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Who really benefits from export processing zones? Evidence from Nicaraguan municipalities☆

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HIGHLIGHTS

- Increase in average levels of expenditure per capita of about 10% overall
- Positive average effects hide large variations across the expenditure distribution.
- Main beneficiaries of policy are at the top of the distribution in treated areas.
- Heterogeneous time dynamics for the rest of the segments
- Bulk is concentrated on the high skill working age group, suggesting skill premium.

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ABSTRACT

Export processing zones are popular trade policies in developing countries, but there is limited empirical evidence on their local effects. This paper examines the impact of their establishment on the levels of per capita expenditure across Nicaraguan municipalities for the period 1993 to 2009. Using the time and cross-section variation of park openings in a difference-in-differences framework, I find that on average consumption levels increased by 10 to 12% in treated municipalities. Yet, average effects mask significant disparities across the expenditure distribution. The results suggest that the policy benefited the upper-tail the most: expenditure levels increased by up to 25% at the 90th percentile. At the opposite of the distribution, only the bottom decile registered a small positive effect of close to 10% across the period.

1. Introduction

The use of Export Processing Zones (EPZs)[1] as a policy for trade and economic development has exponentially grown in the past decades, particularly in developing countries. In 1986, the International Labour

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[1] When discussing EPZs, a variety of terminologies, such as Industrial Free Zones, Free Trade Zones, Special Economic Zones and Maquiladoras are used interchangeably in the literature. Although each has their own particularity, we will consider the broad definition of them being "demarcated geographic areas contained within a country's national boundaries where the rules of business are different from those that prevail in the national territory. These differential rules principally deal with investment conditions, international trade and customs, taxation, and the regulatory environment; whereby the zone is given a business environment that is intended to be more liberal from a policy perspective and more effective from an administrative perspective than that of the national territory" (Farole and Akinci, 2011:23).

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Organization (ILO) estimated that there were 176 zones in 47 countries. By 2008, the number reached more than 3000 zones in 135 countries, accounting for over 40 million direct jobs and over US$200 billion in global exports (Farole and Akinci, 2011). Despite the importance of the phenomenon, there is surprisingly little empirical evidence for evaluating these programmes in the context of developing countries.[2] Further, the focus has remained on aggregate and traditional outcomes (FDI, exports, firm dynamics), and little attention has been paid to understanding the welfare and distributional effects resulting from the establishment of EPZs.

To fill this gap this study takes advantage of the gradual establishment of EPZs in Nicaraguan municipalities during the period 1993–2009 to assess the average effect and the distributional pattern of this spatially-bound policy within the host municipalities. Nicaragua provides an excellent setting to study this phenomenon. The number of firms operating under the regime has increased markedly since its inception in the early 1990s, across more than 20 of the 153 municipalities of the country. By 2010, EPZs accounted for 50% of total exports, and nearly 90% of manufacturing exports. The government considers that in the same year, EPZs jobs represented 25% of total formal work across the country, with on average firms directly employing around 7.5% of the total labour force of the municipalities where they locate.[3] Yet, no empirical assessment exists.

While having a wider reach than traditional place-based policies (i.e. diversify exports, increase FDI), EPZ programmes are prone to have a significant influence on the local economies (Wang, 2013) as they operate with incentives to hire and create economic activity in or near the areas where they locate fostering agglomeration economies (Combes et al., 2010). In this sense, local welfare effects might differ substantially from those at the aggregate level, particularly in cases with labour market frictions. Labour mobility and land-price responses may be such that the jobs created go to non-poor residents and that the gains from land prices benefit higher-income segments (Neumark and Simpson, 2014). As stressed by Kline and Moretti (2014) and shown by Reynolds and Rohlin (2015) for the case of US federal empowerment zones, positive average effects of spatially-tied policies can mask significant disparities in terms of the actual beneficiaries across the income distribution in treated areas. There are heterogeneous effects according to whether individuals are homeowners or renters, or more generally by skill and initial income levels, that are not necessarily captured by looking at the average effect on local wages (Neumark and Simpson, 2014). In this sense, disentangling in what way the establishment of an EPZ profits the different segments of the income distribution within concerned areas helps to shed light on the mechanisms of the policy and ultimate local beneficiaries. The question is of great policy relevance in developing economies with already large levels of overall income disparities.

For the analysis here, I exploit a repeated cross-sectional dataset based on official household surveys and construct a unique municipal-level panel that allows for the examination of different moments of the expenditure distribution before and after the policy implementation. By focusing on the aggregate outcomes at the municipal level and on household per capita expenditures, the study captures general equilibrium effects of EPZ establishment within a locality.[4] The identification strategy is straightforward. I use both the time and cross-section variation of zones' establishment across municipalities to estimate their average effect on the levels of real expenditure per capita of working age individuals, and across all the deciles of the expenditure distribution in treated municipalities. In the main approach, I use a difference-in-differences (DID) strategy extended to quantiles (Athey and Imbens, 2006). An important feature of the overall empirical strategy needs to be emphasized. The approach does not aim at measuring the impact of the EPZ policy on inequality across Nicaragua as a whole. Rather, it looks at the question of knowing whether within municipalities exposed to the establishment of EPZs, certain segments of the distribution capture more or less of the resulting gains or losses.

This analysis builds on two complementary bodies of research. Mostly, it borrows from the large literature that has looked at evaluating place-based programmes in the US and Europe (Busso et al., 2013; Neumark and Kolko, 2010), and adds to the nascent counterpart studying the impact of EPZ policies at the subnational level in developing countries. Findings in these papers are revealing of the extent to which local economies are influenced by the establishment of an exporting zone. Using employment data and census statistics on educational outcomes at the municipal-level for Mexico, Atkin (2012) finds an increase in school dropouts in concerned municipalities following the arrival of new better-paying export jobs. Similar in strategy to this paper, Wang (2013) shows that Chinese Special Economic Zones (SEZ) benefit local economies through higher levels and growth rates of per capita FDI and total factor productivity (TFP), as well as higher average wages that compensate any increase in the local cost of living. Additionally, this paper adds empirical evidence to the literature analysing the effects of trade policies on local labour markets in developing countries. This area of research has emphasized the importance of understanding the impact of trade policies at the local level, particularly in cases of limited factor mobility (see Goldberg and Pavcnik, 2007 for an extensive review).

The validity of my findings rely on the assumption that the different empirical methods successfully account for the underlying differences in municipalities' characteristics that are likely to explain the non-random choice of EPZs location across time and space. In the absence of a credible instrument, I take care of unobservable confounders by allowing for time and municipalities fixed-effects across specifications. I also include province and region-time dummies to control in a flexible manner for the fact that EPZs concentrate in the western part of the country. Further, I use information on accessibility and socio-economic indicators to build alternative sets of control municipalities that are likely to be more meaningful comparable groups under the DID setting. My preferred control group is balanced on covariates and pre-treatment trends of outcomes, and the assumption of parallel trends of main outcomes holds.

Further threats to validity concern possible spillover dynamics and the relocation of individuals across treated and control areas. I address these concerns in several ways. First, I test for the existence of spillovers on the outcomes variables of neighbouring areas using varying degrees of distance. I find no evidence supporting large firm relocations or commuting flows. This is consistent with the literature registering small backward linkages generated by EPZ policies, and the low commuting of the Nicaraguan labour force (Jansen et al., 2007). The potential threat from labour mobility is harder to address. There are two dimensions to consider: in and out-migration. Both may have compositional effects that may alter firms location decisions (related on skills and local wages effects) as well as outcomes. Reassuringly, I find no evidence of large population readjustments across municipalities when measuring the effect of the policy on the likelihood of migrating for the working-age sample. Results are also unchanged when adding baseline population weights. Although, I find evidence of compositional changes towards an increase in the share of the high-to-low skill ratios in treated municipalities, the size is too small to challenge the identification validity, and if anything, contributes to elucidating the mechanisms of the policy. Finally, the results are also robust when excluding the capital city and municipality of Managua.

[2] Aside notable exceptions, the literature is limited to case studies and macro-economic reviews. See Farole and Akinci, 2011; Engman et al., 2007; Aggarwal, 2006 and Glick and Roubaud, 2006, for recent examples.

[3] Data obtained from the Comisión Nacional de Zonas Francas (CNZF) and Nicaragua's Central Bank (BCN).

[4] In the absence of disaggregated property prices and firm-level data, using expenditure data remains the best strategy to capture the net effect of EPZ establishment. A similar strategy was used by Topalova (2010) to measure the local effect of trade liberalization within Indian districts.

The analysis reveals interesting conclusions concerning average and distributional dynamics of EPZ policies at the local level. First, I confirm that there is a positive average treatment effect on the levels of real per capita expenditure in treated municipalities. The magnitude is small but non- negligible, with an average 10 to 12% increase in treated areas for the entire period. Second, I find that the mean effect hides significant disparities across the distribution. Results offer suggestive evidence that those at the upper-tail of the distribution are the main beneficiaries of the policy over the period covered. This effect is incremental with the more years of EPZ operation. The impact is heterogeneous in time for the remaining segments, significant at middle-range deciles after eight years of an EPZ establishment. These results are consistent with the existence of a skill-premium in the exporting sector that may be altering the cost of living (i.e. land price responses) in profit of the higher-skill/higher-income segments. Indeed, I find that the average positive gains are concentrated in the high-skill working-age group.

The remainder of the paper is organized as follows. Section 2 describes the EPZ programme in Nicaragua. Section 3 introduces and examines the data, and Section 4 discusses the empirical strategy. Results are discussed in Section 5, with extensions and robustness checks shown in Section 6. The final section concludes.

2. Nicaragua's export processing zone programme

2.1. Ley de Zona Franca: legal procedures & tax exemptions

Resembling analogous initiatives, Nicaragua's EPZ regime ('Zonas Francas') was put in place with the aim of generating employment, attracting FDI, increasing non-traditional exports, acquiring new technologies and expanding international trade. The programme is relatively young compared to other Central American countries as an earlier initiative was abandoned for a decade during the civil conflict (1979–1990) and was only reintroduced following the shift to democracy and the market economy in 1990. In 1991, Decree N.46–91 formally established the existing regime, with the appropriate regulation passed in the following year (Decree N. 31–92). Together, these two decrees created the legal framework concerning the organization of EPZs, the fiscal incentives, the rights and obligations of operators and users, as well as the regulatory body in the form of the National Commission of Free Trade Zones (CNZF). The law was amended over the years, with its most notable reform aimed at simplifying procedures taking place in 2005 (Decree N. 50–2005).

Under the Nicaraguan legislation, EPZ status is granted to foreign or national private firms oriented towards the export of goods and services that meet a certain array of criteria and follow the official procedures with the CNZF. There have been many efforts to expedite the legal process of application in the recent years (i.e. establishment of a one-stop shop after 2005). Firms applying to be recognized under the EPZ regime need to provide detailed information of their project (which includes financial plans, market analysis, exporting markets, construction or building agreements, estimates of job creation, among others). They also need to comply with environmental, building and health regulations, and the project must be approved by the mayoral office in the municipality where they intend to locate. There is no explicit rule regarding the age of the firm (new or pre-existing).

Following standard practice, the programme is based on attracting and generating business by offering particular tax incentives to qualifying firms. These include the full exemption from payment of (1) Income Tax during the first ten years of operation (and 60% from the eleventh year onwards); (2) all taxes and custom duties of machinery, equipment and intermediate goods, as well as of transportation and support services for the zones (including for instance equipment needed for installation of health care or child care); (3) indirect taxes, taxes on selective sales or consumption; (4) export taxes on processed products made within the regime; (5) municipal taxes on property sales, including the tax on capital gains if any; and (6) municipal taxes. The regime also includes the unrestricted repatriation of capital. In exchange, firms have to pay an annual fee to the CNZF, determined according to the industrial sector and sq. metres of occupied area.

Under Nicaraguan law, EPZs are allowed to sell intermediate goods and inputs to other exporting firms in the country, provided they pay custom duties. However, EPZs are strictly forbidden from selling final goods in the local market. The legislation also provides conditions under which EPZ developers may benefit from tax exemptions, as well as the possibility for them to subcontract to local Nicaraguan firms which are then exempted from VAT payment. The restriction to sell in the local market and the barriers to sell intermediate inputs means that in terms of production the larger effect of EPZs is contained to the exporting sector, without hampering local producers. The possibility to outsource to local providers creates some incentives to generate backward linkages, and any firm relocation or diversion is likely to happen through this indirect mechanism. I expect these to be low. Backward linkages with local firms have been found to be very low in Latin America (Jenkins et al., 1998). The share of domestic expenditures as a share of total value of EPZ exports in Nicaragua has ranged between 25 to 30%, with wages constituting the bulk of the shares (ECLAC, 2012). I formally address this issue in Section 6.1.

Nicaraguan labour laws regulate work in EPZs and minimal wages have been in place since 1999. The country has the lowest manufacturing wages in the region, which is a source of its comparative advantage. Still, minimum wages in EPZs have traditionally been higher than the overall manufacturing and agricultural sectors throughout the period (20 to 70% higher on average per year).

2.2. Definition & characterization of EPZs

The law distinguishes three different categories: administrators (industrial park operators), users (situated within industrial parks) and 'single-factory' EPZs. The distinction is relatively artificial. Operators and users of industrial parks operate in the same area, with the former only providing a service to the using firms and most of time owning some firms within the park. The distinction serves administrative purposes for the payment of different types of fees to the CNZF. Further, the single-factory category is mostly used by large multinational corporations with a wide range of functions. Examples of these are New Holland Apparel, a large US manufacturing firm that produces sportswear for Nike, or Yazaki, a Korean firm in the area of light manufacturing considered to be the single largest employer in the country. Each of these firms employs about 2 to 8 thousands workers, respectively (2013). In this sense, despite the different legal definitions, industrial parks made of smaller firms are thus essentially equivalent in terms of employment and export volume to stand-along EPZ firms. As such, I do not distinguish between the different categories.[5] For the empirical analysis, I define EPZs as stand-alone firms or an industrial park hosting one or more firms, and reported by the CNZF at year end.[6] By this definition, the total number of EPZs in my sample is 64 by 2009,[7] located in 20 different municipalities across 10 provinces.[8] Fig. 1 depicts the pattern of localization across municipalities according to the sequence of establishment.

In terms of numbers of firms and parks, a larger share is concentrated in the capital city Managua (37%), the adjacent provinces of Masaya

[5] Because I do not have access to firm-level data (workers, exports, sales or value-added), I cannot corroborate or disentangle EPZ categories with a continuous treatment variable. Any attempt to categorize EPZs in terms of intensity would likely be misspecified.

[6] I compiled information on EPZs location by the end of each year, origin and sector of operation from the CNZF annual yearbooks. When possible, I cross-checked the information with the EPZ website profile. See Appendix II for more details.

[7] The discrepancy with the official figure of +150 stems from this definition, as I count industrial parks as one irrespective of the quantity of firms. Since the launch of the programme, by this definition, the number of EPZs increased from 1 in 1993 to 64 (+150) in 2009.

[8] There are 153 municipalities, 15 provinces and 2 autonomous regions in Nicaragua.

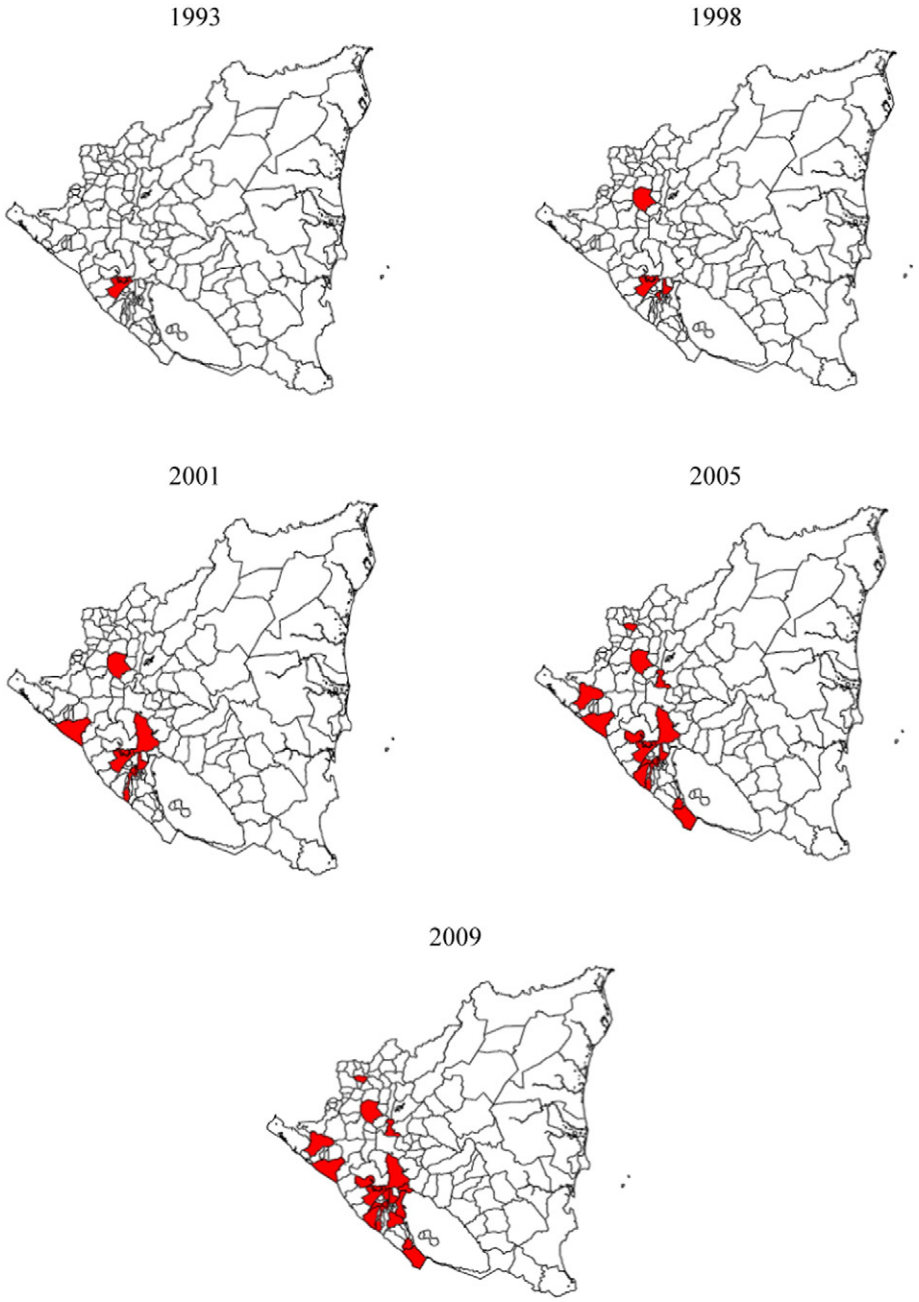

Fig. 1. Gradual establishment of EPZs in Nicaraguan Municipalities (1993–2009). *Notes*: GIS digital maps. Municipalities are highlighted following the establishment of an industrial park or single factory under the EPZ regime. During the period considered some municipalities may contain more than one firm or industrial park. Data was compiled from the annual yearbooks of the Comision Nacional de Zonas Francas (CNZF).

and Carazo (20%), and the north-eastern provinces of Estelí and Chinandega (18%). These provinces also represent 55% of the countries' total population (2012). 76% of EPZs are foreign, the majority of which (40%) are from the US, followed by South Korea (22%). According to official statistics for 2010 (CNZF), American EPZs employed 35% of the total workforce in the sector, followed by South Korean which employed about 30%. Nicaraguan EPZs are attributed only 5% of the workforce in the sector, which implies little firm relocation from previously operating firms. There is no disaggregated statistics on the number of employees within each EPZs. According to the World Bank Enterprise Surveys, only 12% of domestic firms exported in 2010. Near to 90% of these EPZs operate in the manufacturing sector, with a predominance of firms producing textile, apparel and light electronics, followed by cigar production and other agri-businesses. 25 firms are categorized as single factories, the majority of which are also in the apparel and light manufacturing sectors. All of the agri-business EPZs are stand-alone firms. Both the average years of stand-alone firms and industrial parks is close to 5 years, with standard deviations of 3.5 and 3.4 years, respectively. Attrition is relatively low, with about 5% of the EPZs having closed during the period considered, but some measurement error is possible due to the difficulty in tracking earlier EPZs.

In theory, eligible firms can locate anywhere in the country. The regime does not institute a particular area of the territory as being a free-trade zone, but rather grants the status to the firm, which needs to negotiate directly with the municipality (irrespective of locating as a stand-alone firm or within an industrial park, a letter of acceptance from the mayoral office of the relevant municipality is a pre-condition, CZNF 2015). In practice, not all municipalities are prone to receiving

an exporting zone, and the export-oriented nature of the firms has made the western area of the country the natural host.

This dimension underlines the 'de-facto' place-based nature of the EPZ regime in Nicaragua. Place-based policies are defined as policies that particularly target geographic areas for some form of special treatment including special regulations or tax exemptions (Kline and Moretti, 2014), in order to create incentives to hire or generate local economic activity. While in Nicaragua privileges are granted to the firm or industrial park, the location decision is subject to the municipal approval. Once established, EPZs are spatially-fixed, and their propensity to hire or outsource in the area where they operate implies that the local economy will be directly impacted through labour, capital, land and price channels. This is further emphasized in the case of industrial parks that remain spatially-bound but can increase the number of firms located within. In practice, the establishment of EPZs in a given municipality has the same local effects than a traditional place-based policy (Wang, 2013, Atkin, 2012). Limitations to factor mobility may emphasize this dimension. The trade literature has stressed the importance of analysing the effect of trade policies in local and regional labour markets (Autor et al., 2013, Kovak, 2013, Topalova, 2010), particularly due to the lack of perfect factor mobility across geographical areas in both developed and developing countries.

For the empirical study, I set a general EPZ dummy variable, EPZ_{mt}, equal to one if an industrial park or stand-alone firm was authorized within the municipality, and zero otherwise. I discuss the relevance of using this level of analysis further in the next section.

3. Data

3.1. Datasets

The data for this analysis were drawn from several sources. First, I compiled a repeated cross-sectional dataset for the period 1993–2009 based on five LSMS Household Surveys[9] (Encuesta Nacional de Hogares para la Medición del Nivel de Vida, EMNV) collected by the Nicaraguan Institute of Statistics (Instituto Nacional de Información de Desarrollo, INIDE), and representative at the national, regional, urban and rural levels. These contain detailed information on living standards and various other households and individual socio-demographic characteristics. I use this information to construct a panel at the municipal-level and calculate expenditure deciles at this level of aggregation. These measures include a correction for the sample bias using sampling weights. Municipalities with less than 30 households per period were excluded to reduce measurement error. Overall, the sample comprises observations for an unbalanced panel of between 82 to 126 municipalities per year — of which 61 are observed for the full period, and 107 are observed four times (Table 1).[10] The data on EPZs was compiled from various CNZF annual yearbooks, which only contain information on location and sector of operation of each zone at year end.[11] Data on accessibility measures (road density, ports, airports and railways) was compiled from reports of the Ministry of Infrastructure and Transport (MTI)[12] and World Bank logistics' assessments (2004–2011).

3.2. Main definitions

This analysis follows common practice in referring to the measure of living standards as income, which should be interpreted broadly to encompass all the characteristics associated with geographical location including climate or local public good provision, in order to assume that individuals with the same level of income at different locations are equally well-off (Shorrocks and Wan, 2005). Here, following best practice income is proxied by consumption expenditure (Goldberg and Pavcnik, 2007). The choice for using consumption expenditure as a measure of income has been widely discussed in the literature. It is arguably the most appropriate variable for capturing lifetime wellbeing (Deaton, 1997) as it better captures intertemporal shifts of resources and incorporates changes in purchasing power. Expenditure data are of better quality in LSMS Household Surveys (Deaton, 2005; Banerjee and Duflo, 2007), given that reporting problems are less pronounced and consumption is less affected by the redesigning of surveys, which hampers comparability across years. The large period considered in this analysis, and the existence of a large informal sector (73% of all jobs in 2010) in Nicaragua also motivate the use of expenditure levels instead of labour income. Further, wages are under-reported in the household surveys, particularly in 1993 and 2001 when only between 10 to 20% of working age members report a value. There is no information on the location of employment of workers in the household surveys, and I have no access to data on housing prices within municipalities. These data limitations further justify the use of expenditure per capita as the best approximation of a welfare measure that encompasses multiple dimension across time and unit of analysis.

I use constant annual per capita expenditure (in Córdobas of 1999, C$),[13] defined as the household annual net expenditure divided by the number of persons in the household. All households surveyed and all of their members are included (though consumption is adjusted depending on members being children or adults). Missing values and zero incomes have been excluded. Additionally, expenditure has been adjusted for difference in prices across provinces every year using local price indices in the households' surveys. Adjusting for spatial price differences will not alter inequality within regions, and are an important factor to take into account given the strong correlation of prices with welfare levels (Shorrocks and Wan, 2005).

As mentioned, to analyse the moments of the expenditure distribution I follow Milanovic (2005), Milanovic and Ersado (2009) and partition the real per capita expenditure distribution by deciles at the municipal level using sampling weights to correct for sample bias. This method allows me to focus on the pattern of change across all the deciles shares of the distribution, and has the advantage of decomposing the average effect. This method is used for other household characteristics. There are limitations to using this approach for small areas using household survey data. However, I take several measures to control for the quality of the values, such as excluding municipalities with too few observations, and comparing the municipal averages with census data available for 1993 and 2005. Results approximate well. Tables A.1

Table 1
Sample description (cross section and panel datasets).

Sample level	1993	1998	2001	2005	2009	Total
Municipalities	80	115	114	126	92	527
with EPZs	1	3	9	16	18	47
Individuals	24,892	23,182	22,298	36,183	29,749	136,304
Individuals (≥15 years)	13,689	13,185	13,306	22,146	20,146	82,472
Households	4412	4125	4075	6779	6375	25,766

Notes: Not all municipalities are observed every year. Municipalities with less than 30 households each year were dropped from the sample. The minimum number of households per municipality in a given year is 30 and the max excluding Managua is 408 (Managua's max. is 2004).

[9] The Living Standards Measurement Surveys were developed by the World Bank to improve the type and quality of household data collected by statistical offices in developing countries. The years of each EMNV are: 1993, 1998, 2001, 2005, and 2009.

[10] 20 municipalities are omitted because of N < 30 rule across the years. As a robustness check, I run the different specifications with a balanced panel, omitting observations observed less than 4 times. Conclusions remain unchanged.

[11] See Data Appendix II for more details.

[12] Ministerio de Transporte e Infraestructura (MTI) Red Vial de Nicaragua.

[13] I use the Managua CPI from the Central Bank of Nicaragua (CBN) as the overall country CPI is not available before 2000. The CPI is a Laspeyres-type (1999 = 100) index compiled on the basis of the Household Income and Expenditure Survey of 1998–1999. 15 departmental capitals and two autonomous regions were used to select the products to be included in the various baskets and the corresponding weights. These include durables and non-durables as well as residential rents. A summary methodology is available in the CBN Boletín Económico, Volume II, Number 2, April - June 2000 (IMF).

Table 2
Summary statistics main outcome variables (1993–2009).

Variable	Mean	Std. dev	*N*
Average real per capita expenditure, annual	6642.68	7268.44	81,249
Individuals reporting having migrated in the past 5 years	0.15	0.36	82,472
Average real per capita expenditure, annual (municipality)	4824.33	2390.96	527
10th percentile - real per capita expenditure, annual	1976.24	1065.97	527
30th percentile - real per capita expenditure, annual	2890.912	1477.893	527
50th percentile - real per capita expenditure, annual	3890.79	1953.95	527
70th percentile - real per capita expenditure, annual	5289.22	2696.75	527
90th percentile - real per capita expenditure, annual	8792.86	5094.79	527
Share of low-skill (definition 1)	0.34	0.11	527
Share of high-skill (definition 1)	0.09	0.09	527
Share of low-skill (definition 2)	0.37	0.21	527
Share of high-skill (definition 2)	0.12	0.11	527
EPZ Neighbouring Municipalities (5 to 15 km)	0.05	0.21	527
EPZ Neighbouring Municipalities (15 to 30 km)	0.13	0.34	527
Migrant in the past 5 years (municipality share)	0.10	0.12	527

Notes: The first two variables are measured at the working-age individual level. Expenditure measures are expressed in constant Cordobas of 1999. The share of low and high skill according to definition 1 are determined with respect to the skill distribution of the province; definition 2 is defined with respect to years of formal education. See Section 6 for further details. The share of migrants is estimated by individuals reporting having migrated to the current municipalities in the previous 5 years. The EPZ neighbouring municipalities represents the share of municipalities at the given km distance to a treated municipality.

and A.2 in Appendix I contain descriptive statistics at the decile level according to treatment and control groups for the beginning and end of the period studied. These are reassuring in that all deciles display a similar evolution across the period, with the ratios between treated and control groups remaining constant in time. As expected individuals at higher deciles display higher levels of educational achievement, access to electricity and a smaller proportion is employed in the informal sector. Generally, the differences within deciles between treatment and control groups are small. Particularly, household size, average age and individuals at working age are consistently comparable and evolve at a similar ratio.

Because of the characteristics discussed in Section 2, it seems logical to consider municipalities as the unit of analysis. They are the best approximation of the actual functional area at which firms and zones operate, and a fair representation of the local labour market. Labour mobility in Nicaragua is limited, not only because of the low-skill level of the labour force, but because commuting distances are found to be an important deterrent to participate in the labour market (Jansen et al., 2007). Excluding Managua, municipalities had a median population of 20 thousands in 2012. Additionally, they are the ultimate discrete policy unit for which welfare data is available and they remain the reference framework for delivering public services and distributing government funds (Lall and Chakravorty, 2005).

3.3. Descriptive statistics

This section discusses key summary statistics for the main outcomes and control variables used in the analysis. Table 2 displays period averages for selected outcomes. They include both municipal-level variables, and averages for the working age population of the pooled cross-sectional dataset. Table 3 presents descriptive statistics of municipality characteristics according to treatment status and sequence of EPZ establishment. Values are period averages. The table examines whether or not early zones exhibit different characteristics relative to later zones, and both compared to municipalities never having authorized the establishment of an exporting zone. For this, I group the municipalities based on the establishment waves: group 1 [<1999] is composed of four municipalities that were exposed to EPZs by 1999, it includes

Table 3
Summary statistics of the EPZ by establishment sequence (1993–2009).

Timing	Group 1 [<1999]		Group 2 [2000–2001]		Group 3 [2002–2005]		Group 4 [2006–2009]		Municipalities Never EPZ	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
A. Granting sequence										
Municipalities with newly established EPZs.	4		5		7		2			
Total municipalities with EPZs.	4		9		16		18		108*	
B. Municipality characteristics										
Distance to Managua (km)	56.70	51.44	43.09	76.25	84.84	48.07	35.88	13.28	142.77	77.85
Road density (m)	91.30	28.51	92.33	24.50	80.68	29.66	225.42	109.39	46.24	40.69
Landlocked (=1)	0.53	0.51	0.38	0.50	0.57	0.50	0.36	0.49	0.46	0.50
Main trade access (=1 if main port, airport or trade post in municipality)	0.20	0.32	0.21	0.41	0.20	0.41	0.64	0.50	0.31	0.46
Electricity (% households)	0.99	0.01	0.97	0.07	0.94	0.14	0.97	0.03	0.97	0.09
Completed primary education	0.27	0.03	0.29	0.03	0.28	0.44	0.29	0.03	0.26	0.06
Illiterate	0.13	0.06	0.18	0.10	0.20	0.12	0.18	0.08	0.31	0.16
Economically active population	0.61	0.05	0.59	0.42	0.58	0.07	0.57	0.05	0.54	0.07
Unemployment	0.07	0.06	0.10	0.07	0.11	0.09	0.11	0.09	0.08	0.07
Manufacturing (% employed)	0.23	0.05	0.21	0.08	0.17	0.08	0.17	0.03	0.12	0.08
Agriculture (%employed)	0.08	0.11	0.16	0.15	0.30	0.30	0.18	0.04	0.55	0.27
Urban	0.90	0.11	0.74	0.18	0.57	0.33	0.41	0.30	0.35	0.34
Migrants in past five years	0.10	0.14	0.08	0.97	0.07	0.09	0.07	0.08	0.09	0.12

Notes: Municipalities are grouped based on the sequence of EPZ establishment. Values are for sample averages across the period. Distance to Managua measures the centroids' distance of a municipality to the capital city, the measure excludes Managua; road density corresponds to metres of paved roads every 1 sq.km; main trade access is a composite dummy that equal one if a municipality has a major port, airport or trade post; economically active population corresponds to individuals betwene 13 and 68 years old as per the definition of the statistical office; percent employed in different sectors, urban and unemployment levels are census data (1995–2005).Migrants in the past five years is the share of individuals that migrated to the municipality in the last 5 years.*Never EPZ municipalities vary per year, as per the unbalanced nature of the panel. There are between 64 in 1993 to 108 in 2005.

Managua, which is always treated in the sample since the first industrial park was opened in 1994; group 2 [2000–2001] is composed of five municipalities that granted authorization to new EPZs between 1999 and 2001; group 3 [2002–2005] is composed of seven municipalities that authorized establishment during this period; and group 4 [2006–2009] is composed of the final waves of municipalities receiving an exporting zone. Because data was of poor quality (too few observations) I had to exclude two municipalities treated in that last wave.

Overall, municipalities receiving EPZs display significantly better accessibility measures (i.e. density of paved roads, access to airports, ports or trade posts, and proximity to large water bodies), and closer proximity to the municipality of the capital city. Group 4 is a particular outlier, probably due to progress in infrastructure across the period. This trend is to be expected as the choice of location of exporting firms is unlikely to be exogenous and should be highly correlated with accessibility and infrastructure. Similarly, the sectoral compositions also diverge, with non-treated municipalities showing much higher levels of agricultural specialization and a predominance of rural areas. Nicaragua remains overall a rural country, with less than 60% of its population living in urban areas in 2010 (World Bank). The skill composition displays a better distribution across municipalities, differing only in the proportion of illiterate adults which is much lower in EPZs areas (20 vs 30%). On the other hand, all groups show similar levels of access to electricity, unemployment, share of working age populations, and migrants. In this sense, it would seem as if the geographical distribution of economic activities across the country is not necessarily correlated with different labour outcomes. This largely reflects the importance of agriculture for the Nicaraguan economy. Even in 2011, the sector was the main employer (32% of total employment) and represented 19% of total value added (above manufacturing).

Fig. A.1[14] provides a more detailed image of the distribution of the sector of economic activity across the deciles of per capita expenditure by years and treatment groups. Both EPZ and no-EPZ municipalities display similar patterns for services and agriculture, with a higher concentration at the upper-end and lower-end of the distribution, respectively. Discrepancies are noticeable however in manufacturing, construction and transport. EPZ municipalities show a higher concentration in manufacturing at the upper-end of the distribution, despite a general shift towards the median during the period. The opposite is visible in the construction sector. Given the size of the change, it could well reflect the modernization of the sector rather than any trade-related effect.

4. Empirical strategy

The main empirical analysis relies on the difference-in-differences (DID) framework. For the identification strategy, I rely on the variation provided by the timing of zone creation across the sample of municipalities.

4.1. Basic specifications

I define Eqs. (1) and (2) as the extension of the simple DID model for multiple periods and groups, in order to estimate the Average Treatment Effect on the Treated (ATT) using both the pooled individual level dataset and municipal averages. As defined in Section 2.2, I denote EPZ as a dummy variable that switches to one when a municipality m gets treated in year t. The basic specifications take the following forms:

$$Y_{imt} = \gamma_{0m} + \gamma_{1m}.t + \mu_t + \delta EPZ_{mt} + X'_{imt}\beta + \varepsilon_{imt} \quad (1)$$

$$Y_{mt} = \gamma_m + \mu_t + \varphi EPZ_{mt} + Z'_{mt}\theta + \omega_{imt}. \quad (2)$$

[14] Figures and Tables A.1–A.13 are contained in online Appendix I.

Where m and t index for municipality (m = 1 , ... 126) and year (t = 1993, 1998, 2001, 2005, and 2009) respectively, and i indexes for working-age individuals. Y_{mt} is the outcome variable of interest in municipality m at year t; Y_{imt} is the outcome variable of interest for individual i in municipality m at year t; γ_m are municipality-level fixed-effects and μ_t are time effects. The fixed-effects capture the permanent differences in the municipalities' observed and unobserved characteristics that may influence the location of EPZs as well as any time varying shocks. X' and Z' are vectors for time-varying individual and household characteristics. In Eq. (1) they include measures for household size, age squared, and dummies for gender, economic sector of employment, migrant status, education level, access to electricity and urban location. Eq. (2) contains equivalent measures aggregated at the municipal level (i.e. shares of education levels, households with access to electricity, urban population and migrants). ω_{mt} (ε_{imt}) is the error term. Following Bertrand et al. (2004) I use robust standard errors clustered at the municipal level to prevent misleading inference due to serial correlation in the error term across years within a municipality. Results are robust to the clustering of standard errors at the province level to account for possible serial correlation at this level of aggregation.

The parameter of interest are δ and φ which measure the average effect of the establishment of an EPZ on the outcomes Y_{imt} and Y_{mt} (i.e. measures of real expenditure per capita). The effect is identified by comparing municipal outcomes among treated and non-treated groups before and after EPZ establishment. To control even more richly for differences across municipalities, I add a municipality-specific time trend in the form of interactions between a municipality dummy and a linear time trend, γ_{1mt} in Eq. (1). This specification allows for municipalities' unobservable characteristics to follow different trends in time, which is particularly relevant in the case of a gradual treatment. The larger dataset allows introducing the trends without compromising degrees of freedom. I use province-time fixed effects and region-time fixed effects in alternative specification at both individual and municipal level of analysis, to account for sorting across Nicaragua and the concentration of EPZs in the western area of the country.

I measure the effect across the expenditure distribution by extending the DID method to each quantile (τ) instead of the mean (Athey and Imbens, 2006). The specification takes the following form:

$$Y_{mt|EPZ_{mt},\rho_{mt}}(\tau) = \gamma_m(\tau) + \mu_t(\tau) + \psi EPZ_{mt}(\tau) + W'_{mt}(\tau)\,\alpha + \xi_{(\tau,\rho_{mt})}. \quad (3)$$

Where notations are the same as previously defined in the simple DID cases but τ now stands for each decile of the expenditure distribution estimated at the municipality level. All notations are as specified above. I cluster standard errors at both municipal and province levels. This method referred to as the QDID allows comparing individuals across both groups and time according to their quantile. Here, the quantiles are *fixed* and the DID estimator compares changes in the levels of expenditure around them, under the identifying assumption that the growth in expenditure from pre-EPZ to post-EPZ groups at each particular quantile would have been the same in both treatment and comparison groups in the absence of treatment. It relies on the assumptions of rank-preservation[15] and homogeneity of treatment effect across individuals (Athey and Imbens, 2006).

[15] While rank-preservation is in theory a rather constraining assumption, social mobility in Latin America, and Nicaragua, in particular, is in practise very low. The World Bank (2013) estimates that since the 1990s more than one in five individuals remained chronically poor, and approximately the same proportion remained steadily in the middle class in the subcontinent, with Nicaragua being the second-worse country in the region in terms of the proportion of individuals not displaying any type of upward mobility between 1998 and 2005 (80%). Intergenerational mobility shows similar patterns.

Table 4
Balance of covariates for the pre-treatment period, by sequence of establishment — preferred control group (road network).

Sequence of establishment	Group 1 [<1999]	RN Control	t-test	Group 2 [20,002,001]	RN Control	t-test	Group 3 [20,022,005]	RN Control	t-test	Group 4 [20,062,009]	RN Control	t-test
A. Municipality characteristics												
Distance to Managua (km)	72.54	101.72	−1.70*	42.43	107.11	−3.9***	90.96	103.60	−1.00	35.65	107.92	_4 56***
Road density (m)	84.67	71.67	0.47	92.00	71.88	1.26	78.42	72.28	0.51	225.42	156.85	3.01***
Landlocked (=1)	0.67	0.71	−0.18	0.40	0.74	−2.38**	0.57	0.74	−1.50	0.36	0.76	−2.98**
Main trade access (=1 if main port, airport or trade post in municipality)	0.27	0.43	−1.49	0.20	0.43	−1.48	0.42	0.42	−0.01	0.63	0.42	1.43
Electricity (% households)	0.99	0.82	0.99	0.94	0.86	1.00	0.93	0.88		0.97	0.89	1.33
Completed primary education	0.27	0.29	−0.29	0.29	0.30	−0.03	0.27	0.27	−0.01	0.29	0.27	0.97
Illiterate	0.22	0.35	0.22	0.24	0.32	−1.42	0.24	0.31	−1.73*	0.21	0.29	−1.56
Economically active population	0.54	0.51	0.97	0.56	0.53	1.62	0.55	0.54	1.03	0.55	0.54	0.43
Unemployment	0.03	0.02	0.9	0.04	0.04	−0.04	0.04	0.04	1.0	0.12	0.03	1.20
Manufacturing (% employed)	0.18	0.13	1.10	0.16	0.1.	1.17	0.16	0.13	LI	0.17	0.13	1.64
Agriculture (%employed)	0.08	0.47	−2.29**	0.16	0.50	−3.62***	0.30	0.5	−2.16**	0.20	0.48	−2.11**
Urban	0.90	0.38	2.39**	0.78	0.38	3.42***	0.56	0.39	1.98**	0.35	0.40	−0.47
Migrants in past five years	0.01	0.02	−0.36	0.01	0.02	−1.05	0.07	0.07	−0.07	0.08	0.10	−0.47
B. Pre-trend, selected outcomes												
Average real per capita expenditure, annual	0.99	0.59	0.83	0.27	0.46	−0.42	0.48	0.61	−0.56	0.23	0.47	−0.99
10th p- real per capita expenditure, annual.	1.10	0.70	0.73	0.53	0.53	0.00	0.71	0.71	0.03	0.29	0.60	−1.13
50th p- real per capita expenditure, annual	1.13	0.67	0.78	0.32	0.61	−0.51	0.60	0.69	−0.32	0.25	0.53	−1.00
90th p- real per capita expenditure, annual	0.83	0.63	0.31	0.48	0.54	−0.10	0.57	0.64	−0.27	0.23	0.47	−0.83
Share of high-skill over low-skill (1)	4.30	4.02	0.07	2.28	4.21	−1.02	8.80	3.49	2.48**	2.46	2.61	−0.09
Share of high-skill over low-skill (2)	−0.71	−0.50	−0.26	−0.69	−0.48	−0.54	−0.56	−0.47	−0.21	−0.26	−0.09	−0.35
N=	4	101		5	91		7	90		2	93	

Notes: Municipalities are grouped based on the sequence of EPZ establishment. Values are for simple averages for the pre-treatment period. Definitions are as in Table 3. For municipalities treated before 2000, the historical trends denote the average growth rate of the outcomes before 2001. I would otherwise have no pre-treatment trends for this group. A similar approach is done for the rest of the groups, but strictly considering only the pre-treatment period. The control group is defined with respect to the density of paved road.

4.2. Threats to identification and validity of the identification strategy

Given the setting here, there are three main threats to identification to address. As most spatially-bound policies these concern the reverse causality and omitted variable bias related to the non-random choice of EPZs location across time and space (i.e. sorting), and the limitations related to the fact that the experimented created here may not coincide with the administrative boundaries used (i.e. firm spillovers – creation and diversion –, and mobility of individuals). I will empirically evaluate the last two issues in Sections 5.4 and 6.1.

I address the issue of the endogenous location of EPZs areas in several ways. Regarding unobservable factors I allow for municipality and year fixed-effects. These allow me to sweep out time-invariant features of the municipality as well as time-specific dynamics. Additionally, I use province-time dummies and region-time dummies that control for omitted variables that change over time within these levels of aggregation. Some specifications also include municipality-level time trends to control for any omitted variable that varies over time within municipalities in an approximately linear fashion.

I further refine the identification strategy with the construction of alternative control groups based on relevant observable municipal characteristics. One important challenge in the evaluation of spatially-bound policies is the selection of appropriate counterfactuals. The ideal control municipalities here are areas economically similar to EPZ municipalities but lacking zones and firms' establishment. There are many reasons why using all never treated municipalities would yield bias estimates (i.e. different sectoral skill and age compositions for instance). An ideal approach would rely on areas that were targeted at some point in time but where a zone finally was not created, using the eligibility criteria to ensure similarity (Greenstone et al., 2010). An alternative, would be to use the temporal difference between treated municipalities to compare them against each other (Busso et al., 2013; Wang, 2013). I do not have information on eligible municipalities that eventually did not receive an exporting zone. Further, because of the sequence and relatively small number of treated areas in earlier phases I cannot use the temporal sequence to compare municipalities against each other. Instead, I use municipalities' observable characteristics to define alternative control municipalities. While limitations to this approach remain, tests for the balance of covariates and pre-treatment outcome trends give me confidence on their validity.

The export-oriented nature of EPZs and its human capital needs imply that both accessibility conditions (i.e. ports, airports and roads), basic infrastructure and the educational attainments are likely to play an important role in the choice of location of the industrial parks and firms. The first approach is thus to match municipalities based on these and related observable elements, using propensity score matching to complement the DID strategy (Abadie, 2005; Imbens and Wooldridge, 2009). This approach is widely used in the literature, and has generally been shown to produce sensible counterfactual groups. For this, I estimate the propensity score (logit) for a municipality to receive EPZs based on a series of pre-treatment variables for each of the treatment sequences using a k-nearest neighbour approach,[16] and use only matched municipalities as the comparison group. As shown in Tables A.3 and A.4 there is a large overlap in p-scores between treated and matched municipalities, particularly for groups one and four. T-test and pseudo-R2 are both fairly low after matching which suggests that potentially important selection criteria become not significant after matching.

The second approach is to define the control municipalities based on the quality of their road infrastructure and distance to the capital city (Managua). Given the limited development of Nicaraguan ports, airports and railways, road networks remain the main mode of transport

[16] Because of the small sample size, I use different values of k from 3 to 5. I note that the k-nearest neighbouring matching with replacement is likely to use some control municipalities multiple times in different periods.

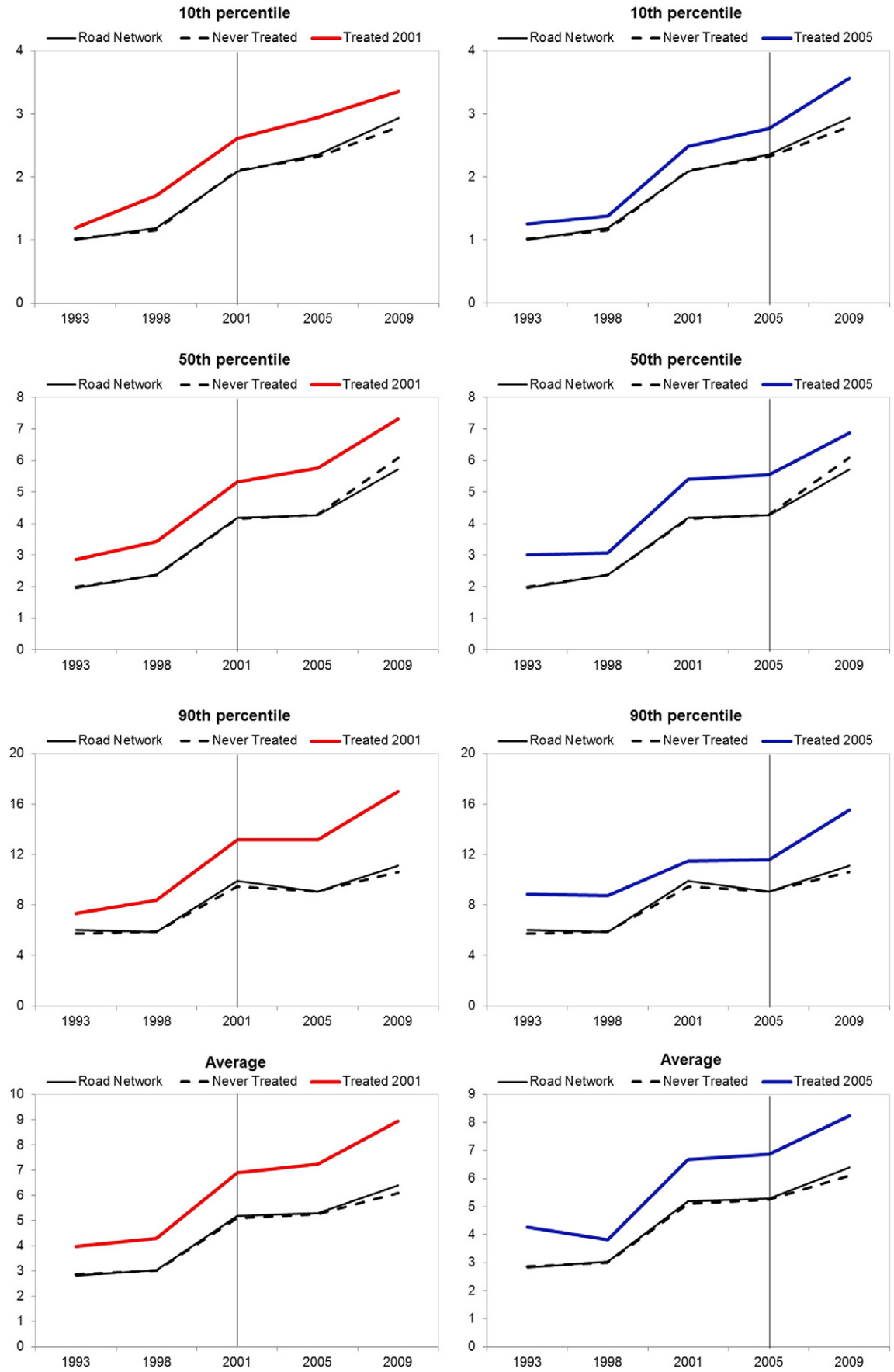

Fig. 2. Parallel trends — selected outcome variables, preferred control group. *Notes*: Figures depict parallel trends of levels of real expenditure per capita according to treatment and control groups. Main control municipalities are selected according to the density of the road network. On the left side, the treated include only those treated in 2001, and on the right side the treated are only those treated in 2005. Values are in thousands of constant Cordobas (C$1999).

for most domestic movements and a large share of imports and exports (World Bank, 2011). Good roads are not only likely to be one key determinant for EPZs location choices, but they may also be correlated with higher levels of regional development. I use the metres of paved roads every 1 km^2 to account for road quality, and set the threshold at the minimum in treated municipalities (40 m). At the same time, distance to the capital city is also likely to be a key factor in EPZ location, as firms are drawn towards it to benefit from agglomeration economies and higher levels of human capital. I set the threshold to the maximum distance for an EPZ municipality (<170 km).[17] Despite the fact this counterfactual is constructed in a more arbitrary way, it imposes less restrictions in terms of observables characteristics. Table 4 shows that there is significant overlap between this control group (group 2) and the treatment areas. While the propensity-score matched municipalities are more balanced in relation to the urban–rural dimension, the second group seems slightly closer in terms of educational attainment. In any case, with both cases, controlling for specific covariates will wipe-out any remaining difference between the two groups.

I choose group 2 as the preferred counterfactual as it imposes less restrictions in terms of sample size, makes fewer assumptions on the municipal characteristics linked to the programme, and parallel trends in levels of pre-treatment outcomes are more consistent (Figs. 2 and A.2). I formally test the assumption of parallel trends for the main specification by including pre-treatment trends; these results are supportive of the identification strategy (Table A.6).[18] The common trend assumption that the two groups' measured outcomes would have followed the same trends over time in the absence of policy, is essential to support the DID framework. Ultimately, I compare results with both methods and conclusions are unchanged (results with the matched municipalities are on Appendix I).

5. Basic results

5.1. Average treatment effect of EPZ establishment

Table 5 presents the results of the basic DID specifications (Eqs. (1) & (2)) using both the working-age population dataset and the municipal-level outcomes. The different columns explore the sensitivity of the results to different area fixed-effects and time trends. Overall, I find that the establishment of EPZs increases the average level of real per capita expenditure in the treated municipalities, with point estimates ranging between C$800–460 depending on specifications. The size is nontrivial but relatively small if we consider the large time span of the analysis. It amounts to about 10 to 12% of the average levels across the period. It is reassuring to see that the magnitude of the effect and its statistical significance remain stable across the specifications, particularly with the addition of municipality-time trends. Further, results are consistent irrespective of the sample used, though they lose precision with the panel dataset as observations are reduced. Using the alternative control group based on propensity-score matching does not alter the results (Table A.5).

5.2. Deciles of the expenditure distribution

Table 6 presents the results of DID estimates across moments of the real per capita expenditure distribution of treated municipalities (Eq. (3)). In interpreting these results, it should be kept in mind that I estimate treatment effect on each decile of the expenditure distribution of treated municipalities strictly with respect to its respective decile in the control group. As noted, this implies the assumption of rank-invariance. While this is a strong assumption, social mobility in Latin America and particularly in Nicaragua has been very low since the early 1990s.[19] Caution is still warranted when interpreting these results, and I refrain from making any reference with respect to within-area inequality dynamics.

The most salient feature in Table 6 is the containment of the treatment effect at the top-end of the distribution (80th and 90th percentiles), with point estimates significant at 10% levels. The size of is much larger in this case, ranging at about 20 to 25% of the average levels of per capita expenditure of the top deciles for the period, suggesting that on average, the establishment of exporting zones profits the top deciles of the distribution in host municipalities the most. Point estimates are consistent to the inclusion of different time-area fixed-effects, as well as to the clustering of standard errors at the province level and against the alternative control group (Table A.7). The coefficient at the 10th percentile are also positive and significant (at 10% level) in one of the specifications. The size is smaller, on average EPZs increase the expenditure levels of the bottom decile by 10% across the period.

The fact that such a strong concentration is visible when decomposing the average treatment effect across the expenditure distribution, provides support for two alternative but complementary theoretical intuitions. The first one, relates to the various models of urban economics that integrate land prices' responses and labour mobility dynamics for estimating welfare at the local level. Kline and Moretti (2014) show that mobility responses may lead the local cost of living to change, which in turn can lead landlords or high-skill incomers, to capture the benefits associated with the firm's arrival. Evidence of this on US empowerment zones is found by Reynolds and Rohlin (2015). The second relates to international trade models that put emphasis on the existence of a high-skill premium in the exporting sector stemming either from productivity gains or higher skill intensity (see Goldberg and Pavcnik, 2007 for review). Topalova (2010) emphasizes how these dynamics are likely to be larger in the case of rigid labour markets. Although information on the skill composition of workers within EPZs is not available, manufacturing firms in Nicaragua do exhibit higher high-to-low skill ratios, with on average only 34% of the workforce in the firms classified as unskilled (Nicaragua Enterprise Survey, World Bank 2010). The positive coefficient of the first decile may also offer some support for polarization theories (Autor and Dorn, 2013), according to which new technologies and the related inflows in skilled labour in local labour markets may have reallocated low-skill labour into lower-earnings services occupations. There is no solid evidence of polarization in developing countries yet, but it is plausible to imagine the changes happening between the formal and informal sectors, particularly in more urbanized areas in countries with small middle-classes.

The conclusions of the basic specifications are somewhat counter-intuitive with the mediatized image of the zones as prime generators of employment for the unskilled labour force. Data limitations prevent me from completely disentangling the mechanisms through which firms operate, however I address possible channels next by looking at the effect across the skill distribution and the effect of EPZ formation on labour mobility.

[17] Earlier versions of this paper also included a control group that excluded the two autonomous regions of the Atlantic coast (RAAN and RAAS). These correspond to the poorest areas of the country and also have very particular ethnic and economic characteristic. The inclusion of province-fixed effects and region-time fixed-effects addresses this issue in a parametrical way.

[18] I run a formal test of parallel trends in the main specifications of Eqs. (1) and (2) by including pre-treatment trends in the fashion of Greenstone et al. (2010). For this, I augment Eqs. (1) and (2) with a municipality-specific trend variable defined as the interaction between municipality specific linear-time trends and the treatment dummy variable EPZ as previously defined. Overall, coefficients on pre-treatment trends are not statistically significant supporting the conclusions of the effect not being the result of different pre-existent trends between treated and control municipalities (see Table A.6 in Appendix I).

[19] The fact that individual characteristics across each decile remain relatively constant in both groups and with similar relative sizes, support the use of this assumption. See footnote 18.

Table 5
DID estimates of the effect of EPZs on average real expenditure per capita

	(1)	(2)	(3)	(4)	(5)
EPZ	*800.5***	*639.6***	*525.8***	*477.4***	*457.9***
	(353.6)	(317.8)	(214.1)	(203.9)	(207.4)
Observations	52,997	52,997	375	375	375
R-sq.	0.341	0.339	0.722	0.818	0.914
Year FE	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓
Municipality-year trend	✓	✓			
Province-year dummies			✓	✓	
Region-year dummies					✓
Covariates		✓		✓	✓
Municipalities	106	106	106	106	106

Notes: Dependent variable is levels of real expenditure per capita. Robust standard errors in parentheses, clustered at municipality. Covariates in column 2 include dummies for time-varying individual and household characteristics: educational level, age square, gender, urban, sector of work, household size and having migrated within the past five years. Individual observations are only for working age individuals. Covariates in column 4–5 include illiteracy rate, the share of population with primary and tertiary education, the share of urban population, migrants and share of households with access to electricity at municipal-level. Results are estimated against the preferred control group using the road network buffer.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

5.3. Heterogeneous time dynamics: an event study

Before introducing any additional extensions, I explore the time dynamics of the establishment of exporting zones. Considering the relatively long period covered, it is important to address the question of how the programme impacts expenditure levels across time. The literature has emphasized that the impact of EPZs may take time to materialize. In some cases it has also been found to diminish with the years, as low labour costs vanish.

To explore the time pattern, I compute a modified version of Eq. (3) adding a set of dummy variables for leads and lags of the year of EPZs authorization (Autor, 2003). Specifically, I add indicator variables for 11, 8 and 3 years before and after EPZ establishment.[20] The dummies are equal to one in only one year each per treated municipality. The treatment dummy EPZ_{mt} equals one only on the first year of EPZ establishment, or year zero. To help visualize the results Fig. 3 displays point estimates at different moments of the distribution with the 95% confidence intervals; estimates are reported in Table 7.

The positive sign and relatively large size of most of the coefficient seem to indicate a consistent incremental effect on the level of real expenditure per capita at all percentiles of the distribution with the more years of EPZ operation. Only the 90th percentile is statistically significant for year zero. The larger effect still occurs at the very upper-tail of the distribution, notably at the 90th percentile with statistically significant point estimates at 5 and 1% levels that imply a growing absolute effect of about 10 to 20% per period. However, the time decomposition indicates statistically significant effects at all deciles above the 30th after 8 years of EPZ operation, sustained only for the median deciles after that period. Different explanations are conceivable here. Classic trickle down mechanisms might be taking place, resulting from both medium-term consumption and production dynamics after the expansion of the zones, or the increases in productivity over the years. Negative values before treatment are too imprecise to make any conclusion,[21] but they are supportive of the effect resulting from the arrival of new exporting firms.

Findings of this section shed light on the more complex dynamics of the EPZ policy. They highlight the predominance of the effect at the upper-tail of the distribution. At the same time, they are also suggestive of a diffusion of the treatment effect at the middle of the distribution and 10th percentile, after 8 and 11 years of zone establishment.

Table 6
DID estimates of the effect of EPZs across the expenditure distribution in treated municipalities.

Real expenditure per capita	(1)	(2)
10th percentile	218.3	232.6*
	(145.1)	(138.0)
20th percentile	143.8	149.8
	(175.7)	(169.9)
30th percentile	169.2	173.1
	(219.7)	(211.4)
40th percentile	213.8	209.5
	(228.2)	(218.5)
50th percentile	258.4	245.2
	(264.8)	(251.5)
60th percentile	271.3	250.9
	(317.7)	(302.5)
70th percentile	463.2	414.9
	(359.9)	(345.4)
80th percentile	844.0*	785.6
	(506.5)	(475.0)
90th percentile	2138*	2061*
	(1,223)	(1,109)
Year FE	✓	✓
Municipality FE	✓	✓
Province-year dummies	✓	
Region-year dummies		✓
Covariates	✓	✓
Observations	375	375
Municipalities	106	106

Notes: Dependent variables are deciles of real expenditure distribution. Robust standard errors in parentheses, clustered at municipality. Covariates include illiteracy rate, the share of population with primary and tertiary education, the share of urban population, migrants and share of households with access to electricity at municipal-level. Results are estimated against the preferred control group using the road network buffer.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

5.4. Skill distribution

I next use the distribution of per capita years of education to verify the predictions of the conceptual channels (i.e. a skill premium related to the establishment of EPZ). Ideally, I would want to break down the skill distribution by industry and test the effect of EPZ establishment across the full interactions of skill and industries (Atkin, 2012). However, data limitations regarding industrial affiliations and labour earnings prevent me from disentangling the effect further.

Given the large proportion of unskilled individuals, testing for the effect of EPZ relative to skill-levels is not straightforward. 30% of the working age sample reports zero years of formal education across the period and only 5.4 reports having completed education at the tertiary level. For this reason, I use relative education levels. First, I calculate the distribution of education (years of completed education[22]) amongst all working age individuals by year and provinces.[23] Following Atkin (2012) I then generate a three level skill-specific measure. I code as low-skill, individuals below and at the 40th percentile of the schooling distribution, as mid-skill, individuals above the 40th and up until the 70th percentile, and as high-skill individuals with years of schooling above the 70th percentile.[24] With these measures in hand, I rerun specifications for Eq. (1), but augment the regression with 3 distinct

[20] Leads and lags of 4 and 8 years also include municipalities with 3 and 7 years, respectively, in order to avoid noise from a dummies with few municipalities. Each lead contains 4 to 16 treated municipalities.

[21] The particular negative shock at −8 years coincides with the post-hurricane Mitch period hitting Nicaragua.

[22] Years of schooling or completed formal education span from 0 to 20 in the sample.

[23] I use working-age individuals given that using the sample of employed instead does not allow me to generate a full distribution. Similarly, using municipalities provide an incomplete picture.

[24] The distribution by skill levels is skewed towards the lower tail in both treated and non-treated municipalities, but the proportion of low-skill is much higher in non-treated municipalities. This is largely due to the period 1993–1998 (Fig. A.3).

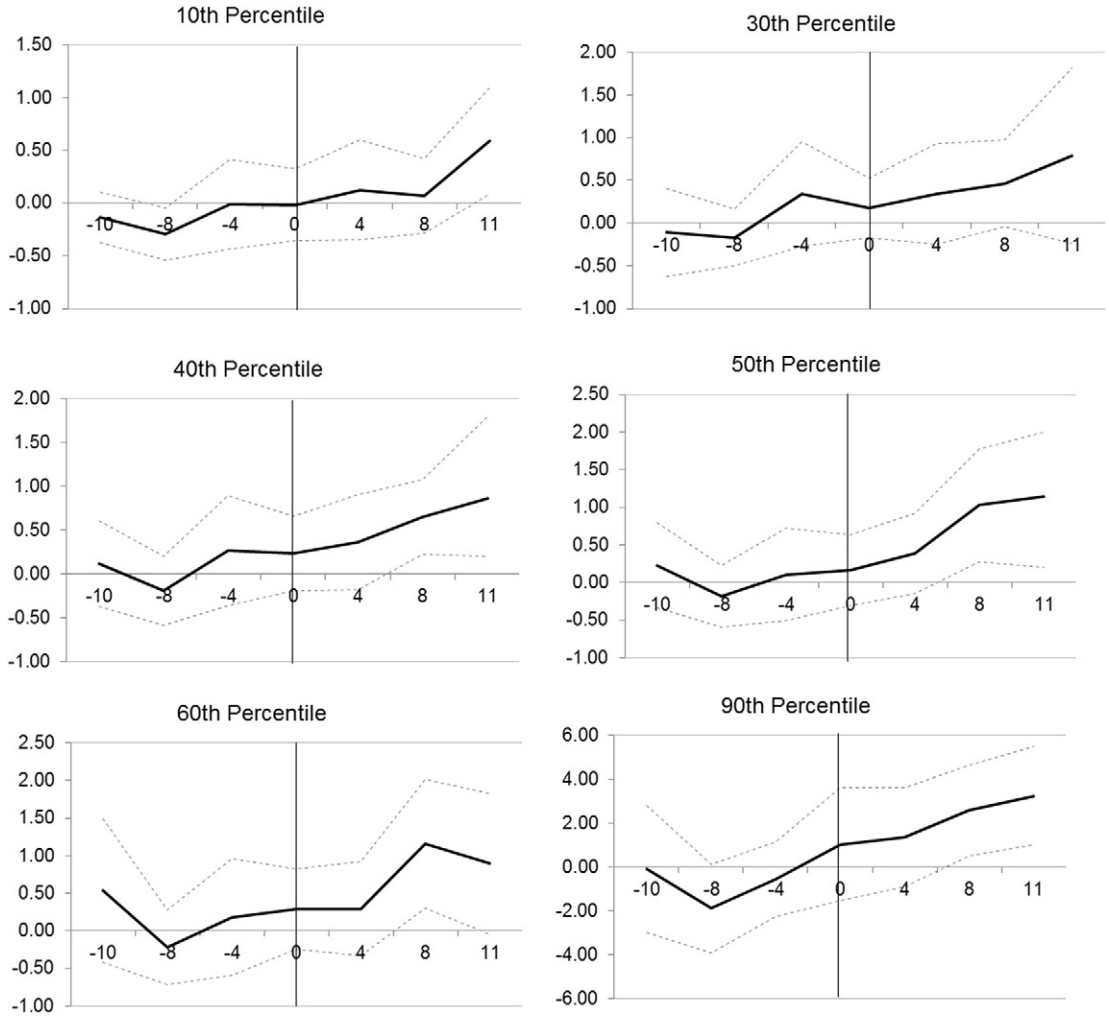

Fig. 3. Events study — selected outcome variables. *Notes*: The horizontal axis measures the number of years since the EPZ programme took place. The vertical line represents thousands of Cordobas. The plots connect by the solid line indicate the effect on the levels of real per capita expenditure compared to the period immediately before, the dotted lines are the 95% CI where the standard errors are clustered at the municipality level. See Table 7 for point estimates.

dummies created from the interaction of the treatment variable and the binary variables for each skill level (low, mid and high).[25]

Table 8 shows the results of this regression for the average level of real per capita expenditures for the working-age individuals. Results are robust to the inclusion of province-fixed effects and the clustering of standard-errors at the province level. The ordering and signs of the coefficients are largely as expected due to findings in Section 5.2. The highly significant and positive effect contained at the high-skill level supports the view that the gains from EPZ establishment are larger for the upper-deciles of the distribution. It is hard to be conclusive on what is driving the high-skill individuals to benefit the most from EPZ formation without decomposing by industry. These findings seem supportive of the channels previously mentioned in Section 5.2. I find similar results using an alternative measure for skill-levels based on the actual level of education achievement[26] (column 2). Low-skill individuals also register significant positive point estimates here, which is consistent with the heterogeneous time dynamics and the bottom decile being significant in some specifications.

[25] Mid-skill is the omitted variable.

[26] Here low-skill corresponds to working age individuals reporting no years of formal education, mid-skill stands for those having completed primary school, and high-skill includes those having completed secondary education and/or university. I include secondary education as a high-skill given that only 5% of the entire sample has tertiary formation.

5.5. Relocation of workers

An important concern that remains relates to possible bias from internal mobility across areas during the period studied. Does low and high-skill labour relocate across space in response to the labour supply shock? The most important threat to validity in the place-based policy literature relates to compositional changes in the distribution of workers within units of analysis. This threat is greater during large time periods, like the one here.

There are two dimensions to consider in this case. The first is possible internal migration across municipalities. This would impose an upward or downward bias on my results, depending on which type of migrants arrives or leaves treated areas. This dimension is closely related to the skill composition of the migrants, as it could be foreseen that the higher skill may be drawn towards EPZ's employment, and on the contrary the lower-skill may be pushed to rural areas. Additionally, large exogenous inflows of migrants may alter both income levels and firms location decisions related to the impact of the flows on local wages. During the entire period considered, 10.7% of the sample reports having moved from another municipality in the past five years.[27] The flow however seems to have been under-reported in 1998 and 2009, both years displaying extremely low percentages (2–5%). Excluding

[27] As defined in the Household Surveys. This is the only measure consistent for the 5 different surveys.

Table 7
The heterogeneous time dynamics of EPZ establishment.

	10th percentile	20th percentile	30th percentile	40th percentile	50th percentile	60th percentile	70th percentile	80th percentile	90th percentile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
EPZ establishment, t − 11	−134.9	−85.65	−109.9	118.3	228.4	536.8	190.9	310.1	−99.58
	(122.3)	(216.1)	(260.9)	(249.7)	(289.2)	(487.8)	(551.6)	(491.2)	(1475)
EPZ establishment, t − 8	**−298.3****	−244.8	−171.0	−193.6	−184.0	−221.0	**−594.3***	−663.6	**−1889***
	(127.1)	(174.7)	(170.4)	(201.1)	(211.4)	(253.4)	(341.9)	(448.8)	(1034)
EPZ establishment, t − 4	−10.84	140.8	337.8	266.8	105.9	180.5	−40.45	34.72	−559.7
	(215.9)	(230.5)	(310.7)	(321.5)	(313.5)	(396.0)	(440.5)	(510.6)	(871.0)
EPZ establishment, t0	−16.95	−20.45	172.4	231.8	163.3	287.7	391.4	537.1	1025
	(175.1)	(189.8)	(177.5)	(216.2)	(239.5)	(272.0)	(511.7)	(548.0)	(1316)
EPZ establishment, t + 4	124.9	271.6	336.3	363.5	390.7	296.7	238.6	722.2	**1032***
	(240.7)	(243.6)	(301.5)	(277.7)	(273.6)	(316.7)	(417.3)	(545.9)	(528.3)
EPZ establishment, t + 8	66.13	399.8	**462.4***	**651.6*****	**1029*****	**1163*****	**1149****	**1942****	**2591****
	(180.1)	(284.4)	(259.0)	(217.8)	(383.6)	(436.2)	(464.6)	(845.2)	(1052)
EPZ establishment, t + 11	**589.1****	492.9	782.7	858.8	**1148*****	**895.0***	1022	962.8	**3237*****
	(258.7)	(344.7)	(523.6)	(532.5)	(433.8)	(478.1)	(680.5)	(634.0)	(1140)
Observations	375	375	375	375	375	375	375	375	375
R-sq.	0.709	0.748	0.752	0.774	0.785	0.778	0.702	0.728	0.630
Municipalities	106	106	106	106	106	106	106	106	106

Notes: Dependent variables are deciles of the real per capita expenditure distribution. Robust standard errors in parentheses, clustered at municipality. All regressions include year and municipality fixed-effects and province-year dummies. Covariates include illiteracy rate, the share of population with primary and tertiary education, and the share of urban population, migrants and households with access to electricity at municipal-level. All leads are equal to one in only one year each per adopting state. EPZ dummy (t0) equals one only for year of establishment. EPZ t +/− 4 includes t +/− 3, and EPZ t +/− 8 includes t +/− 7, to have balanced dummies. Results are estimated against the preferred control group using the road network buffer.

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

these, more than seven percent of working age population is defined as an internal migrant (7.6 and 7.7 in treated and control municipalities, respectively). There is limited information regarding origin and destination of the flows, with data available only for 1998. For that year, 7.8% of the migrants moved to a municipality hosting an EPZ, with most flows taking place among non-treated areas.

To address this issue, I start by estimating the likelihood of migrating for the working age population following the establishment of a zone in a given area, using both OLS and logit estimators. Results are reported in Table A.10. I find no evidence of a statistically significant impact. Additionally, point estimates are always very small, permitting to discard major bias from large displacement following the establishment of a zone in a given area. The absence of perfect factor mobility is not surprising. The World Bank (2009, 2012) estimates that during the 2000s, only 10% of the improvements in living standards were related to population shifts (from rural to urban), and find little evidence of changes in the structural distribution of employment in the last decade, with a continued dominance of the agricultural sector. As a second test, I estimate the effect of EPZ formation on changing the skill composition of the residing population. This may capture finer dynamics due to migration patterns that are not visible using the overall migrants' measure. Results in Table 9 are in line with theoretical intuitions. Although the sign of the coefficients on the share of high skill changes with respect to the definition, this is due to the larger weight given to the medium-skill (primary school) using the second definition. I focus on the first definition. Here point estimates on the high-skill are large and positive though not statistically significant. Conversely, there

Table 8
The effect of EPZs by skill-level.

	Skill-distribution	*Education-level*
	(1)	(2)
EPZ	486.1	204.0
	(466.7)	(485.1)
Low-Skill * EPZ	29.98	**209.1****
	(134.4)	(70.18)
High-Skill * EPZ	**1810*****	**2018*****
	(189.4)	(115.6)
Observations	52,997	52,997
R-sq.	0.338	0.346
Municipalities	106	106

Notes: Dependent variables are levels of real expenditure per capita. Robust standard errors in parentheses, clustered at municipality. All regressions include time and municipality fixed effects, municipality-time trends and province-time dummies. Covariates include education level, illiteracy rate, as well as the share of urban population, migrants and households with access to electricity at municipal-level. Column 1 shows the results using the years of education to define the skill distribution. Column uses the second definition using the levels of formal education. Results are estimated against the preferred control group using the road network buffer.

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

Table 9
The effect of EPZ formation on skill composition.

Definitions	Skill definition 1		Skill definition 2	
	(1)	(2)	(3)	(4)
Share of high skill	0.253	0.134	−0.205	−0.242
	(0.176)	(0.150)	(0.228)	(0.221)
Share of low skill	−0.01	0.000	−0.018	−0.007
	(0.015)	(0.015)	(0.014)	(0.0112)
Share of high to low skill	0.0369*	0.0251	0.005	0.002
	(0.022)	(0.021)	(0.016)	(0.011)
Observations	375	375	375	375
R-sq.	0.753	0.865	0.845	0.833
Year FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Municipality-year trend	✓		✓	
Province-year dummies		✓		✓
Covariates	✓	✓	✓	✓
Cluster Municipalities	106	106	106	106

Notes: Dependent variables are the share of high and low skill working age populations and the proportion of high to low skill populations at the municipal level. Robust standard errors clustered at municipality are in parentheses. Columns 1–2 define skills according to the distribution of years of education, while columns 3–4, and define skills according to the level of formal education achieved. Definition are in Section 5.4. Covariates are as in Table 6. Results are estimated against the preferred control group using the road network buffer.

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

Table 10
The effect of EPZ establishment & spillover dynamics.

	10th percentile	20th percentile	30th percentile	40th percentile	50th percentile	60th percentile	70th percentile	80th percentile	90th percentile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
EPZ	232.8*	121.0	127.4	227.2	271.3	307.3	532.2	942.5*	2123*
	(134.6)	(173.9)	(214.3)	(231.7)	(268.2)	(319.9)	(376.5)	(491.4)	(1142)
Neighbouring municipalities1	−147.1	−401.4	−362.3	109.8	12.05	74.33	454.2	699.8	−238.4
(5 to 15 km)	(327.0)	(306.1)	(381.1)	(389.3)	(343.1)	(322.7)	(421.0)	(538.6)	(493.9)
Neighbouring municipalities2	−139.6	−237.4	−357.6*	−257.7	−239.9	−142.6	−170.1	−104.1	−435.0
(15 to 35 km)	(127.5)	(159.3)	(184.7)	(176.0)	(222.3)	(248.9)	(300.7)	(415.8)	(568.3)
Observations	375	375	375	375	375	375	375	375	375
R-sq.	0.687	0.735	0.739	0.756	0.759	0.753	0.691	0.699	0.687
Number of Muni.	106	106	106	106	106	106	106	106	106

Notes: Dependent variables are deciles of the real per capita expenditure distribution. Robust standard errors in parentheses, clustered at municipality. All regressions include year and municipality fixed-effects and province-year dummies. Covariates include illiteracy rate, the share of population with primary and tertiary education, the share of urban population, migrants and the share of households with access to electricity at municipal-level. The variables Neighbour Municipalities are dummy variables that equal one if a municipality's centroid is situated at 5 to 15 km, and 15 km to 35 km distance from a treated municipality, and zero otherwise. I use the centroid's distance to avoid noise from larger municipalities. Results are estimated against the preferred control group using the road network buffer.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

seems to be negative to zero effects on the low skill. Overall, the share of the high-to-low skill does seem to be impacted, increasing by 3% across the period. Though not robust across specifications, results are significant at 10% level. These changes are in line with the theoretical mechanisms put forward. The small size of the impact means the increase in the proportion of higher-skill workers in treated municipalities following the establishment of an EPZ is unlikely to be driving the average positive effects of the baseline results.

The second dimension to address concerns the general out-migration phenomenon of the poor in Nicaragua. Similar to internal migration, large out-migration flows would also alter the composition of the labour force. It is estimated that near half a million of Nicaraguan migrated to neighbouring Costa Rica since 1994. To address this issue, I run the previous equations holding constant population weights of 1993 (using census data and initial sampling weights). Results remain unchanged discarding any strong compositional changes from out-migration (Table A.11).

6. Extensions & robustness checks

The final empirical section contains some alternative specifications that examine the robustness of the previous conclusions.

6.1. Spillover dynamics & commuting

This section addresses the concerns related to possible interactions taking place between an EPZ municipality and its neighbours. If these exist, not taking them into consideration would lead to a bias estimation of the EPZ effects.

I define spillovers as any form of business diversion or creation that could take place because of agglomeration forces between treated and control municipalities adjacent to each other. Backward linkages between EPZs and local producers are probably the main mechanism through which spillovers can be expected to happen. These could lead to neighbouring areas being indirectly 'treated' or on the contrary, to the relocation of economic activity closer to EPZs, in detriment of control municipalities. Additionally, I consider commuting behaviour as a form of spillover. Individuals working in a treated municipality but residing in a neighbouring area are likely to generate similar types of possible externalities. Both mechanisms are hard to measure. Information on commuting is only available for 2001, when it represented 3.56% of the total sample (5.05 and 3.02% in treated and control municipalities, respectively). This is consistent with some studies finding limited labour mobility in Nicaragua.[28]

In order to formally test for the possibility of these two dynamics, I measure treatment effect across the real expenditure distribution of municipalities adjacent to a treated municipality. For this, I compute a modified version of Eq. (3) adding a set of dummy variables that switch to one for municipalities neighbouring a treated area. Because of the particular small size of Nicaraguan municipalities in the west part of the country, considering the shared border as criteria for proximity would imply denoting the vast majority of control municipalities in the region as neighbours. Instead, I use the municipality's distance from its centroid to the centroid of the nearest treated one using two levels of distance, one set at between 5 to 15 km and the other at 15 to 35 km.[29] This differentiation allows accounting for both dimensions, commuting with the closer group, and more formal spillover effects with the one further away.

Table 10 provides the DID estimates augmented with the neighbour dummies on the deciles of the real per capita expenditure distribution. It is encouraging to find a close similarity in the size, sign and statistical significance of the EPZ coefficients to the ones in previous estimations. Point estimates on neighbouring areas seem to indicate low (negative) to no spillover effect from EPZ location. Only one coefficient shows statistical significance but the size is not higher than 2%. Despite the sign being consistently negative for the larger ring, bias from this phenomena can be expect to be very small, validating the previous estimates.

6.2. Balanced panel & weight of capital city

Given that the identification strategy relies on the timing and cross-section variation of EPZ establishment across municipalities, this final empirical section addresses the possibility that the results are driven by the inclusion of particular outliers. First, I limit the sample to municipalities observed for at least 4 of the 5 periods considered.

[28] Related to the low-skill level of the labour force, the cost of transport with respect to minimum living standards and frequent transportation strikes, commuting distances are found to be an important deterrent for participating in the labour market (Jansen et al., 2007).

[29] The measure is of course imperfect. While using the centroid's distance offers the advantage of reducing the noise from larger municipalities where a large proportion would not be in contact with treated areas, it might also exclude some bordering zones. Smaller units of analysis and household's geo-references would be needed for a more precise measure. For simplicity, I refer to the measure as the direct distance between municipalities, but it always registers the distance between the centroids of two municipalities.

Second, I exclude the capital city (and municipality) of Managua from my sample to verify that this larger agglomeration is not pooling the results. Managua remains by far the largest municipality in the country in terms of economic output and population, with its average level of real expenditure per capita being twice the one of all other municipalities in the country. The range is almost as high across all the decile of the expenditure distribution. Results are reported in Appendices A.12–A.13. Overall, conclusions remain unchanged.

7. Concluding remarks

The present analysis has exploited a rich dataset at both individual and municipal levels to estimate average and distributional effects of EPZ establishment within their host municipalities in Nicaragua for the period 1993–2009. Evidence amassed is consistent across different specifications and robustness checks.

Overall, I find robust evidence that EPZ establishment in a given municipality increased the average level of real expenditure per capita between 10 to 12% across the period. The decomposition of the effect across the expenditure distribution suggests that the upper-tail benefited the most both in terms of size and across time, with middle-range deciles benefiting only after eight years of a zone establishment. The exception is a small significant effect at the 10th percentile. The bulk of the positive effect is concentrated on the higher-skill working-age individuals.

Data limitations do not allow me to be conclusive regarding the ultimate channels at play here. An obvious question is why EPZs would have mainly benefitted higher-income households. Is this due to the effect on the local cost of living, rent or land prices? Or is it the result of the skill premium in the exporting sector? The question remains open on whether the effect is really due to the exporting nature of the firms or the local dynamics on factor prices and possible production externalities.

Theoretical models incorporating responses in land prices and rents (Neumark and Simpson, 2014; Busso et al., 2013; Kline and Moretti, 2014), as well as labour market heterogeneity (Topalova, 2010; Kovak, 2013) seem to be the best framework to understand both possibilities. In this sense, this paper offers some interesting insight on the importance of analysing trade policies at the local level, taking into account local labour market dimensions and agglomeration economies. Goldberg and Pavcnik (2016) acknowledge the move towards this direction, and underline the interest of the empirical literature in influencing richer theoretical models. Findings here encourage the honing of theoretical trade frameworks that incorporate labour market heterogeneity in terms of skills and formality of employment, as well different mobility patterns between low and high income segments.

The findings in this study are also important from a policy perspective. They contribute to elucidate the dynamics of a popular policy tool, and contradict popular belief that EPZs are directly beneficial for the low-skill/low-income groups through local employment. The time lag registered for the policy to reach different segments of the distribution leaves a door open for policy intervention. There is an urgent need for further research in the area in order to identify with more precision the different mechanisms at play.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.labeco.2016.05.016.

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