Is There a Pollution Haven Effect? Evidence from a 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, being listed as a TCZ city causes the amount of FDI to drop by 41%. 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; Two control zones; Natural experiment
JEL Codes: R11; L25; D22

1 Introduction

Extreme weather prevails worldwide, causing not only tremendous economic losses but also significant human casualties. For example, in 2011 the U.S. had a record twelve weather disasters that cost more than $1 billion, according to the National Oceanic and Atmospheric Administration.[1] Environmental damage is widely blamed for such severe weather. Concerned about the further deterioration of living environments, governments across the world are strengthening 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. 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 the 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). Some 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). However, both the instrumental variable estimation and the propensity score matching method require strong identification assumptions. For the former, instrumental variables must be exogenous, whereas for the

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[1] For more information, see "2011 Breaks Record For Most Billion-Dollar Weather Disasters" by Eyder Peralta at *National Public Radio*, December 7, 2011.

[2] For example, Friedman, Gerlowski, and Silberman (1992); Levinson (1996); Becker and Henderson (2000); Eskeland and Harrison (2003); Javorcik and Wei (2004).

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

latter, both observables and unobservables must be matched.

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 (TCZ) policy in China, as an experiment (for details about environmental regulations in China, see Section 2).[4] 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 regulation attract less FDI, which confirms the pollution haven effect. Meanwhile, the magnitude of the pollution haven effect is found to be large: strengthening environmental regulations causes the amount of FDI to drop by around 41%.

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, including checking any differential pre-treatment time trends, including city-specific time trend, using two alternative control groups, controlling for provincial factors and spatial correlation, and conducting a placebo test, 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 largest FDI recipient country in the world. 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 across regions, which provides us with enough variations to identify the pollution haven effect.

Our study is similar to and complements the work of Hanna (2011), who also uses a DID analysis to investigate how tough environmental regulations in the U.S. affect its outflow FDI. Whereas Hanna (2011) looks 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

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[4] Hering and Poncet (2011) also use this setting to investigate how environmental regulations affect export activity of firms.

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 Institutional Background of Environmental Regulations in China

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 or 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 more stringent 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., 0.06 mg/m3) in recent years; (2) its daily average ambient SO2 concentrations exceeded the national Class III standard (i.e., 0.25 mg/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 cities, 175 cities 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 TCZ list, 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 concentration values in TCZ cities decreasing substantially over this period. By 2008, no city reported number above 0.1 mg/m3 and the SO2 emission in TCZ cities became similar to those in non-TCZ cities.

3 Estimation Strategy

In this section, we first lay out our estimation framework for the pollution haven effect and then discuss the various checks on our identification assumption.

3.1 Estimation Framework

To illustrate our identification strategy for the pollution haven effect, we adopt the Rubin causal model. Assume that for city c at time t we can observe two potential outcomes, $Y_{ct}(1)$ and $Y_{ct}(0)$, where Y_{ct} represents our outcome variable, the logarithm of the amount of FDI. $Y_{ct}(1)$ denotes the value when there is an extremely stringent environmental regulation and hence the value is determined by economic factors $\mathbf{X}_{ct}$ as well as city time-invariant factors (α_c) and yearly common shocks (λ_t), i.e.,

$$Y_{ct}(1) = \lambda_c + \lambda_t + \mathbf{X}'_{ct}\boldsymbol{\beta}. \tag{1}$$

$Y_{ct}(0)$ denotes the value when there is no environmental regulation, i.e.,

$$Y_{ct}(0) = Y_{ct}(1) + \gamma, \tag{2}$$

where $\gamma > 0$ captures the pollution haven effect; that is, the effect of environmental regulation on the location choice of FDI.

With these two outcome values ($Y_{ct}(1)$ and $Y_{ct}(0)$), we can readily calculate the pollution haven effect as

$$-\gamma = E\left[Y_{ct}(1) - Y_{ct}(0)\right]. \tag{3}$$

However, in observational data like ours, we are only able to observe one of the two potential outcome values: either $Y_{ct}(1)$ or $Y_{ct}(0)$. This makes identification of the pollution haven effect through equation (3) infeasible. To retrieve the pollution haven effect, we exploit the TCZ policy that was put into effect in 1998 in China as a 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). Denote the indicator of the treatment status TCZ_c as

$$TCZ_c = \begin{cases} 1 & \text{if city } c \text{ is a TCZ city} \\ 0 & \text{if city } c \text{ is a non-TCZ city} \end{cases}. \tag{4}$$

Our DID estimator is

$$\begin{aligned} -\gamma_{\mathbf{DID}} &= E\left[\Delta Y_{c1998} \middle| TCZ_c = 1\right] - E\left[\Delta Y_{c1998} \middle| TCZ_c = 0\right] \\ &= E\left[Y_{c1998}(1) - Y_{c1998}(0) \middle| TCZ_c = 1\right] \\ &\quad + \left(E\left[\Delta Y_{c1998}(0) \middle| TCZ_c = 1\right] - E\left[\Delta Y_{c1998}(0) \middle| TCZ_c = 0\right]\right) \\ &= -\gamma + IA \end{aligned} \tag{5}$$

where

$$IA = E\left[\Delta Y_{c1998}(0) \middle| TCZ_c = 1\right] - E\left[\Delta Y_{c1998}(0) \middle| TCZ_c = 0\right] \tag{6}$$

Equation (6) represents our identification assumption, which states 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. As long as our identification assumption is satisfied (i.e., $IA = 0$), our DID estimator recovers the true pollution haven effect, i.e., $\gamma_{\mathbf{DID}} = \gamma$.[5]

In regression form, our baseline DID estimation has the following specification

$$Y_{ct} = \alpha_c + \lambda_t + \gamma \cdot TCZ_c \times Post_t + \mathbf{X}'_{ct}\boldsymbol{\beta} + \varepsilon_{ct}, \tag{7}$$

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[5] Note that for our DID analysis, we do not require the treatment status to be exogenous, i.e.,

$$E\left[Y_{c1997}(0) \middle| TCZ_c = 1\right] \neq E\left[Y_{c1997}(0) \middle| TCZ_c = 0\right].$$

where λ_t is the time dummy, capturing those factors common to all cities at time t; α_c is the city dummy, capturing city c's all time-invariant characteristics; $Post_c$ indicates the post-treatment period, i.e.,

$$Post_c = \begin{cases} 1 & \forall t \geq 1998 \\ 0 & \text{otherwise} \end{cases}; \tag{8}$$

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).

3.2 Checks on the Identification Assumption

Our identification assumption in regression form (corresponding to equation (6)) is

$$\begin{aligned} & E\left[\Delta\varepsilon_{c1998} \middle| TCZ_c = 1, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}\right] \\ = \; & E\left[\Delta\varepsilon_{c1998} \middle| TCZ_c = 0, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}\right]. \end{aligned} \tag{9}$$

It is reasonable to believe that this identification assumption holds in our setting, because the initiation of the TCZ policy was exogenous to local governments. Meanwhile, the designation of TCZ cities was based on several criteria, in particular past ambient SO2 concentration values and the PH of precipitation, and specific threshold levels, all of which could not be manipulated by city governments. Nonetheless, we discuss in the following a series of robustness checks on the identification assumption (9).

Pre-treatment differential time trends. One way to check whether the identification assumption (9) holds is to examine whether the assumption is satisfied several years before the treatment, i.e.,

$$\begin{aligned} IA_s &= E\left[\Delta\varepsilon_{c1998-s} \middle| TCZ_c = 1, \Delta\mathbf{X}_{c1998-s}, \Delta\lambda_{1998-s}\right] \\ &\quad - E\left[\Delta\varepsilon_{c1998-s} \middle| TCZ_c = 0, \Delta\mathbf{X}_{c1998-s}, \Delta\lambda_{1998-s}\right] \\ &= 0 \; \forall s \geq 1. \end{aligned} \tag{10}$$

A finding of $IA_s = 0 \; \forall s \geq 1$ may imply that our identification assumption (9) continues to hold. The corresponding regression specification is

$$\begin{aligned} Y_{ct} &= \alpha_c + \lambda_t + \gamma \cdot TCZ_c \times Post_t \\ &\quad + \sum\nolimits_{s \geq 1} \delta_s \cdot TCZ_c \times \lambda_{1998-s} + \mathbf{X}'_{ct}\boldsymbol{\beta} + \varepsilon_{ct}, \end{aligned} \tag{11}$$

and the test of $\delta_s = 0 \forall s \geq 1$ corresponds to the check of $IA_s = 0 \; \forall s \geq 1$.

City-specific time trend. Cities in the treatment and control groups may follow different time trends, which may then compound our DID estimate. To address this concern, we allow for city-specific time trend in our DID estimation. Specifically, the first-differenced error term in estimation equation (7) is decomposed as

$$\Delta\varepsilon_{ct} = \lambda_c + \Delta\tilde{\varepsilon}_{ct}, \tag{12}$$

and our identification assumption (9) is relaxed as

$$\begin{aligned} & E\left[\Delta\tilde{\varepsilon}_{c1998}\right| TCZ_c = 1, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}, \lambda_c] \\ = \; & E\left[\Delta\tilde{\varepsilon}_{c1998}\right| TCZ_c = 1, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}, \lambda_c]. \end{aligned} \tag{13}$$

The corresponding new regression specification is

$$Y_{ct} \;=\; \alpha_c + \lambda_t + \gamma \cdot TCZ_c \times Post_t + \mathbf{X}'_{ct}\boldsymbol{\beta} + \lambda_c \times t + \tilde{\varepsilon}_{ct}. \tag{14}$$

Matched control group. Instead of using arbitrary, non-TCZ cities as the control group, we match each city in the treatment group with a city in the control group based on a number of average pre-treatment city characteristics $\mathbf{W}_{c1992-1997}$, following List, Millimet, Fredriksson, and McHone (2003) and Dean, Lovely, and Wang (2009). Specifically, we first estimate a Probit regression, i.e.,

$$\rho_c = \Pr\left(TCZ_c = 1 \middle| \mathbf{W}_{c1992-1997}\right) = F\left(\mathbf{W}_{c1992-1997}\right). \tag{15}$$

Based on the predicted probability $\hat{\rho}_c$, we then match each TCZ city with a non-TCZ city that has the closest value of $\hat{\rho}_c$ compared to the concerned TCZ city. Using this matched control group, we relax our identification assumption (9) as

$$\begin{aligned} & E\left[\Delta\varepsilon_{c1998}\right| TCZ_c = 1, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}, \hat{\rho}_c] \\ = \; & E\left[\Delta\varepsilon_{c1998}\right| TCZ_c = 1, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}, \hat{\rho}_c]. \end{aligned} \tag{16}$$

Meanwhile, if conditional on $\mathbf{X}_{ct}$ our treatment and control groups in the baseline estimation (7) are already balanced, then the use of this matched control group should barely change the statistical significance and the magnitude of our baseline DID estimate, i.e., $\gamma_{\mathbf{Matched}} = \gamma_{\mathbf{Baseline}}$.

Surrounding non-TCZ cities as the control group. We construct another alternative control group, specifically, all of the non-TCZ cities that surround a TCZ city. Because neighboring cities tend to have similar economic, social, and climate conditions, the use of this alternative control group may improve the comparability between the treatment and control groups. Meanwhile, we also compare the estimated magnitude from this alternative control group to that of the baseline estimator as a check on whether the treatment and control groups are indeed balanced in the baseline estimation, or the satisfaction of the identification assumption (9).

Provincial factors and spatial correlation. Chinese provinces usually have different regional policies and guidelines for policy enforcement that could potentially bias our estimate. To address this concern, we allow for any arbitrary (time-varying or time-invariant) provincial compounding factors by including province-time dummies. Meanwhile, the inclusion of province-time dummies provides us with a control for the spatial correlation issues pointed out by Drukker and Millimet (2008). The estimation specification with the inclusion of the province-time dummies is

$$Y_{ct} = \alpha_c + \lambda_t + \gamma \cdot TCZ_c \times Post_t + \lambda_{pt} + \mathbf{X}'_{ct}\boldsymbol{\beta} + \varepsilon_{ct}, \tag{17}$$

where λ_{pt} is the province-time dummy, capturing all provincial time-invariant and time-varying characteristics, and the corresponding identification assumption is

$$\begin{aligned} & E\left[\Delta\varepsilon_{c1998} \middle| TCZ_c = 1, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}, \Delta\lambda_{p1998}\right] \\ = \; & E\left[\Delta\varepsilon_{c1998} \middle| TCZ_c = 1, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}, \Delta\lambda_{p1998}\right]. \end{aligned} \tag{18}$$

Placebo test: an artificial date of treatment. The NEPB began compiling the TCZ list in late 1995; hence, introducing concerns about 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 1996 instead of 1998 as the time of treatment. Hence, our new DID estimator is

$$-\tilde{\gamma}_{\mathbf{DID}} = E\left[\Delta Y_{c1996} \middle| TCZ_c = 1\right] - E\left[\Delta Y_{c1996} \middle| TCZ_c = 0\right]. \tag{19}$$

A finding of $\tilde{\gamma}_{\mathbf{DID}} = 0$ may not only dismiss concerns of an expectation effect, but also show that the treatment and control groups followed similar time trends before the policy change in 1998.

Falsification tests. 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 regulations. Hence, the DID estimator of Z^k_{ct}

is

$$\begin{aligned} -\gamma^{z^k}_{\mathbf{DID}} &= E\left[\Delta Z^k_{c1998}\middle| TCZ_c = 1\right] - E\left[\Delta Z^k_{c1998}\middle| TCZ_c = 0\right] \\ &= -\gamma^{z^k} + IA^{z^k} = IA^{z^k}, \end{aligned} \tag{20}$$

where $\gamma^{z^k} = 0$; and

$$IA^{z^k} = E\left[\Delta\varepsilon^{z^k}_{c1998}\middle| TCZ_c = 1\right] - E\left[\Delta\varepsilon^{z^k}_{c1998}\middle| TCZ_c = 0\right]. \tag{21}$$

A finding of $\gamma^{z^k}_{\mathbf{DID}} = 0\forall k$ means that the treatment and control groups are balanced for these alternative outcome variables Z^k_{ct} (i.e., $IA^{z^k} = 0\forall k$), which may imply the satisfaction of our baseline identification assumption (9). To choose these alternative outcome variables $\mathbf{Z}_{ct}$, we use the number of buses, the number of bus passengers, the number of middle schools, the number of primary schools, the primary school enrolment numbers, and road area, all in logarithm form.

Instrumental variable estimation. The TCZ assignment was based on the criteria listed in Section 2, which creates a discontinuity in the assignment variable. By exploring such discontinuity, we can construct a possibly exogenous instrument for TCZ status. Specifically, the instrumental variable is constructed as

$$TCZ_{IV} = I\left[M_{c95} \geq m_0\right], \tag{22}$$

where $I\left[.\right]$ is an indicator function that takes a value of 1 if the argument in the bracket is true and 0 if false; M_{c95} is the average annual ambient SO2 concentration in 1995 for the northern cities and the average PH value of precipitation in 1995 for the southern cities;[6] and m_0 is 0.06mg/m^3 for the northern cities and 4.5 for the southern cities. The first-stage of the instrumental variable estimation is

$$\begin{aligned} TCZ_c \times Post_t &= \alpha_c + \lambda_t + \pi \cdot TCZ_{IV} \times Post_t \\ &\quad +\phi \cdot M_{c95} \times Post_t + \mathbf{X}'_{ct}\boldsymbol{\beta} + \upsilon_{ct}, \end{aligned} \tag{23}$$

and the second-stage is

$$Y_{ct} = \alpha_c + \lambda_t + \gamma \cdot TCZ_c \times Post_t + \psi \cdot M_{c95} \times Post_t + \mathbf{X}'_{ct}\boldsymbol{\beta} + \varepsilon_{ct}. \tag{24}$$

The inclusion of $M_{c95} \times Post_t$ suggests that the identification of the instrumental variable estimation comes from discontinuity in the distribution of

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[6] Using an average of 1994-1995 or an average of 1993-1995 as the assignment value produces similar results (available upon request).

the assignment variable M_{c95}, i.e., the identification assumption of the instrumental variable estimation is

$$\begin{aligned} & E\left[\Delta\varepsilon_{c1998}\right| I\left[M_{c95} \geq m_0\right] = 1, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}, M_{c95}] \\ = \; & E\left[\Delta\varepsilon_{c1998}\right| I\left[M_{c95} \geq m_0\right] = 0, \Delta\mathbf{X}_{c1998}, \Delta\lambda_{1998}, M_{c95}]\,. \end{aligned} \tag{25}$$

It is reasonable to believe that the identification assumption (25) is satisfied, because the assignment was based on past pollutant emission values and specific threshold levels. However, the implementation of this instrumental variable estimation faces two data challenges. First, we do not have information about the PH values of precipitation. 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. This severe sample attrition is expected to substantially increase the standard error and hence reduce the statistical significance of our estimated coefficient. As a result, we also report the Dubin-Wu-Hausman test, which checks the statistical equivalence between our DID and instrumented DID estimates, or the equivalence of identification assumptions (9) and (25).

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"

From the first data source, we collect information about our outcome variable, the amount of FDI, for each city during the 1992-2009 period. The first data source also provides information about our control variables $\mathbf{X}_{ct}$, including the number of college students, the number of high school students, the number of telephones, GDP, the number of taxis, population, the number of road areas, industrial production, and the number of retail consumptions. To construct the matched control group, we further collect information about total wages and tax revenue. For a detailed description of these variables, see Appendix 1.

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 TCZ. During our sample period (1992-2009), the composition of this list remained unchanged. Appendix 2 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 the time trends of the logarithm of the total amount of FDI 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. More interestingly, before 1998 (the time of the TCZ policy became effective), TCZ and non-TCZ cities had similar time trends, except for a sudden drop in 1997 for 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).

5 Empirical Findings

5.1 Baseline Result

Our baseline DID estimation results corresponding to equation (7) are reported in Table 1. The DID estimator $\gamma_{\mathbf{DID}}$ (i.e., the estimated coefficient of the interaction between the indicator of the treatment status TCZ and that of the post-treatment period $Post_{it}$) is found to be negative and statistically significant. This result implies that cities with tougher environmental regulations (i.e., the TCZ policy) attract fewer FDI, confirming the pollution haven effect.

Meanwhile, the economic magnitude of the pollution haven effect is also significant. The implementation of the TCZ policy causes the amount of FDI to drop by 41.1%. This magnitude is larger than those found in the literature. For example, Henderson (1996) finds a magnitude of $7-10\%$ in the context of 742 urban counties in the U.S. for the 1978-1987 period. Kellenberg (2009) estimates that during 1999-2003, the failing environmental policy causes 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 transitional economies was 32%. Hanna (2011) finds that the Clean Air Act Amendments over the 1966-1999 period increases U.S. multinationals' foreign assets by 5.3% and foreign output by 9%.

The estimated coefficients of other economic determinants of FDI also make economic sense. Better telecommunication infrastructure (i.e., the number of telephones) attracts foreign investment and cities with more domestic production accommodate more FDI, which supports the agglomeration theories. Moreover, foreign firms are more likely to locate in cities with larger domestic consumption.

5.2 Checks on the Identification Assumption of the DID Estimation

Whether our DID estimator in Table 2 captures the true pollution haven effect hinges on the satisfaction of our identification assumption (9), i.e., $IA = 0 \Leftrightarrow \gamma_{\mathbf{DID}} = \gamma$. In this sub-section, we present the results of a series of robustness checks, as illustrated in Section 3.2, on the identification assumption of our DID estimation.

First, Column 1 of Table 2 reports the estimation results regarding the check on any differential pre-treatment time trends according to equation (11). 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 implies that the treatment group may follow the same trend as the control group in the case of no treatment or the satisfaction of our identification assumption (9).

Second, we include city-specific time trend in Column 2 of Table 2. 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 2.

Third, we use the propensity score matching method to match each TCZ city with a non-TCZ city. Specifically, for the matching covariates, we follow List, Millimet, Fredriksson, and McHone (2003) and Dean, Lovely, and Wang (2009) by using total wage, population, GDP, the number of college students, the number of high school students, the number of telephones, road area per capita, tax revenue, and industrial production. The balancing tests reported in Appendix 3 show that after the matching, the treatment and control groups are balanced in all of these covariates. Estimation results using the matched control group are reported in Column 3 of Table 2; such that the estimator remains statistically significant. Meanwhile, although the estimated magnitude falls to -0.343, the Hausman test shows that it is statistically indifferent from the baseline DID estimate (-0.411 in Table 1). These results imply that the treatment and control groups are balanced in the baseline DID estimation, which lends support to the satisfaction of our identification assumption (9).

Fourth, we use the non-TCZ cities that surround each TCZ city as an alternative control group. Appendix 2 reports this list for each of the TCZ cities. Estimation results are reported in Column 4 of Table 2. It is found that the new DID estimate resembles the baseline DID estimate in Table 1, in both statistical significance and magnitude. These results further verify the use of the control group in the baseline DID estimation or the satisfaction of our identification assumption (9).

Fifth, we include province-time dummies in Column 5 of Table 2 to control for any arbitrary provincial time-varying and time-invariant compounding factors and spatial correlation. Clearly, our findings on the pollution haven effect remain robust to the inclusion of province-time dummies.

Sixth, 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 6 of Table 2), which reinforces the validity of our DID estimation.

Seventh, in Table 3, we report a series of falsification tests, in which we replace our outcome variable of interest (the amount of FDI) with seven other outcome variables that are not supposed to be affected by the change in environmental regulations. The estimation results show that none of these seven DID estimates produce any statistical significance and many of the estimated magnitudes are quite close to zero. The finding that our identification assumption (9) holds for these seven alternative outcome variables supports the validity of our DID estimation.[7]

Finally, Table 4 reports the instrumental variable estimation results corresponding to equations (23) and (24). 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 almost identical to our baseline DID estimate. However, as expected, due to the severe sample attrition problem, the standard error of the estimated instrumented DID estimate is quite large.[8] Nonetheless, the insignificant Dubin-Wu-Hausman test show that the instrumented DID estimate is similar to the baseline DID estimate, which implies the satisfaction of our identification assumption (9), given that the IV identification assumption (25) holds.

5.3 Other Robustness Checks

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

First, we experiment with using 1992-1995 instead of 1992-1997 as the pre-treatment period, due to concerns about the noise introduced by the preparation of the TCZ list in the 1995-1997 period. Estimation results are reported in Column 1 of Table 5. Clearly, our main findings on the pollution haven effect remain robust to the use of this alternative pre-treatment period.

Second, 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 2 of Table 5. It is found that our DID estimate barely changes with the exclusion of these four municipalities.

Third, we exclude cities without information about the amount of FDI in 1998 because they do not have post-treatment values. Estimation results are reported in Column 3 of Table 5. The new estimator becomes even more statistically significant, which further confirms our previous findings.

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[7] One may be concerned that the statistical insignifiance is due to the lack of time variations for these seven outcome variables. In Appendix 4, for each of these seven outcome variables, we report the mean value and standard deviation of the coefficient of variation (defined as the standard deviation of the outcome variable for an individual city over time divided by the corresponding mean value), which is a standard measure of the degree of dispersion in the literature. We find significant time variations in these outome variables.

[8] 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).

Finally, 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 4 of Table 5, our main findings on the pollution haven effect continue to hold in this sub-sample.

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.

By using the amount of FDI for 280 cities over the 1992-2009 period, we find that cities designated as TCZ attract around 41% 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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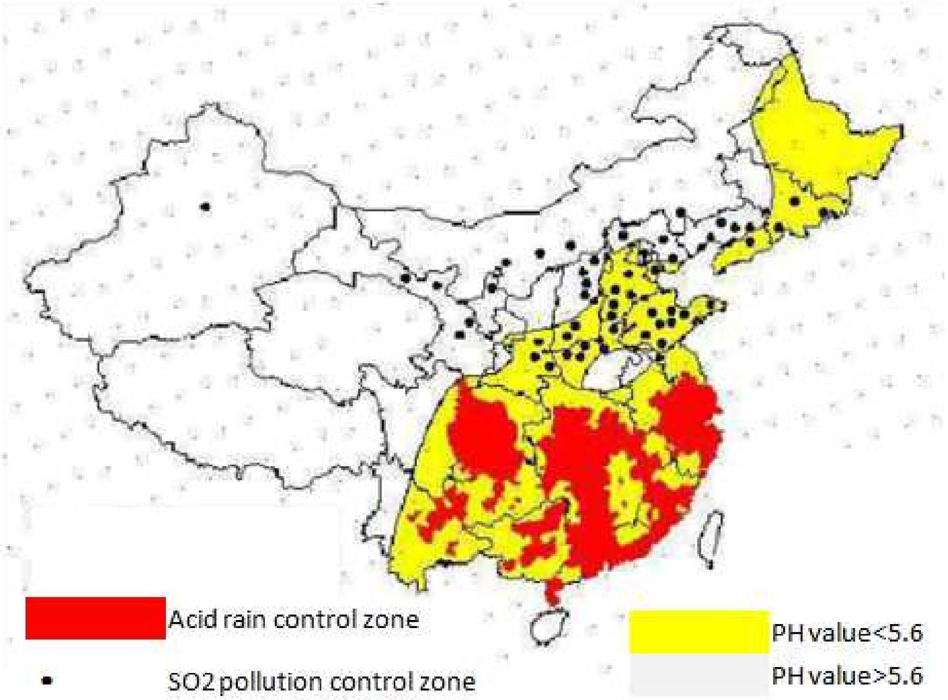

(Resource: The national Environmental Protection Bureau, “The Proposal of Designation for Acid Rain Control Areas and SO2 Pollution Control Areas”)

Figure 1: Distribution of TCZ cities

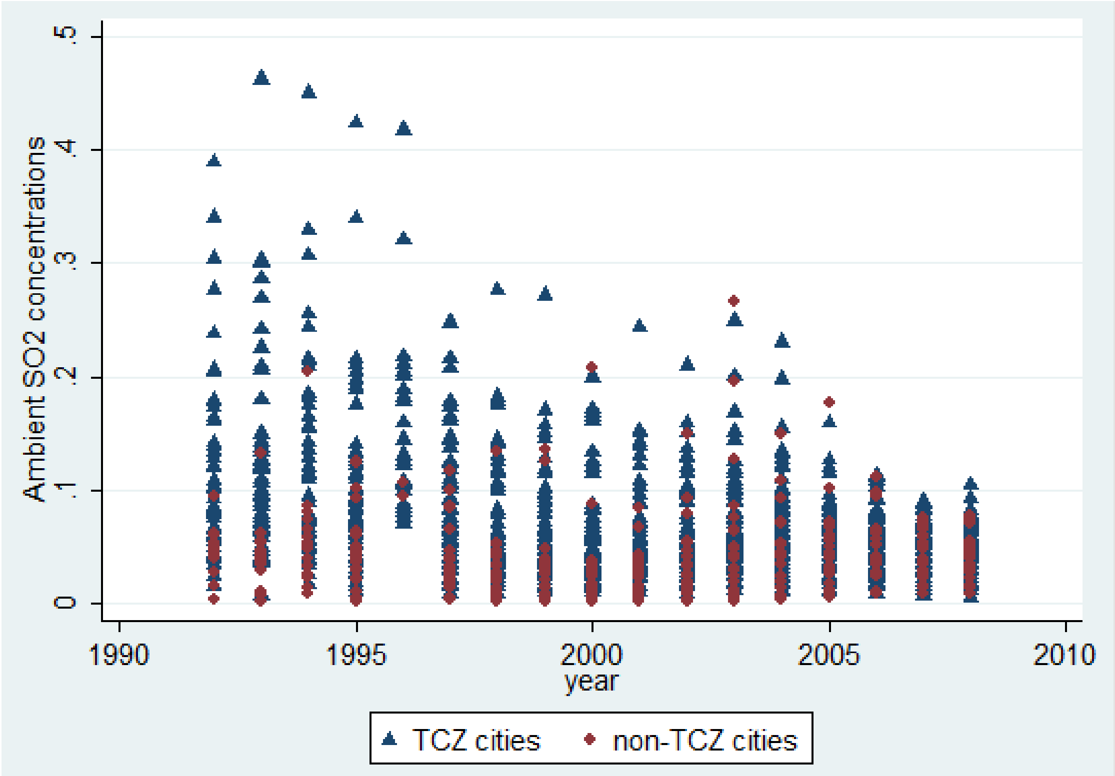

Figure 2: Ambient SO2 concentrations in TCZ cities and non-TCZ cities

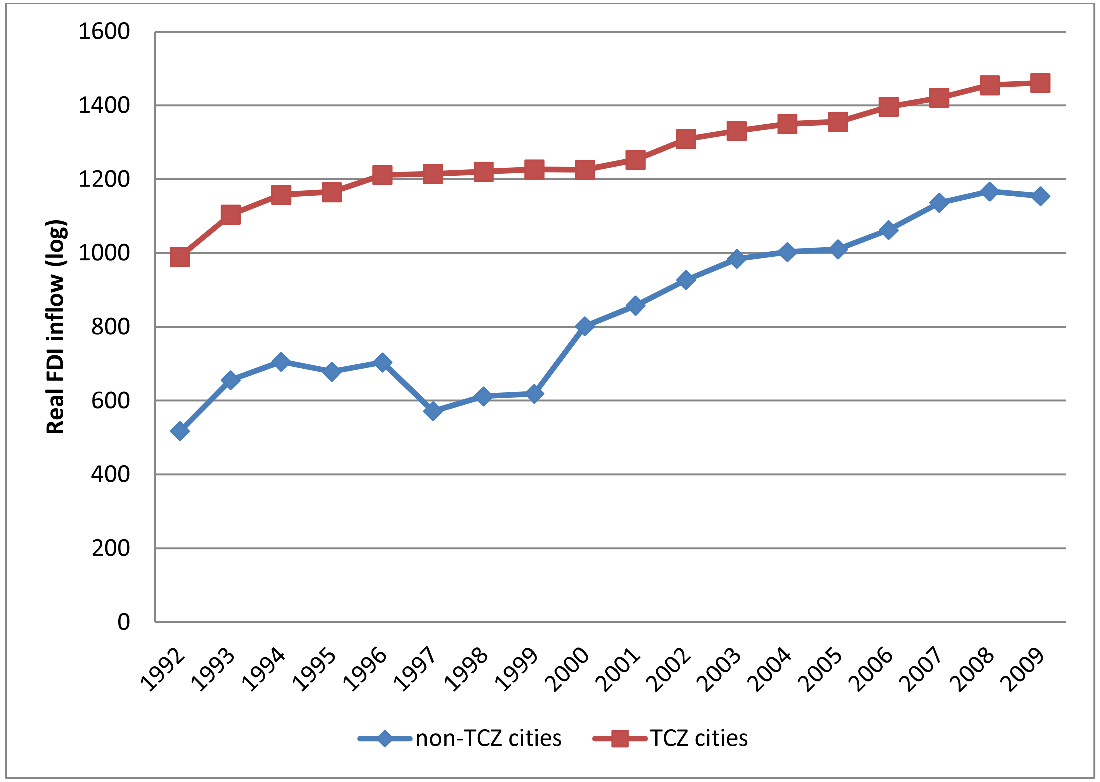

Figure 3: Total FDI inflow into TCZ cities and non-TCZ cities

Table 1, Baseline results

Dependent variable: log FDI	(1)
TCZ * Post	-0.411**
	(0.168)
College students # (log)	0.084
	(0.067)
High school students # (log)	-0.085
	(0.095)
Telephone # (log)	0.308**
	(0.138)
GDP growth rate	-0.396
	(0.254)
Taxi # (log)	-0.052
	(0.070)
Road area per capita (log)	0.066
	(0.099)
Industrial production (log)	0.366***
	(0.105)
Retail consumption (log)	0.286*
	(0.160)
Constant	-1.352
	(2.039)
Year fixed effects	Yes
City fixed effects	Yes
Observations	3,013
R-squared	0.399

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 2, Checks on the identification assumption of the DID estimation

Dep. Var: log FDI	(1) Incl. Pre-treatment	(2) Incl. City-time trend	(3) Matched control group	(4) Neighboring non-TCZs as control group	(5) Incl. Province-time dummies	(6) Use 1996 as event date
TCZ * Post	-0.301*	-0.460*	-0.353**	-0.434***	-0.305**	-0.246
	(0.168)	(0.261)	(0.157)	(0.129)	(0.138)	(0.158)
TCZ * Prior1	0.045					
	(0.275)					
TCZ * Prior2	0.383	-0.301*				
	(0.260)	(0.168)				
College students # (log)	0.085	0.026	0.131	0.090	0.066	0.083
	(0.067)	(0.078)	(0.095)	(0.061)	(0.062)	(0.067)
High school students # (log)	-0.083	-0.192	-0.204**	-0.089	0.040	-0.080
	(0.095)	(0.120)	(0.099)	(0.093)	(0.102)	(0.096)
Telephone # (log)	0.312**	0.000	0.416**	0.541***	0.328**	0.326**
	(0.136)	(0.000)	(0.188)	(0.118)	(0.132)	(0.138)
GDP growth rate	-0.388	0.452**	0.424	-0.366	-0.387	-0.371
	(0.255)	(0.195)	(0.342)	(0.224)	(0.237)	(0.260)
Taxi # (log)	-0.051	-0.562	-0.092	-0.091*	-0.008	-0.032
	(0.070)	(0.527)	(0.078)	(0.053)	(0.057)	(0.068)
Road area per capita (log)	0.068	-0.061	0.207*	0.052	0.018	0.062
	(0.098)	(0.080)	(0.105)	(0.080)	(0.080)	(0.098)
Industrial production (log)	0.367***	-0.039	0.287**	0.439***	0.233*	0.347***
	(0.104)	(0.105)	(0.126)	(0.074)	(0.128)	(0.106)
Retail consumption (log)	0.283*	0.505**	0.278	0.248**	0.210	0.297*
	(0.159)	(0.207)	(0.214)	(0.116)	(0.154)	(0.161)
Constant	-1.602	0.104	0.400	-2.579	1.279	-1.314
	(2.063)	(0.117)	(2.212)	(1.706)	(2.348)	(2.078)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,013	2,413	1,481	4,486	3,013	3,013
R-squared	0.400	0.038	0.473	0.447	0.617	0.397

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 3, Falsification tests

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var: log FDI	Bus #	Bus passenger #	Middle school #	Primary school #	Primary school student #	Total road area
TCZ * Post	-0.109	0.081	0.014	-0.075	0.022	0.047
	(0.100)	(0.135)	(0.053)	(0.111)	(0.044)	(0.064)
College students # (log)	0.055*	0.087*	0.021*	-0.034	-0.001	0.029
	(0.029)	(0.048)	(0.011)	(0.032)	(0.015)	(0.021)
High school students # (log)	-0.080*	0.153**	0.501***	0.698***	0.803***	0.021
	(0.046)	(0.068)	(0.064)	(0.143)	(0.051)	(0.028)
Telephone # (log)	0.038	0.059	0.091**	-0.019	0.068*	-0.042
	(0.050)	(0.076)	(0.040)	(0.049)	(0.040)	(0.037)
GDP growth rate	-0.248**	-0.127	0.008	-0.057	0.026	-0.034
	(0.110)	(0.138)	(0.040)	(0.084)	(0.042)	(0.041)
Taxi # (log)	0.152***	0.179***	-0.014	-0.019	-0.001	0.023
	(0.044)	(0.053)	(0.013)	(0.031)	(0.017)	(0.029)
Road area per capita (log)	0.022	0.087	0.009	-0.056	-0.058***	0.733***
	(0.039)	(0.064)	(0.013)	(0.034)	(0.021)	(0.045)
Industrial production (log)	0.068	0.045	0.002	0.031	0.126***	0.041
	(0.049)	(0.068)	(0.017)	(0.040)	(0.032)	(0.025)
Retail consumption (log)	0.144*	0.184**	-0.017	-0.090*	0.174***	0.097***
	(0.077)	(0.082)	(0.030)	(0.052)	(0.047)	(0.037)
Constant	1.842*	0.143	-1.186	-0.515	-1.229	2.234***
	(1.033)	(1.184)	(0.913)	(1.770)	(0.809)	(0.616)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,080	2,887	2,744	2,744	3,063	3,086
R-squared	0.574	0.485	0.719	0.620	0.803	0.857

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, Instrumental variable estimation result

	1st stage	2nd stage
Dep. VARIABLES	TCZ * Post	
TCZ * Post_IV	0.355***	
	(0.136)	
TCZ * Post		-0.372
		(0.872)
College students # (log)	-0.014	-0.123
	(0.022)	(0.144)
High school students # (log)	0.022	-0.029
	(0.028)	(0.159)
Telephone # (log)	-0.079	0.209
	(0.046)	(0.257)
GDP growth rate	-0.011	0.012
	(0.078)	(0.466)
Taxi # (log)	-0.041	0.042
	(0.031)	(0.138)
Road area per capita (log)	0.018	0.024
	(0.016)	(0.230)
Industrial production (log)	0.012	0.372*
	(0.030)	(0.203)
Retail consumption (log)	-0.017	0.715***
	(0.030)	(0.260)
so2_939495_post	0.001	0.001
	(0.001)	(0.002)
Under-identification test statistic	[7.07]***	
Weak identification test statistic	[6.80]***	
p-value for the Durbin-Wu-Hausman test	0.691	
Year fixed effects	Yes	Yes
City fixed effects	Yes	Yes
Observations	896	896
R-squared	0.841	0.366

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

Table 5, Other robustness checks

Dep. Var: log FDI	(1) Use 92-95 as pre-treatment period	(2) Excl. Municipalities	(3) Excl. cites missing 1998 FDI	(4) Excl. cities missing 1995-1997 FDI
TCZ * Post	-0.414**	-0.397***	-0.343**	-0.295*
	(0.168)	(0.146)	(0.142)	(0.171)
College students # (log)	0.080	0.101	0.126*	0.086
	(0.067)	(0.065)	(0.070)	(0.067)
High school students # (log)	-0.089	-0.167**	-0.159*	-0.073
	(0.095)	(0.085)	(0.083)	(0.091)
Telephone # (log)	0.308**	0.235	0.135	0.312**
	(0.139)	(0.155)	(0.155)	(0.129)
GDP growth rate	-0.406	-0.152	0.189	-0.441
	(0.254)	(0.303)	(0.277)	(0.281)
Taxi # (log)	-0.051	-0.085	-0.056	-0.048
	(0.070)	(0.068)	(0.065)	(0.077)
Road area per capita (log)	0.065	0.197*	0.378***	0.063
	(0.100)	(0.109)	(0.086)	(0.102)
Industrial production (log)	0.368***	0.346***	0.272**	0.341***
	(0.105)	(0.102)	(0.107)	(0.102)
Retail consumption (log)	0.286*	0.204	0.274	0.293*
	(0.160)	(0.170)	(0.179)	(0.159)
Constant	-1.332	1.260	1.311	-1.274
	(2.043)	(1.843)	(2.375)	(2.120)
Year fixed effects	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes
Observations	2,970	2,501	2,329	2,849
R-squared	0.398	0.406	0.429	0.402

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 1, Description of variables

Variable name	Description
FDI (log)	Actual FDI received in each year (10,000 USD)
TCZ* post	TCZ=1 if the city is designated to be TCZ, 0 otherwise; post=1 if year is 1998 or afterwards, 0 otherwise
College students (log)	Number of college students
High school students (log)	Number of high school students
Telephone (log)	The number of telephone owned by every 10000 households
GDP growth rate	The growth rate of GDP
Taxi (log)	Number of taxi
Road area per capita (log)	Average paved road (square meters)
Industrial production (log)	Industrial output (10,000 CNY)
Total wage(log)	Total wages of staff and workers (10,000 CNY)
Population (log)	Total wage (10,000 persons)
Tax revenue (log)	Value-added tax (10,000 CNY)
Retail consumption (log)	Total retail sales of social consumption goods (10,000 CNY)

Appendix 2, 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
	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 2, TCZ cities and their neighboring non-TCZ cities in China (Cont.)

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 3, Balancing test for the matching

Matching covariates	Sample	Mean Treated	Control	%bias	%reduction |bias|	t-test t	p>|t|
Total wage (log)	Unmatched	12.435	12.11	40		2.57	0.011
	Matched	12.22	12.11	13.4	66.3	0.82	0.415
Population (log)	Unmatched	5.7061	5.571	18.4		1.21	0.227
	Matched	5.629	5.571	7.9	57.2	0.44	0.664
GDP (log)	Unmatched	14.526	14.18	42.6		2.74	0.007
	Matched	14.276	14.18	12	71.8	0.71	0.48
College students (log)	Unmatched	8.7284	8.33	29.6		1.88	0.062
	Matched	8.5188	8.33	14	52.6	0.79	0.433
High school students (log)	Unmatched	12.075	12.01	9.9		0.64	0.521
	Matched	12.069	12.01	9	9.7	0.49	0.623
Telephone (log)	Unmatched	3.0911	2.743	39.7		2.5	0.013
	Matched	2.8524	2.743	12.5	68.5	0.78	0.435
Road area per capita (log)	Unmatched	1.3095	1.344	-5.9		-0.38	0.702
	Matched	1.3797	1.344	6.1	-3	0.37	0.713
Tax revenue (log)	Unmatched	11.285	10.81	49.3		3.2	0.002
	Matched	10.94	10.81	13.4	72.9	0.8	0.423
Industrial production (log)	Unmatched	14.56	14.06	51.2		3.26	0.001
	Matched	14.187	14.06	12.7	75.2	0.78	0.436

Note: One-to-one matching is used to construct treatment-control pairs. Matching is based on the characteristics of each city prior to 1998 (average in 1992-97).

Appendix 4, Summary of time-variations of outcome variables used in the falsification tests during 1992-2009 (C.V.)

Variable: coefficient of variation	Bus #	Bus passenger #	Middle school #	Primary school #	Primary school stu #	Road area
Mean	0.102	0.099	0.051	0.067	0.035	0.095
S. E. of mean	0.004	0.005	0.003	0.003	0.002	0.003