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Do Special Transfers to Incentivize Local Tax Revenue Effort Work?

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Abstract

Many decentralized countries struggle with low levels of subnational tax effort. As a remedy for this situation, specific-purpose incentive schemes designed for own revenue mobilization at the subnational level can be simple, efficient, and cost-effective tools. We examine the effects of an incentive transfer program established in 2010 in Peru aimed at increasing property tax revenue at the subnational level. Specifically, Peruvian districts would receive a monetary transfer if they increased their property tax collection above a certain threshold in the previous year, thereby raising their own revenues. Using quasi-experimental methods, we find that the property tax revenue of districts that (actively) participated in the program significantly increased over the following decade. Our results are robust to the choice of estimator and a series of additional tests.

Keywords: property tax, vertical fiscal imbalance, tax effort, Peru

JEL Codes: H71, H77, C21

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1 Introduction

Over the past three decades, fiscal decentralization has expanded significantly across both developed and developing countries. However, this process has often been asymmetric, with substantial delegation of expenditure responsibilities but far more limited delegation of own-source revenues—a pattern commonly referred to as "partial fiscal decentralization" (Brueckner (2009); Borge et al. (2014); Baskaran et al. (2016)). Such asymmetry generates large vertical fiscal imbalances between levels of government, which have been linked to weakened accountability, inefficiencies in spending, reduced tax effort, and diminished fiscal responsibility (Rodden et al. (2003); Bouton et al. (2008); Eyraud and Lusinyan (2013); Jia et al. (2021)).

A common strategy to mitigate these imbalances is to enhance the capacity of subnational governments to generate their own revenues. Among the available instruments, the property tax is widely considered one of the most suitable sources of local revenue (Bahl and Martinez-Vazquez (2006, 2007)). Yet despite being formally assigned to local governments in many countries, property taxes often perform poorly, particularly in developing economies, where collections frequently remain below 0.5 percent of GDP (Vehorn (1997)). This persistent underperformance reflects broader weaknesses in local revenue mobilization and exacerbates fiscal dependence on the central government.

One proposed remedy is the use of dedicated transfers that create incentives for subnational governments to increase their own revenue collection. While permanent transfer-based incentives have limited justification within the theory of fiscal federalism (Lago et al. (2024)), in contexts where decentralization is relatively recent and traditions of local taxation are weak temporary incentives may serve as a pragmatic and effective tool to strengthen local tax effort. Nonetheless, despite the clear relevance of such incentive schemes, the literature provides little systematic evidence on how they should be designed or on their actual effectiveness.

This paper addresses this gap by empirically examining the impact of incentive transfers on subnational tax effort and its mechanisms' design. Peru, where local districts retain discretion over property taxation, leading to substantial variation in collection practices, offers an ideal case study. This variation allows us to separate differences in revenues due to central policy from those reflecting local fiscal effort. Moreover, in 2010 Peru introduced the Plan de Incentivos a la Mejora de la Gestión Municipal (Incentives Plan to Improve Municipal Management) to encourage districts to strengthen revenue mobilization through specific-purpose transfers. This program rewards districts that increase revenues relative to a baseline period. Using a nationwide panel of districts covering the years 2007–2017[1] and combining fiscal and socioeconomic data with quasi-experimental methods, we ask: *"What would tax collection in Peruvian districts have looked like had the incentive program not been implemented?"*. The use of a district-level panel helps us avoid the unobserved heterogeneity that complicates cross-country analyses that

[1] The program saw significant changes in 2018 that limit the possibility of a credible identification strategy thereafter.

address this question.[2]. The paper also sheds light on the mechanisms through which incentive transfers affect tax effort. By conditioning central government transfers on municipal revenue performance, the program directly ties local fiscal actions to the final resources the districts receive from the center, thereby altering their incentives. This design provides a rare opportunity to test how institutional arrangements shape the responsiveness of subnational governments to fiscal incentives and how low subnational tax efforts may be addressed in other countries.

Our results show that Peruvian districts participating in the program significantly increased their fiscal effort, resulting in higher tax revenues after 2010. These effects strengthen over time and are particularly pronounced among districts that more fully engaged with the program. The findings are robust across multiple econometric specifications. Specifically, we find that first of all, districts participating in the program significantly increased their fiscal effort, resulting in higher tax revenues after 2010. Under an intention-to-treat framework, we see significant increases beginning in 2015. In contrast, under an on-off treatment, the effects are more precisely estimated and emerge more clearly: depending on the district grouping, the PI increased property tax collection by 14.8 to 20.9% to 3.1 to 3.3 soles per capita (approximately USD 0.9 to 1).

Second, these effects vary substantially across districts with different characteristics. Districts with high canon transfers (sharing in the royalties from mineral extracting companies) responded approximately four times as strongly as those with low natural resource revenues; similarly, highly indebted districts exhibited significantly larger treatment effects than low-debt districts, while districts with higher baseline property tax collection—a proxy for administrative capacity—showed effects up to five times larger than low-capacity districts. Low-poverty and urban districts also responded more strongly than high-poverty and rural districts. Finally, we observe consistent evidence of anticipation effects, with districts strategically reducing collection in 2009 to establish lower baselines. However, this gaming behavior does not negate the program's longer-term positive impacts. Our findings are robust across multiple econometric specifications and pass standard diagnostic tests for parallel pretrends and the absence of carry-over effects.

The remainder of the paper proceeds as follows. Section 2 reviews the relevant literature. Section 3 describes the property tax framework and the incentive program in Peru. Section 4 presents the data and Section 5 details the empirical strategy. Section 6 reports the findings, heterogeneity results, and robustness checks. Section 7 concludes.

[2]In Peru, there are two types of municipalities: provincial (covering provinces) and district (covering smaller areas within provinces). We focus on district municipalities and use the term "district" to refer to them throughout the paper.

2 Where do we stand? A brief review of the literature

While there is a substantial literature on the impact of transfers (Lago et al., 2024), including unconditional transfers such as revenue-sharing and equalization transfers (Bahl, 2009; Slack, 1980; Ajefu and Ogebe, 2023) and the different types of conditional transfers, on subnational government tax effort (Al-Samarrai and Lewis, 2021; de Carvalho Filho and Litschig, 2022), the literature on the effectiveness and impact of dedicated transfers with regard to incentivizing subnational tax effort is quite limited.

The existing literature, which has focused on the effects of central government transfers on subnational fiscal effort, has generally yielded ambiguous results for developed economies. On the one hand, a handful of studies have found a negative impact of central government grants on subnational tax revenue, with the effect being stronger for untied or unconditional grants that are predictable and unvarying (Baretti et al. (2002); Rajaraman and Vasishtha (2000); Dash and Raja (2013); Liu and Zhao (2011)). On the other hand, a group of studies has found that central government transfers have positive effects on local government tax and nontax revenue collections, especially when grants are made using population-based formulas (Buettner (2006), Miyazaki (2020)).

In developing economies, where decentralized institutions and tax bases are typically weaker, subnational governments are more likely to rely on grants from central governments (Lago et al. (2024)). Nevertheless, there are studies that find a negative (crowding out) effect of national grants on subnational collection that is more pronounced than in high-income countries (Canavire-Bacarreza et al. (2012); Mogues and Benin (2012); Bhadra (2016); Lewis and Smoke (2017); Nath and Madhoo (2022)). On the other hand—and not contradictorily, in fact—some studies have found a crowding-in effect when potential revenue is used instead of actual revenue in the allocation formulas (Brun and El Khdari (2016); Brun and Sanogo (2017); Masaki (2018)).[3]

In the specific case of Peru, several studies have examined the impact of central government transfers, including revenue sharing, on local government tax efforts. For example, Melgarejo and Rabanal (2006) find that central government transfers have a positive effect on local tax collection. However, this effect differs between oil-producing districts (which therefore receive higher transfers from natural resource revenue) and non-oil-producing districts.

Exploring the diverse mechanisms through which central governments can incentivize local government tax collection efforts beyond conventional methods, such as transfers and grants, reveals several alternative strategies, particularly those related to competition, policy frameworks, and regulatory structures. Competition is a strategy employed by central governments to encourage local governments to refine their tax collection strategies, thereby attracting businesses and investments (Liu and Martinez-Vazquez (2014), Liu et al. (2020)). Thus, when local governments compete for economic activity, they may lower tax rates, making it crucial for them

[3]See Lago et al. (2024) for an extensive review of the literature on the effect of intergovernmental grants.

to enhance overall tax compliance and collection efforts.

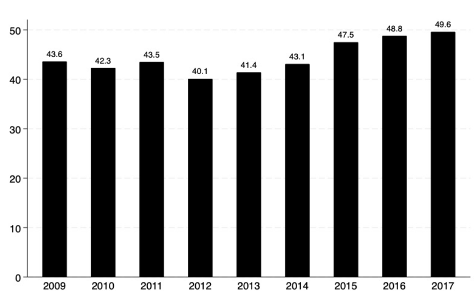
Fiscal reforms have empowered local governments to make decisions regarding taxation and expenditure at the local level. Implementing regulations that enable accountability and provide a clear framework for tax administration increases the likelihood that local governments will optimize their tax collection processes (Oi, 1992). Moreover, local tax collection intensity can be affected by debt repayment pressures for local governments (Tang et al., 2023).

3 Institutional background

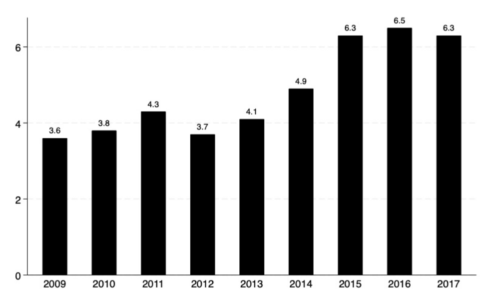
3.1 Property tax in Peru

The property tax in Peru is a standard annual based on the assessed value of urban and rustic properties, including land, buildings, and fixed or permanent installations, regardless of whether they are part of the main parcel. The property owner is liable for the property tax on January 1 each year. The tax base is determined by municipal governments, using property valuations methods established by the Ministry of Housing, Construction, and Sanitation. Among the six local taxes under municipal administration,[4] the property tax stands out, because districts are responsible not only for setting the taxable base but also for collection, inspection, resolution of disputes, application of penalties, and enforcement of payments (MEF (2016)).

The fiscal relevance of the property tax in Peru has steadily increased over the period of study. As shown in Figure 1a, the property tax's share in total local tax collection rose from 43.6% in 2009 to 49.6% in 2017. A similar trend is visible when total municipal income is considered: the share of property tax revenues rose from 3.6% in 2007 to 6.3% in 2017 (Figure 1b). Despite this growth, Peru remains an underperformer by international standards. Property tax revenues average just 0.2% of GDP, well below the 0.6% average for developing countries and the 0.37% recorded across Latin America (Sepulveda and Martinez-Vazquez).



(a) Property Tax's Share of Local Taxes



(b) Property Tax's Share of Total Local Governments' Income

Figure 1: Property Tax, by Year

[4]The other five are the *alcabala* (levied on property sales and transfers), the vehicle tax, the betting and gambling tax, and the tax on public and nonsport shows. While the property tax is assigned exclusively to districts, others, such as the vehicle tax, are collected by provincial municipalities.

The distribution of property tax revenues is highly concentrated geographically. In 2017, half of all collections were concentrated in just 25 districts (out of a total of 1,834), with 3 in Lima—Santiago de Surco, San Isidro, and Miraflores—together accounting for 14.6%. Eighteen of the top-yielding districts are located within Lima, each serving populations of around 250,000. The remaining high-revenue districts are found in Callao, La Libertad, Arequipa, Lambayeque, Piura, Tacna, and Cajamarca. These districts are among the wealthiest in the country, with poverty rates between 10 and 20% and fewer than 5% of households lacking access to water and sanitation services. They are predominantly situated in the coastal urban corridor, the most developed region of Peru. The national averages paint a more challenging picture: poverty stood at 21.7% in 2017 (INEI (2017)), and 10.6% of households lacked access to water and sanitation services (INEI (2018)).

3.2 The Incentives Plan

The Incentives Plan to Improve Municipal Management (hereafter, the Incentives Plan or IP) was launched in 2010 as part of the Ministry of Finance's performance-based budgeting strategy introduced the previous year. Its stated objectives were to encourage districts to strengthen local tax collection, improve the execution of public investment, and contribute to the reduction of chronic child malnutrition. Participation was mandatory for all Peruvian districts.

Under the program, districts received transfers conditional on meeting predefined performance targets. Up to seven goals were established, grouped into three programmatic areas. The first area focused on municipal tax collection, with Goal 1 specifically targeting property tax revenues. The second addressed public investment, with Goal 2 covering expenditures on water and sanitation and Goal 3 expenditures falling under the Articulated Nutritional Program. The third area aimed at reducing chronic childhood malnutrition: it included Goal 4 (establishing registries of children under five), Goal 5 (enrollment of children under five in the integral health insurance system), Goal 6 (monitoring growth and development for infants under one year), and Goal 7 (implementation of household targeting systems). Districts that failed to meet their assigned targets received no transfer, while unallocated funds were redistributed as top-up transfers to compliant districts according to a formula established by the Ministry of Finance.

Designing these goals required the consideration of Peru's substantial district heterogeneity. In 2009, the country had 1,834 districts, which the Incentives Plan classified into three categories based on the number of urban dwellings: districts hosted by major cities (249 in total), districts with more than 500 urban dwellings (555), and districts with fewer than 500 urban dwellings (1,030).[5] This classification was later modified in 2012, when the Incentives Plan merged with the Plan de Modernización Municipal (PM; Municipal Modernization Plan). The merged program,

[5] A parallel classification based on "economic needs" was included in Supreme Decree 003-2010-EF but never operationalized.

renamed the Programa de Incentivos a la Mejora de la Gestión y Modernización Municipal (Incentives Plan to Improve Municipal Management and Modernization)[6] refined the earlier categories by splitting districts hosted by major cities into two subgroups, Type A and Type B, based on social, demographic, and geoeconomic characteristics. The final typology included Type A (40 districts), Type B (209), Type C (555 districts with more than 500 urban dwellings), and Type D (1,030 with fewer than 500 dwellings), where Type A were the most developed districts in the country. As illustrated in Figure 2, Types A and B were concentrated along the coast, while Types C and D were located primarily in the Andean highlands and Amazonian regions. Importantly, while all districts participated in the Incentives Plan, Goal 1 (property tax collection) applied only to Types A and B, whereas Types C and D were subject to other programmatic goals.

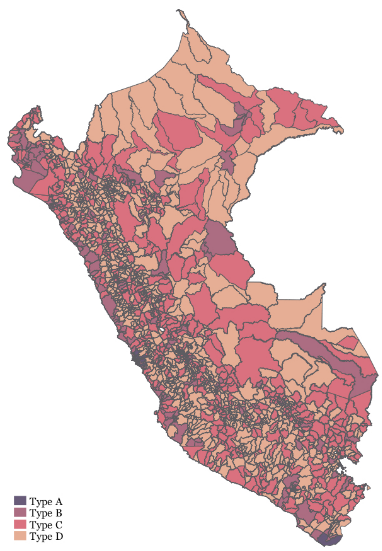


Figure 2: Incentives Plan Classification

The Incentives Plan has since undergone several minor reforms that have changed the design of programmatic areas. For the purpose of this study, however, attention is restricted to the municipal tax collection area and in particular the property tax collection goal, which has not changed.

Between 2010 and 2011, compliance was assessed against a moving five-year average of property tax collections, with targets for revenue growth ranging from 3 to 30% depending on district conditions. Evaluations occurred twice yearly: mid-year and at the end of the fiscal year. In 2012 a uniform annual growth requirement of at least 4% was added. In 2013 and 2014, the growth targets increased and differentiated between Type A and Type B districts, imposing stricter requirements on the latter. Starting in 2015, evaluation shifted to an annual basis, with again progressively higher thresholds for Type B districts. In 2016, quantitative percentage tar-

[6]For simplicity, we continue to refer to it as the Incentives Plan.

gets were abandoned and individualized minimum collection amounts were established for each district. Finally, in 2017, these collection benchmarks were replaced altogether with administrative and managerial targets, such as the implementation of internal controls in budget and procurement systems. The evolution of these requirements is summarized in Table 1.

Table 1: Property Tax Goal Evolution, 2010–2017

Year	Period	Goal	Cut-off
2010	1st sem.	Increase over *half* max in last 5 yrs: +3 to +30%	Aug 2010
	2nd sem.	Increase over max in last 5 yrs: +3 to +30%	Dec 2010
2011	1st sem.	Same as 2010 (half max)	Aug 2011
	2nd sem.	Same as 2010 (max)	Dec 2011
2012	1st sem.	+4% vs. *half* of 2011	Jul 2012
	2nd sem.	+4% vs. 2011 total	Dec 2012
2013	1st sem.	+12% (Type A), +17% (Type B) vs. *half* of 2012	Jul 2013
	2nd sem.	Same vs. 2012 total	Dec 2013
2014	1st sem.	+12% (A), +14% (B) vs. Jul 2013	Jul 2014
	2nd sem.	Same vs. Dec 2013	Dec 2014
2015	Annual	+13% (A), +19% (B) vs. Dec 2014	Dec 2015
2016	Annual	administrative goal: individualized amounts vs. Dec 2015	Dec 2016
2017	Annual	administrative/managerial goal	Dec 2017

Note: Between 2010 and 2014, the property tax goal was evaluated twice a year. For this study, we use the annual evaluation, covering January–December. Since 2015, the goal has been evaluated only once a year (December).

Across its different iterations, the property tax goal consistently demanded significant collection efforts from Type A and Type B districts. Although the design shifted from quantitative targets to more-administrative objectives, the underlying intent remained the same: to compel districts to strengthen their capacity to mobilize property tax revenues in exchange for fiscal incentives.

4 Data

To examine the impact of the Incentives Plan on municipal tax effort, we compiled a comprehensive district-level data set spanning 2007 to 2017. As argued above, the choice of period reflects the availability of data as well as the significant changes in the program that limit the possibility of identifying the effects. This data set integrates information on municipal participation in the Incentives Plan and the achievement of programmatic goals, as reported on the Ministry of Finance's website,[7] with fiscal indicators, including property tax collection, also sourced from the Ministry of Finance. Socioeconomic indicators from the Chronic Malnutrition Map Project, produced by Peru's Instituto Nacional de Estadística e Informática (INEI; National Statistics Bureau), provide detailed district-level measures relevant for understanding municipal

[7] https://www.gob.pe/mef

characteristics and the likelihood of being targeted by the Incentives Plan.

Given the program's characteristics, the unit of analysis is the local district or local government (LG), the third-most disaggregated layer of government in Peru after the national and regional governments.[8] For our main estimations, the outcome variable is annual property tax collection from 2007 to 2017 for Type B and Type C districts.[9]. The outcome variable directly reflects the Incentives Plan goal of increasing property tax collection over time.

To identify districts subject to the program, we use the Ministry of Finance's 2009 municipal classification, which relied on social, demographic, and geoeconomic criteria and served as the main source for the typology.

Additional covariates capture other potential factors influencing tax collection performance.[10] On the demand side, central government transfers and stability of revenue flows affect districts' incentives to increase tax effort. Supply-side determinants, such as population size and economic development, define the tax base and the revenue potential. Fiscal sustainability is proxied by measures of current liabilities or debt level, reflecting the district's capacity to manage financial assets and the need to generate new resources.

External transfers can also shape tax collection incentives. The canon, a revenue-sharing scheme from natural resource rents, is allocated according to the presence of resource exploitation projects, while FONCOMUN is an equalization grant targeting underdeveloped districts based on population, mortality, the Unsatisfied Basic Needs index, and rural population share; it should be noted that no measure of fiscal capacity is included in the formula.[11]

In addition, we include socioeconomic characteristics to capture supply-side effects on tax collection. These variables describe municipal development and tax base potential and include household access to drinking water, sanitation, and registered property ownership; we also include a set of economic development characteristics measured by unmet basic needs. Descriptive statistics for all covariates are reported in Table 5 in the Appendix.

5 Empirical Approach

To evaluate the impact of the Incentives Plan on property tax collections, the ideal empirical design would compare a clearly defined treatment group with a control group from the program's inception through its conclusion. However, this design is not feasible because the Incentives Plan reassesses districts annually to determine compliance with the goals. Consequently, districts can comply or not comply (i.e., enter and exit the program multiple times), creating a dynamic treatment assignment that requires an empirical strategy capable of accommodating this "on–off" structure.

[8] As explained above, Peru has two municipal levels; our focus is on the more disaggregated district level.

[9] https://www.mef.gob.pe/es/seguimiento-de-la-ejecucion-presupuestal-consulta-amigable

[10] See also Canavire-Bacarreza et al. (2012) and Bird (2011) for studies on demand- and supply-side factors affecting local tax performance.

[11] Canon and FONCOMUN.

5.1 Treatment Definition

To estimate the effects of the Incentives Plan, we employ two complementary strategies. First, to examine the participation effect we adopt an intention-to-treat (ITT) approach, defining all Type B districts as the treatment group, because they are required to comply with the property tax target, and Type C districts as the control group, because they are exempt from this requirement. We exclude Type A districts because they are highly developed and substantially more populous, making them less comparable to Type C. Similarly, Type D districts are excluded for the reason that most either do not collect property taxes or generate negligible revenue relative to their budgets; in addition, they are predominantly small and rural, which limits their comparability with Types B and C.

Second, because program participation alone may not capture the true effect, we focus on the actual accomplishment of goals to receive fiscal incentives. In this strategy, we aim to capture the incentive effects. Thus, treated units are defined as Type B districts that received the Incentives Plan transfer in a given year. This refinement allows us to isolate the impact of the monetary incentives themselves beyond eligibility status.

5.2 Counterfactual Estimators

To implement the two sets of treatments described in the previous section, we begin by considering a canonical differences-in-differences (DiD) approach, under an ITT interpretation. The central assumption of the DiD framework is "an additive structure for potential outcomes in the absence of treatment" (Angrist and Pischke, 2009). Formally, suppose that

$$E(Y(0)_{it}|i,t) = \gamma_i + \lambda_t, \tag{1}$$

where i indexes districts and t denotes time. This formulation implies that, absent the Incentives Plan, property tax collection is determined by a district-specific effect and a common time effect. Assuming a constant treatment effect β, the regression model becomes

$$Y_{it} = \alpha + \gamma D_i + \lambda d_t + \beta(D_i \times T_t) + \varepsilon_{it}, \tag{2}$$

where D_i is a dummy equal to one if the district participates in the property tax goal of the Incentives Plan, T_t is a time dummy, and $E(\varepsilon_{it}|i,t) = 0$. To allow for richer specifications, we extend this model to incorporate time-varying covariates:

$$Y_{it} = \alpha + \gamma D_i + \lambda d_t + \beta(D_i \times T_t) + X'_{it}\delta + \varepsilon_{it}, \tag{3}$$

where X_{it} is a vector of district- and time-varying controls. In this setting, β identifies the average treatment effect on the treated (ATET) under the standard assumption of parallel trends.

This approach is valid under an ITT interpretation. However, the nature of the program implies that a district may be treated in one year but not the next. Thus, the canonical DiD model cannot capture the dynamic entry and exit of districts from the program.

To address this limitation, we follow Canavire-Bacarreza et al. (2025) to extend the framework to account for nonstaggered treatment adoption. Consider the panel $\{\{\boldsymbol{Y}_{it}, \boldsymbol{X}_{it}, D_{it}\}_{i=1}^{N}\}_{t=1}^{T}$, where $\boldsymbol{Y}_{it}$ denotes property tax collection, $\boldsymbol{X}_{it}$ is a vector of covariates, and D_{it} is a binary indicator of participation in the Incentives Plan. Our parameter of interest is

$$ATT_s = \mathbb{E}\left[\delta_{it} | D_{i,t-s} = 0, D_{i,t-s+1} = ... = D_{i,t} = 1, C_i = 1\right], \quad s > 0, \tag{4}$$

where $\delta_{it} \coloneqq Y_{it}(1) - Y_{it}(0)$ and $C_i = 1$ if district i switches treatment status at some point. Equation (4) captures the ATET s periods after first entering the program ($ATET_s$), reflecting the impact of the Incentives Plan on property tax collection over time.

Most causal inference estimators are designed for staggered adoption settings (Callaway et al., 2024), where once a unit is treated, it remains so permanently. This assumption does not hold here, as districts may repeatedly achieve or fail to achieve the property tax goal. Accordingly, we employ the counterfactual estimation approach of Liu et al. (2024), which explicitly accommodates treatments that can switch on and off across periods.

Compared to the alternatives, this estimator offers important advantages. Approaches such as those of Sun and Abraham (2021) and Callaway et al. (2024) allow for heterogeneous treatment effects (and continuous treatments in the latter case), but they are restricted to staggered adoption. The estimator of De Chaisemartin and d'Haultfoeuille (2020) accommodates nonstaggered designs, but relies solely on outcomes immediately before and after treatment, discarding substantial information. By contrast, the methodology of Liu et al. (2024) exploits the full panel and is more robust than conventional two-way fixed effects (TWFE) when treatment effects are heterogeneous and unobserved time-varying confounders exist (Imai and Kim, 2021).

The Liu et al. (2024) procedure imputes the counterfactual outcomes that treated districts would have experienced had they not participated in the Incentives Plan. It treats observed outcomes under treatment as missing, estimates predictive models using untreated units, and imputes counterfactuals for treated observations. Within this framework, three estimators are available: Fixed Effects (FEct), Matrix Completion (MCct), and Interactive Fixed Effects (IFEct). We rely on the IFEct estimator, selected via cross-validation; however, our results are robust to alternative specifications and to different ATET variance estimators.

Implementation requires imputing untreated potential outcomes, $Y_{it}(0)$, for districts exposed to the Incentives Plan. Following Gobillon and Magnac (2016), we estimate

$$Y_{it}(0) = \boldsymbol{X}_{it}'\boldsymbol{\beta} + \alpha_i + \xi_t + \boldsymbol{\lambda}_i f_t + \epsilon_{it}, \quad i = 1, ..., N, \quad t = 1, ..., T, \tag{5}$$

where $\boldsymbol{X}_{it}$ denotes observed controls, $\boldsymbol{\beta}$ is a vector of coefficients, α_i and ξ_t are district and time fixed effects, f_t captures unobserved shocks with heterogeneous impacts across districts, and ϵ_{it} is an idiosyncratic error. Let $\boldsymbol{U}_{it} \coloneqq \alpha_i + \xi_t + \boldsymbol{\lambda}_i f_t$ for shorthand.

This model relies on three assumptions:

$$Y_{it}(0) = f(\boldsymbol{X}_{it}) + h(\boldsymbol{U}_{it}) + \epsilon_{it}, \tag{6}$$

$$f(\boldsymbol{X}_{it}) = X_{it}'\boldsymbol{\beta}, \tag{7}$$

$$h(\boldsymbol{U}_{it}) = \alpha_i + \xi_t + \boldsymbol{\lambda}_i f_t, \tag{8}$$

$$\epsilon_{it} \perp \{D_{js}, \boldsymbol{X}_{js}, \boldsymbol{U}_{js}\} \quad \forall i, j \in \{1, ..., N\},\ s, t \in \{1, ..., T\}, \tag{9}$$

where $f(\cdot)$ and $h(\cdot)$ are parametric functions. Equation (6) imposes additivity, (7) assumes linearity in observables, (8) restricts unobserved heterogeneity to a low-rank factor structure, and (9) imposes strict exogeneity, ensuring quasi-random assignment conditional on observables and latent factors.

The estimation algorithm of Liu et al. (2024) involves four steps: (1) the estimation of equation (5) on untreated observations using the linear factor model of Bai (2009), yielding $(\hat{\beta}, \hat{\alpha}_i, \hat{\xi}_t, \hat{\lambda}_i, \hat{f}_t)$; (2) the imputation of untreated potential outcomes $\hat{Y}_{it}(0)$ for treated districts; (3) the computation of treatment effects $\hat{\delta}_{it} = Y_{it}(1) - \hat{Y}_{it}(0)$; and (4) the estimation of

$$A\hat{T}T_s = \frac{1}{|\mathcal{S}|} \sum_{i,t \in \mathcal{S}} \hat{\delta}_{it}, \tag{10}$$

where $\mathcal{S} = \{(i,t) | D_{i,t-s} = 0, D_{i,t-s+1} = ... = D_{i,t} = 1, C_i = 1\}$ and $|\mathcal{S}|$ is the number of treated district-periods meeting these conditions.

5.3 Identification

The identification of causal effects in our setting relies on the counterfactual estimation framework developed by Liu et al. (2024), which explicitly accommodates treatments that can switch on and off across periods. This approach is particularly well suited to the Incentives Plan, where districts can comply with or fail to meet the property tax goal in any given year, creating a dynamic pattern of treatment assignment.

The identification of causal effects in our case relies on the unexpected achievement of the Property Tax goal set each year by each district. Our treatment definition assumes that the achievement of the goals set each year for the districts is unpredictable. [12] Therefore, we

[12] Recall that even though the Property Tax goal was present every year, each year the setting of the goal levels changed and became stricter, thus making it impossible to foresee whether any given district will accomplish the goal set next year.

assume that achieving the goal is unexpected and could not have been predicted in advance, allowing us to consider the treatment effect as exogenous. In cases where the achievement of the goal is predictable, it would result in larger ex ante budgets for districts that can predict such achievement, taking into account the amount provided by the Incentives Plan. If this were the case, the results would be biased.

Several potential threats to identification warrant discussion. A primary threat to identification is the possibility that districts strategically manipulate their property tax collection in anticipation of the program. Our results provide direct evidence of this behavior: we observe negative, statistically significant effects at the onset of treatment (time 0) suggesting that districts reduced collection in 2009—after the program was announced but before implementation—to establish lower baselines against which future performance would be measured. To address this concern, we implement several strategies.

First, we explicitly test for anticipation effects using the placebo test proposed by Liu et al. (2024), which examines whether there are significant effects in periods -2 and -1 (i.e., 2008 and 2009). The presence of a significant negative impact in period -1 confirms anticipation. Still, the absence of effects in period -2 suggests that gaming was limited to the immediate pretreatment period and did not reflect longer-term differential trends.

Second, we include a rich set of pretreatment covariates that proxy for district sophistication and governance quality, including administrative capacity (proxied by baseline property tax collection and budget execution rates), fiscal management (proxied by debt levels), and institutional quality (proxied by compliance with reporting requirements). By conditioning on these observables, we reduce the scope for unobserved heterogeneity in anticipation behavior to confound our estimates.

Third, we conduct sensitivity analyses that exclude 2009 from the estimation sample, effectively treating the anticipation period as part of the treatment. Results from these specifications (available upon request) yield similar treatment effect estimates, suggesting that anticipation effects, while present, do not fundamentally bias our conclusions about the program's impact in subsequent years.

A second threat arises from time-varying unobservables that may be correlated with both treatment assignment and outcomes. For example, districts experiencing positive economic shocks (such as the opening of new businesses or infrastructure investments) might simultaneously find it easier to meet the property tax goal. They would naturally experience higher tax revenues even in the absence of treatment. Conversely, districts experiencing adverse shocks (such as natural disasters or economic downturns) might be unable to meet collection targets and would exhibit lower revenues for reasons unrelated to the program.

The interactive fixed effects (IFE) model partially addresses this concern by allowing for district-specific exposure to standard time-varying shocks. However, it cannot fully control for district-specific shocks that are uncorrelated across units. We address this residual concern by including time-varying covariates that capture primary sources of exogenous variation in district conditions, including canon transfers (which fluctuate with commodity prices and extraction levels), and FONCOMUN transfers (which vary with population and poverty measures). These variables proxy for observable economic shocks, thereby reducing the scope for omitted-variable bias.

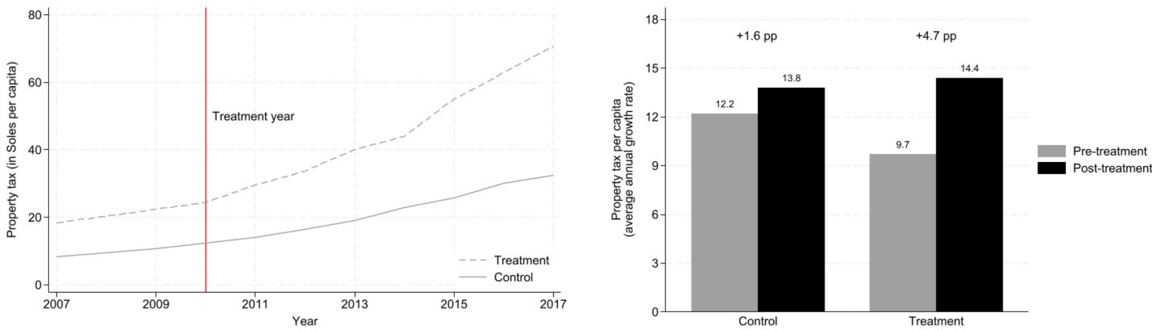
In summary, our identification strategy relies on the assumption that, conditional on observed covariates and latent factors, treatment assignment is uncorrelated with unobserved shocks to property tax collection. While this assumption is inherently untestable, we provide extensive evidence supporting its plausibility: (1) treatment and control districts exhibit parallel pretrends after conditioning on observables and factors; and (2) treatment effects are not driven by compositional changes or selective attrition. The primary threat to identification is the presence of anticipation effects in 2009, for which we explicitly account in our estimation and interpretation. While no identification strategy is perfect, the weight of evidence suggests that our estimates provide credible causal estimates of the Incentives Plan's impact on property tax collection in participating districts.

6 Results

6.1 Pretrends Analysis

Figure 3a and Figure 3b illustrate the temporal evolution of property tax collection across treatment and control districts from 2007 to 2017. Before 2010, the treatment group (Type B districts) consistently had higher property tax levels than the control group, reflecting their larger tax bases and greater administrative capacity. Despite this level difference, both groups displayed parallel upward trends, suggesting comparable pretreatment growth dynamics and supporting the plausibility of the parallel trends assumption necessary for a canonical DiD framework.

The introduction of the Incentives Plan in 2010 appears to have accelerated property tax growth in the treatment group relative to the control group. As Figure 3a shows, the slope of the treatment group's trajectory became markedly steeper following implementation, while the control group maintained a more modest upward trend. Correspondingly, Figure 3b indicates that pretreatment annual growth rates were slightly higher in the control group (12.2%) than in the treatment group (9.7%), but this pattern reversed following program implementation: the treatment group's growth rate increased by 4.7 percentage points, compared with a 1.6 percentage point increase in the rate of the control group.



(a) Property Tax District Average, by Year

(b) District Average Annual Property Tax Growth Rate

Figure 3: Trends Analysis

6.2 Participation Effects

Figure 4 shows the results from the DiD estimator from 2008 to 2017 (see Table 6 in the appendix for numerical results). The black line represents the ATET of the districts that participated in the Incentives Plan, obtained using the DiD estimator. Moreover, each value represents, on average, the amount of property tax (in soles per capita) collected by Type B districts (treatment group) compared to Type C districts (control group), while controlling for supply and demand covariates. The gray dashed lines represent the 95% confidence interval. Note that in general there is a positive effect of the Incentives Plan on property tax; however, the effect is not significant following the implementation of the Incentives Plan. These results fail to provide evidence of the Incentives Plan's effectiveness in increasing property tax collection in a typical Type B district.

Nevertheless, it seems puzzling that there are no significant effects after the implementation of the Incentives Plan. This might be the result of capturing the ITT rather than the ATET, because the treatment is defined as only participating in the program rather than achieving the proposed goal.

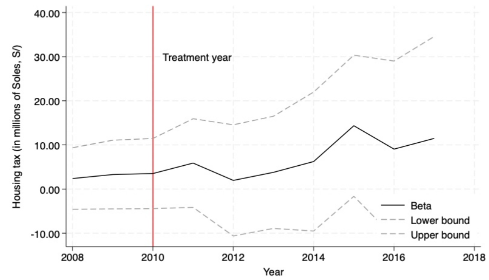
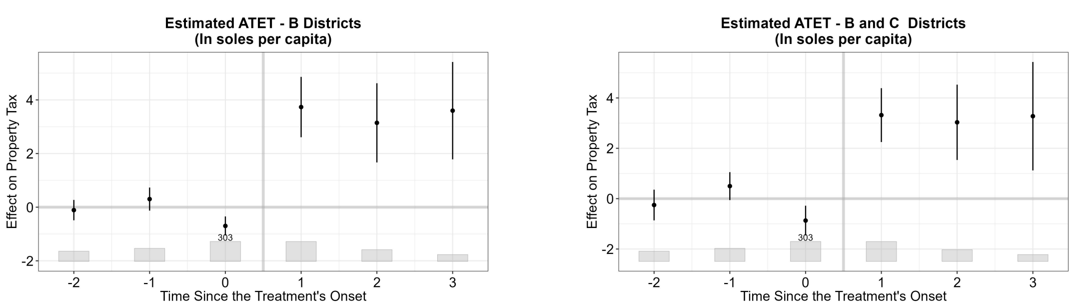


Figure 4: Differences-in-Differences Estimation
Note: The black lines represent the estimated treatment effect for a given year and the dashed gray lines represent the 95% confidence intervals for the respective point estimates.

6.3 Incentives Effects

The Incentives Plan's participation effects ignore the fact that among the districts participating in the property tax goal, only a select few achieve the required tax-collection threshold to receive the monetary incentive. In this section, we will determine whether there is a statistically significant difference between districts that receive monetary incentives and those that do not. This exercise can provide additional evidence to support our initial hypothesis that monetary incentives at the local level significantly affect local tax collection performance, particularly for property taxes. To achieve this goal, we initially drop the Type C districts from our original sample in the first estimation results and then use them only as an extra control group. These districts do not participate in the Incentives Plan property tax goal; consequently, they cannot be considered for the monetary incentive. Additionally, we include the Type A districts (a total of 40) that we initially excluded because they are more economically developed compared to the rest of the sample and, as a consequence, could increase heterogeneity in our original sample. Following Liu et al. (2024), we estimate the on-off participation treatment. Table 7 in the appendix shows that, on average, around 57% of the districts complied with the goal and were thus treated.



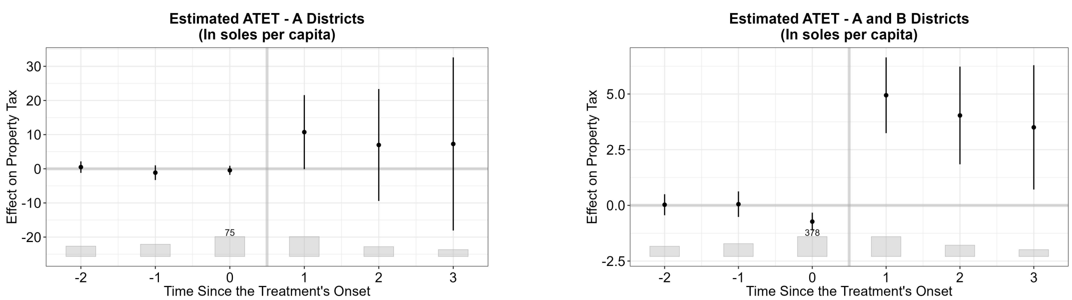
(a) B Districts

(b) B and C Districts

Figure 5: ATET—On-Off Treatment

Figure 5 shows the results for this exercise. Figure 5a depicts the results when taking only Type B districts into account, and Figure 5b when Type C districts are included as extra controls. The numerical results are presented in Table 2 and show the effect of 3.3 and 3.2 soles per capita, respectively, after the Incentives Plan, equivalent to 14.8 and 20.9 percent relative to 2009 levels.[13] In both cases, the Incentives Plan has a positive effect after the treatment. Moreover, there is a clear anticipation effect: at time 0 of the treatment, there is a negative, statistically significant effect, suggesting that districts anticipated the treatment (recall that the implementation of the Incentives Plan was in 2010, but it was announced in 2009). Therefore, it is possible to think that districts actively sought to collect lower amounts of property tax so they could more easily achieve the goal set by the Incentives Plan the following year.

When we obtain results for Type A districts alone and for Type A and B districts combined, the effect is larger for Type A districts alone, but it is not statistically significant, as shown in Figure 6. This shows that Type A districts are extremely different from Type B or Type C districts. Because the former are the most developed and wealthy districts, there is no effect on these districts; they are probably already collecting taxes in the most efficient way possible and the Incentives Program, although it provides further incentives, is unable to improve collection efforts for these districts.



(a) A Districts

(b) A and B Districts

Figure 6: ATET—On-Off Treatment

The numerical results for these four exercises are presented in Table 2. The effect of the Incentives Plan on Property Tax is positive and statistically significant in each case with Type B or Type C districts, ranging from 3.2 to 3.3 soles per capita (equivalent to 15 to 21%, depending on the district's development stage). We also include all the diagnostic tests proposed by Liu et al. (2024). Note that the placebo test, pretrends test, and carry-over test indicate that the model was correctly specified. [14] Moreover, because Type A districts do not respond to the incentive, we will focus on Type B and Type C districts from here on.

[13]We express the annual treatment effect as a percentage of baseline (2009) property tax collection for districts with positive baseline collection. The estimated 15–21% annual increase is consistent with the program's design, which sets growth targets ranging from 3 to 30% depending on district conditions and year (table 1).

[14]Note that, because there is a clear anticipation effect, the placebo test was evaluated for periods -2 and -1 before the onset of the treatment.

Table 2: ATET On-Off Treatment by District

District grouping	ATET	Placebo	Pretrends	Carry-over
Type A	8.377	0.976	0.509	0.950
Type B	3.323***	0.691	0.324	0.695
Type A and B	4.260***	0.364	0.970	0.662
Type B and C	3.161***	0.188	0.104	0.356

*** p<0.01, ** p<0.05, * p<0.1
Note: The table contains the estimated ATET and the p-values of the -tests for the nulls of no pretrends, no placebo effects, and no carry-over effects

6.4 Heterogenous Effects

The aggregate treatment effects reported in the previous section may mask important variation across districts with different fiscal and socioeconomic characteristics. To explore this heterogeneity, we partition the sample along five dimensions that theory suggests may shape responsiveness to fiscal incentives: access to natural resource revenues (canon transfers), fiscal stress (debt levels), administrative capacity (baseline property tax collection), economic development (poverty rates), and urbanization. We focus our analysis on Type B and Type B and C district groupings, as these represent the core treatment and treatment-control comparisons of interest.

Table 3 presents the estimated ATET for each subgroup. Several points are worth noting. First, districts with different levels of canon transfers show contrasting responses. Among Type B districts, high-canon jurisdictions report an ATET of 3.49 soles per capita, while low-canon districts do not show a significant effect. When Type C districts are included as additional controls (Type B and C), high-canon districts display an ATET of 4.70 soles per capita, compared to 1.50 soles per capita for low-canon districts. This pattern suggests that the relationship between windfall revenues and fiscal effort is complex and may depend on district characteristics beyond simple fiscal constraints.

Second, highly indebted districts exhibit significantly larger treatment effects than low-debt districts. For Type B districts, the ATET is 4.74 soles per capita in high-debt jurisdictions, compared to 1.90 soles per capita in low-debt districts—a ratio of approximately 2.5 to 1. This pattern persists when Type C districts are included, suggesting that fiscal stress amplifies responsiveness to incentive-based programs, as districts facing binding debt constraints view the Incentives Plan as an opportunity to simultaneously increase revenues and access additional transfers.

Table 3: Heterogeneous Treatment Effects: Summary

	ATET (Soles per capita)		
Subgroup	Type B	Type B and C	Ratio[a]
Panel A: Canon transfers			
Low canon	0.904	1.500**	–
High canon	3.494***	4.697***	3.9×
Panel B: Debt levels			
Low debt	1.904***	1.761**	–
High debt	4.743***	4.156***	2.5×
Panel C: Administrative capacity			
Low property tax	0.688*	0.819***	–
High property tax	4.815***	2.908***	7×
Panel D: Poverty rate			
Low poverty (<30.8%)	3.916***	2.978***	–
High poverty (≥30.8%)	2.907***	1.646***	1.3×
Panel E: Urbanization			
Rural	4.225	7.120***	–
Urban	2.718***	2.904***	–
Baseline (no partitioning)			
All districts	3.323***	3.161***	–

Note: This table summarizes the heterogeneous treatment effects for Type B and Type B&C district groupings. All estimates use the IFE estimator with cross-validation and 500 bootstrap replications. *** p<0.01, ** p<0.05, * p<0.1.

[a] The Ratio column shows the ratio of high to low effects for Type B districts where applicable (high/low for Canon, Debt, Capacity, and Poverty; low/high for Poverty to show larger effect in low-poverty areas).

Third, administrative capacity—proxied by baseline property tax collection in 2009—emerges as the most critical determinant of responsiveness. Districts with high initial property tax levels show effects that are substantially larger than those in low-capacity districts. Among Type B districts, the ATET is 4.82 soles per capita for high-capacity districts, compared to 0.69 soles per capita for low-capacity districts—almost a sevenfold difference. When Type C districts are included as controls, high-capacity districts exhibit an ATET of 2.9 soles per capita, while low-capacity districts show an ATET of only 0.82 soles per capita. This contrast indicates that existing infrastructure, trained personnel, and enforcement mechanisms are necessary prerequisites for districts to capitalize on fiscal incentives.

Fourth, low-poverty districts respond more strongly than high-poverty districts. For Type B districts, the ATET is 3.92 soles per capita in low-poverty jurisdictions, compared to 2.91 soles per capita in high-poverty districts—a difference of approximately 35%. When Type C districts are included, the estimates are lower, but the distinction between districts remains. This reflects both a larger tax base and greater political space to increase property tax collection in wealthier communities, and it goes hand in hand with the administrative capacity, where wealthier districts may have a richer set of infrastructure and personnel to achieve higher collection levels.

Fifth, the evidence on urban versus rural districts is mixed. Among Type B districts, urban areas show an ATET of 2.72 soles per capita, while rural districts exhibit a nonsignificant effect of 4.22 soles per capita. This might be due to most Type B districts' being urban. However, when Type C districts are included as controls, rural districts show a significant ATET of 7.12 soles per capita. This unexpected pattern may reflect measurement issues in rural areas or heterogeneity in the small number of rural Type B districts subject to the property tax goal.

Finally, we observe anticipation effects across several subgroups, consistent with our main findings. However, all specifications pass standard diagnostic tests for parallel pretrends and absence of carry-over effects (p-values are reported in table 8 in the appendix), providing confidence in the validity of our estimates.

These findings have important policy implications. The stark differences in responsiveness across jurisdictions with varying administrative capacity suggest that incentive programs alone may be insufficient for low-capacity jurisdictions. Complementary investments in cadastral modernization, staff training, and enforcement mechanisms appear necessary. Moreover, the finding that highly indebted and low-poverty districts respond more strongly indicates that the program may inadvertently widen disparities between rich and poor jurisdictions and between capable and incapable districts. To mitigate this risk, future program designs could incorporate progressive features, such as differentiated targets calibrated to local capacity, tailored technical assistance for disadvantaged districts, higher per capita transfers for poorer jurisdictions, or incentive schemes based on estimated potential property tax revenue that explicitly account for differences in baseline conditions and structural determinants across jurisdictions.

6.5 Robustness Checks

We subject our main results to a comprehensive battery of robustness checks to assess the sensitivity of our findings to alternative specifications, sample definitions, and estimation methods. This section summarizes the key robustness exercises; detailed results are reported in table 4.[15]

Alternative Outcome Specifications: First, we address concerns about the distributional properties of the outcome variable. Property tax collection is highly skewed, with a small number of districts accounting for a large share of total revenues. To account for this, we reestimate

[15] The placebo, pretrends, and carry-over tests are available upon request.

the model using log(property tax per capita + 1) as the dependent variable. The results are qualitatively similar to our baseline estimates, with significant positive treatment effects across all district groupings.

Second, we winsorize the outcome variable at the 95th percentile to mitigate the influence of extreme values. Again, the estimated treatment effects remain positive and statistically significant, with magnitudes similar to our baseline results. This suggests that our findings are not driven by a small number of outlier districts.

Third, we construct an alternative outcome measure defined as property tax revenue as a percentage of the Presupuesto Institucional Modificado (PIM; Modified Institutional Budget). This specification addresses concerns that the program may have increased property tax collection mechanically by increasing overall budgets rather than through genuine improvements in tax effort. The estimated treatment effects remain positive and significant, indicating that the program increased property tax revenues relative to total income.

Sample and Time Period Variations: A primary concern is the presence of anticipation effects. Districts may have strategically reduced property tax collection in 2009—after the program was announced but before implementation—to establish lower baselines. To address this, we exclude 2009 from the estimation sample. The results are qualitatively unchanged, with treatment effects of similar magnitude and significance to our baseline estimates. We also examine sensitivity to the choice of pretreatment period by dropping 2007 and starting the analysis in 2008. This specification yields nearly identical results, suggesting that our findings are not sensitive to the inclusion of the earliest baseline year. Finally, we exclude districts in the Lima metropolitan area, which are substantially larger and more developed than the rest of the sample. The results remain qualitatively similar, suggesting that our findings are not driven by the inclusion of these atypical districts.

Table 4: Robustness Checks: Alternative Specifications

	ATET estimate (Soles per capita)	
Specification	Type B	Type B&C
Panel A: Baseline and outcome variations		
Baseline	3.323***	3.161***
Log outcome	0.106***	0.068***
Winsorized outcome	3.078***	2.939***
Share of revenue	0.579**	0.247
Panel B: Sample variations		
Exclude 2009	3.611***	3.602***
From 2008 only	3.705***	3.467***
Exclude Lima	3.325***	3.148***
Panel C: Alternative estimators		
Fixed effects (FE)	3.146**	2.859**
Matrix completion (MC)	3.144***	3.252***

Note: This table reports estimated ATET from alternative specifications. The baseline specification uses property tax per capita as the outcome, the full sample from 2007–2017, and the IFE estimator with cross-validation to select the number of factors. Panel A varies the outcome specification, Panel B varies the sample, and Panel C varies the estimator. Standard errors (not reported) are computed using 500 bootstrap replications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Alternative Estimators and Inference: The IFE estimator employed in our main analysis relies on a low-rank factor structure to capture unobserved time-varying heterogeneity. To assess sensitivity to this assumption, we reestimate the model using alternative methods.[16]. All three methods yield similar estimates of the treatment effect, suggesting that our results are robust to the choice of estimator.

Additional Sensitivity Analyses: We explore robustness to covariate selection by estimating the model with different sets of control variables: (1) a minimal specification with only canon, FONCOMUN, and PIM; (2) an extended specification that adds poverty controls; and (3) our baseline specification. Treatment effect estimates are stable across all specifications, suggesting that our results are not sensitive to the choice of covariates.

Across all robustness checks, the estimated treatment effects remain positive, statistically significant, and of similar magnitude to our baseline results. The consistency of our findings across

[16] We test three different methods in the R `fect` package to model the fixed effects (FE) estimator and the matrix completion (MC) estimator.

alternative specifications, samples, and estimators provides strong evidence that the Incentives Plan successfully increased property tax collection in participating districts. The primary threat to identification—strategic manipulation of baselines in 2009—is directly addressed by excluding the anticipation year, and results remain robust to this modification.

7 Conclusion and Discussion

Under the Incentives Plan, districts meeting annual property tax targets received transfers from the Ministry of Finance totaling approximately US$217 million between 2010 and 2017. Using the program's design and district-level data from 2007 to 2017, this paper finds that the plan significantly increased property tax revenues, with effects that strengthened over time and varied systematically across districts' fiscal, administrative, and socioeconomic characteristics.

The empirical analysis yields three main findings. First, using a DiD framework under an ITT interpretation, we find that Type B districts—those subject to the property tax performance requirement—realized higher property tax revenues than Type C districts, which were not required to meet the target. However, these effects are not statistically significant. This suggests that conventional ITT estimates may understate short-run impacts in settings where treatment depends on repeated, annual compliance rather than permanent assignment.

Second, when treatment is defined based on actual goal achievement—distinguishing districts that received incentive transfers from those that did not—the effects are sharper and emerge more consistently across district types. Applying the on–off treatment framework of Liu et al. (2024), we estimate positive and statistically significant effects ranging from 3.2 to 3.3 soles per capita, which is equivalent to 15 to 21% relative to 2009 levels. We also document a systematic anticipation effect: districts appear to have strategically reduced property tax collection in 2009, prior to program launch, likely to establish lower baselines for future comparisons. While this behavior reduced short-term revenues, it does not offset the program's longer-run gains.

Third, the heterogeneous-effects analysis shows that responsiveness to incentives is strongly shaped by preexisting conditions. Districts with lower canon transfers and hence greater fiscal constraints exhibited smaller gains than resource-rich districts. Highly indebted districts responded more strongly than low-debt districts, consistent with greater marginal returns to additional fiscal resources. Districts with higher baseline property tax collection, a proxy for administrative capacity, experienced substantially larger effects, underscoring the importance of institutional readiness. Responses were also stronger in low-poverty and urban districts, reflecting differences in tax base characteristics, enforcement capacity, and political constraints.

The observed heterogeneity further indicates that fiscal incentives interact strongly with underlying fiscal, administrative, and socioeconomic conditions. Uniform program design may therefore be both inefficient and potentially regressive, as higher-capacity, wealthier, and more-urban districts are better positioned to respond. More-effective approaches would combine differentiated targets with complementary capacity-building interventions in low-capacity and high-poverty districts, while calibrating rewards to local fiscal potential rather than absolute revenue levels. Finally, the muted response among resource-rich districts suggests that windfall transfers can crowd out local tax effort, pointing to the need for broader fiscal frameworks that condition resource transfers on own-source revenue mobilization or fiscal performance or properly calculate potential revenues that consider fiscal capacity.

Despite these contributions, several limitations remain. We cannot identify the specific mechanisms through which districts increased property tax revenues, such as changes in rates, enforcement, cadastral coverage, or tax base expansion. The analysis also focuses exclusively on property taxes, leaving open the possibility of spillovers—or substitution—across other local revenue sources and expenditure categories. Moreover, while we document strategic baseline manipulation, we do not assess its welfare implications or formally evaluate alternative incentive designs that could mitigate gaming. Finally, the Incentives Plan encompasses objectives beyond property taxation, including public investment and child malnutrition targets, the interactions of which with tax collection efforts remain unexplored.

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A Appendix: Tables

Table 5: Summary Statistics

Statistic	N	Mean	St. dev.	Min	Max
Property tax revenue per capita	7,712	28.20	83.63	0.00	1,803.01
Municipal Compensation Fund (FONCOMUN)	7,744	4,599,808.00	9,442,595.00	0.00	283,498,244.00
Canon revenues	7,744	7,137,446.00	22,149,307.00	0.38	867,499,681.00
Modified Institutional Budget (PIM)	7,744	21,104,000.00	66,777,209.00	358,071.70	2,518,053,514.00
Access to drinking water	6,013	9,346.75	18,750.55	0.00	230,944.90
Access to public sanitation	6,013	8,286.08	18,933.02	0.00	230,302.00
Property ownership	6,013	5,514.75	10,835.43	0.00	136,978.60
Inadequate housing (%)	6,013	7.07	13.63	0.00	100.00
Overcrowding (%)	6,013	5.68	7.52	0.00	85.71
Lack of sanitation services (%)	6,013	14.14	18.47	0.00	100.00
Children not attending school (%)	6,013	0.75	2.60	0.00	50.00
High economic dependency (%)	6,013	0.69	2.14	0.00	25.00

Table 6: Differences-in-Differences model (base year: 2007)

Variables	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Treatment	3.606	4.199	4.201	1.203	3.790	1.713	3.383	3.991	4.970	4.872
	(2.937)	(2.956)	(3.066)	(3.399)	(3.160)	(3.411)	(3.597)	(3.505)	(3.971)	(4.595)
Year Dummy	-0.379	0.320	0.749	0.390	-0.405	1.461	2.064	3.708	4.251	1.973
	(3.111)	(3.680)	(3.565)	(3.136)	(3.462)	(3.719)	(3.781)	(3.693)	(4.153)	(3.933)
Interaction	2.388	3.290	3.518	5.889	1.961	3.790	6.239	14.35*	9.053	11.48
	(3.558)	(3.970)	(4.055)	(5.125)	(6.427)	(6.485)	(8.036)	(8.162)	(10.18)	(11.78)
Constant	16.02***	13.62***	14.70***	18.75***	21.52***	22.25***	22.67***	24.03***	23.27***	26.51***
	(3.768)	(3.433)	(3.583)	(4.412)	(5.176)	(4.857)	(5.324)	(5.618)	(4.690)	(5.743)
Observations	888	888	884	928	926	978	994	1,012	1,029	1,030
R-squared	0.058	0.048	0.061	0.074	0.077	0.098	0.084	0.092	0.088	0.092

Robust standard errors in parentheses
*** $p<0.01$, ** pp<0.05, * $p<0.1$
Note: Every DiD regression is controlled for a dummy variable for Lima Region, each district's Municipal Compensation Fund (FONCOMUN), Canon revenues, Modified Institutional Budget (PIM), Access to drinking water, Access to public sanitation, Property ownership, Inadequate housing (%), Overcrowding (%), Lack of sanitation services (%), Children not attending school (%), and High economic dependency (%).

Table 7: Summary of Treated Districts

Year	Type A districts			Type B districts		
	Number of districts	Number of treated	Treated (as %)	Number of districts	Number of treated	Treated (as %)
2010	40	31	77.5	205	129	62.9
2011	40	33	82.5	205	171	83.4
2012	40	37	92.5	205	168	82.0
2013	40	31	77.5	205	134	65.4
2014	40	11	27.5	205	66	32.2
2015	40	29	72.5	205	140	68.3
2016	40	0	0.0	205	85	41.5
2017	40	12	30.0	205	37	18.1

Table 8: Heterogeneous Treatment Effects by District Characteristics

	Type B				Type B&C			
Subgroup	ATET	Placebo	Pretrend	Carry over	ATET	Placebo	Pretrend	Carry over
Panel A: By canon transfers (2009 median cutoff)								
Low canon	0.904	0.908	0.982	0.999	1.500**	0.357	0.029	0.554
High canon	3.494***	0.737	0.343	0.576	4.697***	0.127	0.148	0.562
Panel B: By debt levels (2009 median cutoff)								
Low debt	1.904***	0.510	0.605	0.734	1.761**	0.847	0.751	0.575
High debt	4.743***	0.923	0.320	0.838	4.156***	0.185	0.192	0.245
Panel C: By administrative capacity (2009 median)								
Low property tax	0.688*	0.912	0.261	0.132	0.819***	[b]	[b]	[b]
High property tax	4.815***	0.987	0.126	0.978	2.908***	0.264	0.534	0.651
Panel D: By poverty rate (30.8% cutoff)								
Low poverty	3.916***	0.670	0.105	0.964	2.978***	0.459	0.278	0.344
High poverty	2.907***	0.887	0.139	0.688	1.646***	0.801	0.122	0.735
Panel E: By urbanization (2,560 dwellings cutoff)								
Rural	4.224	[a]	0.038	0.894	7.120***	0.299	0.111	0.922
Urban	2.718***	0.959	0.556	0.848	2.904***	0.590	0.674	0.782

Note: This table reports heterogeneous treatment effects for Type B districts and Type B&C district groupings. ATET is in soles per capita. The sample is partitioned based on 2009 baseline characteristics. Low/High canon and Debt are defined using the median per capita value for Type B districts. Low/High property tax uses the median 2009 per capita collection for Type B districts. Poverty cutoff is the 2009 national average (30.8%). Urban/Rural classification is based on the number of urban dwellings in 2007 (2,560 threshold). All estimates use the IFE estimator with cross-validation. Placebo, Pretrend, and Carry over columns report p-values from diagnostic tests (should be >0.05 for valid identification). Standard errors computed using 500 bootstrap replications. *** p<0.01, ** p<0.05, * p<0.1.

[a] Insufficient observations to compute the placebo test for rural Type B districts.

[b] Insufficient treated observations to compute the placebo, pretrend, or carry-over test for low-property-tax Type B and C districts.