Does Environmental Regulation Drive away Inbound Foreign Direct Investment? Evidence from a Quasi-Natural Experiment in China

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Abstract

This paper investigates whether environmental regulation affects inbound foreign direct investment. The identification uses the Two Control Zones (TCZ) policy implemented by the Chinese government in 1998, in which tougher environmental regulations were imposed in TCZ cities but not others. Our difference-in-difference-in-differences estimation explores three-dimension variations; specifically, city (i.e., TCZ versus non-TCZ cities), industry (i.e.,more polluting industries relative to less polluting ones), and year (i.e., before and after the TCZ policy). We find that tougher environmental regulation leads to less foreign direct investment. Meanwhile, we find that foreign multinationals from countries with better environmental protections than China are insensitive to the toughening environmental regulation, while those from countries with worse environmental protections than China show strong negative responses.

Keywords: Environmental regulation; Foreign direct investment; Pollution haven effect; Difference-in-difference-in-differences estimation; Two control zones
JEL Codes: R11; L25; D22

1 Introduction

Governments across the world, concerned about further deterioration of the environment, are toughening their regulations on pollution in the hope that firms will develop greener technologies and produce more environmentally responsible goods. An unintended consequence, however, is that firms may respond by relocating their production to places with less stringent environmental regulations, a phenomenon known as the pollution haven hypothesis. This may not only counteract the effects of environmental policies, but may also worsen the overall scenario. For example, developing countries may manipulate their environmental policies to attract more foreign direct investment (FDI), which could lead to an increase in overall pollution levels.

Despite much anecdotal evidence, however, empirical studies fail to provide conclusive results on the effects of environmental regulation, with some finding no such effects[1] and others documenting significant effects.[2] As a result, the investigation of the pollution haven hypothesis is considered to be "one of the most contentious issues in the debate regarding international trade, foreign investment, and the environment" (Kellenberg, 2009).

An inherent empirical challenge in identifying an effect of environmental regulation on firms' location choice is how to deal with the potential endogeneity of environmental regulation.[3] Recent studies have started to tackle the potential endogeneity of environmental regulations, for example by using either the instrumental variable approach (see Millimet and Roy, 2015, for a survey) or the difference-in-differences (DD) method (List et al., 2003; List, McHone, and Millimet, 2004; Millimet and List, 2004; Hanna, 2011; Chung, 2014; Broner, Bustos, and Carvalho, 2015).

This paper contributes to the literature on the pollution haven hypothesis on three grounds. First, while recent studies heavily use data from developed countries such as the U.S. (e.g., Hanna, 2011; Millimet and Roy, 2015) and South Korea (Chung, 2014), we examine whether environmental regulation affects inbound FDI in China, the largest developing country in the world. Detecting whether the pollution haven hypothesis exists in developing countries helps in understanding whether the laxity of environmental regulations could be used as a source of comparative advantage to attract FDI and ultimately induce economic growth in developing countries. These findings can also shed

[1] For example, Friedman, Gerlowski, and Silberman (1992); Levinson (1996); Eskeland and Harrison (2003); Javorcik and Wei (2004). In a related study, List (1999) shows that air pollution emissions in the U.S. converged during 1929-1994, suggesting that states in the U.S. did not compete for industries by loosening their environmental regulations.

[2] For example, Henderson (1996); Becker and Henderson (2000); List and Co (2000); Keller and Levinson (2002); List et al. (2003); Kellenberg (2009). For literature reviews, see, Dean (1992), Levinson (2008), Brunnermeier and Levinson (2004), Copeland and Taylor (2004), and Erdogan (2014).

[3] Jeppesen, List, and Folmer (2002) conduct a meta-analysis and conclude that differences in methodological considerations explain much of the variation in the findings on the effects of environmental regulation.

light on concerns about whether tough environmental protection in developed countries can achieve the goals of environmental protection in the presence of possible relocation of dirty production to developing countries. In addition, China provides a good setting for investigating the pollution haven hypothesis. On the one hand, Chinese governments have been attracting FDI aggressively since 1978 when China adopted its opening up and reforming policy, and this has made China the second largest FDI (stock) recipient country in the world.[4] On the other hand, China's fast economic growth in recent decades has been accompanied by severe environmental degeneration, including over-exploitation and mass industrial pollution, which are typical problems in developing countries. China is also a large country with substantial differences in the distribution of FDI and environmental quality, which provides us with enough variation to identify the effects of environmental regulation.

Second, our analyses use the most comprehensive firm data in China; specifically, two censuses data sets covering all establishments in 1996 and 2001, and survey data on foreign invested enterprises (FIEs) covering more than three-fourths of total FIEs in 2001. These data allow us to uncover the whole pattern of FDI flows in China, and provide an advantage over previous studies using small or truncated samples of firms in China (for example, a sample of 2,886 manufacturing equity joint ventures used by Dean, Lovely, and Wang, 2009; and a sample of firms with annual sales above 5 million Chinese currency used by Hering and Poncet, 2014). The FIE survey data contain information on the FIEs' sourcing countries, which allows us to investigate whether the deterrent effect of environmental regulation varies across countries with different degrees of environmental protection. Understanding such differential effects can further shed light on concerns about whether strengthening environmental regulations would force firms to relocate production to developing countries with lax environmental regulations.

Third, one concern in the literature regarding the identification of the pollution haven hypothesis is that environmental regulations could be measured with errors, and this endogeneity problem may contaminate the estimates. Our study circumvents this measurement problem by using a change in environmental policy in China, specifically, the implementation of the Two Control Zones (TCZ) policy. The TCZ policy was initiated and the designation for each city regarding the policy status was conducted by the central government with little influence from local governments. To enforce the policy, the National Environmental Protection Bureau (NEPB) was established, and the targets for environmental controls were clearly posited by the State Council (China's cabinet) for the short run and the long run. This context alleviates the concern that government policies are often poorly carried out in developing countries, which leads to the weak findings. For details about environmental regulations in China, see section 2.

To identify the effects of environmental regulation, we conduct a difference-in-difference-

[4] Based on statistics from the CIA World Factbook (accessed on August 15, 2013).

in-differences (DDD) estimation. Specifically, the first difference comes from the comparison of FDI flows in TCZ and non-TCZ cities (with the former adopting tougher environmental regulations); the second difference compares the FDI flows in more polluting and less polluting industries (with the former having stronger deterrent effects); and the last difference is due to the policy implementation in 1998, which divides the sample into pre- and post-treatment periods. The triple difference allows us to control for full sets of country-industry fixed effects, country-year fixed effects, and industry-year fixed effects, in which all potential omitted variables varying at the city level (time varying and time invariant) and at the industry level (time varying and time invariant) have been properly dealt with. For further validity checks, we check the potential bias from the Asian financial crisis in 1997-1998, investigate the expectation and lagged effects, conduct a placebo test with random assignment of TCZ reform, and use an instrumental variable strategy.

We present two sets of results. First, we find stronger deterrent effects of environmental regulation on FDI flows in more polluting industries relative to less polluting ones, confirming the pollution haven hypothesis. A one-standard-deviation increase in pollution intensity causes the negative effect of environmental regulation on FDI flows to be 8 percentage points lower.

Second, we detect significant heterogeneous effects across FDI sourcing countries. Specifically, we examine whether foreign multinationals from countries with more stringent environmental regulations than China behave differently from those from countries with less stringent regulations than China, as the former goes to a country with weaker regulations than those in their home countries while the latter has the opposite. To this end, we divide countries into two groups, based on whether they joined the international treaties (i.e., the *United Nations Framework Convention on Climate Change* and the *Kyoto Protocol*) on environmental protection before or after China. We find that foreign multinationals from countries with better environmental protections than China are insensitive to the toughening environmental regulation, while those from countries with worse environmental protections than China show strong negative responses. These findings may help relieve the concern that toughening environmental protection in developed countries would cause a shift of dirty manufacturing production to countries with laxer environmental regulations.

This study is related to the recent renaissance in the study of the pollution haven hypothesis. Hanna (2011) uses a DD analysis to investigate how the Clean Air Act Amendments (CAAA) in the U.S. have affected its outflow FDI, and finds that the CAAA increased regulated multinationals' foreign assets by 5.3 percent and foreign output by 9 percent. Chung (2014) also conducts a DD analysis to study how the change in environmental laxity in foreign countries affects foreign investment by Korean multinationals, and finds that Korean multinationals in more polluting industries invest more in countries with less stringent environmental regulations. Applying two novel identification strategies to inbound U.S. manufacturing FDI across 48 contiguous states over 1977-1994, Millimet and Roy (2015) find significant negative effects of environmental stringency on inbound FDI in pollution-intensive industries. Using the meteorological determinants of pollution dispersion as an instrument for environmental regulation at the country level, Broner, Bustos, and Carvalho (2015) find that lax environmental regulations constitute a source of comparative advantage for international trade and the magnitude is comparable to the role of physical and human capital.

The remainder of this paper is organized as follows. The institutional background of environmental regulations in China is described in section 2. Section 3 discusses the data, variables, and estimation strategy. Empirical findings are presented in section 4. The paper concludes with section 5.

2 Environmental Regulations in China

Timeline.—Sulfur dioxide (SO2) emissions generated by coal combustion have substantially increased alongside the fast economic growth in China in recent decades.[5] Concerned about China's long-term sustainable economic development, Chinese governments started to implement a series of regulatory policies since the mid-1980s. Specifically, the Air Pollution Prevention and Control Law of the People's Republic of China (APPCL) was enacted in 1987 and came into force in 1988. In 1995, the APPCL was amended, and a section about the regulation of air pollution and SO2 emissions was included. More important, in January 1998, the State Council approved the setup of two control zones (TCZ) in its document "The Official Reply of the State Council Concerning Acid Rain Control Areas and SO2 Pollution Control Areas" (the 1998 Reply hereafter), which was then put into effect.

From a total of 380 prefecture-cities, 175 cities, accounting for 11.4% of the nation's territory, 40.6% of the population, 62.4% of GDP, and 58.9% of total SO2 emissions in 1995, were designated as TCZ cities (Hao et al., 2001). Figure 1 shows the geographic distribution of TCZ cities in China; specifically, dark grey areas represent two control zones, light grey areas are non-TCZ cities, and black circles show the size of each city in 1996 in terms of the number of firms from our census data (to be introduced in the next section). In general, SO2 pollution control zones are located in Northern China because of the reliance there on thermal energy for heating, whereas acid rain control zones are located in southern China where the climate is relatively more humid.

[Insert Figure 1 Here]

[5] For example, in 1993, 62.3% of cities in China had annual average ambient SO2 concentration values above the national Class II standard (i.e., 60 ug/m3).

Criteria.—The two control zones comprise SO2 pollution control zones and acid rain control zones. The NEPB began designating cities as TCZ cities in late 1995, using several criteria. Specifically, a city was designated as an SO2 pollution control zone if: (1) its average annual ambient SO2 concentration had been larger than the national Class II standard in recent years; (2) its daily average ambient SO2 concentrations exceeded the national Class III standard (i.e., 250 ug/m3); or (3) its SO2 emissions were significant. A city was designated as an acid rain control zone if: (1) the average pH value of its precipitation was equal to or less than 4.5; (2) its sulfate deposition was above the critical load; or (3) its SO2 emissions were large.

New Policies.—Once a city was designated as a TCZ city, tougher regulatory policies were implemented. First, new collieries based on coal with a sulfur content of 3% and above were prohibited, and existing collieries using a similar quality of coal had to reduce the production gradually or be shut down. Second, new coal-burning thermal power plants were prohibited in city propers and in suburbs of large or medium cities, except for cogeneration plants whose primary purpose was to supply heat. Furthermore, newly constructed or renovated coal-burning thermal power plants using coal with a sulfur content of 1.5% and above had to install sulfur-scrubbers, while existing power plants using similar quality coal had to adopt SO2 emission-reduction measures by 2000. Third, in polluting industries such as the chemical engineering, metallurgy, nonferrous metals and building materials industries, production technologies and equipment generating severe air pollution had to be phased out. Finally, local governments had to strengthen the collection, administration, and use of SO2 emission fees.

Enforcement.—In the 1998 Reply, the State Council also laid out the targets for environmental controls in the TCZ cities for the short run (by 2000) and for the long run (by 2010).[6] These new environmental regulations have generated a significant improvement in air pollution control. In 2000, 102 TCZ cities achieved the national Class II standard for average ambient SO2 concentrations and 84.3% of severely-polluting firms achieved the target level for SO2 emissions (China Environment Yearbook, 2001). In 2010, 94.9% of TCZ cities had achieved the national Class II standard for average ambient SO2 concentrations, with no city reporting values above the national Class III standard (Report of the Ministry of Environmental Protection of the People's Republic of China, 2011).

[6] Specifically, by the end of 2000, "the sources of industrial SO2 pollution should achieve the national standard of SO2 emission. The total amount of SO2 emission should be within the required amount. Ambient SO2 concentrations in important cities should achieve the national standards. The acid rain in the acid rain control zones should be alleviated." By the end of 2010, "the total amount of SO2 emission should be lower than that in 2000. Ambient SO2 concentrations in all cities should achieve the national standards. The number of acid rain areas with average pH value of precipitation equal to or less than 4.5 should be reduced significantly."

3 Data, Variables, and Estimation Strategy

3.1 Data and Variables

We obtained a detailed list of the names of the cities designated as TCZ cities from the official State Council document, "The Official Reply of the State Council Concerning Acid Rain Control Areas and SO2 Pollution Control Areas." During the sample period, the composition of this list remained unchanged. Appendix Table A1 contains the list.

Table 1 compares a variety of city characteristics between TCZ and non-TCZ cities before the treatment in 1998. We find that for most of these characteristics, the differences between TCZ and non-TCZ cities are small relative to the two group mean values. For example, while TCZ cities attracted more FDI than non-TCZ cities before 1998, the difference is about 15 percent to 18 percent of the two group means. Significant differences lie in TCZ cities being more trade oriented, more likely to be located in Southern China, and more likely to be big cities (such as municipality cities and provincial capital cities). In the next subsection, we will discuss how to control for heterogeneity across treatment and control cities in identifying the effects of environmental regulation.

[Insert Table 1 Here]

To measure FDI activities in China, we use two large-scale firm level data sets, which both have pros and cons. The first data set comes from the Chinese First National Census of Basic Units and the Chinese Second National Census of Basic Units, conduced at the end of 1996 and 2001, respectively. Two business units are available in China, legal unit and basic unit, where the former is made up of the latter and the latter is under the management and control of the former. The definition of a legal unit is consistent in principle with that of the organization unit in the System of National Accounts (SNA) of the United Nations, whereas the definition of a basic unit is in accord with that of an establishment in the SNA. There are, respectively, 6.35 million and 7.08 million basic units in the two censuses, covering all industries including agriculture, manufacturing, construction, service, etc. The census data contain firms' full basic information, such as date of establishment, address, location code, industry affiliation, and ownership, but only report the amount of employment and sales category for each firm. As a result, we focus on the logarithm of employment for newly established FDI as the measurement of FDI flows.

Despite of their intensive coverage, the census data have two potential drawbacks in our research setting: (1) only one year before and one year after the treatment are available, preventing the investigation of common pre-treatment trends; and (2) no information on the origin country of the FDI is available, making analysis of heterogeneous effects across origin countries infeasible. Hence, we complement our analysis by using a second firm level data set, the survey of FIEs in 2001. This is the most comprehensive survey of foreign firms in China, and the data have around 150,000 observations, accounting for more than 75% of total foreign firms in China in 2001. In addition to firms' full basic information and common accounting measures (such as employment, sales, etc.), the data contain information on the contractual investment capital at the establishment and origin countries of the firm.

For our analysis, we need the information of FDI inflow at the city, industry and year level. While the FIE data are only available in 2001, they contain the information of the contractual investment capital at the time of firm establishment. This enables us to construct for each year the total contractual investment capital of new FDI entries in a city and an industry. We use this value as the measure of FDI inflow from the FDI data. With access to only one year survey, however, we need to extrapolate firms' location information at the time of entry so as to conduct a long panel analysis. To this end, we consider firms' current location as their location of establishment, resulting in a panel from 1992 to 2001. The downside of this practice is that if firms changed their location or exited in response to the TCZ policy, then our estimates would be biased because of the investment deflection. As our null hypothesis is that environmental regulations have no effects on firm location choice, relocation or exiting should not affect the test on the null hypothesis or the findings of the pollution haven hypothesis. However, in the case of rejection of the null hypothesis, such relocation and/or exiting behavior would affect the magnitude of coefficients. We will discuss this issue further when we present the magnitude of our estimates.

The industry level SO2 emission information in 2004 is obtained from the *China Environment Statistical Yearbook 2005*, published by the National Bureau of Statistics of China and the NEPB.[7]

3.2 Specification

The time and regional variations in the adoption of the TCZ policy provide an opportunity for a difference-in-differences (DD) analysis. Specifically, there are two groups of cities, the treatment group comprising cities designated as TCZ cities in 1998, and the control group comprising non-TCZ cities. We can then compare FDI in TCZ cities before and after the adoption of the TCZ policy in 1998 with the corresponding change in non-TCZ

[7] One concern of using the SO2 emission level in 2004 is that the value may be affected by the TCZ policy, e.g., the adjustment by polluting industries. However, a problem of using the SO2 emission information before the TCZ policy enacted in 1998 is that the data were only available for around 20 industries compared with 37 industries in 2004, which leads to a substantial reduction in the cross-industry variations. Nonetheless, we find high correlations between the SO2 emission levels in 1996-1998 and that in 2004 for the 15 common industries; specifically, 0.8917 for the correlation between 1996 and 2004, 0.9336 for the correlation between 1997 and 2004, and 0.9340 for the correlation between 1998 and 2004. These results suggest that the industry aggregate SO2 emission levels were quite persistent during our research period.

cities during the same period.

The DD estimation specification is as follows:

$$Y_{ct} = \alpha_c + \gamma \cdot TCZ_c \times Post_t + \delta_t + \varepsilon_{ct}, \quad (1)$$

where Y_{ct} is the measurement of FDI flows in city c at year t; TCZ_c indicates city c's TCZ status in 1998, i.e., $TCZ_c = 1$ if city c is a TCZ city and $= 0$ if city c is a non-TCZ city; $Post_t$ indicates the post-treatment period, i.e., $Post_t = 1 \forall t \geq 1998$ and $= 0$ otherwise; α_c are city fixed effects, capturing city c's all time-invariant characteristics such as geographic features, climate, natural endowment, etc.; δ_t are year fixed effects, capturing all yearly factors common to all cities such as business cycle, monetary policy, macro shocks, etc.; and ε_{ct} is the error term.

However, a concern about the DD analysis is that there could be some time-varying city characteristics that correlate with our regressor of interest and hence bias the estimate. One example is the agglomeration effect, which is found to be an important determinant of industrial location choice (see, e.g., Arauzo-Carod, Liviano-Solis, and Manjon-Antolin, 2010, for a review). One way to capture agglomeration economies in the literature is to include the historical stock of FDI (e.g., Wagner and Timmins, 2009). However, as discussed by Lechner (2010), the problem with the inclusion of the lagged dependent variable in the DD setting is that "if the DiD assumptions hold unconditionally on the pre-treatment outcome, they are likely to be violated conditional on pre-treatment outcomes." Meanwhile, the inclusion of the lagged dependent variable causes a mechanical correlation between the error term and the lagged dependent variable in the panel estimation, consequently generating inconsistent estimates in equation (1) (see Nickell, 1981; Angrist and Pischke, 2009).

Another example of omitted time-varying variables is the attributes of the neighboring locations, which are found to influence the location choice of FDI (see, for example, Blonigen, et al., 2007; Millimet and Roy, 2015). One way to address this issue in the literature is to control for a spatially lagged FDI measure (i.e., the inverse-distance-weighted average of the FDI received by all other cities) in the regression. However, as pointed out by Gibbons and Overman (2012), estimating such a model produces inconsistent parameter estimates because of a mechanical correlation between the error term and the spatially lagged FDI measure.

In light of these concerns, we exploit the fact that industries having different intrinsic polluting intensity are affected differently, and conduct a DDD estimation as our main identification. Specifically, we use the time variation (e.g., before and after the TCZ policy in 1998), regional variable (e.g., TCZ versus non-TCZ cities), and industry variable (e.g., more polluting industries relative to less polluting ones). The DDD estimation specification is as follows

$$Y_{ict} = \gamma \cdot TCZ_c \times Post_t \times SO2_i + \eta_{ct} + \lambda_{ic} + \varphi_{it} + \varepsilon_{ict}, \tag{2}$$

where $SO2_i$ is the degree of SO2 emission by industry i. Given that the information of $SO2_i$ is only available at the 2-digit industry level (a list of industries is contained in Appendix Table A2), we aggregate the variables and conduct the analyses at the city, 2-digit industry and year level. To deal with potential heteroskedasticity and serial correlation, we cluster the standard errors at the city-industry level.

Compared with the DD estimation, the DDD specification allows us to control for a whole set of industry-year fixed effects, industry-city fixed effects, and city-year fixed effects. In other words, we are able to control for all time-varying and time-invariant city characteristics, such as regional spillovers, agglomeration, corruption, local political activism, energy prices, etc. We also control for all time-varying and time-invariant industry characteristics, such as changes in technology, changes in import competition degrees, industry policies, etc. And we allow for industries to be different in different cities, as long as such industry-city differences remain fixed in our sample period. The remaining possible omitted variables need to vary simultaneously across time, cities, and industries. As further checks on the validity of our DDD estimation, we provide a battery of sensitivity analyses, including controlling for the Asian financial crisis in 1997-1998, checking the expectation and lagged effects, a placebo test with the random assignment of TCZ reform, and an instrumental variable regression following Hering and Poncet (2014). For details, see section 4.2.

4 Empirical Findings

4.1 Main Results

The DDD estimation results are presented in Table 2, where data from the censuses are used in column 1 and the survey data on FIEs are used in column 2. We find that the triple interaction term is consistently negative and statistically significant. These results indicate that cities with tougher environmental regulations (i.e., where the TCZ policy has been implemented) attracted less FDI in more polluting industries, confirming the pollution haven hypothesis.

[Insert Table 2 Here]

There are two possible reasons why tough environmental regulations drive away FDI in China. First, the TCZ policies require that outdated, dirty production technologies and equipment are phased out, and the collection of SO2 emission fees is strengthened, which increases production costs particularly for polluting industries in the TCZ cities.

Second, the TCZ policies also prohibit the establishment of new collieries and new coal-burning thermal power plants that use low quality coal, and they require the installation of desulfurization equipment in the existing power plants, which leads to an increase in electricity costs faced by firms in the TCZ cities, because coal is still the main fuel source for power in China.

Economic Magnitude.—The economic magnitude of the effect of environmental regulation is significant. The coefficients in Table 2 capture the differential responses of FDI flows to the tough environmental regulations (caused by the implementation of the TCZ policy) across industries with different SO2 emissions. Specifically, a one-standard-deviation increase in pollution intensity causes the negative effect of environmental regulation on FDI flows to be 8 percentage points lower.

A caveat in interpreting the magnitude of the effect is the possibility of investment deflection. That is, facing the tough environmental regulations in the TCZ cities, FDI may flow into non-TCZ cities where the environmental regulations are less stringent. In other words, it may not be that the treatment group is negatively affected by the treatment, but that the control group is positively affected by the treatment. While the investment deflection is consistent with the evidence on the pollution haven hypothesis so long as it is driven by the changes in environmental regulations, the estimation magnitude needs to be interpreted with caution. Specifically, it constitutes an upper bound of the pollution haven effect. Although there is no affirmative way to detect how seriously the investment deflection is in this setting, we provide some suggestive evidence. Specifically, we collect the FDI inflow data from the World Investment Report in 1996 and 2002, and plot time trends of overall FDI inflows into China and its share in the world's total FDI during the sample period in Figure 2a. We find that FDI inflows in China started to grow in 1992, reached a peak in 1997, and declined a bit during the Asian Financial Crisis. Meanwhile, FDI in China as share of the world's total FDI grew from 1992 to 1994 and then started to fall until it bottomed out in 2000. Figure 2b further shows Chinese FDI inflow into TCZ and non-TCZ cities from 1992 to 2009, where the FDI information is obtained from the *Chinese City Statistics Yearbook* at various years. We find increasing trends in both groups, despite of some declines during the Asian Financial Crisis. Combined, these results indicate that our findings are not entirely explained by investment deflection.

[Insert Figure 2a and 2b Here]

Our estimated magnitude is comparable to those found in the literature.[8] For example, Becker and Henderson (2000) find that tougher environmental regulations caused the birth rate of firms in polluting industries to drop by $26 - 45\%$ in the U.S.. Kel-

[8] These studies are conducted using different methods, data, and time periods; hence, the magnitude comparison should be interpreted with caution.

lenberg (2009) estimates that during 1999-2003, a failing environmental policy caused the value added of U.S. affiliates located in the top 20th percentile of countries to grow by approximately 8.6%, while the corresponding number for the top 20th percentile of developing and transition economies was 32%. Hanna (2011) finds that the CAAA in the U.S. between 1966 and 1999 increased U.S. multinationals' foreign assets by 5.3% and foreign output by 9%. Using the data on foreign investment by Korean multinationals, Chung (2014) finds that when a foreign country increases its environmental laxity relative to Korea by one standard deviation from the mean, there is a 12.4% increase in investment by Korean multinationals from an industry one standard deviation above the mean pollution intensity than an industry at the mean pollution intensity.

4.2 Robustness Checks

In this subsection, we conduct a battery of further robustness checks on our aforementioned results.

The 1997-98 Asian Financial Crisis.—If other events happened at the same time, any findings about the treatment effect cannot be attributed only to the effect of environmental regulation. One important event regarding foreign investment was the Asian financial crisis in 1997-1998. If the Asian financial crisis hit TCZ cities and more polluting industries more strongly, our aforementioned estimates of the effect of environmental regulation could be contaminated. For example, East Asian countries such as Japan and Korea used to invest more in cities in Northern China that hosted heavy and polluting industries before 1998. If, during the Asian financial crisis, Japanese and Korean multinationals reduced their investment in China, we would find similar negative estimated coefficients in Table 2 even without the effects of environmental regulations. To address this concern, we exploit the advantage of the FIE survey data, which contain the information on the FDI home countries, and exclude FDI from Japan and Korea from the analysis. The regression results are reported in Table 3, column 1. We find a similar estimate in this reduced sample, in terms of statistical significance and magnitude, implying that our findings are not driven by the 1997-98 Asian financial crisis.

[Insert Table 3 Here]

Lags, Leads, and Time Trends.—Another potential concern regards the timing of the change in environmental policy. Specifically, as the NEPB began compiling the TCZ list in late 1995 and took two years to get approval from the State Council, one may be concerned about whether there is any expectation effect, that is, whether the effect of environmental regulation on FDI flows happened before the effective date of the policy (i.e., 1998). Meanwhile, there is also a possibility of the lagged effect of environmental regulation on FDI flows. To address these concerns, we follow Laporte and Windmeijer (2005) by estimating all the lags and leads of the environmental regulation effect. Specifically, we estimate the following equation

$$Y_{ict} = \sum_{j=-5}^{3} \boldsymbol{\gamma}_j \cdot TCZ_c \times \delta_{1998+j} \times SO2_i + \eta_{ct} + \lambda_{ic} + \varphi_{it} + \varepsilon_{ict}, \tag{3}$$

where δ_{1998+j} is the indicator variable for year $1998 + j$; and the default (omitted) year category is 1992. Hence, $\boldsymbol{\gamma}_j$ captures five-years lag and three-years lead effects of environmental regulation. As the census data only are a two-year panel, we conduct this exercise using the FIE survey data. The regression results are plotted in Figure 3. We find that in the pre-treatment period (i.e., before 1998), there are ups and downs in the estimates without clear time trends. Right after the TCZ policy was implemented in 1998, there is a clear decrease in the estimates and they remain significantly negative (despite a jump in 2000). These results demonstrate that the effect of environmental regulation on FDI flows is immediate, and the treatment and control groups do not exhibit differential time trends before the treatment.

[Insert Figure 3 Here]

Placebo Test.—Here, we take a closer look at the identification issues. Specifically, denote $X_{ict} \equiv TCZ_c \times Post_t \times SO2_i$ and let $\varepsilon_{ict} = \beta\omega_{ict} + \tilde{\varepsilon}_{ict}$, such that $E[X_{ict}, \omega_{ict}] \neq 0$ and $E[X_{ict}, \tilde{\varepsilon}_{ict}] = 0$. In other words, all the identification issues come from ω_{ict}. Hence, our estimator $\hat{\gamma}$ is

$$plim\ \hat{\gamma} = (X'X)^{-1}(X'Y) = \gamma + \beta (X'X)^{-1}(X'\boldsymbol{\omega}) = \gamma + \beta\delta \tag{4}$$

where $plim\ \delta \equiv (X'X)^{-1}(X'\boldsymbol{\omega})$. And $\hat{\gamma} \neq \gamma$ if $\beta\delta \neq 0$.

As a further check for whether our results are biased due to the omitted variable at the city-industry-year ω_{ict}, we conduct a placebo test by randomly assigning TCZ status to cities (for similar practices, see, e.g., Chetty, Looney, and Kroft, 2009; La Ferrara, Chong, and Duryea, 2012). Specifically, in our regression sample, there are 160 TCZ cities of 287 cities. We first randomly select 160 cities from the total 287 cities and assign them as TCZ cities, with the remaining being non-TCZ cities; then we construct a *false* treatment variable, i.e., $TCZ_c^{false} \times Post_t \times SO2_i$. The randomization ensures that this newly constructed regressor of interest should have no effect on FDI inflow (i.e., $\gamma^{false} = 0$); hence, if no significant omitted variables exist (i.e., $\beta\delta = 0$), we should have $\hat{\gamma}^{false} = 0$. In other words, any significant findings would indicate the misspecification of our estimation equation. We conduct this random data generating process 500 times to avoid contamination by any rare events.

Table 3, columns 2 and 3 reports the mean values of the estimates from the 500 random assignments for the census data and FIE survey data, respectively. We find that the mean values are almost zero (i.e., 0.001 for the census sample and −0.007 for the FIE survey data), suggesting that $\hat{\gamma}^{false} = 0$. We further plot the distribution of 500 estimated coefficients and their associated p-values for the two data sets in Figures 4a and 4b, respectively. The distributions center around zero and most of estimates' p-values are larger than 0.1. Meanwhile, our true estimates (from columns 1 and 2 of Table 2, respectively) are clear outliers in the placebo tests. Combined, these results suggest that our estimates are not severely biased due to any omitted variables.

[Insert Figures 4a an 4b Here]

Instrumental variable estimation.—To further check whether our estimates are biased to omitted variables at the city-industry-year level or not, we adopt an instrumental variable strategy following Hering and Poncet (2014), who use the ventilation coefficient as the instrument for the TCZ status. According to the Box model (e.g., Jacobsen, 2002), two meteorological forces determine the pollution dispersion. The first one is wind speed, in which faster wind speed is helpful for pollutants to disperse horizontally. The second one is mixing height, which causes pollutants to disperse vertically. Specifically, ventilation coefficient is defined as the product of wind speed and mixing height, with the higher values meaning the faster dispersion of pollutants.

We collect the information on wind speed at 10m height and boundary layer height (which is used to measure mixing height for the grid of 75 * 75 cells) from the European Centre for Medium-Term Weather Forecasting (ECMWF) ERA-Interim dataset. We first match the EAR-interim database with our Chinese cities according to their latitudes and longitudes, and then multiply wind speed and boundary layer height at each cell to obtain the ventilation coefficient. The ventilation coefficient we use in the regression is the average coefficient from 1991 to 1996 for the nearest cell of each city.

The second-stage results of the instrumental variable estimations are reported in columns 4 and 5 of Table 3, with the first-stage results being reported in Appendix Table A3.[9] We continue to find a negative and statistically significant effect of environmental regulations on FDI inflow, with the magnitude being even larger. These results indicate that our findings on the pollution haven effect is not driven by the omitted variables or reverse causality.

Domestic production.—While our aforementioned analyses focus on the sample of foreign firms, it is interesting to examine whether the effect of environmental regulation on location choice also exists for Chinese domestic firms. To this end, we use the census data, and re-do the analysis by using the sample of domestic firms. Estimation results are

[9] We also follow Hering and Poncet (2014) in adding three additional controls; that is, $GDPPC_c \times Post_t \times SO2_i$, $Coastal_c \times Post_t \times SO2_i$, and $Special\ Zone_c \times Post_t \times SO2_i$.

reported in Table 4, column 1. We find a small and statistically insignificant estimated coefficient, suggesting that toughening environmental regulation has no effect on domestic investment. In columns 2 and 3, we further decompose the sample of domestic firms into state-owned enterprises (SOEs) and non-SOEs, and continue to find insignificant effects of environmental regulation.

[Insert Table 4 Here]

These results can be explained by the institutional features in China. First, SOEs are highly controlled by the governments, specifically, by the State-owned Assets Supervision and Administration Commission (SASAC) of the State Council. Important decisions such as the opening or closing of SOEs and adjustment of investment are not generally made by the general managers, but strongly influenced by administrative orders from the governments. For example, during the financial crisis in 2008-2009, China's President Hu Jintao announced publicly that SOEs could not lay off their employees and should instead try to expand labor employment. Similarly, in the summer of 2013, because of the slowdown in China's economic growth, less than half of the university graduates in China found a job. The Chinese government again ordered SOEs to hire as many college graduates as possible. On the other hand, because of the poor economic institutions in China, non-SOEs are often local firms, which have connections and networks. For example, a major source of startup capital comes from informal financing, such as family wealth and borrowings from relatives and friends, as non-SOEs are discriminated against bank loans due to the financial repression system (e.g., Allen, Qian, and Qian, 2005; Ayyagari, Demirguc-Kunt, and Maksimovic, 2008). Meanwhile, poor protection of property rights and weak contract enforcement make non-local transactions risky, and trade expansion over regions is significantly influenced by the political connections of the entrepreneurs (e.g., Lu, 2009). Combined, these institutional imperfections make Chinese domestic firms less foot-loose compared with their foreign counterparts, which explains the insignificant effects of environmental regulation on domestic investment.

4.3 Heterogeneous Effects

Thus far, we have estimated the average effect of environmental regulation on FDI flows from all origin countries. With the information on FDI source country in the FIE survey data, we are able to investigate the possible heterogeneous effects across FDI home countries. Specifically, we examine whether foreign multinationals from countries with more stringent environmental regulations behave differently from those from countries with less stringent regulations. To this end, we categorize foreign countries into two groups depending on their regulations relative to China's. Hence, for countries with more stringent environmental regulations than China, their multinationals go to a country with weaker regulations compared with the levels in their home countries. Meanwhile, multinationals from countries with less stringent environmental regulations than China invest in an country with stronger protection of environments.

We use two methods to rank countries in terms of their environmental regulations. First, we check when each country joined the *United Nations Framework Convention on Climate Change* (UNFCCC), an international environmental treaty put into effect in 1994. The objective of the UNFCCC is to "stabilize greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system." We then divide the sample into two groups, based on whether the country joined the UNFCCC before or after China did in 1994. We conceptualize that the later participant countries may have lower recognition and less stringency of environmental protection than earlier participant countries (the list of countries in the two groups is provided in Appendix Table A4). The regression results for the two groups are presented in Table 5, columns 1 and 2. Interestingly, we find that the effect of environmental regulation on FDI flows is small and statistically insignificant for the group of countries that joined the UNFCCC at the same time as China, but the effect remains economically and statistically significant for the later participant countries.

[Insert Table 5 Here]

However, a problem with the UNFCCC is that "The framework set no binding limits on greenhouse gas emissions for individual countries and contains no enforcement mechanisms." These issues were addressed in the *Kyoto Protocol* in 1997, which "established legally binding obligations for developed countries to reduce their greenhouse gas emissions in the period 2008-2012." Hence, in a second approach, we collect the year when each country signed the *Kyoto Protocol*, and divide the sample into two groups, based on whether the county signed the ratification of the *Kyoto Protocol* before or after China did. Given the binding obligations in the *Protocol*, we hypothesize that countries signed the treaty earlier (later) than China have more (less) stringent environmental regulations than China. The list of countries in the two groups is presented in Appendix Table A5. The estimation results are reported in Table 5, columns 3 and 4. Consistently, we find that the stringent environmental regulation has a sizable and statistically significant effect only on FDI flows from countries that joined the *Protocol* later.

Combined, these results indicate that foreign multinationals from countries with good environmental protection are insensitive to the change in environmental regulation in China. Their investment in China is possibly to exploit other benefits of the country, instead of the lax environmental regulation. However, environmental regulation seems to be an important factor determining investment in China by foreign multinationals from countries that joined international treaties on environmental protection later than China. These findings may help relieve the concern that toughening environmental protection in developed countries would cause a shift of dirty manufacturing production to countries with laxer environmental regulations, which then may not combat the environmental deterioration and would have significant distributional implications on employment and industry structures.

5 Conclusion

In this paper, we investigated whether foreign multinationals respond to environmental regulations by reallocating their production to places with less stringent regulations. To control for the potential endogeneity of environmental regulations, we use a change in environmental policy, namely China's 1998 TCZ policy. Our identification of the effect of environmental regulation comes from a comparison of the outcome variable for TCZ cities in pollution intensive industries versus clean industries with that for non-TCZ cities before and after the policy change, or the DDD estimation.

By using two comprehensive firm data sets in China (i.e., the 1996 and 2001 censuses on basic units and the 2001 survey of FIEs), we find that a one-standard-deviation increase in pollution intensity causes the negative effect of environmental regulation on FDI flows to be 8 percentage points lower, confirming the pollution haven hypothesis. The results are robust to a series of robustness checks on the identifying assumption, and to checks on other econometric concerns. Moreover, we find that foreign multinationals from countries with better environmental protections than China are insensitive to the toughening environmental regulation, while those from countries with worse environmental protections than China show strong negative responses.

This paper contributes to the literature on the pollution haven hypothesis by carefully addressing the endogeneity problem associated with environmental regulations. Meanwhile, our use of data from a developing country complements existing studies that focus more on developed countries, particularly the U.S.

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