# Playing Favorites: Tax Incentives and Urban Growth in China, 1978–2010

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ABSTRACT. *This study examines whether locationally preferential tax incentives for firms encourage differential urban expansion in China. Using satellite-based data from 1978 to 2009, we find that zones established early tend to have large and persistent effects, and that provincially determined zones have little impact relative to those designated by national authorities. Zones established later tend only to have effects where counties already had an export-advantageous position. We conclude that such policies can encourage differential growth in the presence of significant distortions in the rest of the economy, but become less effective as these distortions are removed.* (JEL O18, R58)

## I. INTRODUCTION

In pursuit of economic growth, policy makers often adopt strategies that preference particular cities or regions. Determining the effectiveness of such policies in encouraging differential growth is essential to understanding whether governments can use these tools to mitigate regional income inequality. This paper seeks to determine the impact of region-specific investment policies on urban expansion and, by extension, on growth. In particular, we examine whether a series of "economic zones" established by the Chinese government between 1980 and 2000 resulted in changes in the urban land use trajectories of the targeted cities.

Since economic reforms were put in place across China in the late 1970s, the country's rapid economic development has been accompanied by an equally remarkable spike in urbanization—both the movement of population into cities and the expansion of urban land. The number of cities increased from 193 in 1978 to 667 in 2009. At the same time, urban population has grown from 17.9% to 46.6% of total population (China Statistical Bureau 2009). A key contribution of this paper is to disentangle the impact of city-specific policies from the overall trajectory of urbanization within China.

In order to accomplish this, our methodology takes advantage of the temporal and spatial distribution of these zones by examining the growth of urban land use measured by satellite images at eight intervals from 1978 to 2009. Economic zoning in China includes both permission to invest and tax incentives for foreign firms to locate in specific counties. We use both counties that never received zoning and counties that received zoning quite late as counterfactuals for early-zoned counties. In a first step, we match early zone recipients to similar counties out of this "control" group. Using this subsample, we then estimate fixed effects regressions with both unit and time effects to identify the impact of this policy. We also allow for time effects to vary by physical characteristics of the land, including agricultural suitability. Identification therefore arises from differential growth rates across zoned and unzoned counties with similar baseline characteristics. The estimation includes year-by-year impacts that allow us to assess the validity of the assumption that prior to implementing zoning, counties with zones were on growth paths similar to those of counties that were either never selected for zoning or selected at a later date. We find that this assumption is satisfied.

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The estimations show that these policies have a large effect when they are put in place in a setting where there are few other places for investors to locate. This was the case for the first coastal zones in China, which were put in place in 1980, and for earlier zones in the western region, established in the early 1990s. These early zones were also established by national, rather than provincial, policy makers. This may provide an additional explanation for their greater efficacy if it is the case that national policy makers were able to invest more resources, had superior information on optimal placement, or were deemed more trustworthy by foreign investors. We find weak evidence that national policy makers were able to target areas with better potential, but this is by no means conclusive. We also find some differential impact for later national zones placed in areas with geographic advantage, including favorable channel depth and proximity to Taiwan and Hong Kong.

This analysis speaks to two broad literatures: the work on tax incentive policies designed to encourage differential growth of cities or regions, and the literature examining drivers of urbanization. Much of the former considers the effect of location-based policies on economic outcomes in developed countries and has produced mixed evidence (see reviews by Garcia-Mila and McGuire 2002; Bartik and Eberts 2012; Neumark and Simpson 2014). Related papers on the impact of foreign direct investment (FDI) are often also limited to analysis of developed countries (Feld and Heckemeyer 2011).

Our study differs from these in measurement and identification strategy. Most empirical analysis of investment incentive policies use growth in population, employment, location of new businesses, or investment as outcomes (as noted by Bartik 1991; Mark, McGuire, and Papke 2000; Neumark and Kolko 2010). We employ a less direct but more accurately measured outcome—urban land extent—through which we can extrapolate our findings to GDP. In terms of identification, previous within-country work addresses the endogeneity and counterfactual challenges using a combination of time and locational fixed effects (Mark, McGuire, and Papke. 2000; de Mooij and Ederveen 2003), and/or lags of key variables (Bobonis and Shatz 2007). The paper most similar to ours is by Wang (2013), who also uses later waves of economic zoning in China as a counterfactual for earlier waves. Unlike Wang's, our main outcome is urban land expansion, which we analyze at the county, rather than the coarser, prefectural level, and which corresponds to the unit at which locational choices for zoning policies are made. In addition, we also analyze variation in policy effectiveness depending upon who initially proposed the policy (national or provincial level), and according to geographic location.

Our paper also contributes to the broader literature on urbanization and urban land expansion. Much of this work tests the monocentric model of city growth, absent any policy interventions (Alonso 1964; Brueckner and Fansler 1983; Fujita et al. 1999; Glaeser 2007; Gottlieb and Glaeser 2009; McGrath 2005; Mills 1967; Rosen 1979; Roback 1982), while more recent papers have attempted to quantify the spatial equilibrium model (Fujita et al. 1999; Krugman 1991). Such analyses usually use census data, including those by Glaeser (2007), Gottlieb and Glaeser (2009), and Park and von Rabenau (2011). Recent advances in this literature have relied on satellite imagery to generate outcome variables. This has allowed for the analysis of explicitly spatial issues (Irwin and Geoghegan 2001), such as spatial externalities (Irwin and Bockstael 2002) and urban sprawl (Irwin and Bockstael 2007). The present paper does not explicitly model such spatial interactions, although we do control for the possibility of differential trends by spatial characteristics.

An important innovation of our paper is to analyze urban expansion in a transitioning economy. It is essential to examine the driving forces of urbanization in developing countries for four reasons. First, the transformative speed of urbanization is much faster in developing countries than in developed countries. Between 1970 and 2011, the population living in urban areas of wealthy countries grew by 0.89% annually, while the average rate in developing countries was 3.33% per year (United Nations 2012). Second, the problems associated with urbanization, for example, overcrowding, traffic congestion, housing shortages, unemployment, and so forth, are more severe in developing countries than in developed countries, and cities in developing nations often lack resources of infrastructure to deal with these issues. Third, urban expansion has profound environmental impacts that extend beyond city boundaries, including changes to microclimate; conversion of natural ecosystems; loss of agricultural land; air, soil, and water pollution; and increased water use and runoff (El Araby 2002; Mills 2007). Many of these impacts are exacerbated when new growth is expansive or dispersed (Alberti 2005), making it essential to understand what factors may influence different rates and patterns of urban growth. Finally, the urbanization process in developing countries operates within the context of land and labor market failures, as well as extensive government intervention (Lichtenberg and Ding 2009), making it necessary to build up evidence from a variety of different settings.

Finally, our work complements and extends previous studies on the transformation of farm to urban land in China. These include research focusing on rural to urban migration (Chan and Zhang 1999; Sato and Yamamoto 2005), GDP growth, FDI, and industrialization (Deng et al. 2006, 2008; Liu, Zhan, and Deng 2005; Seto and Kaufmann 2003). Many of these studies use specific cases or regions, such as the Pearl River Delta (Seto and Kaufmann 2003). Other case studies using temporal, but not spatial, variation to estimate drivers of urbanization have been conducted in the coastal areas of Wuxian City (Xie et al. 2005) and Guangzhou (Wu and Yeh 1997). Broader analyses using data aggregated to higher levels include those by Deng et al. (2006, 2008), who use county-level land use change from the 1980s to 2000 and first differences to account for unobserved fixed effects, and Lichtenberg and Ding (2009), who employ change in urban land at the prefectural level as the outcome. Our work differs from this by more cleanly separating specific policies from general economic growth, and by using both a longer (eight time periods over 30 years) and more geographically extensive (covering both the coast and the western region) dataset. While we are unable to assess the optimality of these policies—they may, for example, correct important market failures, or generate important positive spillovers at a national level—we are confident that our results give a reasonable sense of the ability of these policies to generate differentiated impacts.

## II. ECONOMIC ZONING IN CHINA, 1978–2010

The year 1978 marks the takeoff of both China's economy and its accelerated urbanization. At this time, there was no FDI in China. Since 1978, important elements of the central-planning system have been gradually stripped away by the central government. In particular, the right to receive and encourage foreign investment and foreign trade has gradually expanded from the southern coast to other regions of the country, using a very specific set of zoning and tax incentives, which we will refer to jointly as economic zoning (EZ). The intention of these incentives, at least initially, was to encourage economic growth by bringing in foreign capital, technology, and skills, and access to foreign markets (Li 2008). For the purposes of analysis, it is important to note that the policies in which we are interested have well-defined boundaries; economic zoning applies to specific spaces within counties and does not extend beyond these borders.

The first such policy was the Special Economic Zone (SEZ), which was piloted in 1980 in the southern coastal cities of Shenzhen, Zhuhai, Shantou, and Xiamen. These cities were purposefully chosen due to their geographic proximity to Hong Kong, Macau, and Taiwan, the main sources of foreign capital at the time. At the time of selection, these towns were small fishing villages, with little infrastructure to support manufacturing and trade. The policy included two main elements: the right to receive FDI, and specific tax incentives for foreign firms that located within the zones. The latter comprised a reduced corporate income tax rate of 15% plus two years of initial tax exemption, considerably lower than the average corporate income tax of 33%

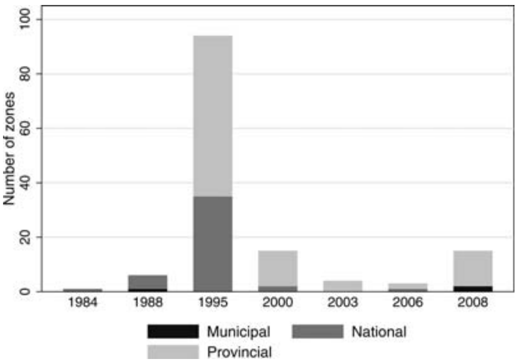
across the rest of the country. Besides attractive tax rates for foreign corporate income, foreign investors were also exempt from customs duty for production materials and individual income tax (Akinci and Crittle 2008).

The SEZ policy was widely seen as successful. Shenzhen alone absorbed $21.6 billion in FDI cumulatively between 1979 and 2002, which accounts for 4.8% of the national total (Cheng and Kwan 2000). This investment transformed the economic base of SEZ counties from agriculture to industry. In Shenzhen, nearly 98% of the working population was engaged in nonagricultural sectors by 2001.

In the wake of this success, the Chinese government expanded the model to 14 coastal areas, known as the Gateway Cities, between 1984 and 1988. In contrast to the SEZ cities, these counties were selected due to their existing transportation infrastructure and history of trade. All were either treaty ports during the Qing Dynasty, or Ming Dynasty ports.[1] The policy was similar in structure to the SEZ, including permission to receive FDI, a slightly higher corporate tax rate than the SEZ(24%), and 3 to 5 years of full tax relief.

After this phase, economic zoning flourished, and provinces began pushing their own zoning efforts, often very near the previously established national zones. For example, in our study areas, 74% of the counties' first economic zones were established due to provincial efforts, whereas only 26% were nationally designated. It is important to note that while the permission for zones was given at either the national or provincial level, their specific location was determined at the level of the county, which is the level at which we define the policy for our analysis. The provincial zones appear to be a reaction to the national-level decisions; in our data, provincial zoning first appears in 1988. It is around this time that national-level zoning begins to slow, and there

FIGURE 1
Number of Zones by Type and Year



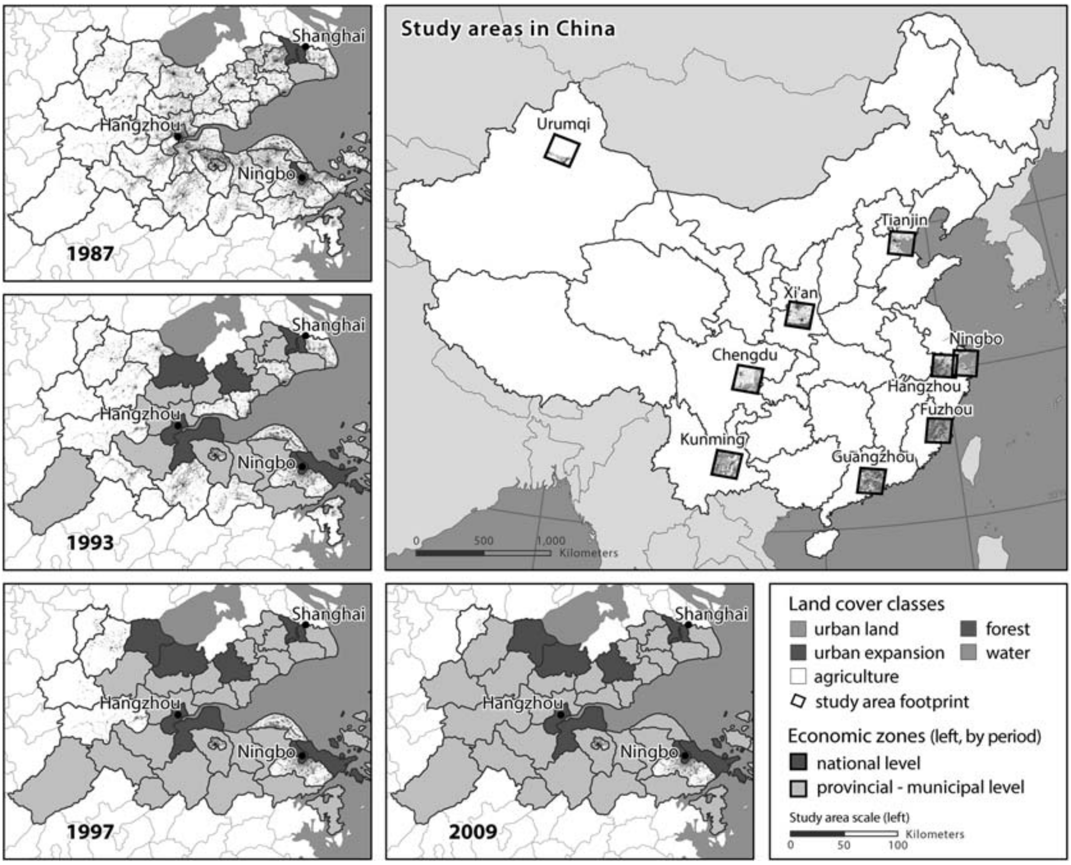

are very few national-level zones established after 1995 that were not transitions of provincial to national zones. Figure 1 shows the number of zones in our data over key year ranges. The *x*-axis shows the end-year for each category. For example, zones established between 1978 and 1983 are in the 1984 bin, those established between 1984 and 1987 are in the 1988 bin, and so on.

Until 1990, the zoning frenzy was largely limited to the coast. In the face of rising inequality between inland provinces and the coast, the policy model was expanded to include zones in central and western China under the guise of Economic and Technology Development Zones (54 zones) and High Technology Industrial Development Zones (52 zones). Like the Gateway Cities, these areas have historically been regional centers of trade. In contrast to the coastal counties, however, they have little trade advantage, being landlocked and in mountainous areas. The tax incentive in these zones was initially a 15% corporate income tax on high-technology production only, with longer exemptions than in the other targeted areas.

Since 1990, there has been significant investment in infrastructure to connect the western areas with the rest of China, through the "Go West" program (Lai 2002; Tung and Cho 2001; Shen 2004). In addition to infrastructure development, the Go West program also includes a uniform business tax rate of 15%, with 3 to 5 years of exemption, starting in 2001. It is important to note that although the

[1] The so-called treaty ports were established after the First Opium War in 1842. The four cities in question—Shanghai, Ningbo, Fuzhou, and Guangzhou—constituted the limited areas in which European countries could house their military. They were chosen for their accessibility by sea, and their connections to other Chinese cities (Banerjee, Duflo, and Qian 2012).

FIGURE 2
Study Sample and Policy Rollout in Hangzhou-Ningbo Footprint




tax incentives were ostensibly available broadly, firms were officially required to locate in specific development zones whose specific geographical locations were established by county authorities, although the broader mandate for the zone came from the provincial or national government. In 2008, under pressure from the WTO, China eliminated preferential treatment for foreign investors, so the differential regional tax policies began to apply to all businesses, not just enterprises with foreign partners (An 2012).

A map of China highlighting the areas that we use for analysis, and the trajectory of policy implementation in one of the sample areas, is shown in Figure 2.[2] Within the temporal trajectory of policy implementation, we shade the establishment of actual spatial zones, so the figure highlights the advance of zoning development across time in this footprint.[3] Appendix Table A1 shows the definitions of zoning in each of the counties in our sample, along with the year in which the first and second zones were established in those counties.

Over time, the policies have spread over much of the country; by 2008, 92% of coun-

[2] Note that the locational labels are the names of major cities, rather than county names.

[3] Between 1985 and 1988, the national government developed "coastal open economic zones," which effectively allowed for zoning to take place in any coastal province (including counties not contiguous with the coast). In our empirics, our results are robust to coding this broad policy as the first zoning experience for an affected county, as well as defining zoning by the establishment of particular spatially limited zones within those counties.

ties in the country had some type of zoning (Wang 2013). The Chinese context creates a setting where one can examine both the isolated impact of such policies, and the possibility of their functioning in a more competitive setting. The selection of target counties because they have either geographic or infrastructure advantages presents a challenge to identifying impacts. While we cannot completely resolve this issue, the strategy applied here allows us to establish bounds on this potential impact, thus lending important insight into the effectiveness of these policies in encouraging economic growth broadly, as well as addressing specific regional inequalities.

## III. DATA DESCRIPTION

### Spatial Data

One advantage of our study is the quality of data we use to analyze land use change over the 30-year time period. Compared with decennial census data and survey data (Brueckner and Fansler 1983; McGrath 2005), remote sensing–derived data have clear advantages for successfully monitoring landscape change (Milesi et al. 2003; Burchfield et al. 2006; Woodcock et al. 2001). These data provide repeated synoptic coverage of the earth's surface and extend to areas where ground surveys are not possible for reasons of either political or geographical accessibility. They are also free of the sort of administrative intervention that renders some economic data suspect.

The land use data for our study were extracted from satellite imagery with a spatial resolution of $30 \times 30$ m (USGS 2011). The raw images are interpreted as described by Schneider (2012). The final maps were calibrated and assessed for accuracy using Google Earth imagery, on-site visits, and photo interpretation of test sites by multiple analysts (for details see Schneider 2012). The overall accuracy, meaning the correspondence between maps and actual land use, is 90% to 94%. The final panel includes land use maps for eight time waves: 1979, 1984, 1989, 1995, 2000, 2003, 2006, and 2009.

The sampling strategy was conducted at the "footprint" level, where a footprint is a spatial unit of approximately $185 \times 185$ km (corresponding to a satellite "scene"). The reason for this is that it is immensely costly to translate satellite imagery, and random sampling of footprints across China would result in far too many scenes with no urban area at all. Footprints were purposely selected to include metropolitan areas with populations above 2 million and to reflect regional differences across China. Beijing and Shanghai were excluded in order to make the sample better reflect average characteristics across Chinese urban areas; the existence of very special policies in those areas makes them difficult to compare to other counties. Most footprints were completely mapped and thus give us information on a large number of counties ($n = 199$). Because footprints are relatively large, they contain a number of other counties that provide potential counterfactuals. The final footprint selection is shown in Figure 1. Due to cloud cover and missing data, some images in the earlier groups (the 1979 and 1984 waves), come from a year before or after the targeted year. Table 1 lists the large cities contained in the selected footprints, as well as the temporal distribution of imagery used to create the maps.

To assess the representativeness of our sample, we examine the characteristics of counties within our sample footprints relative to the distribution of all counties in the 1990 census. Figure 3 shows kernel densities of the distributions of total households, agricultural employment, and illiteracy in the sample counties in 1990, the earliest available census year, as compared to the national distribution. Although our sample counties have a slightly larger number of households, a smaller proportion of households working in agriculture, and slightly lower illiteracy rates, there is significant overlap between the selected counties and the country as a whole.

Four additional characteristics used in our analysis include presence of coastline, slope, elevation, distance to nearest city, and agricultural suitability. Slope and elevation were generated using digital elevation models at 90 m resolution generated from the Shuttle Radar Topography Mission (CGIAR 2012). We also calculated the Euclidean distance to the nearest city, including major metropolitan centers as well as county seat cities (often the largest

TABLE 1
Actual versus Targeted Time Points by Footprint

| | Targeted Time Points | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Region and Main City | 1977 | 1984 | 1990 | 1995 | 2000 | 2003 | 2006 | 2009 |
| Coast | | | | | | | | |
| Tianjin | 1979 | 1984 | 1993 | 1995 | 2000 | 2003 | 2006 | 2009 |
| Hangzhou | 1979 | 1984 | 1991 | 1995 | 2000 | 2003 | 2006 | 2009 |
| Ningbo | 1978 | 1983 | 1987 | 1995 | 2000 | 2003 | 2006 | 2009 |
| Fuzhou | 1979 | 1984 | 1989 | 1994 | 2000 | 2003 | 2006 | 2009 |
| Guangzhou-Shenzhen | 1979 | | 1990 | 1995 | 2000 | 2003 | 2006 | 2009 |
| West | | | | | | | | |
| Xi'an | 1979 | 1984 | 1988 | 1995 | 2000 | 2003 | 2006 | 2009 |
| Chengdu | 1979 | 1984 | 1988 | 1995 | 2000 | 2003 | 2006 | 2009 |
| Kunming | 1979 | | 1989 | 1995 | 2000 | 2003 | 2006 | 2009 |
| Urumqi | 1977 | | 1989 | | 2000 | 2003 | 2006 | 2009 |

city within a county), from the centroid of each sampling unit. Finally, agricultural suitability (a proxy for soil quality/productivity) was estimated at 0.5 degree resolution using the probability that an area will be cultivated based on climate variables (monthly temperature, precipitation, and potential sunshine hours) and soil variables (soil carbon density, soil nitrogen, soil pH, soil water capacity, etc.) (Ramankutty et al. 2002).

### Defining Urban Land

Before moving on to the empirical analysis, it is critical to clarify the definition of urban land based on the image analysis: urban land refers to places dominated by the built environment. The built environment includes all nonvegetative, human-constructed elements such as roads, buildings, runways, and so forth, and dominated implies coverage greater than 50% of a given landscape unit (Schneider 2012). Expansion of urban areas refers to wholesale conversion of land within a landscape unit (it is assumed that the entire pixel area is converted, although that may not be the case). All areas converted to built surfaces are labeled as urban expansion regardless of location (near the city, periurban, or more rural areas).

The land use data are compiled at both the county level and the pixel level using a geographic information system (GIS). To minimize error and to avoid an unreasonably large sample, a sampling unit of $4 \times 4$ pixels was generated, and 1% of units were sampled randomly from each county. This generated a total sample of 131,438 units, observed over eight time periods. The analytical unit in our study is the percentage of urban land within the $4 \times 4$ pixel sampling window within the boundary of each county. From here forward, we will refer to these as "sample areas" of "sample units."

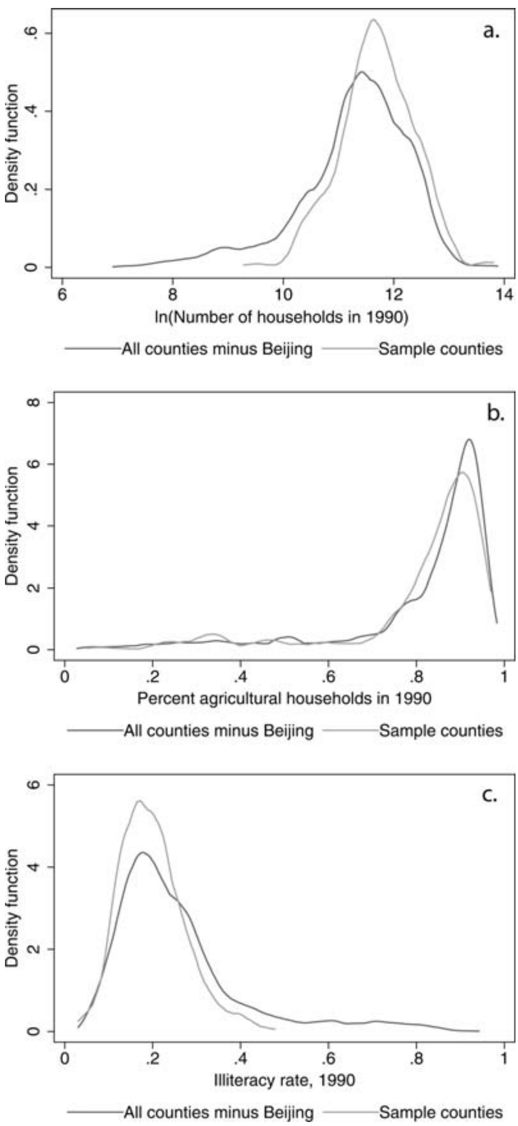
The county is the third administrative level in China, following the province and prefecture. The county is an important administrative level in China; there is evidence that fiscal and administrative decision-making are both conducted at this level (Cheung 2008). In China, some counties had merged into one county, or one county was divided into several counties after the 1990s. To maintain consistency across time and avoid issues of endogeneity related to the redefinition of counties, we use the 1990 county boundaries provided by China Administrative Regions GIS data (1:1 million scale) (CIESIN 1996). In our sample, 80 counties are policy recipients at some point in time, and the remaining 108 counties are nonrecipients.

## IV. SUMMARY STATISTICS AND CORRELATIONS

### Urban Land Use Expansion and GDP

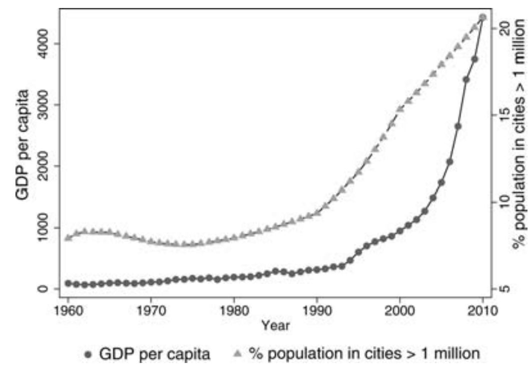
Although conversion of land to urban uses is an interesting outcome in itself—it certainly has important environmental implications—

FIGURE 3
National versus Sampled Counties in 1990:
a. Number of Households, b. Proportion of Agricultural Households, c. Rates of Illiteracy




we are also interested in extrapolating from urbanization to income. It has been well established that higher levels of urbanization are strongly correlated with higher levels of GDP (Bairoch 1988; Acemoglu, Johnson, and Robinson 2002), although there are challenges in establishing the causality of this relationship.

FIGURE 4
GDP per Capita and Urbanization



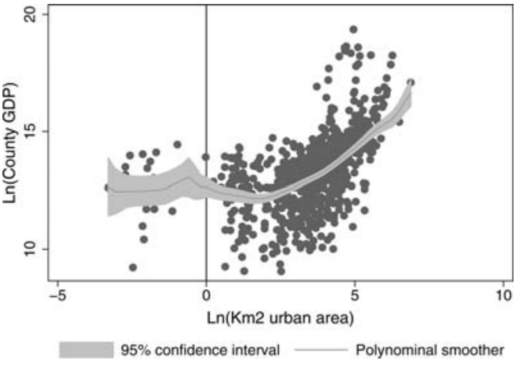

The correlation holds at a national level in China, as can be seen in Figure 4, which shows plots of GDP per capita by year and the percentage of the population in cities of greater than one million inhabitants.

We assess this correlation using county-level data. To accomplish this we use the county-level aggregation of urban land area in square kilometers and, to the extent possible, match this with county-level GDP data from the Chinese Bureau of Statistics (China Statistical Bureau 2010), which begins in 1988. There are significant gaps in the GDP data; while we have observations on 1,440 counties and years from the remote sensing data, there is GDP information to match with only 801 of those observations, and all of this is from 1995 onward.

Figure 5 shows a scatter plot of a log transformation of GDP and area of urban land, as well as a kernel regression with confidence intervals. For counties with less than 1 km of urban area (those to the left of zero on the *x*-axis), the correlation between GDP and urban land is nonexistent. However, a strong and positive relationship begins for those counties with more than 1 km of urban area.

We also run a series of simple ordinary least squares regressions using these data, including time dummy variables for each year in order to minimize spurious temporal correlation. Table 2 shows the results of this exercise. These regressions estimate an elasticity of GDP with respect to urban land area ranging from 0.48 to 0.73. In some specifications

FIGURE 5
GDP and Urban Land Area




we include a test of whether the relationship between urban area and GDP differs across the two main regions of our study: the coast versus the west. While the point estimate on this interaction term is negative, it is small and not statistically different from zero. We conclude that it is not unreasonable to draw conclusions about economic growth from our observations of urbanization.

**Summary Statistics**

The trend of urban land expansion in the two regions is impressive. Figure 6 shows a smoothed trend line across two subsamples of the data: coast and west. The unit of analysis is the sample cell referred to above, with units limited to those within 20 km of the county seat, a limit established using the maximum urban area in target counties in 2009, and assuming that that area was distributed approximately as a circle. We can interpret the *y*-axis as showing the percentage of urban area within this radius at any given time period. Note that the jump in the percent urbanized in 1984 in the west is a result of the missing images in Urumqi and Kunming due to cloud cover. This will not pose a problem in the empirical analysis, since footprint/year effects will limit the impact.

One point of interest in Figure 6 is that in 1979, the urban areas in the west were actually slightly more urbanized than the coastal areas. Both regions experienced significant upward trends, particularly after 1990 on the coast and after 1995 in the west. This is consistent with the overall economic growth in China, as well as the release of labor movement through the loosening of within-China migration restrictions (the Hukou registration system) in the mid-1990s. It will be impossible to identify but important to control for these national-level trends.

Tables 3 and 4 present summary statistics on the baseline variables by region and policy status. The means are calculated as the average across all sample units, with no missing data in the baseline year by policy status in different regions, and are limited to those areas within 20 km of the nearest county seat. On average, sample units in policy-targeted areas, even prior to receiving the policy, have more urban land, higher agricultural productivity, and lower elevation than nonrecipients. The exception to this is the small area targeted for the very first SEZ in 1980, which has an average urbanized area in the baseline similar to that of areas never targeted or targeted very late. This is consistent with the narrative above: the first SEZs were developed in fishing villages, while targeting in the later 1980s went toward well-established coastal trading cities with more infrastructure to support an export economy.

There are substantial differences in land quality and geographic characteristics between the coastal and western areas in our sample. Western areas are found at higher elevations and have lower agricultural productivity. Although many of the magnitudes of the differences appear large, calculations of normalized mean differences comparing the early-targeted groups to those targeted later or not at all reveal that none of these differences is larger than 0.25 standard deviations.

Finally, using all the counties in the sample, Figure 7 presents trends from 1977 to 2009 in percentage of urban land under different policy waves for all nine study footprints. Each sampling unit is sorted into a policy group, depending on the year the first economic zone was established.[4] Vertical

[4] Some counties have multiple zones, but our coding uses just the date of establishment of the first zone.

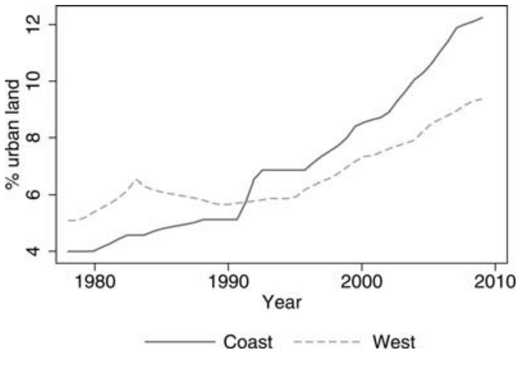
TABLE 2
Correlation between GDP and Urban Land Extent

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| ln(Urban land, km$^2$) | 0.707*** | 0.733*** | 0.584*** | 0.616*** | 0.481*** | 0.490*** |
| | (0.0706) | (0.0724) | (0.0548) | (0.0606) | (0.0624) | (0.0670) |
| Western region × ln(Urban land, km$^2$) | | − 0.128 | | − 0.0967 | | − 0.0312 |
| | | (0.138) | | (0.118) | | (0.127) |
| $R^2$ | 0.343 | 0.416 | 0.708 | 0.710 | | |
| $N$ | 751 | 751 | 751 | 751 | 751 | 751 |

*Note:* Unit of observation is the county. Dependent variable = ln(County GDP). Data are from 1979 to 2009 in the following time steps: 1979, 1984, 1988, 1995, 2000, 2003, 2006, 2009. All estimates include ln(Area with spatial data), and standard errors (in parentheses) clustered at the county level. Columns (3) and (4) use region by wave time effects, and columns (5) and (6) use footprint and time fixed effects, as well as footprint-specific time trends.

*** $p < 0.01$.

FIGURE 6
Average Percentage Urban Land by Region, 1978–2009




lines indicate the timing of the majority of interventions in each region.

In the west we observe that the trends between the early, late, and nontargeted counties are quite similar before 1995, with a slight divergence of early-targeted counties from the trend after 1995.

These graphs also serve to visually emphasize the selection problem discussed earlier: the earliest EZ counties tended to have the least amount of urban land, while later, Gateway counties had more urban land than average, as did the areas selected for preferential treatment in the west. Prepolicy trends across different types of counties both in the west and on the coast are largely similar. The next section discusses our strategy to minimize selection effects on our estimates.

## V. EMPRICIAL STRATEGY

We are interested in evaluating the impact of location-specific tax policies. The main identification issue is that treatment counties were chosen in such a way as to maximize the chances of impact (the exception to this is the early 1980 cluster). Therefore, a simple regression estimation using the entire sample and analyzing the differences in urban trends is likely to find a positive impact from the policies.

Broadly, our empirical strategy relies on regressions comparing changes over time in outcomes for targeted and similar late- or nontargeted counties. Before estimating impacts, we create an estimation subsample by matching on characteristics that are likely to influence urban growth. Matching methods can improve covariate overlap and reduce potential bias in regression analysis by ensuring similarity between treatment and control groups (Dehejia and Wahba 1999, 2002; Ho et al. 2007; Stuart 2010). By eliminating as potential controls counties with different observable characteristics, we generate a more plausible estimation of how growth would have occurred in the absence of zoning. After matching, impacts are identified from regression models with unit and time fixed effects, thus controlling for time-invariant unobservable differences between targeted and untargeted units, as well as common temporal shocks. The underlying assumption is that trends in zoned and unzoned (or late-zoned)

TABLE 3

Summary Statistics for Full Sample by Targeting Stage: Coastal Region

| | Never Targeted | 1980 | 1984–1987 | 1988–1994 | 1995 or Later |
|---|---|---|---|---|---|
| Urban areas, % | 3.654 | 3.002 | 4.409 | 4.683 | 2.666 |
| | (15.58) | (14.28) | (17.47) | (17.65) | (12.89) |
| Agricultural productivity | 0.737 | 0.849 | 0.816 | 0.697 | 0.625 |
| | (0.196) | (0) | (0.172) | (0.261) | (0.182) |
| ln(Median elevation) | 3.398 | 3.243 | 2.727 | 3.153 | 4.825 |
| | (1.533) | (1.124) | (1.748) | (1.838) | (1.734) |
| ln(Median slope) | 0.838 | 1.249 | 0.515 | 0.708 | 1.803 |
| | (1.534) | (1.050) | (1.518) | (1.604) | (1.401) |
| Observations | 20,755 | 289 | 2,791 | 42,684 | 17,013 |

*Note:* For the 1980 zone, the coarse nature of the data means that there is only one value of agricultural productivity for this entire region. Standard deviations are in parentheses.

TABLE 4

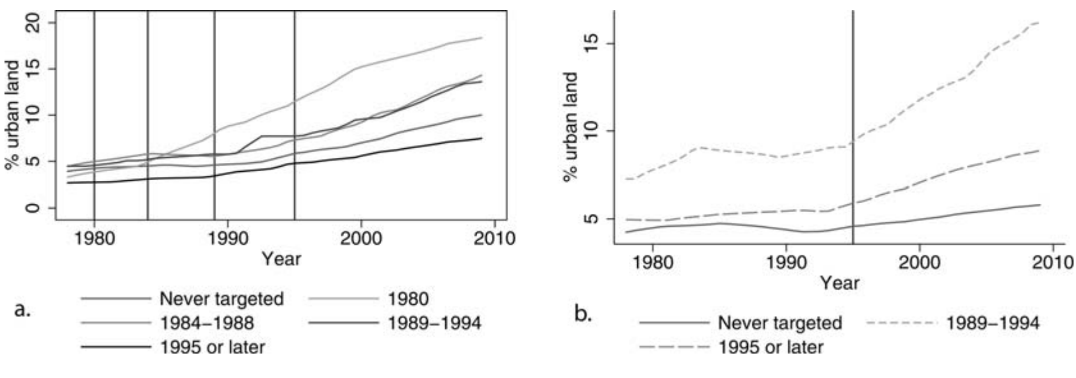
Summary Statistics for Full Sample: Western Region

| | Never Targeted | 1988–1994 | 1995 or Later |
|---|---|---|---|
| Urban areas (%) | 4.044 (15.15) | 7.329 (20.52) | 4.979 (16.90) |
| Agricultural productivity | 0.353 (0.357) | 0.562 (0.388) | 0.464 (0.443) |
| ln(Median elevation) | 6.769 (0.635) | 6.584 (0.623) | 6.291 (0.312) |
| ln(Median slope) | 0.970 (1.420) | 0.694 (1.379) | 0.685 (1.327) |
| Observations | 20,755 | 42,684 | 15,338 |

*Note:* Standard deviations are in parentheses.

FIGURE 7

Average Percentage Urban Land by Policy Group: a. Coast, b. West

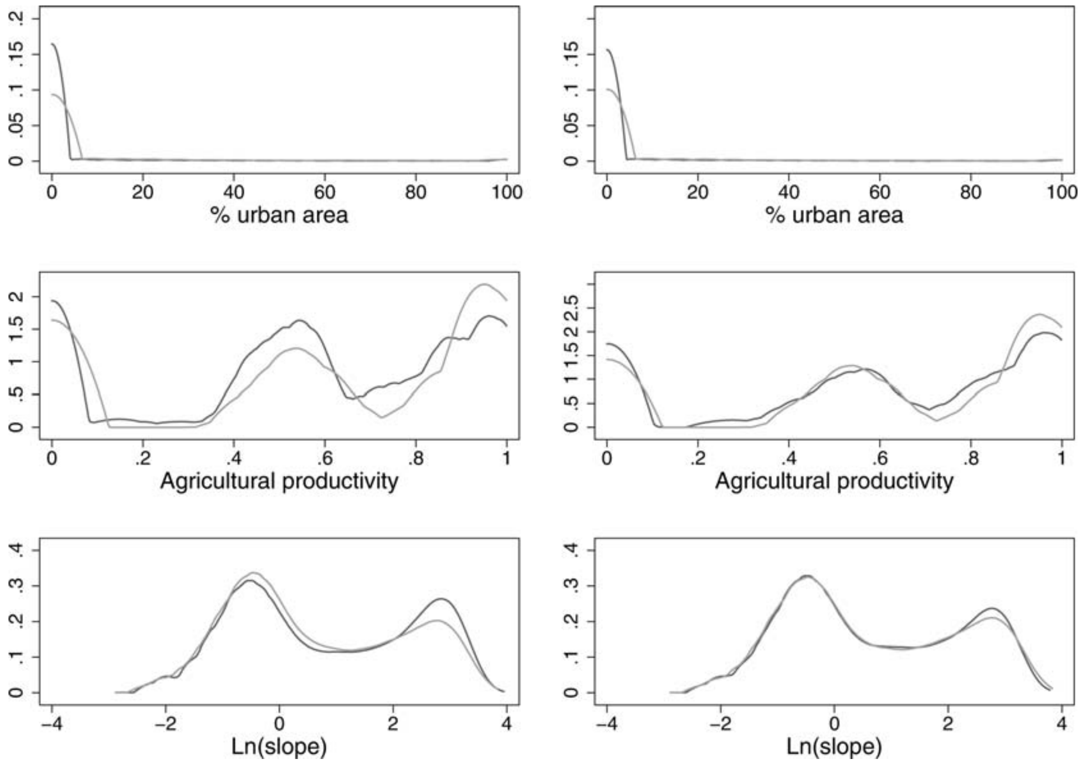



counties would have been the same in the absence of the policy. Our preferred specification contains an implicit test of this assumption, which is validated by the data.

For the first step, we select late- and non-targeted counties that in 1978 were similar to those that were chosen in the early stage of economic zoning, where early is defined as before 1988 on the coast and before 1995 in the west. Using covariate matching where distance is measured using the Mahalanobis metric, for each early policy county we initially select three counties with the most similar agricultural productivity, elevation, slope, and urbanized area in 1978.[5] We also include dummy variables for each footprint within the

[5] Results are similar for four or five matches. They differ for smaller sets of matches, since these smaller sets fail to include some of the later-targeted counties.

FIGURE 8
Improvement in Covariate Overlap: Unmatched *(left)* versus Matched *(right)*




matching algorithm, thus penalizing out-of-footprint matches. We require matching to be exact on urban extent, the presence of coastline, and within each region. We then further restrict the sample to policy-targeted counties with the best 95% of matches. After dropping duplicate matches, we have 26 counties that received early economic zoning and 49 that received later zoning or never received it at all.

The matching process eliminates as potential counterfactuals counties that were very different from those that were targeted in the earlier policy waves—those with very poor agricultural productivity, extremely small initial urban extent, or very different geographic characteristics compared to targeted counties. The elimination of the worst 5% of matches also drops some of the largest targeted urban areas from footprints. This occurs because for some of the largest urban areas, there is simply not a very good comparison. In Chengdu (western China), for example, the best match for the county with the largest 1978 urban area is 40% smaller than the policy-affected zone. Some of the other matches that were eliminated were those that occurred outside of footprints.

The summary statistics for the sample used in estimation are shown in Appendix Tables B1 and B2, where they are divided across all the policy waves. Figure 8 shows the change in overlap of key characteristics for early-targeted versus late- or untargeted counties. The matching process improves the comparability of target and nontarget counties.

In the second step, we use this matched subsample to estimate impacts using a fixed effects specification with effects at the sample-area level. The dependent variable is the percentage of sample unit $p$ in county $c$, located in region $r$ that is urban at time ($u_{pcrt}$). We include time effects for each year in the data ($w_t$) to control for national-level tempo-

TABLE 5
Impact with Time-Varying Policy Variable

| | (1) Both | (2) Both | (3) Both | (4) West | (5) Coast |
|---|---|---|---|---|---|
| EZ(0/1) | 0.739* | 0.480 | 2.474** | 4.057*** | 1.144 |
| | (0.376) | (0.465) | (0.961) | (0.587) | (0.973) |
| EZ × Provincial or municipal zone | | | − 2.669** | − 2.963*** | − 2.841** |
| | | | (1.123) | (0.725) | (1.308) |
| $R^2$ | 0.0742 | 0.0708 | 0.0728 | 0.0640 | 0.0901 |
| $N$ | 594,602 | 337,463 | 337,463 | 163,641 | 173,822 |

*Note:* Unit of observation is the sampling unit. Dependent variable is the percentage of the unit in urban land use in a given wave. Column (1) uses the entire sample, and columns (2)–(5) the matched sample. Standard errors are in parentheses and are clustered at the county level. These are partial results. All estimations include unit fixed effects, time wave dummies, interactions with time and slope, elevation, and distance to county seat. In columns (1) and (2), time interactions for region are also included.

* $p<0.10$; ** $p<0.05$; *** $p<0.01$.

ral shocks that affect all pixels, such as the overall economic growth rate in China. $Target_{ic}$ is a series of dummy variables indicating the "policy wave" to which the pixel belongs. These waves are defined according to the restrictions of our spatial data, which occur at intervals: 1979, 1984, 1988, 1995, 2000, 2003, 2006, and 2009. Policy wave 1 is therefore defined as occurring between 1980 and 1983, wave 2 between 1984 and 1987, and so on. These are interacted with time effects ($w_t$). The coefficient estimates $\beta$ measure the difference in urban land in a given year between a sample unit subjected to a particular policy and the overall time effect ($w_t$). The pixel-level fixed effects control for all baseline differences in covariates. $X_p$ and its interaction with time period allows for differential time effects for sample units with different levels of agricultural productivity, elevation, and distance from the nearest county seat.[6] We also include interactions between region (coastal vs. western) and time dummies to control for differences in time effects across regions. The standard error $\epsilon_{pcrt}$ is clustered at the county level. The final estimation equation is

$$u_{pcrt} = \alpha_0 + \sum_{t=2}^{8} (\gamma_t w_t + \beta_{rt} w_t Coast_r + \beta_{pt} X_p w_t + \sum_{i=1}^{4} \beta_{it} Target_{ic} w_t) + \theta_p + \epsilon_{pcrt}. \quad [1]$$

Because sampling was conducted randomly within each county, and because counties vary in size, the regressions include weights to adjust for the probability that each pixel is included in the sample. Note also that this specification contains within it a test for the parallel trend assumption, since time/policy interactions before a policy occurred implicitly test for prepolicy differences across targeted and nontargeted counties. We also use a specification that calculates the average impact of the policy over the entire period, by generating a variable for each policy that is equal to one when zoning is permitted in a specific county and zero otherwise. Finally, we conduct estimates for the coastal and western areas separately.

## VI. RESULTS

Table 5 shows estimates from a specification that identifies the average effect of each policy over time; the policy variable for the earliest wave of policy interventions, for example, is equal to one after 1980 and zero otherwise. For a county targeted in 1988, the dummy is equal to zero before 1988 and one

[6] It is important to note that the agricultural productivity variable is somewhat coarse, with each county containing only between two and six productivity observations.

thereafter, and so forth. These estimations also include location by year dummies, and year by geographic characteristic interactions. Column (1) uses the full, unmatched sample, and the remaining columns use the matched subsample of the data. The results from column (2) are less than those of column (1) and are not statistically different than zero. This is consistent with our suspicion that using all untargeted counties as a comparison group would generate overestimates of the impact of zoning.

As we saw above, national zoning effectively ended by 1995, and the only municipally driven zones in our data were established on the coast between 1984 and 1987. Column (3) introduces an interaction term between the EZ dummy variable and the level at which the policy was defined: the EZ dummy alone in this case identifies the impact of having the EZ designated by the national government, and the interactions show the difference in impacts if the zone is designated by municipal or provincial authorities. We group the municipal and provincial zones together since there were so few municipal zones in our data. The point estimates in column (3) suggest that municipal- and provincial-level zones have no significant impact on the growth of urban area—the sum of the national zone point estimate with the interaction terms is equal to zero—but that nationally designated zones have large and statistically significant impacts. On average, the urban extent in a national zone is 2.5 percentage points higher than in areas without zoning over the same time period. Columns (4) and (5), however, show that this impact is generated entirely by the zones in the west. Sample units that experienced zoning on the coast appear not to have been positively affected by it. In fact, areas selected for provincial or municipal zones actually appear to have grown more slowly after receiving their zones, relative to areas with national economic zoning or areas that remained unzoned.

These initial results, however, are far from conclusive. They depend upon a parallel trend assumption that we have not formally tested but that is implicit in the estimation equation presented in equation [1]. They also calculate an average impact for all the postpolicy years, rather than allowing for impacts to vary over time. We therefore turn to estimates from equation [1]. These estimations include large numbers of dummy variables and interaction terms, so the table presenting the results is contained in the appendix (Table B3). Figures 9 and 10 graph the coefficients from the time dummy variables interacted with the nonvarying dummy variables that indicate the year in which a county acquired an economic zone. They therefore indicate the difference in a given year between the time trend for the region and the time trend for units within a given policy wave ($\beta_{it}$ from equation [1]). The omitted time category is 1978. We conduct four estimations based upon our proposed test: two for the coast and two for the west. In each region, we first use all of the first zones in the data, and then, based upon our assessment above that the nationally established zones were the ones with a significant impact, we recode the provincial and municipal zones to be part of the baseline time trend.

Figure 9 shows the point estimates and confidence intervals for each year, including all zone types. Because the figure does not include the time trend, there are points on the graph that go below zero, indicating that the urbanized extent in that year was less than in the average established by the baseline trend. The coefficients show no indication that the counterfactual is unreasonable; there are no significant differences between early-targeted and un- or late-targeted counties prior to policy implementation. With the exception of the earliest-targeted zone—the 1980 SEZ (for which the parallel trend assumption cannot be tested)—there is also no evidence of any sort of impact of these policies. The confidence intervals are very wide and always include zero.

In the west, we also observe no difference between the counterfactual trend (untargeted, matched counties) and counties targeted between 1988 and 1999 prior to the implementation of the policies. After 1995, the cohort targeted between 1988 and 1994 appears to diverge from the overall trend, but the point estimates are not statistically different from zero.

Our earlier estimates suggested that national policies might have an impact where

FIGURE 9
Impact of Zoning by Year: a. Coast, b. West

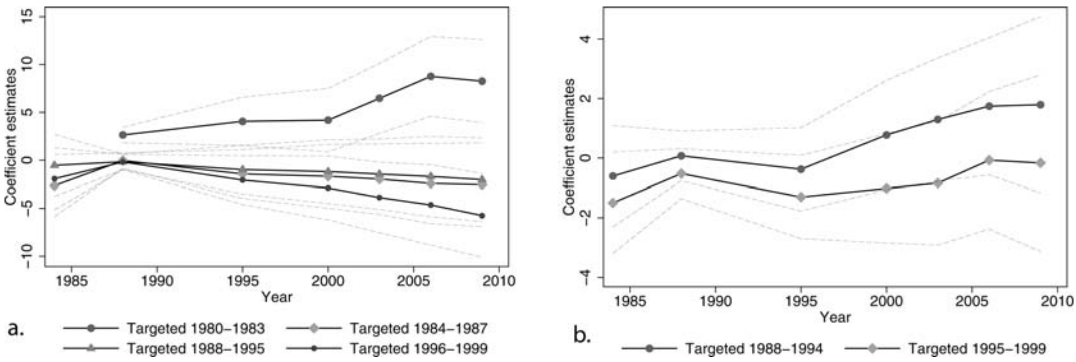
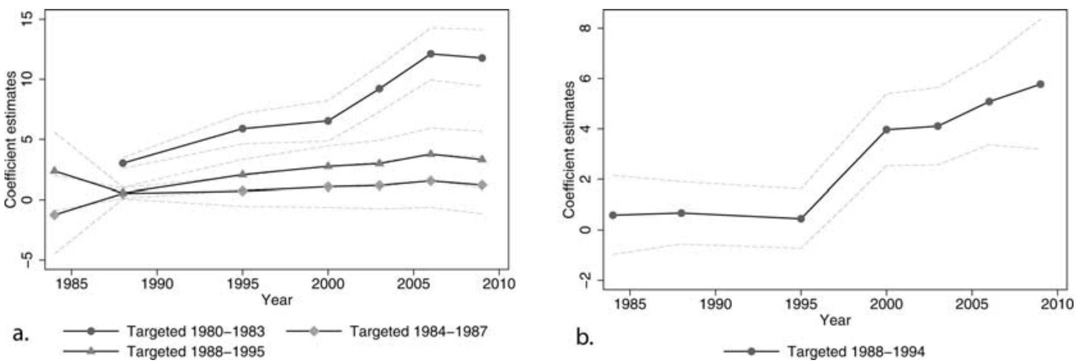



FIGURE 10
Impact of National Zoning by Year: a. Coast, b. West




provincial policies had none. We test this policy in two ways. First, we include provincially targeted counties as part of the counterfactual. Second, we exclude them from the analysis altogether. Both estimates give us similar results, although we are more confident in the standard errors of the first set of estimates, since excluding all counties with provincial or municipal zones decreases the number of clusters in the coastal estimates to 15.

Limiting the analysis to just the national policies tightens the confidence intervals on the estimations of zoning in both regions. Figure 10 plots out the coefficients and confidence intervals of the estimations in columns (2) and (6) in Appendix Table B3. On the coast, the impact of the earliest SEZ is substantial and significant, while that for the later zones does not show obvious breaks from trend, although there are small increases over time that are statistically different from zero for those targeted between 1988 and 1995. There is also a visible break from trend for the 1980 cohort after 2000.

In the west, since all zones after 1995 were provincially driven, there is only one time effect to examine for the national zones: those designated as open between 1988 and 1994. Here, as before, we observe a substantial positive increase from the trend after the implementation of the policy.

From this temporal analysis, we draw three conclusions. First, the earliest economic zoning on the coast appears to have had tremendous impacts, while later zoning waves were relatively ineffectual. Second, the most effective type of zoning seems to have been nationally driven, rather provincial. These effects cannot, unfortunately, be disentangled, since the earliest zoning waves (1980–1983 on the coast, and 1988–1994 in the west) were dominated by national zoning. In the waves where provincial and national zoning coexisted (1988–1995 on the coast), it appears that national zones were more effective, although they were not as effective as the earliest national zones. These differences could be explained by either superior targeting ability of national-level policy makers, important unobservable investments made into these national zones, or higher levels of confidence of foreign investors in national, rather than provincial, zones.

Unfortunately, data on state enterprises or investments are not available for the early periods, and data are not available that would give us insight into investor confidence. Examination of the prepolicy trends estimated by equation [1] for equations where provincial zones are included as part of the treatment (column (1), Table B3) and when they are included as part of the control (column (2), Table B3) gives us some insight into the targeting explanation. The main period of overlap for the provincial and national policies on the coast is for zones established before 1995. For this "treatment," we observe negative (but insignificant) interaction terms for every year for the 1995 cohort when the provincial zones are included. When these counties are included as part of the "control," the interaction terms between policy wave and year are positive (but statistically insignificant) for years prior to 1995. This is weak evidence that national zones were targeted to places with higher prepolicy urban growth and possibly better potential for future growth. A similar (though weaker) pattern also exists for the west (comparing columns (4) and (6), Table B3).

Third, economic zoning induces additional urban expansion in the west. The average urban area in 1995 in the west was about 6%, and the point estimate for 2000 approximately 4%, which constitutes an increase of about 67% over 1995, or 13.4% per year. Given the lowest elasticity of GDP to urban area from Section IV, this suggests an accompanying increase in county GDP of $13.4 \times 0.481 = 6.4\%$ per year. It is possible that we observe this large effect in the west because the pre-1995 zones are in many ways similar to the 1980 zones on the coast; they are taking place in an essentially closed region of the country. However, there are also potential confounds. In particular, it is possible that other investments by the government encouraged differential growth in counties that also received zoning. There are some limited data with which we can test this hypothesis.

Ideally, one would like to observe investment by the state in each county, particularly over the years around when we observe the increase in urban land use in the west. Unfortunately, data on state investment do not exist at this level or within this time window. However, we can observe output from state-owned enterprises and employment in state-owned enterprises at the provincial level from 1995 to 2000, which might serve as an indicator of changes in state investment. These two variables are graphed for our four western provinces in Figure 11.

With the exception of Shaanxi province, the trends in output and employment in state-owned enterprises are largely decreasing over the time when urban land use change is increasing in our analysis. Although our result could still be driven by large differential investment in particular counties within provinces, the differential would have to be quite substantial to result in the effect that we observe.

## VII. HETEROGENIETY IN COASTAL IMPACT

Overall, the results of the previous section show clear impacts in the western region and for the earliest zones on the coast. For zones established between 1984 and 2000 on the coast there are zero to negligible impacts. It is possible, however, that these impacts are heterogeneous across locations. This section examines this possibility, beginning by estimating separate equations for each coastal footprint using our policy dummy variable specification. Note that in this specification the identification comes from changes over time within footprints, so the counterfactual is slightly more limited than in our previous estimates. Results are shown in Table 6.

FIGURE 11
Output and Employment in State-Owned Enterprises in the West, 1995–2000: a. Output, b. Employment

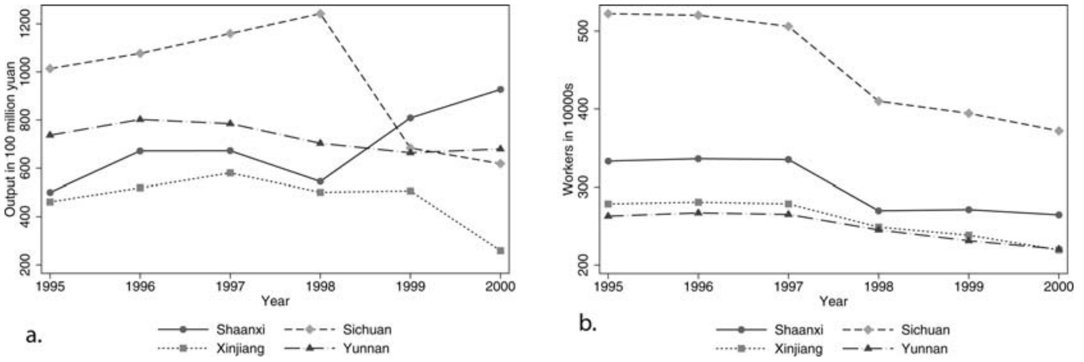



TABLE 6
Impact of Economic Zone (EZ) by Footprint

| | (1) All Policy | (2) National Policy | (3) Early Policy |
|---|---|---|---|
| EZ, Hangzhou | − 1.713** (0.764) | | |
| EZ, Fuzhou | − 0.649 (0.465) | | |
| EZ, Guangzhou | 2.341 (1.435) | | |
| EZ, Tianjin | − 4.228*** (1.131) | | |
| National EZ, Hangzhou | | 7.354* (3.796) | 0.307 (0.635) |
| National EZ, Fuzhou | | − 0.0136 (0.5610 | 0.0416 (0.317) |
| National EZ, Guangzhou | | 4.001* (2.089) | 2.549** (1.260) |
| National EZ, Tianjin | | − 2.009 (1.359) | − 0.528 (1.169) |
| $R^2$ | 0.00914 | 0.0905 | 0.0451 |
| *N* | 173,822 | 173,822 | 83,426 |

*Note:* Unit of observation is the sampling unit. Dependent variable is the percentage of the unit in urban land use in a given wave. Standard errors are in parentheses and are clustered at the county level. These are partial results. All estimations include unit fixed effects, time wave dummies with interactions for region, interactions with time and slope, elevation, and distance to county seat.

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

The first column puts all zones together, and the second two recode the provincially mandated zones as zeros. The negative coefficients in the first column therefore indicate that as a group, economic zones do not induce differential growth in any of the study areas, and in fact, targeted counties show less growth than untargeted counties. The second two columns show results for national zones during all time periods (column (2)) and during years prior to 1996 (column (3)). The results show that national zones had a positive and significant effect in the Guangzhou footprint both overall and in the early years. This is unsurprising, given that this is the study region that contains the earliest SEZ zone. Interestingly, there is also a large impact of national policies in the Hangzhou region, but this effect comes from the years after 1995. These differential results beg the question of why similar policies had no impacts in Fuzhou and Tianjin.

The first potential explanation is that the policies differed significantly across footprints. Table 7 compares approximate maximum and minimum tax rates across the different targeted municipalities in the sample. Because the policies are a combination of reduced yearly tax rates and temporary tax exemptions, some assumptions must first be made. We calculate an average tax rate according to policy type over a 10-year period. This rate is calculated by first multiplying the national percentage of 1999 production in dif-

TABLE 7
Comparisons in Predicted Tax Rates across Footprints

| Study Area | Municipality | Start Year | Max. Tax (%) | Min. Tax (%) |
|---|---|---|---|---|
| Tianjin | Tianjin | 1984 | 17.7 | 10.6 |
| Fuzhou | Fuzhou | 1984 | 17.7 | 10.6 |
| Guangzhou | Shenzhen | 1978 | 9.8 | 8.3 |
| Guangzhou | Guangzhou | 1984 | 15.6 | 13.3 |
| Hangzhou-Ningbo | Shanghai | 1984 | 17.7 | 10.6 |
| Hangzhou-Ningbo | Hangzhou | 1984 | 17.7 | 10.6 |
| Hangzhou-Ningbo | Ningbo | 1984 | 17.7 | 10.6 |

TABLE 8
Shipping Restrictions by Port

| Port | Study Area | Harbor Type | Main Channel Depth (m) | Deadweight Tonnage (DWT) |
|---|---|---|---|---|
| Ningbo | Hangzhou | Sea | 50 | Two-way 250,000 DWT traffic, 300,000 DWT at high tide |
| Guangzhou | Pearl River Delta | Sea | 15.5 | Two-way 50,000 DWT traffic, 120,000 DWT at high tide |
| Shenzhen | Pearl River Delta | Sea/river port | 16 | 100,000 DWT |
| Tianjin | Tianjin | Sea/river port | 19.5 | Two-way 250,000 DWT traffic, 300,000 DWT at high tide |
| Fuzhou | Fuzhou | Sea/river port | 3.8–17.2 | 30,000 DWT |

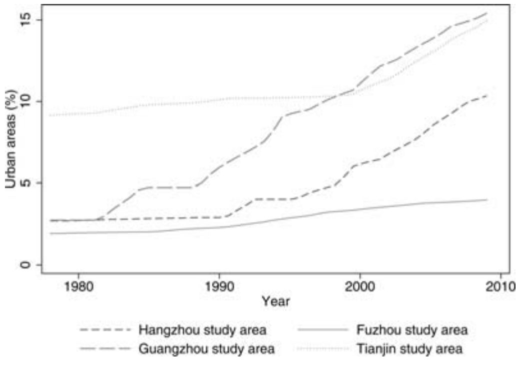
ferent categories—industrial, high-tech, services, and so forth—by the relevant tax rate. Under the assumption that a business will stay for 10 years, we then accord the appropriate tax relief for each year and calculate the 10-year average. Maximums and minimums are derived for cases where the policy allows for 1 to 2 or 3 to 5 years of tax exemption.

If differential tax rates were the driving force behind the heterogeneous treatment effect, we would expect to see lower relative rates in both the Guangzhou and the Hangzhou-Ningbo footprints. While rates are lower in Shenzhen and Guangzhou, there is no difference between the approximate rates in the Hangzhou-Ningbo footprints and those in Tianjin or Fuzhou.

Another potential source of variation is geography: Guangzhou is located very close to Hong Kong—one of the reasons why it was targeted early. Hangzhou may benefit from its proximity to Shanghai, a historically international city. Fuzhou, in addition to being somewhat farther away from historical centers of trade, seems to have significant challenges with regard to its potential to receive large ships. Table 8 details the harbor depth and maximum ship capacity of all the ports in our sample. Fuzhou, with the ability to receive ships of only 30,000 deadweight tons, relative to Hangzhou's 250,000 or the Pearl River Delta's 100,000, may be constrained by its port.

While port capacity and location provide some explanation for the lack of effect in Fuzhou, they do not explain why Tianjin, with a port capacity equivalent to Ningbo, shows no policy impact. A final possibility is that there are limits on city growth in some areas. Figure 12 shows the urban area trends by footprint in our matched sample. Although here we do not exploit within-footprint variation in the timing of policies, we do observe that Tianjin as a whole begins with a considerably higher level of urbanization than any of the other study regions. It is likely that targeted areas within this footprint had urban extents significantly higher than this average and, therefore, had limited room to grow. We submit this as tentative evidence that in addition to the timing of policies and geography of recipient counties, starting points matter.

FIGURE 12
Average Percentage Urban Land by Coastal Area and Year




## VIII. CONCLUSION

In this paper we have examined the potential differential impacts of preferential economic zoning on the growth of cities in China. We have shown that in a closed economy, as coastal China was in the early 1980s and western China up until the early 1990s, such policies can generate differential growth. There is also some evidence that once established, these differences continue over time. Furthermore, we observe that nationally mandated zones have a larger effect than provincially driven zones. We cannot, however, identify the mechanism that drives this, since national zones mostly tend to have been established earlier than provincial zones. We do note that once more such zones open up, it may be that there is less room for differential growth to occur. Impacts of later zones on the coast appear to be much smaller, although there is some evidence of impacts in regions that appear to have some specific historical trade advantages, such as being close to a historically open city and having a large and accessible port.

These results taken together create some doubt regarding the utility of such zones in economies where restrictions on the location of foreign capital are not as tight as they are in China. It is possible that they might be used to encourage investors to locate in somewhat trade-disadvantaged areas, as is the case for western China. However, it is unclear that such incentives would be enough to entice investors to locate firms in areas that are trade disadvantaged in situations where there had not been previous restrictions on this decision. If it is the case that their ability to generate growth where it is desired is limited, then the policies result in lost government revenue with limited development benefits.

The rate of urbanization in China over the past 30 years has been staggering. This is likely broadly due to the gradual liberalization of the economy, particularly the loosening of restrictions on internal labor movements. Our work does not show a substantial role for location-specific investment policies in driving differences in this growth across counties in general, although it does identify an important first mover effect. This begs the question of what forces sustain this differential growth over time. It seems plausible that this is driven by agglomeration economies; a more formal test of this hypothesis would be a useful future line of inquiry.

## APPENDIX A: ZONING DATA

The following table shows the specific zones and the timing of their establishment in the counties of our data. These are spatially explicit; in other words, we list zones in counties where there has been an area identified for use as a special economic zone. We list here only the first two zones established, although some counties have three. The table entries for type include Economic and Technological Development Zone (ETDZ), High Tech Industrial Development Zone (HTDZ), Export Processing Zone (EPZ), Standard Industrial Zone (SIZ), Free Trade Zone (FTZ), and Industrial Park (IP). In our analysis, we use the date of the first-established zone.

TABLE A1
Data for Spatially Explicit Economic Zones

| Footprint and County | First Zone Level | First Zone Type | First Zone Year | Second Zone Level | Second Zone Type | Second Zone Year |
|---|---|---|---|---|---|---|
| *Coast* | | | | | | |
| Fuzhou[a] | | | | | | |
| Lianjiang Xian | Provincial | ETDZ | 2006 | | | |
| Youxi Xian | Provincial | ETDZ | 2006 | | | |
| Dehua Xian | Provincial | ETDZ | 2006 | | | |
| Jian'ou Xian | Provincial | ETDZ | 2006 | | | |
| Pingnan Xian | Provincial | ETDZ | 2006 | | | |
| Xianyou Xian | Provincial | ETDZ | 2000 | | | |
| Minhou Xian | Provincial | ETDZ | 1999 | | | |
| Luoyuan Xian | Provincial | ETDZ | 1998 | | | |
| Fu'an Shi | Provincial | ETDZ | 1998 | | | |
| Putian Xian | Provincial | ETDZ | 1996 | Provincial | HTDZ | 2002 |
| Yongchun Xian | Provincial | ETDZ | 1993 | | | |
| Ningde Shi | Provincial | ETDZ | 1993 | Provincial | ETDZ | 1999 |
| Fuqing Shi | National | ETDZ | 1992 | Provincial | ETDZ | 1999 |
| Changle Xian | Provincial | ETDZ | 1989 | | | |
| Fuzhou Shi Shixiaqu | National | ETDZ | 1985 | National | HTDZ | 1991 |
| Hangzhou[b] | | | | | | |
| Guangde Xian | Provincial | ETDZ | 2006 | | | |
| Yin Xian | Provincial | ETDZ | 2003 | | | |
| Jiande Xian | Provincial | ETDZ | 2002 | | | |
| Cixi Shi | Provincial | ETDZ | 2002 | National | EPZ | 2005 |
| Lin'an Xian | Provincial | ETDZ | 2001 | | | |
| Xinchang Xian | Provincial | HTDZ | 2001 | | | |
| Ningguo Xian | Provincial | ETDZ | 2000 | | | |
| Haining Shi | Provincial | ETDZ | 1997 | Provincial | ETDZ | 2006 |
| Tonglu Xian | Provincial | ETDZ | 1994 | | | |
| Zhuji Shi | Provincial | ETDZ | 1994 | | | |
| Sheng Xian | Provincial | ETDZ | 1994 | | | |
| Xiangshan Xian | Provincial | ETDZ | 1994 | | | |
| Daishan Xian | Provincial | ETDZ | 1994 | | | |
| Haiyan Xian | Provincial | ETDZ | 1994 | | | |
| Changxing Xian | National | ETDZ | 1994 | | | |
| Anji Xian | Provincial | ETDZ | 1994 | | | |
| Pujiang Xian | Provincial | ETDZ | 1994 | | | |
| Jinshan Xian | Provincial | ETDZ | 1994 | Provincial | ETDZ | 1998 |
| Nanhui Xian | Provincial | ETDZ | 1994 | National | HTDZ | 1995 |
| Fuyang Xian | Provincial | ETDZ | 1993 | | | |
| Chun'an Xian | Provincial | ETDZ | 1993 | | | |
| Shangyu Xian | Provincial | ETDZ | 1993 | | | |
| Fenghua Shi | Provincial | ETDZ | 1993 | | | |
| Yuhang Xian | Provincial | ETDZ | 1993 | National | ETDZ | 1993 |
| Shaoxing Xian | Provincial | ETDZ | 1993 | National | ETDZ | 2000 |
| Yuyao Shi | Provincial | ETDZ | 1993 | Provincial | ETDZ | 2002 |
| Pinghu Xian | Provincial | ETDZ | 1993 | Provincial | ETDZ | 2000 |
| Jiashan Xian | Provincial | ETDZ | 1993 | Provincial | ETDZ | 1998 |
| Tongxiang Xian | Provincial | ETDZ | 1993 | Provincial | ETDZ | 2006 |
| Deqing Xian | Provincial | ETDZ | 1993 | Provincial | ETDZ | 1993 |
| Qingpu Xian | Provincial | ETDZ | 1993 | Provincial | ETDZ | 1995 |
| Xiaoshan Shi | National | ETDZ | 1993 | Private | HTDZ | 1995 |
| Ningbo Shi Zhenhai Qu | National | ETDZ | 1993 | Provincial | ETDZ | 2006 |
| Wujiang Xian | National | ETDZ | 1993 | Private | HTDZ | 1995 |
| Dinghai Qu | Provincial | ETDZ | 1992 | | | |
| Jiaxing Shi Shixiaqu | National | ETDZ | 1992 | Provincial | ETDZ | 1998 |

*(table continued on following page)*

TABLE A1
Data for Spatially Explicit Economic Zones *(continued)*

| Footprint and County | First Zone Level | First Zone Type | First Zone Year | Second Zone Level | Second Zone Type | Second Zone Year |
|---|---|---|---|---|---|---|
| Huzhou Shi Shixiaqu | National | ETDZ | 1992 | Provincial | ETDZ | 2006 |
| Songjiang Xian | Provincial | ETDZ | 1991 | County | HTDZ | 2000 |
| Hangzhou Shi Shixiaqu | National | HTDZ | 1991 | National | FTZ | 1992 |
| Shanghai Xian, Minhang | National | ETDZ | 1986 | Provincial | ETDZ | 1995 |
| Fengxian Xian | Municipal | ETDZ | 1984 | Municipal | ETDZ | 1995 |
| Ningbo Shi Cheng Qu | National | ETDZ | 1984 | National | HTDZ | 1999 |
| Pearl River Delta[c] | | | | | | |
| Qingyuan Shi Shixiaqu | Provincial | ETDZ | 2008 | | | |
| Shunde Xian | Provincial | ETDZ | 2003 | | | |
| Gaoming Xian | Provincial | ETDZ | 2003 | | | |
| Conghua Xian | Provincial | ETDZ | 1998 | | | |
| Guangzhou Shi Shixiaqu | National | ETDZ | 1993 | National | EPZ | 2000 |
| Panyu Xian | National | ETDZ | 1993 | National | EPZ | 2005 |
| Zhaoqing Shi Shixiaqu | Provincial | ETDZ | 1993 | National | HTDZ | 2001 |
| Doumen Xian | National | HTDZ | 1993 | Provincial | ETDZ | 2003 |
| Hua Xian | Provincial | ETDZ | 1992 | | | |
| Dongguan Shi | Provincial | ETDZ | 1992 | National | HTDZ | 2001 |
| Jiangmen Shi Shixiaqu | National | HTDZ | 1992 | | | |
| Nanhai Xian | National | HTDZ | 1992 | | | |
| Huiyang Xian | National | HTDZ | 1992 | | | |
| Zhongshan Shi | National | HTDZ | 1991 | | | |
| Xinhui Xian | Provincial | HTDZ | 1991 | | | |
| Zengcheng Xian | National | ETDZ | 1984 | National | ETDZ | 1988 |
| Zhuhai Shi Shixiaqu | National | SEZ | 1984 | National | FTZ | 1996 |
| Shenzhen Shi Shixiaqu | National | SEZ | 1984 | National | FTZ | 1996 |
| Tianjin[d] | | | | | | |
| Hangu Qu | Municipal | ETDZ | 2006 | | | |
| Xianjiao Qu | Municipal | ETDZ | 2006 | | | |
| Wen’an Xian | Provincial | ETDZ | 2000 | | | |
| Yutian Xian | Municipal | ETDZ | 1994 | | | |
| Huanghua Shi | Municipal | ETDZ | 1993 | | | |
| Ninghe Xian | Provincial | ETDZ | 1992 | | | |
| Tianjin Shi Cheng Qu | Provincial | ETDZ | 1992 | | | |
| Fengnan Xian | Provincial | ETDZ | 1992 | | | |
| Bazhou Shi | Provincial | ETDZ | 1992 | | | |
| Dongjiao Qu | Provincial | ETDZ | 1992 | Provincial | ETDZ | 2009 |
| Nanjiao Qu | Provincial | ETDZ | 1992 | Provincial | ETDZ | 1992 |
| Baodi Xian | Provincial | ETDZ | 1992 | Provincial | ETDZ | 2003 |
| Jinghai Xian | Provincial | ETDZ | 1992 | Provincial | ETDZ | 2006 |
| Beijiao Qu | Provincial | ETDZ | 1992 | Municipal | ETDZ | 2006 |
| Wuqing Xian | Provincial | ETDZ | 1991 | Provincial | ETDZ | 2002 |
| Tanggu Qu | National | ETDZ | 1984 | National | HTDZ | 1991 |
| *West* | | | | | | |
| Chengdu[e] | | | | | | |
| Qingbaijiang District | Provincial | ETDZ | 2007 | | | |
| Xinjin Xian | Provincial | ETDZ | 2006 | | | |
| Pengshan Xian | Provincial | ETDZ | 2006 | | | |
| Dujiangyan Shi | Provincial | ETDZ | 2003 | | | |
| Meishan Xian | Provincial | ETDZ | 2000 | | | |
| Jianyang Xian | Provincial | ETDZ | 1999 | | | |

*(table continued on following page)*

TABLE A1
Data for Spatially Explicit Economic Zones *(continued)*

| Footprint and County | First Zone Level | First Zone Type | First Zone Year | Second Zone Level | Second Zone Type | Second Zone Year |
|---|---|---|---|---|---|---|
| Jintang Xian | Provincial | ETDZ | 1994 | | | |
| Santai Xian | Provincial | ETDZ | 1993 | | | |
| Ziyang Xian | Provincial | ETDZ | 1993 | | | |
| Wenjiang Xian | Provincial | ETDZ | 1992 | | | |
| Peng Xian | Provincial | ETDZ | 1992 | | | |
| Xindu Xian | Provincial | ETDZ | 1992 | Provincial | ETDZ | 1993 |
| Shuangliu Xian | Provincial | ETDZ | 1992 | National | EPZ | 2000 |
| Guanghan Shi | Provincial | ETDZ | 1991 | | | |
| Chengdu Shi Shixiaqu | Provincial | ETDZ | 1991 | Provincial | HTDZ | 2002 |
| Longquanyi district | National | ETDZ | 1991 | Provincial | HTDZ | 2002 |
| Kunming[f] | | | | | | |
| Songming Xian | Provincial | ETDZ | 1992 | | | |
| Yuxi Shi | Provincial | ETDZ | 1992 | | | |
| Kunming Shiqu | National | ETDZ | 1992 | National | ETDZ | 1992 |
| Urumqi[g] | | | | | | |
| Urumqi county | Provincial | SIZ | 2006 | National | EPZ | 2003 |
| Shhezi Shi | National | ETDZ | 2000 | Provincial | ETDZ | 2006 |
| Miquan Xian | Provincial | SIZ | 1996 | | | |
| Changji Shi | Provincial | HTDZ | 1992 | | | |
| Urumqi district | National | HTDZ | 1992 | National | ETDZ | 1994 |
| Xi'an[h] | | | | | | |
| Yanliang district | National | HTDZ | 2004 | | | |
| Hu Xian | Provincial | IP | 2000 | | | |
| Wugong Xian | National | HTDZ | 1997 | | | |
| Lantian Xian | Provincial | IP | 1997 | | | |
| Chang'an Xian | Provincial | IP | 1995 | National | HTDZ | 2006 |
| Yao Xian | Provincial | ETDZ | 1993 | | | |
| Gaoling Xian | Provincial | IP | 1993 | | | |
| Xianyang Shi Shixiaqu | Provincial | HTDZ | 1992 | | | |
| Xi'an Shi Shixiaqu | Provincial | HTDZ | 1991 | National | ETDZ | 1993 |
| Weinan Shi | Provincial | HTDZ | 1988 | | | |

[a] Fuzhou, no zone: Minqing Xian, Yongtai Xian, Pingtan Xian, Datian Xian, Shunchang Xian, and Xiapu Xian.
[b] Hangzhou, no zone: Shaoxing Shi Shixiaqu, Putou Qu, and Langxi Xian.
[c] Pearl River Delta, no zone: Foshan Shi Shixiaqu, Sanhui Xian, Hong Kong, Heshan Xian, Huizhou Shi Shixiaqu, Boluo Xian, Longmen Xian, Fogang Xian, Yingde, Xian, Sihui Xian, and Bao'an Xian.
[d] Tianjin, no zone: Fengrun Xian, Dacheng Xian, and Qing Xian.
[e] Chengdu, no zone: Pi Xian, Dayi Xian, Pujiang Xian, Qionglai Xian, Chongqing Xian, Shifang Xian, Mianzhu Xian, Zhongjiang Xian, Lezhi Xian, Renshou Xian, and Deyang Shi Shixiaqu.
[f] Kunming, no zone: Chenggong Xian, Jinning Xian, Anning Xian, Fumin Xian, Yiliang Xian, Lu'nan Yizu Zizhixian, Luquan Yi-Miaozu Zizhi Xian, Jiangchuan Xian, Chengjiang Xian, Yimen Xian, Eshan Yizu Zizhixian, Huaning Xian, Mile Xian, Luxi Xian, Wuding Xian, Lufeng Xian, Luliang Xian, and Malong Xian.
[g] Urumqi, no zone: Fukang Xian, Hutubi Xian, Manasi Xian, and Shawan Xian.
[h] Xi'an, no zone: Lingtong district, Zhouzhi Xian, Xingping Xian, Sanyuan Xian, Jingyang Xian, Qian Xian, Liquan Xian, Yongshou Xian, Chunhua Xian, Mei Xian, Fufeng Xian, Pucheng Xian, Fuping Xian, and Tongchuan Shi Shixaqu.

# APPENDIX B: SUPPLEMENTAL TABLES

TABLE B1
Summary Statistics for Matched Sample: Coastal Region

| | Never Targeted | 1980 | 1984–1987 | 1988–1994 | 1995 or Later |
|---|---|---|---|---|---|
| Urban areas, % | 4.350 (15.34) | 3.002 (14.28) | 4.409 (17.47) | 3.400 (13.77) | 1.861 (10.56) |
| Agricultural productivity | 0.609 (0.208) | 0.849 (0) | 0.816 (0.172) | 0.710 (0.246) | 0.660 (0.202) |
| ln(Median elevation) | 2.963 (1.536) | 3.243 (1.124) | 2.727 (1.748) | 3.481 (1.827) | 4.798 (1.693) |
| ln(Median slope) | 0.528 (1.583) | 1.249 (1.050) | 0.515 (1.518) | 0.931 (1.592) | 1.752 (1.404) |
| Observations | 11,210 | 289 | 2,791 | 25,535 | 6,502 |

*Note:* For the 1980 zone, the agricultural productivity layer did not have coverage. Therefore, we assigned the county level average for this zone, which means it has only one observation and no standard deviation. Standard deviations are in parentheses.

TABLE B2
Summary Statistics for Matched Sample: West Region

| | Never Targeted | 1988–1994 | 1995 or Later |
|---|---|---|---|
| Urban areas, % | 4.697 (16.11) | 6.682 (19.41) | 5.145 (16.83) |
| Agricultural productivity | 0.347 (0.372) | 0.601 (0.371) | 0.707 (0.367) |
| ln(Median elevation) | 6.701 (0.632) | 6.620 (0.626) | 6.182 (0.205) |
| ln(Median slope) | 0.786 (1.405) | 0.740 (1.386) | 0.561 (1.109) |
| Observations | 11,210 | 25,535 | 6,502 |

*Note:* Standard deviations are in parentheses.

TABLE B3
Estimation of Equation [1] by Region

| | (1) Coast | (2) Coast | (3) Coast | (4) West | (5) West | (6) West | (7) West |
|---|---|---|---|---|---|---|---|
| 1984 | 1.942 | 1.547 | 2.130 | 3.416 | | 4.121 | 2.577 |
| | (1.298) | (1.150) | (2.059) | (6.272) | | (5.218) | (4.979) |
| 1988 | 1.068* | 0.803* | 0.349 | 2.076 | 2.076 | 2.353 | 3.622** |
| | (0.543) | (0.439) | (0.703) | (1.263) | (1.263) | (1.814) | (1.613) |
| 1995 | 2.625** | 1.666 | 1.904 | 6.268** | 6.268** | 5.059* | 7.183* |
| | (1.137) | (1.029) | (1.631) | (2.732) | (2.732) | (2.830) | (3.670) |
| 2000 | 3.835** | 2.689* | 3.264 | 1.036 | 1.036 | 4.681 | 5.700 |
| | (1.546) | (1.442) | (2.212) | (5.837) | (5.837) | (4.220) | (5.822) |
| 2003 | 5.181** | 3.963* | 4.661 | 2.649 | 2.649 | 6.501 | 7.392 |
| | (1.955) | (1.988) | (2.766) | (5.865) | (5.865) | (4.726) | (5.993) |
| 2006 | 8.265*** | 6.792*** | 6.880** | 1.665 | 1.665 | 7.394 | 7.364 |
| | (2.346) | (2.251) | (3.286) | (6.636) | (6.636) | (5.378) | (6.199) |
| 2009 | 10.65*** | 9.270*** | 11.03*** | 1.095 | 1.095 | 7.572 | 7.579 |
| | (2.794) | (2.730) | (3.883) | (7.693) | (7.693) | (6.978) | (7.305) |
| 1988 × Policy 1984 | 2.631*** | 3.018*** | 3.077*** | | | | |
| | (0.411) | (0.230) | (0.376) | | | | |
| 1995 × Policy 1984 | 4.054*** | 5.891*** | 3.709** | | | | |
| | (1.292) | (0.646) | (1.562) | | | | |
| 2000 × Policy 1984 | 4.177** | 6.537*** | 3.635* | | | | |
| | (1.691) | (0.863) | (1.918) | | | | |
| 2003 × Policy 1984 | 6.455*** | 9.211*** | 5.315** | | | | |
| | (1.868) | (0.982) | (2.488) | | | | |
| 2006 × Policy 1984 | 8.750*** | 12.10*** | 8.252*** | | | | |
| | (2.129) | (1.104) | (2.858) | | | | |

*(table continued on following page)*

TABLE B3
Estimation of Equation [1] by Region *(continued)*

| | (1) Coast | (2) Coast | (3) Coast | (4) West | (5) West | (6) West | (7) West |
|---|---|---|---|---|---|---|---|
| 2009 × Policy 1984 | 8.243*** | 11.76*** | 6.113 | | | | |
| | (2.218) | (1.200) | (3.724) | | | | |
| 1988 × Policy 1988 | − 0.0410 | 0.522 | 0.707* | | | | |
| | (0.471) | (0.381) | (0.391) | | | | |
| 1995 × Policy 1988 | − 1.436 | 0.722 | − 1.146 | | | | |
| | (1.240) | (0.572) | (1.439) | | | | |
| 2000 × Policy 1988 | − 1.670 | 1.079 | − 1.433 | | | | |
| | (1.678) | (0.987) | (1.889) | | | | |
| 2003 × Policy 1988 | − 1.960 | 1.190 | − 2.447 | | | | |
| | (2.027) | (1.479) | (2.683) | | | | |
| 2006 × Policy 1988 | − 2.408 | 1.557 | − 2.088 | | | | |
| | (2.276) | (1.523) | (2.988) | | | | |
| 2009 × Policy 1988 | − 2.529 | 1.225 | − 4.330 | | | | |
| | (2.550) | (1.970) | (3.966) | | | | |
| 1984 × Policy 1995 | − 0.527 | 2.364* | 2.498* | − 0.606 | | 0.590 | 0.729 |
| | (0.757) | (1.255) | (1.379) | (0.865) | | (0.794) | (0.538) |
| 1988 × Policy 1995 | − 0.167 | 0.559 | 1.129** | 0.0729 | 0.0729 | 0.675 | 1.094** |
| | (0.280) | (0.360) | (0.409) | (0.425) | (0.425) | (0.628) | (0.529) |
| 1995 × Policy 1995 | − 0.981 | 2.066* | 1.653 | − 0.375 | − 0.375 | 0.447 | 1.188*** |
| | (0.704) | (1.170) | (1.600) | (0.710) | (0.710) | (0.598) | (0.308) |
| 2000 × Policy 1995 | − 1.192 | 2.762* | 2.231 | 0.776 | 0.776 | 3.965*** | 4.764*** |
| | (0.889) | (1.544) | (2.037) | (0.935) | (0.935) | (0.730) | (0.491) |
| 2003 × Policy 1995 | − 1.451 | 2.988* | 1.631 | 1.296 | 1.296 | 4.105*** | 5.154*** |
| | (1.071) | (1.725) | (2.629) | (1.055) | (1.055) | (0.783) | (0.464) |
| 2006 × Policy 1995 | − 1.693 | 3.762* | 2.880 | 1.742 | 1.742 | 5.080*** | 6.135*** |
| | (1.310) | (1.997) | (3.127) | (1.175) | (1.175) | (0.873) | (0.647) |
| 2009 × Policy 1995 | − 2.024 | 3.305 | 0.402 | 1.791 | 1.291 | 5.771*** | 7.002*** |
| | (1.485) | (2.083) | (3.995) | (1.510) | (1.510) | (1.319) | (1.196) |
| 1984 × Policy 2000 | − 1.925 | | | − 1.497 | | | |
| | (1.306) | | | (1.092) | | | |
| 1988 × Policy 2000 | − 0.192 | | | 0.521 | − 0.521 | | |
| | (0.446) | | | (0.572) | (0.572) | | |
| 1995 × Policy 2000 | − 2.889* | | | − 1.015 | − 1.015 | | |
| | (1.586) | | | (1.338) | (1.338) | | |
| 2000 × Policy 2000 | − 2.889* | | | − 1.015 | − 1.015 | | |
| | (1.586) | | | (1.338) | (1.338) | | |
| 2003 × Policy 2000 | − 3.875** | | | − 0.839 | − 0.839 | | |
| | (1.777) | | | (1.789) | (1.789) | | |
| 2006 × Policy 2000 | − 4.650** | | | − 0.0724 | 0.0724 | | |
| | (2.088) | | | (2.816) | (2.816) | | |
| 2009 × Policy 2000 | − 5.760** | | | − 0.165 | − 0.165 | | |
| | (2.332) | | | (4.016) | (4.016) | | |
| $R^2$ | 0.0879 | 0.0903 | 0.137 | 0.0645 | 0.0635 | 0.0672 | 0.0606 |
| $N$ | 173,822 | 173,822 | 75,048 | 162,001 | 147,936 | 162,001 | 92,048 |

*Note:* The dependent variable is the percentage of the sample unit that is urban. Columns (1), (4), and (5) use all zones together (national and provincial), although column (5) runs this specification without the 1984 wave. Columns (2) and (6) recode the provincial zones as zeros, and columns (3) and (7) drop provincial zones altogether. Standard errors are in parentheses.

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

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