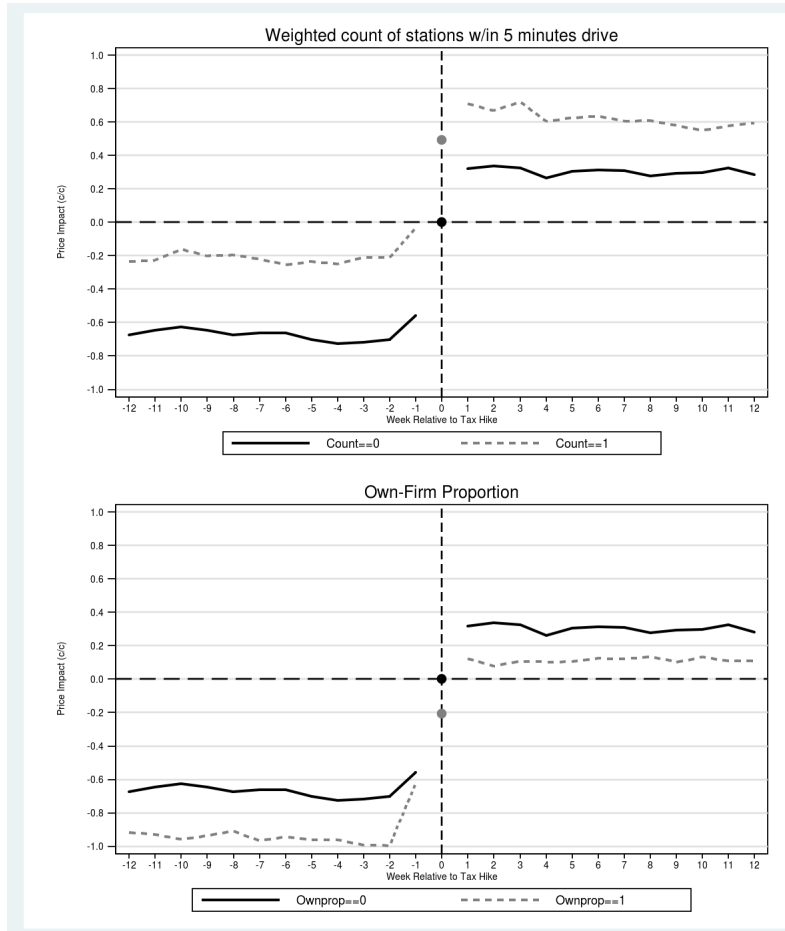
where  $k$  indexes the  $K$  variables in  $\mathbf{X}_{it}$ . The coefficient  $\pi_0^j$  predicts the price response at relative period  $j$  in the omitted group. The coefficient  $\pi_k^j$ , meanwhile, predicts the *differential* price response in period  $j$  given a one-unit increase in variable  $X_{it}^k$ .

I estimate Equation 1.5 using the exact same implementation choices as described in Section 1.4.1. For conciseness, I limit my main graphical analysis to two primary indicators: the weighted count of nearby rivals, and the own-firm proportion variable. The former measures market power through spatial isolation, while the latter measures market power through ownership concentration. Figure 1.7 plots the same predicted price responses to taxes as Figure 1.6, except that trends are shown separately for stations with different values of the two competition variables. I provide event-study results for other supply- and demand-side characteristics in Appendix A.2.

To calculate the data points in Figure 1.7, I compute the value of  $\frac{\pi_0^j D_{it}^j + \pi_k^j D_{it}^j X_{it}^k}{EventSize_{it}^j}$  given  $X_{it}^k = 0$  and  $X_{it}^k = 1$ , for each station-week observation. From these predictions, I calculate mean values in each relative week  $j$  and plot them against  $j$ . The solid line denotes price trends given  $X_{it}^k = 0$ , while the dashed line pertains to  $X_{it}^k = 1$ . A comparison of these two lines tests whether a gas station's temporal response to taxes varies with its local competitive environment.

Figure 1.7 shows that pre- and post-trends are flat. Stations of different types do not seem to respond differentially over time to tax hikes. Rather, both panels show two trends moving in striking parallel. Figure 1.7 does not, on its own, prove the exogeneity of brand

**Figure 1.7:** Event Study of Tax Hikes: Temporal Trends by Brand and Location



Notes: All lines are constructed from coefficients estimated simultaneously using Equation 1.5. The y-axis measures the average price associated with a given value of  $x$  (week), relative to Week 0, whose coefficients are omitted from the regression. In each panel, the solid line is the predicted price given a value of zero for the relevant competition variable, while the dashed line is the predicted price given a value of one.

and location, as these may still be cross-sectionally correlated with unobserved determinants of pass-through. However, it is clear that the mean shift categorization of average pass-through in Figure 1.7 holds across different competitive environments. I therefore deem a fixed-effects specification suitable for quantifying the difference in pass-through rates predicted by competition indicators.

The plotted trends do, however, provide early indication of a relationship between pass-through and local competition. The gap between trends at a station with no rivals and a station with weighted rival count equal to one narrows. Meanwhile, the gap between the zero-concentration trend and the effective-monopoly trend widens immediately after tax hikes. Both trends suggest a positive relationship between market power and pass-through; I use fixed effects to quantify that relationship.

## 1.5.2 Difference-in-Difference Regression

### Model and threats to identification

I modify Equation 2.1 to capture heterogeneity in pass-through:

$$P_{it} = \alpha + \beta Tax_{it} + \sum_{k=1}^K \left( \gamma_k Tax_{it} * X_{it}^k \right) + \delta \mathbf{X}_{it} + \lambda_i + \sigma_t + \varepsilon_{it} \quad (1.6)$$

The  $\gamma_k$  provide an estimate of the association between pass-through and a one-unit increase in  $X_{it}^k$ . However, interacting  $Tax_{it}$  with  $X_{it}^k$  introduces significant risk of endogeneity. Consider branding and location. These characteristics are not randomly assigned in space; rather, the choice of where to locate a gas station and what brand to sell is likely made by considering potential profits and thus local demand and supply characteristics, some of which are unobservable. Station fixed effects control for the average effect of omitted variables on prices but not on *pass-through*. If would-be station owners choose spatial and branding characteristics based on local wealth or, more generally, local preferences for diesel, then I run the risk of conflating the effect of competition with those preferences.

In the case of station location, correlation with unobservable determinants of demand would most likely bias estimates of  $\gamma_k$  in Equation 2.2 upwards. This is because station

owners presumably prefer, all else equal, to locate in areas with more inelastic demand, which itself drives pass-through upwards. The prediction for endogenous brand (and contract) choice is less clear, as it depends on the strategy of each specific brand. If, for example, a certain brand likes to concentrate in areas with more inelastic demand, then parameter estimates corresponding to that brand's concentration may be biased upwards. However, if *all* brands would like to locate in these areas, then it is not clear which precise branding pattern emerges, and the potential bias is difficult to sign.

The demand attributes faced by specific stations are inherently difficult to measure, especially because consumers sort into stations based on commuting patterns and willingness to price-shop. However, controlling for group-average observables has the potential to absorb much of the selection of my competition "treatments" on unobservables (Altonji and Mansfield 2015). I therefore compare results of estimation of Equation 2.2 using just competition interactions versus additionally including interactions with my observable proxies of demand: population density, house prices, and education levels. House prices act as a city-level proxy for wealth; robustness to their inclusion would suggest my competition results are not being driven by the average wealth of a station's municipality. Similarly, insofar as population density is a proxy for infrastructure investments like public transit, robustness to the inclusion of an interaction between it and the tax would suggest that my results are not driven by certain stations locating in areas with fewer transportation alternatives. Finally, interactions between the tax and indicators of educational attainment allow a robustness test using a very different level of variation – the educational indicators that I use are cross-sectional (from 2011), but they are also disaggregated to the 1-km<sup>2</sup> geographic level. Thus, if stations choose brands and/or locations based on the preferences of the population living in the immediate vicinity, then I can control for the part of those preferences that is correlated with education. More generally, my underlying logic is that, even if house prices, population density, and education do not fully absorb selection on unobservables, they remain useful as a guide to the degree to which remaining selection might affect my estimates (Altonji, Elder, and Taber 2005). If one assumes that the effect

**Table 1.4:** *Pass-Through and Competition: Each Metric Separately*

	(1)	(2)	(3)	(4)
Mean Tax Level (c/L)	0.845*** (0.046)	0.943*** (0.036)	0.868*** (0.036)	0.598*** (0.076)
Mean Tax Level X 1[Refiner Brand]	0.138*** (0.029)			
Mean Tax Level X # of Stations w/in 1 km		-0.117*** (0.035)		
Mean Tax Level X Own-Brand Proportion			0.173*** (0.045)	
Mean Tax Level X Avg. House Price				0.220*** (0.037)
N	730,146	730,146	730,146	730,146

Notes: Dependent variable is retail price (c/L). An observation is a station-week. All specifications are estimated via OLS with station and week fixed effects and a control vector of competition and demographic indicators. Standard errors, clustered at the state level, are in parentheses.

Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

of *unobservable* aspects of demand is bounded above by the effect of observable aspects, then the change in point estimates brought about by inclusion of observables is equal to that upper bound.

## Results

Table 1.4 provides point estimates on each interaction of interest separately. In column 1, branding is the characteristic in focus; in column 2, it is the number and proximity of rival stations; in column 3, it is the proportion of nearby stations under the same ownership as the reference station; and in column 4, it is the average house price of a municipality.

Each indicator (except for the wholesaler-brand dummy) is a statistically significant predictor of pass-through when examined separately. All are significant at the 5% level, while three of the four are significant at the 1% level. The refiner-brand point estimate has the following interpretation: switching from being unbranded to bearing the brand of a refiner is associated, on average, with a rise in pass-through of 13.8 percentage points.

Meanwhile, pass-through *drops* an average of 11.7 percentage points per each one-unit increase in weighted rival count. Since the latter variable runs from 0 to  $\sim 2$  in the data, the implication is that concentrated spatial competition can potentially reduce pass-through by as much as  $\sim 23.4$  percentage points. Concentrated ownership also is associated with higher pass-through: a local monopoly (own-firm proportion=1) is associated with a pass-through rate 17 percentage points higher than a station with negligible concentration (own-firm proportion $\rightarrow$ 0). Finally, a one-unit rise in average house prices predicts a 22 percentage-point rise in pass-through.

The column 1 result suggests that something about refiner brands – whether it is market power generated by brand loyalty, the degree of vertical integration, or some other factor – drives pass-through upwards. Columns 2 and 3 indicate possible effects of market power through spatial isolation (column 2) and ownership concentration (column 3). Column 4 shows that areas with higher property values are, for one reason or another, places with larger price impacts of taxation. These coefficients are strong motivation for continued study of local pass-through patterns, but they are also estimated in isolation. For simultaneous estimation, I move on to Table 1.5.

Column 1 of Table 1.5 shows the results of simultaneous estimation of three competition variables in 1.5. Each coefficient is reduced in magnitude to some degree, but all remain statistically significant. In particular, the refiner-brand indicator and the rival count variable retain their statistical significance at the 1% level and imply predictive effects of over 10 percentage points on pass-through for a one-unit change in their values. The coefficient on own-firm proportion, meanwhile, drops from 0.17 to 0.11 but is still significant at the 5% level.

Columns 2 through 4 successively add other observable indicators of both the supply side and the demand side. Column 2 includes interaction terms between the tax and four station amenities: carwash services, tire and fluid services, convenience store, and cafeteria. The inclusion of these variables shows whether the results for my primary competition indicators are driven by differences in the services provided by each station. The point estimates in



Table 1.5: Heterogeneous Pass-Through: Full Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Mean Tax Level (c/L)	0.814*** (0.045)	0.838*** (0.051)	0.767*** (0.073)	0.231 (0.203)	0.532*** (0.144)	0.772*** (0.052)
Mean Tax Level X 1[Refiner Brand]	0.124*** (0.028)	0.136*** (0.030)	0.117*** (0.024)	0.095*** (0.023)	0.100*** (0.021)	0.115*** (0.017)
Mean Tax Level X # of Stations w/in 1 km	-0.114*** (0.030)	-0.099*** (0.031)	-0.106*** (0.030)	-0.090*** (0.017)	-0.054** (0.019)	-0.036* (0.018)
Mean Tax Level X Own-Brand Proportion	0.110** (0.043)	0.113** (0.043)	0.079* (0.039)	0.090** (0.038)	0.085*** (0.028)	0.057* (0.030)
Mean Tax Level X # of Avg. House Price				0.195*** (0.042)	0.124*** (0.036)	
Station amenities interactions		X	X	X	X	X
Brand proportion interactions			X	X	X	X
Demand-side interactions				X	X	X
State-year fixed effects					X	
Rural subsample						X
N	730,146	730,146	730,146	730,146	730,146	2,599,966

Notes: Dependent variable is retail price (c/L). An observation is a station-week. All specifications are estimated via OLS with station and week fixed effects and a control vector of competition and demographic indicators. Tax interactions with a dummy variable for being a 'wholesaler-brand' station are included in regression but omitted from the table; the corresponding point estimates are not statistically significant. 'Station amenities interactions' are tax interactions with dummy variables for the presence of a carwash, a convenience store, a cafeteria, and tires and fluids services. 'Brand proportion interactions' are tax interactions with proportions of refiner-brand and wholesaler-brand stations, as well as the local Herfindahl Hirschman Index. 'Demand-side interactions' are tax interactions with municipal population density and census-block educational attainment. Standard errors, clustered at the state level, are in parentheses. Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

column 2 suggest that this is not the case; conditional on the effect of station amenities, pass-through is still strongly associated with a gas station's brand, its spatial isolation, and the extent of shared ownership in its vicinity. The same can be said after including indicators of local refiner-brand and wholesaler-brand proportions and a Herfindahl Hirschman Index, as is done in column 3. The addition of these variables is motivated by the significance of the own-brand market power measures; if, e.g., one's own connection to a refiner brand is important, then perhaps the connection of other nearby stations is also important. In that case, the coefficient on own-firm proportion could be driven not by shared ownership generally but by the intensity of refiner-brand activity specifically. Column 3 suggests that even conditional on local refiner-brand proportion, own-firm proportion remains statistically significant.

Column 4, however, is the truest test of the robustness of my measured competition effects. In this column I include interactions between the tax and my three demand-side characteristics: average house prices, population density, and educational attainment. These are, of course, mere proxies for the wealth, consumption base, and public transit infrastructure that more directly affect demand; I am unable to completely control for the effect of the demand side on pass-through. Robustness of my competition results to the inclusion of these demand shifters is therefore not a sufficient condition for a causal interpretation. However, it *is* a necessary condition. Furthermore, the degree to which my point estimates and their significance drop in response to the new demand-side variables provides a guide to the remaining bias due to omitted variables.

Observable characteristics of local consumers do not, according to column 4, affect the size or significance of competition effects in a meaningful way. The coefficients on refiner brand and rival count drop approximately two percentage points but still imply economically significant 9-9.5 percentage-point impacts in pass-through per unit change. The coefficient on own-firm proportion actually rises in significance, from the 10% level to the 5% level. The fact that refiner brand, rival count, and own-firm proportion move only minimally while retaining high economic and statistical significance suggests that the

effect of unobservable market conditions would have to be a good deal larger than the effect of observable ones in order to negate such significance. The evidence supporting a causal impact of local competition is therefore strong.

In the case of refiner branding, it is difficult to explain the precise mechanism of the pass-through impact. Two possible explanations are that customers have brand loyalty that creates market power for larger brands (60% of Spanish stations are refiner-branded), and that vertical integration by a retail gas station and an upstream refiner changes either the cost structure or the retail pricing strategy employed. In the case of own-firm proportion and rival-station count, the identified impacts are most easily explained by traditional market power stories. A firm owning multiple stations in the same area may have a stronger incentive to raise prices in response to a cost shock, because the sales lost from these price hikes at any one of its stations will partially be recouped by its other stations. Meanwhile, a lack of spatial competition may have a similar incentive effect, because consumers have fewer options for switching away from their usual station when its prices rise. Through both both of these channels – branding patterns and spatial isolation – market power thus appears to raise pass-through.

Two further specifications, whose results are shown in columns 5 and 6 of Table 1.5, provide additional robustness checks. Column 5 displays results of estimation with state-year fixed effects. All three key competition indicators remain significant, though their relative importance changes slightly – the own-firm proportion coefficient becomes significant at the 1% level, while the rival-count coefficient drops in magnitude to a 5.4 percentage-point effect and is significant at only the 5% level. Column 6, in contrast, uses the whole of Spain in estimation. To run this regression, I must omit two of my demand shifters (house prices and education levels), but the results are nonetheless informative. The three key competition indicators remain statistically significant, while, as suggested in Section 1.3, the point estimates on variables corresponding to gas stations' surroundings are much noisier. Interestingly, *all* of the competition indicators examined in this table are significant when the full national panel is used.

Lastly, but not least importantly, the interaction between the diesel tax and municipal-average house prices is very significant, both economically and statistically, according to my preferred specification in column 4. A one-unit change in the house-price variable corresponds to a 1,000 Euro/m<sup>2</sup> rise, in a measure whose standard deviation is 640 Euro/m<sup>2</sup> (as shown in Table 1.2). This one-unit change is associated with a 19.5 percentage-point increase in pass-through which is statistically significant at the 1% level. I do not make any claim on causality here; many things are correlated with house prices. However, insofar as house prices are a proxy for lifetime wealth, my result has significant implications for the joint distribution of wealth and the price impacts of taxation. I return to this idea in detail in Section 2.5.

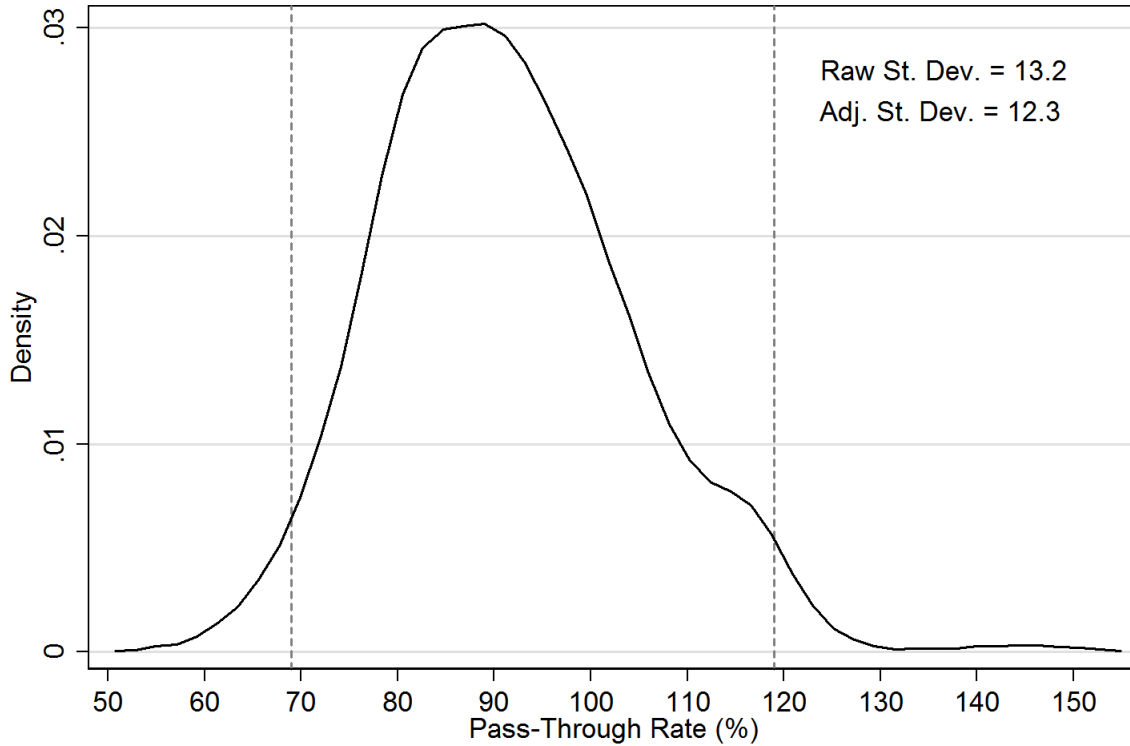
### 1.5.3 The empirical distribution of pass-through

Regardless of whether the effects identified in Table 1.5 have a causal interpretation, they provide strong evidence that pass-through is heterogeneous. Ultimately, the point of this research is to show that pass-through varies from location to location; distributional analyses that assume away this heterogeneity run the risk of yielding inaccurate results. How significant could this inaccuracy be? To begin answering this question, I use my estimated coefficients to calculate station-specific pass-through rates and graph them to explore their overall distribution.

I calculate station-specific price impacts as the linear combination of the predictive effects of all tax terms  $-\beta Tax_{it} + \sum_{k=1}^K (\gamma_k Tax_{it} * X_{it}^k)$  in Equation 2.2 above. I divide this value by  $Tax_{it}$  to yield an estimate of pass-through  $\frac{dp_{it}}{dt_{st}}$  for each station  $i$  in week  $t$ . In Figure 1.8, I plot these rates on the last day of observation for each station, using a kernel density estimator. Not surprisingly, the central tendency is 91% pass-through. However, the full range of observed pass-through rates ranges from 50% to 150%. 95% of these rates fall between 72% and 115%.

It is natural to ask how much of the pass-through distribution's spread is due simply to noise. To answer this question, I calculate the empirical variance of the pass-through

**Figure 1.8:** *Predicted Empirical Distribution of Pass-Through*



Notes: The figure displays the empirical distribution of pass-through rates across stations using kernel density estimation. Each input data point is a pass-through rate calculated from Equation 2.2, according to its observable characteristics and the estimated predictive effects of those characteristics on pass-through. There is one data point for each station, corresponding to the last day of its observation in the data. Vertical dashed lines denote percentiles 2.5 and 97.5 of the empirical distribution. The raw standard deviation of this distribution is reported in the top right corner. Below it, the adjusted standard deviation of the 'shrunk' distribution is reported. This adjusted standard deviation is equal to the sample variance of pass-through rates minus noise, where I estimate noise as the average of the variances of each station-specific pass-through estimate.

rates used in Figure 1.8 and subtract off an estimate of noise. To estimate noise, I compute the standard error of each station's pass-through estimate, square it, and take the average across all stations. As the top-right corner of Figure 1.8 indicates, removing noise drops the standard deviation of the station pass-through rate from a raw value of 13.2 to an adjusted value of 12.3. That change corresponds to a contraction in the 95% confidence range of about 4 percentage points<sup>16</sup>.

Pass-through patterns provide indirect insight into the nature of demand for automotive fuel. 24% of retail gas stations pass-through more than 100% of taxes to end consumers; this fact is inconsistent with both perfect competition and linear demand, both of which are common assumptions in the energy tax incidence literature. The most plausible explanation for rates above 100% is a setting of imperfect competition and sufficiently convex demand (like the isoelastic demand curve plotted in Figure 1.1). Other possible explanations – such as a lack of salience of taxes that drives consumers to under-respond to tax movement (Chetty, Looney, and Kroft 2009) – are less likely to be relevant, given the tax-inclusive nature of posted prices.

In sum, both local preferences and competition levels appear to play a significant role in determining rates of energy tax pass-through in the Spanish diesel market. The analysis suggests that, from station to station and from market to market, there can exist extremely large differences in the size of the consumer tax burden. In the next section, I explore what this means for policy design and assessment.

## 1.6 Pass-Through and the Wealth Distribution

How does pass-through heterogeneity affect who ultimately bears the burden of automotive fuel taxes? The *average* pass-through rate is most commonly used to provide insight into the consumer-producer breakdown of the tax burden, but *station-specific* rates allow me to compare burdens across different consumer groups. I focus on wealth, since regressive

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<sup>16</sup>While there is additional noise coming from the explanatory variables themselves, it is more than counteracted by attenuation of the estimates due to measurement error.

incidence across the wealth distribution is one of the most oft-cited properties of energy taxes.

The consensus finding in the energy tax incidence literature (described above in Section ??) is that such taxes are regressive. This is generally due to the fact that poorer households are observed to spend a greater portion of their wealth on energy, at least in the U.S. However, several factors that mitigate this regressivity have been identified. First of all, regressivity estimates are sensitive to the specification of wealth; Poterba (1991) shows that annual expenditure is a better proxy for lifetime wealth than annual income, and that using the former leads to smaller magnitudes of regressivity in the U.S. gasoline tax. Second of all, the poorest households often do not own energy capital such as automobiles; including these households in analysis can vastly reduce regressivity (Fullerton and West 2003), especially in the developing country context (Blackman, Osakwe, and Alpizar 2009). Third of all, the demand response to taxes is unlikely to be static across the wealth distribution; West (2004) and West and Williams (2004) estimate that the gasoline demand elasticity drops (in absolute magnitude) as income rises in the U.S., which makes consumer surplus impacts less regressive than when demand response is assumed to be homogeneous.

One of the primary contributions of this paper is to add a fourth-mitigating factor: pass-through heterogeneity. Just like the demand elasticity – indeed, *because* of the demand elasticity – pass-through need not be static across the wealth spectrum. In fact, pass-through heterogeneity is likely to have a much greater effect on tax incidence than corresponding heterogeneity in demand elasticity, because the welfare lost due to higher prices on maintained consumption probably dwarfs the welfare lost from consumption foregone. In my own context, I find economically significant variation in pass-through rates across the house-price distribution. Pass-through rises in municipal wealth, and this, in turn, should make the retail diesel tax relatively less regressive (or more *progressive*).

Return to Figure 1.1 to see the direct consumer surplus impacts of a tax hike shaded in gray. I do not estimate the demand curve itself, so I am unable to calculate the deadweight loss triangle component. However, pass-through provides traction for estimation of the

rectangular component, which is the welfare lost from consumption maintained in the face of the tax hike. For small changes, this rectangle is mathematically the first-order approximation of consumer surplus impacts. Given low elasticities of demand for retail energy, it is also likely the larger of the two welfare components<sup>17</sup>. Pass-through measures the height of the rectangle, so combining it with a measure of the width (i.e., consumption) allows for calculation of the rectangle's area –  $\frac{dp}{dt} Q_1$ .

In distributional welfare analysis, the goal is compare the size of consumer surplus impacts across, e.g., the wealth spectrum. In the absence of a demand curve, the most common method of assessing regressivity is a comparison of  $\frac{dp}{dt} \frac{Q_1}{W}$  across quantiles of wealth  $W$ . Dividing by  $W$  converts consumer surplus changes to proportions of total wealth. Examples of this in the context of automotive fuel taxation are Poterba (1991) and Fullerton and West (2003). The Treasury Department's Office of Tax Analysis does the same for its own estimates of tax burdens (Fullerton and Metcalf 2002).

If  $\frac{dp}{dt} \frac{Q_1}{W}$  rises with wealth decile, then tax  $t$  is progressive; if it falls, then  $t$  is regressive. In practice, the latter is almost always true, at least for some portion of the wealth distribution. However, implementation of the exercise has, to date, relied on an assumption of full, uniform pass-through – i.e.,  $\frac{dp}{dt}$  is identically 1 and does not vary with wealth. The expression then collapses to  $\frac{Q_1}{W}$ , which accurately captures tax *revenues* per unit consumption but is only proportion to tax burden if pass-through is uniform<sup>18</sup>. This is precisely the opposite of what I find empirically in Spain's retail diesel market.

To show the effect of systematic variation in pass-through with wealth, I carry out the incidence calculation both with and without the assumption of uniform pass-through, using data from the 2013 Spanish Household Budget Survey (Encuesta de Presupuestos Familiares (EPF)). I divide households' fuel consumption  $Q$  (in liters) by their overall expenditure  $E$  –

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<sup>17</sup>Equivalently, it is likely that the first 'cost' on a car owner's mind when a tax is raised is the extra cost paid for all the gasoline that he/she will continue to purchase, rather than the utility lost from reducing purchases.

<sup>18</sup>Moreover, data limitations mean that implementation usually relies on expenditure of energy rather than consumption. fuel expenditure is only proportional to fuel *consumption* if prices are the same for all households, so the calculation relies on an unrealistic assumption of uniform pricing.



a smoother proxy for wealth than income (Poterba 1991) – and collapse these values into averages within each decile of overall expenditure. As is, these average values of  $\frac{Q}{E}$  can be interpreted as estimates of the government revenues generated by households per unit tax hike, as a proportion of their overall wealth.

I then replicate the calculation while relaxing the assumption of uniform pass-through. This, of course, requires estimates of pass-through corresponding to wealth, of the form

$$\tau = \alpha + \beta Q_E + \varepsilon \quad (1.7)$$

where  $\tau$  is pass-through and  $Q_E$  is a quantile (decile) of household expenditure. I do not jointly observe  $(\tau, Q_E)$ . Instead, I observe  $(\tau, Q_{HP})$ , where  $Q_{HP}$  is the average house-price decile. The two proxies for wealth are related as follows:

$$Q_E = a + bQ_{HP} + e \quad (1.8)$$

I estimate pass-through as a function of house prices rather than expenditure, which is equivalent to substitution of Equation 1.8 into Equation 1.7. This yields

$$\tau = \alpha + a\beta + \beta bQ_{HP} + \varepsilon + \beta e \quad (1.9)$$

The coefficient on  $Q_{HP}$  underestimate the magnitude of the the rise in pass-through with wealth to the extent that  $b < 1$ , as would occur due to measurement error.

However,  $Q_{HP}$  is unlikely to be a valid instrument for  $Q_E$ , because house prices are additionally correlated with pass-through for unobserved reasons that have little do with income. For instance, some poorer people live in richer neighborhoods, and vice versa. The extent to which poorer individuals are forced to buy automotive fuel in richer areas is likely mitigated to some degree by sorting: some consumers like to price shop, and applications like Gas Buddy in the U.S. and Spain's own *Geoportal* target precisely those consumers. Moreover, demand estimation in the industrial organization literature nearly always finds a lower disutility of price among richer individuals (again, see Houde 2012 for an example).

Still,  $\beta b$  may be overestimated on net due to incomplete sorting.

I nonetheless proceed with the exercise, to illustrate how large variation in local pass-through rates can translate to welfare impacts. The regression analog of Equation 1.9 is below:

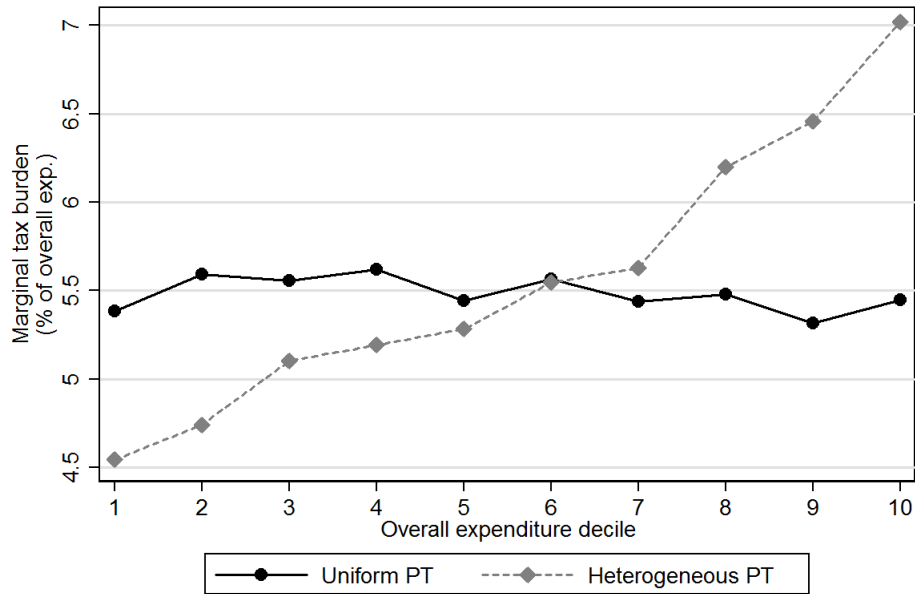
$$P_{it} = \alpha + \beta_1 Tax_{it} + \sum_{D=2}^{10} (\beta_D Tax_{it} * 1[HPDecile = D]_{it}) + \delta \mathbf{X}_{it} + \lambda_i + \sigma_t + \varepsilon_{it} \quad (1.10)$$

The coefficients  $\beta_1$  and  $\beta_D$  provide estimated pass-through rates corresponding to each decile of the house price distribution. These rates are then used to compute  $\frac{dp}{dE}Q$  at different expenditure deciles.

Figure 1.9 plots the proportional tax burdens with and without the pass-through adjustment. Interestingly, when pass-through is assumed full and uniform (solid line), households appear to have roughly equal fuel tax burdens as a proportion of their full budget (i.e., equal fuel-tax rates). Incidence is neither regressive nor progressive in this formulation of the exercise. This pattern runs counter to the belief that poorer households spend more of their budget on fuel than richer ones, which would yield a downward-sloping graph in Figure 1.9. Understanding the flat trend with respect to Spanish automotive fuel consumption is thus a subject for further research; however, the main point of Figure 1.9 is the effect of heterogeneous pass-through relative to this flat baseline. When pass-through heterogeneity is explicitly accounted for in analysis (dashed line), higher-expenditure households appear to have much higher effective fuel-tax rates. Incidence now looks strongly progressive.

While the magnitude of the pass-through effect on progressivity is large, it should not be surprising. Pass-through is inherently related to demand elasticity, so the pass-through/wealth relationship is inseparable from the demand elasticity/wealth relationship. Some have argued that richer people are more sensitive to fuel prices than poorer ones (Keyser 2000; Hughes, Knittel, Sperling 2008), because, for example, the rich have more “discretionary” uses of automotive fuel. A large body of research in the structural industrial organization literature, however, suggests that disutility of prices falls in income (e.g., Houde

**Figure 1.9:** *The Joint Distribution of Tax Burden and Wealth*



Notes: The y-axis measures estimated per-unit tax burdens as a percentage of overall household expenditure, averaged within deciles of that overall expenditure. The solid line plots this percentage unadjusted, which is parallel to the true distribution under an assumption of uniform, full pass-through. The dashed line plots this percentage adjusted by house-price-specific pass-through rates (estimated from Equation 1.10), which yields approximate burdens that reflect real variation in the price impacts of taxes.

Source: Expenditure data come from the 2013 Spanish Household Budget Survey; pass-through rates are the author’s calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

2012), which implies *less* price sensitivity among the rich. Furthermore, the effect of variable demand elasticities is the focal point of research by West (2004) and by West and Williams (2004); they estimate that demand for gasoline is more inelastic in richer areas. My findings are consistent with this result; a question for future research is, does pass-through rise in wealth for taxes on other goods, in other markets and countries?

## 1.7 Conclusion

In this paper, I have leveraged highly detailed data from the Spanish retail automotive fuel market to investigate the price impacts of energy taxes. My primary tool for this investigation has been pass-through – the degree to which taxes physically imposed on retail gas stations are passed through to final consumers. While there are dozens of published studies of energy tax pass-through, my research uniquely focuses on competition and local preferences as determinants of the pass-through parameter. I estimate station-specific pass-through rates, which I find to vary widely around a central tendency of 90-95%, from at least 70 to 115%. On the competition side, pass-through rises significantly when a station bears the brand of a vertically-integrated refiner, when it is spatially isolated, and when it shares an owner with other stations in its vicinity. On the demand side, pass-through rises steeply with municipal-average house prices, which are a good proxy for lifetime wealth.

These results have major implications for the distributional impacts of the diesel tax in question and energy taxes more generally. Because pass-through measures the extra cost that a consumer must pay for his or her automotive fuel, per unit tax, it has great power to describe the welfare impacts of taxation. Heterogeneous pass-through, unaccounted for, will always lead to mistaken estimates (and forecasts) of these welfare impacts at a local geographic level. Since pass-through is positively correlated with wealth in Spain, ignoring pass-through heterogeneity in this context will produce estimates that are not just mistaken, but also biased. I illustrate this by estimating marginal tax burdens by the average household in each decile of the wealth distribution, both with and without an assumption of uniform pass-through. What looks like a policy with roughly flat incidence across the

wealth distribution becomes a strongly progressive policy when my empirically estimated pass-through rates are factored into the calculation.

An accurate picture of the distributional impacts of energy taxes is important because of widespread reliance on these taxes across the world and the potential for even more. For instance, a recent drop in retail gasoline prices in the U.S. has precipitated calls for both a higher federal gas tax (Washington Post 2015a) and the establishment of a federal carbon tax (Washington Post 2015b). The realization of these policy recommendations hinges on their political feasibility, which is in part a function of distributional equity. Moreover, regardless of whether such policy changes are absolutely progressive or regressive, it is vital to know who bears what burden so that redistribution can accurately target those who are most adversely affected.

My analysis of pass-through has implications not just for distributional equity, but also for economic efficiency. The evidence strongly suggests that competition in such markets is highly imperfect. Thus, the traditional notion of optimal Pigouvian tax levels being equal to the social marginal cost of the relevant externalities no longer holds. The fact of market power in markets for retail automotive fuel implies that prices are already above private marginal costs, so the optimal Pigouvian tax is now lower than the social marginal cost of consumption.

Pass-through thus has great potential as a tool of economic analysis. While a full estimation of demand and supply curves would obviate the need to focus on pass-through, the data and computational challenges of such estimation make reduced-form pass-through analysis a worthwhile endeavor. Its accurate estimation, especially at a local level, facilitates a greater understanding of optimality, distributional equity, and the way in which firms compete.

## Chapter 2

# Pass-Through and Border Competition: Industry-Wide Costs vs. Firm-Specific Costs

### 2.1 Introduction

Cost pass-through – the degree to which cost shocks are “passed through” from one segment of a market to another – has long been a valued tool of economic analysis (Jenkin 1871-72; Weyl and Fabinger 2013). However, the empirical economics literature on pass-through has largely focused on identifying the price impacts of market-wide cost changes, which are the same for for all firms involved (Office of Fair Trading, UK Government 2014). In reality, many cost shocks are of non-uniform magnitude across an entire market. Changes in the cost of labor, the efficiency of capital, or the negotiated price of an input can easily be firm-specific, but so can seemingly uniform policies like taxes. An input tax may, for example, have heterogeneous effects on marginal costs if each firm uses a different set of inputs; consider a uniform dollars-per-ton carbon tax on firms with differing energy sources. Alternatively, a tax with incomplete coverage – whether due to lobbying, administrative borders, or a size threshold for eligibility – will exempt some firms from the cost change.

In both of these cases, what looks like an “industry-wide” cost shock is actually a series of heterogeneous, or “firm-specific”, cost shocks. In this paper, I study an example of the latter case: I estimate pass-through of state-specific diesel taxes among retail gas stations situated along state borders. I use data from Spain, whose government collects daily retail price data from all 10,000 of its gas stations, and whose diesel tax has a state-specific component that discretely rises fourteen times between 2010 and 2013. When one state raises its diesel tax, stations just inside the border experience an own-cost shock, while stations just outside the border experience a rival-cost shock. This allows for identification of own- and rival-cost pass-through rates, as opposed to the usual aggregation of these two which occur as a result of an industry-wide cost shock.

Firm-specific pass-through rates provide valuable information about welfare impacts and market function. Pass-through rates are frequently used to measure the relative incidence of a policy change on producers versus consumers (e.g., Miller, Osborne, and Sheu 2015); firm-specific rates can thus be used to estimate how this producer/consumer surplus breakdown changes when cost shocks are not uniform. Moreover, pass-through provides evidence on market structure. The full pass-through matrix, which consists of own- and rival-cost pass-through rates, can be used to estimate the price impacts of a merger (Jaffe and Weyl 2012). More generally, the presence and magnitude of rival-cost pass-through measures a firm’s incentive to raise its rivals costs, such as by lobbying for tax changes whose physical incidence will be relatively higher for rivals.

I estimate own- and rival-cost pass-through using event study and fixed effects regression, leveraging the quasi-random pattern of state-specific tax hikes in Spain. I first focus on the spread between competing gas stations on either side of a state border. I find strong evidence that this spread changes in the aftermath of a tax hike. I also find that the spread change is related to the toughness of competition, decreasing in the number of cross-border rivals. Next, I regress retail prices on taxes (and fixed effects and controls) while varying the observation sample. The full national sample, as well as the sample of all stations with at least one rival within five minutes, is characterized by a 93% point estimate on pass-through;

that is, every additional cent per liter (c/L) of diesel taxation is associated with a 0.93 c/L rise in retail prices. Meanwhile, average pass-through in the sample of 31 gas stations with at least one cross-border rival is only 57%.

I then use the full sample to examine the relationship between pass-through and cross-border rivalry. I interact both own-state and rival-state tax levels with different measures of this rivalry: a dummy for at least one cross-border rival; an absolute count of such rivals; and a count that is inverse-weighted by driving distance from rivals. All of these parameterizations yield significant statistical relationships. Each additional cross-border rival is associated with a 19 percentage-point drop in own-cost pass-through and a 13 percentage-point rise in rival-cost pass-through, both of which are significant at the 2% level or below. The distance-weighted measures of cross-border rivalry are even stronger and suggest that the impact of an “unaffected” rival rises faster than linearly in its proximity to the affected station. In the extreme – i.e., the maximum weighted count of cross-border rivals observed in my sample – the estimated coefficients imply that own-cost pass-through would be 59 percentage-points higher if the cross-border rivals were one minute further away.

My estimates contribute to a limited existing literature with mixed evidence on own-cost pass-through (e.g., Doyle and Samphantharak 2008) and no direct evidence on rival-cost pass-through. Together, the results strongly suggest that automotive fuel retailers in Spain are very much restricted in their ability to pass through their own cost shocks when those shocks are not shared by competitors. At the same time, competitors actually raise their prices as well, which suggests that they are re-optimizing in the face of greater demand for their fuel.

The rest of this paper is laid out as follows: Section 2.2 describes the intuition for and existing research on firm-specific pass-through; Section 3.4 describes the empirical context and methods; Section 3.5 presents results of cross-border spread analysis as well as own- and rival-cost pass-through estimation; Section 2.5 discusses the implications of these results and concludes.



## 2.2 Firm-Specific vs. Industry-Wide Pass-Through

### 2.2.1 The pass-through matrix

Pass-through is usually discussed and measured as a response to industry-wide cost shocks. That is, firm  $i$  changes price  $p_i$  as a function of industry-wide cost  $c$ , defined as  $\frac{dp_i}{dc}$ . However, this industry-cost pass-through is really an aggregation of responses to firm-specific cost changes,  $\frac{dp_i}{dc_j}$ , where  $j$  indexes all firms within a given market. To see this, consider the derivation of the pass-through matrix in a model of Bertrand single-product competition<sup>1</sup>, most recently attributable to Jaffe and Weyl (2012) and Miller, Remer, and Sheu (2013). Each firm  $i$  sells its product according to a twice-differentiable residual demand curve and firm-specific, constant marginal cost  $c_i$ , taking as given all other firms' prices. A firm's profit function is equal to

$$\pi_i(\mathbf{p}) = q_i(\mathbf{p})[p_i - c_i]$$

where  $\mathbf{p}$  is a vector of all  $J$  prices in the market. The first-order condition (FOC) for firm  $i$ , which sets  $\frac{\partial \pi_i(\mathbf{p})}{\partial p_i} = 0$ , can be written as

$$f_i(\mathbf{p}) = \frac{1}{\frac{\partial q_i(\mathbf{p})}{\partial p_i}} q_i(\mathbf{p}) + [p_i - c_i] = 0$$

After a tax change  $\mathbf{t}$ , which is a vector of length  $J$ , the set of FOCs can be expressed in matrix notation with

$$\mathbf{f}(\mathbf{p}) + \mathbf{t} = 0$$

By the implicit function theorem, total differentiation with respect to  $\mathbf{t}$  yields

$$\frac{\partial \mathbf{f}(\mathbf{p})}{\partial \mathbf{p}} \frac{d\mathbf{p}}{d\mathbf{t}} = -\mathbf{I}$$

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<sup>1</sup>Single-product competition simplifies the exposition but yields qualitatively similar results to the multi-product case.

Thus, the pass-through matrix is equal to the opposite inverse of the Jacobian of FOCs,

$$\frac{d\mathbf{p}}{d\mathbf{t}} = - \left[ \frac{\partial \mathbf{f}(\mathbf{p})}{\partial \mathbf{p}} \right]^{-1}$$

and is entirely a function of first and second own- and cross-price derivatives of quantity demanded. It is this fact that makes pass-through valuable for understanding the shape of demand, potential price impacts of mergers, and economic primitives in general.

The above matrix has dimensions  $N \times N$ . Bertrand duopoly, for example, would therefore produce a  $2 \times 2$  matrix with the following elements:

$$\frac{d\mathbf{p}}{d\mathbf{t}} = \begin{bmatrix} \frac{dp_1}{dt_1} & \frac{dp_1}{dt_2} \\ \frac{dp_2}{dt_1} & \frac{dp_2}{dt_2} \end{bmatrix}$$

If  $t_1 = t_2$ , so that the tax change is a industry-wide cost shock, then one cannot recover the individual elements of this matrix. What is instead estimated in empirical work is the sum across rows of this matrix – the aggregate price response of firm  $i$  to all firms' marginal cost shocks.

### 2.2.2 Evidence from industry-wide cost shocks

Industry-wide cost pass-through has been estimated for dozens of products. Besley and Rosen (1999), for example, identify pass-through of sales taxes for each of twelve different commodities and find wildly divergent rates across commodities – from nearly negligible in the case of McDonald's cheeseburgers to 242% in the case of bread<sup>2</sup>. Within energy markets, pass-through has been found to be well above 50% across a variety of different cost types, from prices of permits under the European Union Emissions Trading System (Fabra and Reguant 2014) and certificates under the U.S. Renewable Fuel Standard (Knittel, Meiselman, and Stock 2015), to crude and refined oil prices (Borenstein, Cameron, and Gilbert 1997), to sales and excise taxes on automotive fuel (Doyle and Samphantharak 2008; Marion and

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<sup>2</sup>Besley and Rosen (1999), among others, estimate pass-through *elasticities*, which are percentage changes in price per percentage change in costs. In this paper as well as most of those that I cite here, pass-through is estimated as the absolute change in price per absolute change in cost.

Muehlegger 2011). Automotive fuel tax pass-through, in particular, has been consistently estimated to be approximately 100% on average (Chouinard and Perloff 2004; Alm, Sennoga, and Skidmore 2009; Bello and Contín-Pilart 2012; Stolper 2016).

Most of the aforementioned studies utilize cost shocks that are either physically uniform across firms in a market (such as federal tax changes) or measured as an average across that market (such as benchmark crude oil prices). As such, estimates are interpretable as industry-cost pass-through rates. Some studies interact an industry-cost variable with firm- or area-specific measures, which allows for estimation of local responses to industry-wide changes. For example, Doyle and Samphanthark (2008) and Scharfstein and Sunderam (2014) interact cost measures with indicators of spatial and ownership concentration, respectively; Marion and Muehlegger (2011) interact state-level taxes with proxies for supply elasticity; and Stolper (2016) interacts taxes with proxies for wealth.

### **2.2.3 Evidence from firm-specific cost shocks**

The difficulty of obtaining comprehensive data on firm-specific costs makes estimates of own-cost pass-through rare in the literature. Ashenfelter et al. (1998) show empirically that, even when one has price and cost data for one specific firm (Staples, in their case), omission of other firms' costs (such as Office Depot's) can bias estimates of own-cost pass-through. This is because Staples' costs aggregate industry-wide costs and firm-specific ones, and the inclusion of rival Office Depot's costs controls for the common, industry-wide component. In their preferred regression, Ashenfelter et al. estimate Staples' own-cost pass-through rate to be approximately 15%.

Similarly, several studies of pass-through examine cost shocks that are neither industry-wide nor single-firm-specific. Miller, Osborne and Sheu (2015) estimate fuel cost pass-through by U.S. cement producers which rely variously on coal, petroleum coke, natural gas, and fuel oil for energy inputs. Ganapati, Shapiro, and Walker (2016) estimate the pass-through of energy input costs in six different U.S. manufacturing industries, using variation in coal and electricity prices that affect multiple (but not all) firms in a market

simultaneously. Both of these studies provide aggregate pass-through measures which likely fall somewhere in between firm-specific and industry-wide. Relatedly, Atkin and Donaldson (2015) measure the pass-through of origin prices to destination prices among intranationally traded goods. While the price of a given good at its port of arrival is, naturally, firm-specific, it may be correlated with omitted prices of substitutes and thus is not interpretable as an own-cost shock in pass-through analysis.

Leveraging administrative borders can be a simple, yet powerful, way to identify the effects of own-cost shocks. For instance, researchers have identified cross-border shopping behavior in response to heightened in-state cigarette prices (Chiou and Muehlegger 2008), higher in-state lottery prices (Knight and Schiff 2013), and more stringent in-state gun laws (Knight 2013). With respect to price impacts (as opposed to the quantity impacts just described), there are at least four relevant cross-border studies, and they provide very mixed evidence on own-cost pass-through. In each of these studies, the identification strategy is to compare price changes at different locations relative to a state border, where one tax regime ends and another begins.

Hanson and Sullivan (2009) and Harding, Leibtag, and Lovenheim (2012) both study cigarette tax changes. The former finds that stores near one state border pass on significantly less of a tax hike while stores near a second state border pass on significantly more. The latter, meanwhile, finds a strong increasing trend in pass-through with distance from a neighboring state with a lower tax rate. Bergman and Hansen (2013) focus instead on Danish national beverage taxes but are unable to discern any relationship between pass-through and distance to neighboring Germany. Finally, and most relevantly, Doyle and Samphantharak (2008) study the response of gasoline prices to a repeal and subsequent reinstatements of sales taxes in Wisconsin and Indiana. The repeal is associated with a *larger* drop in prices at gas stations nearer to borders with control states, but the reinstatements are associated with *smaller* rises. There is thus some evidence that own-cost pass-through is smaller than industry-cost pass-through, due to the competition provided by nearby substitutes not subject to cost changes. However, the evidence is far from consistent. Furthermore, there

are, to date, no existing estimates of *rival*-cost pass-through, which is no less important a component of the pass-through matrix.

## 2.3 Empirical Context

The Spanish retail market for automotive fuel is a convenient place to study firm-specific cost shocks for two main reasons: first, a government informational mandate has produced high-resolution data on pricing and market structure over time; and second, applicable taxes vary both across states and over time. Since the start of 2007, every gas station in the country has had to submit its retail fuel prices to the Ministry of Energy whenever they change, and weekly at a minimum<sup>3</sup>. Over the length of my sample – January 2007 to June 2013 – 9,277 mainland-Spanish gas stations appear in the data. I observe prices of retail diesel, brand, wholesale contract type, amenities, and geographic coordinates of every one of these stations<sup>4</sup>. Into these data, I merge information on excise taxes, applicable to retail diesel and with statutory incidence on the gas stations.

### 2.3.1 Cross-border markets in Spain

Figure 2.1 maps the full sample of mainland Spanish gas stations, while highlighting those stations that are within five kilometers of a state border. 459 stations satisfy the latter criterion; I highlight them (in red) because they represent one of the sub-samples that I use in my analysis. Table 2.1 lists summary statistics for both the full and 5-km samples, in addition to two others.

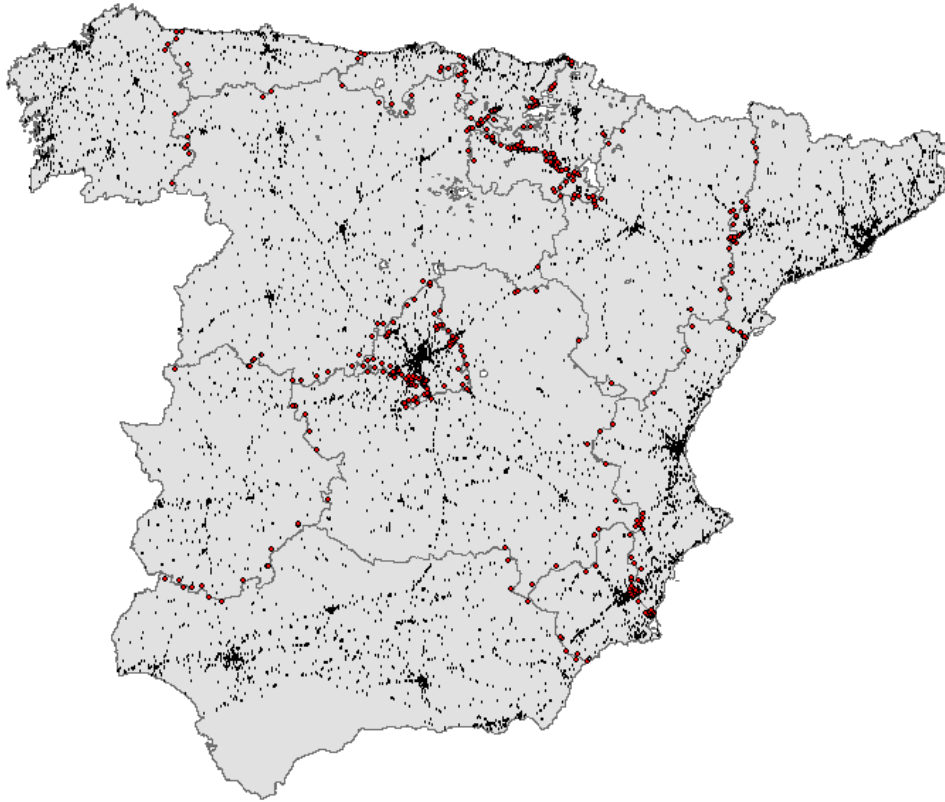
Column 1 of Table 2.1 shows the average station has a mean after-tax retail price of 98.37

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<sup>3</sup>This policy is motivated by consumer welfare interests. Spain's oil market is highly concentrated and subject to frequent allegations of collusion. See El País (2015) for an example of government investigation on this subject, or Contín-Pilart, Correljé, and Palacios (2009) and Perdiguero and Borrell (2007) for further background on the Spanish oil market. Stolper (2016) describes the data, especially with respect to brands and contracts, in greater detail.

<sup>4</sup>The non-price characteristics, however, are only available cross-sectionally, from the time of entry into the sample.

**Figure 2.1:** *Gas Stations on the Spanish Mainland*



Notes: All dots are Spanish retail gasoline stations. Large red dots indicate the 459 stations that lay within five kilometers of a state border; small black dots denote the remaining 8,809 stations that comprise the full sample.

Source: Author's calculation, using data from the Ministries of Industry, Energy and Tourism

**Table 2.1:** *Summary of Selected Station Samples*

	Full	Rivals	Border	CB Rivals
# stations	9,277	6,753	459	31
Avg. retail price (c/L)	98.37	98.44	97.79	98.27
P(Refiner branded)	0.60	0.61	0.61	0.52
P(Unbranded)	0.26	0.25	0.26	0.26
Avg. # of rival stations	2.13	2.99	1.13	3.13
Avg. # of cross-border rival stations	0.00	0.01	0.09	1.29
Distance to nearest state border (km)	63.03	66.36	2.57	1.14

Notes: “Rivals” are other stations within 5 minutes’ drive of the reference station. The ‘Full’ sample includes all mainland stations with non-missing price and tax data. ‘Rivals’ restricts to all stations with >0 stations within 5 minutes’ drive. ‘Border’ restricts to all stations within 5 km of a state border. ‘CB Rivals’ restricts to all stations with >0 stations within 5 minutes’ drive and situated in a different state.

Eurocents/liter (c/L) over the seven-year sample time period.<sup>5</sup> 60% of stations bear the brand of one of the three oligopolistic oil refiners in Spain, 26% of stations are unbranded independents, and the remaining 14% bear the brand of a retail chain with no refining capacity. The average station has a bit more than two other stations within five minutes’ drive and is 63 km from a state border.

I am primarily interested in estimating tax pass-through at stations “treated” by competition from out of state. In much of my analysis, I compare such stations to the remainder of the full sample. However, restrictions to the full sample may be useful if one is concerned about the adequacy of the control group in the full sample. Columns 2 and 3 of Table 2.1 thus display the analogous summary stats after two such restrictions are made. In column 2, the sample is all stations with at least one rival within five minutes’ drive<sup>6</sup>, while in column 3, it is the 459 stations within five kilometers of a state border.

All three of these samples are to be compared to the sample summarized in column 4: stations with at least one cross-border rival. Here, again, I define a rival to be any

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<sup>5</sup>Sales tax, however, is removed from these prices, to avoid the multiplicative effect sales taxes have on other taxes.

<sup>6</sup>I choose five minutes largely because Perdiguero and Borrell (2012) estimate 5-6 minutes as the relevant extent of spatial competition in Spanish retail automotive fuel markets. However, I test the robustness of my results to smaller “market sizes” in Table 2.6.

other station within five minutes' drive. This subset of 31 stations can be thought of as the treatment group. Notably, a few things are different about this treatment group, relative to other samples. First, there are fewer refiner-branded stations – 52% in the treatment group as compared to 60-61% in the broader samples. Second, there are *more* nearby rivals – 3.13 in column 4 as compared to 2.13, 2.99, and 1.13 in columns 1-3, respectively. Thus, branding and spatial competition appear to differ among stations with cross-border rivals, relative to the stations included in the broader samples.<sup>7</sup> One of the primary challenges to estimating the effect of cross-border competition on pass-through is controlling for these characteristics and other potentially omitted variables correlated with proximity to a state border.

Table 2.2 gives exclusive focus to the treatment group, by examining each of the local areas in which cross-border rivals are within five minutes' drive of each other. There are twelve such areas; they exhibit variation in the number of tax changes experienced, the number of stations on each side of the border<sup>8</sup>, and their brand and spatial concentrations. For instance, market #1 straddles the states of Murcia and Valencia. There are six stations total in this border market – four in the former and two in the latter. On average, these stations are 3.35 minutes away from their nearest rivals. Finally, these six stations are owned by six different firms; there is no brand concentration in market #1. Figure 2.2 depicts this market geographically, showing the four stations closest to the border.

The most obvious source of variation within the twelve border markets, according to Table 2.2, is the number of rivals on either side of the border. In contrast, there is very little variation in brand concentration or average drive time across markets. Only one market – #4, with four Repsol-owned stations – has any brand concentration whatsoever. The average drive time ranges from 3.35 to 4.92 minutes – though individual stations can be as close as 44 seconds to a nearby cross-border rival. In analysis, I explore the impact of each additional

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<sup>7</sup>One might theorize that the 5-km sample in column 3 is the best control group, because it holds distance to the border roughly constant while comparing stations *with* cross-border rivals to stations without them. However, column 3 shows that the former set tends to have almost three times more nearby rivals.

<sup>8</sup>Note that in Table 2.2, a station is counted as being within a border market if it is five minutes away from a rival in either direction (to *or* from); 41 stations satisfy this criteria. In contrast, a station is counted in column 4 of Table 2.1 only if it is five minutes' drive *to* a cross-border rival; 31 stations satisfy that criteria.



**Table 2.2: Characteristics of Border Markets**

Market	State 1	State 2	# Tax Changes	# Stations, S1	# Stations, S2	Avg. Drive Time (Min)	% Independent
1	Murcia	Valencia	3	4	2	3.35	100
2	Valencia	Castilla La Mancha	2	2	4	3.75	100
3	Castilla La Mancha	Madrid	1	1	1	3.68	100
4	Castilla La Mancha	Madrid	1	1	1	4.78	100
5	Castilla La Mancha	Madrid	1	5	2	3.5	38
6	Castilla La Mancha	Madrid	1	2	1	4.39	100
7	Castilla La Mancha	Madrid	1	2	2	4.58	100
8	Catalunya	Aragon	1	1	1	4.88	100
9	Navarra	La Rioja	1	1	1	3.92	100
10	Castilla y Leon	Pais Vasco	1	2	1	4.47	100
11	Navarra	Pais Vasco	1	1	1	4.82	100
12	Navarra	Pais Vasco	1	1	1	4.92	100

Notes: A station is included in a market if there is at least one other station that is less than 5 minutes away in one direction. "Avg. Drive Time" is the drive time between each station and its nearest cross-border rival, averaged across stations in a market. "% Independent" divides the number of *unique* firms in the market by the total number of firms in the market.



cross-border rival and corresponding driving distance on tax pass-through.

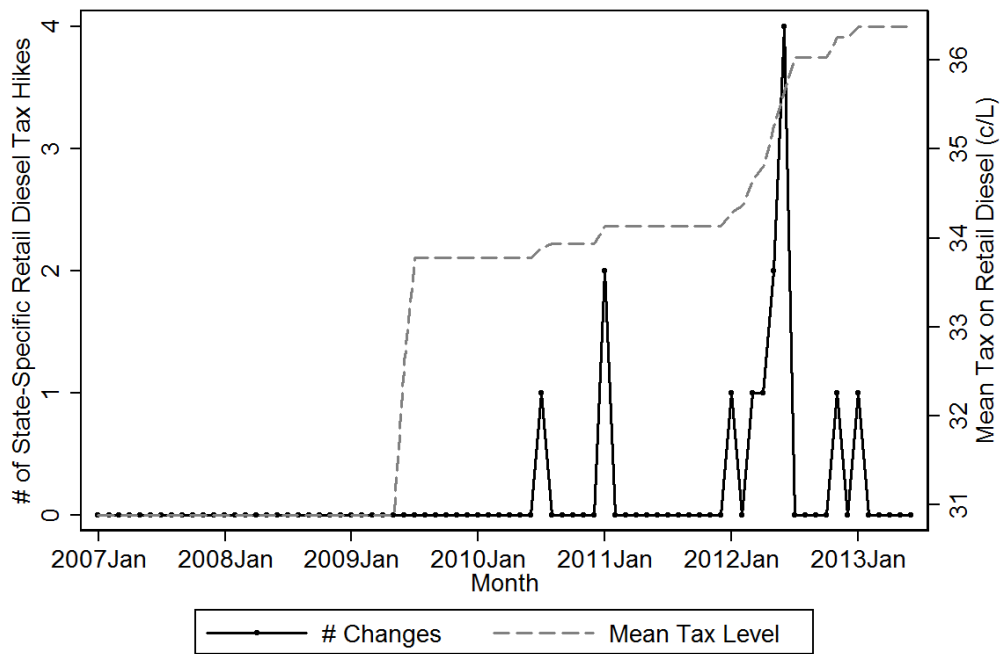
### **2.3.2 Diesel tax variation in Spain**

There are three taxes applicable to retail diesel in Spain: the national sales tax; the national excise tax on diesel; and the 'centimo sanitario' ("public health" tax), another per-unit tax which has a national and a state-specific component and has a stated purpose of generating revenues to be used for public health improvements. The state-specific component, which is what I use in all analyses, varies from 0 to 4.8 Eurocents/liter (or about 0-5% of average retail prices, net of sales tax) across states and discretely rises 14 times over my seven-year sample time period. This variation is plotted in Figure 2.3. While my data begin in January 2007, no state increases its diesel tax until early 2010. From that month forward, anywhere between 0 and 4 states raise their own tax levels in a given month. Meanwhile, the national excise tax jumps once, from 30.2 c/L to 33.1 c/L, in June 2009. In total, the mean per-unit tax on diesel rises from just under 31 c/L at the start of my sample time period to above 37 c/L at the end.

## **2.4 Estimating Pass-Through at State Borders**

My empirical analysis has two primary components. The first is an analysis of cross-border spreads – i.e., the difference between prices on one side of a border versus the other, and how that difference changes when one side experiences a tax hike. The primary advantage of this methodology is that it controls for all period-specific determinants of prices that affect both sides of the market equally. Thus, graphical inspection of cross-border spreads can be used to test whether a tax imposed on only part of a market is passed through heterogeneously by the two sides of a market. However, the magnitudes of own- and rival-cost pass-through cannot be disentangled using spreads – only their aggregate effect. For this reason, I conduct a second analysis of tax pass-through at borders using a difference-in-differences framework, which allows me to separately identify, in the same single regression, the response of firms on the tax-hike side of the border as well as the response of firms on the non-tax-hike side.

**Figure 2.3: Tax Variation**



Note: The solid line plots state-specific tax hikes. The dashed line plots the national mean tax level; it rises discretely in June 2009 because the *national* component of the diesel tax rises in that month.

Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism

### 2.4.1 Spread analysis

Consider a State 1 that faces a tax hike, and a neighboring State 2 that does not. A station situated in State 1 but close enough to be competing with a station in State 2 experiences an own-cost shock, and passes it through to retail price  $p_1$  at some rate  $x$ . The station in State 2 experiences a rival-cost shock, and passes it through to price  $p_2$  at some rate  $y$ . If we measure their cross-border spread  $p_1 - p_2$  before and after State 1's tax hike, and the spread does not change, this suggests that  $x = y$ , i.e., own-cost pass-through is equal to rival-cost pass-through. If, on the other hand, the cross-border spread changes as much as would be expected away from the border – where the tax hike would be an industry-wide cost shock – that would suggest that own-cost pass-through is no different from industry-cost pass-through, and that rival-cost pass-through is zero.

To examine the empirical analog of the above scenario, I first trim the full sample to include only the twelve border markets listed in Table 2.2. In that table, “State 1” always refers to the state that first sees a tax hike; I define State 1 similarly for spread analysis. I then calculate, for each market and month, the mean price on each side of the market, and subtract the State-2 mean from the State-1 mean. These are my cross-border spreads, and I graph them in the vicinity of tax hikes for each market in Figure 2.4.

The results provide striking evidence of a change in spreads in direct response to tax hikes. Every market can be described as having a noticeable jump (or drop) in the spread right around the month of a tax hike. Of course, there is underlying movement in every one of these spreads, and the jump in the spread does not always occur in precisely the same month as the tax hike. Nonetheless, this raw evidence strongly suggests that the two sides of a border market do not respond equally. Figure 2.5 provides a cleaner picture by graphing the *average* spread across the twelve border markets and within relative month. One month before a tax hike, the cross-border spread averages very nearly zero. But as soon as the tax rises (i.e., in month 0), the spread jumps to nearly 1.5 c/L. There is some movement in the average spread after month 0, but the spread remains above 1 throughout the ensuing six-month period.

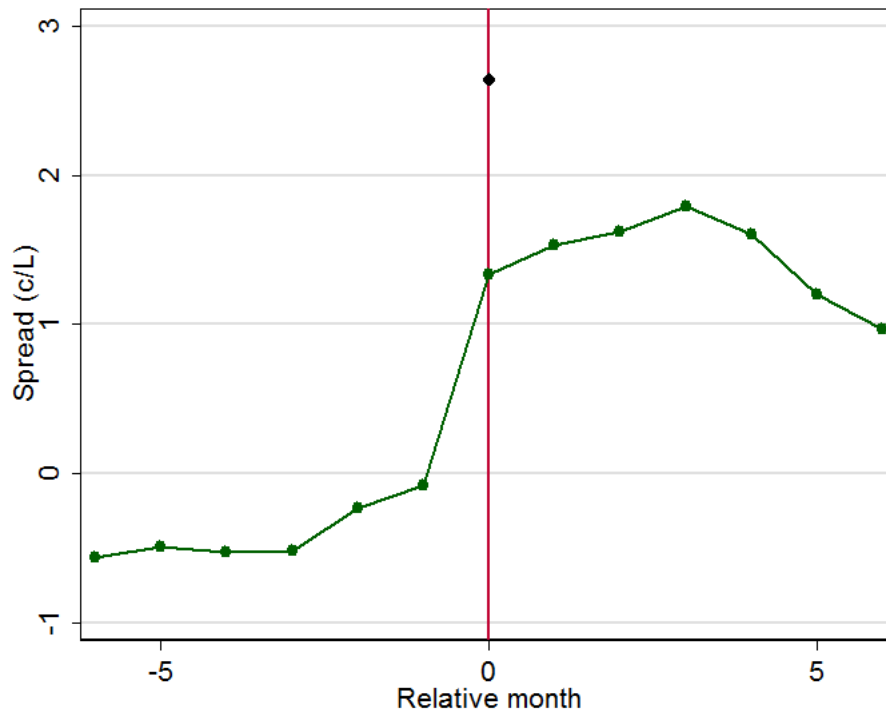
**Figure 2.4:** Individual Time Series of Cross-Border Spreads around Tax Hikes



Notes: The figure displays cross-border spreads over time in each of the twelve border markets which experience at least one tax change. The cross-border spread is defined as the average price on 'Side 1' of the border minus the average price on 'Side 2'. In all cases, I set 'Side 1' to be the side that experiences the first tax hike. Red lines denote a tax hike on 'Side 1'; blue lines denote a tax hike on 'Side 2'.

Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

**Figure 2.5:** Average Cross-Border Spread around a Tax Hike



Note: avg. tax hike = 2.7 c/L

Notes: The figure displays the average cross-border spread as it changes over time in the vicinity of a tax hike. Mathematically, the figure plots averages across the 12 markets (and 15 tax changes) depicted in Figure 2.4, within each month relative to a tax hike. The red line at  $x=0$  denotes the month in which a tax changes. Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

The clear jump in spreads illustrated by Figures 2.4 and 2.5 strongly suggests that  $x \neq y$ , i.e., that own-cost pass-through differs from rival-cost pass-through. Furthermore, it is clear from the latter figure that if own-cost pass-through is not 0, it is also not fully 100%. Depending on how one measures average pre-tax-hike and post-tax-hike prices from Figure 2.5, the difference ranges from about 1-2.2 c/L. Even at the upper bound of that range, the spread does not change as much as the average tax hike, which is 2.7 c/L in the border-market sample.

If the average trend depicted in Figure 2.5 smooths over the noise inherent in the individual trends, it also obscures the fact that border markets with varying degrees of competition may not respond uniformly to a tax hike. Since the single most variable characteristic of these border markets is the number of stations present (see Table 2.2), I explore the relationship between changes in spread and number of cross-border rivals. This relationship is displayed in Figure 2.6. The x-axis indexes the number of cross-border rivals faced by the average station on the tax-hike side of a given market. The y-axis measures the change between the average pre-tax-hike spread and the average post-hike spread (“dSpread”), divided by the size of the tax hike (dT). A linear fit of these  $(x, y)$  pairs is overlaid to emphasise the main point: the spread changes less as cross-border competition intensifies.

## 2.4.2 Difference in differences

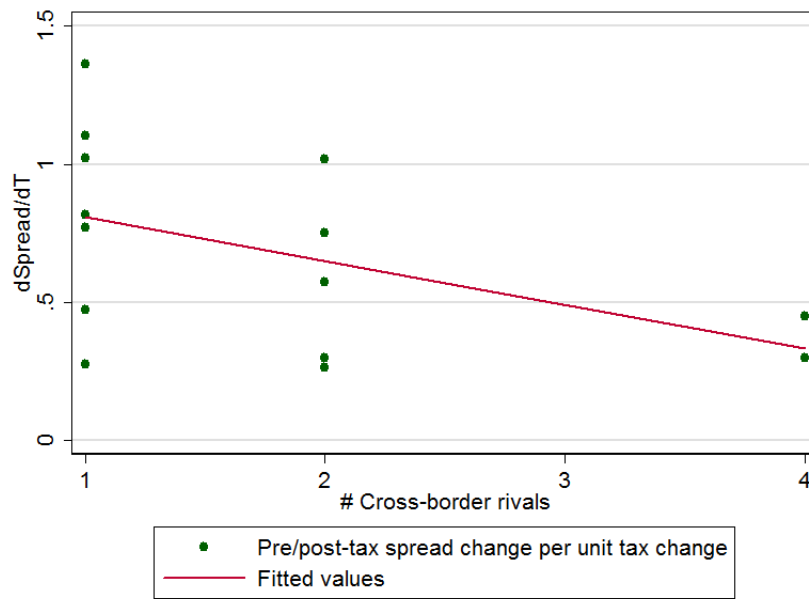
While the structural equation for pass-through in asymmetric oligopoly is not linear, one can imagine firm  $i$  responding to firm-specific costs in linear fashion (Miller, Osborne, and Sheu 2015):

$$P_{it} = \rho_{ii}C_{it} + \sum_{j \neq i} \rho_{ij}C_{jt} + X'_{it}\gamma + \lambda_i + \sigma_t + \varepsilon_{it}$$

In the above equation, there is a unique pass-through coefficient ( $\rho_{ij}$ ) corresponding to the cost of each firm competing with firm  $i$ . I do not observe the costs of every firm in every market; indeed, such data are extremely rare. Rather, I observe tax levels in each state,



**Figure 2.6:** *Change in Cross-Border Spread vs. # of Cross-Border Rivals*



Notes: The figure plots the change in cross-border spread in a market versus the number of cross-border rivals in that market. Mathematically, each point is the average cross-border spread over the time period [0,6] (where the number denotes the month relative to a tax hike) minus the average cross-border spread over the time period [-6,-1].

Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

which are own or rival costs depending on the location of a station with respect to state borders. I thus begin my regression analysis by estimating the following equation:

$$P_{it} = \alpha + \beta \text{OwnTax}_{it} + \gamma \text{RivalTax}_{it} + \delta \mathbf{X}_{it} + \lambda_i + \sigma_t + \varepsilon_{it} \quad (2.1)$$

Here,  $\text{OwnTax}_{it}$  measures the tax in station  $i$ 's state and in month  $t$ , while  $\text{RivalTax}_{it}$  measures the tax in the nearest neighboring state to station  $i$ , as measured by drive-time.  $\mathbf{X}_{it}$  is a vector of station and area characteristics; in regressions this will usually contain a count of all rivals (defined as stations under different ownership and within five minutes' drive), but a number of other controls are included in robustness checks.  $\lambda_i$  and  $\sigma_t$  are station and month fixed effects, respectively, which capture the impact of cross-sectional characteristics (like brand and contract) and national time-specific shocks (like the price of crude oil).

$\text{OwnTax}_{it}$  should have a strong correlation with price  $P_{it}$  because it is a very real cost to station  $i$ .  $\text{RivalTax}_{it}$ , however, should only predict price if (a) station  $i$  competes with another station situated in the state corresponding to  $\text{RivalTax}_{it}$ , and (b) rival-cost pass-through is truly non-zero. Thus, in the national sample, I do not expect this latter variable to be a significant predictor of prices; the average station in Spain is nowhere near a state border, and retail automotive fuel markets are, for the most part, highly localized. In the border sample, where every station is less than five minutes' drive from a cross-border rival, I expect the coefficient  $\gamma$  to be significant.

Identification of both  $\beta$  and  $\gamma$  may be confounded if the tax variables are correlated with omitted costs or demand properties. This may occur if, for example taxes are systematically lower in states with higher-cost supply of diesel, or if tax hikes are precipitated by downward trends in prices and/or demand. However, Stolper (2016) uses event study of Spain's state-level diesel tax hikes to show that price trends are, on average, very flat throughout the six months prior to (as well as after) a tax hike; the pass-through response appears to be about three weeks long, centered on the week of the hike itself.

Table 2.3 displays the results of estimation of Equation 2.1 using four different samples. Column 1's point estimates imply that pass-through of state-wide taxes is, on average, 93.1%

**Table 2.3:** Average Pass-Through of State Taxes Among Different Samples

	(1)	(2)	(3)	(4)
Own-State Tax Level (c/L)	0.931*** (0.039)	0.937*** (0.041)	0.729*** (0.049)	0.572*** (0.096)
Rival-State Tax Level (c/L)	0.081 (0.051)	0.092 (0.055)	0.065 (0.061)	0.211 (0.223)
Sample	Full	Rivals	Border	CB Rivals
N	581,452	416,774	30,393	2,200

Notes: Dependent variable is retail price (c/L). An observation is a station-month. The 'Full' sample includes all stations with non-missing price and tax data. 'Rivals' restricts to all stations with >0 stations within 5 minutes' drive. 'Border' restricts to all stations within 5 km of a state border. 'CB Rivals' restricts to all stations with >0 stations within 5 minutes' drive and situated in a different state. All specifications are estimated via OLS with station and month fixed effects. Standard errors, clustered at the state level, are in parentheses.

Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

in the full national sample. This estimate can be interpreted as a measure of industry-cost pass-through, since very nearly all 9,277 stations in this sample are not within five minutes of a border and therefore compete (roughly) exclusively with other stations facing the same tax levels. Column 1 also confirms that neighbor-state tax levels are not a significant predictor of one's own price among stations far from a border. The results in column 2, which restricts the sample to those stations with at least one rival within five minutes' drive, are very similar.

Column 3 shows results from the sample of stations within 5 km of a border. Here we see average pass-through drop significantly, to 72.9% as a point estimate. This could be driven by cross-border competition, or it could be driven by any other difference in the makeup of stations near state borders as compared to stations further away from them. Notably, stations near borders tend to be much more rural than the average station, and this could be associated with, for example, a different type of consumer (or supplier). Column 4 thus zeros in on the stations of primary interest: those within five minutes' drive of a cross-border rival. In this sample, pass-through is at its lowest yet: 57.2% on average. Since the 31 stations in this sample are very likely competing with out-of-state rivals,  $OwnTax_{it}$  is no longer an industry cost, and  $RivalTax_{it}$  is a real rival cost. The coefficient on this latter

variable is still not significant at conventional levels, but it is interesting to note that its magnitude is more than double the corresponding point estimates in columns 1-3.

Another way to estimate the degree to which pass-through of diesel taxes differs at borders is to compare stations “treated” with border competition with untreated control stations, conditional on tax levels, local characteristics, and fixed effects. Equation 2.2 captures this type of framework:

$$P_{it} = \alpha + \beta_1 OwnTax_{it} + \beta_2 OwnTax_{it} * CBR_{it} + \gamma_1 RivalTax_{it} + \gamma_2 RivalTax_{it} * CBR_{it} + \theta CBR_{it} + \delta X_{it} + \lambda_i + \sigma_t + \varepsilon_{it} \quad (2.2)$$

Here,  $CBR_{it}$  is a variable measuring cross-border rivalry at station  $i$  in month  $t$ . I experiment with three different parameterizations of this variable: (1) a dummy for having at least one cross-border rival (where ‘rival’ again indicates a station less than five minutes’ drive away); (2) a raw count of the number of cross-border rivals; and (3) a count of cross-border rivals weighted by inverse distance (1/minutes). These three options together provide a broad picture of the relationship between pass-through and cross-border competition. I include the level of  $CBR_{it}$  as well as its interaction with each of the two tax variables.  $X_{it}$  contains a count of all rivals again, as well as its interaction with  $OwnTax_{it}$ .<sup>9</sup>

In this formulation,  $\beta_1$  and  $\gamma_1$  represent average pass-through rates of one’s own and one’s neighboring-state taxes in the non-border sample at large. The former should be nearly 100% and the latter should be indistinguishable from zero, matching column 1 of Table 2.3.  $\beta_2$  and  $\gamma_2$  are the key explanatory variables, measuring the average difference in pass-through associated with cross-border rivalry. I expect  $\beta_2$  to be negative, to confirm that own-cost pass-through is less than industry-cost pass-through. I expect  $\gamma_2$  to be positive, if rival-cost pass-through is greater than zero.

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<sup>9</sup>When parameterizing  $CBR_{it}$  as a count, I define the rival count control as “the number of rival stations in the *same* state”; this makes  $\beta_2$  and  $\gamma_2$  interpretable as pass-through changes associated with an additional cross-border rival.

Columns 1-3 of Table 2.4 provide the results of using the full national sample and the each of the three parameterizations of  $CBR_{it}$ . Uniformly, pass-through of one's own state tax is about 94% and pass-through of one's neighbor's is not distinguishable from zero, for stations without cross-border rivals (CBRs). The first two coefficients match the results of Table 2.3, column 1 and again imply 94% industry-cost pass-through. Meanwhile, the bottom three coefficients speak to firm-specific pass-through, via CBRs. Column 1 implies that stations with at least one CBR pass through 27.7 fewer percentage points of their own cost shock, and 15.9 more percentage points of a rival's cost shock, relative to those with a CBR. These coefficients are significant at the 4% and 12% levels, respectively. Column 2 says that each additional CBR (controlling for the number of in-state rivals) is associated with 19.2 percentage points lower own-tax pass-through and 12.9 percentage points higher rival-tax pass-through, significant at the 1% and 2% levels, respectively.

Column 3's own- and rival-cost pass-through coefficients are interpreted differently because of their weighting; the impact of an additional cross-border rival is being modeled as non-linear in distance. The raw coefficients on own-tax and rival-tax are -0.754 and 0.554, respectively and are both significant at the 1% level. One way to interpret these numbers is to consider a station facing a single CBR, as Table 2.5 does. The impact of *moving* that CBR closer depends on how close it is to begin with. The change in pass-through of both own and rival taxes is in the low single-digit percentage points for a station five minutes away, but it rises faster than linearly as that drive time falls. Moving a station from two minutes away to one is associated with a 37.3 percentage-point drop in own-tax pass-through and a 27.7 percentage-point rise in rival-tax pass-through.

It is helpful to compare the impact of a cross-border rival with the impact of an in-state rival; this is why I tabulate the estimated coefficient on 'Own-state tax X Rival count'. In all columns, that coefficient differs from 'Own-state tax X CBR' by two orders of magnitude. In column 3, the coefficient is significant at the 8% level, suggesting that rivalry in general may matter. But it does not matter in any way relative to the degree that *cross-border* rivalry matters.

**Table 2.4: Pass-Through and Cross-Border Rivalry**

	(1) Dummy	(2) Count	(3) Weighted	(4) Dummy	(5) Count	(6) Weighted
Own-State Tax Level (c/L)	0.938*** (0.039)	0.941*** (0.040)	0.934*** (0.039)	0.754*** (0.051)	0.760*** (0.051)	0.767*** (0.048)
Rival-State Tax Level (c/L)	0.078 (0.050)	0.078 (0.050)	0.080 (0.051)	0.053 (0.058)	0.053 (0.060)	0.053 (0.059)
Own-State Tax X CBR	-0.277** (0.123)	-0.192*** (0.062)	-0.745*** (0.208)	-0.116 (0.174)	-0.106 (0.067)	-0.399** (0.186)
Rival-State Tax X CBR	0.159 (0.096)	-0.129** (0.049)	-0.554*** (0.187)	0.139 (0.083)	0.101* (0.050)	0.446** (0.199)
Own-State Tax X Rival count	-0.004 (0.005)	-0.007 (0.006)	-0.003* (0.002)	-0.005 (0.016)	0.024 (0.018)	-0.094*** (0.028)
Sample	Full	Full	Full	Border	Border	Border
N	581,452	581,452	581,452	26,264	26,264	26,264

Notes: Dependent variable is retail price (c/L). An observation is a station-month. "Rival-State Tax Level" is defined as the tax level in the nearest neighboring state. "CBR" refers to the cross-border rivalry variable; it is parameterized as a dummy (columns 1 and 4), an unweighted count (2 and 5), or a count weighted by inverse travel time (3 and 6). The 'Full' sample includes all stations with non-missing price and tax data. 'Border' restricts to all stations within 5 km of a state border. All specifications are estimated via OLS with station and month fixed effects. Standard errors, clustered at the state level, are in parentheses.  
Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

**Table 2.5:** *Non-Linear Impacts of Cross-Border Proximity*

Change in proximity of CBR	(1) $\Delta$ Own-Tax PT	(2) $\Delta$ Rival-Tax PT
5 minutes $\rightarrow$ 4 minutes	-3.7	+2.7
4 minutes $\rightarrow$ 3 minutes	-6.2	+4.6
3 minutes $\rightarrow$ 2 minutes	-12.4	+9.2
2 minutes $\rightarrow$ 1 minute	-37.3	+27.7

Notes: Numbers in columns 1 and 2 are the percentage-point changes in own- and rival-tax pass-through, respectively, associated with a single cross-border rival moving closer, as described under the column heading 'Change in proximity of CBR'. Changes are calculated from coefficients in column 3 of Table 2.4, as the predictive effect of changing the value of the CBR variable.

Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.

Columns 4-6 of Table 2.4 act as a first robustness check on the full-sample results, by restricting to the 5-km sample. The statistical significance of the firm-specific pass-through coefficients is not quite as consistent in these columns, but the qualitative result remains. Cross-border rivalry, especially when modeled on an intensive margin (as measured in columns 5 and 6), continues to predict significant changes in pass-through rates, and the magnitude of all estimated cross-border impacts dwarf the corresponding in-state impacts. Table 2.6 delves further into robustness checks by displaying the results of several other adjustments to the specification of Equation 2.2. I experiment with the inclusion of a more detailed set of control tax-interactions and state-year fixed effects in columns 1 and 2, and I use a stricter definition of spatial rivalry in columns 3 and 4. In all columns, cross-border rivals continue to significantly predict changes in own- and rival-tax pass-through rates.

## 2.5 Conclusion

Across a variety of graphical and regression analyses, cross-border rivalry consistently predicts deviations in a gas station's rates of pass-through from both zero and the >90% rate of industry-cost pass-through. The first evidence of this is that cross-border price spreads change significantly – but not one-for-one – with a tax hike on one side of the market; this

**Table 2.6: Robustness Checks on Own- and Rival-Cost Pass-Through**

	5-km (1)	5-km (2)	4-km (3)	3-km (4)
Own-State Tax Level (c/L)	0.855*** (0.037)	0.924*** (0.042)	0.939*** (0.040)	0.938*** (0.039)
Rival-State Tax Level (c/L)	0.081 (0.050)	0.032 (0.031)	0.078 (0.050)	0.078 (0.050)
Own-State Tax X CBR	-0.183** (0.057)	-0.190*** (0.058)	-0.260*** (0.068)	-0.362*** (0.074)
Rival-State Tax X CBR	0.125** (0.047)	0.140** (0.051)	0.256*** (0.095)	0.275*** (0.030)
Controls	X			
State-year FE		X		
N	581,396	581,452	581,452	581,452

Notes: Dependent variable is retail price (c/L). An observation is a station-month. All regressions use the full (mainland) national sample and parameterize the CBR variable as an absolute count. Column 1 includes interactions between the own-tax variable and: rival count; dummies for refiner and retailer brands; dummies for station amenities; and municipal population density. Column 2 includes state-year fixed effects. Columns 3 and 4 define rival stations (both in-state and cross-border) according to 4- and 3-minute driving radii, respectively. All specifications are estimated via OLS with station and month fixed effects. Standard errors, clustered at the state level, are in parentheses.

Source: Author's calculation using data from the Spanish Ministry of Industry, Energy, and Tourism.



is a clear sign that responses are different on each side of the border. The second piece of evidence is that average pass-through of in-state taxes is vastly reduced among stations within five minutes' drive of a cross-border rival: such stations pass-through only a bit more than half (57%) of a tax hike, as compared to nearly all (93%) at the average Spanish station. And the third piece of evidence is that interacting in-state and rival-state tax variables with measures of cross-border rivalry indicate robustly significant relationships. Own-cost pass-through rates are lower than industry-cost rates, and rival-cost pass-through is greater than zero.

Competition in this market, then, has a very real effect on incidence. While a tax affecting all competing firms equally is, on average, passed through nearly fully to retail consumers, a tax affecting only part of the market is borne in relatively greater proportion by the suppliers in that part of the market. Moreover, competing firms not affected by that tax actually *raise* their prices, so that patrons of those unaffected stations nonetheless share some of the tax burden.

In principle, the coefficients which I estimate here could be used to calibrate the full pass-through matrix, as a function of the number and distance of local rivals. This, in turn, could enhance the forecasting accuracy for a variety of policies – such as merger decisions and energy tax changes – on a variety of outcomes – including prices, quantities, and economic welfare. Furthermore, the pass-through patterns which I identify here have application beyond excise taxes. Consider one policy example that is quite relevant today and almost assuredly characterized by non-uniform cost shocks: the U.S. Environmental Protection Agency's Clean Power Plan (CPP). This regulation would impose limits on greenhouse gas emissions by electric power plants. It is expected that power plants would, to some degree, pass through the costs of compliance to consumers; indeed, existing research suggests that the pass-through rate would be, on average, quite high (Fabra and Reguant 2014). However, power plants use a variety of different energy sources to produce power, and each energy source has a different emissions profile. In addition, many types of power plants are likely to be exempt from the regulation. Thus, the cost shocks engendered by CPP emissions limits

would be highly non-uniform. In order to forecast the price, quantity, and welfare impacts of the CPP – especially with distributional impacts in mind – one requires an understanding of competition and firm-specific behavior. This is precisely what I have sought to capture in Spain’s retail automotive fuel markets.

## Chapter 3

# Environmental Regulation, Water Pollution, and Infant Mortality: Evidence from *Mehta vs. Union of India*<sup>1</sup>

### 3.1 Introduction

River pollution is a growing problem. In developing countries, as much as 70 percent of industrial waste and 80 percent of domestic waste is said to flow untreated into rivers (World Water Development Report 2012). Direct exposure to untreated water is blamed for a variety of health risks: infections, chronic illnesses, reproductive issues, and premature mortality for those living near water sources. *Indirect* exposure through contaminated food chains and groundwater imparts a health risk even at substantial distances from the site of pollution (World Health Organization 2008a and 2008b). In the case of rivers, pollution's impact can be particularly severe: polluted water generally flows downward to a continuum

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<sup>1</sup>Co-authored with Quy-Toan Do (The World Bank) and Shareen Joshi (Georgetown University)

of downstream communities, creating a trail of ecological degradation and toxicity (Oates 2001, Lipscomb and Mobarak 2007).

This paper examines these issues in the context of India, where the issue of water pollution is increasingly regarded as a crisis. According to the latest estimates, more than half of India's rivers and other surface water bodies are now significantly polluted (Central Pollution Control Board 2015, as reported in *The Daily Mail* April 15 2015). The issue is particularly salient for the largest and holiest river, the Ganga (or Ganges), which routinely hosts some of the largest bathing rituals in the world and has experienced a significant reduction in water flow as well as a rise in pollution levels over the past two decades. The current Prime Minister, Narendra Modi, made the cleaning of this river a major electoral promise when he campaigned from the riverside pilgrimage city of Varanasi. Within a month of being in office, the government announced "Namami Ganga" (Sanskrit for "Respect for the Ganga"), an Integrated Ganges Development Project that received funding of US \$334 million and promised a clean Ganga in three years. The Modi administration recently launched the Ganga River Basin Management Plan – 2015, featuring a comprehensive action plan for cleaning the river in the short term (three years), medium term (five years), and long term (ten years and beyond).<sup>2</sup> There is also strong international support for such initiatives. Since 2011, the World Bank has spent more than \$1 billion on the National Ganga River Basin Project to help the National Ganga River Basin Authority (NGRBA) build institutional capacity for cleaning the river.<sup>3</sup>

These are ambitious actions, but they are far from guaranteed to be effective in reducing the health burden of water pollution. While there is documented evidence on successful regulation of water quality, most of it comes from the developed-country context (e.g.,

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<sup>2</sup>The document has not been released to the public, but the content was widely reported across the Indian media in the week of May 9th, 2015. The report was prepared by a cluster of seven Indian Institutes of Technology (Bombay, Delhi, Madras, Kanpur, Kharagpur, Guwahati, and Roorkee), and the plan was released at a conference in early May from the Oval Observer Foundation. Some details of the documents recommendations are summarized on the foundation's website (<<http://ovalobserver.org/Event/economic-financial-instruments-restoration-ganga/>>), accessed on May 12th, 2015.

<sup>3</sup><<http://www.worldbank.org/en/news/feature/2015/03/23/india-the-national-ganga-river-basin-project>>, accessed on May 12th, 2015.

Alsan and Goldin 2015 and Cutler and Miller 2005). In developing countries – where environmental policies are often poorly designed, feature numerous loopholes, and are loosely enforced – there is very little evidence of policies other than piped water provision (Ravallion and Jalan 2003; Gamper-Rabindran, Khan, and Timmins 2010) being successful.<sup>4</sup> Greenstone and Hanna (2014), for example, find no statistically significant impact of India’s flagship river pollution control program – the National River Conservation Plan – on surface water quality in India.

Even when environmental policies are effective, it is often difficult to identify the specific mechanisms that are at work in generating the impacts. Much environmental regulation is motivated at least in part by the desire to improve public health, but when such improvements are achieved, is it because of better environmental quality, or because of a change in the behavior of the target population? Policy implementation often raises awareness of environmental problems, either explicitly through informational and educational programs or implicitly through the media and the myriad observable changes produced by the policy. Moreover, regulation raises costs for polluters, which can filter through to health via wage and employment impacts. Assessment of policy impacts typically measures the combined effect of environmental policy on health.

In this research, we directly target these outstanding questions about pollution policy. We study a unique and historically-important policy intervention: Supreme Court rulings sparked by pioneering environmental public-interest litigation in India. The lack of precedent for and quasi-random geographic incidence of these rulings – which targeted the highly-polluting tanning industry of Kanpur, India – facilitate a difference-in-difference analysis of policy impacts. We find that the Supreme Court verdicts produced a significant drop in both river pollution (as measured by Biochemical Oxygen Demand) and health risk (as measured by infant mortality). This provides evidence that bottom-up regulation

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<sup>4</sup>There is, however, a small but growing literature demonstrating impacts of experimental interventions in water quality. Kremer et al (2011) show reductions in diarrheal incidence associated with protection of springs in rural Kenya, and Duflo et al. (2013) show reductions in water pollution associated with randomized matching of water quality auditors to Indian firms.

with local (as opposed to national) geographic scope can produce desirable environmental and health outcomes. It further contributes to a growing body of research findings on environmental policy impacts in the developing world (such as those of Galiani et al. 2004, Almond et al. 2009, and Greenstone and Hanna 2014).

Armed with this finding, we then shift our focus to the mechanisms of policy impact. While the Supreme Court verdict unquestionably targeted river pollution and was motivated by evidence of health concerns, the link between the two may be driven by other factors. The salience of the ruling may have improved citizen information about both pollution and health and encouraged citizens to change behavior. This is particularly a possibility in the Ganga Basin, where informational campaigns were part of the policy response. Another potential channel is economic: there was significant concern about the economic impacts of regulation on an industry that was such a major source of employment and wealth in Kanpur.

To shed light on the relative importance of the pollution channel – as opposed to income, behavioral, and other channels – in policy impacts, we construct two instruments for river pollution. The first is *upstream* river pollution, which we argue is a valid instrument conditional on our controls and fixed effects. The second is the Kanpur policy itself, which we do not argue to be necessarily valid because of its simultaneous effect on pollution, wages, and information (among other possible determinants of health). We compare results of two separate instrumental variables (IV) regressions of infant mortality on river pollution: one in which we use only the upstream pollution instrument; and one in which we additionally use the Kanpur policy instrument. Our intuition is that the former regression captures the direct effect of river pollution on infant mortality; therefore, if the Kanpur policy's mortality impacts came predominantly through the pollution channel, then its inclusion as a second instrument should not alter our estimates of the pollution-mortality relationship.

We propose a basic model of health that motivates a direct comparison of the coefficients generated by these two IV regressions. Because we have two potential instruments and one endogenous regressor, the Sargan-Hansen test of overidentification restrictions provides a

statistical 'test of mechanisms'. We find high p-values for the Sargan-Hansen test statistic in most cases, which suggests that the Kanpur policy did indeed reduce infant mortality primarily by reducing river pollution. This result speaks directly to a common uncertainty about whether targeting pollution reduction is valuable when informational campaigns and incentives for avoidance behavior are viable alternatives.

Our estimate of the BOD-infant mortality dose-response function represents another data point in the literature on pollution and health in developing countries. In Bangladesh, Field, Glennerster, and Hussam (2013) show that switching to wells contaminated by domestic pollution has driven infant mortality upwards. In India, Brainerd and Menon (2014) show that agricultural water pollution is associated with increases in infant mortality. Our own work shows that industrial water pollution is another contributor to infant health risk. Furthermore, our work shows for the first time that upstream pollution affects not just downstream pollution (Sigman 2004; Lipscomb and Mobarak 2007) but also downstream health.

The remainder of this paper is organized as follows. Section 3.2 provides an overview of our study context, particularly with respect to river pollution and environmental legislation and regulation in India. Section 3.3 describes our model, including the key equations we seek to estimate. Section 3.4 describes the various sources of data. Section 3.5 provides empirical results from both reduced form and instrumental variable estimation strategies. Section 3.6 concludes.

## **3.2 Context**

### **3.2.1 Rivers and River Pollution**

More than a century's worth of epidemiology research has established a strong link between water pollution and human health. The seminal work of John Snow (1854) connected the Broad Street cholera outbreak in London to fecal bacteria leaking from the sewage system. Epidemiology research (see, e.g., Fewtrell and Bartram 2001) has subsequently advanced to

produce evidence linking water pollution to a host of pathogens (e.g., E. coli, rotavirus) and illnesses (cholera, diarrhea, etc.). Furthermore, drinking is not the only way that one's health can be adversely affected by water pollution; Cifuentes et al. (2000) identify irrigation to be a link between water pollution and health, while Carr (2001) highlights bathing, food, and person-to-person contact as modes of transmission of diseases from polluted water. More recently, the link between heightened river pollution and mortality has been documented in both China and India. Ebenstein (2010) finds that a one-grade deterioration in Chinese river water quality is associated with a 9.7 percent increase in the incidence of digestive cancer. Brainerd and Menon (2011), meanwhile, find that a 10-percent increase in agrochemical levels in Indian rivers during the month of conception is associated with an 11-percent increase in one-year mortality.

Along with human and agricultural waste, industrial pollution has been a major contributor to water quality degradation, thanks to economic growth and industrialization in some parts of the developing world. Small-scale factories, in such industries as textile dyeing, pulp and paper, pharmaceuticals, leather tanning, lead battery manufacture, and metal smelting, among others, tend to produce large amounts of waste that contain hazardous substances such as chromium, mercury, lead, and cyanide. When untreated, this waste pollutes rivers, streams, lakes, soil, and also groundwater resources (International Labor Organization (ILO) 2011). In Shanghai, for example, 3.4 million cubic metres per day of industrial and domestic waste pour into Suzhou creek and Huangpu river, which flow through the heart of the city. Because of serious pollution, the river and creek have essentially become devoid of life and are blamed for high rates of cancer as well as other chronic illnesses in the surrounding area (ILO 2011).

In India, the direct health risks of poor river water quality are compounded by both the cultural importance of rivers and the country's reliance on rivers not just for drinking water, but also for transportation and irrigation. Rivers have played a critical role in shaping India's economy, society, culture and religion for more than 5,000 years. Seven major rivers, along with their many tributaries, provide potable water, cheap transportation, agricultural



livelihoods, and spiritual anchors for India's population of 1.3 billion people. The most significant is the Ganga. Worshipped as a goddess by Hindus worldwide, it flows more than 2,200 kilometers through eight Indian states. Its basin holds 47 percent of India's irrigated land and feeds 500 million people (Hollick 2008; Government of India 2009).

In the aftermath of several decades of population and industrial growth, India's rivers are heavily polluted (United Nations 2013). The results of water quality monitoring carried out by India's Central Pollution Control Board (CPCB) using the indicator Biological Oxygen Demand (BOD) – which measures organic compounds (see Section 3.4 for further discussion) – suggest that water at approximately half of all sampling stations did not meet the agency's threshold of acceptability for bathing (BOD below 3 mg/L). The pollution challenge has also been growing over time: the number of polluted river stretches, which are again defined as not meeting the bathing-class standard for BOD, has doubled from 150 in 2009 to 302 currently (CPCB 2015, as reported in *Daily Mail* 2015). Only 160 out of nearly 8,000 towns have sewerage systems and treatment plants (CPCB 2013). As a consequence, exposure to contaminated water can be high in the basins of rivers. High levels of population density along the banks of the river, coupled with the ritual significance of bathing in the river, increases individuals' exposure to river water. Two million people are said to bathe in the Ganga river each day, and 60,000 in Varanasi alone (Hamner et al. 2013). Religious festivals frequently occur on the banks of rivers. At the Kumbh Mela, which rotates between Haridwar, Allahabad, Ujjain, and Nashik, more than 100 million people can bathe in a river within a single month (Iliyas et al. 2013). A growing number of studies document a relationship between pollution and health, particularly along the Ganga and its tributaries. Pandey et al. (2005) find that high concentrations of nitrate, chloride, and fecal coliforms in the city of Varanasi are associated with the prevalence of enteric diseases. Even in the case of treated water, improperly maintained pipes and seepage into the piped water system introduce contamination (Pandey et al. 2005). Several studies have attempted to estimate the various impacts of industrial pollution and sewage on human health, agriculture and livestock and other sectors of the economy (Shankar 2001, Dasgupta 2001, Reddy and Behera

2006).

### 3.2.2 Water Pollution Policies

The harms associated with water pollution, in India and the world over, have spawned a great many policies targeting water quality, water access, and sanitation. Researchers have, in turn, used many of these policies as natural experiments in environmental regulation. In recent years, program evaluation has linked various public health initiatives – such as water filtration and chlorination (Cutler and Miller 2005), piped water access (Gamper-Rabindran et al. 2010), spring protection (Kremer et al. 2011), deep-water tube wells (Field et al. 2011), privatization of water provision (Galiani et al. 2005), and sanitation projects (Watson 2006, and Spears 2013) – to infant health impacts.

In India, there is little evidence that water quality interventions have been successful. The most salient government effort to reduce river pollution is the National River Conservation Plan (NRCP), a national, top-down program targeting domestic pollution into India's surface waters. NRCP began in 1985 as the Ganga Action Plan but has expanded over thirty years to now cover 190 towns in 41 rivers across India. Its goal since 1987 has been to restore the Ganga River to the "Bathing Class" standard, as defined by India's "Designated Best Use" (DBU) classification system. The primary lever for achieving this goal has been the "interception, diversion, and treatment" of sewage (Government of India 2003); to that end, 4,704 million-liters per day of sewage treatment capacity have been created since its inception (Ministry of Environment and Forests (MoEF) 2013).<sup>5</sup> Despite all of this, the popular media has panned NRCP for reasons such as poor inter-agency cooperation, funding imbalances across sites, and an inability to keep pace with growing sewage loads (Suresh 2007). Confirming public opinion priors, Greenstone and Hanna (2014) find no discernible impact of NRCP on water quality levels.

The executive branch, however, is not the only source of environmental regulation in

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<sup>5</sup>Improvements to riverside bathing ghats, crematoria, toilets have also been a part of NRCP interventions (MoEF 2013).

India; the Indian judiciary has, through the years, developed a reputation for environmental activism. Article 21 of the Indian Constitution provides citizens with the “Right to Life”, and much jurisprudence has centered on the protection of this constitutional right. In the famous case *Subash Kumar vs. the State of Bihar*, the Supreme Court invoked the Water Act of 1974 together with Article 21 of the Constitution to rule in favor of the citizen who accused a major industrial plant of polluting the waters of the local river (Murlidhar 2006). In this judicial ruling, as well as the ruling on which we focus in this paper, the Supreme Court repeatedly stated that the Government of India has a responsibility to protect the environment.

### 3.2.3 *Mehta vs. Union of India*

Just as the Ganga Action Plan was ushering in ambitious interventions by the executive branch to reduce *domestic* river pollution, the groundwork was being laid for judicial-branch action to reduce *industrial* river pollution. The story of Supreme Court involvement in river pollution began in the pilgrimage city of Hardwar along the Ganga River; a matchstick tossed by a smoker resulted in the river catching on fire for more than 30 hours due to a toxic layer of chemicals produced by a pharmaceutical firm (Mehta 2001). In response to this event, environmental lawyer and social activist M.C. Mehta filed a writ petition in the Supreme Court of India charging that government authorities had not taken effective steps to prevent environmental pollution in the Ganga’s waters. The scale of the case, the whole 2,500-km stretch of the river, proved to be intractable. The court requested that Mr. Mehta narrow his focus; he chose the city of Kanpur.<sup>6</sup>

Kanpur is a city of 2.9 million people lying directly on the Ganga River in Uttar Pradesh state (see Figure 3.2). For more than 100 years, Kanpur has been a major center for India’s tannery industry. Most of the tanneries are located in the neighborhood of Jajmau, which lies on the southern bank of the Ganga River. Leather is a highly polluting industry; the processes of washing, liming, fleshing, tanning, splitting, and finishing involve a large

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<sup>6</sup>This is described in Writ Petition (Civil) No. 3737/1985 (Supreme Court of India 1985).

number of chemicals (Cheremisinoff 2001).<sup>7</sup> Tannery effluent is generally characterized by its strong color (reddish dull brown), high BOD, high pH, and high concentration of dissolved solids, as well as highly toxic chromium ions. In Jajmau, that effluent is routinely discharged from the tanneries directly into the river, rendering both river water and groundwater unfit for drinking, irrigation, and general consumption (Beg and Ali 2008, Tewari, Dubey, and Singh 2012). As the Indian economy grew during the 1980s, pollution in Kanpur increased significantly as a result of many factors: increased diversion of the Ganga's upstream waters to meet the growing demand for power; increased municipal waste in the city of Kanpur; and increased pollution from the cluster of leather tanneries as they kept up with rising demand. Though Kanpur is relatively unique in its concentration of tanneries, the story of growing economic activity, growing demand for electricity, and growing pollution is common to most cities in the Ganga Basin, as well as other rivers of India.

Mehta selected Kanpur despite not having been born in or lived in Kanpur. In interviews, he explained that “[Kanpur] was in the middle of the Ganga basin, the reddish color of the pollution made the pollution highly salient, and the city seemed representative of many other cities in the Ganga Basin” (Mehta 2014). In his petition, Mr. Mehta named eighty-nine respondents; among them were seventy-five tanneries in the Jajmau district, the Union of India, the Chair of the Central Pollution Control Board, the Chair of the Uttar Pradesh Pollution Control Board, and the Indian Standards Institute (Singh 1995). The petition also claimed that the Municipal Corporation of Kanpur was not fulfilling its responsibilities. The court subsequently bifurcated the petition into two parts. The first dealt with the tanneries of Kanpur and the second with the Municipal Corporation. These are now called *Mehta I* and *Mehta II* in legislative digests, together known as the “Ganga Pollution Cases” – the most significant water pollution litigation in the Indian court system. In October 1987, the Court invoked the Water Act and Environment (Protection) Act as well as Article 21 of the

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<sup>7</sup>One ton of hide generally leads to the production of 20 to 80 m<sup>3</sup> of turbid and foul-smelling wastewater, with chromium levels of 100-400 mg/l, sulfate levels of 200-800 mg/l, and high levels of fat and other solid wastes, as well as notable pathogen contamination. Pesticides are also often added for hide conservation during transport (Cheremisinoff 2001).

Indian Constitution to rule in Mr. Mehta's favor and order the tanneries of Jajmau to clean their wastewater within six months or shut down entirely. This was followed by a January 1988 judgment that required the Kanpur local municipal bodies to take several immediate measures to control water pollution: the relocation of 80,000 cattle housed in dairies or the safe removal of animal waste from these locations; the cleaning of the city's sewers; the building of larger sewer systems; the construction of public latrines; and an immediate ban on the disposal of corpses into the river. The court also required all schools to devote one hour each week to environmental education and awareness.

In the aftermath of the verdict, Kanpur became the focal point of efforts to clean the Ganga. The Ganga Action Plan had already made funds available to pollution prevention in this area. The government of Uttar Pradesh used the attention from the Supreme Court ruling to divert a great deal of funding to Kanpur (Alley 2002). One particular institution – the Uttar Pradesh Jal Nigam (i.e., Water Board) – received the lions share of that diverted money, particularly after the Supreme Court flagged the poor record of the local Kanpur municipalities in a second case in 1988. This is best seen in the case of the “Indo-Dutch Environmental and Sanitary Engineering Project Kanpur and Mirzapur”. This bilateral agreement between the Indian and Dutch government was launched in late 1987, under the umbrella of initiatives precipitated by the Ganga Action Plan (Government of India and Government of the Netherlands 1989). The project was fully funded by the Dutch government, but the execution was largely managed by the Government of India, with the Uttar Pradesh Jal Nigam playing a key role in local execution.<sup>8</sup> The project included many initiatives: improvements in infrastructure for sewerage and stormwater drainage; the building of handpumps; establishment of new systems for the treatment of tannery

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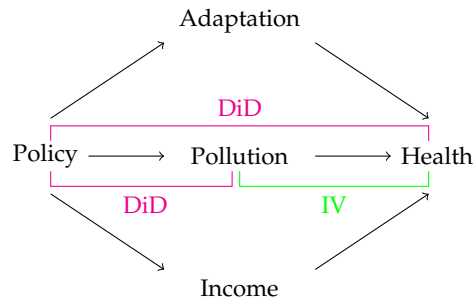
<sup>8</sup>In 1985, the Dutch government expressed an interest in collaborative projects with the Government of India on issues of the environment. In mid-1985, two Dutch experts traveled to India to explore these opportunities. At the request of the Government of India, they were requested to direct their attention to the Ganga Action Plan. They selected two towns – Jajmau and Mirzapur – for further consideration. After a fact-finding mission to both these towns in 1986, it was decided that the partnership would include several interventions. The project was officially launched in 1987 but no work was begun until early 1988 due to hurdles in finding suitable Dutch and Indian implementing partners (Government of India and Government of the Netherlands 1989). Project documents from this period mention the Supreme Court rulings as a reason to accelerate the pace of the programs and build the capacity for pollution control that could meet the Supreme Courts requirements.

waste water; the establishment of new systems for the collection and disposal of solid waste; public health education and community development.

The records from this era suggest that several changes occurred in the Jajmau area in the aftermath of the rulings. In 1987 and 1988, many “crash programs” were implemented to clean drains, expand the number of handpumps, and build latrines to improve sanitation systems in Jajmau (though precise metrics of success are hard to obtain from project documents). On February 16th, 1989, the largest tannery (Pioneer Tannery) was provided a technology which “recovered” chromium from the water. Project documents suggest that this was quite effective: it was “running continuously and recovering the chromium of six lots of 1,000 kg of hides per week” (Government of India and Government of Netherlands 1989). In 1989, construction began on a Common Effluent Treatment Plant (CETP), capable of treating 36 million liters of effluent per day. The plant was innovative in that it used a Dutch technology, the upflow anaerobic sludge blanket (UASB), to treat a mix of industrial and domestic wastewater. A new sewer line was constructed to carry the waste from the smaller tanneries of Jajmau to the CETP.

There is also evidence of greater vigilance and monitoring. Of the 87 tanneries named in Mr. Mehta’s writ, approximately 20 were shut down and more than 60 tanneries established primary treatment plants (PTPs) (Alley, 2002). Subsequent cases in the Supreme Court over the past 25 years, and indeed many academic researchers of pollution in Kanpur, have argued that these projects were a failure and the newly established technologies failed to be appropriately maintained or used (Alley 2002, Singh 2006, Greenstone and Hanna 2014). Weak capacity, institutional complexity, faulty program design, weak incentives to regulate, and political economic factors have all been cited. In our analysis, however, we shall illustrate that for at least the first five years after the verdict, the changes in Kanpur may have had some positive effects.

**Figure 3.1:** Schematic Diagram of Empirical Strategy



### 3.3 Modeling Policy, Pollution, and Health

We are interested in whether or not the 1987 Supreme Court decision affected environmental quality or health outcomes. As in the existing literature (Greenstone and Hanna 2014; Spears 2012), we observe the incidence of an environmental policy as well as some measures of target outcomes. We argue that the timing of the Supreme Court decision was exogenous with respect to pollution and health: public interest litigation had no prior precedent in India; and the selection of Kanpur was arbitrarily made by Mr. Mehta when asked to reduce the scale of his original petition. We therefore use difference-in-differences (DiD) regression to estimate the impacts of the ‘policy’ on water pollution and infant mortality, respectively. These estimates are denoted ‘DiD’ in Figure 3.1, a schematic diagram of our empirical strategy.

Our DiD estimates can tell us whether the policy worked or not. But they cannot tell us about specific mechanisms of impact. As Figure 3.1 shows, policy may affect health through a variety of different channels: pollution levels, behavioral change (such as water-source switching or increased home water treatment), and income effects (through lost wages or employment) are intuitive examples. Typically, the reduced-form estimated relationship between policy health and conflates all of these channels. We seek to better understand which individual mechanisms drove the policy’s success, in spite of lacking data on household behavior and incomes.

Our strategy for investigating mechanisms starts with a direct estimation of the pollution-health dose-response function. Since pollution is closely related to industrialization and urbanization – which themselves tend to improve health – an Ordinary Least Squares regression of health outcomes on pollution levels is liable to be biased towards zero. We construct an instrument using upstream pollution levels, described at length in Section 3.4, and estimate the function using Two Stage Least Squares (‘IV’ in Figure 3.1).

Figure 3.1 then shows how our estimated relationships line up. We can multiply the effect of policy on pollution by the effect of pollution and health to identify the policy’s impact on health specifically through pollution. Comparing this to our reduced-form policy-health relationship then shows whether the policy’s health impacts are fully explained by the pollution channel or additionally driven by other channels. Our null hypothesis is that pollution is the only channel at work; our econometric model in the subsections below builds up a statistical test of that hypothesis.

### 3.3.1 Reduced-Form Impact

To assess the impact of the Supreme Court ruling on welfare, we specify a simple reduced-form model of mortality:

$$Mortality_{idt} = a + bT_{dt} + X_{idt}\gamma + e_{idt} \quad (3.1)$$

where  $Mortality_{idt}$  is a dummy variable indicating whether a child  $i$ , born in district  $d$ , in year-month  $t$ , died within the first month of life,  $T_{dt}$  is the policy variable – the October 1987 *Mehta vs. Union of India* court decision in our case – in district  $d$  and year-month  $t$ , and  $X_{idt}$  is a vector of individual, location-by-time characteristics, which includes district and year-month fixed effects. Typically,  $T_{dt}$  will be a dummy variable that takes value 1 in districts that are subject to environmental regulation and in periods following the date on which regulation was enacted. The crux of our identification strategy is that the error term  $e_{idt}$  is such that  $Cov(T_{dt}, e_{idt}) = 0$ . As discussed earlier, Mr. Mehta indicated that one key factor motivating the choice of Kanpur for his legal challenges was related to the salience



of pollutants coming from the tanneries. It therefore seems that Kanpur was unlikely to have been chosen for (unobserved) characteristics that could independently affect infant mortality, reducing the concern of endogenous policy.

### 3.3.2 Mechanisms

While the reduced-form estimate of  $b$  in Equation 3.1 is of interest in itself, we further seek to gauge the relative importance of the various channels that could lead environmental policy to affect infant mortality. To do so, we specify a parsimonious model of the determinants of infant mortality rates:

$$Mortality_{idt} = \alpha + \beta Poll_{dt} + \tilde{X}_{idt}\gamma + (Z_{idt}\delta + \varepsilon_{idt}), \quad (3.2)$$

where  $Poll_{dt}$  is the recorded average river pollution in district  $d$  and year-month  $t$ , and  $\tilde{X}_{idt}$  is a vector of observable district-month characteristics. We partition the space of unobserved risk factors into two:  $Z_{idt}$  and  $\varepsilon_{idt}$ . The former is a vector of all unobserved risk factors that are also correlated with environmental policy  $T_{dt}$ . These include, but are not restricted to, individual awareness about river water contamination, changes in factor prices stemming from the implementation of environmental policy  $T_{dt}$ , or any type of private or public interventions that might have been triggered by  $T_{dt}$ . The latter captures the other risk factors of infant mortality and is, by construction, such that,  $Cov(T_{dt}, \varepsilon_{idt} | \tilde{X}_{idt}) = 0$ .

To the extent that environmental policy  $T_{dt}$  successfully reduced infant mortality rates, we are interested in investigating the channels through which it did so. To aid in this pursuit, we put additional structure on the mechanisms and assume that  $Z_{idt}$  responds to environmental policy according to

$$Z_{idt} = \alpha^1 + \beta^1 T_{dt} + \tilde{X}_{idt}\gamma^1 + \varepsilon_{idt}^1. \quad (3.3)$$

while pollution responds analogously to

$$Poll_{dt} = \alpha^2 + \beta^2 T_{dt} + \tilde{X}_{idt}\gamma^2 + \varepsilon_{idt}^2, \quad (3.4)$$

We can rewrite Equation 3.2 by substituting for both  $Z_{idt}$  and  $Poll_{dt}$ , so as to obtain the reduced-form expression:

$$\begin{aligned} Mortality_{idt} = & \left[ \alpha + \beta\alpha^2 + \alpha^1\delta \right] + \left[ \beta\beta^2 + \beta^1\delta \right] T_{dt} \\ & + \tilde{X}_{idt} \left[ \beta\gamma^2 + \gamma + \gamma_1\delta \right] + \left[ \beta\varepsilon_{idt}^2 + \varepsilon_{idt}^2\delta + \varepsilon_{idt} \right]. \end{aligned} \quad (3.5)$$

The total impact of environmental policy  $T_{dt}$  on infant mortality is given by  $[\beta\beta^2 + \beta^1\delta]$  and can be decomposed into a pollution channel of magnitude  $[\beta\beta^2]$  and other channels that account for a share  $\frac{\beta^1\delta}{\beta\beta^2 + \beta^1\delta}$  of the total. While  $\beta^2$  can be estimated directly from Equation 3.4,  $\beta^1$  cannot be obtained directly, because  $Z_{idt}$  is unobservable. Instead, rewriting Equation 3.2 by substituting for  $Z_{idt}$  only yields

$$Mortality_{idt} = \left[ \alpha + \alpha^1\delta \right] + \beta Poll_{dt} + \tilde{X}_{idt} \left[ \gamma + \gamma^1\delta \right] + \left[ \beta^1\delta \right] T_{dt} + \left( \varepsilon_{idt}^1\delta + \varepsilon_{idt} \right) \quad (3.6)$$

so that we can directly estimate  $\beta$  and  $\beta^1\delta$  by regressing  $Mortality_{idt}$  on  $T_{dt}$  and  $Poll_{dt}$  after controlling for  $\tilde{X}_{idt}$ . However, while  $T_{dt}$  is argued to be orthogonal to the error term (see earlier discussion), the identification assumption  $Cov(Poll_{dt}, \varepsilon_{idt}^1\delta + \varepsilon_{idt}) = 0$  might not hold; pollution could well be correlated with other factors affecting mortality such as urbanization levels (and access to health care facilities or education) or agricultural productivity. We address this endogeneity problem by revisiting Equation 3.4 and assuming that pollution levels are also driven by  $Poll_{dt}^{-1}$ , river pollution upstream of district  $d$  at time  $t$ . By writing  $\varepsilon_{idt}^2 = \tilde{\varepsilon}_{it}^2 + \eta^2 Poll_{dt}^{-1}$ , we obtain

$$Poll_{dt} = \alpha^2 + \beta^2 T_{dt} + \tilde{X}_{idt} \gamma^2 + \eta^2 Poll_{dt}^{-1} + \tilde{\varepsilon}_{it}^2, \quad (3.7)$$

and hence exclude  $Poll_{dt}$  from second-stage Equation 3.6. We assume that upstream pollution affects downstream infant mortality rates only through its persistence as the river flows – that is,  $Cov(Poll_{dt}^{-1}, Z_{idt}\delta + \varepsilon_{idt} | \tilde{X}_{idt}) = 0$ . We can then estimate Equation 3.6 using two-stage least squares (2SLS), which will give unbiased estimates of both  $\beta$  and  $\beta^1\delta$ . We can then test  $H_0 : \beta^1\delta = 0$ .

We note that under this null, Equation 3.6 can be rewritten

$$Mortality_{idt} = [\alpha + \alpha^1 \delta] + \beta Poll_{dt} + \tilde{X}_{idt} [\gamma + \gamma^1 \cdot \delta] + (\varepsilon_{idt}^1 \delta + \varepsilon_{idt}) \quad (3.8)$$

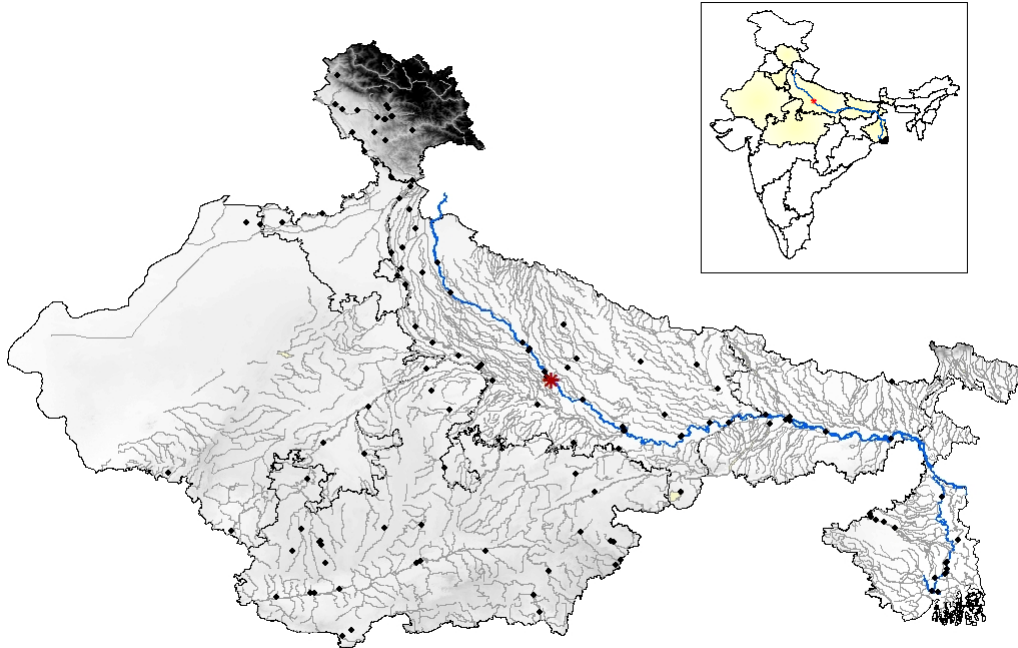
so that  $T_{dt}$  is alongside  $Poll_{dt}^{-1}$  excluded from the second stage Equation 3.8 and becomes another valid instrument for  $Poll_{dt}$ . One test of  $H_0$  is therefore an overidentification test that assesses the orthogonality condition for  $T_{dt}$ , as part of the larger set of instruments  $\{T_{dt}, Poll_{dt}^{-1}\}$ . To conduct such a test, we construct a *C-statistic* (see, e.g., Eichenbaum, Hansen, and Singleton 1988) or difference-in-Sargan test statistic, which is equal to the difference of the two Sargan-Hansen *J-statistics* obtained from the regression using both  $T_{dt}$  and  $Poll_{dt}^{-1}$  as instrument on the one hand and the one using only  $Poll_{dt}^{-1}$  on the other hand.

## 3.4 Data

### 3.4.1 Pollution data

Our main source of data is a subset of the universe of data collected under India's national water quality monitoring program, culled from a combination of CPCB online and print records. These data were originally gathered and used by Greenstone and Hanna (2014). We limit our analysis to the years 1986-2004, because our most recent infant mortality data are from 2004 and our earliest pollution data are from 1986. In most of our analysis, we further restrict our sample to the geographic region encompassed by the Ganga River Basin – depicted in Figure 3.2. We make this second restriction because of the singularity of the Ganga in the context of our analysis. The Ganga River Basin is not only a much more densely populated region than anywhere else in India; it is also a region in which water issues have received special government attention. Furthermore, at the time of the Supreme court decision at the heart of this analysis, the National River Conservation Plan (NRCP) was exclusively aimed at the Ganga Basin (the Yamuna, Damodar, Gomti, Mahananda, and – most extensively – Ganga Rivers). Extending the analysis beyond the Ganga and its tributaries might then confound the effect of NRCP. The aforementioned sample restrictions

**Figure 3.2:** *Locus of the Study*



Notes: The asterisk marks the city of Kanpur. Black dots indicate pollution monitors within the Ganga Basin, and the blue line highlights the Ganga River itself.

produce a set of 101 unique pollution monitors situated along 29 rivers within the Ganga Basin. Over the nineteen-year sample time period, this set provides 13,466 monitor-month observations of water quality. As many as 46 different measures of water quality are recorded at these monitoring stations, but only a few measures are consistently recorded over the whole sample timeframe. To mitigate measurement errors and missing values, we construct moving averages of the data over a four-month window at the district level.

For our analysis, we choose to focus primarily on BOD. This common, broad-based measure of water pollution measures the amount of dissolved oxygen needed by water-borne, aerobic organisms to break down organic material present (at a certain temperature, over a specific time period). Its units are milligrams of oxygen consumed per liter (mg/l). Reduction of BOD is the primary goal of waste treatment plants in general (Brown and Caldwell 2001), but BOD is a particularly good choice for pollution measurement in the

setting of Kanpur. Pollution from the tanning process primarily comes from two sources: the animal hides themselves, and the chemicals used to tan them. Both of these sources contain large amounts of organic matter and are reflected in abnormally high effluent levels of BOD. According to the United Nations Industrial Development Organization (UNIDO 2011), effluent discharge into surface water typically is required to have BOD below 30-40 mg/l, while the typical BOD in *raw* tannery effluent is approximately 2,000. Total suspended solids (TSS), and total dissolved solids (TDS) are potential alternatives to BOD in analysis, but the first of these is not recorded in large numbers in our data, and the second does not provide adequate coverage of our policy pre-period. Chromium, perhaps the highest-profile pollutant in the tanning process, was not widely measured by the CPCB as of 2004.

To support the evidence provided by BOD, we also consider four other pollutants that shed light on the impacts of the Kanpur Supreme Court verdicts: calcium, sulfate, chloride, and fecal coliforms (FCOLI). Calcium is the key component of lime, which is a standard ingredient used in the removal of hair, the removal of flesh, and the splitting of the hide into its two primary layers. Sulfate and chloride ions, meanwhile, are the main components of the TDS produced in tanning. FCOLI is a measure of domestic (as opposed to industrial) pollution, which is the major focus of the National River Conservation Plan. Together, calcium and sulfate are a robustness check on our primary BOD-based analyses: if the policy truly reduced pollution, and if what we are capturing in our difference-in-difference analysis is indeed that policy impact, then we should find a reduction in these pollutants after policy implementation. Chloride and FCOLI, on the other hand, provide falsification checks. The former is present in high numbers in tannery effluent but is so soluble in water that it is not affected by standard tannery waste treatment (UNIDO 2011). The latter should not be affected by tannery regulation, since it is not produced in large quantities by tanning.

Our econometric model and identification strategy rely heavily on the measurement of *upstream* pollution values. Many water quality monitors in our dataset have more than one possible upstream counterpart. We take the choice of which one to use seriously, because there is a balance to be struck between the upstream instrument's strength (or

“relevance”) and its validity. When an upstream monitor is relatively closer to its downstream counterpart, more of its pollution will remain in the river at the downstream location (this is our desired source of identification); however, it will also, in general, lead to a higher spatial correlation, driven by off-river factors like region-wide economic shocks (this is variation we wish to avoid using). The ideal upstream monitor is far enough away to minimize this latter concern but not so far away that all of its pollution decays before arriving at the downstream locale. We therefore adopt a variety of definitions of “upstream”. To assign an upstream counterpart to a given pollution monitor, we use the following algorithm: first, we follow the river upstream until it reaches a new district; next, we locate the nearest monitor along the river that falls within a distance range (in km) of  $[X, Y]$  from the original monitor, where  $X \in \{0, 20, 50, 100\}$  and  $Y \in \{200, 300\}$ . When a river splits upstream of a given monitor, so that there is an upstream monitor on each of two tributaries, we take the average of these monitors as our upstream measure. When there is no upstream monitor to be found, we use the river’s origin as an upstream location (subject to the distance-range requirement) and assign the sample-wide minimum value of pollution as our upstream measure.

### **3.4.2 Health data**

Research has documented a wide variety of adverse impacts on health due to water pollution; adults (e.g., Ebenstein 2012) and children (e.g., Galiani et al. 2005) alike are susceptible, and morbidity (e.g., Kremer et al. 2012) as well as mortality (e.g., Brainerd and Menon 2014) are potentially affected. For our own study of the health burden imposed by river pollution in India, we choose infant mortality as our key health outcome. This choice follows those of many others in the literature and is motivated by science, policy, and statistical considerations. Research in epidemiology has shown that infants are highly susceptible to pollutants (Fewtrell and Bartram 2001). Recent work suggests that this susceptibility – even *in utero* – can have long-term impacts on individual welfare, through channels such as birth weight, cognitive development and susceptibility to diseases (Currie 2008 and Currie and Almond 2011). The vulnerability of infants to water pollution is of particular policy interest

in India, where infant death rates remain quite high relative to the global average (United Nations 2011). While infant mortality is clearly an incomplete measure of the health costs imposed by water pollution, it nonetheless represents a very large loss of life in the Indian context.

Furthermore, the use of infant mortality as an outcome of interest conveys at least two significant advantages. The first is general to infant health outcomes, as noted by Chay and Greenstone (2003) and Currie et al. (2009): newborns do not have a long history of prior exposure to pollution, so the link between water quality and their health is immediate, and an analysis of pollution levels during the first year of life nearly fully captures lifetime exposure (in direct contrast with studies of adult, or even under-5, mortality). The second pertains to statistical power: complete birth histories are available in certain Indian demographic surveys, so we can construct long pseudo-panels of infant survival status. Variables such as diarrhea incidence and low birth weight, on the other hand, are only available cross-sectionally from the time of survey. Panel variation in infant mortality allows us to include detailed temporal and cross-sectional fixed effects in regression analysis, removing some of the concern we have about omitted variable bias.

Our infant health data come from the Reproductive and Child Health II (RCH-2) module of the District-Level Household Survey II (DLHS-2), a national demographic survey conducted in two phases from 2002 to 2005. In the RCH-2 module, mothers report age and survival for all of their children; from these birth histories, we create a panel of district-month infant mortality rates. We start with the raw total of 1,393,431 births from 1967 through 2004 that are reported in RCH-2. We then match each birth to pollution data from the district in which the birth took place<sup>9</sup>; this restricts our sample to 264,375 births. Collapsing from the individual level to district-level means yields a panel of 25,349 district-month observations with non-missing infant mortality rate. Finally, restricting our analysis to the Ganga Basin produces a sample of 5,785 district-months spanning 41 districts and 8 states.

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<sup>9</sup>In DLHS-2 as well as all other potential data sources of which we are aware, infant location is only available down to the district level.

### **3.4.3 Other data**

We include several types of variables as controls in many of our regression analyses. The main body of these controls is composed of cross-sectional survey answers about birth, mother, and child characteristics taken from RCH-2. A pair of climate controls are created using monthly, gridded rainfall averages from the University of Delaware and air temperature averages from the Indian Meteorological Institute; we use these gridded averages to interpolate rainfall and temperature values at each monitor-month. We further include our measured distance between a monitor and its upstream pair. Finally, we observe Common Effluent Treatment Plant (CETP) capacity and the incidence of major river cleanup policy (NRCP; see description in Section 3.2).

## **3.5 Empirical Results**

We begin our analysis with a brief statistical description of the key variables measuring mortality, pollution, and policy. We then move on to an exploration of policy impacts, using a difference-in-differences framework. We estimate Equation 3.1 in order to identify the infant mortality impacts of the Supreme Court verdict; and we estimate Equation 3.4 to identify the pollution impacts. After establishing these impacts, we then use instrumental variables to investigate the channels of this link. We estimate Equation 3.6 via both Ordinary Least Squares (OLS) and Two-Stage Least Squares (2SLS) with our upstream instrument; we compare the latter specification to a specification using both upstream pollution and the policy as instruments – which corresponds to 2SLS estimation of Equation 3.8.

### **3.5.1 Summary Statistics**

Table 3.1 reports summary statistics for the key variables used in our regression analyses. With respect to health, we focus on neonatal (i.e., one-month) mortality because 95% of infant (i.e., one-year) mortality occurs in the first month of life. The adverse effect of pollution on one-year olds is thus more likely to be captured by morbidity, which we do



not observe over time. With this focus on the first month of life, we first observe that, from 1986-2004, the national average of district-level neonatal mortality is 0.046; on average, then, 4.6% of a district's newborns die before the end of the first month of life. From the right panel of Table 3.1, that neonatal mortality rate is even higher in the Ganga Basin: 0.060, or 6%. This could potentially be explained by higher pollution in the Ganga Basin, greater use of polluted water, or higher poverty, among many other patterns.

Statistics for BOD and FCOLI confirm that pollution in the Ganga Basin is greater than elsewhere in India. We take the natural logarithm of BOD because its empirical distribution appears strongly log-normal. District-level log-BOD averages 0.733 across the whole of India but 0.925 within the Ganga Basin. Accordingly, the proportion of district-months exceeding the "bathing class" standard for BOD (3 mg/l) is also higher in the Ganga Basin: 40.5% vs. 33.0%. This trend is also apparent from FCOLI patterns, which are specifically a domestic pollution metric. For these and our other pollutants (calcium, sulfate, and chloride), we focus our analysis on dummies high versus low pollution levels because such dummies reduce the degree of noise in the raw pollution data. One may be tempted to use the level (or log-level) of pollution as the preferred pollution metric, but we note that there no a priori reason to believe that mortality risk responds linearly to pollution.

The final set of variables in Table 3.1 pertain to the Supreme Court verdict. The mean values of 1[Born after 10/1987] and 1[Kanpur] highlight the fact that, regardless of which sample is used, the overwhelming majority of observations come from after the date of the verdict and from outside of Kanpur, respectively. The indicator for incidence of the National River Conservation Plan (NRCP), meanwhile, shows that the Ganga Basin is the predominant focus of government water pollution policy: 20.1% of observations are covered by NRCP in the 'All India' sample, while 52.5% are covered in the Ganga Basin sample. For us, the stark differences in baseline infant mortality, pollution, and policy between the two samples justifies a primary focus on the Ganga Basin in our regression analysis.

**Table 3.1: Summary Statistics**

	<b>All India</b>			<b>Ganga Basin</b>		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.
District-month neonatal mortality rate	25,349	0.046	0.089	5,785	0.060	0.090
ln(BOD)	26,446	0.733	0.863	6,042	0.925	0.881
1[BOD > 3]	26,446	0.330	0.470	6,042	0.405	0.491
1[Fecal coliforms > 5,000]	20,789	0.157	0.364	4,613	0.409	0.492
1[Born after 10/1987]	26,948	0.966	0.182	6,202	0.944	0.229
1[Born in Kanpur]	26,948	0.012	0.109	6,202	0.052	0.222
1[Born after 10/1987] X 1[Born in Kanpur]	26,948	0.011	0.106	6,202	0.049	0.217
1[National River Conservation Plan]	26,948	0.201	0.401	6,202	0.525	0.499

Notes: All statistics are computed from district-month observations. In the left panel, the sample is all of India. In the right panel, the sample is the Ganga Basin only.

### 3.5.2 The Impact of *Mehta vs. Union of India* on Infant Health

Determining whether the Supreme Court verdict improve water quality and health in the Kanpur area is important because there is no precedent for successful water pollution policy in India. The most recent, rigorous analysis of such policy (Greenstone and Hanna 2014) suggests that the most high-profile, expansive regulation – the National River Conservation Plan (NRCP) – has not reduced surface water pollution. Our analysis is differentiated from Greenstone and Hanna’s (2014) along two primary dimensions. First, the *Mehta vs. Union of India* ruling is fundamentally a judicial-branch policy, as compared to the executive-branch action of NRCP. Second, the judicial mandate targets industrial pollution, whereas NRCP targets domestic pollution. While our study of one particular policy does not allow us to make any credible point about judicial action or industrial pollution policy in general, we think the results are nonetheless relevant to broader Indian environmental policy design. Indeed, the question about how best to attack rampant water pollution problems in India frequently revolves around regulatory jurisdiction and sectoral targeting. Is the Indian judiciary the most effective environmental regulator? Its history of “activism” (discussed in Section 3.2) is corroborated by Greenstone and Hanna (2014) insofar as, with respect to *air* pollution, Supreme Court policy has been quite successful. Is the regulation of industry worth prioritizing? Sewage appears to be a far greater source of surface water pollution in India (CPCB 2013b), and the tanning industry is so concentrated (Schjolden 2000) that the burden of regulatory compliance could disproportionately hurt cities like Kanpur.

Table 3.2 speaks directly to such questions by establishing a causal link between the Supreme Court verdict and neonatal mortality. Each column displays results from a different specification, with respect to geographic coverage, time period, and parameterization of the policy variable. All regressions estimate Equation 3.1 – i.e., the predicted mortality impact of being in Kanpur after the ruling came down. Columns 1-3 do so using the Ganga Basin only, and using either the full 1986-2004 time period, 1986-1999, and 1986-1994, respectively. Column 4 uses the full time period but breaks the policy impact into a short-run, medium-run, and long-run term. Columns 5-8 use analogous specifications for the ‘All

India' sample.

Table 3.2 provides consistently strong evidence that neonatal mortality dropped in Kanpur in the aftermath of the verdict. The point estimates in columns 1-3 imply that the magnitude of the mortality reduction is in the range of 1.8 to 2.9 percentage points, on a baseline of 6% (which comes from Table 3.1). Column 4 suggests that there may have been some attenuation in this mortality impact over the long run. The All-India results in columns 5-8 exhibit the same patterns as the Ganga Basin results, except with higher magnitudes. These higher magnitudes could be symptomatic of a general downward time trend in water pollution within the Ganga Basin, given the government's focus on this region from the 1980s onwards (we do, however, control for NRCP incidence and CETP capacity in all regressions). We believe the conservative choice is thus to omit non-Ganga districts from the control group for Kanpur, so we prioritize the tabulation of Ganga Basin results in the remainder of this paper.

### **3.5.3 The Impact of *Mehta vs. Union of India* on River Pollution**

If there is an economic cost to regulation of the tanning industry, then it is to be weighed against the significant improvements to public health indicated by the results in Table 3.2. But such results beg the question: through what channel(s) did the verdict affect neonatal mortality? In terms of outcomes, the primary target of the Supreme Court ruling was river pollution. However, environmental education and awareness of water quality issues were also included in the portfolio of actions mandated by the ruling. The ruling could thus have acted on infant health entirely through a behavioral channel – e.g., water-source switching or increased home treatment of water. To test this hypothesis, we estimate the change in pollution levels associated with the Kanpur policy.

Results are shown in Table 3.3 and correspond to Equation 3.4. Columns 1-5 focus on our primary pollutant, BOD, while columns 6-9 leverage other relevant pollutants as logic tests of the policy's impact. The BOD results uniformly imply a significant drop in pollution. Given the parameterization of BOD as a dummy for exceeding the "bathing class" threshold,

Table 3.2: Mehta vs. Union of India and Infant Mortality

	(1)	(2)	(3)	(4)
<i>Panel A. Ganga Basin</i>				
1[Kanpur] X 1[t > 10/1987]	-0.025*** (0.009)	-0.029*** (0.009)	-0.018* (0.010)	
1[Kanpur] X 1[t ∈ [10/1987,12/1994]]				-0.028*** (0.009)
1[Kanpur] X 1[t ∈ [1/1995,12/1999]]				-0.034*** (0.010)
1[Kanpur] X 1[t ∈ [1/2000,12/2004]]				-0.004 (0.012)
Last year of sample	2004	1999	1994	2004
R-Squared	0.08	0.09	0.09	0.015
N	5,785	4,042	1,984	5,785
<i>Panel B. All India</i>				
1[Kanpur] X 1[t > 10/1987]	-0.038*** (0.005)	-0.043*** (0.005)	-0.039*** (0.005)	
1[Kanpur] X 1[t ∈ [10/1987,12/1994]]				-0.040*** (0.005)
1[Kanpur] X 1[t ∈ [1/1995,12/1999]]				-0.044*** (0.006)
1[Kanpur] X 1[t ∈ [1/2000,12/2004]]				-0.021*** (0.006)
Last year of sample	2004	1999	1994	2004
R-Squared	0.06	0.06	0.07	0.06
N	25,340	18,707	10,057	25,340

Notes: An observation is a district-month. The dependent variable in all regressions is the district-month neonatal mortality rate. All regressions include a set of controls (CETP capacity, air temperature, total precipitation, and NRC dummy) and district and year-month fixed effects. Standard errors are clustered at the district level in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1, 5, and 10% levels, respectively.

the estimated magnitude is a 40-50 percentage-point drop in the likelihood of exceedance. Furthermore, columns 2, 3, and 4 all suggest that the BOD impact does not lessen over time.

As discussed in Section 3.4, BOD is an excellent pollutant to study in the context of tanning, but so are several others which we observe in our pollution sample. Both a reduction in tanning volume and an improvement in effluent treatment should cause drops in calcium and sulfate concentrations. If the BOD impacts are being driven by compliance with the Supreme Court verdict, then we should see corresponding drops in calcium and sulfate levels. Meanwhile, changes made by the tanneries should not affect levels of FCOLI – which is a domestic pollutant – nor should they affect chloride concentration – chloride is not affected by standard tannery waste treatment (UNIDO 2011). Table 3.3 shows that all four tests of logic are passed. Calcium and sulfate levels drop in Kanpur after the verdict date, while FCOLI and chloride levels do not move. The combined evidence suggests that *Mehta vs. Union of India* both saved lives and reduced pollution into the Ganga River.

### **3.5.4 Mechanisms of Policy Impact**

The avoided loss of life identified in Table 3.2 need not be fully explained by the pollution reduction identified in Table 3.3. Households could still have changed their behavior with respect to water treatment and/or usage. Moreover, there is an income channel through which the policy could have affected mortality: regulation tends to raise compliance costs for industrial firms, which may compel those firms to reduce wages or employment, thereby making households worse off. Thus, there are non-pollution channels of potential impact on mortality in both directions.

The reduced form impact of environmental regulation on health, such as we estimate in Table 3.2, represents the aggregate of all channels. Up to this point, the literature has not disentangled the effects of these different channels. We seek to change that. Our strategy is to calibrate the dose-response function of water pollution and neonatal mortality in India, and use it as a yardstick for the policy's impact on health. Section 3.3 illustrates this with sparse models of mortality and pollution. To carry out the empirical test, we first need to

**Table 3.3:** Mehta vs. Union of India and River Pollution

	(1)	(2)	(3)	(4)	(5)
<i>Panel A. Biochemical Oxygen Demand</i>					
1[Kanpur] X 1[t > 10/1987]	-0.392*** (0.050)	-0.437*** (0.044)	-0.556** (0.059)	-0.401*** (0.148)	-0.426*** (0.033)
1[Kanpur] X 1[t ∈ [10/1987,12/1994]]				-0.384*** (0.082)	
1[Kanpur] X 1[t ∈ [1/1995,12/1999]]				-0.389*** (0.125)	
1[Kanpur] X 1[t ∈ [1/2000,12/2004]]					
Geographic coverage	Ganga	Ganga	Ganga	Ganga	India
Last year of sample	2004	1999	1994	2004	2004
R-Squared	0.57	0.56	0.65	0.57	0.58
N	6,042	3,911	1,901	6,042	26,434
<i>Panel B. Other Pollutants</i>					
1[Kanpur] X 1[t > 10/1987]	Calcium -0.204*** (0.062)	Sulfates -0.623*** (0.072)	Chlorides -0.005 (0.058)	FCOLI -0.74 0.132	
Geographic coverage	Ganga	Ganga	Ganga	Ganga	
Last sample year	2004	2004	2004	2004	
R-Squared	0.55	0.37	0.56	0.56	
N	5,114	4,867	6,202	4,613	
Notes: An observation is a district-month. In Panel A, the dependent variable is whether the district-month average BOD is above 3 mg/l (i.e., the government's "bathing class" standard). In Panel B, columns 1, 2 and 3, it is whether concentrations of calcium, sulfate, and chloride are above their sample-wide medians, respectively. In Panel B, column 4, it is whether fecal coliforms exceed 5,000 MPN. All regressions include a set of controls (CETP capacity, air temperature, total precipitation, and NRCP dummy) and district and year-month fixed effects. Standard errors are clustered at the district level in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10% levels, respectively.					

identify the dose-response function.

Estimation of Equation 3.6 via OLS may produce biased estimates. Pollution is not randomly assigned; it is associated with urbanization, industrialization, and economic activity, all of which are unobservable on some level and yet could easily affect health through non-pollution channels. For that reason, we instrument for pollution with its upstream analog. As described in Section 3.4, we experiment with a number of different assignment rules to define “upstream” pollution. Table 3.4 reveals the results of this experimentation, by displaying the point estimates from the first stage of 2SLS using the upstream instrument. Each point estimate in Table 3.4 comes from a different regression, corresponding to a particular  $[X, Y]$  range and either the Ganga Basin sample or the All-India sample.

Notably, the first stage is both strong and robust to the choice of upstream range. Columns 1-5 vary the lower bound of that range from 0 km to 100 km while holding constant the upper bound of 200 km. Column 6 uses our preferred lower bound of 75 km with an upper bound of 300 km instead of 200. We prefer  $[75, 200]$  because it is the most conservative from among those with consistently strong first stages (point estimates with the 100-km lower bound retain the flavor of the other results but are not across-the-board statistically significant). As the lower bound rises, the risk of conflating pollution flow downstream with spatial correlation due to off-river correlates of pollution. As the upper bound drops, the more likely it is that pollution measured *upstream* actually flows into the area of measurement *downstream*.

With a strong first stage, we can move on to estimation of the pollution-mortality dose-response function, using 2SLS. We can furthermore compare the result to a 2SLS specification that uses both upstream pollution and the policy itself as instruments. Table 3.5 shows both sets of 2SLS regression results. The first column, however, tabulates the results of OLS. The point estimate on pollution here is a statistical zero, which is consistent with the notion that OLS is biased downwards by the positive correlation between pollution and other factors that are beneficial to health. In stark contrast, columns 2-4 reveal a strong



**Table 3.4:** *First-Stage Results of Upstream IV*

	(1)	(2)	(3)	(4)
[0,200]	0.148** (0.058)	0.130** (0.050)	0.130** (0.055)	0.260*** (0.030)
[20,200]	0.188*** (0.067)	0.156** (0.060)	0.167** (0.061)	0.277*** (0.031)
[50,200]	0.226*** (0.070)	0.192*** (0.064)	0.198*** (0.064)	0.278*** (0.035)
[75,200]	0.177** (0.073)	0.175** (0.068)	0.199*** (0.068)	0.253*** (0.036)
[100,200]	0.157* (0.084)	0.121 (0.086)	0.228** (0.096)	0.224*** (0.041)
[75,300]	0.161** (0.061)	0.145*** (0.052)	0.189** (0.073)	0.228*** (0.032)
Geographic coverage	Ganga	Ganga	Ganga	India
Last sample year	1994	1999	2004	2004
N	46,675	46,675	31,683	46,675

Notes: An observation is a district-month. The dependent variable in all regressions is whether the district-month average BOD is above 3 mg/l (i.e., the government's "bathing class" standard), and the key independent variable is the upstream analog. Each coefficient is from a different regression, distinguished by its geographic coverage, time period, and upstream range [X,Y]. The latter denotes the lower and upper bound distances (in km) between each monitor and its upstream predictor. All regressions include a set of controls (CETP capacity, air temperature, total precipitation, and NRCP dummy) and district and year-month fixed effects. Standard errors are clustered at the district level in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1, 5, and 10% levels, respectively.

**Table 3.5: Comparison of Instruments for Pollution**

	OLS (1)	1 IV (2)	2 IV (3)	1 IV (4)	2 IV (5)
<i>Panel A. First stage</i>					
1[US BOD > 3]		0.199*** (0.068)	0.199*** (0.068)	0.177** (0.073)	0.177** (0.073)
<i>Panel B. Second Stage</i>					
1[BOD > 3]	-0.001 (0.007)	0.105*** (0.027)	0.092*** (0.024)	0.137** (0.055)	0.073*** (0.018)
1[Treatment]	-0.022* (0.023)	0.018 (0.016)		0.058 (0.039)	
P-value of C-Stat			0.361		0.239
Last sample year	2004	2004	2004	1994	1994
N	2,907	2,870	2,870	964	964

Notes: An observation is a district-month. The dependent variable in all regressions is the district-month neonatal mortality rate. Columns 2 and 3 show second-stage results of 2SLS, where the endogenous variable 1[BOD>3] is instrumented using its upstream analog. Columns 4 and 5 show 2SLS results where 1[BOD>3] is instrumented using both its upstream analog and a dummy for policy incidence in district  $d$  and year-month  $t$ . The C-statistic tests whether 1[BOD>3] is overidentified by these two instruments. All regressions include a set of controls (CETP capacity, air temperature, total precipitation, and NRCP dummy) and district and year-month fixed effects. Standard errors are clustered at the district level in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1, 5, and 10% levels, respectively.

positive relationship between pollution and mortality after instrumenting for the former. The implication is that a deterioration of water quality from “bathing class” standard to below that standard is associated with roughly a 10 percentage-point rise in the neonatal mortality rate (10.5 in column 2, 9.8 in column 3, and 13.7 in column 4).

More than just being a baseline for comparison with our two-instrument strategy, the results in columns 2-4 provide the literature’s first estimates of the impact of industrial pollution on infant health. Prior work has focused on domestic pollution (Field, Glennerster, and Hussam 2011) or agricultural pollution (Brainerd and Menon 2014). Moreover, the fact that the pollution variation in these regressions comes from upstream indicates that regional spillovers in pollution impart a very real health burden on downstream districts. Previous work on regional pollution spillovers have linked upstream water pollution only to corresponding downstream measures (Sigman 2005; Lipscomb and Mobarak 2007).

Nonetheless, the primary purpose of IV estimation is to facilitate an exploration of the policy's channels of impact. For this, we must compare columns 2-4 with columns 6-8, respectively. The latter columns detail the results of 2SLS with the policy dummy as an additional instrument. Inspection of the differences in point estimates provides a visual test of mechanisms, while the p-value for the C-statistic provides the statistical test. Point estimates of very similar magnitudes suggest that the policy's impact on neonatal mortality does not significantly differ from the mortality effect predicted by the policy's pollution reduction – i.e., that pollution fully explains the policy's health impact. Point estimates of very *different* magnitudes suggest the opposite – that the policy's health impact does not match up with expectations based on the policy's pollution effect, and that some other channel(s) must be at work.

In practice, the 1-IV and 2-IV point estimates are neither exactly the same nor significantly different. The eye test indicates that, if anything, the 2-IV point estimates are smaller than the 1-IV analogs. This, in turn, implies that the net mortality reduction identified in Table 3.2 combines the effect of pollution reduction with an income effect, or some other countervailing channel. However, overidentification tests fail to reject the null that 1-IV and 2-IV estimates are the same. The p-value for the test statistic ranges from 0.24 to 0.53. The test is not sufficiently high-powered to reject the null. Nonetheless, we have shown how information about the structural relationship between pollution and health can, in principle, be used to gain a more intricate understanding of policy impacts.

### **3.6 Conclusion**

The paper provides empirical evidence that the 1987 Supreme Court decision in *Mehta vs. Union of India*, which primarily targeted the tanning industry in Kanpur district, induced a drop in both surface water pollution and neonatal mortality. Our investigation of the mechanisms of policy impact strongly suggest that pollution is indeed a major channel of the mortality effect. It also suggests the possibility that other channels, especially related to income, contribute to the policy's net mortality reduction. The derivation and application of

our statistical test of mechanisms illustrates how information about the different potential mechanisms can be backed out from analysis even when data on all possible mechanisms are not available.

We believe our analysis represents an important contribution to the literature for several other reasons. First, we have identified a precedent for successful water pollution policy in India. This is not trivial, given the thirty-year, high-cost failure of India's National River Conservation Plan. It is moreover interesting that the successful water pollution policy was precipitated by the judiciary, targeting industrial pollution, in contrast to NRCP's source in the executive branch of government and target of domestic pollution.

Second, we have demonstrated that river pollution – in particular, of the industrial variety – has a real, adverse impact on infant health in India. This is important because there is a dearth of evidence on water pollution's impacts in the developing-country context, and because the demand for water quality has historically been lower in India than the demand for air quality (Greenstone and Hanna 2014). This relatively low demand, when coupled with significant detrimental impacts of water pollution on health, suggest that imperfect information may explain part of the puzzle of low environmental quality in developing countries (Greenstone and Jack 2015).

Third, we have shown that the ultimate incidence of the costs of pollution is not limited to the origin of that pollution. Rather, water pollution flows downstream to other communities living along rivers, reducing not just water quality but also the likelihood of infant survival. This finding provides even stronger motivation for inter-jurisdictional bargaining to achieve optimal pollution levels; otherwise, free-riding by upstream districts will impose real social costs.

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# Appendix A

## Appendix to Chapter 1

### A.1 Theoretical Derivation of Pass-Through

The structural determination of pass-through depends integrally on the nature of competition. To illustrate this fact, below I derive the equation for pass-through under (a) perfect competition, (b) monopoly, and (c) Bertrand oligopoly. None of the derivations below are original. To my knowledge, the perfect competition result is due to Jenkin (1872); the monopoly result is due to Bulow and Pfleiderer (1983); and the oligopoly result is due to Anderson, de Palma, and Kreider (2001).

#### Perfect competition

In the special case of perfect competition, all firms are identical and there is one market price ( $p_c$ ). Equilibrium is given by the meeting of aggregate demand with competitive supply, given a tax  $t$ :

$$D(p_c) = S(p_c, t)$$

Total differentiation yields an expression for pass-through  $\frac{dp_c}{dt}$ , which is the same for all firms:

$$\frac{dp_c}{dt} = \frac{-\frac{\partial S}{\partial t}}{\frac{\partial S}{\partial p_c} - \frac{\partial D}{\partial p_c}}$$

Finally, assuming  $\frac{\partial S}{\partial t} = -\frac{\partial S}{\partial p_c}$ , substituting, and multiplying the numerator and denominator by  $p_c/q$  yields:

$$\frac{dp_c}{dt} = \frac{\frac{\partial S}{\partial p_c}}{\frac{\partial S}{\partial p_c} - \frac{\partial D}{\partial p_c}} * \frac{p_c}{q} = \frac{\epsilon_S}{\epsilon_S - \epsilon_D} = \frac{1}{1 - \frac{\epsilon_D}{\epsilon_S}} \quad (\text{A.1})$$

Thus, equilibrium pass-through under perfect competition is a function only of the ratio of absolute demand elasticity ( $\epsilon_D$ ) to supply elasticity ( $\epsilon_S$ ). Importantly, pass-through need not be one-for-one (100%) in this setting; it is, however, bounded between 0 and 100%. To see this, consider the polar cases of demand: A market with perfectly inelastic consumption ( $\epsilon_D = 0$ ) will be characterized by 100% pass-through, since suppliers will lose no sales from raising prices; on the other hand, a market with perfectly *elastic* consumption ( $\epsilon_D \rightarrow -\infty$ ) will be characterized by 0% pass-through, since consumers will cease buying all energy if the price rises at all. Similarly, perfectly elastic supply ( $\epsilon_S \rightarrow +\infty$ ) and perfectly inelastic supply ( $\epsilon_S = 0$ ) produce 100% and 0% pass-through, respectively.

## Monopoly

The monopolist's profit function is:

$$\pi_m(q) = qp_m(q) - c(q) - qt$$

where  $c(q)$  is a total cost function. Retail gasoline supply is likely very elastic in the short run, since oil production is steady and the great majority of marginal cost in retailing is the purchase of fuel. For simplicity, I therefore proceed with the assumption that marginal costs are constant. This produces the familiar monopoly first-order condition (FOC):

$$\frac{\partial \pi_m}{\partial q_m} = p_m(q) + q \frac{\partial p_m}{\partial q} - c - t = 0$$

where the first two terms comprise marginal revenue and the last two terms comprise marginal cost. Total differentiation of this FOC with respect to  $t$  defines monopoly pass-through:

$$\frac{dp_m}{dt} = \frac{\frac{\partial p(q_m)}{\partial q_m}}{2 \frac{\partial p(q_m)}{\partial q_m} + q_m \frac{\partial^2 p(q_m)}{\partial q_m^2}} \quad (\text{A.2})$$

The monopoly price impact of a tax change thus depends most integrally on the shape of demand. If demand is linear, then the second term in the denominator drops out and pass-through is 50%. If demand is non-linear, then the second derivative of demand dictates the relative change to pass-through: concave demand produces less than 50% pass-through; convex demand produces greater than 50% pass-through and is no longer bounded above by 100%.

## Oligopoly

Cost pass-through in an oligopolistic market is determined by a much more complex process. Each firm now has its own residual elasticity of demand, and it also now has incentive to respond to the pricing decisions of its neighbors. To see this, consider a model of Bertrand multi-product (-station) competition. There is a set of stations  $S$ , indexed  $i = \{1, 2, \dots, N\}$ , each with its own, constant marginal costs  $c_i$ . The  $N$  stations are owned by  $F$  firms, indexed  $f = \{1, 2, \dots, F\}$ , with  $F \leq N$ . The set of stations run by firm  $f$  is denoted  $S_f$ . Profits for firm  $f$  are given by:

$$\pi_f(\mathbf{p}) = \sum_{i \in S_f} q_i(\mathbf{p}) [p_i - c_i - t]$$

The profit maximization problem for this firm  $f$  is to choose price  $p_i$  at each station  $i \in S_f$  to maximize  $\pi_f(\mathbf{p})$ . The resulting first-order condition for firm  $f$ , station  $i$  is:

$$\frac{\partial \pi_f}{\partial p_i} = q_i + \frac{\partial q_i}{\partial p_i} [p_i - c_i - t] + \sum_{k \neq i, k \in S_f} \frac{\partial q_k}{\partial p_i} [p_k - c_k - t] = 0$$

Totally differentiating this FOC with respect to  $t$ , and rearranging terms, produces:

$$\begin{aligned}
\frac{dp_i}{dt} &= \left[ \frac{\partial q_i}{\partial p_i} + \sum_{k \neq i, k \in S_f} \frac{\partial q_k}{\partial p_i} \right. \\
&\quad \left. - \sum_{j \neq i} \left( \frac{\partial q_i}{\partial p_j} + \frac{\partial^2 q_i}{\partial p_i \partial p_j} m_i + \sum_{k \neq i, k \in S_f} \left( \frac{\partial q_k}{\partial p_i} \frac{\partial p_k}{\partial p_j} + \frac{\partial^2 q_k}{\partial p_i \partial p_j} m_k \right) \right) \frac{dp_j}{dt} \right] \\
&\quad \left/ \left( 2 \frac{\partial q_i}{\partial p_i} + \frac{\partial^2 q_i}{\partial p_i^2} m_i + \sum_{k \neq i, k \in S_f} \frac{\partial^2 q_k}{\partial p_i^2} m_k \right) \right. \tag{A.3}
\end{aligned}$$

where markup  $m_i = p_i - c_i - t$ .

Equation A.3 expresses tax pass-through firm  $i$  as a function not just of market primitives (demand elasticities and marginal costs) but also of the  $j$  other firms' pass-through; it is difficult to simplify further without additional assumptions. If one assumes symmetry among firms in a market, then Equation A.3 reduces to the following:

$$\frac{dp_i}{dt} = \frac{\frac{\partial q_i}{\partial p_i}}{2 \frac{\partial q_i}{\partial p_i} + \sum_{j \neq i} \frac{\partial q_i}{\partial p_j} + (p_i - m) \left[ \frac{\partial^2 q_i}{\partial p_i^2} + \sum_{j \neq i} \frac{\partial^2 q_i}{\partial p_i \partial p_j} \right]} \tag{A.4}$$

where  $m$  is the now-homogeneous sum of marginal cost and retail tax. This structural equation is a generalized version of Equation A.2, which defines monopoly pass-through - if there were no other firms  $j$  in the market, Equation A.4 would collapse back down to Equation A.2. Just as in the monopoly case, both first and second derivatives of demand matter in oligopoly. However, other stations now affect the decision of station  $i$ . Its pass-through rate is now additionally a function of the cross-price elasticities  $\frac{\partial q_i}{\partial p_j}$  as well the cross-price derivatives of own-price elasticities  $\sum_{j \neq i} \frac{\partial^2 q_i}{\partial p_i \partial p_j}$ .