

ESSAYS ON ENERGY AND ENVIRONMENTAL ECONOMICS

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Abstract

This dissertation consists of three essays on energy and environmental economics.

The first chapter examines whether and how municipal mergers affect river pollution. By analyzing the case of Japan's large-scale municipal mergers in the 2000s, the paper finds that, contrary to some previous findings in the literature, municipal mergers increase pollution in rivers. The paper further highlights suggestive evidence of weaker pollution control, through coordination cost and political economy, as a possible channel of this deterioration in water quality.

The second chapter investigates how oil supply responds to price changes in oil. Using well-level data from North Dakota and distinguishing between unconventional (shale) and conventional wells, the paper shows that oil producers operating the former type of wells react asymmetrically to price increases and decreases, while those operating the latter type do not respond to price changes. The existence of asymmetries in responses indicates that previous estimates of price elasticity of supply for oil may be misleading if treated as bidirectional and unique across types of wells.

Finally, the third chapter touches on the intersection between trade and environment, and analyzes the impact of environmental regulations on firms' production behavior, focusing on environmental technology standards. In particular, it develops a theoretical model of heterogeneous firms and offshoring to show that technology standards can induce a selection pattern of offshoring that is different from the case of widely-analyzed pollution taxes. The paper also conducts preliminary industry-level empirical analysis to test the pollution haven effect of a technology standard, by evaluating the Revision of Japan's Air Pollution Control Law implemented in 2018.

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Essays on Energy and Environmental Economics

Chapter 1

Impact of Municipal Mergers on Pollution Control: Evidence of River Pollution in Japan

Kazuki Motohashi and Michiyoshi Toya

Abstract:

Municipal mergers are widely adopted by policymakers for improving efficiency in public service provision, including pollution control. Municipal mergers can improve environmental quality by internalizing pollution spillovers, but coordination costs and skewed distribution of political power among pre-merger municipalities can work in the opposite direction. We test this relationship in the context of Japan's large-scale municipal mergers that almost halved the number of municipalities. By using the staggered-implementation design of municipal mergers and 30-year water quality data, we find that the mergers increased water pollution and find no evidence of negative externality theory. Our results point to alternative mechanisms, namely weaker pollution control through coordination cost and political economy channels.

1.1 Introduction

Municipal mergers, where two or more municipalities combine to form one municipality, have recently become a widely-adopted policy, especially in developed countries such as Austria, Sweden, Germany, Japan, and New Zealand. This policy aims to improve efficiency and quality of public service provision through economies of scale and increased local public expenditure (OECD 2014). One core area of public service provided by municipalities is controlling environmental pollution, including air, water, and soil pollution. Municipalities monitor environmental pollution, enforce environmental regulations through reporting and inspections, and develop infrastructure to contain environmental pollution.

We examine how municipal mergers affect environmental outcomes through changes in pollution control efforts of municipalities. One may expect that municipal mergers can improve environmental quality based on improved efficiency in pollution control by municipalities. This positive effect can be further reinforced by the internalization of pollution spillovers as the number of municipalities decreases through municipal mergers.¹ On the other hand, coordination costs and skewed distribution of political power among pre-merger municipalities may hamper the pollution control effort of merged municipalities, leading to the deterioration of environmental quality. The relationship between municipal mergers and environmental outcomes appears ambiguous and deserves careful empirical examination.

We test this relationship in the context of Japan's municipal mergers in the late 1990s to 2000s. We consider river water quality as an environmental outcome. This merger policy in Japan has unique characteristics to effectively answer our research question. First, this policy drastically reduced the number of municipalities by about 50%, from 3,238 in 1998 to 1,725 by 2012. This large-scale and universal occurrence of municipal mergers across all prefectures in Japan increases the external validity of our results. Second, municipal mergers occur over time from 1999 to 2011. We use the staggered implementation of municipal mergers as a setting of quasi-natural experiment to provide causal impacts of mergers on environmental outcomes. Lastly, the wide-scale occurrence

¹ A recent theory based on the idea of negative externalities, where pollution from a municipality located upstream of a river adversely affects other municipalities located downstream, shows that decentralization, a policy that increases the number of municipalities along a river path, was found to worsen river pollution (Lipscomb and Mobarak 2016). Conversely, municipal mergers, which decrease the number of municipalities and facilitate the internalization of negative externalities, could improve environmental quality.

of municipal mergers allows us to use water quality data of 3,000 monitoring stations across Japan. We use rich water quality data over 30-years to examine the long-run impact of municipal mergers on water quality.

We investigate whether municipal mergers improve or worsen river pollution by exploiting the staggered implementation of municipal mergers. The existence of both merged municipalities and never-merged municipalities and the variation in the timing of municipal mergers among merged municipalities allow us to adopt difference-in-differences (DID) and event study specifications. Here, we compare the outcomes of municipalities that merged (*Treatment*) and municipalities that did not merge (*Control*), as well as comparing the outcomes of municipalities that merged earlier (*Treatment*) and municipalities that merged later (*Control*). In this staggered-adoption DID design, simple two-way fixed effects may be subject to the bias which arises from bad comparison between early merged municipalities and late merged municipalities (Goodman-Bacon 2021). Thus, we adopt recently-developed alternative estimators (Callaway and Sant’Anna 2021, Sun and Abraham 2021) that are robust to this concern as the main specification.

Strikingly, we find that municipal mergers do not improve water quality but rather increase river pollution on average by around 6 percent. This negative effect persists for about 15 years in the event study specification. This is a finding that runs counter to the mechanisms of improved efficiency in public service provision and internalization of pollution spillovers.

This DID specification identifies the causal effect of municipal mergers on water quality under the assumption that the treated municipalities and untreated municipalities would move in parallel trends in the absence of municipal mergers. A plot of raw water quality data comparing never-merged versus merged municipalities shows the parallel pre-trends, and event study plots do not show any differential impact before the mergers. The results are also robust to an alternative specification where we only use variation in merger timings among municipalities that experienced municipal mergers.

Next, we examine whether the internalization of pollution spillovers is plausible by directly testing the negative externality theory suggested in Lipscomb and Mobarak (2016). We follow their specification to exploit the changes in river distances between monitoring stations and municipality borders. Specifically, we examine how river distances from monitoring stations to their closest upstream and downstream municipality borders affect water quality. We find that pollution levels

do not change as a river flows downstream within a municipality, a pattern inconsistent with the negative externality theory. Without the evidence of negative externality theory, municipal mergers are unlikely to internalize pollution spillovers and improve water quality. This result is consistent with negative effects observed in the DID design.

To identify alternative mechanisms of increased water pollution due to municipal mergers, we investigate two mechanisms: (i) weaker pollution control on existing pollution sources due to coordination costs and skewed distribution of political power among pre-merger municipalities at the *intensive* margin, and (ii) new pollution sources due to increased economic activities and change in land use at the *extensive* margin.

As for the intensive margin mechanism, we first test whether municipal mergers with higher coordination costs lead to larger water pollution because insufficient coordination of public services after mergers can weaken pollution control. We show that pollution increased for “equal-footing” mergers that entail higher coordination costs but not for “incorporating” mergers with lower coordination costs (coordination cost channel).² Second, we examine whether incorporated municipalities with smaller political power experience larger water pollution than incorporating municipalities with larger political power, where mayors continue to hold the same position after municipal mergers and thus reduce public service provision outside their electoral base (political economy channel). We find that incorporated municipalities experience water pollution while incorporating municipalities did not experience water pollution.

To identify pollution sources affected by the intensive margin mechanism, we examine the change in operation of domestic wastewater in sewage treatment plants, which are operated under the responsibility of municipalities. We do not find statistically significant changes in the water quality of discharge from sewage treatment plants. This result suggests pollution control efforts did not change after municipal mergers in the case of domestic wastewater. Thus, weaker pollution control has likely affected emissions of industrial wastewater, which is another major pollution source regulated by municipalities through reporting and inspections.

The extensive margin mechanism is not supported in our analysis. We examine the impact of municipal merges on the land use around water quality monitoring stations, but we do not find

² Equal-footing mergers involve mergers between municipalities of similar size, with a new municipality name given after the merger. On the other hand, incorporating mergers involve a larger municipality incorporating other smaller municipalities, with the resulting municipality inheriting the name of the incorporating municipality.

effects on any land use classifications, including agriculture, built-up, forest, and non-use.

Therefore, we conclude that municipal mergers can have unintended negative effects on water quality of rivers when coordination costs among municipalities and neglect of incorporated municipalities weaken pollution control. Future policy discussions of municipal mergers should incorporate these negative effects as potential costs in the cost-benefit analysis. Policymakers also need to keep in mind that the compositions of municipalities matter for public service provision after municipal mergers.

Our paper contributes to the literature in three ways. First, we contribute to the literature on the impact of municipal mergers. Most previous studies investigate the macroeconomic effects of municipal mergers on local public finance and economic activities (Blom-Hansen 2010, Egger et al. 2018, Hinnerich 2009, Hirota and Yunoue 2017, Miyazaki 2018, Moisio and Uusitalo 2013, Pickering et al. 2020, Reingewertz 2012) and determinants of municipal mergers (Weese 2015). We instead focus on the environmental impact of municipal mergers, where only little evidence exists.

One recent relevant paper which examines the environmental impact of border changes is Wang and Wang (2021). They show that township mergers in China internalized the negative externalities of polluting firms and *reduced* emissions by these firms. Contrary to this paper, we show that municipal mergers *increased* pollution and that negative externality theory may not matter in the context of developed countries. Another difference is that we examine long-term impacts of municipal mergers based on 30-year panel data on water pollution, while Wang and Wang (2021) focus on short-run effects over a few years. We show that the negative impact of municipal mergers persist for about 15 years. Another relevant paper is Mizunoya et al. (2021) which examines the impact of municipal mergers on watershed management of the Lake Kasumigaura Basin based on the simulation of a dynamic expanded input-output model. This paper is a case study of one water body and relies on a simulation based on the model. Instead, we examine the causal impact of municipal mergers across Japan based on the quasi-experimental design.

Second, we uncover additional underlying mechanisms of pollution in the “centralization” and “decentralization” processes. Previous studies focus on the negative externality theory to explain the change in pollution by these changes in boundaries in the context of developing countries (Lipscomb and Mobarak 2016, Wang and Wang 2021). We assess this theory under a developed country setting and find that the negative externality theory does not hold in the case of municipal mergers in Japan.

Then, we show alternative mechanisms, i.e., weaker pollution control due to coordination costs and skewed distribution of political power among pre-merger municipalities.

Third, we broadly contribute to the literature on the causes of ambient water pollution and the impact of water pollution regulations. Recent studies have found that ambient water pollution increases with political boundaries (Kahn et al. 2015, Lipscomb and Mobarak 2016, Sigman 2002) and sanitation investment without adequate fecal sludge management (Motohashi 2022). Conversely, several studies have examined the effectiveness of government interventions in reducing pollution, including water pollution regulations in the United States (Keiser and Shapiro 2019) and India (Greenstone and Hanna 2014), and court rulings in India (Do et al. 2018). This paper points to additional causes of ambient water pollution, which is municipal mergers, i.e., decrease with political boundaries.

The rest of the paper is organized as follows. Section 1.2 provides background information on municipal mergers and water quality in Japan. In Sections 1.3 and 1.4, we explain the data and empirical strategy, respectively. Section 1.5 discusses the results. In section 1.6, we analyze the underlying mechanisms behind our results. Section 1.7 concludes.

1.2 Municipal Mergers and Water Quality

1.2.1 The “Great Heisei Municipal Mergers” in Japan

The “Great Heisei municipal mergers” took place in Japan predominantly in the late 1990s to 2000s. They are large-scale municipal mergers that occurred universally in all prefectures in Japan (Figure 1.1). The number of municipalities in Japan drastically decreased by about 50% from 3,238 in 1998 to 1,725 by 2012.

The financial incentives under the revisions of the “Act on Special Provisions of the Merger of Municipalities” in 1995 and 1999 played a key role in fostering these municipal mergers.³ The 1995 revision announced that the purpose of the Act was to push forward municipal mergers and introduced five-year preferential treatment in local tax allocation for merging municipalities. Municipalities, especially those in rural areas, were experiencing dwindling birth rates, aging, and declining population, as well as increasingly difficult fiscal conditions following the burst of the

³ The Act had been in place since 1965, The focus of the Act was originally on facilitating merger procedures.

bubble economy in the early 1990s. Municipal mergers were seen as a tool to rejuvenate such municipalities.

The 1999 revision further strengthen financial incentives. The above preferential treatment in local tax allocation was extended to 10 years. Furthermore, merged municipalities were now allowed to issue “Special Provision Bonds”, for up to 10 years after merging to fund public projects included in their merger proposals. These municipalities were only asked to pay back 30 percent of what they borrowed using these bonds, and the central government paid the remaining 70 percent. The bonds were essentially a 70 percent public project subsidy for merging municipalities. The government initially announced that, in order to be eligible for these benefits, municipalities had to merge by March 2005. However, this was later extended by one year to March 2006 in order to provide more time for municipalities that were struggling to complete mergers by the original deadline.

Thanks to the strong push by these financial incentives, the number of municipal mergers in Japan increased over time from late 1990s. Figure 1.2 illustrates the frequency of municipality mergers since 1995. The first merger occurred in 1999 when financial incentives were strengthened. However, merger counts remained low for the next few years. The vast majority of mergers took place between 2004 and 2006. This not only reflects the fact that it can take several years to complete the merger process but also indicates bunching behavior by municipalities to meet the initial and final deadlines of 2005 and 2006, respectively. Municipal mergers continued even after 2006 until 2011. As discussed here, there is a variation in the timings of municipal mergers. Also, the “Great Heisei municipal mergers” were not mandatory for municipalities, so we have a variation between municipalities that experienced municipal mergers and those that did not. We use these variations in the difference-in-differences and event-study specification as explained in Section 1.4.1.

The municipal mergers under the 1995 and 1999 revisions were not a forced policy. The autonomy of municipalities was respected, and municipalities could choose whether or not to negotiate mergers and which municipalities to negotiate with. The merger process took the following steps. Firstly, “interested” municipalities were to vote and form a panel to discuss potential mergers. Secondly, the panel was to negotiate and formulate merger proposals. Finally, a merger was formally announced and implemented after final voting by the municipalities involved and administrative approval by the prefectural governor and the Minister of Internal Affairs and Communications.

Lastly, municipal mergers can be categorized into two types: “equal-footing” mergers and “incorporating” mergers. The former type involves mergers between municipalities of similar size, with a new municipality name given after the merger. A mayor of the new municipality will be newly elected in the election held after the completion of the municipal merger. On the other hand, “incorporating” mergers involve a larger municipality incorporating other smaller municipalities, with the resulting municipality inheriting the name of the incorporating municipality. A mayor of an incorporating municipality continues to be the mayor of the newly created municipality, while mayors of incorporated municipalities lose their jobs after municipal mergers.

1.2.2 Water Quality and Pollution Control in Japan

Water quality in Japan is regulated by the Environmental Quality Standards for Water Pollution. One important regulated indicator of water quality is BOD (Biochemical oxygen demand). BOD is the amount of dissolved oxygen needed by aerobic biological organisms to break down the organic material present, which captures the overall level of water contamination from various sources such as domestic, agricultural, and industrial wastewater. These standards set the limit values of BOD to be from 1 up to 10 mg/L in rivers, which depends on the usage of water in a given location.

Under these environmental standards, water quality in Japan has generally improved over time. The average BOD of rivers declined from about 3.5 to 1.5 mg/L from 1979 to 2018 (Ministry of the Environment, Government of Japan 2019), which are within the range of limit values of BOD. Thus, our analysis examines whether the magnitude of the improvement in the water quality of rivers becomes larger or not after municipal mergers.

The main sources of water pollution can be categorized into three sources: (i) domestic wastewater (sewage), (ii) industrial wastewater, and (iii) agricultural wastewater. Municipalities in Japan are responsible for controlling pollution from domestic wastewater and industrial wastewater. Domestic wastewater is usually treated in sewage treatment plants which are managed by municipalities. Effluent standards on industrial wastewater are enforced through reporting and inspections by prefectures and designated cities.⁴ On the other hand, agricultural wastewater is a non-point source that is diffused over a wide area as a result of factors such as precipitation, and thus it is

⁴ A designated city is a city that has a population greater than 500,000 and has been designated by the ordinance of the Japanese government under the Local Autonomy Law. A designated city is delegated many of the functions normally performed by prefectural governments.

more difficult to control this source. Municipalities do not have an explicit responsibility to control agricultural wastewater.

1.2.3 How Do Municipal Mergers Affect Water Quality?

Municipal mergers can affect water quality through several channels. One channel which has been examined in the past literature is negative externalities along rivers (Lipscomb and Mobarak 2016). Lipscomb and Mobarak (2016) develop a theoretical framework where pollution within a municipality located upstream of a river adversely affects other municipalities located downstream. They show that decentralization, i.e., having more municipalities along a river path, worsens water pollution. Based on their model, we may expect that municipal mergers, which decrease the number of municipalities along a river path, would improve water quality in rivers.

The key to the negative externality theory is the spatial pattern of river pollution within a municipality. As in Figure A1, which is based on Lipscomb and Mobarak (2016), consider a municipality, which span an area from 0 to 1 on the horizontal axis, that is located along a river with a population uniformly populated in this area. The river flows from 0 to 1, and thus 0 and 1 are the upstream and downstream municipality borders, respectively. A local governor chooses how much economic activity to pursue and hence how many pollutants to emit at each point within his/her municipality. Because the local mayor wishes to minimize the negative impact of emissions on his/her population and does not care about people living in other municipalities, including those downstream, he/she will choose to focus most of the economic activity and emissions near the downstream border, 1. Most of his/her population would be living upstream of this point and would not be adversely affected by emissions that take place at this point.

Thus, the Lipscomb and Mobarak (2016) model predicts, among others, that pollution will increase exponentially as the river flows downstream within a municipality and that there will be a structural break in the slope of the pollution function at the municipality border. The latter prediction follows from the fact that emission is high just upstream of a municipality border but low just downstream of a municipality border. We test the presence of such spatial patterns by using specifications in the spirit of Lipscomb and Mobarak (2016).

In addition to the negative externality theory, we hypothesize that two additional channels,

namely the coordination cost channel and the political economy channel, have weakened pollution control of municipalities. First, as for the coordination cost channel, post-merger surveys and reports indicate lack of organization and solidarity among municipality officials, including difficulty and delay in policy coordination, as negative consequences of the “Great Heisei Municipal Mergers” (Nakazawa and Miyashita 2016, National Association of Towns and Villages 2008). If coordination costs among merging municipalities are high, a newly created municipality will have difficulties in reformulating public services, which were separated by each pre-merger municipality, into newly coherent public services. Thus, the quality of pollution control in merged municipalities with higher coordination costs may deteriorate. In terms of the types of municipal mergers, “equal-footing” mergers between municipalities of similar size are expected to have higher coordination costs than “incorporating” mergers when a large municipality will incorporate other small municipalities. Therefore, we expect that the water pollution is salient in the case of “equal-footing” mergers with higher coordination costs.

Second, as for the political economy channel, municipal mergers can cause skewed distribution of political power between incorporating and incorporated municipalities in the case of “incorporating” mergers. A mayor of an incorporating municipality continues to be the mayor of the newly created municipality, while mayors of incorporated municipalities lose their jobs after municipal mergers. Because the mayor of the newly created municipality has an electoral base in the former incorporating municipality, he or she may prioritize pollution control in that area. Similarly, post-merger, council members of the new municipality may consist mainly of members from the incorporating municipality. Indeed, reports by the National Association of Towns and Villages note that voices of people living in incorporated municipalities were not adequately reflected after mergers took place (National Association of Towns and Villages 2008). As a result, pollution control of incorporated municipalities may be weakened. Thus, we may expect that incorporated municipalities experience larger water pollution due to weaker efforts to control water pollution.

Finally, another channel could be the change in land use, which results from increases in economic activity as suggested by Egger et al. (2018). For example, if enhanced economic activity leads to deforestation and increases in built-up areas, river pollution may increase after mergers due to an increase in industrial wastewater.

1.3 Data

In this paper, we combine novel datasets on ambient water quality and detailed information on municipal mergers to examine the effects of municipal mergers on water quality. We also use geospatial data, including municipality boundaries and river lines, to identify the changes in distances between monitoring stations and municipality borders and test the negative externality theory.

1.3.1 Water Quality

The main outcome variable is water quality. We use data from water quality monitoring stations, which are provided by the Ministry of Environment, Government of Japan. These water quality data are originally collected by each prefecture in Japan under Article 15 of the Water Pollution Prevention Act and reported to the Minister of the Environment. Then, the Ministry of Environment publicizes these reported data on their website. These data include yearly average indicators of water quality, which are measured at monitoring stations in Japan. Among multiple water quality indicators, we use the yearly average BOD (Biochemical oxygen demand) as a representative indicator in the analysis. As afore-mentioned, BOD is a standard measure of water quality that is being monitored under the “Environmental Water-Quality Standard” in Japan. A higher level of BOD values means a higher level of water pollution.

In our analysis, we use a balanced panel data of 3,219 water quality monitoring stations along rivers in Japan from 1990 to 2018 (Figure 1.3). We drop 3,590 stations whose water quality data are partially available during this period to address the concern of endogenous construction of monitoring stations. Thanks to large panel data of thousands of stations across Japan over 30 years, we are able to assess the dynamic impacts of municipal mergers on water quality over a long period.

We complement this dataset with geospatial data of water basins in Japan, provided by the Ministry of Land, Infrastructure, Transport, and Tourism, to identify the basin where a given monitoring station is situated.

1.3.2 Municipal Merger

The key treatment variable in our analysis is an indicator of whether a municipal merger occurred in the municipality where a given monitoring station is located in a given year. Thus, we obtain data on the timing of municipal mergers and on the involved municipalities from the Ministry of Internal Affairs and Communications. The data allow us to compare the change of water quality between merged municipalities and non-merged municipalities, as well as between municipalities that merged earlier and those that merged later.

This dataset also includes information on the types of municipal mergers (“equal-footing” mergers versus “incorporating” mergers), and whether the municipality is incorporating municipality or incorporated municipality in the case of “incorporating” mergers. We use this information to examine the heterogeneous impacts of municipal mergers by merger types in the analysis of the coordination cost channel and political economy channel.

1.3.3 River Distance

To test the negative externality theory as in Lipscomb and Mobarak (2016), we calculate the changes in distances between monitoring stations and municipality borders.

As for data sources, we obtain pre-merger (1984) and post-merger (2018) municipality boundary data, provided by Kirimura et al. (2011). We also obtain detailed river node data with elevation information, as well as river line data, provided by the Ministry of Land, Infrastructure, Transport, and Tourism. Lastly, we use the locations of water monitoring stations in the water quality data.

In this analysis of negative externality theory, we construct two distance variables, U and D , in the spirit of Lipscomb and Mobarak (2016). U refers to the distance along the river from a monitoring station to its closest upstream municipality border. Likewise, D indicates the river distance between a monitoring station and its closest downstream border. U and D require calculating distances along rivers instead of Euclidean distance and judging flow direction.

For constructing U and D , we use the river node data, which contains elevation information, to compute a fine-resolution elevation raster data. Based on this elevation data, we identify the upstream-downstream relationships among monitoring stations and municipality borders. Then, we

calculate these two distance variables along rivers.⁵

1.3.4 Other Municipality Characteristics

We supplement the above information with further data to account for municipality characteristics that might affect both water quality and the likelihood of municipal mergers. Specifically, we use an economic indicator and population.

As an economic indicator, we use “Product shipment values” from 1990 to 2012 from the Census of Manufacture, provided by the Ministry of Economy, Trade, and Industry.

We also use population data from the Census. Because the Census is conducted every five years, we compute the yearly population from 1990 to 2015 based on the linear interpolation of the reported population in 1995, 2000, 2005, 2010, and 2015.

1.3.5 Data Matching and Sample Construction

For matching water quality data with municipal merger data, we use municipality boundary GIS data provided by the Ministry of Land, Infrastructure, Transport, and Tourism. We first use this boundary data and the GPS coordinates of monitoring stations to identify the latest prefecture and city names of these stations. Then, we match both data based on the latest prefecture and city names. All other data, including water basins and other municipality characteristics, are similarly merged based on the latest prefecture and city names.

After data matching, we construct a balanced panel of 3,219 water quality monitoring stations in 936 municipalities from 1990 to 2018 for the difference-in-differences specification. The main specification in the analysis uses panel data from 1996, 5 years before the first municipal merger was observed in our dataset in 2001. We also conduct robustness checks by using alternative sample periods, (i) 1990-2018 and (ii) 1999-2018, which shows similar results (Figure A4).

In the specification that uses river distance to test the negative externality theory, we only use monitoring stations that are situated along the river lines that are used in our data construction. We also exclude monitoring stations that are situated either in uppermost cities or downstream cities because one of the distance measures (U or D) cannot be calculated. We use observations until 2015 since we use population as a control, and population data are available until 2015. Then,

⁵ Detailed steps are written in the Appendix 4.1.3.4.

the final sample for the river distance specification becomes 700 monitoring stations from 1996 to 2015.

1.3.6 Summary Statistics

Summary statistics of all variables are shown separately for pre-merger and post-merger periods in Table 1.1. In the pre-merger periods, merged municipalities (treatment group) are different from non-merged municipalities (control group) in most variables, including product shipment values and population. Although large level differences are observed, we rely on the assumption of parallel trends in the difference-in-differences specification. We test this assumption later in Section 1.4.1 and Section 1.5.1. Also, BOD levels decreased in post-merger periods both in treatment and control groups. However, the magnitude of decrease in BOD level is smaller in the treatment group, which implies that municipal mergers may have caused water pollution. A formal econometric analysis of this impact is conducted in the following sections.

1.4 Empirical Strategy

We examine the impact of municipal mergers on water pollution in two ways. First, we directly assess the impact of municipal mergers on water pollution by adopting a difference-in-differences model with two-way fixed effects. Simple ordinary least squares (OLS) estimates are subject to bias due to potential endogeneity through potential reverse causality and omitted variables. Municipal mergers could be implemented to better mitigate the water pollution in those municipalities, although this could be highly implausible because the decisions of municipal mergers are said to be primarily based on macroeconomic conditions such as local government budgets. Also, spurious correlations may be caused by omitted unobservables such as different priorities on water pollution control across municipalities. Thus, we adopt a difference-in-differences model to deal with potential endogeneity. By including monitoring station fixed effects and basin-year fixed effects, we control for the time-invariant unobservable differences across monitoring stations and secular time trends which may vary across river basins.

Second, using a specification similar to Lipscomb and Mobarak (2016), we test whether relationships between locations along the river and pollution predicted by the negative externality

theory hold in the case of Japan. Similar to the difference in difference specification, we include station-fixed effects and basin-year fixed effects to control for station-specific characteristics that are constant over time as well as annual trends that vary across river basins.

1.4.1 Difference-in-Differences Specification

We first adopt a difference-in-differences model with two-way fixed effects:

$$\ln(BOD_{i,t}) = \alpha + \beta_1 Merger_{i,t} + \lambda X_{i,t} + \delta_i + \theta_{b,t} + \varepsilon_{i,t} \quad (1.1)$$

where the dependent variable, $\ln(BOD_{i,t})$, is a log of BOD at monitoring station i in year t . $Merger_{i,t}$ is an indicator variable which switches on and stays on for all subsequent years when a merger takes place in the municipality associated with station i . $X_{i,t}$ is a vector of time-varying control variables, which include municipality-level economic indicator and population. Given the “bad control” concerns of population, which may be affected by municipal mergers, we only include an economic indicator as control in our main specifications. We include monitoring station fixed effects (δ_i), which control for time-invariant characteristics of each monitoring station, including positions (downstreamness) along rivers. To account for any secular trends in water quality across years, which may vary across river basins, we include year dummies interacted with indicators for each of the river basins b ($\theta_{b,t}$). Lastly, standard errors are clustered at municipality-level, while we also adopt conservative clustering at basin-level for a robustness check.

The coefficient of particular interest is β_1 . If municipal mergers decrease river pollution, as negative externality theory would suggest, then β_1 should be negative. On the other hand, β_1 could be positive if negative externality theory does not hold, and high coordination costs and skewed distribution of political power among pre-merger municipalities cause an increase in river pollution.

Note that we examine a staggered policy: municipal mergers took place in different years. Thus, the estimate of β_1 in the regression (1.1) is a weighted average of all possible two-group/ two-period difference-in-differences estimators (Goodman-Bacon 2021). In other words, the estimate reflects all possible cases with different definitions of treatment and control groups. One case could be comparing monitoring stations in municipalities that experienced mergers (treatment group) with monitoring stations in municipalities that never experienced mergers (control group). Another case

could be comparing monitoring stations in municipalities that experienced mergers in early years (treatment group) with monitoring stations in municipalities that experienced mergers in later years (control group).

The difference-in-differences specification hinges on the parallel trend assumption between treatment and control groups. A simple comparison of evolution of BOD levels between the treatment and control groups already encouragingly shows signs of parallel pre-trends (Figure 1.4). To empirically test the parallel trend assumption, we adopt a following event-study specification. This specification also allows us to examine the long-run dynamic impacts of municipal mergers up to 17 years.

$$\ln(BOD_{i,t}) = \alpha + \sum_{\tau=-15}^{17} \beta_{\tau} Merger_{\tau,i} + \lambda X_{i,t} + \delta_i + \theta_{b,t} + \varepsilon_{i,t} \quad (1.2)$$

where $Merger_{\tau,i}$ is an indicator for each year τ relative to the municipal merger, where τ is normalized to equal zero in the year that the municipal merger was implemented. τ ranges from -15 to 17 in our sample years. For monitoring stations where municipal mergers did not take place, all $Merger_{\tau,i}$'s are set equal to zero. $\tau = -1$ is set as a reference year.

The β_{τ} 's are the parameters of interest. β_{τ} 's should be around zero and statistically insignificant from $\tau = -15$ to $\tau = -2$ to satisfy parallel pre-trends. After $\tau = 0$, β_{τ} 's capture the dynamic treatment effects in the short and long runs.

Recent literature indicates that, in the case of staggered difference-in-differences designs where treatment effects vary over time, standard two-way fixed effect regression estimates and event study estimates are subject to bias. As mentioned above, two-way fixed effect estimates are weighted averages of all possible two-group/ two-period difference-in-differences estimators. However, comparisons between early merger group (as control group) and late merger group (as treatment group) becomes problematic if treatment effects vary over time, i.e., when we have a trend-break rather than a level shift. This comparison leads to bias from negative weights (Callaway and Sant'Anna 2021, Goodman-Bacon 2021, Sun and Abraham 2021). To overcome this issue and obtain unbiased estimates, we apply alternative estimator of Callaway and Sant'Anna (2021) that is robust to negative weights as a main specification throughout this paper.⁶ We present the results of Callaway

⁶ We also adopt an estimator of Sun and Abraham (2021) as a robustness check, which leads to same results as a Callaway and Sant'Anna (2021) estimator and two-way fixed effects specification (Figure A3).

and Sant’Anna (2021) estimator together with the results of two-way-fixed effects specification of the regressions (1.1) and (1.2).

Finally, the main specification includes comparison between never-merged municipalities and merged municipalities, where selection bias can be an issue. Given that the primary purpose of municipal mergers is the improvement of any public service provision and local finance, the selection bias is perhaps not a large concern in this setting. However, as a robustness check, we run the same regressions (1.1) and (1.2) by restricting our sample to monitoring stations in municipalities that experienced mergers, and by considering municipalities that merged early as treatment group and municipalities that merged later as control group. These municipalities should be more similar and balanced in characteristics because both groups experienced municipal mergers, which are less susceptible to the concern of selection bias. We show that the results are robust to this specification in Section 1.5.1.

1.4.2 River Distance Specification

To formally test the negative externality theory predictions, we investigate spatial patterns of pollution using the following specification:

$$\ln(BOD_{i,t}) = \alpha + \eta_1 \cdot Downstream_{i,t} + \eta_2 \cdot Downstream_{i,t}^2 + \lambda X_{i,t} + \delta_i + \theta_{b,t} + \varepsilon_{i,t} \quad (1.3)$$

We first obtain distances along the river from monitoring station i to its closest upstream municipality border in year t ($U_{i,t}$), and to its closest downstream municipality border ($D_{i,t}$), as outlined in Section 1.3. We then construct a variable $Downstream_{i,t} = U_{i,t}/(U_{i,t} + D_{i,t})$. $Downstream_{i,t}$ takes a value between 0 and 1, and measures the relative “downstreamness” of a monitoring station within its municipality. The closer the value is to 1, the more downstream a station is within its municipality. As before, $X_{i,t}$ is a vector of control variables, consisting of municipality-level GDP and population. Likewise, δ_i and $\theta_{b,t}$ are station fixed effects and basin-year fixed effects respectively. Standard errors are clustered by station.

The coefficients of prime interest here are η_1 and η_2 . Negative externality theory predicts that pollution rises at an increasing rate as we go downstream along a municipality, as illustrated in Figure A1. In such a case, the first and second derivatives with respect to “downstreamness” would

both be positive, and we expect a convex relationship with $\eta_1 > 0$ and $\eta_2 > 0$.

A specification following Lipscomb and Mobarak (2016) is also considered:

$$\ln(BOD_{i,t}) = \alpha + \gamma_1 \cdot U_{i,t} + \gamma_2 \cdot U_{i,t}^2 + \gamma_3 \cdot D_{i,t} + \gamma_4 \cdot D_{i,t}^2 + \lambda X_{i,t} + \delta_i + \theta_{b,t} + \varepsilon_{i,t} \quad (1.4)$$

where the variables are as explained above. According to negative externality theory, pollution should increase exponentially as the distance from the downstream border gets smaller (i.e., the further downstream within the municipality), so we expect $\gamma_3 < 0$ and $\gamma_4 > 0$. Also, the existence of a structural break in the pollution function at the municipality border implies that γ_1 should be different from γ_3 (Lipscomb and Mobarak 2016).

1.5 Results

1.5.1 Difference-in-Differences Specification

The results of the difference-in-differences specification are reported in Table 1.2. Panel A uses the Callaway & Sant’Anna estimator, and is our preferred panel. The results for standard two-way fixed effect analyses are provided for reference in Panel B. Looking at our baseline specification (Column 1) for Panel A, we find that municipal mergers do not improve water quality, but rather increase water pollution on average by around 5.7%. The estimates are statistically significant at 1% level. This is in stark contrast to what the negative externality theory and results of Lipscomb and Mobarak (2016) would suggest. The results are robust to more conservative clustering at basin level (Column 2). They are also robust when we include population in the regression, although population can be “bad control” that is affected by the treatment (Column 3).

The result of the event study specification, using the Callaway and Sant’Anna (2021) estimator, is shown in Figure 1.5. The estimates for pre-merger years are largely insignificant, which suggests that parallel pre-trends holds. After period 0, which corresponds to the beginning of municipal mergers, statistically significant impacts of increased water pollution are observed. The point estimates increase over time and remain statistically significant for about 15 years, which suggests that the negative impacts on water quality exacerbated in the short to medium run after the merger. The point estimates decrease slightly and become statistically insignificant after 15 years, which could suggest that the increase in pollution is being mitigated in the long-term. However, it should be noted that estimates become imprecise over time with larger standard errors. These results are

due to the smaller sample sizes in the later periods, which reflects the impacts of water monitoring stations that experienced mergers earlier.

These results are robust to various specifications. First, the results are robust to alternative estimators (Figure A3) and alternative sample periods (Figure A4). Second, to address the concern of selection bias, we conduct the same analysis only on the sample of municipalities that experienced mergers over the sample period. An event study plot (Figure A2) also points to increasing water pollution due to municipal mergers over time, especially 6 years after municipal mergers. Difference-in-differences results in Table B1 do not show statistically significant effects of increased pollution, probably because of shorter periods covered in this specification than in the main specification. However, the sign of the coefficients remains positive as in the main specification (Columns 1-2).

1.5.2 River Distance Specification

Table 1.3 reports the results of the spatial analysis that uses the changes in river distances between monitoring stations and municipality borders. First, the results on the relative downstreamness (regression (1.3)) are provided in columns 3 and 4. The coefficients on both downstream indicator and its squared term are both statistically insignificant. Also, the sign of the coefficient for the downstream indicator is positive, which is contrary to the prediction by the negative externality theory.

Second, results for the Lipscomb and Mobarak (2016) specification (regression (1.4)) in Columns 1 and 2 are not supportive of the negative externality channel either. In our preferred specification with basin-year fixed effects (column 2), the coefficient of D is again positive and not statistically significant. Furthermore, we cannot reject the equality of the coefficients of U and D when we test the structural break in the pollution function at the municipality border.

In sum, the negative externality channel of Lipscomb and Mobarak (2016) does not hold true in the case of municipal mergers in Japan. This may be partly due to differences between developing/emerging countries like Brazil and developed countries like Japan. In a developed country case, it may be more difficult or costly to relocate polluting sources (to internalize any externality) after municipal mergers, where for instance, property rights are more rigid, and there is less available land area for relocation. Null impacts of municipal mergers on land in Section 1.6.2 are consistent with this explanation.

1.6 Mechanisms

We find that municipal mergers had negative impacts on water quality and that the negative externality channel is not relevant in our setting. These negative impacts of municipal mergers can be caused by the following two potential mechanisms.

First, municipal mergers can weaken pollution control on existing pollution sources, such as industrial wastewater from existing factories and domestic wastewater from existing sewage treatment plants, at the *intensive* margin. A failure to coordinate effectively (coordination cost channel), or neglect of small, incorporated areas after municipal mergers (political economy channel), could weaken pollution control on these existing sources. We investigate the evidence of this *intensive* margin channel by analyzing the heterogeneous effects of municipal mergers, as well as examining the change in pollution control in sewage treatment plants.

Second, municipal mergers can increase economic activities as Egger et al. (2018) showed, which could worsen pollution as a result of new emitting sources along rivers, i.e., an increase in pollution at the *extensive* margin. This may, for instance, involve turning forest areas near rivers into industrial and residential areas, which would lead to increases in water pollution from industrial and domestic wastewater. We test this *extensive* margin channel by examining the impact of municipal mergers on land use patterns.

1.6.1 Intensive Margin: Change in Pollution Control on Existing Pollution Sources

1.6.1.1 Coordination Costs

We test the coordination cost channel by comparing the case of “equal-footing” mergers which entail higher coordination costs, with “incorporating” mergers which have lower coordination costs. We expect that “equal-footing” mergers result in larger water pollution because of higher coordination costs. We estimate the impact of “equal-footing” mergers by the regression (1.1) after only keeping the municipalities that have experienced “equal-footing” and never treated municipalities. The same procedure is repeated for the case of “incorporating” mergers.

Columns 1 and 2 in Table 1.4 reports the impacts of municipal mergers of both types. We find a statistically significant negative impact in the case of “equal-footing” mergers (Column 1). On

the other hand, the impact becomes less salient in the case of “incorporating” mergers (Column 2). The effect is statistically significant only at 10% level, and the magnitude of the effect is reduced to 4.6%. As we have expected, the negative impacts are more salient in the case of “equal-footing” mergers, which entail higher coordination costs among pre-merger municipalities.

1.6.1.2 Political Economy

We also test the political economy channel by comparing the change in water quality of incorporating municipalities with that of incorporated municipalities. We expect that incorporated municipalities experience larger water pollution due to neglect by a mayor originally from the incorporating municipality. We estimate the impact of municipal mergers in incorporating municipalities by the regression (1.1) after only keeping the incorporating municipalities and never treated municipalities. The same procedure is repeated for the case of incorporated municipalities.

Columns 3 and 4 in Table 1.4 report the results. Incorporated municipalities experience a larger increase in water pollution by about 9%, and this result is statistically significant at 1% level (Column 3). On the other hand, we find no effect for the incorporating municipalities (Column 4). This difference in results aligns with the argument of the political economy channel where incorporated municipalities incur negative effects.

1.6.1.3 Pollution Control on Wastewater in Sewage Treatment Plants

In the coordination cost and political channels, we show that municipal mergers can weaken water pollution control. Concretely, this could take the form of insufficient treatment of domestic wastewater in sewage treatment plants and industrial wastewater in factories, which are under the responsibility of municipalities (Section 1.2.2). In an effort to explicitly test the change in pollution control, we examine the change in the quality of treatment of domestic wastewater at sewage treatment plants.

Treated wastewater from sewage treatment plants is discharged into rivers. Hence, if merged municipalities reduce their efforts to treat wastewater, water quality in rivers may deteriorate. This weaker pollution control can occur in the form of delaying the renewal of aging sewage treatment plants, which can inhibit sufficient wastewater treatment at these plants.⁷

⁷ The aging of sewage treatment plants is a growing issue in Japan. More than 80% of sewage treatment plants

To test this possibility, we compute the BOD values of effluent and influent⁸ from sewage treatment plants from 1996 to 2017 in Japan. We use the Sewage Statistics compiled by the Japan Sewage Works Association and aggregate plant-level data into municipality-level data by computing the mean of BOD values. In this analysis, we adopt the difference-in-differences specification that is similar to the main specification in Section 1.4.1, although we include municipality fixed effects and cluster the standard errors at the municipality-level.

Table B3 and Figure A6 report the results of this analysis. We do not find statistically significant impacts of municipal mergers on effluent water quality, which suggest that there has not been a weaker effort in pollution control by delaying renewal. The placebo check, which looks at the impact on influent water quality, also shows null effects as expected. These null results of sewage treatment plants suggest that municipal mergers may have caused weaker pollution control on other pollution sources, i.e., either industrial wastewater or agricultural wastewater. However, as discussed in 1.2.2, agricultural wastewater is a non-point source that is difficult to be controlled by the municipality's policy.

Hence, the results from this analysis on sewage treatment plants, combined with the characteristics of agricultural wastewater as well as the relevance of the coordination cost and political economy channels, offer indirect and suggestive evidence that the most likely source of weaker pollution control is industrial wastewater.

1.6.2 Extensive Margin: New Pollution Sources Due to Change in Land Use

We examine the impact of municipal mergers on land use patterns. This analysis is based on the 100-meter raster data of land use in 1991, 1997, 2006, 2009, 2014, and 2016 from the dataset of the Ministry of Land, Infrastructure, Transport, and Tourism. We focus on the land use pattern within the 150-meter buffer from each monitoring station. Land use is categorized into 4 classifications: agriculture, forest, built-up, and non-use. Then, we construct the share of each land use classification and the binary indicator of which land use classification is major for each monitoring station.⁹

Based on the constructed dataset on the shares and binary indicators of 4 land use classifications

have been operating for 15 years since their inceptions and need to be renewed to maintain the quality of wastewater treatment.

⁸ Effluent is wastewater flowing out of a sewage treatment plant, while influent is wastewater entering a sewage treatment plant.

⁹ Detailed steps are written in the Appendix 4.1.3.6.

over 6 periods, we adopt the same difference-in-differences approach as the main specification in section 1.4.1. In this case, the panel data does not cover consecutive years, so we cannot use the Callaway and Sant’Anna (2021) estimator, which requires a balanced panel with consecutive years. Thus, we use the two-way fixed effects specification and Sun and Abraham (2021) estimator.

As shown in Table B2 and Figure A5, we do not find statistically significant impacts of municipal mergers on land use patterns (agriculture, built-up, forest and non-use). Thus, the water pollution induced by municipal mergers cannot be explained by this extensive margin channel.

1.7 Conclusion

We examine the environmental impact of municipal mergers, where two or more municipalities combine to form one municipality. Although municipal mergers are pursued to improve efficiency and quality of public service provision, we find that municipal mergers aggravate water pollution.

To document this negative environmental impact, we examine the case of Japan’s large-scale municipal mergers in the late 1990s to 2000s, which drastically reduced the number of municipalities by about 50%. Based on 30-year water quality data on more than 3,000 monitoring stations and detailed information on municipal mergers, we investigate the long-run impacts of municipal mergers on water quality.

We adopt two main identification strategies. First, we use variations in the occurrence and timings of municipal mergers to adopt the difference-and-differences and event study specifications. Because simple two-way-fixed effects specification is susceptible to bias in estimates due to negative weights, we also adopt alternative estimators that are robust to this bias. Second, we use the Lipscomb and Mobarak (2016) specification, which directly tests pollution spillovers along rivers based on negative externality theory. Specifically, we use the change in distances from monitoring stations to their closest upstream and downstream municipality borders along rivers.

Strikingly, we find that municipal mergers increase river pollution by around 6 percent, and this negative effect persists for about 15 years. We also find no evidence of negative externality theory in the river distance specification.

To answer why pollution increased due to municipal mergers, this paper shows weaker pollution control due to coordination cost and political economy channels. Heterogeneity analysis by merger

type shows that pollution increased in “equal-footing” mergers with higher coordination costs but not so much for “incorporating” mergers with lower coordination costs. We further show that in the case of “incorporating” mergers, incorporated municipalities experience a larger increase in water pollution than incorporating municipalities, whose political power is higher because a mayor there continues to hold the same position after municipal mergers. The paper finally investigates the possibility of weaker pollution control in the case of wastewater treatment in sewage treatment plants. The null result of this analysis indirectly suggests that the river pollution may be a result of weaker pollution control on industrial wastewater, which is another major source of wastewater that can be controlled by municipalities.

Our results have several implications for policy and research direction on municipal mergers. First, while proponents emphasize positive impacts on macroeconomic outcomes such as local public finance, municipal mergers can have unintended negative impacts on environmental outcomes. These negative effects should be carefully considered in the cost-benefit analysis of future municipal mergers.

Second, the negative externality narrative of river pollution may not be as relevant in a developed country’s case of municipal mergers as in a developing country’s case of decentralization. Our results point to the relative significance of coordination cost and political economy channels in the case of Japan. Conducting careful pre-analysis of likely relevant channels and designing relevant countermeasures on a country-by-country basis would be important in mitigating any negative impact of municipal mergers on water quality. Investigating why the relative importance of each channel differs between developed and developing countries may be an interesting area for future research.

Finally, the negative effects we observed can be similarly present in other policies managed by municipalities, including educational investment and healthcare. The coordination cost and political economy channels can play a role in weakening the implementation of these policies. This would be an interesting topic to be carefully examined in future studies.

1.8 Figures and Tables

1.8.1 Figures

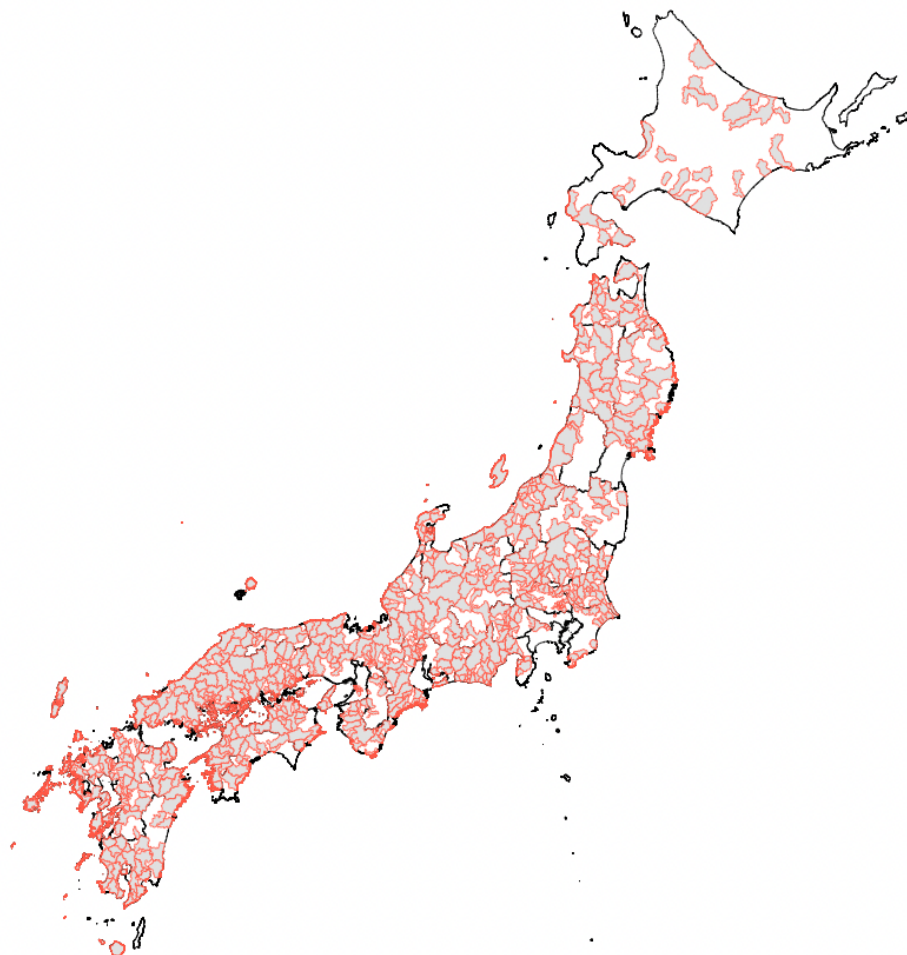


Figure 1.1: Locations of Municipal Mergers in Japan

Notes: This figure shows the boundaries of municipalities which experienced municipal mergers (red lines surrounding grey areas) and the boundaries of prefectures in Japan (in black).

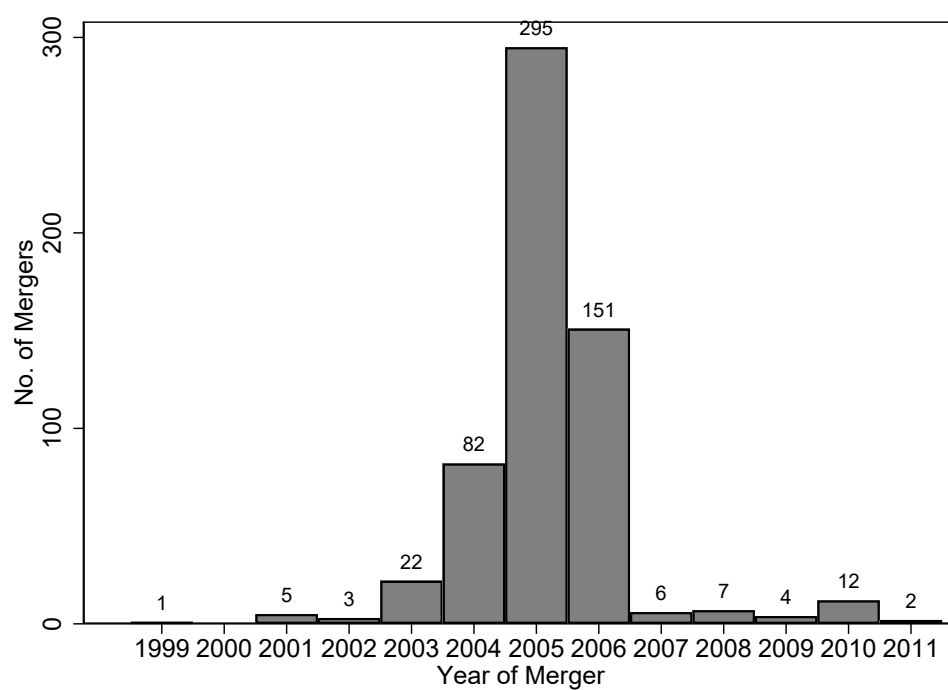


Figure 1.2: Timing of Municipal Mergers in Japan

Notes: This figure shows the number of municipal mergers by year in Japan based on the merger data of the Ministry of Internal Affairs and Communications. In our analysis, we only use variation of merger years from 2001 to 2011 based on our sample of municipalities along rivers.

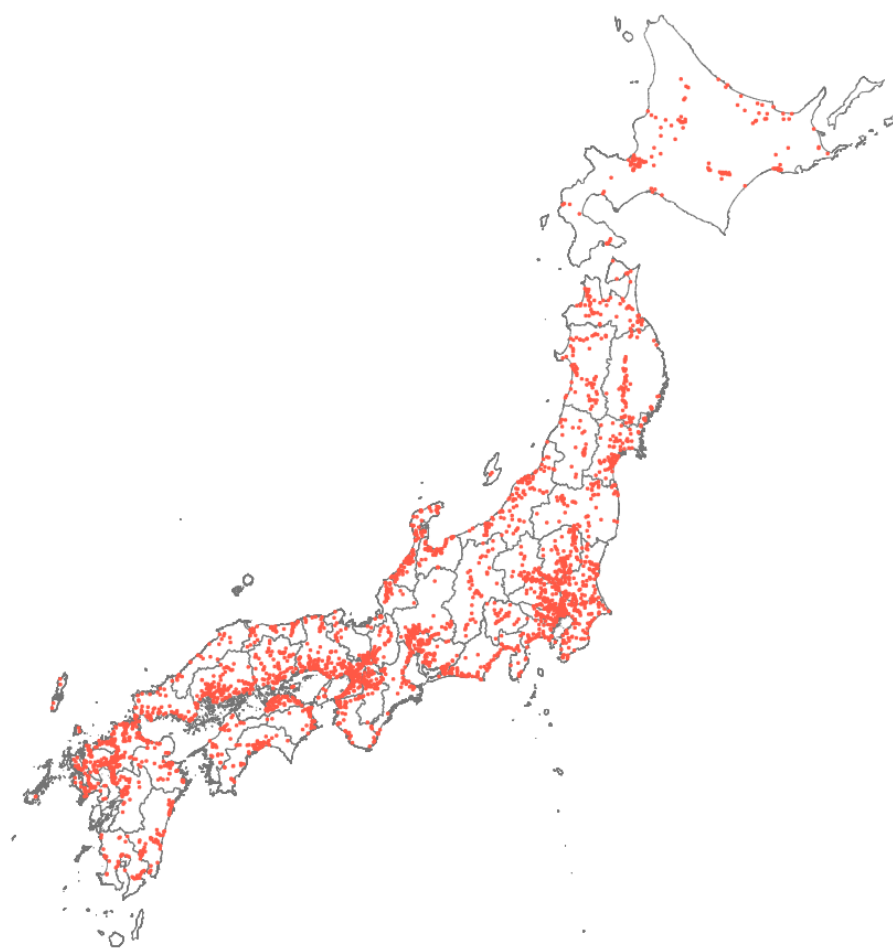


Figure 1.3: Locations of Water Quality Monitoring Stations in Japan

Notes: This figure shows the locations of water quality monitoring stations (in red) which are included in our sample along rivers based on the data of Ministry of Environment. Boundaries of prefectures in Japan are also shown (in black).

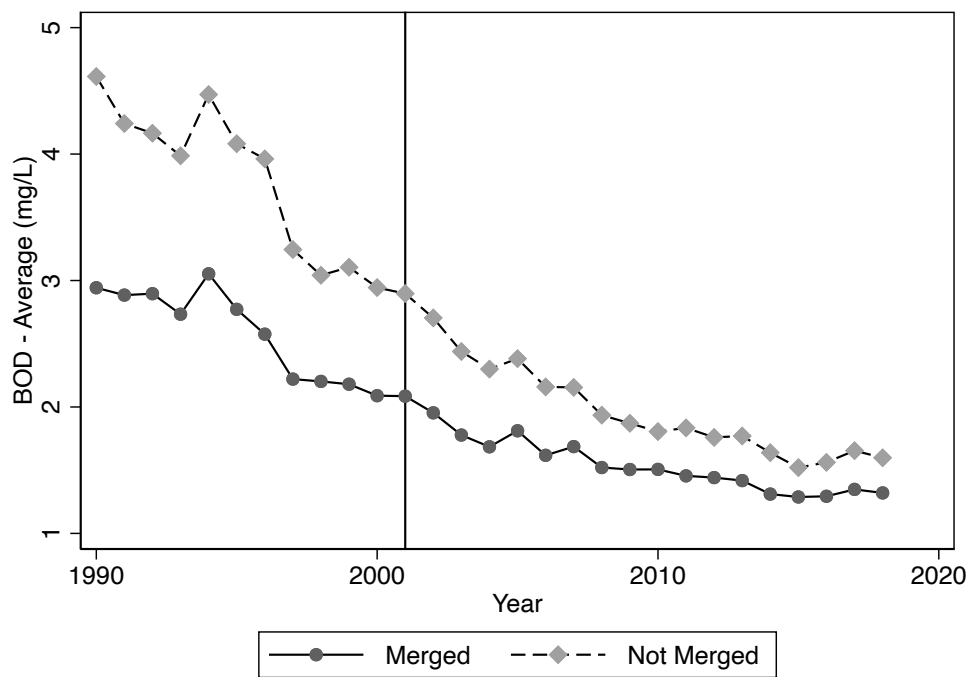


Figure 1.4: Trends of BOD level in Treatment and Control groups

Notes: This figure compares the changes in average BOD values (mg/L) from 1990 to 2018 between the municipalities which experienced municipal mergers (Merged) and the municipalities which did not experience the mergers (Not Merged). The vertical in 2001 shows the year when the first municipal mergers took place in the our sample.

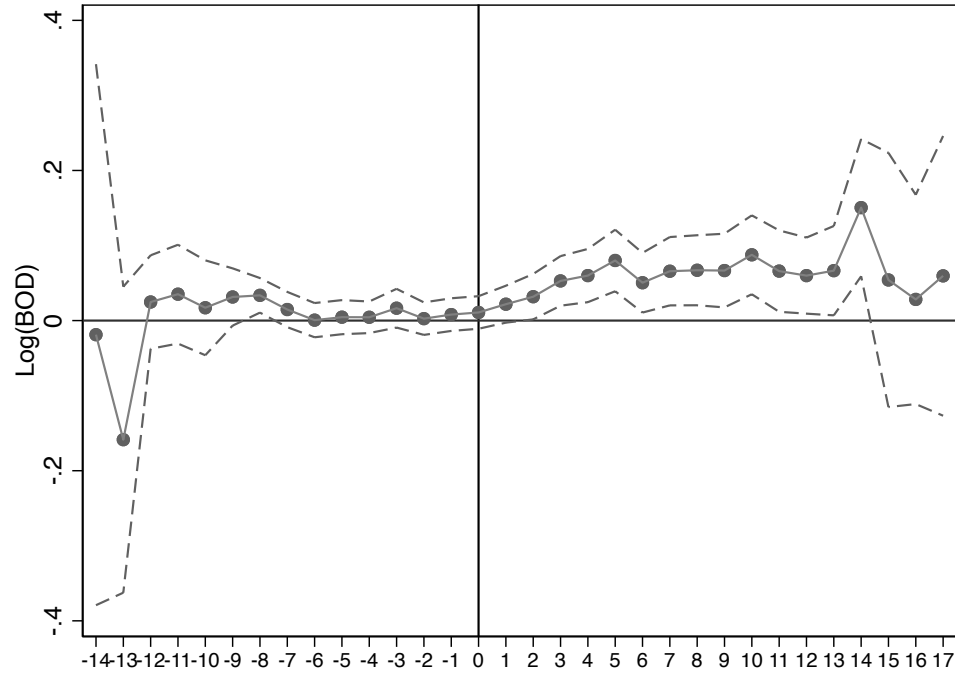


Figure 1.5: The Dynamic Impact of Municipal Mergers on Water Pollution

Notes: This figure shows the coefficients of the estimators according to the Callaway and Sant'Anna (2021) methodology. The 95% confidence intervals are shown with dashed lines. Standard errors are clustered at the municipality level. This specification includes monitoring station fixed effects, year fixed effects, and product shipment values as a control.

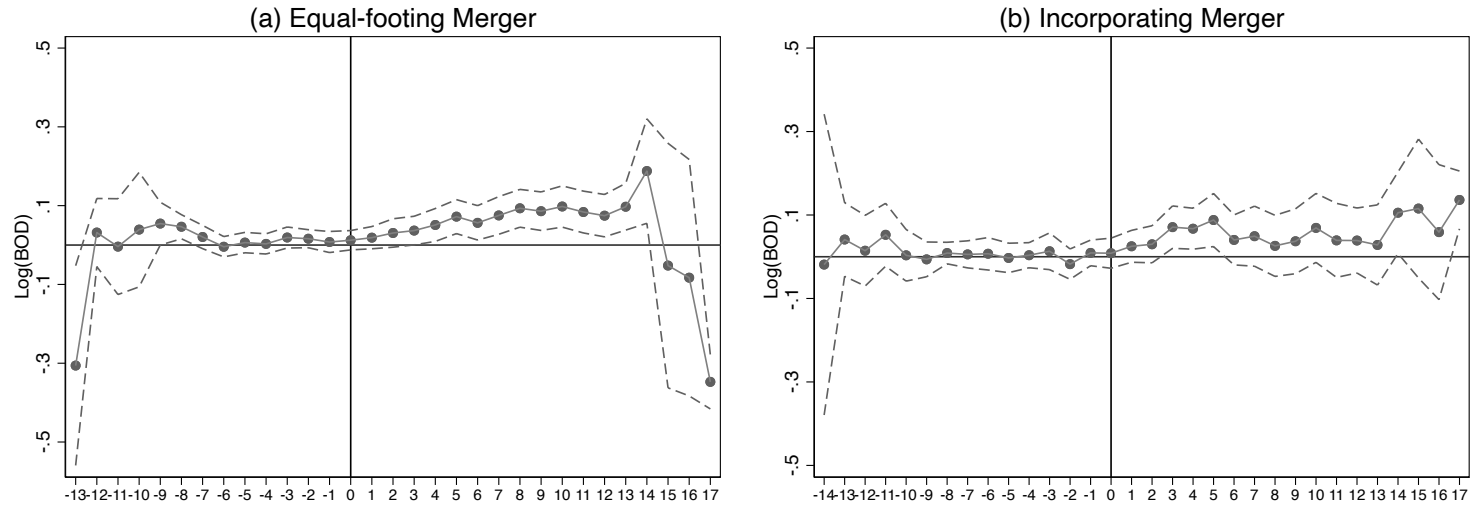


Figure 1.6: Mechanism on Coordination Cost: Event Study Results

Notes: These figures show the coefficients of the estimators according to the Callaway and Sant'Anna (2021) methodology. Panel (a) shows the impact of equal-footing mergers based on the data of these municipalities and never-merged municipalities, while panel (b) similarly shows the impact of incorporating mergers. The 95% confidence intervals are shown with dashed lines. Standard errors are clustered at the municipality level. All specifications include monitoring station fixed effects, year fixed effects, and product shipment values as a control.

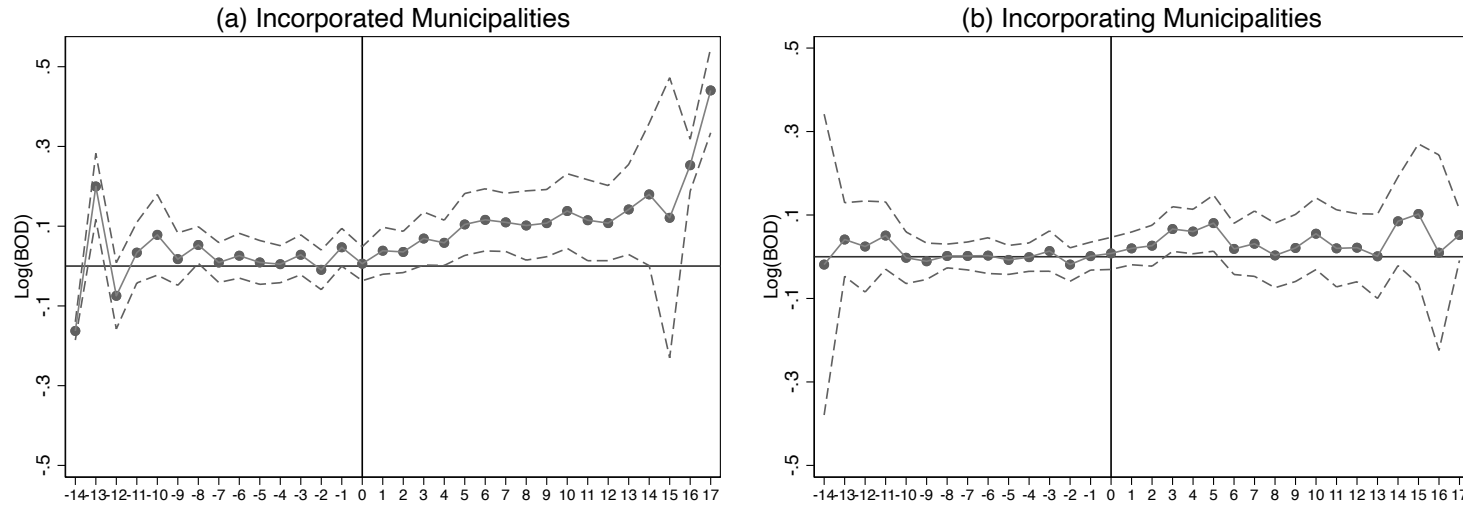


Figure 1.7: Mechanism on Political Economy: Event Study Results

Notes: These figures show the coefficients of the estimators according to the Callaway and Sant'Anna (2021) methodology. Panel (a) shows the impact of mergers in incorporated municipalities based on the data of these municipalities and never-merged municipalities, while panel (b) similarly shows the impact of mergers in incorporating municipalities. The 95% confidence intervals are shown with dashed lines. Standard errors are clustered at the municipality level. All specifications includes monitoring station fixed effects, year fixed effects, and product shipment values as a control.

1.8.2 Tables

Table 1.1: Summary Statistics

Variable	Means		Difference	Obs.
	Not Merged	Merged		
<i>Panel A. 1990-2000</i>				
BOD - Yearly average (mg/l)	3.805 (5.172)	2.595 (4.956)	-1.210*** (0.264)	35,706
Distance from upstream border to station (km)	4.402 (4.741)	4.161 (4.899)	-0.241 (0.461)	7,700
Distance from station to downstream border (km)	3.518 (4.174)	3.914 (4.287)	0.396 (0.429)	7,700
Downstream indicator (0-1)	0.554 (0.278)	0.533 (0.272)	-0.021 (0.022)	7,700
Product shipment values (100 billion JPY)	2.553 (6.197)	2.974 (5.775)	0.421*** (0.117)	10,461
Population (thousand)	100.124 (268.369)	111.504 (156.001)	11.380*** (4.366)	10,285
Major land use around stations: Agriculture (0/1)	0.382 (0.486)	0.455 (0.498)	0.072*** (0.026)	10,804
Major land use around stations: Forest (0/1)	0.139 (0.346)	0.227 (0.419)	0.088*** (0.020)	10,804
Major land use around stations: Buildup (0/1)	0.458 (0.498)	0.311 (0.463)	-0.147*** (0.031)	10,804
BOD - Effluent from Sewage Treatment Plants (mg/l)	5.426 (3.228)	7.537 (34.439)	2.111 (1.753)	1,084
<i>Panel B. 2001-2018</i>				
BOD - Yearly average (mg/l)	1.999 (2.095)	1.557 (1.700)	-0.442*** (0.098)	58,428
Distance from upstream border to station (km)	4.402 (4.741)	6.053 (8.252)	1.651** (0.775)	10,500
Distance from station to downstream border (km)	3.518 (4.174)	5.668 (7.622)	2.149*** (0.679)	10,500
Downstream indicator (0-1)	0.554 (0.278)	0.531 (0.261)	-0.023 (0.021)	10,500
Product shipment values (100 billion JPY)	2.209 (4.963)	3.066 (7.284)	0.857*** (0.095)	17,118
Population (thousand)	104.229 (285.869)	110.711 (161.852)	6.482 (3.958)	14,025
Major land use around stations: Agriculture (0/1)	0.266 (0.442)	0.332 (0.471)	0.066*** (0.025)	10,974
Major land use around stations: Forest (0/1)	0.132 (0.338)	0.217 (0.412)	0.085*** (0.019)	10,974
Major land use around stations: Buildup (0/1)	0.587 (0.492)	0.442 (0.497)	-0.145*** (0.031)	10,974
BOD - Effluent from Sewage Treatment Plants (mg/l)	4.120 (3.032)	3.716 (3.477)	-0.404*** (0.102)	4,607

Notes: Panel A compares the summary statistics between merged municipalities and non-merged municipalities before municipal mergers started in our sample (before 2001), while panel B compares the summary statistics since 2001 when the first mergers are observed in our data. The unit of observations of product shipment value, population, and BOD of effluent is the municipality, but it is the station for other variables. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The standard errors of differences are clustered at municipality-level when the unit of observations is the station.

Table 1.2: DID Results - The Impact on Water Quality

	Log(BOD)		
	(1)	(2)	(3)
Panel A: Callaway & Sant'Anna (2021) Estimator			
Merger (= 1)	0.057*** (0.018)	0.057*** (0.018)	0.050*** (0.016)
Panel B: Two-way Fixed Effects			
Merger (= 1)	0.040** (0.018)	0.040 (0.024)	0.036** (0.017)
Observations	74,037	74,037	63,460
R ²	0.881	0.881	0.889
Number of Stations	3,219	3,219	3,173
Number of Municipalities	936	936	920
Clustered SE at...	Municipality	Basin	Municipality
Station FE	YES	YES	YES
Product Shipment Values	YES	YES	YES
Population	NO	NO	YES
Mean of Dep. Variable	2.659	2.659	2.619

Notes: The coefficients are reported. Standard errors are in parentheses and clustered at municipality-level in columns 1 and 3 and basin-level in column 2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All specification in panel A include year fixed effects, while all regressions in panel B include basin-year fixed effects. Mean dependent variables are the mean of BOD values before municipal mergers started in our sample (before 2001) in each specification.

Table 1.3: Results of River Distance Specification

	Log(BOD)			
	(1)	(2)	(3)	(4)
Distance from upstream border to station (U)	0.010*** (0.003)	0.004 (0.003)		
Squared distance from upstream border to station (U^2)	-0.000** (0.000)	-0.000 (0.000)		
Distance from station to downstream border (D)	0.002 (0.004)	-0.003 (0.005)		
Squared distance from station to downstream border (D^2)	0.000 (0.000)	0.000 (0.000)		
Downstream indicator ($U/(U + D)$)			-0.191 (0.230)	-0.215 (0.225)
Squared downstream indicator ($(U/(U + D))^2$)			0.273 (0.212)	0.334 (0.224)
Observations	14,000	13,360	14,000	13,360
R ²	0.876	0.915	0.875	0.915
Number of Stations	700	668	700	668
Number of Municipalities	700	668	700	668
Year FE	YES	NO	YES	NO
Basin-Year FE	NO	YES	NO	YES
Station FE	YES	YES	YES	YES
Product Shipment Values	YES	YES	YES	YES
Population	YES	YES	YES	YES
F-stat for $U = D$	1.631	1.578	-	-
p-value for $U = D$	0.202	0.209	-	-
Mean of Dep. Variable	2.365	2.401	2.365	2.401

Notes: The coefficients are reported. Standard errors, clustered at station-level, are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Mean dependent variables are the mean of BOD values before municipal mergers started in our sample (before 2001) in each specification.

Table 1.4: DID Results: Mechanisms on Coordination Cost and Political Economy

	Coordination Cost		Political Economy	
	(1)	(2)	(3)	(4)
	Equal-footing	Incorporating	Incorporated	Incorporating
Panel A: Callaway & Sant'Anna (2021) Estimator				
Merger (= 1)	0.063*** (0.019)	0.046* (0.027)	0.091*** (0.034)	0.033 (0.028)
Panel B: Two-way Fixed Effects				
Merger (= 1)	0.069*** (0.022)	0.006 (0.026)	0.103*** (0.028)	-0.019 (0.028)
Observations	54,579	48,139	32,568	44,988
R ²	0.885	0.886	0.891	0.887
Number of Stations	2,373	2,093	1,416	1,956
Number of Municipalities	813	598	541	593
Clustered SE at...	Municipality	Municipality	Municipality	Municipality
Station FE	YES	YES	YES	YES
Product Shipment Values	YES	YES	YES	YES
Population	NO	NO	NO	NO
Mean of Dep. Variable	2.671	3.033	3.130	3.144

Notes: The coefficients are reported. Standard errors, clustered at municipality-level, are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All specification in panel A include year fixed effects, while all regressions in panel B include basin-year fixed effects. Mean dependent variables are the mean of BOD values before municipal mergers started in our sample (before 2001) in each specification.

Chapter 2

Asymmetric Price Response of Oil Supply

Ozgur Bozcaga and Michiyoshi Toya

Abstract:

Understanding how oil producers respond to fluctuations in oil prices is often of great interest to policymakers in designing environmental policies and reducing volatility in energy markets. In this paper, we investigate whether asymmetries exist in supply responses to increases and decreases in oil prices. Using monthly well-level data extending from 1990 to 2021 from North Dakota, we examine how operators of conventional and unconventional wells respond to oil price volatility in the extraction stage of oil production. Our findings show that in unconventional wells, oil production does not respond to oil price increases and decreases in symmetric magnitudes. A 1% price increase in oil prices is associated with 0.022% increase in contemporaneous oil production in unconventional wells and a 1% decrease in oil prices is associated with a 0.08% decrease in contemporaneous oil production. The difference between these two supply elasticities is statistically significant. On the other hand, oil production in conventional wells do not respond to oil price changes. The existence of these asymmetries in production responses indicate that previous estimates of price elasticity of supply for oil might be misleading if treated as bidirectional.

2.1 Introduction

Understanding how oil producers respond to fluctuations in oil prices is often of great interest to policymakers. For instance, the impact of environmental policies affecting oil demand or supply (e.g. taxes/subsidies to oil production, EV subsidies, biofuel subsidies), including the resulting market equilibria and the incidence on consumers and producers, depends crucially on the price elasticity of supply for oil. Finding this elasticity is also important in understanding the underlying mechanisms and designing policies that help reduce oil price volatility.

Existing literature on this policy-relevant issue suggests that oil production levels are highly price inelastic. While some recent papers consider differences between conventional and non-conventional wells, micro-level evidence spanning a wide range of years including post-fracking boom years (mid-2000s onward) remains scarce.

In addition, to the best of our knowledge, there appears to be no literature analyzing the possibility of asymmetric effects on oil supply of increases and decreases in prices. Regarding asymmetric responses, previous papers focus on effects on oil and gas demand using macro-level data, and find suggestive evidence of asymmetries. In explaining the asymmetric consumption responses, these papers point to the fact that investment on energy-using equipment involves large sunk costs and is often irreversible.

A similar feature may apply in the case of oil production as well. Drilling a new well involves large costs, especially for non-conventional wells. Furthermore, due to geological constraints, once a well is tapped, altering production levels are costly, particularly in the case of conventional wells. Finally, the fact that non-conventional wells have higher decline rates and shorter lifespan may also induce different responses among different types of wells. It is therefore of empirical interest to carefully examine asymmetries in price elasticity of supply for both conventional and non-conventional wells using micro-level data. Such an analysis could potentially yield significant policy implications; if asymmetries in supply responses exist, then ignoring this asymmetric response will bias general estimates of price elasticity of supply for oil, thereby implying that previous estimates should be treated with caution.

In this paper we examine well operators' oil supply response to oil price increases and decreases. Particularly, in this paper we investigate if asymmetries exist in oil supply responses to changes

in price distinguishing between conventional and unconventional wells. We use monthly well-level data from approximately 18,000 unconventional and 5,000 conventional wells drilled in North Dakota between January 1990 and December 2021.

We find that in unconventional wells drilled with horizontal drilling and hydraulic fracturing, oil supply responds to price changes. A 1% price increase in oil prices is associated with 0.022% increase in contemporaneous oil production and a 1% decrease in oil prices is associated with a 0.08% decrease in contemporaneous oil production. The difference between these two supply elasticities is also statistically significant, indicating an asymmetric price response in oil supply in unconventional wells. For conventional wells drilled vertically, we did not find a significant oil supply response to changes in oil prices, which is in line with the findings in the literature focused on the supply elasticity of oil supply in conventional wells.

The paper's main contribution to the literature is that it carefully combines the traditional literature on asymmetric responses to price changes with recent empirical literature relating to price elasticity of supply for oil. First, regarding the former wave of literature, the paper shifts the focus of the analysis from asymmetric price elasticity of energy demand to energy supply. Previous literature predominantly focused on the asymmetry of natural gas, oil or gasoline prices on energy demand. This project aims to understand how producers respond to oil price volatility by adjusting their oil supply. As such, the paper equally sheds a new light on the asymmetric supply response literature by using micro-level evidence (as opposed to macro-level analyses widely adopted in this literature) and by incorporating geological characteristics of oil production. Second, the paper provides valuable findings to the emerging literature on price elasticity of supply for oil, by newly examining the possibility of asymmetric responses using granular, well-level monthly oil production data.

The paper is organized as follows: Section 2.2 provides an overview of the literature on oil supply elasticity and asymmetric price response in oil and gas markets. Section 2.3 gives background information on oil production processes in unconventional horizontally drilled wells and conventional vertically drilled wells. Sections 2.4 and 2.5 introduce the empirical methods and data we used to answer the research questions for this project, and Section 2.6 presents the findings. The paper concludes with a discussion about policy implications and suggestions on future research in Section 2.7.

2.2 Existing literature

The conceptual framework of Hotelling (1931), where owners of exhaustible resources equate the marginal values of extraction across time, implies that production of such commodities should increase when price increases.¹ However, in the case of oil, recent findings tend to suggest that production levels do not respond to prices, pointing to elasticity estimates that are close to zero and/or not statistically different from zero (Anderson et al. 2018, Dahl and Yücel 1991, Ramcharran 2002, Rao 2018). In particular, Anderson et al. (2018) and Bjornland et al. (2017) offer theory and micro-level empirical evidence that production rates from existing wells are physically or geologically constrained and do not respond to prices, and that producers respond to prices by adjusting the rate of drilling new wells (with an elasticity of drilling of roughly 0.7). Newell and Prest (2019) also look at the price responsiveness of US conventional and unconventional oil supply at drilling, completion and production stages; and find similar results (i.e, higher elasticities at the drilling stage). While their observations are at the quarterly level and not at the monthly level, the geographical coverage (Texas, North Dakota, California, Oklahoma and Colorado) of their study is broad.

Nevertheless, well-level evidence on how oil production responds to prices are still rare. Many papers do not seem to adequately cover the period after which hydraulic fracturing (fracking) and horizontal drilling technologies became widely available (mid-2000s onward), and do not clearly distinguish between conventional wells and unconventional shale wells.

In particular, existing papers do not seem to examine the possibility of asymmetric effects on oil supply of increases and decreases in prices. Regarding asymmetric responses, previous papers focus on effects of oil price changes on oil and gas demand using macro-level data (Gately and Huntington 2002). They find that oil demand response to a price increase is not necessarily reversed symmetrically by an equivalent price decrease. These papers point to the fact that energy consumption is closely related to the capital stock for energy-using equipment. For instance, when oil prices go up, investment in retrofitting such as building insulation increases. However, given the large sunk costs, such retrofitting is often not reversed when oil prices decline. Griffin and Schulman (2005) questioned Gately and Huntington (2002)’s approach by underlining that it only captures the energy saving technology advances. In response, Huntington (2006) showed that for aggregate energy and

¹For an overview of the Hotelling model see: Slade and Thille (2009)

oil demand both asymmetric price response and exogenous energy saving technical change play a role (Adeyemi et al. 2010).

A similar feature of capital stock impact may also hold in the case of oil production for both conventional and unconventional wells, possibly at different magnitudes. First, drilling a new well involves large costs, especially for unconventional wells. Second, recent empirical observations regarding shale oil production suggest that shale wells have high decline rates than conventional wells (Wachtmeister et al. 2017). On a similar note, conventional wells have relatively uniform project economics and are long-life, allowing producers to make production decisions that are based on long-term oil price, while unconventional oil wells have more flexible production cycles and shorter lifespans, which may induce producers to respond more to short-term price changes (Livingston 2014). Finally, on the technology side, while the rate of production from a conventional well is physically constrained and altering production levels is very costly once tapped (Anderson et al. (2018)), shale wells are technologically more flexible and can be stimulated with water and chemicals many times during its lifetime, implying that production levels can be adjusted with more ease (Bjornland et al. (2017)).

It is therefore of empirical interest to carefully examine whether asymmetries exist in price elasticity of supply for both conventional and unconventional wells, using recent well-level data that reveal production decisions from both types of wells.

2.3 Background information

2.3.1 Oil production and shale technology

Before the widespread use of horizontal drilling and hydraulic fracturing technologies, geologists and oil and gas practitioners were aware of the fossil fuel reservoirs in shale formations. However high costs involved in using horizontal drilling and the hydraulic fracturing methods did not allow these reserves to be recovered efficiently. As costs dropped precipitously with the introduction of modern downhole motors and slickwater fracturing techniques, the oil and gas that has been trapped in shale formations started to be recovered economically in early 2000s.

The main determinant for the choice of technology (horizontal or vertical drilling) depends on the geology of the formation where oil is trapped. Conventional wells are quite efficient when oil

reservoir is in porous and permeable formations such as sandstone. In these porous and permable formations, a body of gas will gather at the top of the reservoir and oil will sit underneath it in porous rock. When a well is drilled vertically directly into this reservoir, the porous nature of the rock allows oil and gas in the reservoirs to flow naturally into the wellbore with the pressure difference inside the formation and on the earth's surface connected by the vertical well (Devold 2013). In contrast to this natural flow in conventional wells, unconventional wells with horizontal drilling require hydraulic fracturing to stimulate tight shale oil and gas resources to flow into the wellbore and then to the earth's surface which we explain below (Newell et al. 2019).

Shale oil reservoirs are usually 1 mile or below the surface which is similar to the conventional oil reservoirs. In Figure 2.1, we see that the vertical drilling can go as deep as 3000 meters, or approximately 2 miles. These depths could also be reached in conventional wells as well, so, the vertical drilling part is not different from the drilling process of conventional vertical wells. Yet, in contrast to conventional reservoirs, where drilling is completed when the wellbore reaches the reservoir, shale oil reservoirs are trapped in thin layers of rock formations which is not economically feasible to pump oil with just the vertical part of wellbore reaching the reservoir. For the specific case of North Dakota, the middle Bakken geological layer, in which majority of the oil reserves are stored, has a thickness of approximately 50 meters (150 feet) and have very low permeability which makes it unsuitable for vertical drilling (Kuhn et al. 2012, Meissner 1984). Hence, to recover the oil in shale reservoirs the well should be drilled further at an angle. Following the vertical drilling, the wellbore is drilled horizontally which can extend several miles. When horizontal drilling stage is completed it is first enforced with a casing to avoid collapsing of the horizontally drilled segment of the well. Then, a perforating gun is used to create perforation holes in the horizontal segment of the wellbore (Perforation Stage in Figure 2.1). These holes connect the oil reservoir and the wellbore. Following perforation, a mixture of water, sand and chemicals are pumped into the wellbore creating long fractures in the perforated holes which bears the name hydraulic fracturing (Dirty Volume Stage in Figure 2.1). After fractures are opened, the water in the wellbore is pumped out and the sand in the mixture stays in the fractures to keep them open to facilitate the flowing of oil (or natural gas) into the wellbore (Closure Stage in Figure 2.1). This process is repeated along the horizontal segment of wellbore until all productive parts of the reservoir is fractured. When all the steps of horizontal drilling and hydraulic fracturing are completed, the pumping of remaining water mixture

and flow of oil (or natural gas) with high pressure begins.

The initial flow of oil from unconventional wells are much higher than the conventional wells but as the effect of initial fracturing fades the perpetual decline in production is higher as well which can be seen in Figure 2.2. While the average unconventional or horizontal well in North Dakota reaches a maximum of almost 9000 barrels per month a few weeks after starting production, the average conventional or vertical well could only reach to 2000 barrels per month on average. Yet, the rate of decline in an average unconventional well is much steeper than a conventional one. To increase production over the lifespan of an unconventional well, operators can repeat the fracturing process several times which creates a spiky production profile (Bjornland et al. 2017). This adds to the operating cost of the well and increase the marginal cost of producing oil in unconventional wells. The costly horizontal drilling and hydraulic fracturing processes cause the marginal cost of oil production and breakeven prices for unconventional wells to be substantially higher than conventional wells (Kleinberg et al. 2018). In 2013, the reported cost of fracking was in the range of \$2-5 million per well, out of the total average well cost of \$9 million (Covert 2015).

In addition to the differences in the drilling process, there are three important distinctions between conventional and unconventional oil production: aggregation of reservoirs, initial production levels and control over production levels (Newell et al. 2019). First, compared to conventional reservoirs, unconventional reservoirs are clustered in a single formation and uniformly distributed across that formation which prevents drilling too many dry holes. Second, both aggregate and initial production levels in unconventional wells are much higher than in conventional wells. Figure 2.2 illustrates this with respect to the oil wells in North Dakota spudded between 1990 and 2021. Third, firms have control over the production in unconventional wells to a certain degree. This is not the case for conventional wells which work in terms of “tapping the keg” principle as (Anderson et al. 2018) show in their paper on the limited applicability of the Hotelling rule in conventional wells. The flexibility in unconventional wells allows operators to respond to price changes. We see the difference in production flexibility between conventional and unconventional wells in Figures 2.3 and 2.4. In Figure 2.3 we plot the average monthly production in vertically drilled conventional wells in North Dakota against the WTI spot oil price. As clearly recognized in this plot, oil supply from conventional wells do not respond to price movements in global oil prices and is characterized

with a steady decline over time. In Figure 2.4, we can discern that average oil supply from horizontally drilled unconventional wells follow WTI spot oil price movements closely especially after 2015 following the initial phase of the North Dakota shale oil boom between 2005-2015. Even though we do not examine the oil price elasticity of drilling in this paper, we see a similar pattern in oil price response of drilling conventional and unconventional wells. In Figures 2.5 and 2.6, we see that while the number of spudded conventional wells do not present a clear pattern with respect to oil price volatility, while the number of new spudded unconventional wells explicitly follow the oil price movements.

2.3.2 ND oil sector and the shale boom

The first major oil discovery in North Dakota was in 1951 (Clarence Iverson farm in Williams county). Production slowly increased in the 1950s and 1960s as more oil was discovered. Spurred by the sustained high prices stemming from the two major global oil crisis due to the Yom-Kippur War of 1973 and the Iranian Revolution of 1979, the region experienced an oil boom in the 70s and early 80s with a surge in production and number of drilled wells. Production peaked at 150,000 barrels per day in 1984 and this period could be named as the “vertical” drilling boom in North Dakota. Yet both drilling rates and aggregate production quickly declined in the late 1980s following the fall in oil prices. The 1990s and early 2000s followed a gradual declining trend. Figure 2.7 shows the peak in early 1980s and the gradual decline in the succeeding period for the oil production with vertical conventional wells.

Meanwhile, around year 2002, the combined technology of horizontal drilling and hydraulic fracturing (i.e., extended reach horizontal drilling) became available, allowing oil companies to extract oil and gas from the shale rock as explained in the previous section. The discovery of the Parshall Oil Field in the Bakken Formation in 2006, coupled with the availability of this new technology, led to rapid increase in shale oil production in North Dakota. The high oil prices in the 2000s before the 2007-08 financial crisis also contributed to the boom, offsetting the high costs associated with fracking and horizontal drilling². Figure 2.7 show the extent of the shale oil boom in North Dakota with the spiking number of new wells after 2005. The combination of the introduction

²For more details on the history of North Dakota Oil Production, see e.g. [North's Dakota's official government website](#)

of cost effective methods for hydraulic fracturing and horizontal drilling with high oil and gas prices before the 2007-2008 financial crisis spurred the shale supply boom not only in North Dakota but in the US as a whole (Hausman and Kellogg 2015, Kilian 2017). For instance, while shale gas provided only 1% of U.S. natural gas production, in 2010 it provided over 20% (Melek First Quarter 2015). The case for shale oil is even starker, while shale oil provided 1% of U.S. oil production, in 2021 it provided over 65% (EIA 2022)

As shown in Figures 2.8 and 2.9, North Dakota experienced a surge in both proven oil reserves and oil production from around 2005; much more so than its neighboring states Wyoming and Montana and also other states on the Bakken Shale. In 2015, there were 10,619 active shale wells in North Dakota, and production from shale wells accounted for 95.7% of oil and gas production in the state. As of December 2021, North Dakota is producing approximately 1.13 million barrels of oil per day, which accounts for roughly 9.7% and 1.2% of US and world oil production respectively³. Figure 2.10 shows oil and gas producing counties in North Dakota. The state has 17 active oil and gas producing counties, with the core four counties of Williams, McKenzie, Dunn and Mountrail accounting for at least 75% of total production. Figure 2.11 shows the locations of oil wells and it can be seen that most of the wells in North Dakota is concentrated in the core. This is inline with the geological characteristics of shale formations where oil reserves are clustered in a well defined area which does not allow the “hit and miss” drilling of dry wells that is common in conventional oil reservoirs.

The high exposure to shale boom and the recent prevalence of shale wells in addition to currently active conventional wells make North Dakota an ideal setting to investigate operators’ differential asymmetric oil well drilling and oil supply response in conventional and unconventional wells.

2.4 Empirical Methods

In this section we present a graphical illustration of symmetric and asymmetric oil price response of production, the empirical models we use to test these hypotheses and the related hypothesis that we test.

³See [North Dakota Oil and Gas statistics](#) and [EIA’s crude oil production statistics](#)

The underlying assumptions for symmetric and asymmetric price response of oil supply can be illustrated with Figure 2.12.⁴ In the case of symmetrical response (shown in the bottom plot in Figure 2.12 with the title “Symmetric Response”), oil supply responds symmetrically to oil price changes and the supply-increasing response to a price increase in *Price boom 1* would be reversed by the supply-decreasing response to a price decrease in *Price slump*. In other words, the slopes of the lines would be the same for *Price boom 1* and *Price slump*. Also any price recovery following an oil price decrease would induce a similar supply reaction, meaning the slope for *Price boom 2* should be the same as the slopes of the other two lines. In contrast to this, in the case of asymmetrical response (shown in the plot at the top of Figure 2.12 with the title “Asymmetric Response”) oil supply responds asymmetrically to oil price increases and decreases and the supply-increasing effect of an oil price increase in *Price boom 1* would not be reversed with a supply decrease of the same magnitude in response to an oil price decrease during *Price slump*. Hence, the slopes of the lines for *Price boom 1* and *Price slump* would not be the same. With regard to this graphical illustration, we will test the equality of the magnitude of oil supply responses to oil price increases and decreases in conventional and unconventional wells. We expect that in unconventional wells, (1) oil supply responds to oil price changes in statistically significant magnitudes and (2) the difference of oil supply response to oil price increases and decreases are significantly different from each other, indicating an asymmetrical price response. In unconventional wells, we expect (1) oil supply not to respond to oil price changes and (2) the difference of oil supply response to price increases and decreases are not significantly different from each other.

To test these hypotheses, we run a model where we place the monthly production at the well level as the outcome variable and have the decomposed monthly oil price as price increases and decreases as predictors to estimate the separate effects of oil price increases and decreases on monthly production at the well level. We also use lagged dependent variables to control for the production of a well in previous months. We run this model separately for unconventional and conventional wells to discriminate the oil price elasticity of supply for wells drilled with different technologies. The model we estimate is below in equation 2.1:

⁴The figure is adopted from Gately and Huntington’s (2002) paper on asymmetric price response of oil demand.

$$\begin{aligned}
IHS(Prod_{i,t}) = & \sum_{j=1}^k \beta_{prod_j} IHS(Prod_{i,t-j}) + \beta_{pc}^+ [IHS(Price_t) \times (PC_t^+)] + \gamma_{pc}^+ (PC_t^+) \\
& \beta_{pc}^- [IHS(Price_t) \times (PC_t^-)] + \beta_p IHS(Price_t) + \sum_{n=1990m1}^{2021m12} \eta_n month_n + \epsilon_{i,t}
\end{aligned} \tag{2.1}$$

In this model we regress the hyperbolic sine transformed monthly oil production in each well on hyperbolic sine transformed monthly negative and positive oil price changes. We control for the oil production in each well in previous months.⁵ $IHS(Prod_{i,t})$ is the hyperbolic sine transformed oil production in barrels in each well i , in each month t . $IHS(Prod_{i,t-j})$ are the lags of the dependent variable. In our main specification, j goes back three months, but we also try alternative specifications where j goes back as far as 12 months. $IHS(Price_t)$ is the hyperbolic sine transformed WTI spot oil prices at month t . PC_t^+ and PC_t^- are the binary variables to indicate positive or negative price change at month t . PC_t^+ is equal to one if the change in WTI spot oil price is positive from month $t-1$ to t , zero otherwise. PC_t^- is equal to one if the change in WTI spot oil price is negative from month $t-1$ to t , zero otherwise. We interact these two binary variables with the $IHS(Price_t)$ to estimate the price elasticity of oil supply in response to price increases and decreases at month t . Our coefficients of interest are β_{pc}^+ and β_{pc}^- . $month_n$ is the month dummy variable for all calendar months in the panel, from January 1990 to December 2021. It is equal to one for calendar month n and 0 otherwise. In total we add 383 month dummy variables in our model but since our panel has more than 2.5 million observations, it does not cause a problem for estimation. $\epsilon_{i,t}$ is the mean zero error term. To estimate the model we could have relied on several different options given its dynamic panel data structure with the inclusion of lagged dependent variables. The autoregressive nature of the model causes estimates to be biased when we use an OLS or regular two-way fixed effects estimator with well and month fixed effects, so we eliminate these options. To estimate this dynamic panel data model, we could use the GMM estimators created by Arellano and Bond (1991) (difference GMM) and Arellano and Bover (1995) and Blundell and Bond (1998) (system GMM), however these estimators are suited for small T, large N samples. Our sample consists of 383 months which makes it unsuitable for implementing

⁵For a random variable x , taking the inverse hyperbolic sine transformation yields a new variable such that, $\tilde{x} = arcsinh(x) = \ln(x + \sqrt{x^2 + 1})$

difference and system GMM estimators. Instead of these options we first difference the model and use Anderson-Hsiao (AH) estimator (Anderson and Hsiao 1982).⁶ We include second and third lags of our dependent variable as instruments of the first lag of the dependent variable which will still be highly correlated with the lagged dependent variable but will be uncorrelated with the error term.

We also run a model with global macroeconomic indicators as control variables to examine if our model predicts the impact of global macroeconomic effects on oil production at the well level. The model we estimate is very similar to equation 2.1:

$$IHS(Prod_{i,t}) = \sum_{j=0}^k \beta_{prod_j} IHS(Prod_{i,t-j}) + \beta_{pc}^+ [IHS(Price_t) \times (PC_t^+)] + \gamma_{pc}^+ (PC_t^+) + \beta_{pc}^- [IHS(Price_t) \times (PC_t^-)] + \beta_p IHS(Price_t) + \gamma X'_t + \epsilon_{i,t} \quad (2.2)$$

Different from equation 2.1, equation 2.2 includes X' , which contains the battery of macroeconomic control variables that might impact both oil production and also oil price changes. It includes MSCI All Countries World Index, U.S. 10 year treasury bond yield, trade weighted broad USD index, copper prices. Other variables are as described above for equation 2.1.

We also run our model where we have the natural logs of the variables instead of the hyperbolic sine transformation. This approach has a problem as there are quite a few zeros in production and price change variables. To be able to use these values, we added 0.001 to each observation of these variables that are equal to zero.

Concerning causal interpretation of price elasticity, we have to consider the potential influence of changes in North Dakota oil production on global oil prices. North Dakota accounts for 1.7% of global and 12% of US oil production. Despite being cautious about potential reverse causality, changes in North Dakota oil production levels, production capacity and new well drilling might not have substantial impact on global spot or futures oil prices.

As mentioned above, in equations 2.1 and 2.2 our coefficient estimates of interest are β_{pc}^+ and β_{pc}^- . For asymmetric price response of oil production to hold, these two coefficient estimates should be different from each other at statistically significant levels. To test this we conduct a Wald test for the equality of these two coefficient estimates where the null hypothesis is that the sum of these two

⁶See a comment by Arellano Arellano and Bond (1991) on the AH estimator.

estimates are equal to or greater than zero. The alternative hypothesis is that the sum is less than zero, so we are running a one-sided test. The reason for this is related to the oil production process especially in unconventional wells. While operators can shut off wells or decrease at later stages of a well's productive life, increasing production in productive wells is possible but quite expensive given the costs involved in re-fracking. Hence, we expect the absolute value of β_{pc}^+ estimate to be less than the β_{pc}^- estimate. We can define our hypothesis test as follows:

$$\begin{aligned} H_0 : \beta_{pc}^+ + \beta_{pc}^- &\geq 0 \\ H_1 : \beta_{pc}^+ + \beta_{pc}^- &< 0 \end{aligned} \tag{2.3}$$

For conventional wells, we expect estimates of β_{pc}^+ and β_{pc}^- to be statistically insignificant as it is quite costly to alter the production of conventional wells after the well starts producing oil. For unconventional wells, we expect estimates of β_{pc}^+ and β_{pc}^- to be statistically significant at conventional levels and the estimate of β_{pc}^+ to be less than β_{pc}^- which would allow us to reject the null hypothesis in equation 2.3.

2.5 Data

We use North Dakota Industrial Commission Oil (NDIC) and Gas Division's monthly well-level data on oil production. The well-index from the NDIC has data starting from 1957, however the granularity and accuracy of the data is questionable before 1990, so we use the production data spanning from January 1990 to December 2021 in our analysis. This rich dataset includes information on location (county, file, latitude, longitude etc) and monthly oil production, water retention and gas release of each well and company and field name the well is associated with. For our analysis we will focus on the monthly oil production in each well. The technology used for drilling the well is also included in the dataset. So, we know which wells are drilled horizontally (with hydraulic fracking) and vertically from the data, which allows us to conduct our analysis separately for wells drilled using these two types of technology.

Summary statistics of the data we use in this paper is in Table 2.1. Our unbalanced panel has more than 1.6 million observations from unconventional (horizontal) wells, 1 million from conventional (vertical) wells. There are more than 2.8 million observations in total but these include

observations from dry wells and confidential wells which we did not include in our analysis. The total number of wells is 17,699 for unconventional wells and 4,926 for conventional wells, which shows that more than 2/3 of wells were drilled using horizontal drilling and hydraulic fracturing. We see the difference in the average monthly oil production across unconventional and conventional wells in Table 2.1. While an average unconventional well produces approximately 2,500 barrels per month, an average conventional well produces below 500 barrels per month. This is in line with the general assumption that shale oil wells produce more oil on average than conventional wells.

For crude oil price data we use the monthly average of the Western Texas Intermediate (WTI) spot crude oil price for delivery in Cushing Oklahoma, New York Mercantile Exchange (NYMEX) futures prices provided by the Energy Information Administration (EIA) ⁷. From Table 2.1 we can see that the WTI spot price and futures price for a 4-month contract on NYMEX have very similar mean and standard deviation values. Also, in Figure 2.13, we see that WTI spot, WTI 4-month futures and Brent spot prices follow each other pretty closely for the time frame of our analysis. So we can be assured that we WTI spot prices instead of the futures prices to measure the impact of oil price change on contemporaneous oil production. We did not use Brent spot prices for our analysis as it is not relevant for the producers in the U.S.

As mentioned in Section 2.4, we use several macroeconomic control variables for our alternative specification. The MSCI Index is the average monthly return of the MSCI All Country World Index (AWCI) that is publicly provided by MSCI.⁸ The US 10 year treasury yield is the market yield on U.S. Treasury securities at 10-Year constant maturity from St. Louis FED's economic database, FRED.⁹ USD index is the nominal broad U.S. dollar index from FRED¹⁰ and the copper price is the global price of copper both from FRED.¹¹

2.6 Findings

We present the main estimation results of our analysis in Tables 2.2 and 2.3, and models with alternative specifications and functional forms are in Appendix Tables D1 - D9.

⁷To access the spot oil price data see [EIA's spot crude oil data page](#). For futures oil prices see [EIA's crude oil NYMEX futures price page](#)

⁸To access the data see [MSCI AWCI's end of day dataset](#).

⁹To access the data see [FRED GS10 dataset](#).

¹⁰To access the data see [FRED DTWEXBGS dataset](#).

¹¹To access the data see [FRED PCOPPUUSD dataset](#).

Before we interpret the results we should have a note about how to interpret the coefficient estimates on hyperbolic inverse sine transformed variables for calculating elasticity of oil supply with respect to price increases and decreases. In their simulation exercise Bellemare and Wichman (2020) show that for values larger than 10 in the outcome and predictor variables, the interpretation of the coefficient estimates in models involving hyperbolic inverse sine transformations to estimate elasticities is similar to that of log-log models. That is, the coefficient estimate on the predictor variable is equal to the elasticity of the outcome variable with respect to the predictor variable. Their Monte-Carlo simulations also show that for arcsinh-arcsinh specifications, the coefficient estimate of the predictor variable yields the elasticity estimate for values even above one. Following this finding, we will interpret the coefficient estimates on the hyperbolic sine transformed variable.

In Table 2.2, we present the results of the OLS and fixed effects estimators. Columns with “H” indicate estimates for unconventional, horizontally drilled wells, whereas columns with “V” show estimates for conventional, vertically-drilled wells. The Table 2.2 results are somewhat puzzling. While coefficient estimates for “PC Negative” ($\widehat{\beta}_{pc}^-$) are negative and statistically significant (implying that an oil suppliers decrease production as oil prices decline, as expected), and while it is equally encouraging to find that conventional wells appear to be less responsive to oil price changes than unconventional wells; the fact that coefficients for “PC Positive” ($\widehat{\beta}_{pc}^+$) are consistently negative and statistically significant across the four columns is problematic. For instance, results from column 1 would imply that a 1% increase in the WTI spot price of oil is associated with a 0.29% decrease in oil supply (from unconventional wells), which would run counter to economic theory. However, it should be noted that due to the autoregressive nature of the specification, a simple OLS or a regular two-way fixed effects model would be subject to bias.

In Table 2.3, we present our results estimated with the Anderson-Hsaio estimator.¹² In the first two columns we present the results of the estimation where we used second lag of the dependent variable as an instrument for the first lag of oil production at month t . In the third and fourth columns we present the results of the estimation where we used the second and third lags of the dependent variable as instruments for the first lag of the outcome variable. As before, columns

¹²To estimate the models in this table we used Roodman’s `xtabond2` package in Stata. For details of the `xtabond2` package, see Roodman (2009)

with “Prod-H” indicate models we ran for unconventional and horizontally drilled wells and “Prod-V” indicate models for conventional and vertically drilled wells. First, we see that the coefficient estimates for the lagged dependent variable are all under unity, indicating a dynamic stability of the panel data model structure we are using. The first column has the results for the unconventional horizontally drilled wells and the estimates are in line with the predictions of asymmetric price response hypothesis. Both coefficient estimates of the price change variables have the expected signs and statistically significant at conventional levels. The coefficient estimate for negative price change ($\widehat{\beta}_{pc}^-$) indicate that a 1% decrease in the WTI spot price of oil is associated with a 0.085% decrease in contemporaneous oil production on average. On the other hand, the coefficient estimate for positive price change ($\widehat{\beta}_{pc}^+$) indicate that a 1% increase in the WTI spot price of oil is associated with a 0.021% increase in contemporaneous oil production on average. The difference of these two coefficients are also statistically significant at 5% level (p-value of 0.0498) showing that oil supply responds asymmetrically to price increases and decreases in unconventional wells. In column 2, we see that oil supply in conventional wells do not respond to price increases or decreases, coefficient estimates of neither negative price change nor positive price change variables are statistically significant and the signs are not in expected directions. This is in line with previous literature focusing on the price elasticity of oil supply in conventional wells and suggest that oil supply does not respond to price changes. When we add the third lag of the dependent variable as an instrument to the model, we see in columns 3 and 4 that the magnitude of the coefficient estimates do not change substantially, and the signs and significance levels are the same with the results in columns 1 and 2.

Since we used multiple lags of the dependent variable as instruments of the first lag of the dependent variable, we also checked for the instrumental variable tests. The test statistics for underidentification test and Sargan-Hansen test for overidentification tests point to the fact our models are properly identified. Also, to examine if using the second or third lags of the dependent variables are appropriate after first differencing our model for Anderson-Hsaio estimator, we ran an AR(2) test. Test results indicate that the differenced residuals do not exhibit significant AR(2) behavior. Hence, it is appropriate to use second lags as instruments for the first lag of the dependent variable.

In Appendix Tables D1 - D9 we also present the estimation results of OLS, two-way fixed effects and Anderson-Hsaio models in levels, logs and hyperbolic sine transformation. We see that models

with OLS and two-way fixed effects estimate the coefficients of the price change variables imprecisely and with unexpected signs under different functional forms of the outcome and predictor variables. Models estimated with Anderson-Hsaio estimator with month dummies or control variables for monthly macro economic indicators are the only set of estimates that are in line with our theoretical expectations.

One concern with the estimator used in this model could be related to the proportion of zeros in our outcome variable. If we had too many zeros in our outcome variable, we could model the data-generating process explicitly and use a Tobit, zero-inflated Poisson or negative binomial model. Our outcome variable, monthly oil production at the well-level has approximately 400,000 zeros in a panel with more than 2.7 million observations for vertical and horizontal wells. This means around 15% of our outcome variable is composed of zeros. Bellemare and Wichman (2020) posits that a rule of thumb could be using limited dependent variable models if the outcome variable has more than one third zero-valued observations. We follow this advice and instead of using Tobit, zero-inflated Poisson or negative binomial models, we rely on first differencing with Anderson-Hsaio estimator.

2.7 Discussion and Conclusion

In this paper, we have investigated whether asymmetries exist in oil supply responses to increases and decreases in oil prices, focusing on well-level oil production in North Dakota and distinguishing between conventional and unconventional wells. Our findings interestingly show that in the case of unconventional wells, such asymmetries exist. A 1% price increase in oil prices is associated with 0.022% increase in contemporaneous oil production in unconventional wells and a 1% decrease in oil prices is associated with a 0.08% decrease in contemporaneous oil production. The difference between these two supply elasticities is also statistically significant. The fact that unconventional oil producers respond asymmetrically to oil price changes is a novel finding in the literature. As for conventional wells, consistent with emerging literature, we observe that producers do not respond to oil price changes. These findings imply that, when designing taxes/subsidies to oil production for instance, policymakers should not only be mindful of different behaviors between conventional and non-conventional oil producers, but also of different behaviors between price increases and price decreases (in the case of conventional oil producers).

We will end this paper by discussing possible extensions. First, there are three stages in the oil production process, drilling the well, starting oil production from the well and producing oil over time. In this paper we focused on one stage, production over time. While this dimension cover an important part of decisions involved in the production process of oil, the decision to drill the well, start producing oil from it and the time between spudding a well and actual production of oil are important aspects of oil production as well. An extension of this paper is to include asymmetries in operators' response to price in their decision to start producing oil from a spudded well. This could be easily incorporated to the analysis in this paper given the richness of monthly well-level data in North Dakota.

Second, oil and gas departments states other than North Dakota on the Williston Basin and Bakken Shale, namely Montana and Wyoming in the US and Saskatchewan in Canada provide monthly well-level data to researchers working in this area. (see Figure C1 for the reach of Williston Basin which encompasses Bakken shale.) A further extension of this analysis to include monthly well-level data from other states on the Bakken formation to expand the breadth of analysis.

Third, as explained in the previous sections, for both conventional and unconventional wells, the natural initial flow of oil is high due to pressure differences between the well head on the earth's surface and the oil reservoir. Controlling production by increasing or decreasing oil flow at this stage is very costly. However, after the initial phase, extra pumping is required to maintain the flow of oil. For unconventional wells, this happens at an early stage compared to the conventional wells due to the high decline in production. Hence, the decision to spud and drill the well and start production is particularly important for unconventional wells. A further extension of this study would be to examine how unconventional and conventional oil operators respond differently to spot-future price spreads in terms of the decision to drill the well after getting drilling approval and also when to start producing after drilling is completed.

2.8 Figures and Tables

2.8.1 Figures

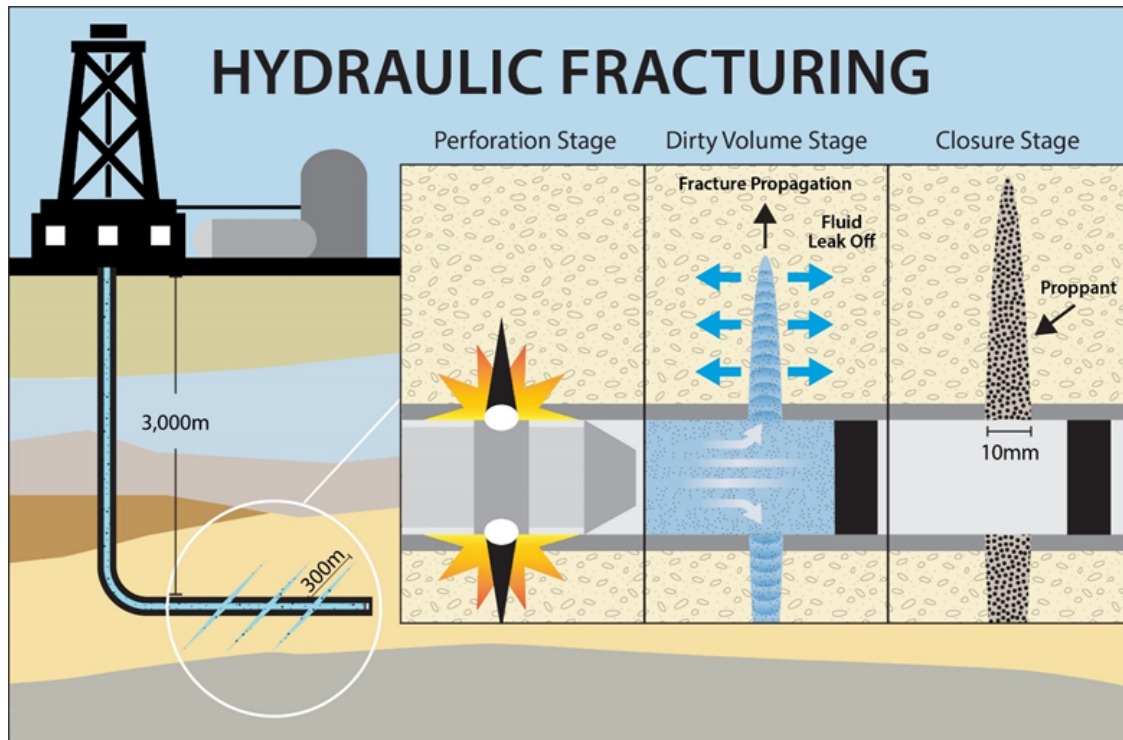


Figure 2.1: Fracking process
Source: Kwon Research Group, TAMU

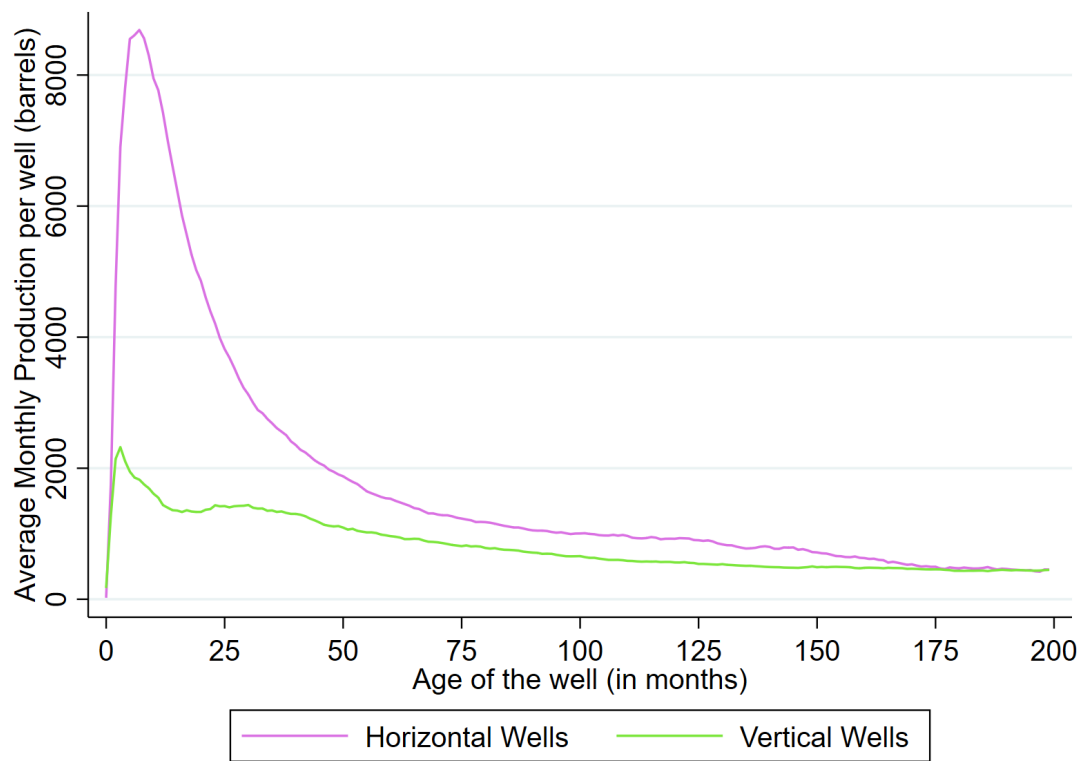


Figure 2.2: Average production after spudding in horizontal and vertical wells

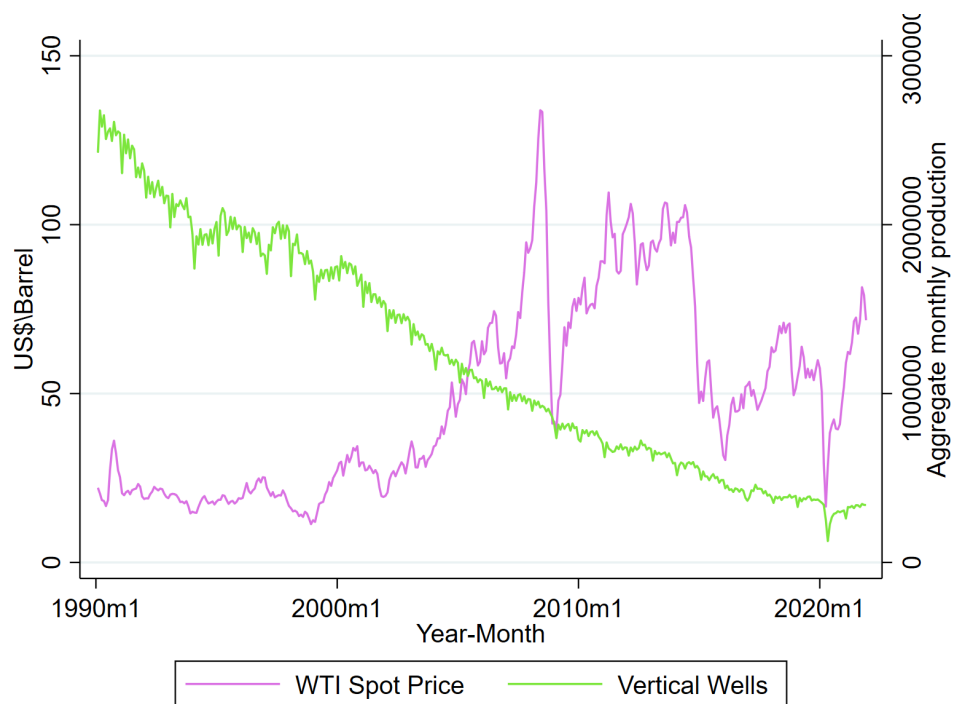


Figure 2.3: Production by vertical wells

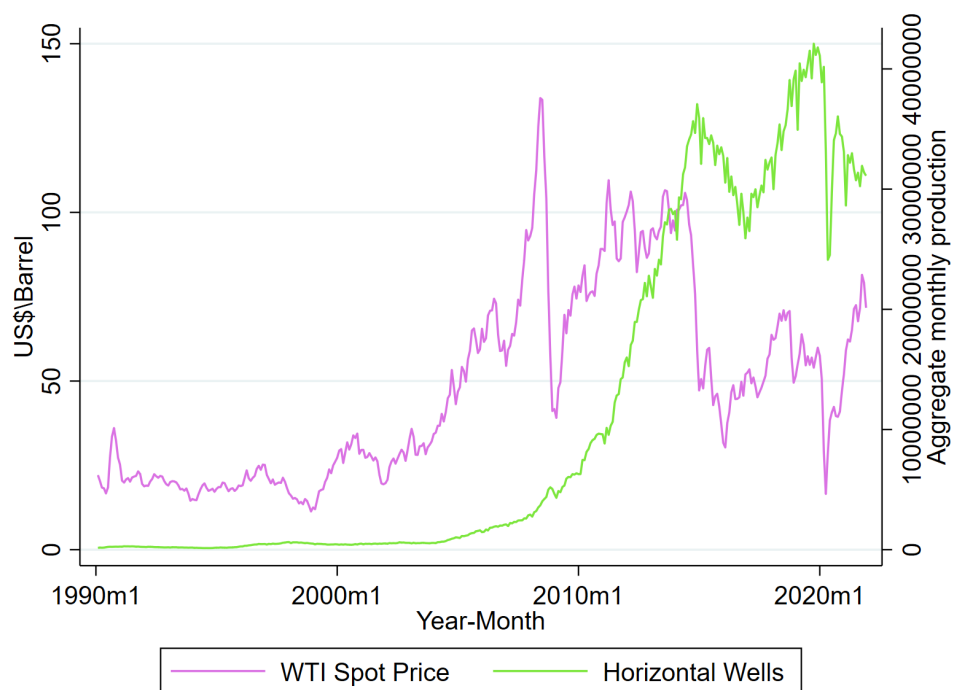


Figure 2.4: Production by horizontal wells

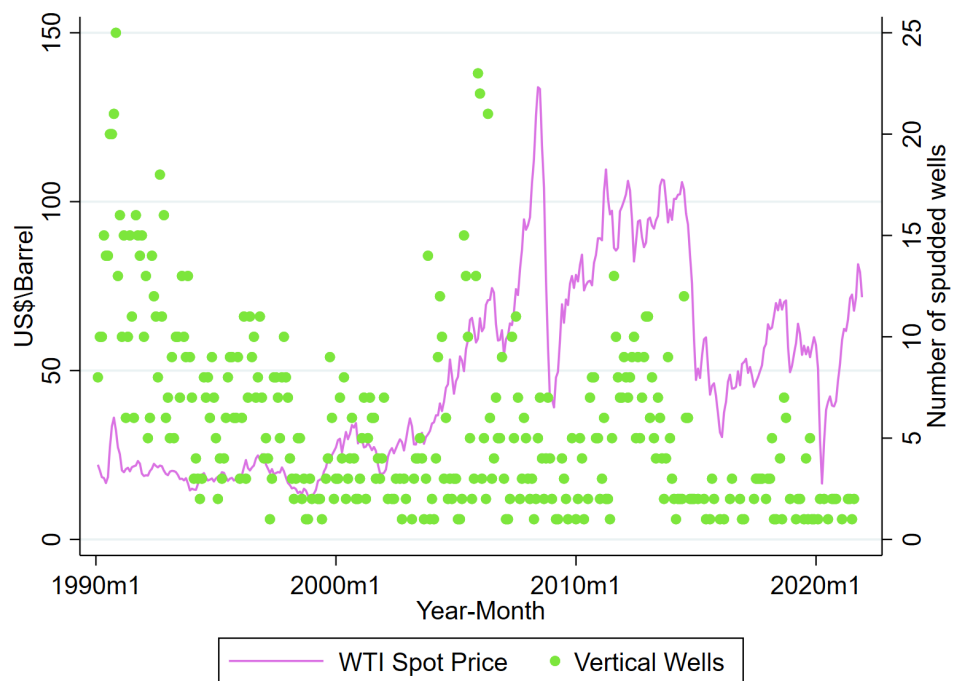


Figure 2.5: Number of spudded vertical wells

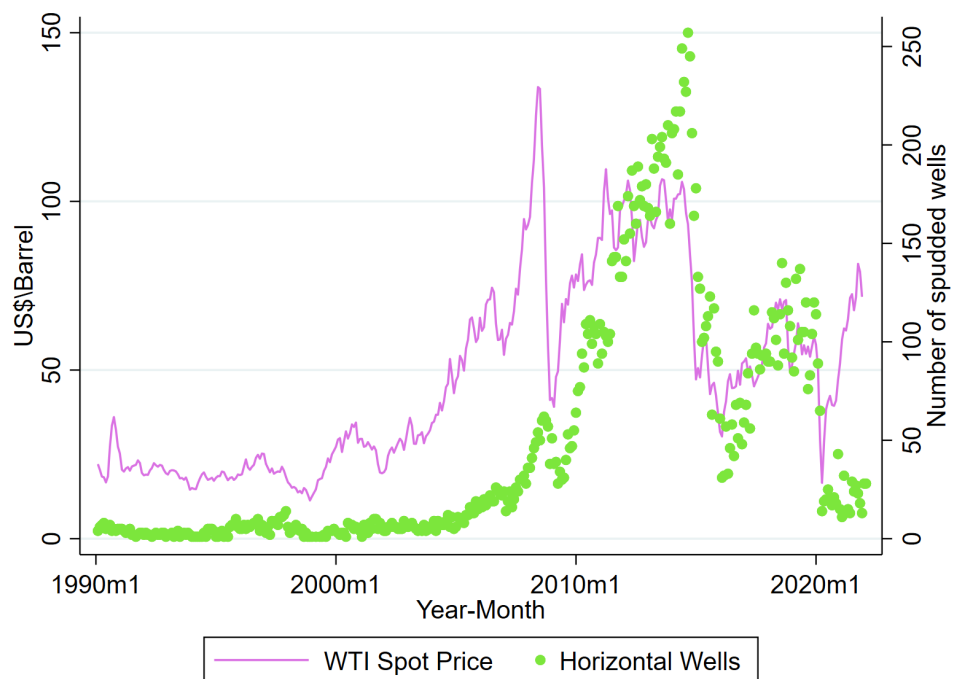


Figure 2.6: Number of spudded horizontal wells

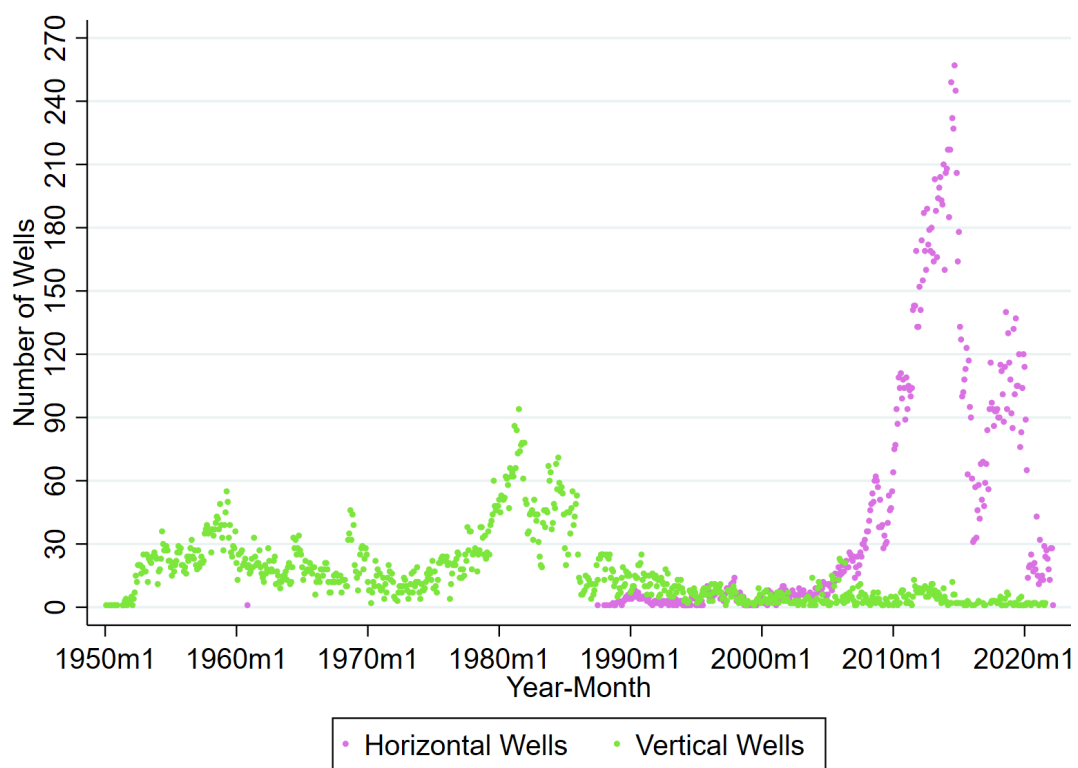


Figure 2.7: New wells spudded in each month for horizontal and vertical wellbores between 1990 and 2021

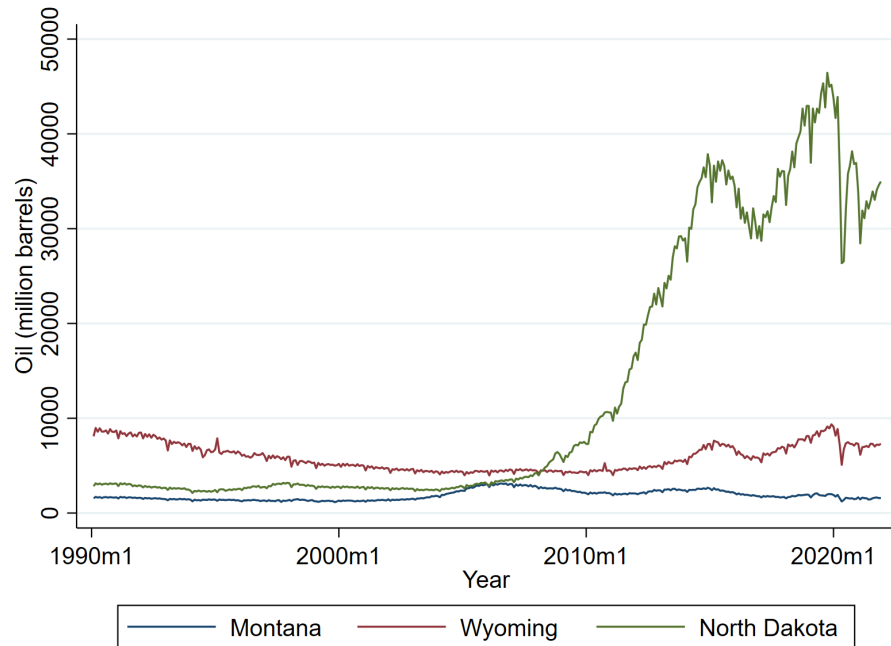


Figure 2.8: Oil production in North Dakota, Montana and Wyoming between 1990 and 2021
Source: [EIA's crude oil production by U.S. states database](#).

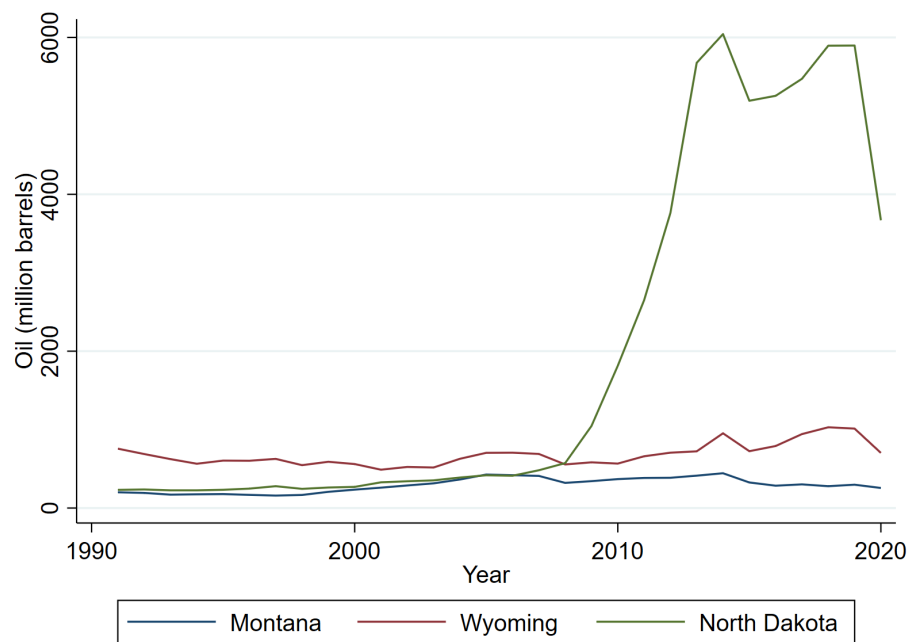


Figure 2.9: Proved oil reserves in North Dakota, Montana and Wyoming between 1990 and 2021
Source: [EIA's Crude Oil Proved Reserves, Reserves Changes, and Production by U.S. states database](#).

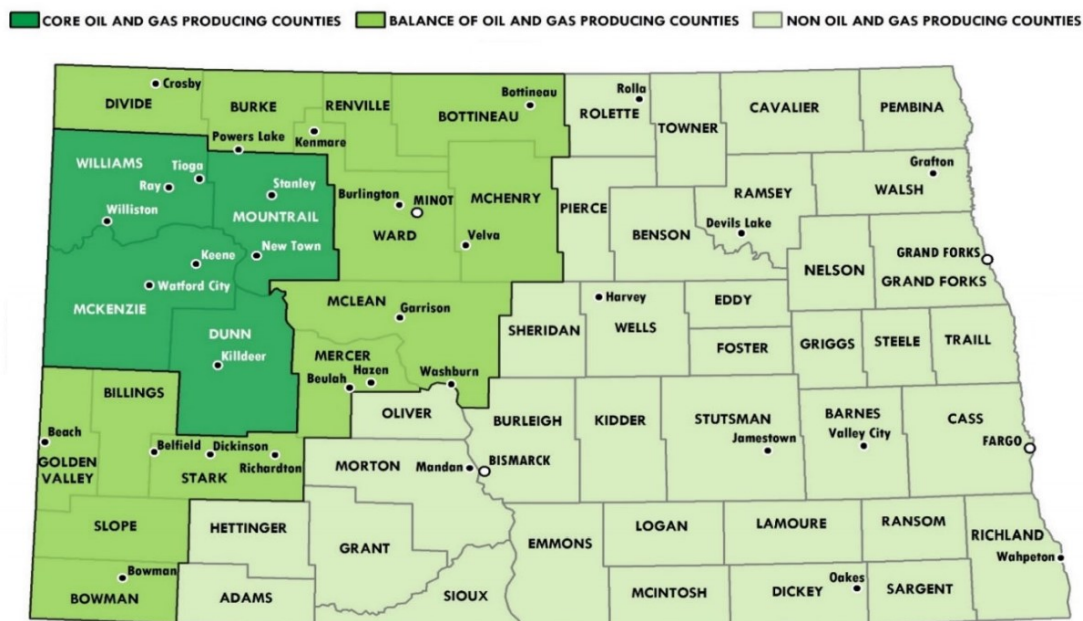


Figure 2.10: North Dakota Oil Producing Counties
Source: NDIC Oil and Gas Division

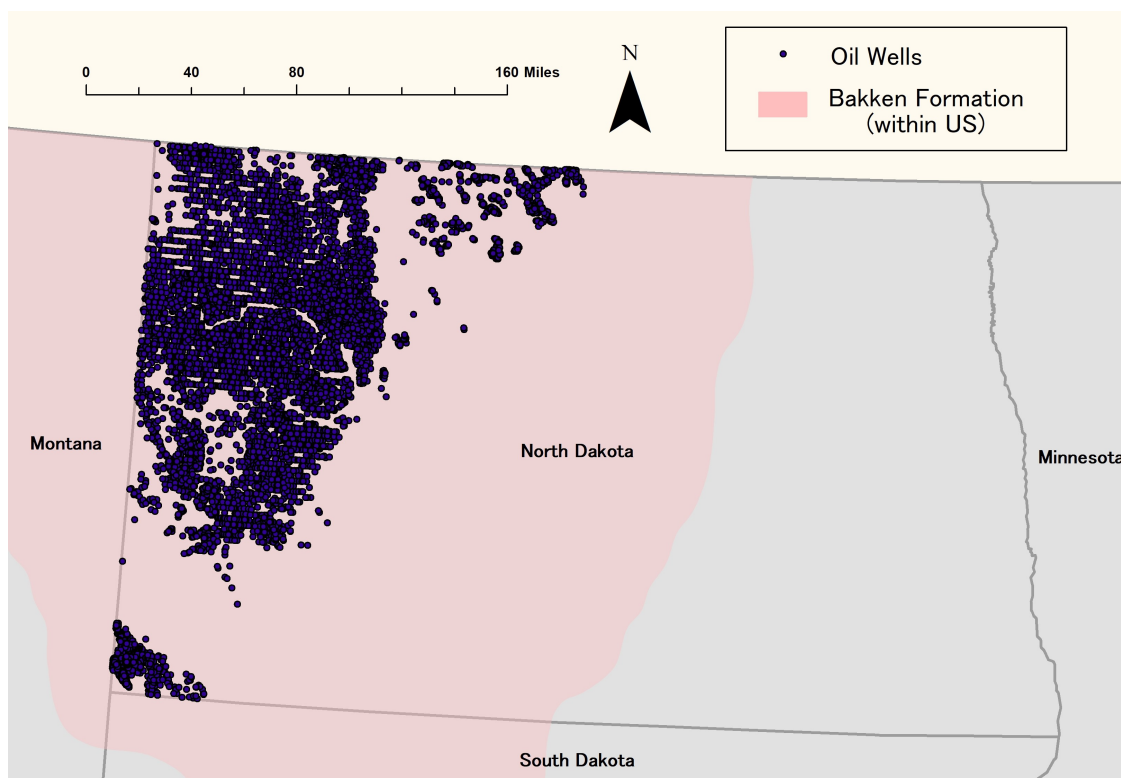


Figure 2.11: Locations of oil wells in North Dakota on Bakken Formation
Data Source: Monthly production reports (North Dakota Industrial Commission, Oil and Gas Division); 2010 US Census boundaries (ESRI) Source: Authors' calculations.

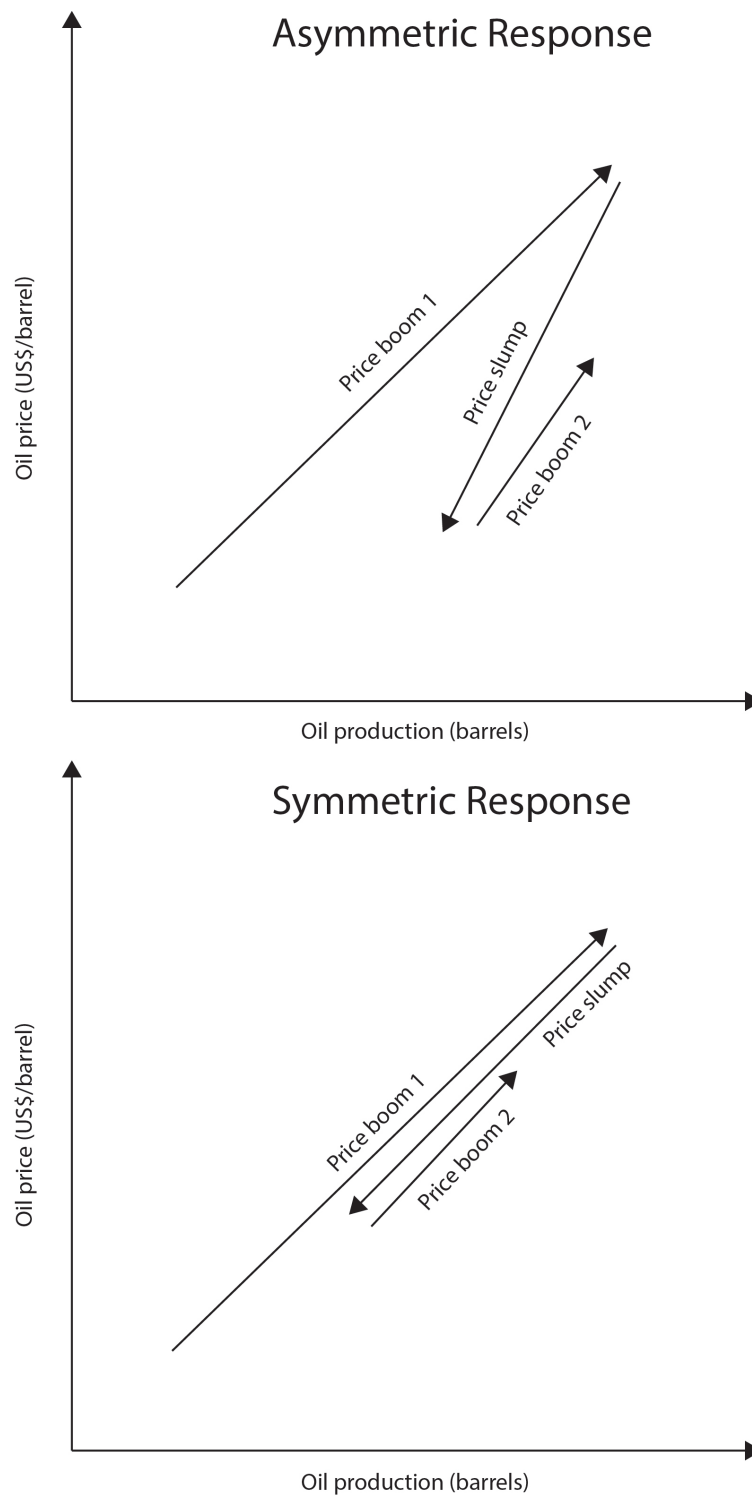


Figure 2.12: Asymmetric and symmetric supply response to oil price changes

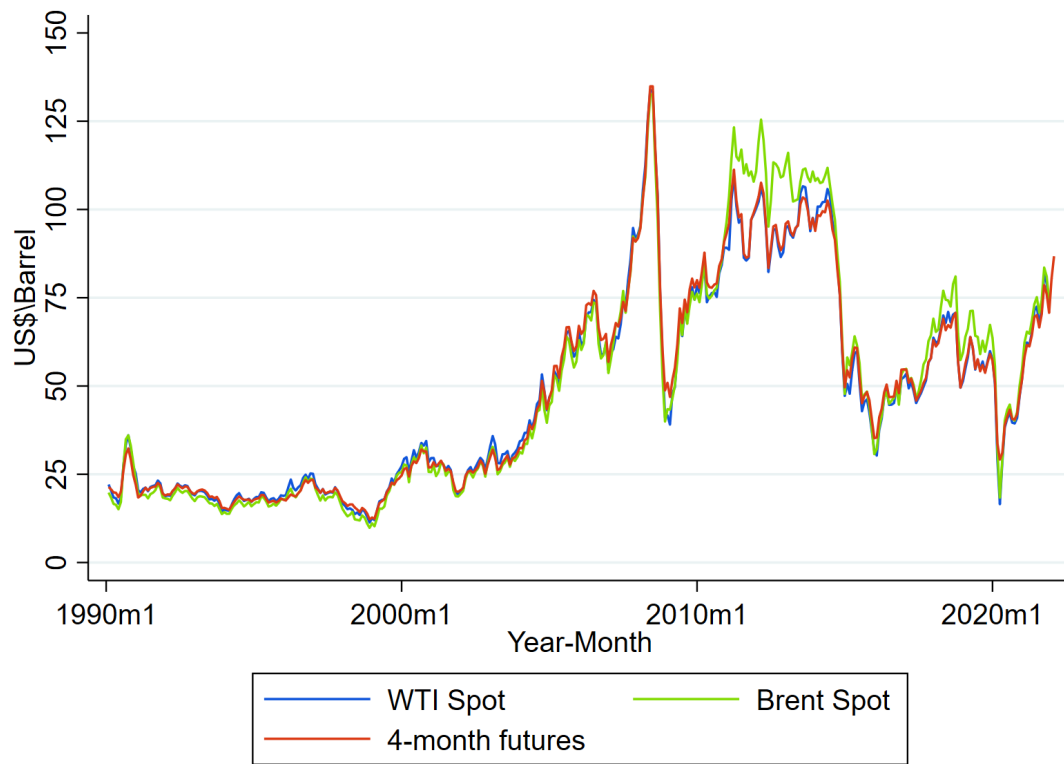


Figure 2.13: Spot and futures prices

2.8.2 Tables

Table 2.1: Summary Statistics

	Macro variables			
	Mean	SD	Min	Max
WTI Spot Price (\$/Barrel)	54.61	25.94	11.35	133.88
WTI 4 month futures (\$/Barrel)	55.03	25.69	12.23	134.89
Negative price change (\$/Barrel)	1.85	3.68	0.00	27.50
Positive price change (\$/Barrel)	1.88	2.66	0.00	14.28
US 10 Year Tresury Yield	3.23	1.92	0.62	8.89
MSCI Index (AWCI)	395.5	155.3	104.3	754.8
USD Index	87.79	8.23	69.06	112.17
Copper Price (\$/Tonne)	5,545	2,344	1,377	10,166

	Well-level variables			
	Count			
Observations (All)	2,856,749			
Observations (Horizontal)	1,644,904			
Observations (Vertical)	1,074,359			
Number of wells (All)	23,351			
Number of wells (Horizontal)	17,699			
Number of wells (Vertical)	4,926			

	Mean	SD	Max
Monthly oil production (Barrels) (All)	1,682.8	3,418.5	139,068
Monthly oil production (Barrels) (Horizontal)	2,560.7	4,106	139,068
Monthly oil production (Barrels) (Vertical)	430.6	1,274.9	92,094

Table 2.2: OLS and FE Models with IHS

VARIABLES	(1) OLS - H	(2) OLS - V	(3) FE - H	(4) FE - V
Production (t-1)	0.7064*** (0.0019)	0.7086*** (0.0055)	0.6321*** (0.0020)	0.6697*** (0.0057)
Production (t-2)	0.1033*** (0.0019)	0.2413*** (0.0054)	0.0453*** (0.0017)	0.2037*** (0.0054)
PC Negative	-0.3603*** (0.0670)	-0.1135*** (0.0193)	-2.0238*** (0.1182)	-0.4140*** (0.0199)
PC Positive	-0.2883*** (0.0605)	-0.0946*** (0.0137)	-1.7965*** (0.1072)	-0.3722*** (0.0147)
Constant	2.2495*** (0.1811)	0.4837*** (0.0343)	7.3964*** (0.3204)	1.4585*** (0.0381)
Observations	1,608,958	1,065,735	1,608,958	1,065,735
R-squared	0.6512	0.8831	0.5471	0.7833
Well FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	Yes	Yes
Macro Controls	Yes	No	No	No

Dependent variable is the hyperbolic sine transformed monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: FD Models with IHS

VARIABLES	(1) Prod - H	(2) Prod - V	(3) Prod - H	(4) Prod - V
Production (t-1)	0.6056*** (0.0100)	0.2455*** (0.0282)	0.5554*** (0.0061)	0.3056*** (0.0170)
PC Negative	-0.0842*** (0.0239)	-0.0724 (0.0480)	-0.0804*** (0.0233)	-0.0741 (0.0494)
PC Positive	0.0205** (0.0104)	-0.0313 (0.0226)	0.0222** (0.0102)	-0.0313 (0.0236)
Constant	0.0844*** (0.0169)	0.0603* (0.0354)	0.0801*** (0.0165)	0.0593 (0.0362)
Observations	1,591,175	1,060,184	1,573,466	1,054,730
Number of Wells	17,552	4,679	17,497	4,662
Month FE	Yes	Yes	Yes	Yes
Prod (t-2) IV	Yes	Yes	Yes	Yes
Prod (t-3) IV	No	No	Yes	Yes
<i>Hypothesis Test</i>				
$H_0 : \beta_{pc}^+ + \beta_{pc}^- \geq 0$				
$H_1 : \beta_{pc}^+ + \beta_{pc}^- < 0$				
<i>P - value</i>	0.0402		0.0485	

Dependent variable is the hyperbolic sine transformed monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

The Standard Question of Sourcing: Impact of environmental technology standards on sourcing decisions of heterogeneous firms

Michiyoshi Toya

Abstract:

This paper analyzes the impact of environmental regulations on firms' production behavior, focusing on technology standards. In particular, it develops a theoretical model of heterogeneous firms and offshoring to investigate whether technology standards always lead to similar outcomes to the widely-analyzed pollution tax case. Interestingly, I find that in cases where the standard increases fixed costs of domestic production significantly, lower productivity firms may choose to offshore while higher productivity firms do not; a selection pattern that is opposite to the case of pollution taxes. The paper also conducts preliminary industry-level empirical analysis to test the pollution haven effect prediction of the model, focusing on the Revision of Japan's Air Pollution Control Law implemented in 2018.

3.1 Introduction

3.1.1 Motivation

The industrial sector is a significant source of air emissions of toxic and environmentally harmful pollutants worldwide. For instance, in the US, combined emissions from fuel combustion, chemical manufacturing, metals processing and petroleum-related industries in 2017 accounted for roughly 23%, 28%, 82% of total emissions for VOC, CO and SO₂ respectively (EPA). As such, it is not surprising that policies to curb emissions of pollutants have often targeted the industrial sector specifically.

The trade and the environment literature has widely analyzed the rationales for and impacts of such environmental regulations. Traditionally, the literature has focused on macro-level analyses, with a large interest in assessing the pollution haven hypothesis; that stringent policies induce industries to shift production abroad where environmental regulations are low (e.g. Eskeland and Harrison (2003), Cole and Elliott (2005), Elliott and Shimamoto (2008), Levinson (2020)).

However, inspired by new-new trade models (most notably Melitz (2003)), recently some researchers have developed and tested theoretical models that investigate how environmental regulations impact firms' offshoring and exporting decisions under the setting of heterogeneous firms. For example, Kreickmeir and Richter (2014), Cao et al. (2016) develop models where firms can use a fraction of inputs to reduce pollution and can also invest in advanced abatement technology. The latter paper uses Chinese firm-level data to test model predictions. Cole et al. (2014) develop a model where firms have the option of outsourcing dirty production at the extensive margin, and test the model predictions empirically using Japanese data. The same authors further find empirical evidence of outsourcing of emission-intensive processes by Japanese firms (Cole et al. (2021)). Cherniwchan et al. (2017) develop a framework where firms have the option of outsourcing dirty production at the intensive margin.

The typical approach in these papers is to analyze the impact of pollution taxes. These models assume that the government sets a pollution tax, which enters into the firms' cost functions. However, actual environmental regulations include many policies other than pollution taxes, such as pollution quotas, taxes on pollution-generating inputs and environmental standards.

In particular, papers focusing on the impact of environmental standards on offshoring are scarce.

Yet, environmental standards are the most common form of environmental regulation and are widely used to require actors to account for environmental externalities (IPCC (2007, 2014)). Firm-level survey studies on seven OECD countries also suggest that firms in those economies perceive technology-based standards and performance-based standards they face to have significant influence on production activity (OECD (2007)).

The gap in the above emerging literature may not be important if pollution taxes and standards both led to similar production outcomes by firms, much like the ‘equivalence’ concept between a tariff and quota. However, while pollution taxes may often be thought of as a proportional price increase (that increases marginal cost), environmental standards may be associated with more discrete changes (e.g. an increase in fixed cost). It follows that there may be important distinctions in their impacts on sourcing and on pollution levels. This may yield rich implications in terms of which policy should be preferred if a government wishes to keep production of certain types of firms within the country, or if it wishes to reduce domestic or global pollution. Thus, it would be of high relevance to investigate the impact of environmental standards under the heterogeneous firm framework, and to see whether equivalence holds between environmental standards and pollution taxes. Doing so may bring about new findings to the trade and environment literature, as well as the broader environment literature stemming from Weitzman (1974), which analyzes cases under which taxes and standards lead to different outcomes.

In this paper, I investigate whether environmental standards and pollution taxes are always equivalent (i.e., whether one can always replicate a result under a standard with a pollution tax set at an appropriate level), under the newly emerging setting of heterogeneous firms and sourcing. I focus on one popular type of environmental standards, environmental technology standards, which involve a regulator stipulating the specific technology or equipment that the polluter must use. After briefly discussing examples of such technology standards (in Japan), in section 3.2, I develop a heterogeneous firm model of sourcing where firms are required to adopt a specific pollution-reducing equipment in their production process. The model predicts, among other things, that firms offshore to countries that have lower levels of technology standards, that more firms offshore once environmental standards are put in place, that selection into offshoring can be different for environmental technology standards and pollution taxes, and that equivalence therefore may not always hold. In section 3.3, I conduct a preliminary empirical analysis to investigate one of the

model predictions, namely that environmental technology standards lead to offshoring by more firms within an affected industry, in the context of tightening of Japanese mercury air emission standards in 2018. Finally, section 3.4 concludes and discusses possible extensions.

3.1.2 Examples of Environmental Technology Standards: the Case of Japan

Currently in Japan, the Basic Environment Law (established in 1993) provides the legislation for environmental regulations by the state. Regarding air pollution, the law is implemented mainly through a media-specific pollution control law named Air Pollution Control Law (first enacted in 1968, revised on numerous occasions since). The Air Pollution Control Law includes some prominent examples of environmental technology standards. For instance, coke ovens with production capacities of greater than or equal to 50-tons per day are identified as ‘Facilities That Generate Ordinary Particulates’, and these coke ovens must all install (among others) smokeless coal charges, charging cars with cover hoods and dust collectors. Mineral storage facilities of areas over 1000 square-meters are likewise identified as ‘Facilities That Generate Ordinary Particulates’, and must install (among others) water-spraying equipment and dust covers Ministry of Environment, Japan (MOE).

In addition, local governments often set environmental policies to complement national regulations. Once again, it is not uncommon to see technology standards in practice. For instance, the Tokyo Metropolitan Government has a voluntary requirement for installing low-NO_x, low-CO₂ boilers and heat-pumps and provides a list of products meeting the standard Tokyo Metropolitan Govt (n.d.).

For the purposes of the theoretical model below, I focus on a slightly simplified case where the government stipulates that all firms (within a sector) must adopt the same abatement technology.

3.2 Theoretical Framework

3.2.1 Basic Setting

I build on the frameworks presented in Antràs and Helpman (2004) and in Antràs (2015) to model the impact of environmental technology standards on heterogeneous firms that have an option to offshore production. I consider a simple two-country, two-sector, one-factor of production framework. Let us

call the two countries ‘Home’ and ‘Foreign’. Each economy consists of a homogeneous good sector and a differentiated good sector, with labor as the sole factor of production. Consumer preferences are the same in both countries and are given by the Cobb-Douglas form:

$$U = \beta_z \log z + \beta_s \log Q_s \quad (3.1)$$

where $\beta_z + \beta_s = 1$, and Q_s is:

$$Q_s = \left(\int_{\omega} q(\omega)^{(\sigma-1)/\sigma} d\omega \right)^{\sigma/(\sigma-1)} \quad (3.2)$$

where $\sigma > 1$.

z is the homogeneous good which I treat as the numeraire to pin down wages in Home and Foreign. Q_s is the differentiated good (with a continuum of varieties ω), represented by the ‘love of variety’ Dixit-Stiglitz preferences of equation (3.2), where σ is the elasticity of substitution between any pair of varieties.

The differentiated goods sector is characterized by monopolistic competition and firm heterogeneity (i.e., firms differ in productivity). After paying a sunk fixed cost of entry, firms draw a productivity level φ from a cumulative distribution function $G(\varphi)$. There also exists fixed costs of (domestic) production. These fixed costs are assumed to be common across firms. As opposed to standard firm heterogeneity models that focus on the selection into exporting, I assume that exporting involves neither fixed nor variable costs, meaning that all firms producing the differentiated good export. This allows me to simplify the model and to focus on firms’ selection into offshoring.

To model offshoring, I assume that the production of the differentiated good consists of two stages: headquarter services (which can be thought of as RD, finance or assembly) and manufacturing. The key distinction is that, while headquarter services must be done domestically, manufacturing production can be offshored. Offshoring, however, involves fixed costs (expressed for convenience in terms of units of domestic labor, f_{ow}) and variable trade costs (which is of the standard ‘iceberg’ type, $\tau > 1$). Given the presence of factors such as search costs and language barriers, I assume further that the fixed cost of offshoring is higher than the fixed cost of domestic production (i.e., in terms of units of domestic labor: $f_{ow} > f_{dw}$). I assume the fixed cost of domestic production is incurred if the firm engages in the manufacturing process domestically. For simplicity, I suppose that

there are no losses in a firm's productivity level φ when that firm offshores. For similar reasons, I impose symmetry between Home and Foreign regarding fixed costs and technology. The technology for producing the differentiated good in both countries, expressed as a function of productivity, is given by the Cobb-Douglas form:

$$q(\varphi) = \varphi \left(\frac{h(\varphi)}{\eta} \right)^\eta \left(\frac{m(\varphi)}{1-\eta} \right)^{1-\eta} \quad (3.3)$$

where h and m are headquarter services and manufacturing production respectively.

Finally, I introduce environmental standards into this model of offshoring. I assume that the 'manufacturing production' process creates pollution (i.e., pollution is increasing in units of manufacturing), and that the Home government sets an environmental technology standard to reduce this pollution. Specifically, as briefly outlined in the previous section, I consider the case where the Home regulator specifies an equipment that firms producing the differentiated good must all adopt in their production process, such as a filter system for production plants. I assume that this entails exogenous fixed costs $f_s w$. In other words, if a firm decides to carry out the manufacturing process domestically, it must introduce the filter regardless of the size of production. For simplicity, I assume that Foreign does not have a technology standard in place.

3.2.2 Benchmark case with no environmental technology standard

To begin with, I analyze the case with no environmental technology standard, focusing on Home firms in the differentiated good sector. Recalling the production function of equation (3.3), and given that firms can either manufacture domestically or offshore, the total cost of production for the two cases can be expressed as:

Domestic:

$$C_D(q, \varphi) = f_D w + \frac{q}{\varphi} w \quad (3.4)$$

Offshore:

$$C_O(q, \varphi) = f_O w + \frac{q}{\varphi} (w^\eta) (\tau w^*)^{1-\eta} \quad (3.5)$$

Noting that firms will charge a constant markup ($\frac{\sigma}{\sigma-1}$ times the marginal cost) for its price, the corresponding profit functions are:

Domestic:

$$\pi_D(\varphi) = w^{1-\sigma} B \varphi^{\sigma-1} - f_D w \quad (3.6)$$

Offshore:

$$\pi_O(\varphi) = ((w^\eta)(\tau w^*)^{1-\eta})^{1-\sigma} B \varphi^{\sigma-1} - f_O w \quad (3.7)$$

where

$$B = \frac{1}{\sigma} \left(\frac{\sigma}{(\sigma-1)P} \right)^{1-\sigma} \beta L (w + w^*)$$

and

$$P = \left(\int_{\omega} p(\omega)^{1-\sigma} d\omega \right)^{1/(1-\sigma)}$$

B is the residual demand, L is the amount of labor at Home (which is equal to that of Foreign), w^* is Foreign wage and P is the common price index associated with equation (3.2).

The profit functions are increasing linearly in the transformation of productivity, $\varphi^{\sigma-1}$, where $\sigma > 1$ as before.

Firms essentially face a trade-off between producing domestically and facing lower fixed costs, no trade costs but (potentially) higher wages, and offshoring and facing (potentially) lower wages, but higher fixed costs and trade costs. Higher productivity firms (who will produce more) are more likely to offshore as they can spread out the fixed costs of offshoring. By comparing the profit functions, one can say that, whenever

$$f_O > \left(\frac{w}{\tau w^*} \right)^{(1-\eta)(\sigma-1)} f_D$$

or hence as long as $w > \tau w^*$, some firms will always offshore to Foreign.

The case where $w > \tau w^*$ is depicted in Figure 3.1. Firms with a productivity level below $(\tilde{\varphi}_D)^{\sigma-1}$ can never make positive profits so exit the market. Firms between $(\tilde{\varphi}_D)^{\sigma-1}$ and $(\tilde{\varphi}_O)^{\sigma-1}$ can achieve higher (and positive) profits by producing domestically so produce at Home. Firms above $(\tilde{\varphi}_O)^{\sigma-1}$ find it more profitable to offshore so offshore the manufacturing process to Foreign.

The case where $w < \tau w^*$ is illustrated in Figure 3.2. In this case, trade costs or Foreign wages are so high that offshoring is never the preferred option for any of the firms. Firms below $(\tilde{\varphi}_O)^{\sigma-1}$ exit, and firms above this threshold simply produce domestically.

3.2.3 Case with environmental technology standard

Now, let us consider a case with environmental technology standards in place. As described earlier, if a firm decides to produce domestically, it faces an additional fixed cost of $f_s w$.

Thus, the total cost of production becomes:

Domestic:

$$C_D(q, \varphi) = f_D w + \mathbf{f}_S \mathbf{w} + \frac{q}{\varphi} w \quad (3.8)$$

Offshore:

$$C_O(q, \varphi) = f_O w + \frac{q}{\varphi} (w^\eta) (\tau w^*)^{1-\eta} \quad (3.9)$$

Associated profits are:

Domestic:

$$\pi_D(\varphi) = w^{1-\sigma} B \varphi^{\sigma-1} - f_D w - \mathbf{f}_S \mathbf{w} \quad (3.10)$$

Offshore:

$$\pi_O(\varphi) = ((w^\eta) (\tau w^*)^{1-\eta})^{1-\sigma} B \varphi^{\sigma-1} - f_O w \quad (3.11)$$

where B and P are the same as before.

By looking at the difference between (3.11) and (3.10) and taking partial derivatives, one can say that any firm is more likely to offshore: the higher the relative wage w/w^* , the lower the trade cost τ , the lower the fixed cost of offshoring, the higher the fixed cost of domestic production, and finally, the higher the level of environmental technology standard (expressed in terms of f_s).

Figure 3.3 refers to the case where $w > \tau w^*$. The increase in fixed cost of domestic production shifts down the profit function associated with domestic production, $\pi_D(\varphi)$. As a result, the productivity threshold for making positive profits, $(\tilde{\varphi}_D)^{\sigma-1}$, increases, forcing some of the less productive firms out of the market. More interestingly, the productivity cutoff for offshoring decreases, and more firms offshore compared to the benchmark case. Thus, the model predicts a familiar ‘pollution haven effect’, where firms offshore to countries that have lower levels of environmental technology standards.

In the benchmark case, offshoring would never occur when $w < \tau w^*$. However, if the fixed costs arising from the technology standard is sufficiently high, a situation depicted in Figure 3.4 can

arise. Just as in the previous case, the productivity cutoff for remaining in the market increases. However, out of the firms that survive, the less productive firms choose to offshore, whereas the most productive firms continue producing domestically. Intuitively, due to the technology standard, the fixed costs of domestic production are now higher than that of offshoring.

Thus, even if Foreign wage or trade costs are high, the less productive firms would prefer to offshore, whereas the more productive firms can afford the fixed costs and find it more profitable to operate domestically.

3.2.4 Comparison: Case with pollution tax

Finally, to investigate the differences between the ‘technology standard’ model and a ‘pollution tax’ model, I analyze the latter case where the government sets a (multiplicative) pollution tax, $t > 1$, to each unit of ‘manufacturing’ done domestically. The total costs are:

Domestic:

$$C_D(q, \varphi) = f_D w + \frac{q}{\varphi} (w^\eta) (t w)^{1-\eta} \quad (3.12)$$

Offshore:

$$C_O(q, \varphi) = f_O w + \frac{q}{\varphi} (w^\eta) (\tau w^*)^{1-\eta} \quad (3.13)$$

The corresponding profits are:

Domestic:

$$\pi_D(\varphi) = ((w^\eta) (t w)^{1-\eta})^{1-\sigma} B \varphi^{\sigma-1} - f_D w \quad (3.14)$$

Offshore:

$$\pi_O(\varphi) = ((w^\eta) (\tau w^*)^{1-\eta})^{1-\sigma} B \varphi^{\sigma-1} - f_O w \quad (3.15)$$

The impact on selection in offshoring, for the case $w > \tau w^*$, is analyzed in Figure 3.5. The pollution tax enters the marginal cost side (and not the fixed cost side) of the cost function, and hence has the effect of rotating the domestic profit function downwards. The y-intercept, $-f_D w$, is unchanged. The pollution tax has a similar effect to the pollution tax case when $w > \tau w^*$, in the sense that the productivity cutoff for survival increases and that more firms offshore.

When $w < \tau w^*$, the impact can be very different from that of standards. Figure 3.6 depicts this case. Just as in the technology standard case, a pollution tax may give rise to offshoring even in

the case of $w < \tau w^*$. However, the selection effect is exactly the opposite. Under pollution taxes, similar to the benchmark case, the firms that offshore are always the most productive ones. Since the y-intercept of $\pi_D(\varphi)$ does not shift down, there can never be cases where the less productive firms offshore and the more productive firms produce domestically. This result is consistent with findings from recent papers analyzing the impact of pollution taxes on offshoring behavior (e.g. Cole et al. (2014), Cherniwchan et al. (2017)).

3.2.5 Summary of model predictions and generalizability

The simple framework with environmental technology standards analyzed in this section predicts that firms are more likely to offshore: the higher the relative wage (domestic wage over foreign wage), the lower the trade cost, the lower the fixed cost of offshoring, the higher the fixed cost of domestic production, and more interestingly, the higher the domestic level of environmental technology standards. On a similar note, the model predicts a pollution haven effect, and that differences in standards can act as a source of trade. Finally, and most importantly, when $w < \tau w^*$, the model predicts that technology standards can give rise to selection effects into offshoring that are opposite to predictions from benchmark ‘pollution tax’ models, in that the less productive firms offshore and more productive firms produce domestically.

One should also note that the model results may be easily generalizable to environmental policies affecting fixed costs or affecting both fixed costs and marginal costs. Figure 3.7 illustrates that the ‘peculiar’ case of Figure 3.4 can persist in the case of a policy affecting both these costs. In other words, while we have focused on technology standards that only affect fixed costs, the theory predictions may well carry through for other types of standards and policies. This observation is relevant given empirical findings that large firms face lower per unit costs of environmental regulatory compliance (e.g. Brock and Evans (1985), Markusen (1997), Dean et al. (2000), Chakrabarti and Mitra (2005)), which suggest that in many cases, there are substantial fixed costs associated with environmental regulations (be them standards or other types of policy).

3.3 Empirical Analysis

In this section, I conduct a preliminary empirical analysis to test one key model prediction, namely that of the pollution haven effect. The model suggests that more firms within an industry offshore if an environmental technology standard is enforced in that industry. While the precise cost of environmental standards to firms are difficult to quantify (and hence difficult to observe in data), one can focus on examples of introduction of new environmental technology standards and compare the pre and post of the policy change to see whether environmental technology standards cause a higher fraction of firms to offshore. In this context, I will turn to the recent 2015 Revision of the Air Pollution Control Law in Japan (enacted in 2018), which newly regulates mercury air emissions.

3.3.1 Background

Annual global air emissions of mercury vary in the range of 5500 to 8900 tonnes. Anthropogenic emissions account for a sizeable 30% of global emissions, which is three times that of natural emissions (that account for 10% of global emissions). The remaining 60% of emissions consists of secondary re-emissions of previously-emitted mercury (through the cycle of evaporation and precipitation). Hence, reducing anthropogenic emissions is crucial in mitigating mercury pollution over the long-term (MOE).

In Japan, annual air emission of mercury ranges from 17 to 21 tonnes, and anthropogenic emissions play a large role. The main sources are: cement sector (29%), steel sector (25%), waste incineration (24%), volcanoes (8%), nonferrous metals sector (5%), and coal-fired power plants (5%) (METI).

Once released, mercury can travel long distances in the atmosphere before it is deposited back to land or water, after which it can bioaccumulate and biomagnify through the food chain. In particular, methylmercury, which is formed when bacteria decompose mercury, is highly toxic and cause significant harm to central and peripheral nervous systems (EPA, WHO).

In the case of Japan, while mercury discharged directly into rivers has been regulated since the 1970s through the Water Pollution Control Law, there were no regulations in place regarding air emissions of mercury. However, following increasing international awareness regarding the importance of controlling mercury air emissions, as well as the adoption of the Minamata Convention

on Mercury in 2013, the Japanese government revised the Air Pollution Control Law in 2015 to cover mercury as well. The revisions were implemented in 2018, following the entry into force of the Minamata Convention.

Specifically, the revision required firms producing cement, nonferrous metals, steel; as well as coal-fired power plants and waste incineration facilities; to introduce BAT (best available technologies) such as filters and scrubbers in furnaces, and to meet ambient standards (e.g. 50-80 $\mu\text{g}/\text{Nm}^3$ of mercury for cement production) (NRI, MOE). The fact the policy was an environmental technology standard targeting specific industries, and the fact that no regulations were in place prior to the policy, makes this Air Pollution Control Law revision an interesting case for analysis.

3.3.2 Data

The dataset of interest is the *Kigyō Katsudō Kihon Chōsa Houkokusho* (The Results of the Basic Survey of Japanese Business Structure and Activities) published by the Ministry of Economy, Trade and Industry (METI) of Japan, which covers over 20,000 Japanese firms annually from the 1990s to 2019. Unfortunately, firm-level data are confidential and not readily available. However, data aggregated at the industry-level are available, so I will conduct a preliminary industry-level analysis based on this dataset.

Interestingly, since 2009, the survey began to regularly monitor the number of firms within each industry that are engaged in overseas offshoring of their manufacturing process. From this we can obtain the share of firms that offshore within an industry, which is our dependent variable of interest. The data also contain information on industry-level characteristics such as ‘average number of employees per firm’ and ‘average profits per firm’.

I focus on industries in the manufacturing sector, and drop retailing and services industries (which are likely to be characteristically different) from the sample. Of the 61 manufacturing industries, I identify 9 industries belonging to the cement, nonferrous metals and steel sector, which will serve as the treatment group. (The coal-fired power industry and waste incineration industry are not covered in the sample.) The remaining 52 industries act as the control group, since they were not affected by the policy.

Following the adoption of the Minamata Convention in 2013, the policy was concretized in 2015 and then implemented in 2018. Pre-policy (2009-2017) and post-policy (2018-2019) summary

statistics for both the control and treatment groups are provided in Table 3.1 and Table 3.2. As these tables demonstrate, foreign offshoring of the manufacturing process is undertaken by a small fraction of firms. Pre-policy, the share of firms that offshore is 8.8% for the control group industries and 6.6% for the treatment group on average, and this difference is statistically significant. Post-policy, the share of firms that offshore appears to increase for both groups, but we observe a slightly larger increase for the treatment group. The difference between the two groups is no longer statistically significant (possibly due to low sample size for post-policy years), but the results would imply a crude difference-in-difference estimate of 0.2% point increase in offshoring share following the policy. On another note, while some of the differences are not statistically significant, firms in the treatment group industry are somewhat smaller on average and appear to make slightly less profits than firms in the control group.

3.3.3 Empirical Strategy

To carefully investigate the impact of the Revision of the Air Pollution Control Law on offshoring, I use the following difference-in-difference specification with two-way fixed effects:

$$OffshoreShare_{i,t} = \alpha + \beta_1 Standard_{i,t} + \lambda X_{i,t} + \delta_i + \theta_t + \varepsilon_{i,t} \quad (3.16)$$

The dependent variable is the share of firms that offshore abroad in industry i in year t . $Standard_{i,t}$ is an indicator variable that switches on for post-policy periods (i.e., years 2018 and 2019) for the treated industries. (For industries in the control group, this indicator variable equals 0 for all sample years.) The coefficient of interest is β_1 , which we expect to be positive if environmental technology standards cause more firms to offshore abroad. $X_{i,t}$ are control variables. Since average size and profits of firms within an industry can affect the probability of offshoring (i.e., as implied by the model, the larger the average size and profits of surveyed firms, the more likely we are to observe offshoring), ‘average number of employees per firm’ and ‘average profits per firm’ are included as control variables. δ_i and θ_t industry fixed effects and year fixed effects respectively.

I also conduct an event study analysis to test parallel pre-trends and to examine yearly impacts, using the following specification:

$$OffshoreShare_{i,t} = \alpha + \sum_{\tau=-9}^1 \beta_{\tau} Standard_{\tau,i} + \lambda X_{i,t} + \delta_i + \theta_t + \varepsilon_{i,t} \quad (3.17)$$

where $Standard_{\tau,i}$ is an indicator for each year τ relative to the policy implementation year of 2018. τ ranges from -9 to 1 in the sample. For the control group, all $Standard_{\tau,i}$'s are set equal to zero. $\tau = -1$ is set as a reference year.

A potential concern of this analysis is that of simultaneity bias. As Cole et al. (2014) discuss, while environmental regulations (including technology standards) may cause firms to offshore, offshoring could also impact regulations. For instance, in sensitive industries which the government would like to protect, an increase in offshoring may induce the government to relax environmental regulations. To address this problem, I also consider a lagged dependent variable specification:

$$OffshoreShare_{i,t} = \alpha + \beta_1 OffshoreShare_{i,t-1} + \beta_2 Standard_{i,t} + \lambda X_{i,t} + \theta_t + \varepsilon_{i,t} \quad (3.18)$$

where the dependent variable lagged by one year enters the right hand side. However, this specification is subject to Nickell bias (due to correlation between the lagged dependent variable and the error term). Since we are dealing with a 'small T, larger N' sample here, I further adopt the GMM approach developed by Blundell and Bond (1998), using second and third lags as instruments.

3.3.4 Results

The results of specification (3.16) are provided in Table 3.3. The coefficients, taken at face value, would imply that the environmental standard introduced in 2018 is associated with a 0.15% point increase in the 'share of firms that offshore' on average. Given that the share of firms that offshore is fairly low to begin with (i.e., around 6.6% pre-policy), this 0.15% point increase may be thought of as a 2% increase in relative terms. The sign is consistent with the model prediction. The estimates also suggest that, if 'workers per firm' increase by 100 people within an industry, that industry is likely to see 0.13% more offshoring. An increase in 'average profits per firm' within an industry by 100 million yen (or around \$770,000) is associated with 0.002% more offshoring in that industry, which may not be very significant in economic magnitude. However, importantly, it should be noted that all of these coefficients are statistically insignificant.

Figure 3.8 shows the event study plot. All estimates are statistically insignificant. While we cannot reject parallel trends prior to the policy, we cannot reject null results post-policy either. Looking at the coefficients, one could perhaps argue that there may be some suggestive evidence of the announcement effect, given that we start observing larger estimates from around 3-4 years before the implementation of the policy (which coincides with the discussion and concretization period of the Revision). Nevertheless, given the statistical insignificance, it would be difficult to make too strong a claim.

Table 3.4 demonstrates the results of the lagged dependent variable specification. Estimates for the slightly naive specification of equation (3.18) is provided in column 1. While statistically insignificant, the coefficient on standard is mildly negative. Column 2 corrects for the possible Nickell bias using the Blundell and Bond (1998) methodology. Here, the sign of the coefficient is consistent with model predictions. The environmental standard is associated with a 2% point increase in share of firms that offshore, which is larger in magnitude than the estimates from Table 3.3. However, once again, this estimate is not statistically different from zero.

In sum, while the signs of the coefficients are consistent with the model, the results are statistically insignificant across all specifications. This is largely due to small sample size, in particular, of the treatment group. With only 9 observations per year for the treatment group, and with only 2 years post-policy to consider at this point in time, it is quite difficult to obtain statistically significant results. The 2018 Revision of the Air Pollution Control Law is a neat empirical example in that the regulated industries are easily identifiable, but given that the policy only affected a few industries, there are some limitations to what one can conclude from an industry-level analysis.

3.4 Conclusion and Discussion

In this paper, I developed a heterogeneous firm model of sourcing where firms are required to adopt a specific pollution-reducing equipment in their production process. The model predicts the pollution haven effect and importantly shows that selection into offshoring can be different for environmental technology standards and pollution taxes. The fact that equivalence may not always hold yields rich implications especially in terms of which policy should be preferred if a government wishes to keep production of certain types of firms within the country. Standards may be preferred over

pollution taxes if the government does not wish the most productive firms to offshore production. The findings of this paper point to the importance of research to focus not only on the impact of pollution taxes but also that of environmental standards.

Although its simple structure is useful, the model developed in this paper is by no means a definitive one. For instance, firm productivity, once drawn, is assumed to be fixed. In a more dynamic setting, one may expect firm productivity to differ across time in particular, with environmental technology standards in place, firms' productivities may increase over time conditional on domestic production.

The model also assumes that all firms (regardless of productivity or amount of output) face the same level of fixed cost due to the environmental technology standard, but governments often set technology requirements based on the size of the firm (e.g. firms/plants above a certain threshold size must adopt the filter; or small firms are only required to install small filters whereas large firms are required to install large filters), in which case the fixed cost incurred by smaller firms would generally be less than that incurred by larger firms.

Likewise, the model assumes perfect enforceability, but on a more realistic note, firms may choose their abatement levels based on the cost of complying with the standard, the penalty fee of non-compliance and the likelihood of inspection (i.e., the probability that non-compliance will be found out). Another possible limitation is that the model does not allow for horizontal FDI or moving the headquarters abroad, where firms may produce the final good elsewhere.

Finally, in terms of the model, it would be also be interesting to analyze the impact on pollution levels (both domestically and globally), which will crucially depend on the pollution and abatement functions as well as the productivity distribution of firms.

This paper also conducts preliminary empirical analysis to test the pollution haven effect of environmental technology standards suggested by the model. Focusing on Japan's 2018 Revision of the Air Pollution Control Law, which introduced mercury abatement technology standards on cement, nonferrous metals and steel industries, I use a difference-in-difference approach to estimate the impact of the regulation at the industry-level. Although the results are statistically insignificant (largely due to small sample size), estimated signs of the coefficients are consistent with theory predictions, suggesting an increase in share of firms that offshore following the policy change.

Mild suggestive evidence at the sectoral level gives a rationale for conducting further analyses

at the firm level. Collaborating with authorized personnel and analyzing the firm-level version of the METI dataset would be a promising next step. Firm-level data would immensely mitigate the low sample size issue of the industry-level data, and would allow for logit regressions in the spirit of:

$$\text{logit}[Pr(Offshore(i, t)) = 1] = \alpha + \beta_1 Standard_{i,t} + \lambda X_{i,t} + \delta_i + \theta_{s,t} + \varepsilon_{i,t} \quad (3.19)$$

where subscripts i , s , t denote firm, sector and time respectively. This is the firm-level variant of equation (3.16), where the dependent variable is now the probability that a firm offshores, and $\theta_{s,t}$ is the sector-year fixed effect. Firm-level data would allow for estimation of heterogeneous effects, from which we can for instance investigate which types of firms (e.g. low productivity vs high productivity firms) choose to offshore following the 2018 policy change.

Digressing from the Japanese example, another promising approach may be to investigate firm behavior for countries that recently joined the EU (e.g. Czech Republic, Poland, Bulgaria, Croatia), and to run regression (3.19). The rationale is that joining the EU may be thought of as an exogenous shock to environmental regulation levels. One would not expect countries to join the EU because they wish to tighten environmental regulations, so the simultaneity bias discussed in section 3 would be less of an issue. An implicit assumption in these analyses would be that wages in countries that recently joined the EU are low compared to other EU countries that are likely to be the main destination countries, and so we may well observe the interesting case highlighted by the model where $w < \tau w^*$ and low productivity firms offshore. Considering firm-level analyses as well as model extensions discussed above would be promising areas for future research.

3.5 Figures and Tables

3.5.1 Figures

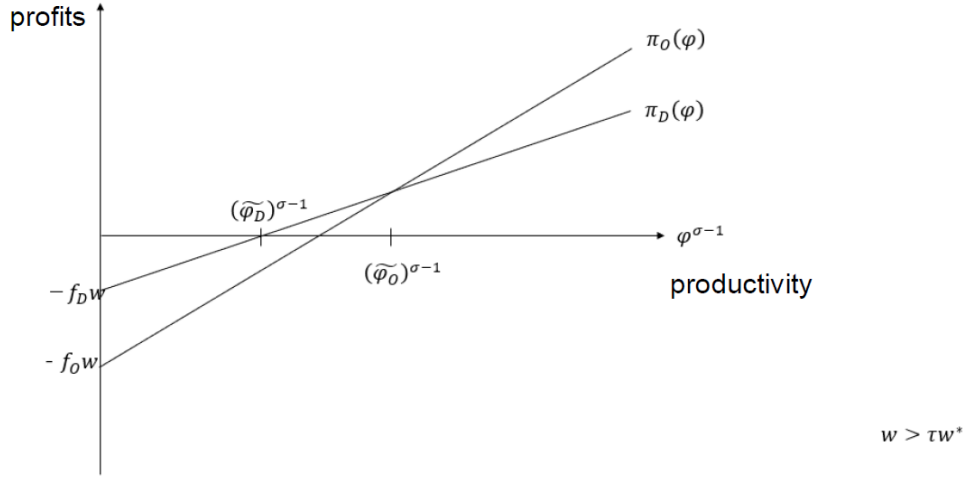


Figure 3.1: Benchmark case (when $w > \tau w^*$)

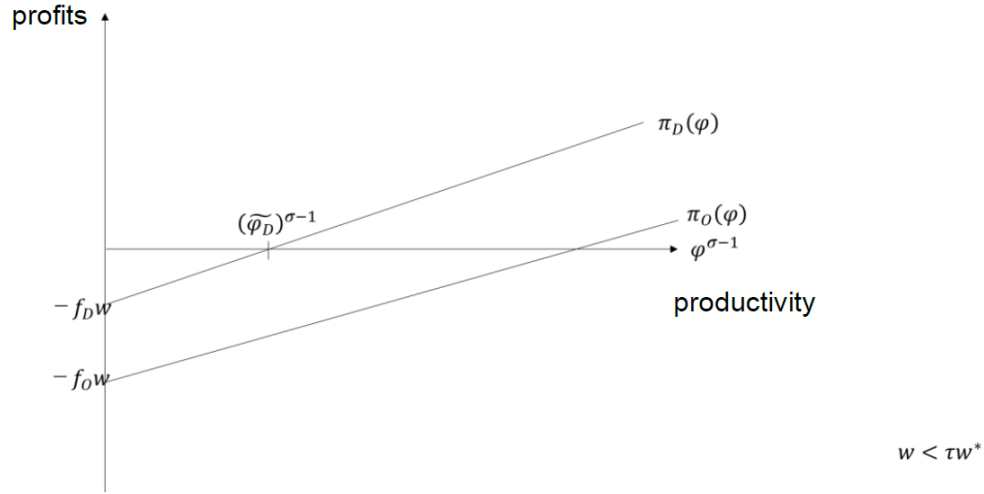


Figure 3.2: Benchmark case (when $w < \tau w^*$)

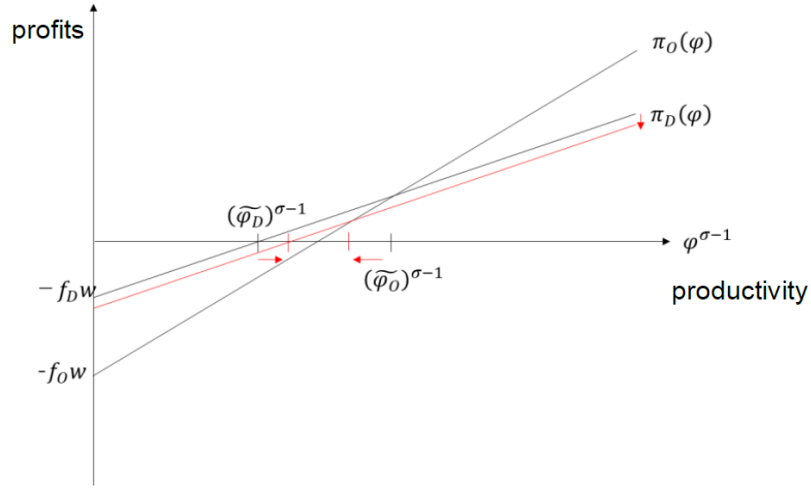


Figure 3.3: Environmental technology standard case (when $w > \tau w^*$)

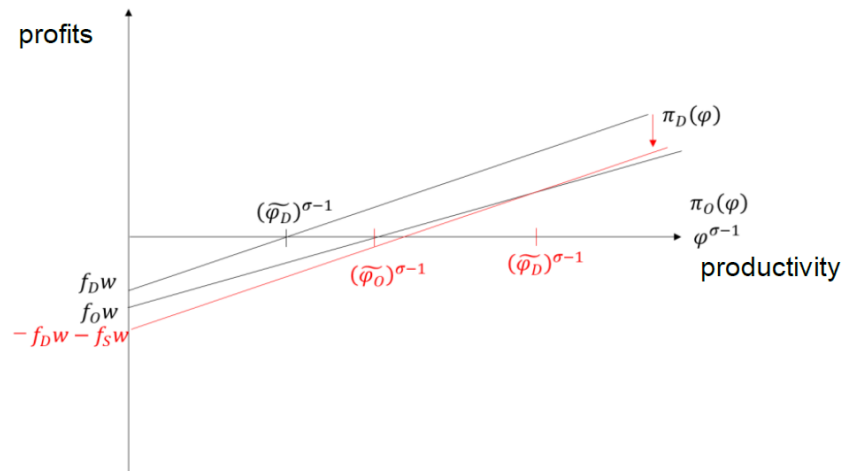
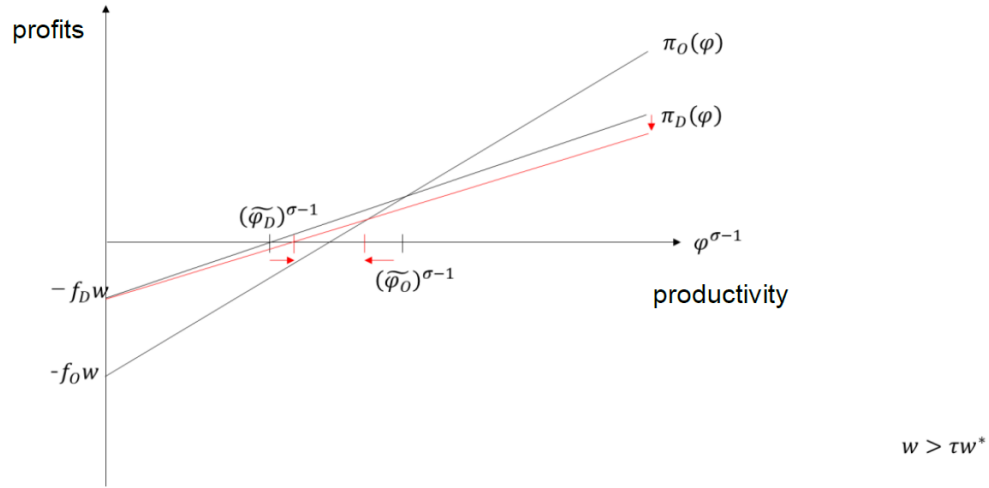
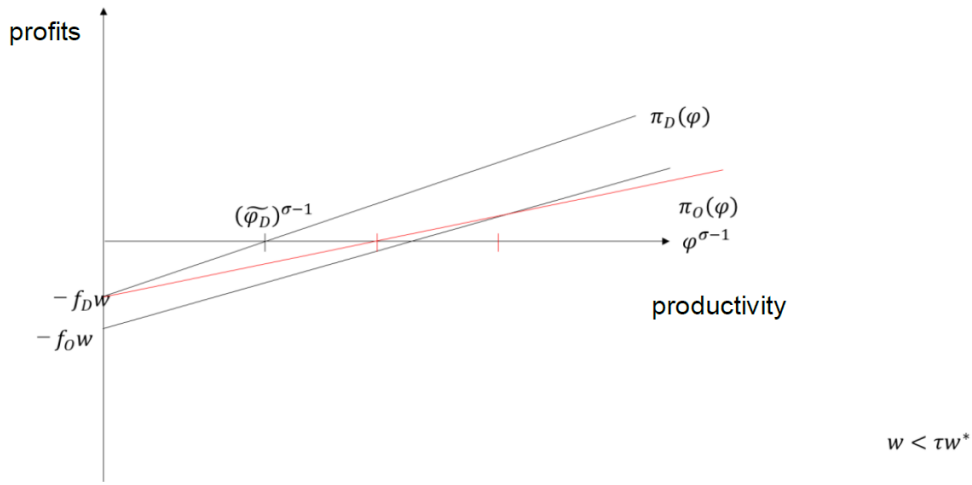


Figure 3.4: Environmental technology standard case (when $w < \tau w^*$)

Figure 3.5: Pollution tax case (when $w > \tau w^*$)Figure 3.6: Pollution tax case (when $w < \tau w^*$)

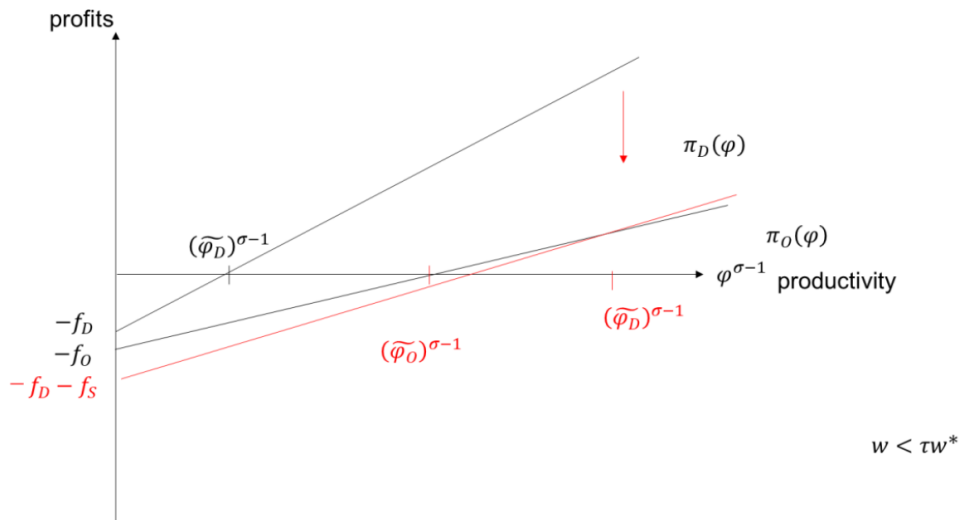


Figure 3.7: Policy affecting both fixed and variable cost (when $w < \tau w^*$)

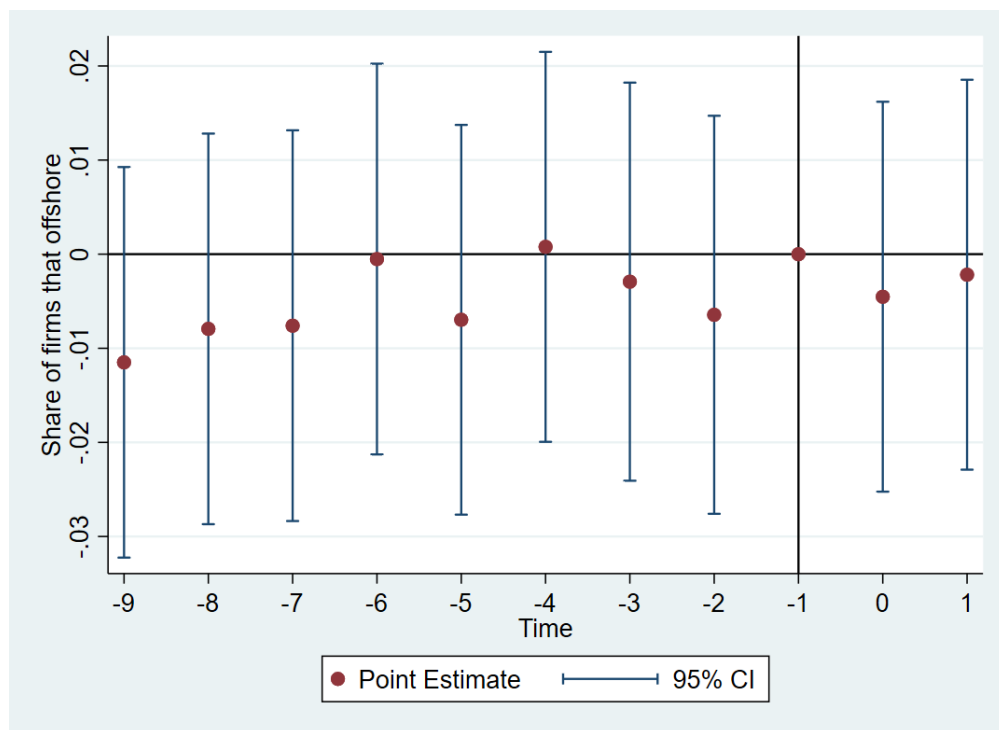


Figure 3.8: Event Study Plot

3.5.2 Tables

2009-2017 Average Variable	(1) Control	(2) Treatment	(3) Difference
share of firms that offshore	0.088 (0.057)	0.066 (0.033)	-0.022*** (0.007)
number of firms	208.500 (235.374)	257.111 (189.166)	48.611* (27.582)
workers per firm	502.539 (545.892)	308.092 (136.688)	-194.447*** (61.026)
profits per firm (million yen)	857.690 (3,743.474)	401.243 (656.932)	-456.447 (417.352)
Observations	468	81	549

Note: ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 3.1: Summary Statistics for Pre-Policy Years (2009-2017)

2018-2019 Average Variable	(1) Control	(2) Treatment	(3) Difference
share of firms that offshore	0.100 (0.068)	0.079 (0.039)	-0.020 (0.016)
number of firms	203.500 (238.847)	258.444 (201.166)	54.944 (59.706)
workers per firm	577.440 (758.634)	325.554 (157.606)	-251.886 (180.065)
profits per firm (million yen)	1,095.442 (6,307.410)	342.289 (1,161.092)	-753.153 (1,495.950)
Observations	104	18	122

Note: ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 3.2: Summary Statistics for Post-Policy Years (2018-2019)

VARIABLES	share of firms that offshore
standard	0.00149 (0.00567)
workers per firm	1.31e-05 (8.94e-06)
profits per firm	2.08e-07 (7.90e-07)
Observations	603
R-squared	0.892
Industry FE	YES
Year FE	YES

Note: Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Results of the two-way FE model

VARIABLES	(1) share of firms that offshore	(2) share of firms that offshore
lag of share of firms that offshore	0.505*** (0.0416)	0.894*** (0.0577)
standard	-0.000572 (0.00509)	0.0230 (0.0487)
workers per firm	2.09e-05** (1.04e-05)	1.76e-05 (4.35e-05)
profits per firm	-3.40e-07 (8.61e-07)	-2.85e-06 (7.61e-06)
Observations	536	536
R-squared	0.920	-
Industry FE	YES	-
Year FE	YES	YES

Note: Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Results of the lagged dependent variable model

Appendices

4.1 Chapter 1 Appendix

4.1.1 Additional Figures

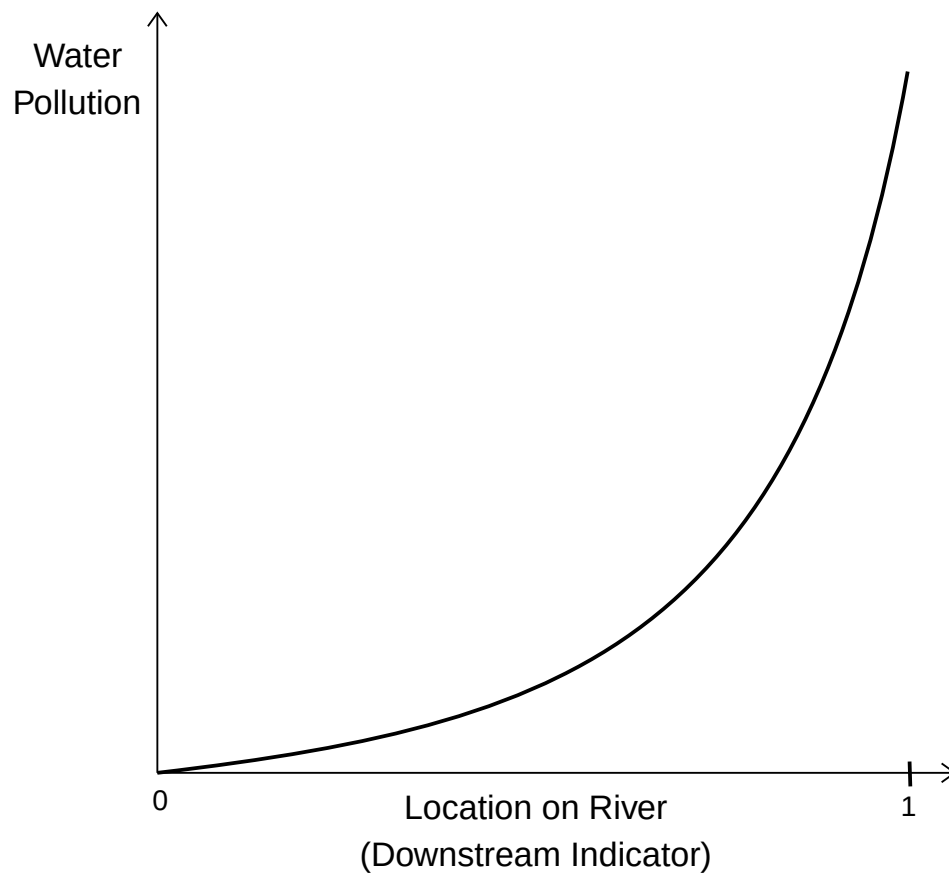


Figure A1: Prediction of Negative Externality Theory

Notes: This figure shows a convex relationship between downstream indicator and water pollution implied by the negative externality theory of Lipscomb and Mobarak (2016).

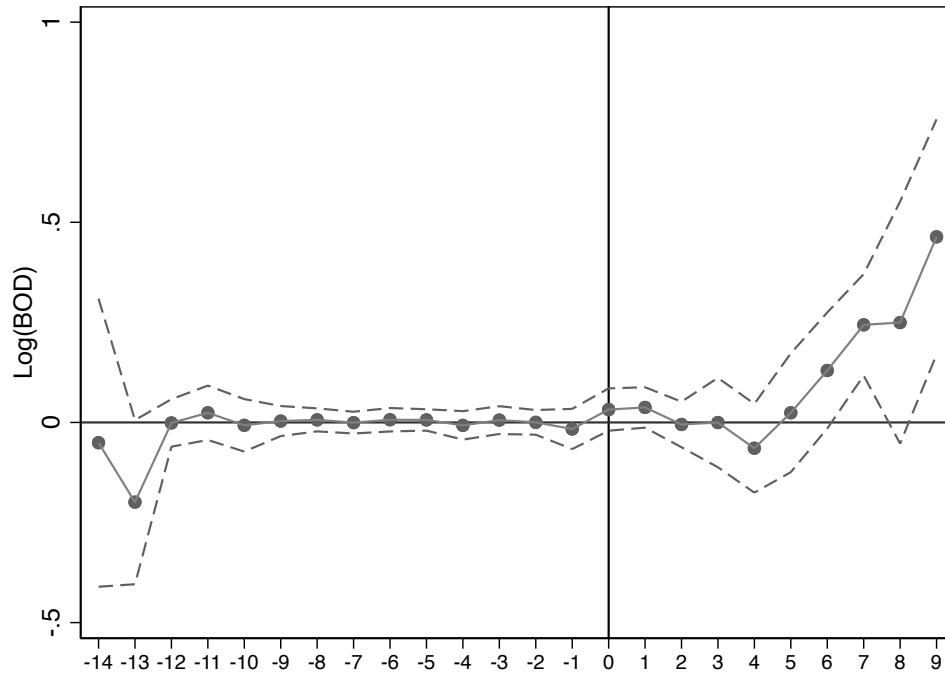


Figure A2: Event Study Results (Early Mergers versus Late Merger)

Notes: The sample is limited to monitoring stations in municipalities which have experienced mergers. (We drop monitoring stations in municipalities that have never merged.) This figure shows coefficients of estimators according to the Callaway and Sant'Anna (2021) methodology. The 95% confidence intervals are shown with dashed lines. Standard errors are clustered at the municipality level. This specification includes monitoring station fixed effects, year fixed effects, and product shipment values as a control.

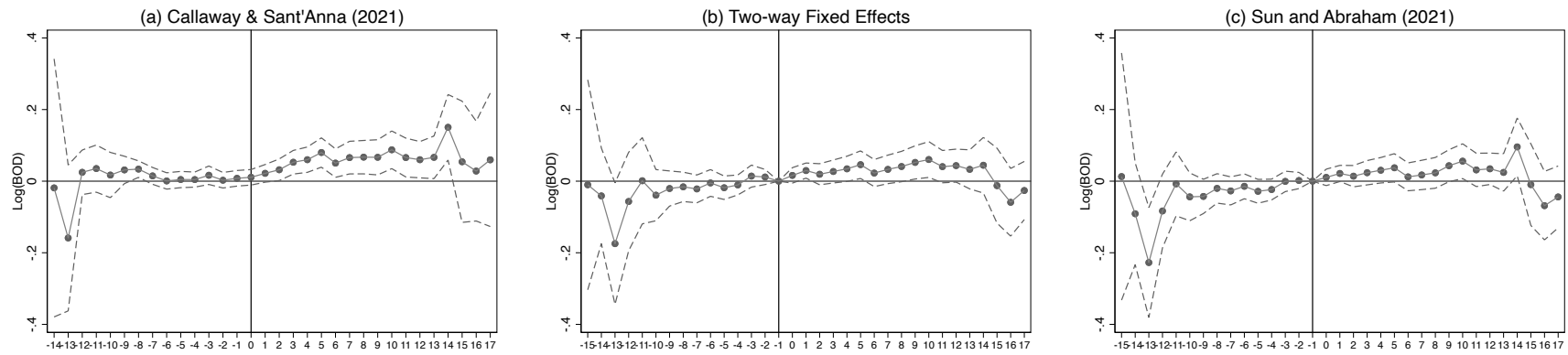


Figure A3: Event Study Results with Alternative Estimators

Notes: These figures compare the event study results among 3 alternative estimators. Panels (a), (b), (c) show the coefficients of the estimators according to the Callaway and Sant'Anna (2021) methodology, two-way fixed effects, and the Sun and Abraham (2021) methodology, respectively. The 95% confidence intervals are shown with dashed lines. Standard errors are clustered at the municipality level. Panel (a) includes monitoring station fixed effects, year fixed effects, and product shipment values as a control, while panels (b) and (c) includes monitoring station fixed effects, basin-year fixed effects, and product shipment values as a control.

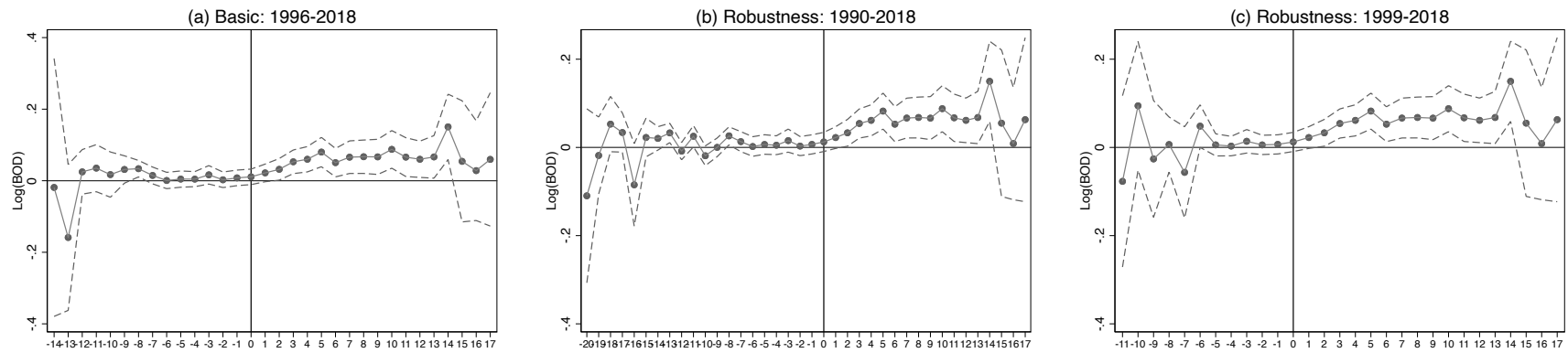


Figure A4: Event Study Results with Alternative Sample Periods

Notes: These figures show the coefficients of the estimators according to the Callaway and Sant'Anna (2021) methodology. Panel (a) is the basic specification when the sample years are 1996–2018, while panels (b) and (c) are shown for robustness checks when the sample years are 1990–2018 and 1999–2018, respectively. The 95% confidence intervals are shown with dashed lines. Standard errors are clustered at the municipality level. All specifications include monitoring station fixed effects, year fixed effects, and product shipment values as a control.

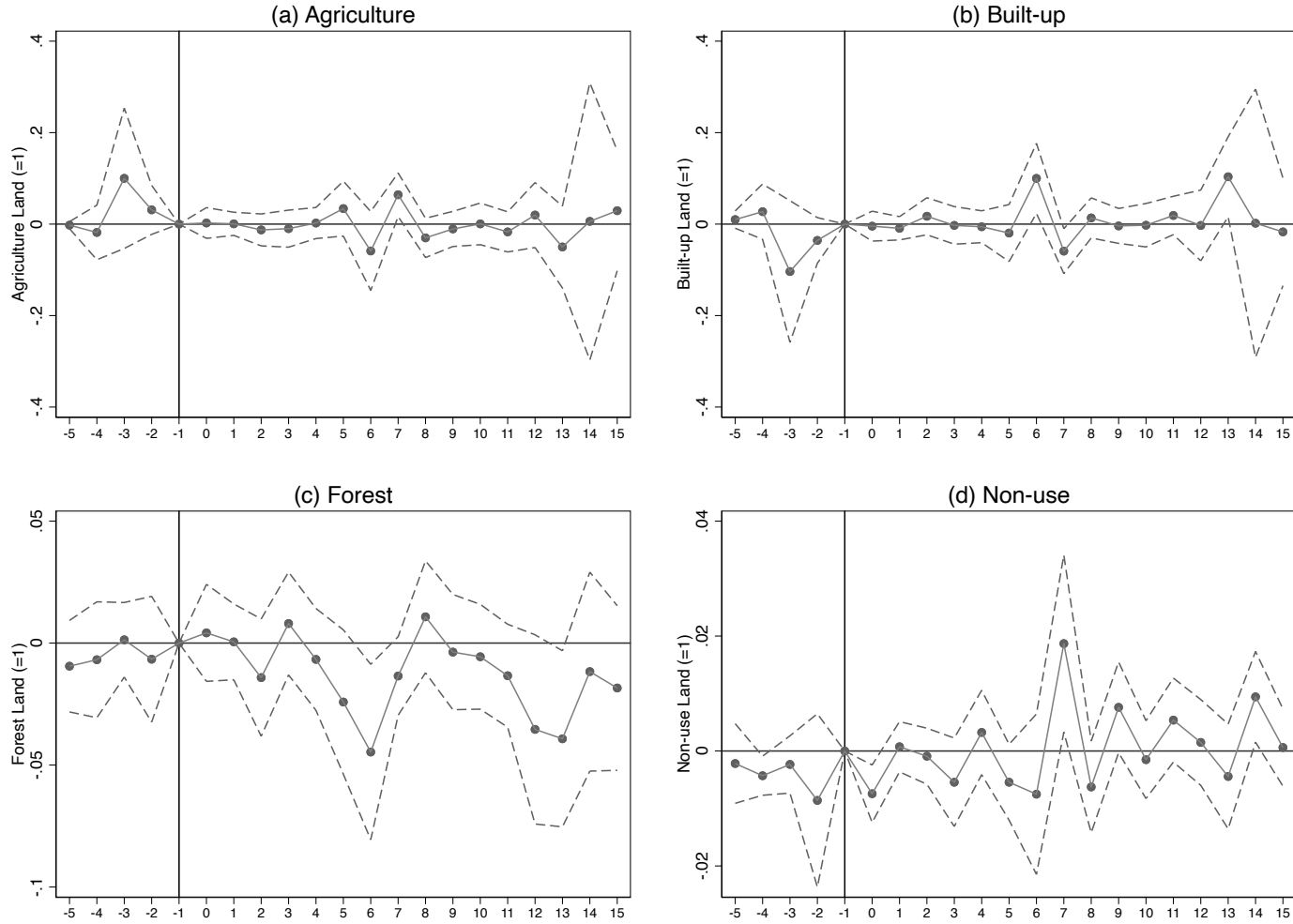


Figure A5: Event Study Results: Change in Land Use

Notes: These figures show the coefficients of the estimators according to the Sun and Abraham (2021) methodology. Four panels show results when the dependent variables are the indicator which takes one if the majority of land use within 150 meters from monitoring stations is the corresponding type. The 95% confidence intervals are shown with dashed lines. Standard errors are clustered at the municipality level. All specifications include monitoring station fixed effects, basin-year fixed effects, and product shipment values as a control. The coefficients of event time from -4 to -12 are not shown because most of them are dropped in the regressions.

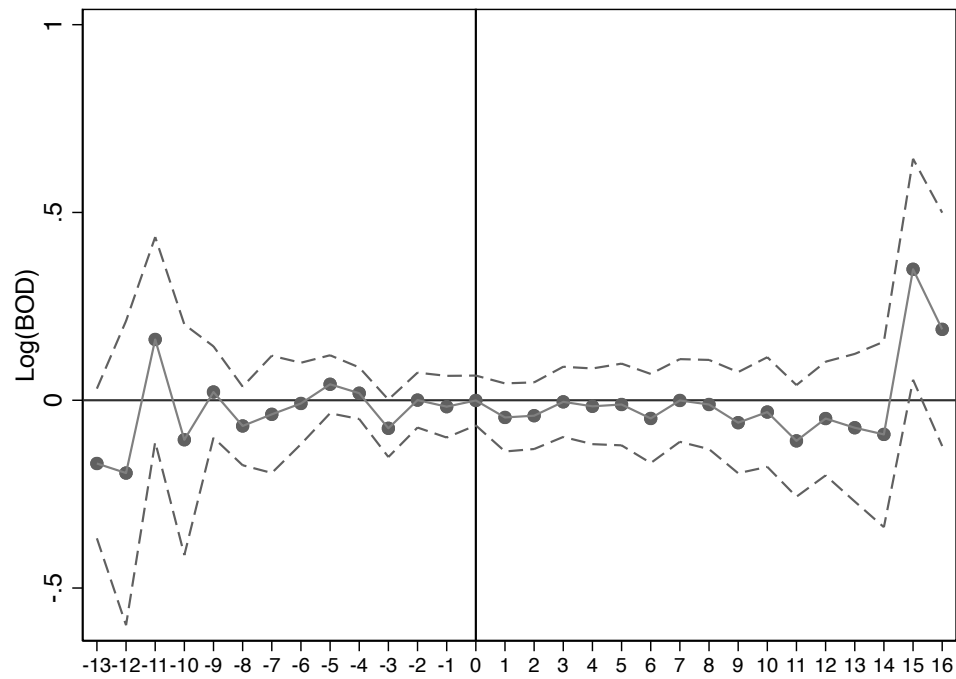


Figure A6: Event Study Results: Wastewater Treatment in Sewage Treatment Plants

Notes: This figure shows the coefficients of the estimators according to the Callaway and Sant'Anna (2021) methodology. The 95% confidence intervals are shown with dashed lines. This specification includes city fixed effects, year fixed effects, and product shipment values as a control.

4.1.2 Additional Tables

Table B1: DID Results - The Impact on Water Quality (Early Mergers versus Late Mergers)

	Log(BOD)		
	(1)	(2)	(3)
Panel A: Callaway & Sant'Anna (2021) Estimator			
Merger (= 1)	0.011 (0.031)	0.011 (0.033)	-0.005 (0.030)
Panel B: Two-way Fixed Effects			
Merger (= 1)	0.010 (0.013)	0.010 (0.016)	0.009 (0.012)
Observations	44,666	44,666	38,840
R ²	0.878	0.878	0.885
Number of Stations	1,942	1,942	1,942
Number of Municipalities	442	182	442
Clustered SE at...	Municipality	Basin	Municipality
Station FE	YES	YES	YES
Product Shipment Values	YES	YES	YES
Population	NO	NO	YES
Mean of Dep. Variable	2.260	2.260	2.260

Notes: Sample is limited to monitoring stations in municipalities which have ever merged. The coefficients are reported. Standard errors are in parentheses and clustered at municipality-level in columns 1 and 3 and basin-level in column 2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All specification in panel A include year fixed effects, while all regressions in panel B include basin-year fixed effects. Mean dependent variables are the mean of BOD values before municipal mergers started in our sample (before 2001) in each specification.

Table B2: DID Results: Change in Land Use

	Agriculture		Built-up		Forest		Non-use	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dummy	Share	Dummy	Share	Dummy	Share	Dummy	Share
Merger (= 1)	-0.010 (0.012)	-0.002 (0.006)	0.006 (0.013)	-0.003 (0.006)	-0.005 (0.006)	0.001 (0.003)	0.003 (0.003)	0.001 (0.002)
Observations	13,471	13,471	13,471	13,471	13,471	13,471	13,471	13,471
R ²	0.867	0.953	0.884	0.955	0.919	0.975	0.697	0.768
Number of Stations	2,774	2,774	2,774	2,774	2,774	2,774	2,774	2,774
Number of Municipalities	871	871	871	871	871	871	871	871
Basin-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Station FE	YES	YES	YES	YES	YES	YES	YES	YES
Product Shipment Values	YES	YES	YES	YES	YES	YES	YES	YES
Population	NO	NO	NO	NO	NO	NO	NO	NO
Mean of Dep. Variable	0.383	0.377	0.424	0.411	0.177	0.170	0.006	0.015

Notes: The coefficients are reported. Standard errors, clustered at municipality-level, are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Columns 1, 3, 5, 7 report results when the dependent variables are the indicator which takes one if the majority of land use within 150 meters from monitoring stations is the corresponding type. Columns 2, 4, 6, 8 reports results when the dependent variables are the shares of the area of corresponding type within 150 meters from monitoring stations. All regressions are based on two-way fixed effects, which include both basin-year fixed effects and monitoring station fixed effects. Mean dependent variables are the mean of dependent variables before municipal mergers started in our sample (before 2001) in each specification.

Table B3: DID Results: Wastewater Treatment in Sewage Treatment Plants

	Effluent Water Quality	Influent Water Quality (Placebo)
	(1)	(2)
	ln(BOD)	ln(BOD)
Panel A: Callaway & Sant'Anna (2021) Estimator		
Merger (= 1)	-0.032 (0.046)	0.035 (0.032)
Panel B: Two-way Fixed Effects		
Merger (= 1)	-0.065 (0.042)	0.014 (0.026)
Observations	5,691	5,691
R ²	0.579	0.553
Number of Municipalities	271	271
Municipality FE	YES	YES
Year FE	YES	YES
Station FE	YES	YES
Product Shipment Values	YES	YES
Population	NO	NO
Mean of Dep. Variable	6.782	195.258

Notes: The coefficients are reported. Standard errors, clustered at municipality-level in panel A, are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Mean dependent variables are the mean of BOD values before municipal mergers started in our sample (before 2001) in each specification.

4.1.3 Data Appendix

4.1.3.1 Water Quality

- We obtain the ambient water quality data from 1990 to 2018 from the following data source.
 - Water environment information website (*Mizu kankyo sogo joho site*), Ministry of Environment
 - <https://water-pub.env.go.jp/water-pub/mizu-site/mizu/download/download.asp> (accessed January 24, 2021)
- We use the yearly average BOD values and GPS coordinates of each water quality monitoring station in this dataset.
- We complement this dataset with the geospatial data of water basins from the following data source.
 - Water basin boundary Ver.1.1 (*Ryuikikai/hishusuiiki dai 1.1 ban*), Ministry of Land, Infrastructure, Transport and Tourism
 - https://nlftp.mlit.go.jp/ksj/gmlold/datalist/gmlold_KsjTmplt-W12.html (accessed February 6, 2021)

4.1.3.2 Municipal Merger

- We obtain the municipal merger data from the following data source.
 - Collection of municipal merger documents (*Shichoson gappei shiryoshu*), Ministry of Internal Affairs and Communications Environment
 - <https://www.soumu.go.jp/gapei/gapei.html> (accessed July 16, 2021)
- We use the following information of all cases of municipal mergers since 1999.
 - Names of newly created municipalities
 - Dates of municipal mergers
 - Names of involved municipalities
 - Types of municipal mergers (“equal-footing” mergers versus “incorporating” mergers)

4.1.3.3 Municipality Boundary for Data Matching

- We obtain the municipal boundary data from the following data source.
 - Administrative boundary data (*Gyosei kuiki deta*), Ministry of Land, Infrastructure, Transport and Tourism
 - <https://nlftp.mlit.go.jp/ksj/jpgis/datalist/KsjTmplt-N03.html> (accessed January 16, 2021)
- We use the municipal boundary to identify the latest prefecture and city names of all monitoring stations. Then, we match water quality data with municipal merger data based on the latest prefecture and city names.

4.1.3.4 River Distance

Data Source for River Distance:

- We obtain river polyline data from the Global Map dataset developed by Geospatial Information Authority(GSI), Ministry of Land, Infrastructure, Transport and Tourism.
 - https://www.gsi.go.jp/kankyochiri/gm_japan_e.html (accessed June 27, 2021)
- We use detailed river node (point layer) data, which include elevation information, provided by the Ministry of Land, Infrastructure, Transport and Tourism.
 - <https://nlftp.mlit.go.jp/ksj/index.html> (accessed June 27, 2021)
- We obtain both pre-merger and post-merger municipality boundaries. As such, we use 1984 and 2018 municipality boundary information from the Municipality Map Maker dataset developed by Kirimura et al. (2011).
 - <http://www.tkirimura.com/mmm/> (accessed June 27, 2021)

Construction of River Distance:

- We construct two distance variables, U and D, based on the following steps.

1. We perform inverse distance weighting (IDW) interpolation on the river node point layer to create a 300-meter raster elevation layer, which is at a finer resolution than readily available DEMs (Digital Elevation Models), and is highly accurate along river paths.
2. We use the river polyline layer and the municipality boundaries layer and intersect them by point to obtain municipality entry/exit points of rivers.
3. We attach elevation information to both the “entry/exit point” layer and the monitoring station layer using the raster feature to point tool.
4. To reduce margins for error, we only focus on monitoring stations that are located within 2km of the river polyline, by conducting spatial queries.
5. We build a network based on the river polyline layer, and perform a “find closest facility” analysis to pick six closest border boundary points along the river for each monitoring station, and obtain distances for these routes.
6. We use the elevation information to determine whether each selected border boundary points are upstream or downstream of the monitoring station, after which we find the closest upstream and downstream border boundaries for each monitoring station, and finally obtain values for U and D.

4.1.3.5 Other Municipality Characteristics

- Product shipment values (*Seizouhin shukka gaku tou*)
 - We obtain the data of product shipment values from two data sources, which covers different periods.
 - * 1990-2012: Municipality-level population and economic data (*Shichoson betsu jinko/keizai kankei deta*), Cabinet Office
 - https://www5.cao.go.jp/keizai-shimon/kaigi/special/future/keizai-jinkou_data.html (accessed February 13, 2020)
 - * 2013-2018: Census of manufacture (*Kogyo tokei chosa*), Ministry of Economy, Trade and Industry,

- <https://www.meti.go.jp/statistics/tyo/kougyo/result-2.html> (accessed June 3, 2021)

- Population

- We obtain the data of population from two data sources, which covers different periods.

- * 1990, 1995, 2000, 2005, 2010: Municipality-level population and economic data (*Shichoson betsu jinko/keizai kankei deta*), Cabinet Office

- https://www5.cao.go.jp/keizai-shimon/kaigi/special/future/keizai-jinkou_data.html (accessed February 13, 2020)

- * 2015: Census (*Kokusei Chosa*), Ministry of Internal Affairs and Communications Environment,

- <https://www.e-stat.go.jp/stat-search?page=1&toukei=00200521> (accessed June 3, 2021)

- Because population data are available every 5 years, we conduct linear interpolation to estimate population for other years.

4.1.3.6 Land Use

- We obtain 100-meter mesh raster data of land use in 1991, 1997, 2006, 2009, 2014, and 2016 from the following data source.

- Land use mesh data (*Tochi riyo saibun mesh deta*), Ministry of Land, Infrastructure, Transport and Tourism

- <https://nlftp.mlit.go.jp/ksj/gml/datalist/KsjTmplt-L03-b.html> (accessed December 6, 2020)

- We compute the numbers of pixels corresponding to the following 4 land use classifications within the 150 meter buffer of each monitoring station¹. Then, we compute the share of pixels for each classification and the binary land use indicator of whether each classification has the largest number of pixels.

¹ Although the original data have finer land use classifications, we regrouped them into 4 classifications for the analysis. We also dropped the pixels corresponding to sea and river water.

- Agriculture
- Forest
- Built-up
- Non-use

4.1.3.7 Sewage Treatment Plants

- We obtain sewage treatment plants data from 1996 to 2017 from the following data source.
 - Sewage statistics (*Gesui tokei*), Japan Sewage Works Association
 - We obtain this dataset from the Japan Sewage Works Association in July 8, 2021.
- We use data of BOD values of both effluent (wastewater flowing out of a sewage treatment plant) and influent (wastewater entering a sewage treatment plant) in each sewage treatment plant. We then aggregate these data into city-level average BOD values of effluent and influent.

4.2 Chapter 2 Appendix

4.2.1 Additional Figures



Figure C1: Location of the Williston Basin at the border of United States and Canada
Source: Kuhn et al. (2012).

4.2.2 Additional Tables

Table D1: OLS Models with Levels

VARIABLES	(1) Prod - H	(2) Prod - V	(3) Prod - H	(4) Prod - V	(5) Prod - H	(6) Prod - V
Production (t-1)	0.7064*** (0.0034)	0.8187*** (0.0392)	0.7059*** (0.0034)	0.8214*** (0.0395)	0.7059*** (0.0034)	0.8214*** (0.0395)
Production (t-2)	0.1032*** (0.0031)	0.1549*** (0.0380)	0.1014*** (0.0031)	0.1520*** (0.0382)	0.1014*** (0.0031)	0.1520*** (0.0382)
PC Negative	-11.6456*** (0.4484)	-0.8019*** (0.0663)	-412.3378*** (92.1797)	-40.2172*** (4.5215)	-398.2157*** (89.2547)	-37.2172*** (3.1851)
PC Positive	-4.1786*** (0.5895)	-0.3774*** (0.0980)	-99.6812*** (21.8083)	-9.5679*** (1.0775)	-93.7541*** (19.7589)	-8.8732*** (1.2126)
Constant	411.7934*** (3.5563)	8.8606*** (1.4274)	1,257.9308*** (214.1263)	97.7796*** (10.0871)	-1224.1228*** (245.7067)	73.9163*** (8.9236)
Observations	1,608,958	1,065,735	1,608,958	1,065,735	1,608,958	1,065,735
R-squared	0.7297	0.9613	0.7312	0.9616	0.7312	0.9616
Well Dummies	No	No	Yes	Yes	Yes	Yes
Month Dummies	No	No	Yes	Yes	No	No
Macro Controls	No	No	No	No	Yes	Yes

Dependent variable is the monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Table D2: OLS Models with Logs

VARIABLES	(1) Ln(Prod) - H	(2) Ln(Prod) - V	(3) Ln(Prod) - H	(4) Ln(Prod) - V	(5) Ln(Prod) - H
$Ln(Prod_{t-1})$	0.6750*** (0.0017)	0.6730*** (0.0023)	0.6677*** (0.0017)	0.6682*** (0.0023)	0.6677*** (0.0017)
$Ln(Prod_{t-2})$	0.1251*** (0.0015)	0.2717*** (0.0023)	0.1280*** (0.0015)	0.2737*** (0.0023)	0.1280*** (0.0015)
PC Negative	-0.0571*** (0.0019)	-0.0335*** (0.0017)	-0.0747*** (0.1876)	-0.0213*** (0.0050)	-0.0688*** (0.0021)
PC Positive	-0.0428*** (0.0019)	-0.0247*** (0.0017)	-0.0624*** (0.0158)	-0.0169*** (0.00380)	-0.0534*** (0.0142)
Constant	0.8915*** (0.0134)	-0.1004*** (0.0111)	-2.5966*** (0.9347)	-0.9434*** (0.2641)	-16.0180** (6.9332)
Observations	1,608,922	1,065,711	1,608,922	1,065,711	1,608,922
R-squared	0.6098	0.8586	0.6157	0.8599	0.6157
Well Dummies	No	No	Yes	Yes	Yes
Month Dummies	No	No	Yes	Yes	No
Macro Controls	No	No	No	No	Yes

Dependent variable is the natural log of the monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Table D3: OLS Models with IHS

VARIABLES	(1) lhs(Prod) - H	(2) lhs(Prod) - V	(3) lhs(Prod) - H	(4) lhs(Prod) - V	(5) lhs(Prod) - H	(6) lhs(Prod) - V
$IHS(Prod_{t-1})$	0.7145*** (0.0016)	0.7131*** (0.0023)	0.7064*** (0.0016)	0.7086*** (0.0023)	0.7064*** (0.0016)	0.7086*** (0.0023)
$IHS(Prod_{t-2})$	0.1009*** (0.0015)	0.2392*** (0.0023)	0.1033*** (0.0015)	0.2413*** (0.0023)	0.1033*** (0.0015)	0.2413*** (0.0023)
PC Negative	-0.0669*** (0.0016)	-0.0394*** (0.0014)	-0.5648*** (0.1050)	-0.1779*** (0.0303)	-0.0389** (0.0165)	-0.0281 (0.0379)
PC Positive	-0.0246*** (0.0017)	-0.0112*** (0.0013)	-0.2957*** (0.0555)	-0.0777*** (0.0138)	-0.0199*** (0.0029)	-0.0183 (0.0248)
Constant	1.4062*** (0.0074)	0.2353*** (0.0028)	2.1686*** (0.1653)	0.4583*** (0.0297)	-11.1011*** (4.1800)	5.8141 (9.5830)
Observations	1,608,958	1,065,735	1,608,958	1,065,735	1,608,958	1,065,735
R-squared	0.6451	0.8819	0.6512	0.8831	0.6512	0.8831
Well Dummies	No	No	Yes	Yes	Yes	Yes
Month Dummies	No	No	Yes	Yes	No	No
Macro Controls	No	No	No	No	Yes	Yes

Dependent variable is the hyperbolic sine transformed monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Table D4: FE Models with levels

VARIABLES	(1) Prod - H	(2) Prod - V	(3) Prod - H	(4) Prod - V
Production (t-1)	0.6451*** (0.0034)	0.8149*** (0.0318)	0.6231*** (0.0031)	0.8015*** (0.0311)
Production (t-2)	0.0588*** (0.0031)	0.1462*** (0.0305)	0.0554*** (0.0029)	0.1423*** (0.0298)
PC Negative	-1,520.2575*** (117.5832)	-53.2279*** (6.0230)	-1,219.7824*** (112.7468)	-45.6814*** (5.8457)
PC Positive	-361.6102*** (27.8448)	-12.6808*** (1.3715)	-357.7482*** (23.9052)	-11.1068*** (1.1578)
Constant	3,547.3866*** (273.1847)	124.9889*** (14.1257)	2,621.7706*** (487.7878)	360.8531*** (129.9377)
Observations	1,608,958	1,065,735	1,608,958	1,065,735
R-squared	0.6318	0.9383	0.6318	0.9383
Number of wells	17,598	4,685	17,598	4,685
Well FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	No	No
Macro Controls	No	No	Yes	Yes

Dependent variable is the monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Table D5: FE Models with Logs

VARIABLES	(1) Prod - H	(2) Prod - V	(3) Prod - H	(4) Prod - V
$Ln(Prod_{t-1})$	0.6065*** (0.0022)	0.6291*** (0.0055)	0.6023*** (0.0020)	0.6262*** (0.0051)
$Ln(Prod_{t-2})$	0.0803*** (0.0019)	0.2358*** (0.0052)	0.0803*** (0.0019)	0.2358*** (0.0052)
PC Negative	-0.3619*** (0.0297)	-0.0895*** (0.0051)	-0.3357*** (0.0282)	-0.0567*** (0.0046)
PC Positive	-0.3047*** (0.0251)	-0.0744*** (0.0039)	-0.2801*** (0.0245)	-0.0641*** (0.0033)
Constant	-16.6335*** (1.4821)	-4.5214*** (0.2734)	-10.6930*** (0.7006)	-1.6587*** (0.0854)
Observations	1,608,922	1,065,711	1,608,922	1,065,711
R-squared	0.5180	0.7522	0.5180	0.7522
Number of wells	17,598	4,685	17,598	4,685
Well FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	No	No
Macro Controls	No	No	Yes	Yes

Dependent variable is the natural log of monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and

*** p<0.01, ** p<0.05, * p<0.1.

Table D6: FE Models with IHS

VARIABLES	(1) Prod - H	(2) Prod - V	(3) Prod - H	(4) Prod - V
$IHS(Prod_{t-1})$	0.6321*** (0.0020)	0.6697*** (0.0057)	0.6321*** (0.0020)	0.6697*** (0.0057)
$IHS(Prod_{t-2})$	0.0453*** (0.0017)	0.2037*** (0.0054)	0.0453*** (0.0017)	0.2037*** (0.0054)
PC Negative	-0.3172*** (0.0185)	-0.6490*** (0.0312)	-0.30045*** (0.0161)	-0.6186*** (0.0576)
PC Positive	-0.1679*** (0.0098)	-0.3300*** (0.0146)	-0.1422*** (0.0132)	-0.2991*** (0.0989)
Constant	6.9423*** (0.2939)	1.3656*** (0.0340)	10.3786** (4.1864)	1.0106*** (0.0378)
Observations	1,608,958	1,065,735	1,608,958	1,065,735
R-squared	0.5471	0.7833	0.5471	0.7833
Number of wells	17,598	4,685	17,598	4,685
Well FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	No	No
Macro Controls	No	No	Yes	Yes

Dependent variable is the hyperbolic sine transformed monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Table D7: FD Models with Logs

VARIABLES	(1) Prod - H	(2) Prod - V	(3) Prod - H	(4) Prod - V
$Ln(Prod_{t-1})$	0.4587*** (0.0094)	0.1484*** (0.0229)	0.4560*** (0.0094)	0.1483*** (0.0229)
PC Negative	-0.0048** (0.0021)	-0.0019 (0.0018)	-0.0071*** (0.0021)	-0.0005 (0.0018)
PC Positive	0.0034 (0.0021)	-0.0013 (0.0018)	0.0118*** (0.0022)	-0.0000 (0.0018)
Ln(MSCI)			-1.2485*** (0.0828)	-0.2413*** (0.0561)
Ln(US 10 Year)			0.9809*** (0.0274)	0.7970*** (0.0401)
Ln(USD Index)			-5.4679*** (0.1921)	-1.5186*** (0.1408)
Ln(Copper)			0.3085*** (0.0572)	0.1322*** (0.0351)
Constant	-0.0066*** (0.0005)	-0.0275*** (0.0008)	0.0108*** (0.0009)	-0.0232*** (0.0009)
Observations	1,591,127	1,060,152	1,591,127	1,060,152
Number of wells	17,553	4,679	17,553	4,679
$Prod_{t-2}$ IV	Yes	Yes	Yes	Yes
$Prod_{t-3}$ IV	No	No	No	No
Macro Controls	No	No	Yes	Yes

Dependent variable is the natural log of monthly oil production in barrels.
Robust standard errors clustered at the well and month level in parentheses
and *** p<0.01, ** p<0.05, * p<0.1.

Table D8: FD Models with Logs

VARIABLES	(1) Prod - H	(2) Prod - V	(3) Prod - H	(4) Prod - V
$Ln(Prod_{t-1})$	0.4641*** (0.0064)	0.2034*** (0.0154)	0.4637*** (0.0064)	0.2034*** (0.0154)
PC Negative	-0.0048** (0.0021)	-0.0026 (0.0019)	-0.0070*** (0.0021)	-0.0003 (0.0019)
PC Positive	0.0033 (0.0021)	-0.0018 (0.0019)	0.0117*** (0.0022)	-0.0007 (0.0019)
Ln(MSCI)			-1.2528*** (0.0832)	-0.2397*** (0.0578)
Ln(US 10 Year)			0.9759*** (0.0274)	0.7804*** (0.0399)
Ln(USD Index)			-5.4209*** (0.1916)	-1.4075*** (0.1392)
Ln(Copper)			0.3309*** (0.0575)	0.1449*** (0.0363)
Constant	-0.0083*** (0.0004)	-0.0257*** (0.0006)	0.0089*** (0.0009)	-0.0215*** (0.0007)
Observations	1,573,407	1,054,690	1,573,407	1,054,690
Number of api_wellno	17,498	4,663	17,498	4,663
$Prod_{t-2}$ IV	Yes	Yes	Yes	Yes
$Prod_{t-3}$ IV	Yes	Yes	Yes	Yes
Macro Controls	No	No	Yes	Yes

Dependent variable is the natural log of monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and ***
p<0.01, ** p<0.05, * p<0.1.

Table D9: FD Models with IHS

VARIABLES	(1) Prod - H	(2) Prod - V	(3) Prod - H	(4) Prod - V
$IHS(Prod_{t-1})$	0.6329*** (0.0104)	0.2491*** (0.0284)	0.6299*** (0.0122)	0.2489*** (0.0354)
PC Negative	-0.0241*** (0.0019)	-0.0069*** (0.0016)	-0.0109*** (0.0020)	-0.0036 (0.0025)
PC Positive	0.0134*** (0.0020)	-0.0009 (0.0016)	0.0167*** (0.0022)	-0.0015 (0.0016)
Ln(MSCI)			-0.5595*** (0.0509)	-0.1166*** (0.0281)
Ln(US 10 Year)			0.5761*** (0.0165)	0.3734*** (0.0197)
Ln(USD Index)			-2.9553*** (0.1168)	-0.6496*** (0.0709)
Ln(Copper)			0.0958*** (0.0353)	0.0810*** (0.0174)
Constant	-0.0060*** (0.0003)	-0.0133*** (0.0005)	0.0032*** (0.0005)	-0.0114*** (0.0005)
Observations	1,591,175	1,060,184	1,591,175	1,060,184
Number of wells	17,553	4,679	17,553	4,679
$Prod_{t-2}$ IV	Yes	Yes	Yes	Yes
$Prod_{t-3}$ IV	No	No	No	No
Macro Controls	No	No	Yes	Yes

Dependent variable is the hyperbolic sine transformed monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Table D10: FD Models with IHS

VARIABLES	(1) Model 3	(2) Model 4	(3) Model 3	(4) Model 4
$IHS(Prod_{t-1})$	0.5768*** (0.0062)	0.3102*** (0.0171)	0.5658*** (0.0060)	0.3001*** (0.0165)
PC Negative	-0.0230*** (0.0018)	-0.0079*** (0.0016)	-0.0096*** (0.0012)	-0.0048 (0.0461)
PC Positive	0.0119*** (0.0019)	-0.0011 (0.0016)	0.0155*** (0.0023)	-0.0017 (0.0019)
Ln(MSCI)			-0.5949*** (0.0496)	-0.1191*** (0.0290)
Ln(US 10 Year)			0.5799*** (0.0161)	0.3618*** (0.0193)
Ln(USD Index)			-3.0297*** (0.1135)	-0.5741*** (0.0692)
Ln(Copper)			0.1297*** (0.0343)	0.0914*** (0.0180)
Constant	-0.0073*** (0.0003)	-0.0122*** (0.0004)	0.0020*** (0.0005)	-0.0103*** (0.0004)
Observations	1,573,466	1,054,730	1,573,466	1,054,730
Number of wells	17,498	4,663	17,498	4,663
$Prod_{t-2}$ IV	Yes	Yes	Yes	Yes
$Prod_{t-3}$ IV	Yes	Yes	Yes	Yes
Macro Controls	No	No	Yes	Yes

Dependent variable is the hyperbolic sine transformed monthly oil production in barrels.

Robust standard errors clustered at the well and month level in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

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