Is There a Pollution Haven Effect? Evidence from a Quasi-Natural Experiment in China

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Abstract

In this paper, we investigate whether there is a pollution haven effect, specifically, the effect of environmental regulations on firm location. Our identification uses the Two Control Zones (TCZ) policy implemented by the Chinese government in 1998. The difference-in-differences (DID) estimation shows that cities with tougher environmental regulations attract less foreign direct investment (FDI). Specifically, toughening environmental regulations causes the amount of FDI to drop by 25.6%. Our results are robust to various robustness checks on the validity of the DID estimation and other estimation concerns.

Keywords: Pollution haven effect; Difference-in-differences estimation; Environmental regulations; Two control zones; Quasi-natural experiment

JEL Codes: R11; L25; D22

1 Introduction

Concerned about the further deterioration of living environments, governments across the world are toughening their regulations on pollution with the hope that firms will develop greener technologies and produce more environmentally responsible goods. An unintended consequence, however, is that firms may respond by reallocating production to places with less stringent environmental regulations, a phenomenon known as the pollution haven effect. This may not only counteract the effects of environmental policies, but 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 the overall pollution levels.

Despite much anecdotal evidence, however, empirical studies fail to provide conclusive findings on the pollution haven effect.[1] Some studies find no such effect,[2] while others detect the effect of environmental regulations on the location choice of firms.[3] As a result, the investigation on the pollution haven effect 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 to finding a pollution haven effect is how to deal with the potential endogeneity of environmental regulations. Much of the existing literature treats environmental regulations as exogenous (see Levinson, 2008 for a survey). Recent studies start to tackle the potential endogeneity of environmental regulations, for example, by using either the instrumental variable approach (see Millimet and Roy, 2011, for a survey) or the propensity score matching method (List, Millimet, Fredriksson, and McHone, 2003).[4]

We examine whether there is a pollution haven effect by using a change in environmental regulations, i.e., the implementation of the Two Control Zones

[1] Jeppesen, List, and Folmer (2002) conduct a meta-analysis and conclude that differences in methodological considerations explain much of variations in these findings.

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

[3] For example, Henderson (1996); Becker and Henderson (2000); List and Co (2000); Keller and Levinson (2002); List, Millimet, Fredriksson, and McHone (2003); Dean, Lovely, and Wang (2009); Kellenberg (2009).

[4] Both the instrumental variable estimation and the propensity score matching method require some identification assumptions. For the former, instrumental variables must be exogenous, whereas for the latter, both observables and unobservables must be matched.

(TCZ) policy in China, as a quasi-natural experiment (for details about environmental regulations in China, see Section 2).[5] Specifically, we explore two variations, time (before and after the policy change) and cross-sectional (some cities had the new environmental policy — treatment group, and others did not — control group), to conduct a difference-in-differences (DID) analysis. Our DID estimation shows that cities with tougher environmental regulations attract less FDI, which confirms the pollution haven effect. Meanwhile, the magnitude of the pollution haven effect is found to be large: toughening environmental regulations causes the amount of FDI to drop by around 25.6%. While this estimate is comparable with those in the literature (e.g., Becker and Henderson, 2000; Kellenberg, 2009), we conduct two further analysis to preclude the concern that the effect is largely driven by the defection of FDI from treatment to control groups.

The validity of our DID estimation hinges on the condition that the treatment group would have followed the trend of the control group in attracting FDI if they had not implemented the new environmental policy. To verify the satisfaction of this identification assumption, we conduct a series of sensitivity analyses. First, we show that the designation of TCZ status is not driven by pre-existing economic performance and political considerations, and is largely compliant with the government policy. We then experiment with saturating the model with city-specific time trend, checking any differential pre-existing time trends, and conducting a placebo test, a series of falsification tests, and an instrumental variable estimation. Our findings on the pollution haven effect remain robust to all of these validity checks.

In addition to the change in the environmental policy, China provides an ideal setting for investigating the pollution haven effect. On the one hand, since it adopted the open and reform policy in 1978, Chinese governments have been aggressively attracting FDI, which has made China the seventh-largest FDI (stock) recipient country in the world.[6] On the other hand, China's fast economic growth in recent decades has been accompanied by severe environmental degeneration, such as over-exploration and mass industrial pollution, which are typical problems in developing countries. Meanwhile, China is a large country with substantial differences in the FDI distribution and environmental quality, which provides us with enough variations to identify the pollution haven effect.

Our study is similar to and complements the work using the change of environmental regulations, specifically, the Clean Air Act and its amendment

[5] Hering and Poncet (2011) use the same setting to investigate how environmental regulations affect export activity of firms.

[6] Based on the statistics by the CIA World Factbook (accessed on July 18, 2012).

in the U.S. For example, Becker and Henderson (2000) use a panel data for the 1963-1992 period and find that firm birth rate for polluting industries in non-attainment areas (that have tougher regulations) is reduced by $26-45\%$. A recent work by Hanna (2011) also uses a DID analysis to investigate how the Clean Air Act Amendments in the U.S. affects its outflow FDI. Whereas these studies look at the U.S., the largest developed country in the world, we use data from China, the largest developing country in the world. Meanwhile, we investigate how environmental regulations affect the amount of FDI a city receives (or the FDI recipient side), whereas Hanna (2011) examines whether U.S. multinationals reallocate their production to foreign countries in response to domestic environmental regulations (or the FDI sourcing side).

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 estimation framework of the pollution haven effect, along with a number of robustness checks on the identification assumption. Data and variables are described in Section 4, and empirical findings are reported in Section 5. The paper concludes with Section 6.

2 Environmental Regulations in China

2.1 Background

The SO2 emissions generated by coal combustion have increased substantially alongside the fast economic growth in China in past decades. National coal consumption in 1990 was 1.05 billion tons and increased to 1.28 billion in 1995. In 1993, 62.3% cities in China had annual average ambient SO2 concentration values above the national Class II standard. In Chongqing, the annual ambient SO2 concentration reached 270 or 4.5 times the national Class II standard. Around the same period, 40% of the national territory reported acid rain with average PH value lower than 5.6.

SO2 and acid rain may hurt human health and destroy ecosystems, which may consequently impede economic growth. Concerned with its long-term sustainable economic development, Chinese governments started to tackle air pollution issues in the mid 1980s by implementing a series of regulatory policies. The Air Pollution Prevention and Control Law of the People's Republic of China (APPCL) was enacted in 1987 and executed in 1988. This new environmental law provided general principles of regulation for air pollution for local governments and related agencies. However, the APPCL was considered very sketchy. For example, it did not present any concrete policies on how to control SO2 emissions and specify which government body should be responsible for enforcing the policies. As a result, the effect of the regulation on air pollution was limited, with SO2 emissions and acid rain continuing to increase in the late 1980s and early 1990s.

With a growing concern over the air pollution problem, Chinese governments decided to take stricter measures. In 1995, the 1987 APPCL was amended, and one chapter about the regulation on air pollution and SO2 emissions was included. More importantly, a new policy, namely the Two Control Zones (TCZ) policy, was proposed to prevent the air quality of those heavily-polluted areas from deteriorating further.

The two control zones include SO2 pollution control zones and acid rain control zones. The National Environmental Protection Bureau (NEPB) began designating cities as TCZ in late 1995, based on several criteria. Specifically, a city was designated as a SO2 pollution control zone if: (1) its average annual ambient SO2 concentration was larger than the national Class II standard (i.e., 60 ug/m3) 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. And a city was designated as an acid rain control zone if: (1) its average PH value of precipitation was equal or smaller than 4.5; (2) its sulfate deposition was above the critical load; or (3) its SO2 emissions were large.

In 1997, "The Request for Approval of the Proposal of Designation for Acid Rain Control Areas and SO2 Pollution Control Areas" was issued by NEPB and sent to State Council for approval. In January 1998, this proposal was approved by the State Council in the document "The Official Reply of the State Council Concerning Acid Rain Control Areas and SO2 Pollution Control Areas". It was then put into effect. Among a total of 380 prefecture-cities, 175 were designated as TCZ. Figure 1 shows the geographic distribution of TCZ cities in China. In general, SO2 pollution control zones are located in northern China because of the heating system, whereas acid rain control zones are located in southern China where the climate is relatively more humid.

Once a city was designated as TCZ, tougher regulatory policies were implemented. For example, according to the amendment, if new thermal power plants, medium or large firms with serious SO2 emissions were to be built in these zones, desulfurization, dust-collecting facilities and other required equipment must be installed. For the existing SO2-emitting plants, SO2-reducing and dust-collecting measures must be taken.

In the 1998 approval document for the list of TCZ cities, the State council also laid out the targets for environmental control in TCZ cities in the short run (2000) and in the long run (2010). Specifically, for 2000, "the sources of industrial SO2 pollution should achieve the national standard of discharging SO2. 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 areas should be alleviated." For 2010, "the total amount of SO2 emission should be lower than that of 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 or smaller than 4.5 should be reduced significantly."

These new environmental regulations have generated significant improvement in air pollution control. In 2000, 102 TCZ cities achieved the national Class II standard of average ambient SO2 concentrations and 84.3% of severely-polluted firms achieved the target level of SO2 emissions (China Environment Yearbook, 2001). The average growth rate of SO2 emissions from industries and livelihood in TCZ cities from 2001 to 2006 was -6.5% (Annual Statistic Report on Environment in China, 2007). In 2010, 94.9% of TCZ cities had achieved the national Class II standard of average ambient SO2 concentrations, with no city reporting values above the national Class III standard (Report of Ministry of Environmental Protection of the People's Republic of China, 2011).

In Figure 2, we report the annual average ambient SO2 concentrations for TCZ and non-TCZ cities from 1992 to 2008 (China Environment Yearbook, various years). There is a clear pattern of annual average ambient SO2 concentrations in TCZ cities decreasing substantially over this period. By 2008, no city reported number above 100 ug/m3 and the SO2 emission in TCZ cities became similar to those in non-TCZ cities.

2.2 Designation of TCZ Cities

A key challenge of using TCZ policy as a quasi-natural experiment to investigate the pollution haven effect is that the designation of TCZ cities may be correlated with some unobserved determinants of FDI. Note that we employ city fixed effects in all the following regressions; hence, the relevant concern is whether the designation of TCZ cities may be correlated with any pre-existing city trends, in particular, the time trend of FDI. Two facts presented in the previous sub-section may help relieve such a concern. First, the initiation of the TCZ policy and the designation of TCZ status were conducted by the central government and largely exogenous to lower-level (such as city) governments. Second, the designation of TCZ cities was based on several criteria, in particular past pollution levels (i.e., ambient SO2 concentration value or the PH value of precipitation) and specific threshold levels, both of which could not be manipulated by city governments retrospectively.

In addition, we conduct a quantitative analysis to better understand the determinants of TCZ designation. Specifically, we look at economic factors (such as economic development, industrial production, infrastructure, and consumption), geographic factors (such as coastal versus inland versus northern areas), and political factors (such as the administrative level of city), as well as the FDI growth rate and past pollution levels. See Table 2 for the details of the construction and summary statistics of these variables. The dependent variable of the analysis is a dummy variable, TCZ_c, indicating city c's TCZ status in 1998, i.e.,

$$TCZ_c = \begin{cases} 1 & \text{if city } c \text{ is a TCZ city} \\ 0 & \text{if city } c \text{ is a non-TCZ city} \end{cases} .$$

The Probit regression results are reported in Table 1. In Column 1, we only include economic, geographic and political factors, all measured as the average of pre-TCZ period (i.e., the 1992-1997 period, with 1992 being the first year we have city-level information). It is found that none of these economic and political factors has any significant statistical power. Instead, TCZ cities were less likely to be found in Northern and coastal areas in China. However, these geographic considerations may not cause any troubles for our investigation of the pollution haven effect, given that city fixed effects are always included in the analysis. In Column 2, we further include FDI growth rate during the pre-TCZ period. It is found that the estimated coefficient of FDI growth rate is not only statistically insignificant, but also very small in magnitude.

In Columns 3-4, we add past pollution levels and the specific threshold levels (the criteria used by the central government to designate TCZ cities) to investigate the effectiveness of the government policy. It should be pointed out that the construction of these two pollution-related explanatory variables face two data challenges. First, we do not have information about the PH values of precipitation (the assignment variable for TCZ cities in southern China). Our remedy is to use the average annual ambient SO2 concentration to replace the PH value for southern cities, because the dissolution of SO2 in water reduces the PH value and generates acid rain, and the assignment should be comparable across northern and southern cities. Second, information of average annual ambient SO2 concentrations is only available for around 80 cities, about 30% of the whole sample. Nonetheless, the regression results are consistent with the government policy: first, as shown in Column 3, the past pollution level (i.e., average annual ambient SO2 concentrations in 1995)[7] is positively and statistically significantly correlated with the TCZ

[7] Using average of 1993-1995 or 1994-1995 produces similar results (available upon request).

status; second, cities with SO2 concentrations above the specific threshold level (i.e., 60 ug/m3) set by the central government to designate TCZ cities are more likely to become TCZ cities (see Column 4); and third, conditional on the specific threshold level, SO2 concentrations level does not have any independent effects on the TCZ status.

In summary, our quantitative analysis shows that the TCZ status is not determined by past economic performance and political considerations, in particular the growth prospect of FDI, while the distribution of TCZ cities is much concentrated in the southern, inland part of China. More importantly, we find evidence of compliance with the government policy: the designation of TCZ status is determined by whether cities' past pollution levels are beyond the specific threshold level set by the central government.

3 Estimation Strategy

To identify the pollution haven effect, we exploit the TCZ policy that was put into effect in 1998 in China as a quasi-natural experiment to conduct a DID analysis. Specifically, there are two groups of cities, the treatment and control groups. The treatment group comprises cities designated as TCZ in 1998 (or TCZ cities), whereas the control group includes cities not designated as TCZ in 1998 (or non-TCZ cities). The baseline DID estimation has the following specification

$$Y_{ct} = \alpha_c + \gamma \cdot TCZ_c \times Post_t + \delta_{pt} + \mathbf{X}'_{ct}\boldsymbol{\beta} + \varepsilon_{ct}, \tag{1}$$

where Y_{ct} is the logarithm of FDI in city c at year t; α_c is the city dummy, capturing city c's all time-invariant characteristics; $Post_c$ indicates the post-treatment period, i.e.,

$$Post_c = \begin{cases} 1 & \forall t \geq 1998 \\ 0 & \text{otherwise} \end{cases} ;$$

and ε_{ct} is the error term. To deal with potential heteroskedasticity and serial correlation, we cluster the standard errors at the city level, following Bertrand, Duflo, and Mullainathan (2004).

As Chinese provinces usually have different regional policies and guidelines for policy enforcement, we include province-year dummies, δ_{pt}, to control for any arbitrary (time-varying or time-invariant) provincial compounding factors. Meanwhile, the inclusion of province-year dummies provides us with a control for the spatial correlation issues pointed out by Drukker and Millimet (2008).

We also in the baseline estimation control for many other potential determinants of FDI, $\mathbf{X}_{ct}$, to isolate the effect of environmental policy. These controls include education (i.e., number of college students and number of high school students), infrastructure (i.e., number of telephone and road density), economic growth, and market size (i.e., industrial production and retail consumption). See Table 2 for the detailed construction and summary statistics of these variables.

Our identification assumption thus requires that conditional on a whole list of controls $(\alpha_c, \delta_{pt}, \mathbf{X}_{ct})$, our regressor of interest, $TCZ_c \times Post_t$, is uncorrelated with the error term, ε_{ct}, i.e.,[8]

$$E\left[\varepsilon_{ct} | TCZ_c \times Post_t, \alpha_c, \delta_{pt}, \mathbf{X}_{ct}\right] = E\left[\varepsilon_{ct} | \alpha_c, \delta_{pt}, \mathbf{X}_{ct}\right]. \tag{2}$$

Even we have argued in the previous section that the designation of TCZ status is not correlated with pre-existing economic and political factors and hence can be considered to be largely exogenous to local community, there may remain several concerns about the satisfaction of our identification assumption (2).

One potential concern is the existence of some unidentified (or unobserved), pre-existing differential trends that may generate both the TCZ status and differential trajectories of *ex post* FDI between the treatment and control groups. We conduct two robustness checks on this potential concern. First, we saturate the baseline model by including city-specific linear trend, that is, the interaction between city dummy λ_t and time variable t. Second, we explicitly check whether there are differential trends of FDI between the treatment and control groups one year and two years before the implementation of the TCZ policy.

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 whether there is any expectation effect, that is, the effect of environmental regulation on FDI happened before the effective date of the policy. As a robustness check, we conduct a placebo test, that is, using year 1996 instead of year 1998 as the time of treatment. Given that the TCZ

[8] Note that the identification does not require our control variables to be exogeneous, i.e.,

$$E\left[\varepsilon_{ct} | \alpha_c, \delta_{pt}, \mathbf{X}_{ct}\right] = 0.$$

See Stock and Watson (2012, p274) for more discussion on this point.

policy was put into effect only after 1998, the use of 1996 as the beginning of treatment should not produce any significant treatment effect.

Finally, if there are other policies adopted in the same time, any findings of the treatment effect cannot be attributed only to the pollution haven effect. There are two important events in 1998, the on-going Asian financial crisis and SOEs reform launched by Premier Zhu Rongji. If the Asian financial crisis hit TCZ cities more badly and if SOEs reform was carried out more prominently in TCZ cities than non-TCZ cities, these may contaminate our estimation of the pollution haven effect. To address this concern, we employ two additional robustness checks. First, we construct two variables, $Trade\ Exposure_c$ (the ratio of total trade over GDP)[9] and $SOE\ Presence_c$ (the percentage of SOEs) both measured as the average of pre-treatment period, to capture potential impacts of the Asian financial crisis and SOEs reform across cities, respectively. We then interact these two variables with the indicator of the post-treatment period ($Post_t$) and include the two interaction terms (i.e., $Trade\ Exposure_c \times Post_t$ and $SOE\ Presence_c \times Post_t$) in the baseline regression to isolate the pollution haven effect. Second, instead of looking at FDI as the outcome variable, we examine other outcome variables $\mathbf{Z}_{ct}$ that are supposed to be unaffected by the change in environmental regulation. Any findings of insignificant effects of the TCZ policy on these outcome variables $\mathbf{Z}_{ct}$ may indicate that there are no other compounding policies taking place in the same time.

4 Data and Variables

The data used in this study come from the following three sources:

1. *Chinese City Statistical Yearbook* for the period 1992 (the earliest) - 2009 (the most recent)

2. *Chinese Environment Yearbook* for the period 1992 (the earliest) - 2008 (the most recent)

3. The State Council's official document, "The Official Reply of the State Council Concerning Acid Rain Control Areas and SO2 Pollution Control Areas"

[9] Unfortunately, there is no information regarding the trade exposure to those financial-crisis-affected Asian countries like Tailand. Hence, we resort to the general level of trade exposure.

From the first data source, we collect yearly data about our outcome variable, the amount of FDI, for each city during the 1992-2009 period,[10] as well as our control variables $\mathbf{X}_{ct}$, outcome variables $\mathbf{Z}_{ct}$ used in the falsification tests and potential determinants used in the investigation on the designation of TCZ status, such as the number of college students, the number of high school students, the number of telephones, GDP, population, the number of road areas, industrial production, and the number of retail consumptions. Summary statistics of key variables are presented in Table 2.

From the second data source, we obtain information about the annual average ambient SO2 concentrations. The SO2 concentration statistics come from the records of many monitoring stations in a few cities, the number of which has steadily increased over time. For example, there were only 65 cities with records of pollution in 1992, whereas in 2003 that number rose to 113. To construct the instrumental variable, we use information from 1995, which contains information on the value of the annual average ambient SO2 concentrations for 80 cities.

The third data source provides us with a detailed name list of cities designated as the TCZ. During our sample period (1992-2009), the composition of this list remained unchanged. Appendix Table 1 supplies this list of these TCZ cities. Among a total of 280 cities for which the *Chinese City Statistical Yearbook* has information, 158 are TCZ cities.

Figure 3 shows (unconditional) time trends of the average of the total amount of FDI (in logarithm form) in TCZ and non-TCZ cities during the 1992-2009 period. In general, TCZ cities attracted more FDI than non-TCZ cities. Meanwhile, both groups exhibited an upward trend in the amount of FDI in this time period, which reflects the effects of China's open and reform policy and rapid economic growth. Interestingly, before 1998 (the time of the TCZ policy became effective), FDI grew faster in TCZ than non-TCZ cities. After the implementation of the TCZ policy, the growth of FDI in TCZ cities slowed while that in non-TCZ cities caught up. At the end of the sample period (i.e., 2009), the gap in the amount of FDI between these two groups was much smaller than it had been at the beginning of the sample period (i.e., 1992).

[10] However, the *Chinese City Statistical Yearbook* does not have information about FDI for each industry or sector, as a result of which we have to look at the average effect across all industries instead of examining the differential effects in polluting and non-polluting industries.

5 Empirical Findings

5.1 Main Results

Our baseline DID estimation results corresponding to equation (1) are reported in Column 1 of Table 3. The DID estimator γ (i.e., the estimated coefficient of the interaction between the indicator of the treatment status TCZ_c and that of the post-treatment period $Post_t$) is found to be negative and statistically significant. This result implies that cities with tougher environmental regulations (i.e., the implementation of the TCZ policy) attracted fewer FDI, confirming the pollution haven effect.

The estimated coefficients of other economic determinants of FDI also make economic sense, though it is caveat to interpret these as causal. Better telecommunication and transportation infrastructure attracts more foreign investment. And FDI are clustered in cities with larger domestic production, which supports the agglomeration theories. Moreover, foreign firms are more likely to locate in cities with larger domestic consumption.

Interpretation of the magnitude. The economic magnitude of the pollution haven effect is found to be significant. The implementation of the TCZ policy caused the amount of FDI to drop by 25.6%. This magnitude is comparable to those found in the literature. For example, Becker and Henderson (2000) find that tougher environment regulations cause firm birth rate in polluting industries to drop by $26-45\%$. Kellenberg (2009) estimates that during 1999-2003, the failing environmental policy caused the value added of U.S. affiliates located in the top 20th percentile countries to grow by approximately 8.6% while the corresponding number for the top 20th percentile developing and transition economies was 32%. Hanna (2011) finds that the Clean Air Act Amendments over the 1966-1999 period increased U.S. multinationals' foreign assets by 5.3% and foreign output by 9%.

One may be concerned about whether the aforementioned significant pollution haven effect is due to the withdrawn of FDI into China or the defection of FDI from TCZ to non-TCZ cities, as the latter may lead to an over-estimation of the pollution haven effect. Based on the premise that defection is relatively easier among neighboring places, we conduct two additional analysis to shed light on this concern. First, we replace province-year dummies (δ_{pt}) in the baseline specification with year dummies (λ_t), which essentially relax the comparison between treatment and control groups in the same province. Hence, if there is a defection effect, we should find a much smaller pollution haven effect in the regression with year dummies than in the one with province-year dummies. As shown in Column 2 of Table 3, we indeed find a larger estimation coefficient, though it is statistically indifferent from the baseline estimate (i.e., Column 1 of Table 3).

Second, we specifically for each TCZ city construct a group of all its non-TCZ cities as the control group. If there is a defection effect and the defection is relatively more easier among neighboring cities, we should detect a larger pollution haven effect using this alternative control group.[11] However, as shown in Column 3 of Table 3, the estimated coefficient instead drops to -0.211 (from -0.256 in the baseline regression). And the Hausman test shows that these two estimates are statistically indifferent.

Combined, these two analysis suggest that the significant pollution haven effect we have found should not be entirely driven by the defection of FDI from the treatment to control groups.

5.2 Checks on the Identification Assumption

Whether our DID estimator in Table 3 captures the true pollution haven effect hinges on the satisfaction of our identification assumption (2), that is, the treatment would have followed the same trend of the control group in the case of no treatment. In this sub-section, we present the results of a number of robustness checks discussed in Section 3.

First, we include city-specific time trend in Column 1 of Table 4. It is found that our DID estimate of the pollution haven effect remains statistically significant. Despite an increase in the estimated magnitude, the Hausman test shows that the DID estimate with the inclusion of the city-specific time trend is statistically indifferent from the baseline DID estimate in Table 3.

Second, Column 2 of Table 4 reports the estimation results regarding the check on any differential pre-existing time trends one year and two years before the treatment. Neither $TCZ \times Prior1$ (an indicator of one year before the treatment) nor $TCZ \times Prior2$ (an indicator of two years before the treatment) has any statistical significance. These findings suggest that the treatment and control groups have similar time trends (at least) two years before the treatment, which precludes any pre-existing differential trends between the treatment and control groups.

Third, as a placebo test, we use 1996 as the time of treatment instead of the real effective date, 1998. If there is no expectation effect and the treatment and control groups are comparable before the treatment, then the DID estimate using 1996 as the time of treatment should not produce any statistical significance. Indeed, we find that it is statistically insignificant (Column 3 of Table 4), which reinforces the validity of our DID estimation.

[11] Note that the number of observation increases due to the fact that a non-TCZ city may be used multiple times in constructing control group for different TCZ cities.

Fourth, in Columns 4-6 of Table 4, we stepwisely include the interaction between trade exposure and the indicator of the post-treatment period and the interaction between SOEs presence and the indicator of the post-treatment period, which can allow us to isolate the effect of environmental policy from another two big episodes happened around the same time (i.e., the on-going Asian financial crisis and the SOEs reform). Clearly, none of these two interaction terms has any statistical power. More importantly, our estimated pollution haven effect remains nearly unchanged in terms of both statistical significance and economic magnitude.

Fifth, in Table 5, we report a series of falsification tests, in which we replace our outcome variable of interest (the amount of FDI) with six other outcome variables that are not supposed to be affected by the change in environmental regulations. The estimation results show that none of these six DID estimates produce any statistical significance and many of the estimated magnitudes are quite close to zero.[12] These results suggest that the pollution haven effect we have identified may not be compounded by other time-varying factors. However, one may be concerned whether the lack of significance is due to the fact that an important determinant of these outcome variables is the amount of FDI, which is in turn substantially affected by the change in environment regulations. In an unreported table (available upon request), we add the logarithm of FDI in all these six regressions and find six DID estimates barely change in both statistical significance and magnitude.

In summary, despite the fact that the identification assumption of the DID estimation cannot be fully verified, the analysis conducted in this sub-section boosts our confidence that our DID estimates may not suffer from severe estimation bias.

5.3 Other Robustness Checks

In this sub-section, we conduct additional robustness checks on our aforementioned findings.

First, we exclude four municipalities (Beijing, Chongqing, Shanghai, and Tianjin), which have higher administrative levels and hence potentially different government policies. Estimation results are reported in Column 1 of

[12] One may be concerned that the statistical insignifiance is due to the lack of time variations for these six outcome variables. In Appendix Table 2, for each of these six outcome variables, we report the mean value and standard deviation of the coefficient of variation CV (a standard measure of the degree of dispersion in the literature, defined as the standard deviation of the outcome variable for an individual city over time divided by the corresponding mean value of all cities). We find significant time variations in these outome variables among all cities during 1992-2009.

Table 6. It is found that our DID estimate barely changes with the exclusion of these four municipalities.

Second, we exclude cities without information about the amount of FDI in 1998 because they do not have immediate post-treatment values. Estimation results are reported in Column 2 of Table 6. The new estimator becomes even more statistically significant, which further confirms our previous findings.

Third, we exclude cities without information about the amount of FDI in the 1995-1997 period because they do not have enough pre-treatment values. As shown in Column 3 of Table 6, our main findings on the pollution haven effect continue to hold in this sub-sample.

Finally, we exploit the discontinuity in the designation of TCZ cities to construct a plausibly exogenous instrument for TCZ status. Specifically, the instrumental variable is constructed as

$$TCZ_{IV} = I\left[SO2_{c95} \geq 60ug/m^3\right],$$

where $I[.]$ is an indicator function that takes a value of 1 if the argument in the bracket is true and 0 if false; and $SO2_{c95}$ is the average annual ambient SO2 concentration in 1995. Regression results are reported in Appendix Table 3. As shown in Column 1, the instrumental variable is found to be positive and statistically significantly correlated with our regressor of interest. With respect to our central issue, the instrumented DID estimate remains negative and its magnitude is statistically indifferent from our baseline DID estimate according to the insignificant Dubin-Wu-Hausman test. However, as expected, due to the severe sample attrition problem (i.e., 30% of the total sample), the standard error of the estimated instrumented DID estimate is quite large.[13]

6 Conclusion

In this paper, we investigate whether there is a pollution haven effect, specifically, whether firms 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 pollution haven effect comes from a comparison of the outcome variable for TCZ cities with that for non-TCZ cities before and after the policy change, or the DID estimation.

[13] Another possible explanation for the statistical insignificance is that our instrumental variable may be weak, as the weak identification statistic is below the conventional value for the safety zone of strong instrument (i.e., 10; see Straiger and Stock, 1997).

By using the amount of FDI for 280 cities over the 1992-2009 period, we find that cities designated as TCZ attract around 25.6% less FDI than their non-TCZ counterparts. The results are robust to a series of robustness checks on the identification assumption, along with other econometric concerns.

Our paper contributes to the literature on the pollution haven effect 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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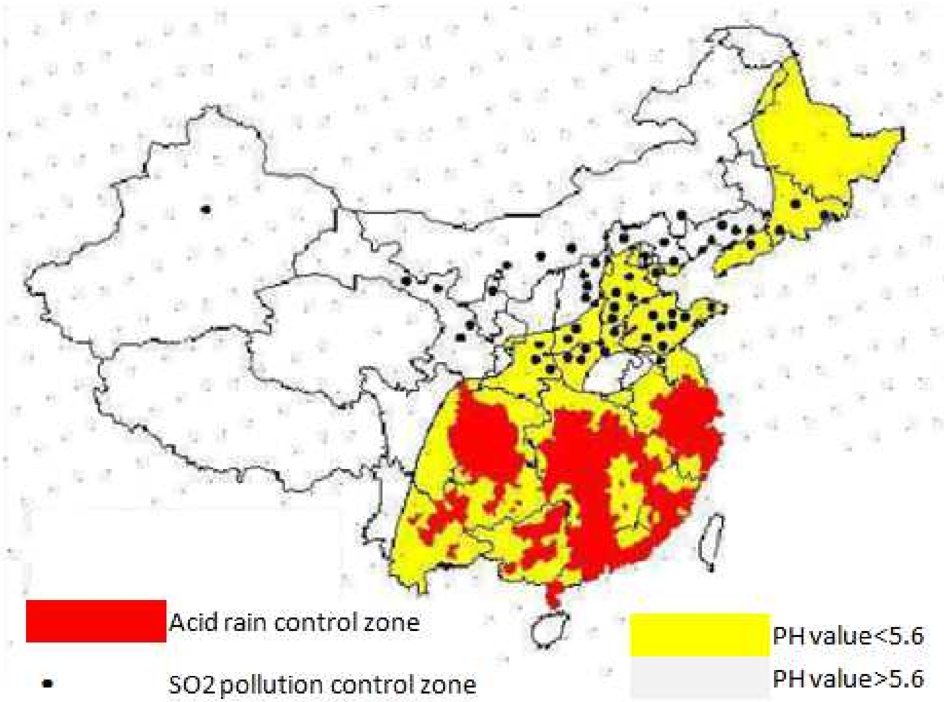

(Source: The national Environmental Protection Bureau, “The Proposal of Designation for Acid Rain Control Areas and SO2 Pollution Control Areas”)

Figure 1: Distribution of TCZ cities

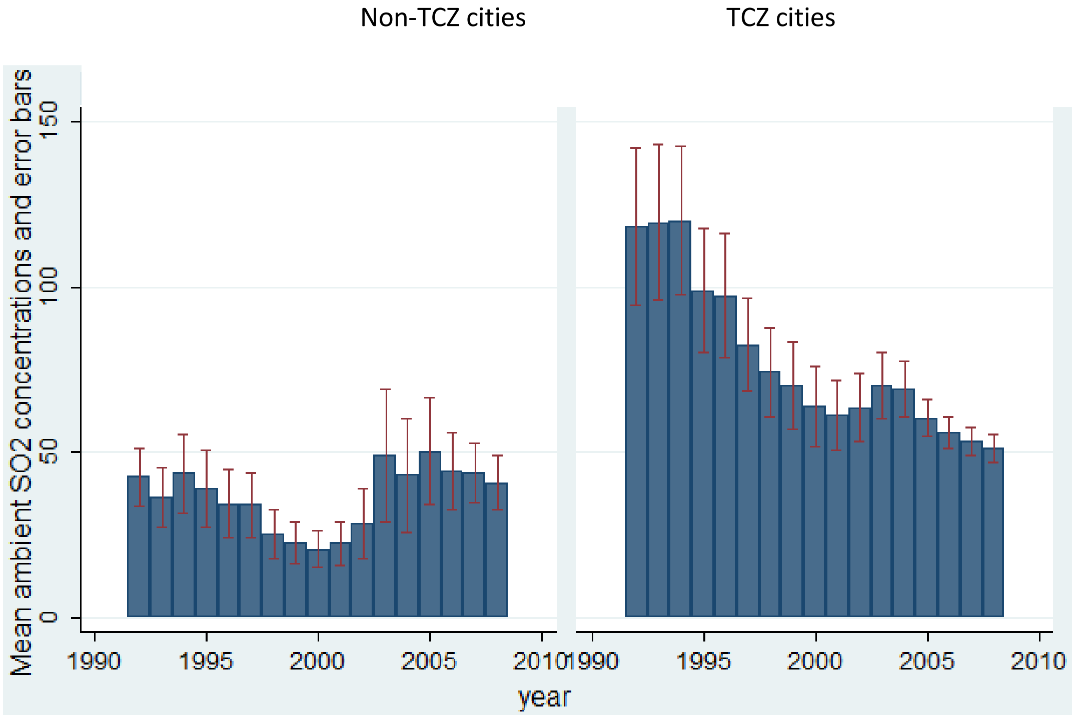

Figure 2: Plot of the mean and standard deviations for ambient SO2 concentrations (ug/m3) in TCZ cities and non-TCZ cities

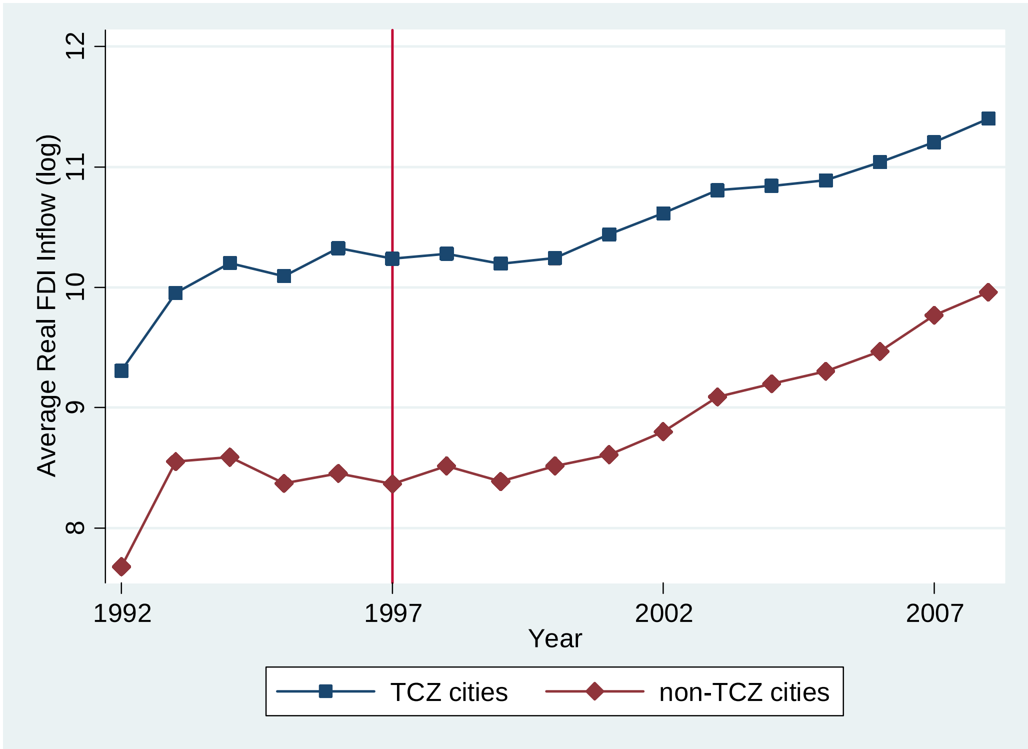

Figure 3: Average FDI inflow into TCZ cities and non-TCZ cities

Table 1, Determinants of TCZ designation

Variables	(1)	(2)	(3)	(4)
GDP (log)	0.020	0.007	-0.106	-0.150
	(0.100)	(0.105)	(0.217)	(0.166)
GDP growth rate	0.021	0.027	-0.038	-0.000
	(0.074)	(0.079)	(0.106)	(0.094)
Number of firms (log)	0.047	0.043	0.036	-0.004
	(0.065)	(0.066)	(0.117)	(0.104)
Total employment (log)	-0.137	-0.149	-0.347*	-0.217
	(0.092)	(0.094)	(0.207)	(0.198)
Retail consumption (log)	0.188	0.248	0.381	0.271
	(0.152)	(0.173)	(0.302)	(0.288)
Telephones (log)	0.017	-0.001	0.112	0.203
	(0.099)	(0.110)	(0.212)	(0.185)
Total road area (log)	0.063	0.072	0.004	-0.015
	(0.061)	(0.063)	(0.119)	(0.092)
Northern	-0.207***	-0.202***	-0.063	-0.166
	(0.069)	(0.073)	(0.133)	(0.125)
Coastal	-0.321***	-0.324***	-0.110	-0.028
	(0.072)	(0.071)	(0.142)	(0.179)
Municipality	-0.035	-0.091	-0.186	-0.245
	(0.160)	(0.167)	(0.276)	(0.180)
Provincial capital city	-0.036	-0.054	-0.135	-0.178
	(0.106)	(0.108)	(0.125)	(0.118)
FDI growth rate		0.002	0.003	-0.004
		(0.008)	(0.020)	(0.013)
SO2			0.002***	0.001
			(0.001)	(0.001)
I [SO2>=60ug/m3]				0.474***
				(0.120)
Observations	251	235	76	76
R-squared	0.236	0.241	0.300	0.474

Note: Roust standard errors are reported in the parenthesis. *, ** and *** represent statistical significance at the 10%, 5% and 1% level, respectively.

Table 2, Summary statistics and description of variables

Variable name	Obs.	Mean	S.D.	Min	Max	Description
TCZ	5166	0.56	0.50	0	1	Dummy variable taking value of 1 if the city is designated as TCZ, 0 otherwise
Post	5166	0.67	0.47	0	1	Dummy variable taking value of 1 if year is 1998 or afterwards, 0 otherwise
FDI (log)	4485	8.45	2.06	0.69	13.87	Amount of real FDI received (10,000 USD)
FDI growth rate	4119	1.29	22.90	-1.00	128.81	Growth rate of real FDI received
GDP (log)	4329	14.83	1.17	10.56	18.83	GDP (10,000 CNY)
GDP growth rate	3995	0.18	0.43	-0.88	10.58	Growth rate of GDP
Industrial production (log)	4764	14.68	1.46	9.81	19.34	Output of the industrial sector (10,000 CNY)
Total employment (log)	4266	3.35	0.78	0.31	6.32	Total number of employed persons (10,000)
Number of firms (log)	3304	6.22	1.14	2.94	9.84	Total number of industrial firms
Retail consumption (log)	4566	13.65	1.23	8.99	17.79	Total retail sales of social consumption goods (10,000 CNY)
Trade exposure	4644	0.05	0.17	0.00	0.85	Ratio of Trade value (import and export) in GDP
SOEs presence	4644	0.21	0.08	0.04	0.55	Share of SOEs in all firms (in number)
Telephones (log)	4009	3.78	1.11	-0.35	7.46	Number of telephones owned by every 10,000 households
Buses (log)	4080	5.73	1.23	0.00	10.14	Number of buses
Bus passengers (log)	3477	8.11	1.76	0.00	13.17	Number of bus passengers (10,000 person-time)
Total road area (log)	4150	6.03	1.03	0.00	9.98	Total paved road area (square meters)
Road area per capita (log)	4250	1.65	0.72	-3.91	6.04	Per capita paved road area (square meters)
College students (log)	4143	9.37	1.53	3.22	13.65	Number of college students
High school students (log)	4606	12.04	0.90	4.61	14.65	Number of high school students (10,000)
Primary school students (log)	4501	12.47	0.88	8.13	14.86	Number of primary school students (10,000)
Primary schools (log)	3438	6.82	0.96	0.00	9.70	Number of primary schools
Northern	5148	0.48	0.50	0	1	Northern cities of China
Coastal	5166	0.15	0.36	0	1	Coastal cities of China
Municipality	5166	0.01	0.12	0	1	Four municipalities of China (Beijing, Shanghai, Tianjin, Chongqing)
Provincial capital city	5166	0.09	0.29	0	1	Provincial capital cities in China (27 cities)
SO2	1439	66.57	53.86	1.00	463	Ambient SO2 concentration (ug/m3)

Table 3, Main results

Dependent variable: log FDI	(1)	(2)	(3) Neighboring cities
TCZ * Post	-0.256**	-0.333***	-0.211***
	(0.112)	(0.119)	(0.080)
College students (log)	0.057	0.089**	0.068*
	(0.039)	(0.045)	(0.038)
High school students (log)	0.004	-0.039	-0.018
	(0.065)	(0.058)	(0.058)
GDP growth rate	0.055	0.052	0.385***
	(0.037)	(0.040)	(0.088)
Telephones (log)	0.254**	0.187	-0.237
	(0.106)	(0.115)	(0.221)
Road area per capita (log)	0.108*	0.132*	0.151***
	(0.061)	(0.080)	(0.048)
Industrial production (log)	0.186**	0.269***	0.337***
	(0.091)	(0.090)	(0.063)
Retail consumption (log)	0.296**	0.340**	0.136
	(0.121)	(0.138)	(0.098)
City fixed effects	X	X	X
Province-Year effects	X		X
Year effects		X	
Observations	3,470	3,470	5,037
R-squared	0.629	0.447	0.663

Note: Standard errors, clustered at the city level, are reported in the parenthesis. *, ** and *** represent statistical significance at the 10%, 5% and 1% level, respectively.

Table 4, Checks on the identification assumption

Dep. Var. (log FDI)	(1) Incl. City-time trend	(2) Incl. Pre-existing	(3) Use 1996 as event date	(4) Incl. Trade exposure	(5) Incl. SOEs presence	(6) Incl. Trade exposure & SOEs presence
TCZ * Post	-0.282*	-0.252*	-0.216	-0.258**	-0.261**	-0.258**
	(0.171)	(0.149)	(0.145)	(0.113)	(0.114)	(0.114)
TCZ * Prior1		-0.019				
		(0.188)				
TCZ * Prior2		0.028				
		(0.217)				
College students (log)	0.039	0.057	0.058	0.042	0.042	0.042
	(0.037)	(0.039)	(0.039)	(0.039)	(0.040)	(0.039)
High school students (log)	-0.096	0.004	0.009	0.007	0.007	0.007
	(0.078)	(0.065)	(0.066)	(0.066)	(0.066)	(0.066)
GDP growth rate	-0.020	0.055	0.053	0.054	0.055	0.198*
	(0.049)	(0.037)	(0.038)	(0.041)	(0.042)	(0.112)
Telephones (log)	0.258*	0.254**	0.269**	0.198*	0.205*	0.054
	(0.135)	(0.107)	(0.107)	(0.109)	(0.112)	(0.042)
Road area per capita (log)	0.007	0.108*	0.106*	0.108*	0.108*	0.108*
	(0.060)	(0.060)	(0.060)	(0.063)	(0.063)	(0.063)
Industrial production (log)	0.132	0.186**	0.167*	0.180*	0.175*	0.180*
	(0.108)	(0.091)	(0.091)	(0.093)	(0.092)	(0.093)
Retail consumption (log)	0.289**	0.296**	0.297**	0.354***	0.350***	0.354***
	(0.124)	(0.120)	(0.120)	(0.124)	(0.124)	(0.124)
Trade exposure * Post				-0.160		-0.160
				(0.220)		(0.220)
SOEs presence * Post					-0.027	-0.025
					(0.855)	(0.862)
City fixed effects	X	X	X	X	X	X
Province-Year effects	X	X	X	X	X	X
Observations	3,118	3,470	3,470	3,291	3,291	3,291
R-squared	0.228	0.629	0.628	0.638	0.638	0.638

Note: Standard errors, clustered at the city level, are reported in the parenthesis. *, ** and *** represent statistical significance at the 10%, 5% and 1% level, respectively.

Table 5, Falsification tests

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variables (in log)	Buses	Bus passengers	Middle schools	Primary schools	Primary school students	Total road area
TCZ * Post	-0.119	-0.053	0.039	0.028	0.042	0.019
	(0.079)	(0.151)	(0.030)	(0.049)	(0.034)	(0.048)
College students (log)	0.031	0.025	-0.009	-0.028	-0.010	0.022
	(0.025)	(0.047)	(0.010)	(0.025)	(0.012)	(0.020)
High school students (log)	-0.036	0.199**	0.746***	0.852***	0.914***	0.004
	(0.032)	(0.084)	(0.046)	(0.066)	(0.038)	(0.019)
GDP growth rate	-0.047*	-0.025	-0.016	-0.013	0.006	0.037
	(0.027)	(0.042)	(0.015)	(0.020)	(0.006)	(0.032)
Telephones (log)	0.163***	0.178**	0.067**	0.015	0.035	-0.005
	(0.055)	(0.085)	(0.032)	(0.046)	(0.038)	(0.013)
Road area per capita (log)	0.057*	0.132**	-0.000	-0.033	-0.045***	0.725***
	(0.034)	(0.057)	(0.012)	(0.027)	(0.017)	(0.042)
Industrial production (log)	0.021	0.072	0.008	-0.062	0.031	0.045
	(0.052)	(0.080)	(0.021)	(0.039)	(0.024)	(0.029)
Retail consumption (log)	0.144**	0.140	0.032	0.016	0.167***	0.094**
	(0.073)	(0.085)	(0.031)	(0.052)	(0.043)	(0.038)
City fixed effects	X	X	X	X	X	X
Province-Year effects	X	X	X	X	X	X
Observations	3,447	2,930	3,098	3,098	3,530	3,498
R-squared	0.675	0.589	0.875	0.823	0.911	0.899

Note: Standard errors, clustered at the city level, are reported in the parenthesis. *, ** and *** represent statistical significance at the 10%, 5% and 1% level, respectively.

Table 6, Other robustness checks

Dep. Var. (log FDI)	(1) Excl. Municipalities	(2) Excl. cites missing 1998 FDI	(3) Excl. cities missing 1995-1997 FDI
TCZ * Post	-0.277**	-0.251**	-0.185*
	(0.113)	(0.115)	(0.110)
College students (log)	0.055	0.068*	0.040
	(0.039)	(0.038)	(0.037)
High school students (log)	0.010	-0.074	-0.064
	(0.066)	(0.063)	(0.063)
GDP growth rate	0.055	0.067	0.058
	(0.037)	(0.045)	(0.043)
Telephone (log)	0.246**	0.172	0.165
	(0.108)	(0.116)	(0.118)
Road area per capita (log)	0.108*	0.168***	0.226***
	(0.061)	(0.060)	(0.057)
Industrial production (log)	0.186**	0.244***	0.300***
	(0.092)	(0.086)	(0.087)
Retail consumption (log)	0.285**	0.120	0.166
	(0.122)	(0.127)	(0.129)
City fixed effects	X	X	X
Province-Year effects	X	X	X
Observations	3,411	3,006	2,785
R-squared	0.627	0.649	0.668

Note: Standard errors, clustered at the city level, are reported in the parenthesis. *, ** and *** represent statistical significance at the 10%, 5% and 1% level, respectively.

Appendix Table 1, TCZ cities and their neighboring non-TCZ cities in China

Province	TCZ city	Neighboring non-TCZ cities
Beijing		Langfang
Tianjin		Langfang Cangzhou
Hebei	Shijiangzhuang	Jinzhong
	Tangshan	Qinhuangdao
	Handan	Liaocheng Changzhi Puyang
	Xingtai	Liaocheng Jinzhong
	Baoding	Langfang Cangzhou
	Zhangjiakou	Wulancabu
	Chengde	Caoyang Qinhuangdao
	Hengshui	Cangzhou
Shanxi	Taiyuan	Luliang Jinzhong
	Datong	Wulancabu
	Yangquan	Jinzhong
	Shuozhou	Wulanchabu
	Yuncheng	Jincheng
	Xinzhou	Luliang
	Linfen	Jincheng Changzhi
Inner Mongolia	Huhehaote	Wulancabu Eerduosi
	Baotou	Bayanzuoer Eerduosi
	Wuhai	Eerduosi
	Chifeng	Chaoyang Tongliao
Liaoning	Shenyang	Tieling
	Dalian	Yingkou Dandong
	Anshan	Panjin Yingkou Dandong
	Fushun	Tieling
	Benxi	Dandong
	Jinzhou	Panjin Chaoyang
	Fuxin	Tongliao Chaoyang
	Liaoyang	Dandong
	Huludao	Qinhuangdao Chaoyang
Jinlin	Jilin	Changchun Baishan
	Siping	Changchun Tieling Songyuan Tongliao
	Tonghua	Baishan
Shanghai		Yancheng
Jiangsu	Nanjing	Huaian Yancheng
	Wuxi	Huaian Yancheng
	Xuzhou	Linyi Lianyungang Suqian Suzhou Huaibei
	Changzhou	Chuzhou
	Suzhou	Huaian Yancheng
	Nantong	Yancheng
	Yangzhou	Huaian Yancheng Chuzhou

Province	TCZ city	Neighboring Non-TCZ cities
	Zhenjiang	Huaian
	Taizhou	Yancheng
Zhejiang	Hangzhou	Lishui Shangrao
	Ningbo	Zhoushan
	Wenzhou	Lishui Ningde
	Jiaxing	Zhoushan Lishui
	Huzhou	Zhoushan Chizhou
	Shaoxing	Lishui
	Jinhua	Lishui
	Quzhou	Lishui Shangrao
	Taizhou	Lishui
Anhui	Wuhu	Chizhou
	Manshan	Chuzhou
	Tongling	Anqing Chizhou
	Huangshan	Chizhou Jingdezhen Shangrao
	Xuancheng	Chizhou
Fujian	Fuzhou	Ningde Putian Nanping
	Xiamen	Putian
	Sanming	Nanping
	Quanzhou	Putian
	Zhangzhou	Meizhou
	Longyan	Meizhou
Jiangxi	Nanchang	Shangrao Yichun Fuzhou
	Pingxiang	Yichun
	Jiujiang	Huanggang Anqing Yichun Shangrao
	Yingtan	Shangrao Nanping Fuzhou
	Ganzhou	Heyuan
Shandong	Jinan	Liaocheng Binzhou
	Qingdao	Rizhao
	Zibo	Dongying Binzhou Linyi
	Zaozhuang	Linyi
	Yantai	Weihai
	Weifang	Rizhao Dongying
	Jining	Linyi Heze Puyang
	Taian	Linyi
	Laiwu	Linyi
	Dezhou	Liaocheng Cangzhou Binzhou
Henan	Zhengzhou	Kaifeng Xinxiang Xuchang
	Luoyang	Jincheng Nanyang Pingdingshan
	Anyang	Changzhi Xinxiang Hebi Puyang
	Jiaozuo	Xinxiang Jincheng

Appendix Table 1, TCZ cities and their neighboring non-TCZ cities in China

Province	TCZ city	Neighboring Non-TCZ cities
	Sanmenxia	Nanyang
Hubei	Wuhan	Huanggang Xiaogan
	Huangshi	Huanggang
	Yichang	Xiangfan
	Ezhou	Huanggang
	Jingmeng	Xiangfan Xiaogan Suizhou
	Jingzhou	Xiaogan
	Xianning	Huanggang
Hunan	Changsha	Yichun
	Zhuzhou	Yichun
	Xiangtan	Yichun
	Hengyang	Shaoyang Yongzhou
	Yueyang	Yichun
	Changde	Shaoyang
	Zhangjiajie	Shaoyang
	Yiyang	Shaoyang
	Chenzhou	Yongzhou
	Huaihua	Shaoyang
	Loudi	Shaoyang
Guangdong	Guangzhou	Heyuan
	Shaoguan	Heyuan
	Shenzhen	Heyuan
	Zhuhai	Yangjiang
	Shantou	Meizhou
	Foshan	Yangjiang
	Jiangmen	Yangjiang
	Zhanjiang	Maoming Beihai
	Zhaoqing	Yongzhou
	Huizhou	Heyuan
	Shanwei	Heyuan Meizhou
	Qingyuan	Yongzhou
	Dongguan	Heyuan
	Zhongshan	Yangjiang
	Chaozhou	Meizhou
	Jieyang	Meizhou
	Yunfu	Yangjiang Maoming
Guangxi	Nanning	Laibin Qinzhou Chongzuo
	Liuzhou	Laibin
	Guilin	Yongzhou
	Wuzhou	Laibin Guigang
	Guigang	Laibin
	Yulin	Maoming Beihai Qinzhou Guigang
	Hezhou	Yongzhou
	Hechi	Baise Laibin
Chongqing		Dazhou Guangan Ziyang
Sichuan	Chengdu	Yaan Ziyang
	Zigong	Laibin
	Panzhihua	Lijiang
	Luzhou	Ziyang
	Deyang	Ziyang
	Mianyang	Guangyuan Longlan
	Suining	Ziyang
	Neijiang	Ziyang
	Leshan	Yaan
	Nanchong	Guangyuan Bazhong Dazhou
	Yibin	Yaan Ziyang
	Guangan	Dazhou
	Meishan	Yaan Ziyang
Guizhou	Guiyang	Liupanshui
	Zunyi	Liupanshui
	Anshun	Liupanshui
Yunnan	Kunming	Simao Lincang
	Qujing	Liupanshui
	Yuxi	Simao Lincang
	Zhaotong	Lijiang Liupanshui
Shaanxi	Xian	Xianyang Baoji Ankang
	Tongchuan	Yanan Xianyang
	Weinan	Yanan Xianyang
	Shangluo	Ankang
Gansu	Lanzhou	Dingxi Wuwei
	Jinchang	Wuwei
	Baiyin	Wuwei Dingxi Guyuan Pingliang Zhongwei
	Zhangye	Jiuquan
Ningxia	Yinchuan	Wuzhong
	Shizuishan	Eerduosi
Xinjiang	Wulumuqi	Kelamayi

Appendix Table 2, Summary of time-variations of outcome variables used in the falsification tests during 1992-2009

Variable: CV	Buses	Bus passengers	Middle schools	Primary schools	Primary school students	Total road area
Mean	0.102	0.099	0.051	0.067	0.035	0.095
S. E.	0.004	0.005	0.003	0.003	0.002	0.003

Appendix Table 3, Instrumental variable estimation results

	1st stage	2nd stage
Dep. Var.	TCZ * Post	log FDI
TCZ * I [SO2>=60ug/m3]	0.353*	
	(0.188)	
TCZ * Post		-0.351
		(0.679)
College students (log)	-0.006	-0.048
	(0.021)	(0.082)
High school students (log)	0.001	0.112
	(0.029)	(0.103)
Telephones (log)	0.018	-0.002
	(0.075)	(0.209)
GDP growth rate	0.008	0.067
	(0.049)	(0.065)
Road area per capita (log)	-0.031	-0.057
	(0.027)	(0.115)
Industrial production (log)	0.034	0.034
	(0.061)	(0.186)
Retail consumption (log)	0.068	0.546***
	(0.050)	(0.186)
SO2	0.002	-0.000
	(0.001)	(0.003)
Under-identification test statistic	[5.69]**	
Weak identification test statistic	[3.52]*	
p-value for the Durbin-Wu-Hausman test	0.687	
City fixed effects	X	X
Province-Year effects	X	X
Observations	1121	1121
R-squared	0.927	0.704

Note: Standard errors are clustered at city level. *, **, and *** denote 10%, 5%, and 1% significance level, respectively.