Journal of Economic Behavior and Organization 215 (2023) 244–267

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

The effects of increased monitoring on high wealth individuals: Evidence from a quasi-natural experiment in Indonesia☆

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ARTICLE INFO

JEL codes:
H26
H24
C99

Keywords:
Tax compliance
Quasi-natural experiment
Institutional reform
High wealth individuals
Spillover effects

ABSTRACT

In 2009, Indonesia introduced a specialised tax office focused on High Wealth Individuals (HWIs), increasing the audit probability and monitoring of around 1200 wealthy taxpayers in Jakarta. Leveraging a quasi-natural experiment and analysing 141,097 de-identified individual tax return records between 2006 and 2012, we developed a set of counterfactuals to evaluate the post- and pre-treatment tax declarations of the monitored taxpayers relative to their synthetic control groups. Our results indicate that although post-treatment declaration of taxable and earned income, as well as income tax, increased, the effect was short-lived. The increase in reported earned income was larger relative to taxable income, suggesting that monitored individuals exerted efforts to reduce tax payments. Furthermore, we identified a comparable positive spillover effect on income declaration for non-treated taxpayers in Jakarta who met the HWI criteria. We also demonstrated that the spillover effect drastically decreased as financial characteristics (such as income and wealth) diverged from those of the targeted population. Overall, the additional tax revenue generated from this program is small in comparison to the total tax revenue collected.

1. Introduction

Tax compliance amongst high-wealth individuals (HWIs) has been a long-standing and complex challenge for tax authorities around the world, impacting both revenue collection and public trust in the tax system. Throughout history, wealthy individuals have posed considerable revenue risks for tax authorities and severe challenges to the integrity of tax system. For example, wealthy citizens in ancient Greece would voluntarily contribute a 'liturgy' to the city-state to pay for its expenses, even competing for the honour of

☆ This study is a substantially revised version of a chapter out of Mohammad Wangsit Supriyadi's doctoral thesis entitled "Beyond Deterrence: An Empirical and Experimental Analysis of Tax Compliance Behaviour in Indonesia". Tax administrative data used in the present study were anonymized and de-identified, and were used and stored strictly in accordance with Queensland University of Technology Human Research Ethics and research clearance from Directorate General of Taxes (DGT) Indonesia.

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https://doi.org/10.1016/j.jebo.2023.09.017
Received 3 February 2022; Received in revised form 5 September 2023; Accepted 14 September 2023
Available online 25 September 2023

funding public buildings. In contrast, the Romans had a progressive war tax that targeted wealth,[1] and the failure to pay could led to harsh punishments, such as being sold into slavery (Adams, 1993).

In contemporary times, individuals in the top end of the income and wealth distribution account for a large share of the overall individual income tax revenue paid across many countries (Förster et al., 2014; Keen et al., 2015; OECD, 2009). For instance, in the UK and US, the top 1 % of taxpayers are responsible for approximately one-quarter and one-third of the total revenues, respectively (Keen et al., 2015).[2] Given the critical role HWIs play in revenue collection, tax authorities have increasingly focused on strategies to discourage non-filing and secure sufficient cooperation from this group. This issue is particularly acute in developing countries such as Pakistan (Gangl et al., 2017), Colombia (Londoño-Vélez and Ávila-Mahecha, 2021), and Rwanda (Kangave et al., 2020), where non-compliance amongst HWIs is a pervasive problem.

Maintaining a high level of tax compliance or cooperation amongst wealthy taxpayers is essential to financing public goods and sustaining a well-functioning society (OECD, 2008). However, HWIs often have the means and the motivation to strategically circumvent tax obligations. Their complex business affairs, high mobility, and access to expert advice—from accountants, tax specialists, and lawyers alike—provide them with ample opportunities for tax avoidance or evasion (Keen et al., 2015; OECD, 2009). Using leaked data from offshore bank accounts and administrative income and wealth records, Alstadsæter et al. (2019) showed that the top 0.01 % of wealth holders evade around a quarter of their tax liabilities. This challenge has been further exacerbated in recent years by the rise of large multinational corporations (Frijters et al., 2020), which has fuelled a public perception that the wealthy are not contributing their 'fair share' to the tax coffers (Keen et al., 2015; OECD, 2009). Such beliefs can erode citizens' trust in government and may even spur resistance to taxation.[3]

Despite the urgency and importance of these issues, research focused on promoting tax compliance amongst HWIs remains underdeveloped (see Gangl and Torgler 2020). Most existing studies on HWI taxation focus largely on behavioural responses to tax rate changes (Alm and Wallace, 1997; Goolsbee, 2000; Gruber and Saez, 2002).[4] The most common tactics used by tax authorities to target the wealthy are to increase deterrence (audits and fines, e.g., Allingham and Sandmo 1972) and to employ whistle-blower processes (e.g., Casaburi and Troiano 2015), a traditional economics-of-crime approach whose core notion is that compliance depends upon audit and fine rates (Torgler, 2007; Alm, 1999, 2012).

Despite substantial evidence that audits are more influential than fines in promoting tax compliance (Alm, 1999; 2017), the reported income-audit rate elasticity remains low and inconsistent across the literature. It has been shown that strategic audit selection yields better compliance than random selection (Alm, 2017). At the same time, it is evident that taxpayers often misperceive actual audit rates, overweighting their probability (Alm et al., 1992; Bergolo et al., 2023). In response to an audit threat letter, high-income earners have been observed to significantly reduce their reported tax liability (relative to the field experimental control group), a finding that stands in contrast to the behaviours of low and middle-income taxpayers (Slemrod et al., 2001). Similarly, Castro and Scartascini (2015) report in their field experiment that wealthy individuals are less responsive to deterrence measures than their less affluent counterparts. This contrasts with earlier findings by Tauche et al. (1993) and Ali et al. (2001), which found that audit activities had a notably stronger deterrent effect on high-income earners compared to lower-income taxpayers. Moreover, Coleman (1996) reports that high-income taxpayers exhibit mixed reactions to audit threat in his experimental study. Finally, a recent study by Alstadsæter et al. (2022) examined wealthy Norwegian taxpayer records and found that mandatory disclosure of overseas assets did not lead to increased tax avoidance. This study suggests that effective crackdowns on tax evasion amongst wealthy individuals is an effective way to raise tax revenue.

During the first decades of tax compliance research, tax administrations did not collaborate closely with scholars nor allow them access to the data required to provide scientifically sound and credible policy advice due to its highly confidential nature. One early exception was the Taxpayer Compliance Measurement Program (TCMP) established by the Internal Revenue Service (IRS) in the 1980s (see, e.g., Clotfelter 1983, Witte and Woodbury 1985, Dubin and Wilde 1988). Since then, collaborations between tax authorities and researchers have flourished (Pomeranz and Vila-Belda 2019; Slemrod 2019). For instance, randomized and natural field experiments conducted in partnership with tax administrations have become an important data source examining tax compliance (Hallsworth, 2014), with the number of such studies increased drastically in the last couple of years (Torgler, 2016).

At the same time, the increasing accessibility of large-scale, disaggregated administrative data at the individual or firm level—compared to the traditional aggregated data, such as zip-code level information—has transformed research capabilities (Almunia et al., 2019). Recent studies have examined the effect of size-dependant taxation or large taxpayer units (LTUs)[5] on corporate reporting

[1] For example, luxury items (e.g., expensive jewels, carriages, apparels) were assessed at 10 times the actual value (Adams, 1993, p. 76).

[2] In research and practice, high-wealth individuals or high-net-worth individuals are frequently defined as those with financial assets (excluding primary residence) valued greater than USD 1 million.

[3] According to previous empirical studies, the perceived level of non-compliance among others affects the willingness to pay taxes (Frey and Torgler, 2007), so a prevailing public opinion that the wealthy pay too little might also impact the perceived legitimacy and accountability of the government, which would in turn contribute to a general unwillingness to comply (Bird et al., 2008).

[4] For instance, whereas Kleven et al. (2013) find that the migration decisions of professional football players respond strongly to tax changes, Young et al. (2016) observe a relatively small migratory response of high-income earners to an income tax increase in some American states. Conversely, Day and Winer (2006), using Canadian data, find no evidence that a tax change influences the decision to relocate.

[5] Large taxpayers are commonly referred to as corporate taxpayers who contribute substantially to the total tax revenues. For example, in an IMF report, large taxpayer units (LTUs) are specialized units responsible for monitoring the compliance of taxpayers who represent over 50 percent or more of the overall tax collection (Benon et al., 2002).

behaviour (see Bachas et al. 2019, Carrillo et al. 2017, Asatryan and Peichl 2017). For example, using data on Spanish firms' financial statements, Almunia and Lopez-Rodriguez (2018) showed that firms tend to reduce income and bunch below the eligibility threshold in order to avoid stricter tax enforcement, and the strategic response is stronger amongst industries with higher transaction traceability. In another study, Tennant and Tracey (2019) employed a before-during regression discontinuity approach using confidential administrative firm-level data, where they reported that the establishment of Jamaica's large taxpayer unit has effectively increased pre-tax profit margins declared by large corporations. However, the study also concluded that size-dependant tax enforcement is less efficacious at increasing the effective tax rates of large firms in developing countries. A recent investigation by Basri et al. (2021), based on Indonesian firm data, revealed that increasing the administrative staff-to-taxpayer ratio is a highly cost-effective and sustainable measure in raising tax revenue. The study showed that the tax revenue from medium-sized firms affected by the large-scale tax administration reform more than doubled—a result equivalent to an eight-percentage-points increase in the corporate tax rate across all firms.

Our study addresses the substantial research gap concerning the compliance behaviour of high-wealth individuals (HWIs), by leveraging the quasi-natural experiment occasioned by Indonesia's 2009 introduction of a special HWI monitoring unit within its tax office. This enabled us to analyse anonymized, micro-level data from Indonesian HWIs' income tax returns between 2006 and 2012 to compare income declarations before and after this unit's formation. We applied a difference-in-difference methodology to assess the compliance of HWIs intensified post-2009 monitoring, in contrast to a synthetic control group that was not. Combining the availability of this unique HWI dataset and the application of recently developed methodology overcomes the fundamental difficulty faced by most tax compliance research relating to a lack of reliable information on taxpayer compliance (Alm, 2012)—especially that of the very wealthy. To of our knowledge, this is the first study to analyse the impact of specialised tax units focusing on individual taxation (as opposed to large taxpayer units, which focus on high-earning firms) within developing nations.

Our results reveal a complex relationship between increased monitoring on individual tax compliance. The analyses of the *reported income* suggest a significant positive effect of the monitoring intervention, with audit threats leading to a 30 % and 34 % rise in reported *taxable* and *earned income* amongst the monitored HWIs compared to the non-monitored counterparts in the post-treatment period. This translated to an average 32 % increase in *income tax paid*. The difference in the treatment effects on earned and taxable income may suggest that wealthy taxpayers find ways of avoiding taxes when monitoring increases. Moreover, the effects appear short-lived and faded over the four-year duration of the program, losing statistical significance by its end in 2012. Additionally, we found no impact on the reporting of post-pre monitoring *net wealth*, possibly because Indonesia does not levy taxes on net wealth/worth and that taxpayers under HWI Tax Office scrutiny cannot opt-out by later manipulating their wealth, thereby reducing the incentive to under report their wealth status. Furthermore, we noticed a considerable positive spillover effect on income reporting amongst non-treated wealthy taxpayers in Jakarta, resulting in higher tax revenues overall. However, this effect diminished or was absent altogether amongst Jakarta taxpayers whose financial profiles—specifically in terms of income and wealth—differed substantially from those in the monitored HWI group. The cost-effectiveness of the HWI program remains an open question when balancing the incremental tax revenue against administrative costs.

The remainder of this paper is structured as follows: Section 2 outlines the quasi-natural experimental context, Section 3 describes the sample, Section 4 introduces the synthetic control group, Section 5 discusses the analyses and their results, and Section 6 presents our concluding remarks.

2. Quasi-natural experimental context

Indonesia's Directorate General of Taxes (DGT) is a semi-autonomous body under the Ministry of Finance, incorporating over 570 operational units ranging from regional taxpayer offices down to localised tax service, dissemination, and consultation offices. Indonesia's basic tax unit, the district tax office, is responsible for overseeing individual, firms and corporate taxpayers grouped according to their residence or company address. As of 2014, the DGT's workforce numbered 34,500 employees, oversees between 20,000 to over 200,000 individuals in any one district tax office.[6] These figures typically contrast with much smaller counts of corporate taxpayers, therefore, the supervision of individual taxpayers is considered less strict than that of corporate taxpayers, implying a lower likelihood of being audited. Before 2009, this lower audit probability also encompassed High-Wealth Individuals (HWIs), who were not segregated from the broader taxpayer base for specialized monitoring.

Indonesia's individual income tax system applies a progressive rate, with income brackets ranging from 5 to 30 % (see Appendix Table A1). The system also features a non-taxable income allowance, known as the main personal relief (MPR), which varies according to marital and dependant status. Specifically, an individual taxpayer is granted an annual MPR of IDR 54,000,000 (approx. USD 6750),[7] along with an additional MPR of IDR 4500000 (approx. USD 563) for a spouse and an equivalent amount for each dependant, up to a maximum of three.

2.1. High-wealth-individuals tax office

Established in 2009 with technical guidance from the Australian Taxation Office (ATO), Indonesia's High-Wealth Individual (HWI)

[6] Indonesia's 'regular' tax office is based on district and thus called a district tax office, also known as a small taxpayers' office (STO) to differentiate it from two other types: a large and a medium taxpayers' office (LTO and MTO, respectively).

[7] Based on the approximate 2010 exchange rate of USD 1 = IDR 8000.

Tax Office specifically focuses on monitoring the wealthiest residents in Jakarta, the country's capital. Although it has the same number of officers and auditors as other tax offices, it only oversees around 1200 registered taxpayers.[8] Given its concentrated focus, both the DGT Head Office and the public had high expectations for the success of the HWI unit, meaning a substantially increased likelihood that any HWI may be unexpectedly audited. The significance of the office was underscored by the attendance of Indonesia's President and other ministers and prominent business individuals (see Adamrah and Suharmoko 2009) at the festive celebration that heralded its opening, which garnered nationwide attention. While the identities of the wealthy assigned to this office were never publicly disclosed, it is common knowledge that some high-profile individuals were amongst the taxpayers it monitored (see Widihartanto 2014, Widihartanto and Braithwaite 2016).

In terms of its operational guidelines, the DGT defines HWIs as either shareholders of the firm or principal business owners with either financial assets and property worth at least IDR 10 billion (USD 1 million) or reported taxable income of at least IDR 1 billion (USD 100,000) (Widihartanto and Braithwaite, 2016).[9] This definition encapsulates both wealthy (high-net-worth) and high-income individuals.[10] Nevertheless, the crucial criterion for taxpayer assignment to the HWI Tax Office[11] is being the owner (or family member of an owner) of a large, Indonesia-based corporation. Some individuals were assigned to the HWI Tax Office despite not having reported high income and/or wealth but is directly linked to a large firm or corporate group (e.g., holds a significant portion of the company's shares/stocks). Conversely, because one of the main assignment criteria is being a firm shareholder or principal business owner, some individual taxpayers with high income and/or wealth are not assigned to the HWI Tax Office because they are not associated with large corporations. The latter may include highly paid employees or self-employed individuals such as lawyers, doctors or medical practitioners, accountants, and other professionals.

The HWI Tax Office was launched unexpectedly, with plans for its formation unknown to the public until its formal establishment. Hence, the subsequent enhanced monitoring and supervision implemented after the 2009 financial year offered a unique context for investigating the compliance behaviour of wealthy taxpayers—a group now subjected to rigorous scrutiny with no exit strategy or opportunity to opt-out by migrating to another tax office. Selected individuals were formally notified by the DGT, making it mandatory for them to submit their future tax returns to this new specialized unit. Notably, no such notifications were issued to other Jakarta taxpayers, including those who potentially met the HWI income or net wealth criteria but were not selected due to the lack of corporate ties. Thus, those individuals would continue filing tax returns through their current tax offices, experiencing no alteration in their monitoring levels. At the same time, this implies there existed a group of similarly wealthy individuals residing outside Jakarta who were unlikely to feel any threat of assignment to the HWI Tax Office given the DGT's lack of plans to expand it.

2.2. *Potential spillover effects*

While Jakarta taxpayers not selected for the HWI Tax Office remained ostensibly unaffected by its formation, it is possible that those with high wealth or taxable income (i.e., meeting the DGT definition) might feel heightened scrutiny given the highly publicized establishment of the specialized tax office. In essence, those who closely resembled the targeted HWI taxpayers in financial characteristics (e.g., income and wealth status) might be prompted to adjust their subsequent tax-filing behaviour due to a potential spillover effect (higher perceived audit threat) from the stricter enforcement aimed at the city's wealthiest residents.[12] Therefore, our study goes beyond merely assessing the direct effect of the HWI Tax Office on those specifically reassigned to the HWI Tax Office, we also explore its broader deterrence effect on similarly affluent taxpayers who were not directly targeted, which we term 'enforcement externalities'.[13]

To this end, we supplement our primary analysis of the average treatment effect on the treated (ATET) by considering the potential spillover effect using an *intention-to-treat* (ITT) approach.[14] Specifically, we contrast the post-intervention tax reporting behaviours of non-selected Jakarta taxpayers—whose pre-intervention total wealth and taxable income exceeded the DGT HWI threshold—with comparable non-Jakarta tax residents with similar characteristics.[15]

[8] According to one internal document, the average number of individual taxpayers registered with a district tax office in Jakarta is 31,000.

[9] The OECD (2009) uses the HWI term to refer to individuals at the top of the wealth and income scale.

[10] We use the acronyms HWI and HNWI (high net-worth individual) interchangeably throughout the study.

[11] These facts are taken from several DGT documents, including internal reports and presentation materials.

[12] Alternatively, it could be argued that the high-profile launch of the HWI treatment might generate an unintended negative consequence, such as a decrease in the tax compliance among non-treated HWIs. This could occur because those who are not informed may feel as though they've "fallen through the cracks.".

[13] For example, Lediga et al. (2020) finds a positive but short-lived spillover effect of tax audits that increases tax liability reporting of neighbouring non-audited companies, while Lopez-Luzuriaga and Scartascini (2019) find increased tax enforcement on one tax base leads to a positive effect on compliance with another tax. In addition, Rincke and Traxler (2011) find strong increases in compliance with TV license fees due to field inspections of households in the nearby vicinity.

[14] Specifically, ITT includes both ATET and the estimated spillover effect. We use the term *intention-to-treat* loosely here as the treatment assignment is not randomized and all assigned taxpayers receive the treatment due to the lack of exit strategy.

[15] Similar strategies were employed to estimate the direct treatment effect and spillover effect of police-monitoring on crime (e.g., Munyo and Rossi, 2020).

3. Sample

3.1. Data

Our dataset comprises de-identified individual income tax return records for the years 2006–2012, obtained from the DGT, and includes 141,097 yearly observations from 23,209 private-sector individual taxpayers.[16] These records includes self-reported measures such as earned and taxable income, income tax and wealth (assets and liabilities), together with demographic characteristics like age, marital status, and the number of dependents. The treatment group consists of all 1209 wealthy individual Jakarta-residing taxpayers monitored by the HWI Tax Office. For comparative analysis, we include the top 2 % of high-income taxpayers from the 2006 fiscal year who were not allocated to the HWI Tax Office in our dataset, totalling 22,000 individuals. Of these, 49.41 % (10,870) reside in Jakarta, and the remaining 50.59 % (11,130) are registered as a taxpayer in other regions. We excluded 14 targeted (monitored) individuals from the treatment group from the analysis because their records contain only either pre- or post-intervention tax returns.[17]

As is common in compliance studies (e.g., Slemrod et al. 2001), our access is restricted to unaudited tax returns, precluding a direct assessment of tax compliance or, more precisely, the tax return 'accuracy'. If, however, we observe differences between the taxable income and income tax figures, we could conclude that taxpayers have sought creative and strategic ways of reducing their tax burden. Hence, to analyse the impact of increased supervision on tax reporting behaviour, we focus on four key elements of the individual income tax: *earned income, taxable income, income tax*, and *net wealth*. All monetary variables are converted to real terms (in IDR with 2010 as the base year) using the consumer price index obtained from the World Bank Open Databases (Azevedo, 2011).

Table 1 provides the summary statistics of the pre-treatment averages (2006 to 2008 inclusive) of income and wealth variables, along with taxpayers' demographic characteristics, for treated and non-treated taxpayers residing in and outside Jakarta.

3.2. Pre-intervention differences

As illustrated in Fig. 1, apparent differences emerge in the four monetary variables during the pre-treatment period amongst individuals being monitored by the HWI Tax Office and non-targeted high-income earners, both within and outside Jakarta. For taxable income and net wealth, the common threshold definition of a HWI taxpayer (red dashed line) is IDR 10 billion (~23.03 in log terms) for net wealth and IDR 1 billion (~20.72 in log terms) for taxable income.[18] This threshold, however, seems to serve only as a guideline in taxpayer selection into the HWI office. That is, a subset of treated (monitored) taxpayers falls below the wealth and income thresholds, while a group of non-treated taxpayers not assigned to the HWI office lie above the cut-offs.[19] For instance, based on the maximum amount[20] of pre-treatment taxable income or wealth, most treated HWIs (1144 out of 1209) meet the DGT definition of HWI in terms of either wealth (1092) or taxable income (751), and 699 have exceeded both thresholds.

Conversely, amongst the 10,870 non-treated Jakarta high-income earners, 831 (7.6 %) have pre-treatment taxable income and net wealth above the DGT's definition of HWI.[21] As discussed in Section 2, while this group of Jakarta taxpayers was not reassigned to the new HWI office, they may also feel threatened by the potential increase in monitoring due to the high publicity of the HWI office. A suitable control group to assess this spillover effect would naturally come from comparable non-Jakarta high-income earners, of which 606 out of 11,130 (5.4 %) meet both HWI eligibility thresholds.

A unique aspect of our study is the use of granular, individual micro-level data derived from actual tax returns that also contain information on several demographics, including age, marital status, and the number of dependents. As shown in Table 1 and Appendix Fig. A1, the average age of the monitored taxpayers in 2006 was 50.32 years (SD = 11.44), which is roughly 3.39 years older than their non-monitored counterparts in Jakarta,[22] whose average age was 46.18 (SD = 9.12). Outside Jakarta, the average age of non-monitored taxpayers was 46.15 (SD = 8.25). During the pre-treatment period, 8.69 % and 8.6 % of the treated and non-treated groups were 'not married'. while treated taxpayers had on average 1.4 (SD = 1.17) dependents compared with 1.7 (SD = 1.05) for non-treated taxpayers.

[16] We assume that the potential spillover effect only occurs within the Jakarta region due to proximity whereas non-Jakarta taxpayers are highly confident that the levels of monitoring would not change as only Jakarta residents are included in the HWI office. This includes taxpayers who registered themselves in the tax system as an "employee" of a company (e.g., CEOs, managers, regular employees or self-employed person), and excludes government employees, contractors (paid wholly or principally for labour), or business taxpayers.

[17] Six (8) out of 14 HWIs have only a pre (post) 2009 tax return record. Non-treated taxpayers with no pre- or post-treatment period were also dropped from the analysis (3 individuals). Additionally, we exclude five yearly observations due to data record error (inconsistent and abnormally large values of income or wealth).

[18] Pre-treatment wealth and taxable income are taken as averages over the available yearly observation across the three-year period from 2006 to 2008.

[19] Some members of this group are Jakarta residents (being a Jakarta resident is a necessary criterion for selection into the HWI office).

[20] Using the maximum value of taxable income and net wealth in the pre-treatment period allows us to more accurately capture taxpayers who are most likely to be treated, rather than using pre-treatment average as the cut-off.

[21] Additionally, 4089 and 1593 non-treated Jakarta taxpayers have pre-intervention maximum taxable income and net wealth above the DGT's definition of HWI, respectively.

[22] The age differences are significant at 1 percent using a *t*-test on pooled non-HWIs, although the difference between the mean age of non-HWIs inside and outside the Jakarta region is not statistically significant. The distributional difference between HWIs and non-HWIs is confirmed by a two-sample Kolmogorov–Smirnov test at a 1 percent level of significance.

Table 1
Summary statistics: Pre-treatment taxpayer wealth, income and demographics.

	Variables	M	SD	Min	Max	N
Targeted individuals	*ln*(Earned income)	21.04	1.35	16.86	26.35	1196
	ln(Taxable income)	21.00	1.39	15.50	26.35	1195
	ln(Income tax)	19.74	1.65	12.51	24.51	1195
	ln(Net wealth)	24.29	1.24	16.08	28.09	1192
	Age	50.32	11.44	23.28	103.71	1016
	Single	0.13	0.31	0	1	1203
	# of dependents	1.40	1.17	0	3	1202
Non-targeted high-income earners (inside JKT)	*ln*(Earned income)	20.50	0.72	17.92	25.58	10,869
	ln(Taxable income)	20.46	0.73	16.55	25.58	10,869
	ln(Income tax)	19.20	0.84	12.56	24.53	10,869
	ln(Net wealth)	21.11	1.68	8.30	27.05	10,187
	Age	46.18	9.12	21.02	105.61	10,199
	Single	0.18	0.34	0	1	10,870
	# of dependents	1.54	1.06	0	6	10,870
Non-targeted high-income earners (outside JKT)	*ln*(Earned income)	20.30	0.60	16.98	26.01	11,126
	ln(Taxable income)	20.26	0.62	15.68	26.01	11,127
	ln(Income tax)	18.97	0.73	12.68	24.96	11,128
	ln(Net wealth)	20.50	2.13	−0.85	28.79	9495
	Age	46.15	8.25	19.56	104.13	10,579
	Single	0.10	0.26	0	1	11,128
	# of dependents	1.86	1.03	0	12.67	11,127

Notes: JKT = Jakarta region. Values are averaged across the three pre-treatment years for each individual. Earned income, income tax, taxable income, and net wealth are natural log values in Indonesian rupiah (IDR in real terms). Age is based on the fiscal year 2006. An individual is designated single = 1 if the marital status is not listed as HB (*hidup berpisah*; couple living separately), K (*kawin*; married), KI (*kawin dan isteri bekerja;* married and wife has her own income combined or aggregated into this tax return) or pH (*pisah harta*; married but with a cohabitation [asset separation] agreement).

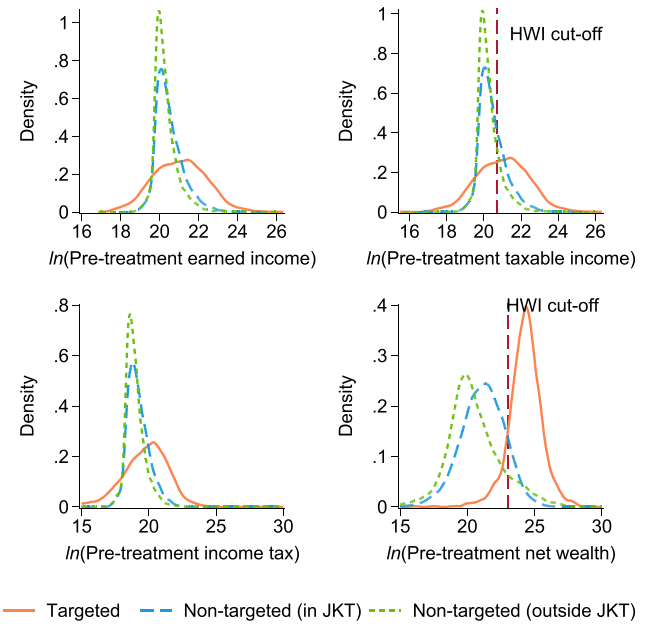

Fig. 1. Distributions of key pre-treatment factors.
Notes: Earned income, income tax, taxable income and net wealth are natural log values in Indonesian rupiah (IDR in real terms). Kernel density estimate is based on the Epanechnikov kernel function.

4. Synthetic control method

To estimate the treatment effect of the monitoring intervention (being assigned to the HWI Tax Office) on tax reporting behaviour, we construct a counterfactual (Abadie et al., 2010)[23] for each targeted taxpayer from the pool of non-targeted taxpayers that are most similar in pre-treatment tax reporting behaviour and demographic background. The counterfactual is composed using the Synthetic Control Method (SCM), whereby outcome variables and other covariates in the pre-treatment periods serve as predictors or matching variables. Each synthetic control unit is thus constructed from a weighted average of the non-treated units, with non-negative weights adding up to 1 to prevent extrapolation[24] of the outcome measures for non-treated units. Let $Y_i = (Y_{i,1}, \ldots, Y_{i,t^*}, \ldots, Y_{i,T})'$ be the observed outcome variable of interest corresponding to the tax reporting behavioural measure for taxpayer i, where the first 1 to J_1 taxpayers are targeted and J_1+1 to J_1+J_2 are the control taxpayers. The intervention (monitoring by the HWI tax office) starts in $t^* = 2009$. We estimate the average treatment effect on the treated $\overline{\delta}_t$ (counterfactual difference, ATET)[25] by taking the average of the estimated treatment effect for each individual treated taxpayer i,

$$\delta_{i,t} = Y_{i,t} - \sum_{j=J_1+1}^{J_1+J_2} w_{i,j}^* Y_{j,t}$$

with $\boldsymbol{w}_i^* = (w_{i,J_1+1}^*, \ldots, w_{i,J_1+J_2}^*)'$ as the optimal set of weights for each non-treated unit in the donor pool derived from the SCM. $\boldsymbol{w}_i^*$ is chosen such that it minimizes the pre-intervention discrepancy in the predictors of the outcome variable values between the synthetic control and the treated unit. A synthetic control group is constructed for each outcome variable of interest: namely, taxable income, earned income, income tax, and net wealth. In the main analyses, we use three sets of variable groups as the predictors—pre-treatment average of (i) taxable income, earned income and income tax, (ii) net wealth, and (iii) age, the number of dependents, and marital status.[26] Past values of the outcome variable are also included as predictors[27] to produce the synthetic control unit that best resembles the pre-treatment trajectory of the treated unit. This, in turn, produces an estimate of $\widehat{Y}_{i,t}(0)$ for $t \geq 2009$, which is the post-treatment tax filing behavioural measure of the targeted taxpayer, absent the intervention. We assume no treatment anticipation effect prior to 2009, so that $Y_{i,t}(0) = Y_{i,t}(1)$ is the observed outcome given treatment exposure, for the period before $t^* = 2009$ due to the unforeseen introduction of the HWI Tax Office.

Heterogeneity amongst the treated taxpayers necessitates imposing certain criteria before considering any non-treated observations in the donor pool. Although such criteria are subjectively chosen, they may safeguard against choosing candidates with vastly different characteristics from the intended match, thus reducing the risk of over-fitting and potential interpolation biases (see Abadie 2021 and Abadie and Vives-i-Bastida 2022). First, for demographic variables, we consider only non-treated taxpayers who are no more than ten years younger or older than the corresponding treated taxpayer, have no more (fewer) than one dependant and have the same marital status as the treated taxpayers in the pre-treatment period. In terms of wealth and income, we limit the potential control pool to non-exposed taxpayers with no more (no less) than 50 % of their intended correspondent's wealth and income. Nevertheless, as a robustness check, we will loosen these restrictions at a later stage to obtain better matching on the pre-treatment outcome variables.

To assert the statistical significance of the average treatment effect, we first test whether the post-intervention synthetic control estimates are statistically different from both zero and the pre-treatment differences ($\overline{\delta}_{t<2009}$). For added robustness, we include a distribution of placebo effects; here, a placebo treatment is randomly assigned to units within the donor pool, where any effect is expected to be zero.[28] We then compare the average treatment effect estimated through the SCM to this placebo effects distribution. Including these placebo effects serves two key purposes. First, if the placebo effects systemically deviate from zero, it suggests that our model may be capturing some forms of spurious relationships instead of genuine treatment effects. Second, this comparison provides additional inference for the estimated treatment effect. The two-sided p-value of the treatment effect can be calculated based on the proportion of the placebo distribution that is at least as extreme as the estimated average treatment effect $(\overline{\delta})$ in absolute value. We repeat these exercises when examining the statistical significance of the average treatment effect in each post-treatment year $\overline{\delta}_t$.

[23] See, for example, Abadie and Gardeazabal (2003), Abadie et al. (2015), Campos and Kinoshita (2010), Cavallo et al. (2013), Chan et al. (2014), Imbens and Wooldridge (2009), Moser (2005), and Sanso-Navarro (2011).

[24] This ensures the synthetic control unit is a convex combination of the units in the donor pool.

[25] Another approach to obtain the average treatment effect on the treated (ATET) is to first aggregate all the treated units and obtain the synthetic control estimates using the aggregated unit (e.g., Acemoglu et al. 2016, Gobillon and Magnac 2016, and Kreif et al. 2016). However, this might produce larger interpolation biases particularly with many treated units, compared to aggregating the individual synthetic control estimates of the treatment effect for each treated unit (Abadie and L'hour, 2021).

[26] In the SCM matching process, age is not used as a predictor for any HWI observation that contains no age information (193 of the total 1209). We similarly exclude potential controls whose age is unknown (5.6% of the sample).

[27] While including all pre-treatment outcomes as separate predictors may increase the pre-treatment fit of the outcome, this reduces predictor weights of other covariates, potentially leading to biased estimator (Kaul et al., 2022). Thus, we also conduct an analysis where we use the pre-treatment outcome average as additional predictor alongside other covariates (see Section 5.2).

[28] Specifically, the placebo effects are obtained by iteratively reassigning the treatment variable to 50 % of the units (randomly drawn 5381) from the non-Jakarta control group without replacement. For configurations where both Jakarta and non-Jakarta taxpayers were used as control group, we include a further 5577 non-treated Jakarta taxpayers in the placebo distribution (randomly drawn 50 % from those residing in Jakarta).

4.1. Estimating spillover effect

The potential spillover effect from the intervention on non-treated Jakarta taxpayers raises two concerns. First, it may invalidate the use of non-treated Jakarta taxpayers as potential controls for estimating the intervention effect on the targeted HWIs. This is because these taxpayers may either increase compliance due to their perceived higher chance of being monitored closely or they may under-declare future income or wealth to avoid detection.[29] To address this concern, we first implement the SCM using only non-Jakarta taxpayers to construct the donor pool, thus eliminating any potential contamination in the estimation of $\widehat{Y}_{i,t>2009}(0)$. Nevertheless, due to the dissimilarities of economic (industrialisation) and demographic structure, as well as the geographic proximity between Jakarta and the rest of Indonesia, we also consider including all non-treated Jakarta taxpayers in the potential donor pool.[30]

The second concern is that estimated ATET does not capture such spillover effects due to the nature of the endogenous treatment selection. As mentioned above, some Jakarta taxpayers were not selected but would have been if selection were based solely upon the DGT's HWI definition regarding taxable income and wealth criteria (Fig. 1). We therefore complement the primary analysis by conducting an *intention-to-treat* approach to estimate the overall treatment effect. Specifically, we first implement the SCM for non-treated Jakarta taxpayers who met both the pre-treatment taxable income and the pre-treatment wealth thresholds to obtain the spillover effect estimates. Then, we pooled the individual spillover estimates with the individual treatment effects of the treated taxpayers to retrieve the ITT estimates. For this analysis, we again consider only non-Jakarta taxpayers in the potential donor pool for the SCM.

4.2. Matching and pre-treatment fit

In some instances, our matching process is hampered by missing pre-treatment observations[31] (e.g., 39 treated HWI taxpayers have missing income information on some of the variables before 2009) or by the failure of pre-filtering for the pre-intervention income, wealth, or demographics[32] to produce a suitable control safeguarding interpolation and overfitting bias. Bearing this in mind, we are only able to construct a viable synthetic counterpart for 864–916 (71.5–75.5 %) of the total 1209 treated HWI taxpayers.[33] Notwithstanding, compared to the significant pre-treatment differences between targeted and non-targeted individuals in earned and taxable income, income tax, net wealth, age, and the number of dependents (Table A2), the pre-treatment differences between targeted individuals and their corresponding synthetic counterfactuals are substantively reduced (Table A3).[34] This demonstrates SCM's ability to enable comparability by reducing characteristic differences between the treated and non-treated groups. Not only are differences between key factors such as pre-treatment earned and taxable income, income tax, and net wealth no longer statistically significant across the treatment and synthetic control groups, but the *t*-values for all other factors decreased substantially.

The difference in the pre-treatment number of dependents has been reduced to 0.18 from 0.46 using the SCM approach. The distribution of core factors is also similar for both groups (see Fig. 1), as are the pre-treatment average values.[35] Despite the similarity of the pre-treatment values of the outcome variables between the treated units and their corresponding synthetic control unit over the 3-year period, we do not necessarily have a good fit for the pre-treatment trajectory. We therefore assessed the pre-treatment mean square prediction error (MSPE) of each individual match and discarded treated–synthetic control pairs with MSPE greater than the average value of the corresponding pre-treatment outcome variable of the treated taxpayers. Appendix Table A6 (panel A) shows that about 2.9 %–4.1 % of the treated-synthetic control pairs were excluded due to poor pre-treatment fit of income variables (earned income, taxable income, and income tax), and 8.3 % of the matches were discarded due to weak matching of net wealth. While excluding matches with poor pre-treatment fit increases the internal validity of the synthetic control estimates, it does so at the expense of external validity. In particular, excluded matches are likely from treated taxpayers with extremely high pre-treatment income or wealth where there is no combination of suitable control units that can reproduce the outcome variable trajectory before the intervention. We therefore caution the reader to carefully interpret the estimates of the causal effect, which may not apply to ultra-high net wealth individuals.

[29] In this case, the potential bias would depend on two elements. First, the size and direction of the potential spillover effect of the intervention on non-treated HWIs in Jakarta. If the intervention positively affects non-treated Jakarta HWIs' tax reporting behaviour, the synthetic control estimate would produce a lower bound estimate of the causal effect. In contrast, if the treatment results in non-treated Jakarta HWIs under-declaring taxable income or wealth, this would lead to an upper bound estimate. Second, the share of non-treated Jakarta HWIs in the construction of the synthetic control unit will determine the severity of the bias. Our results including all non-treated Indonesia taxpayers suggest that, on average, a synthetic control unit is composed of 0.58 to 0.6 (SD between 0.3 and 0.35) donors from Jakarta control taxpayers.

[30] However, a potential drawback for using non-treated Jakarta HWIs as donors is that they might be perceived to be less likely to evade taxes compared to the treated individuals. This may in turn risk overestimating the treatment effect on the treated.

[31] To ensure post-treatment comparison with the treated unit, we include only control units with at least as many non-missing time observations as the treated unit in the donor pool. Failure to do so would result in synthetic control unit with combined weight less than unity.

[32] For example, for 180 treated HWIs, there is no available control unit that satisfied all pre-filtering criteria.

[33] By relaxing the matching criteria (e.g., including Jakarta taxpayers in the control pool and excluding the demographic matching) increases the number of viable matches.

[34] Pre-treatment differences between treated and all non-treated taxpayers, and non-treated residents in and outside Jakarta are reported in Table A4.

[35] Distributional difference in all outcome variables is also absent when we exclude Jakarta taxpayers from the control group, exclude demographic variables from the list of predictors, or both (see Appendix Table A5, panel A).

5. Results

5.1. Direct effects of the HWI monitoring

The average treatment effects for the treated taxpayers (ATET) are presented for four tax reporting variables: *earned income, taxable income, income tax*, and *net wealth*. These effects are illustrated in Fig. 2. The dark blue bars (right side of each panel) show the post-treatment effect and the light blue bars (left side of each panel) for pre-treatment counterfactual differences. The thinner grey bars show the placebo effects.[36] For the variables earned income, taxable income, and income tax, the data shows an increase in differences between the treated and synthetic control groups in the post-treatment period. This suggests that higher monitoring had a positive effect ($p < 0.01$). Specifically, since the introduction of the HWI unit in 2009, the average reported earned income, taxable income, and income tax for HWIs increased by 699 (34.2 %), 605 (29.9 %), and 182 (32.4 %) million IDR (2010 constant)[37] more than their synthetic counterfactual, respectively.[38] These mean values for individual treatment effects are statistically different from both zero and the pre-treatment average differences ($p < 0.01$). Furthermore, these average causal effects are significantly more pronounced than what one would expect when compared to the distribution of placebo effects (see Table A8).[39] However, the intervention does not appear to affect post-treatment reported net wealth as the effect (post-intervention difference in net wealth between treated and synthetic control) is not statistically significant.

Fig. 3 illustrates the counterfactual gap over time. The treatment effects on earned and taxable income and income tax seem to have sustained over the first three years after establishing the HWI office (left panel; mean differences are significant at the 5 % level) but were most profound in the first year since introduction (see Table A9).[40] However, the positive impact on tax declaration begin to wane after the first year of intensified monitoring and continue to diminish over the subsequent three years. By 2012, which is four years post-treatment, these effects fall within 95 % of the distribution of the placebo effect, suggesting that they may be short-lived. Conversely, the post-intervention trajectory of net wealth for the synthetic control group showed a high degree of resemblance to that of the treated taxpayers, indicating that the intervention does not have a statistically significant impact on reported net wealth.[41]

5.2. Robustness

To test the robustness of these results, we tightened and relaxed the matching criteria for the synthetic control group (Fig. 4 and Table 2). First, we relaxed the matching process by removing any restrictions on demographic characteristics (Configuration 2).[42] Next, we included non-treated residents from the Jakarta region in the potential control pool, both with and without demographic characteristics (Configuration 3 and 4, respectively). Lastly, to assess whether our main results might be biased due to including all pre-intervention outcomes as predictors (Kaul et al., 2022), we relied solely on the pre-treatment averages of the outcome variables (in addition to demographics) in Configuration 5. While the approach in the main configuration mitigates concerns around potential spillover effects skewing the synthetic control estimates, incorporating Jakarta taxpayers into the matching process could enhance the pre-treatment fit and improve comparison validity, given their similar economic and business environments to the treated HWIs. Nevertheless, estimates obtained from Configurations 3 and 4 tend to provide a lower-bound estimate of the ATET due to the positive spillover effects, as discussed in Section 5.3.

Overall, the results across all synthetic control group configurations align closely with our main findings. Post-pre differences between the treated and synthetic control groups indicate a statistically significant increase in earned and taxable income (Table 2).

[36] The pre- and post-treatment average outcomes of the treated HWI and the corresponding synthetic control groups are presented in Fig. A2. The figure also shows the respective statistics for non-treated taxpayers.

[37] Using 2010 exchange rate, these values translate to about 87,375 (earned income), 75,625 (taxable income), and 22,275 (income tax) in USD, respectively.

[38] The ATETs derived from the traditional Difference-in-Differences method, using control groups identified by the Synthetic Control Method (SCM) as donors, yield results that are strikingly similar. Specifically, the estimated average treatment effects for earned income, taxable income, and income tax amount to 725, 628, and 152 million IDR, respectively (see Table A7).

[39] Table A2, panel B shows the average pre-treatment differences between the placebo units and their corresponding synthetic control group are not statistically different. Table A4, panel B also shows that only a small proportion of placebo runs were discarded due to poor pre-intervention fit of the outcome variables.

[40] The notable decrease in income variables for the synthetic control group in 2009 is likely influenced by the 2007-08 Global Financial Crisis (GFC), despite Indonesia's economy remained relatively stable during this period. The underlying assumption is that both the treated and synthetic control groups experienced similar financial impacts from the crisis due to their similar post-treatment net wealth. However, a potential concern arises if the treated HWIs were either less affected by the GFC or experienced a delayed impact compared to their counterparts outside Jakarta. Such a scenario could introduce an upward bias in our treatment effect estimates. To mitigate this concern, we conducted an additional analysis that accounts for a potential delayed impact of the GFC on the treatment group. Specifically, we evaluated the post-treatment differences between the treatment and synthetic control group using one-year lag for the latter. Our findings indicate the treatment effects remain statistically significant (see Table A10).

[41] Except for the year 2011, which is at 5% level of significance. However, the estimate may not be reliable as large fraction of the 2011 HWI records were lost due to internal data migration issues in late 2010.

[42] Pre-treatment fit of the outcome variables improves slightly when matching does not rely on demographics and by including non-treated Jakarta taxpayers in the control group (Table A4).

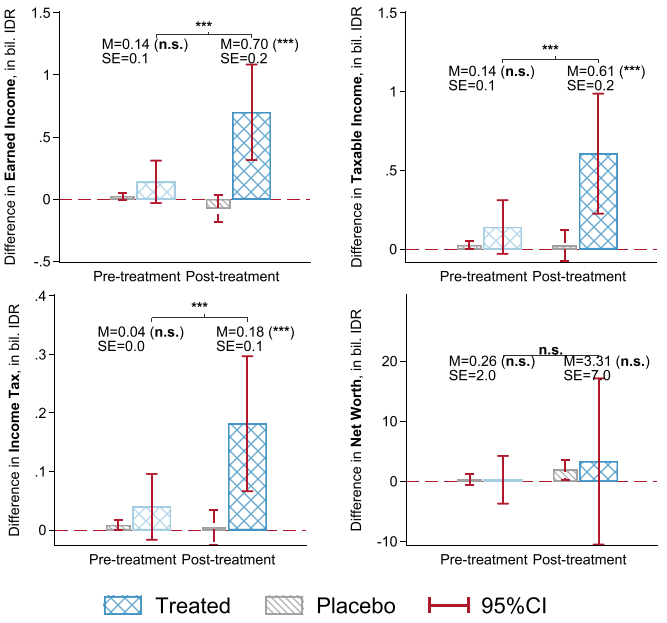

Fig. 2. Average treatment effects on taxable income, earned income, income tax, and net wealth.
Notes: Pre- and post-treatment differences are calculated by taking the average of the treated-synthetic control difference of all (individual-year) observations. We show the mean (M) and standard error of the mean (SE) of the four outcome differences in both pre- and post-treatment periods of the treatment group (treated HWIs in blue) and the placebo group (in grey). Significance levels in parentheses indicate whether the mean (treatment group) is statistically different from zero, based on a two-tailed *t*-test. The red-capped spikes indicate the 95 % confidence intervals of the mean difference. *, **, and *** denote statistical significance at the 10 %, 5 %, and 1 % levels, respectively. n.s. indicates statistical significance above the 10 % level.

Specifically, earned income rose by approximately 544 to 672 million IDR (equivalent to 40,000–42,400 USD), reflecting a 20.7–34.3 % post-treatment difference. Taxable income increased by 406 to 551 million IDR (about 30,000–40,800 USD), equating to a 16–29.9 % post-treatment difference.[43] We also observed a significant treatment effect on the actual income tax paid. The average post- and pre-treatment difference in income tax ranged from 142 to 179 million IDR (approximately 10,500–13,300 USD), or a 22.5–32.4 % post-treatment difference. In contrast, the post-pre differences in net wealth did not reach statistically significant. Additionally, we provide estimates based on a subsample of HWIs whose matching results are unaffected by missing pre- or post-treatment observations across all configurations (Table A11). These results are highly comparable (statistically and economically) to those in Table 2.

These findings strongly suggest that the enhanced scrutiny by the HWI unit led to a sizeable increased income declarations from wealthy taxpayers, thereby boosting income tax collection. Yet, we found that the size of the treatment effects on earned income was larger than that on taxable income. This discrepancy may suggest that these high wealth individuals were finding strategies to deduct more expenses and avoid taxation (e.g., shifting money into tax havens). For example, based on a back-of-the-envelope calculation using the average proportion of taxable income as earned income, 99 % of total earned income was declared as taxable in the pre-treatment period for both the treated taxpayers (99 %) and their corresponding synthetic control unit (98.9 %). However, in the post-treatment period, the ratio of taxable income to earned income dropped to 96 % for the treated taxpayers, whilst their synthetic counterparts continued to declare a high proportion (99.2 %) as taxable income.[44]

5.3. *Intention-to-treat and spillover effects*

Until now, we have focused on the direct positive impact of the HWI intervention on the income reporting behaviour of taxpayers who received the treatment, i.e., reassigned to the HWI Tax Office with strict monitoring. However, it remains unclear whether and how the non-treated Jakarta wealthy taxpayers—who were not subject to increased probability of audit but met the DGT's HWI taxable income and net wealth threshold criteria—responded to such policy intervention. Such individuals might reassess their audit risk due

[43] *P*-values derived from placebo distribution (Table A5) also indicate the average treatment effects on all income variables are statistically significant.
[44] This is consistent across all configurations.

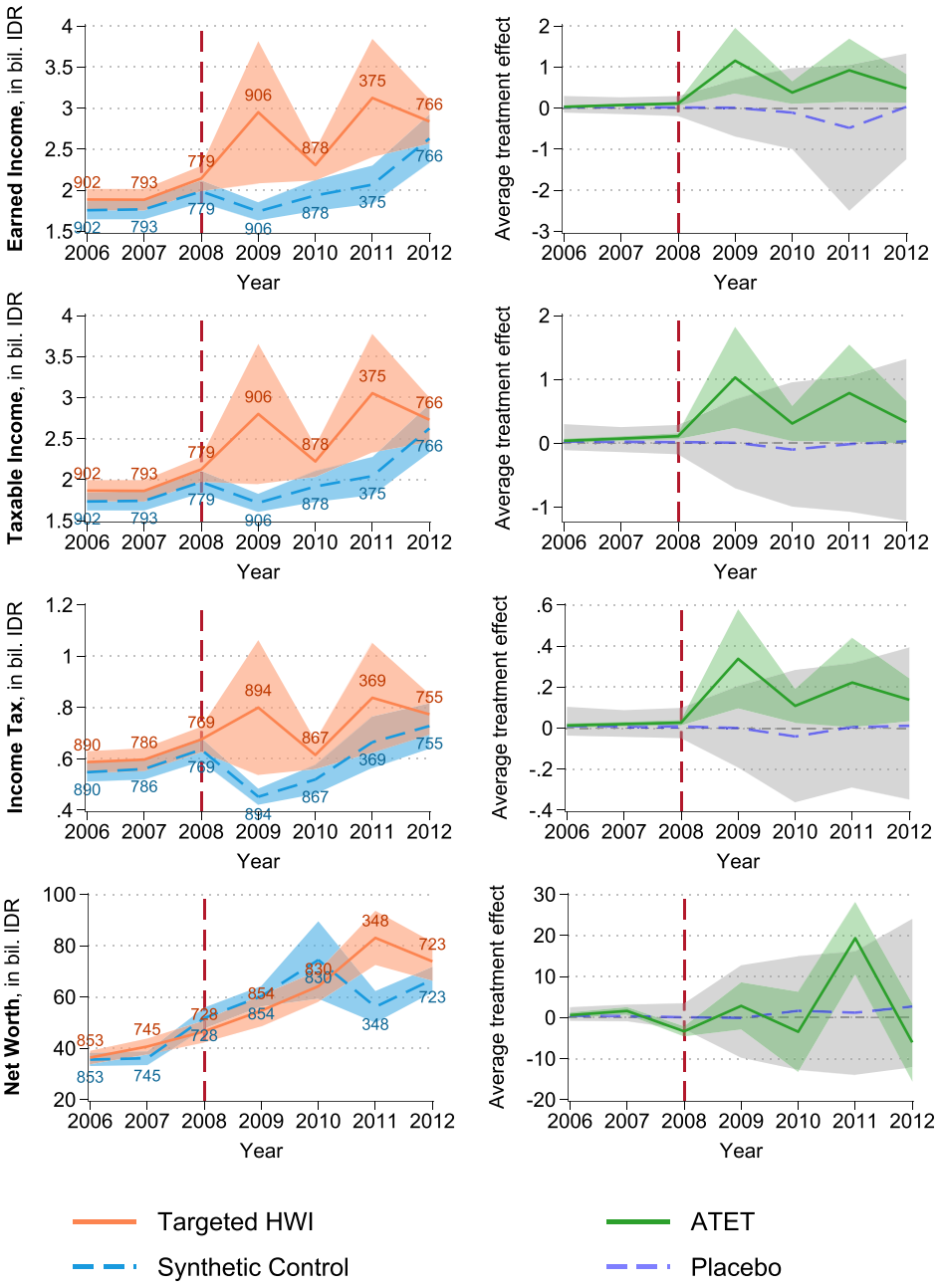

Fig. 3. Yearly counterfactual difference on taxable income, earned income, income tax, and net wealth.
Notes: Left panels show the average values of the outcome variables for treated taxpayers and their corresponding synthetic control group (coloured areas represent the 95 % confidence intervals of the mean). Right panels show the corresponding average treatment effect on the treated (coloured areas represent the 95 % CI of ATET) and the placebo effect with coloured areas representing the middle 95 % of placebo distribution. The marker labels (left panels) indicate the number of available observations in each year. If an observation is missing for either the treatment or synthetic control group, we perform pairwise deletion.

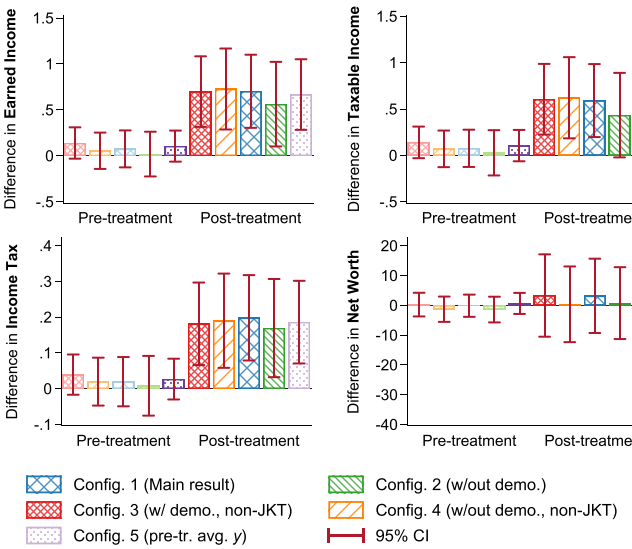

Fig. 4. Robustness test with alternative matching configurations.
Notes: The red-capped spikes indicate the 95 % confidence intervals of the mean estimate (average treated-synthetic control difference). Configuration 1 = main result; Configuration 2 = excludes demographics from the list of predictors; Configuration 3 = same as the main result but restricted to non-Jakarta sample only; Configuration 4 = same as Configuration 2 but restricted to Jakarta sample only. Configuration 5 = same as the main result but include only pre-treatment outcome averages rather than yearly values as predictors. Difference measured in billion IDR.

Table 2
Pre- and post-treatment average treated-synthetic control difference and post-pre differences of different matching configurations.

Outcome variable	Config.	N	Pre-treatment	Post-treatment	Post-pre difference	*t*-stat.
Earned income	1	916	0.140	0.699***	0.559***	−3.15
	2	1025	0.055	0.728***	0.672***	−3.51
	3	987	0.076	0.703***	0.627***	−3.57
	4	1080	0.019	0.563**	0.544***	−2.79
Taxable income	1	916	0.140	0.606***	0.465***	−2.65
	2	1030	0.071	0.622***	0.551***	−2.89
	3	987	0.076	0.591***	0.515***	−2.97
	4	1083	0.028	0.434*	0.406**	−2.10
Income tax	1	904	0.040	0.182***	0.142***	−2.65
	2	1023	0.020	0.190***	0.170***	−2.96
	3	979	0.020	0.198***	0.179***	−3.39
	4	1078	0.008	0.170**	0.161***	−2.77
Net wealth	1	864	0.257	3.309	3.052	−0.46
	2	985	−1.290	0.356	1.646	−0.28
	3	942	−0.121	3.199	3.321	−0.55
	4	1042	−1.407	0.754	2.161	−0.39

Notes: Earned income, income tax, taxable income and net wealth in billion Indonesian rupiahs (IDR). Configuration 1 = donor pool restricted to non-Jakarta taxpayers, with demographic variables included in the list of predictors (main result); Configuration 2 = matching only on pre-treatment income and wealth related variables, with donor pool restricted to non-Jakarta taxpayers; Configurations 3 and 4 are the same as 1 and 2, respectively but the donor pool was not restricted. *, **, and *** denotes statistical significance at the 10 %, 5 %, and 1 % levels, respectively.

to their proximity to the targeted group, suggesting a possible spillover effect of the treatment. To capture the comprehensive impact of the intervention, we derive the *total* treatment effect using an *intention-to-treat* approach. This entails pooling the abovementioned ATET with the synthetic control estimates on non-treated wealthy taxpayers in Jakarta.[45]

We present the pre- and post-treatment average and yearly counterfactual differences in Fig. 5 and Fig. 6, respectively. Generally, our findings support the existence of positive spillover effects, albeit of a smaller magnitude compared to the ATET (see Table A12). That is, the establishment of the HWI Tax Office potentially serves as a deterrence signal regarding tax enforcement efforts. Non-treated wealthy taxpayers in Jakarta declared a higher post-intervention earned income (ranging from 377 to 629 million IDR, or a

[45] Again, we implement two configurations of synthetic control method for each outcome variable, i.e., by including or not including demographic variables in the list of predictors. We present results of the former in the main text. Table A2, panel C shows the pre-treatment counterfactual difference of the list of predictors and Table A4, panel C presents the share of observations disposed due to poor or absence of match.

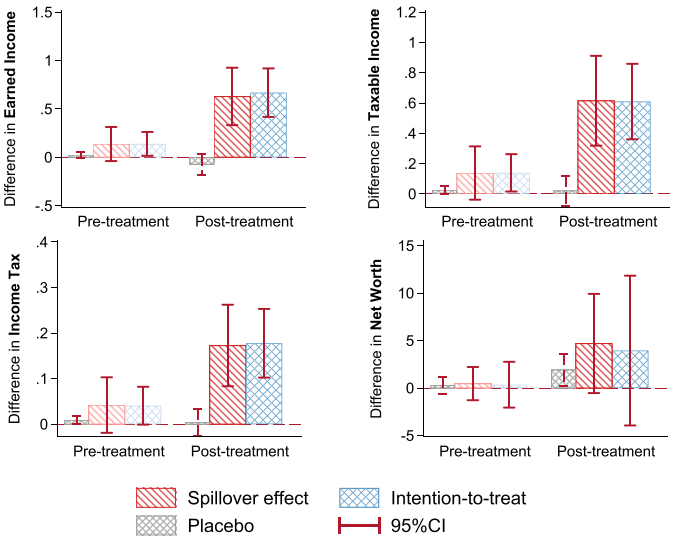

Fig. 5. Mean estimates of *intention-to-treat* and *spillover* effect on non-treated Jakarta HWIs.
Notes: The red-capped spikes indicate the 95 % confidence intervals of the mean estimate. Synthetic control estimates of the intention-to-treat and spillover effect (on non-treated Jakarta HWIs) are averaged across individuals and pre-and post-intervention years. Difference measured in billion IDR.

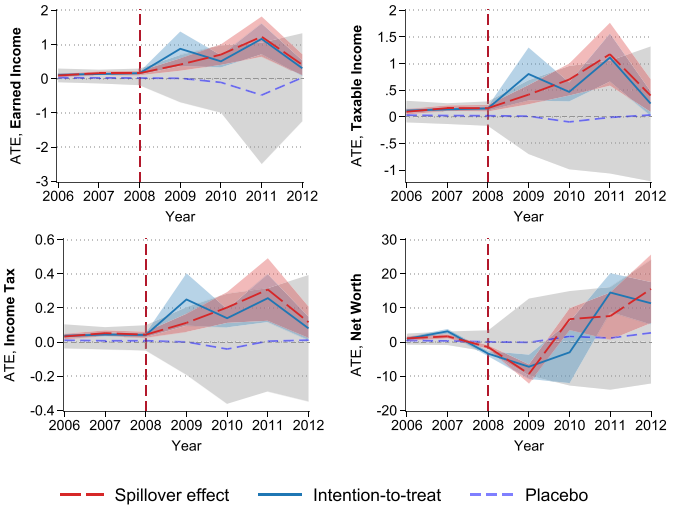

Fig. 6. Intention-to-treat estimates and spillover effect over time.
Notes: Coloured areas represent the 95 % CI of mean ITT and spillover effect. Grey areas show the middle 95 % of the placebo effect distribution.

14.5–28.5 % post-treatment counterfactual difference) and taxable income (288–477 million IDR, or a 13–28 % post-treatment counterfactual difference) relative to their synthetic counterpart in non-Jakarta regions. However, the spillover effect on income tax and net wealth remain inconclusive as matching without using demographics gives (conflicting) insignificant results.[46]

By combining the ATET estimates (*direct* effects) with the treatment effects on non-treated HWIs in Jakarta (*indirect* effects), we find the average ITT estimates on all income variables (earned and taxable income and income tax) are both positive and statistically significant (relative to the placebo effect[47]) (Table A12). Specifically, the ITT effects show a post-intervention increase of 575–668 million IDR in earned income (or a 23.4–31.6 % boost), 499–610 million IDR in taxable income (or a 20.2–29 % surge), and 133–178 million IDR in income tax (or an 18.6–30.5 % rise). Interestingly, unlike the immediate impact of ATET or ITT, which were already

[46] Matching with demographic predictors indicates a highly significant positive effect on income tax paid (at 1% level). The post-treatment counterfactual difference ranges from 58 to 173 million IDR (7.5–28.2 % increase).
[47] Here, we use either configuration 1 or 2 for ATET and only compare to the placebo distribution from non-Jakarta taxpayers.

observable in the first year of the intervention, the spillover effect gradually increased and peaked in 2011 before diminishing at the year of the termination of the HWI Tax Office in 2012 (Fig. 6). Additionally, it was found that there is no difference in the pre-post treatment ratio of taxable and earned income for non-treated Jakarta HWIs, similar to their synthetic control group (~99 % of earned income declared as taxable in both periods) and the difference in their relative effect sizes are smaller for non-treated Jakarta HWIs compared to treated HWIs. This might suggest that non-treated Jakarta HWIs exert less effort in tax avoidance. Conversely, we found that non-treated taxpayers seemed to reduce the amount of net wealth in the first year of the intervention, perhaps as an attempt to reduce detection by the tax office to avoid inclusion in the HWI special unit.[48]

As an extension, we explored additional potential spillover effects amongst the broader population of Jakarta taxpayers who are financially less akin to the treated group, aiming to understand the extent of the impact on income declaration behaviour (Fig. 7). Specifically, we used the non-Jakarta taxpayers sample to construct the synthetic control estimates for (1) non-treated Jakarta residents with more than 10 billion IDR pre-treatment net wealth but does not meet the taxable income threshold (most relevant to HWI); (2) non-treated Jakarta high-income earners with pre-treatment taxable income exceeding 1 billion but did not meet the DGT wealth threshold; and (3) a randomly selected sample of Jakarta taxpayers who meet neither of the two thresholds (least relevant, with the highest level of dissimilarity to HWIs).

We identified a statistically significant spillover effect on income declaration measures for Jakarta residents who have substantial wealth but are not HWI-eligible; however, the effect sizes shrink substantially (Table A13).[49] Contrarily, we did not find a significant positive spillover effect on taxpayers falling below the net wealth threshold or those who did not meet both thresholds.[50] This echoes the findings of Lediga et al. (2020), in which the spillover effect dissipates as the individual's characteristics diverge from the targeted group. Unexpectedly, we found evidence that non-eligible taxpayers with pre-treatment taxable income and net wealth below the HWI threshold declared less income relative to their non-Jakarta synthetic counterparts in the post-treatment period. This raises the possibility that the HWI initiative might have inadvertently triggered negative spillover effects on tax compliance for this specific cohort. Non-treated individuals who are aware of the program but not directly affected might feel as though they are 'flying under the radar, perhaps due to the perception that the tax administrative resources are now focused on the treated group, leading them to believe that scrutiny on non-treated HWIs is reduced. It is important to note, however, that these effects are relatively mild and fall within the range of the placebo effects distribution. For example, these non-eligible Jakarta taxpayers reported income tax figures that were 5.7–7.2 % less than their synthetic counterfactuals (see bottom panel of Table A13). Finally, we found negligible differences in the ratios of taxable-to-earned income for all three subgroups in both the pre- and post-treatment periods.

6. Conclusions

Existing empirical evidence in tax compliance relies heavily on the middle classes. To expand our empirical knowledge base, this study focuses on rich individual taxpayers in Indonesia. Globalization has offered very rich individuals a larger playground and increased their opportunities to acquire further resources (Torgler and Piatti, 2013). Thus, enhancing tax compliance amongst the wealthy is paramount for bolstering public tax revenues. Motivated by the limited empirical studies on tax compliance in developing nations, and particularly on how the wealthy respond to different policy strategies, we conducted an analysis using individual micro-level data on tax returns in Indonesia. Specifically, our study leveraged a quasi-natural experiment—the 2009 establishment of a specialised tax office for High Wealth Individuals (HWI) in Indonesia—that increased the audit probability and monitoring of approximately 1200 wealthy taxpayers in Jakarta.

Using 141,097 de-identified individual tax return records from 2006 to 2012, we applied a synthetic control method to develop a set of counterfactual controls to estimate the pre- and post-treatment differences between the treated (subject to heightened monitoring) and synthetic control groups. Our results indicate that the HWI intervention led to increases in declared earned and taxable income, as well as income tax. However, such effect appeared to be short-lived, as they diminish across the four-year program. More interestingly, we found that the ratio of taxable income to earned income actually decreased for the treated taxpayers compared to the synthetic control group and prior to the HWI monitoring. In other words, this indicates that treated taxpayers might be able to find ways of reducing their tax burden through income deductions (e.g., Bastani and Selin, 2014, Engström et al. 2015, Doerrenberg et al., 2017; Doerrenberg et al., 2022; Paetzold, 2019; Bergolo et al., 2021).

Additionally, we found a noteworthy positive spillover effect on income declarations amongst Jakarta taxpayers who met the eligibility for the pre-intervention income and wealth thresholds but were not selected for the treatment, which is comparable to the direct treatment effect. Interestingly, the taxable-to-earned income ratio for these taxpayers remain stable post-intervention, which may suggest that these individuals are less inclined to avoid paying tax when the increase in audit probability is uncertain. In addition, we found that the positive spillover effect was confined to non-treated Jakarta taxpayers whose net wealth exceeded the HWI threshold. Conversely, it did not extend to other non-eligible taxpayers, suggesting that the positive spillover effect increases in proximity. Remarkably, we observed that Jakarta taxpayers who did not meet either the income or wealth thresholds reported marginally lower income (compared to their non-Jakarta counterparts) following the establishment of the HWI office. This could be

[48] The subsequent increase in reported net wealth could also imply that they reduced the cost of underreporting as expansion of the HWI office is unlikely, possibly due to lack of personnel and resource capacity.

[49] The small mean effects also fall within the 70% of the placebo effects distribution.

[50] Despite the fact that post-treatment differences are sometimes significant for matching without demographics, the post-pre differences are only significant (at 5%) for income tax.

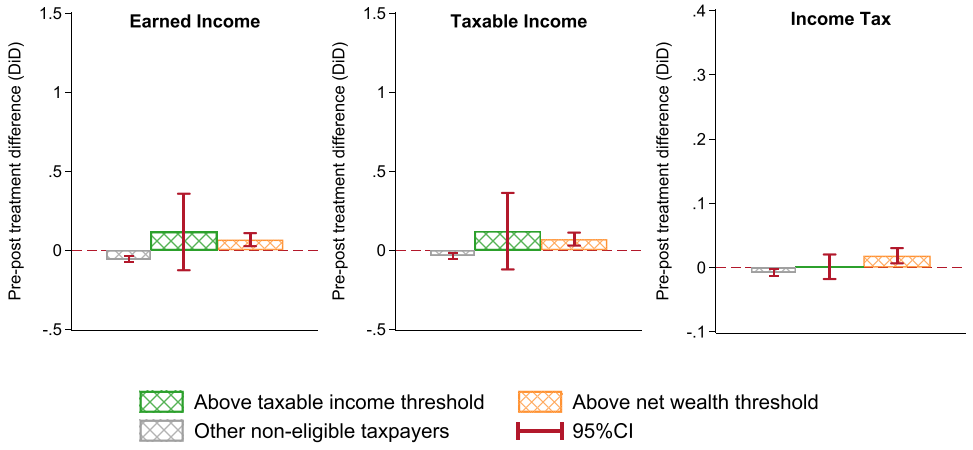

Fig. 7. Spillover effects on non-eligible Jakarta HWIs.
Notes: The red-capped spikes indicate the 95 % confidence intervals of the mean post- and pre-treatment counterfactual differences (DiD). The scale is adjusted for comparison.

attributed to a perceived lower level of audit as more resources were shifted to monitoring wealthier taxpayers or, it could also be due to a decrease of tax morale as the existence of a HWI tax unit may signal to the lower wealth individuals that the rich are not paying their share of taxes. This mirrors concerns about the 'crowding out' of the intrinsic compliance motivation previously voiced in the context of tax amnesties (Torgler et al., 2003; Torgler and Schaltegger, 2005). As a result, these findings highlight the importance of considering both positive and negative externalities when designing policy interventions.

Based on our estimates of the effect on income tax paid, the HWI program had generated approximately 1.7 trillion IDR (212 million USD, in 2010 constant) in additional tax revenue to the Indonesian government over the four years the program was active from 2009 to 2012. This additional revenue can be dissected into the two primary sources: funds obtained directly from the treated HWIs (monitoring effect), accounting for 822–958 billion IDR (103–113 million USD),[51] and funds acquired through a deterrence effect on other non-treated Jakarta HWIs, amounting to 446–1171 billion IDR (55–116 million USD).[52] To put this in perspective, the estimated state revenue generated from individual taxes on income, profits and capital gains across the four years amounts to about 1500 trillion IDR (2022).[53] While the estimated cost of the HWI Tax Office was not available, the program was not perceived as cost-efficient as expected (see Widihartanto 2014). Based on this assessment, it appears as though enhanced monitoring seems less effective in increasing additional tax revenues from wealthy taxpayers, even after accounting for the potential deterrence effect. Future research avenues should explore and identify alternative policy interventions that could effectively increase tax revenues from wealthy taxpayers. For example, no empirical studies yet exist that test whether positive reputation, such as publicly honouring honest taxpayers, could increase tax morale. It may also be interesting to investigate how the effects of participation could influence tax compliance and tax morale, e.g., by allowing (wealthy) taxpayers to have a voice in how the tax money is spent.

To some degree, our findings are limited by the following factors. First, although our strict matching procedure implies that the synthetic control group should closely mirrors the treated group in terms of pre-treatment income and wealth characteristics, we do not have access to information on affiliations with large companies or corporate groups for the control non-Jakarta taxpayers, which is one of the treatment selection criteria. While we think that the matched synthetic controls are likely to have corporate ties to the degree of which is similar to that of the matched treated taxpayer due to the similarity of not only pre-treatment income and wealth structure but also *post-treatment net wealth*, the current estimate may be biased if this assumption does not hold. In this case, the ATET estimates would be upward biased (upper bound estimate) if taxpayers with ties to large companies have greater capability or propensity to evade or avoid paying tax. Another nuance that warrants attention is the influence of the 2008 Global Financial Crisis (GFC) on our treatment estimates. While Indonesia was relatively insulated from the crisis,[54] we anticipate that both treated taxpayers and their synthetic counterpart would exhibit adjustments in response to the GFC. The key underlying assumption is that, in the absence of any real treatment effect, the treated group would similarly display significant drop in income variables for 2009, akin to the control group. However, if treated HWIs from Jarkata were to experience a delay or attenuated impact, this could introduce an upward bias in our estimates, thereby inflating the perceived treatment effects. While we show that treatment effect remains robust accounting for a

[51] Here we take the range of estimates from Table 2 (average post-treatment differences) and multiply by 1,209 HWIs over 4 years.
[52] Estimates are based on Table A11 and A12, assuming 800 (meeting both the HWI net wealth and taxable income thresholds), 750 (meeting the net wealth threshold), 3000 (meeting the taxable income threshold) and 3000 (below both thresholds) Jakarta taxpayers, respectively. Note that the estimated effects for the those below both thresholds are negative.
[53] Widihartanto (2014) reports the estimated revenue contribution from the HWI unit account for only 0.25 % of the total tax revenue from the Large Taxpayers Office in 2011, whereas our estimate across the four years amount to about 0.11 %.
[54] For example, Indonesia reported a drop in real GDP growth of around 2.5 percentage points in 2009 (IMF, 2021).

potential lag in the GFC's impact on the treatment group, future studies should further scrutinize the nuanced role the GFC may play in such settings.

Analogously, the estimated spillover effects on non-treated wealthy Jakarta taxpayers could be conservative (lower bound estimate) if their synthetic controls consist of non-Jakarta residents with significant corporate ties who may have felt heightened scrutiny by the tax office, and therefore acted more compliantly. Moreover, the observed positive spillover effect on non-treated Jakarta HWIs rests on the assumption that the highly publicized treatment may lead to a revision of subjective audit probabilities due to their closeness in eligibility and geography to their treated peers. Nevertheless, other factors such as the history of non-compliance and individual belief (e.g., Slemrod et al. 2001) may influence the sensitivity to such further potential audit threats.

Furthermore, we excluded observations with no viable control taxpayers and ill-fitted pre-treatment matchings. These observations are likely to arise from ultra-wealthy taxpayers or those who experienced income or wealth shocks. If true, we might be underestimating the treatment effects. Similarly, whilst the exact reasons for missing values and sample attrition are unknown (e.g., due to deceased or migration), if they are correlated with the introduction of the HWI Tax Office, the treatment effects might be biased. We thus caution the reader against overgeneralizing our findings.

Future studies based on matching methods should consider the inclusion of additional relevant predictors, which could increase the internal validity of the results. For example, information about the taxpayer's non-compliance history and factors related to the willingness, incentives, and opportunities of non-compliance (e.g., employment status and economic sector) would be useful. Extending the pre-treatment period and accounting for the (economic) similarity between Jakarta and the residing province of the control units could also minimize the threat of overfitting and improve match comparability.

Additionally, knowing taxpayers' income sources (e.g., shares of business income, capital income, or rental income) may also reveal their ability and capacity to avoid taxes, as some sources of income are easier to shelter from tax authorities. This could shed light on the cause of post-treatment reduction in the share of taxable income for the treated taxpayers. Similarly, future research could add to our findings by exploring potential heterogeneous effects by taxpayers' wealth or income level (e.g., the wealthiest 1 % HWIs versus the lower parts of the wealth distribution) as the treatment effects on tax return reporting may differ across groups due to variations in asset composition (e.g., resulting to employing different tax planning/avoidance techniques (Alstadsæter et al., 2022)). This could add to the literature on top-end inequality (Alstadsæter et al., 2019) by focusing on developing countries.

As aforementioned, data access was only available on unaudited tax returns, thus, direct assessment of tax compliance may be prone to measurement errors. By incorporating audit records or changes in audit probabilities, one could also distinguish the deterrence effect of the dedicated tax unit from its direct effect of audits, which captures the amount of unreported income found.

Although the operation of the HWI Tax Office was terminated in 2012 (Widihartanto, 2014; Widihartanto and Braithwaite, 2016), the unit still monitors HWI taxpayers but with the additional responsibility of monitoring state-owned company taxpayers.[55] This new form of operation could add a significant burden for its tax officers, one that may affect the quality of monitoring given to wealthy taxpayers. While our results have indicated that the treatment effect on income declaration has diminished in the last treatment year (2012), future research might consider using more recent data to assess whether the lower (perceived) audit probability implied by the extension of the unit's purview is shifting income and wealth declarations towards pre-treatment values. In addition to their usefulness for policymakers, such findings would provide a good robustness test for the results reported above.

CRediT authorship contribution statement

Ho Fai Chan: Methodology, Software, Formal analysis, Data curation, Writing – original draft, Writing – review & editing, Visualization. **Katharina Gangl:** Writing – review & editing. **Mohammad Wangsit Supriyadi:** Conceptualization, Methodology, Data curation, Writing – original draft, Writing – review & editing. **Benno Torgler:** Conceptualization, Methodology, Writing – original draft, Writing – review & editing, Supervision.

Declaration of Competing Interest

Mohammad Wangsit Supriyadi is currently employed by the Directorate General of Taxes (DGT), Indonesia. Other authors declare no conflicts of interest in preparing this article.

Data availability

The data that has been used is confidential.

Appendix

Figs. A1 and A2 and Tables A1, A2, A3, A4, A5, A6, A7, A8, A9, A10, A11, A12, A13

[55] The official name of the unit, HWI Tax Service Office, has also changed to Large Taxpayers Tax Service Office IV.

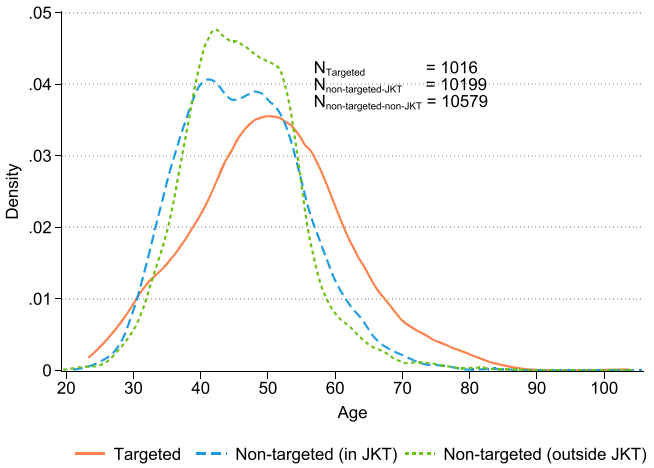

Fig. A1. Age distribution (in 2006) between treated (solid line) and non-treated high-income earners (dashed and dotted lines). Kernel density estimate is based on the Epanechnikov kernel function.

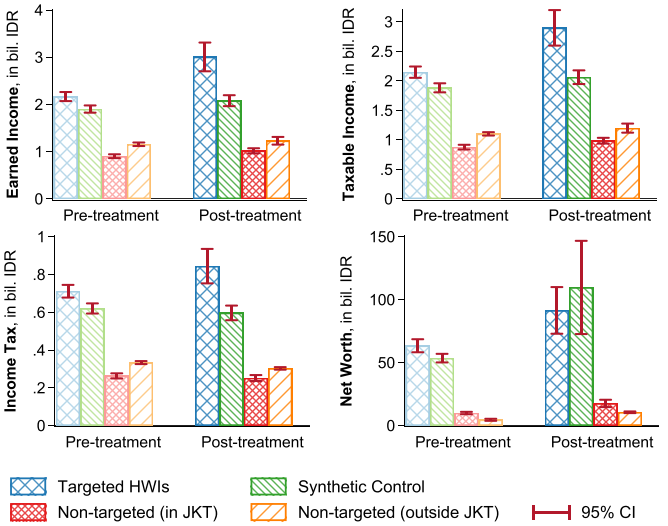

Fig. A2. Pre- and post-treatment average outcomes for taxpayers subject to HWI treatment, matched synthetic control group, and non-targeted taxpayers (by Jakarta residency status). The red-capped spikes indicate the 95 % confidence intervals of the mean.

Table A1
Income tax rate for individual taxpayers.

Taxable Income (IDR)	Income Tax (Percentage)
Up to 50,000,000	5
50,000,001 – 250,000,000	15
250,000,001 – 500,000,000	25
Above 500,000,000	30

Notes: Source from Indonesian Income Tax Law. The approximate exchange rate for the 2010 US dollar is 1 USD = 8000 IDR.

Table A2
Test of pre-treatment differences between monitored HWIs and non-Jakarta taxpayers.

Targeted HWIs vs. non-Jakarta taxpayers						
Variables	$N_{targeted}$	$N_{non\text{-}JKT}$	Targeted	Non-JKT	Difference	*t*-stat.
ln(Earned income)	1196	11,126	21.04 (1.35)	20.3 (1.35)	−0.74***	−34.23
ln(Taxable income)	1195	11,127	21 (1.39)	20.26 (1.39)	−0.75***	−33.56
ln(Income tax)	1195	11,128	19.74 (1.65)	18.97 (1.65)	−0.77***	−29.49
ln(Net wealth)	1192	9495	24.29 (1.24)	20.5 (1.24)	−3.79***	−60.07

(*continued on next page*)

Table A2 (*continued*)

Targeted HWIs vs. non-Jakarta taxpayers						
Variables	$N_{targeted}$	$N_{non\text{-}JKT}$	Targeted	Non-JKT	Difference	*t*-stat.
Age	1016	10,579	50.32 (11.44)	46.15 (11.44)	−4.17***	−14.80
Single	1209	11,130	.09 (0.28)	.06 (0.28)	−0.03***	−4.25
# of dependents	1202	11,127	1.4 (1.17)	1.86 (1.17)	.46***	14.68

Notes: The pre-treatment averages for earned income, income tax, taxable income, and net worth are in log with SD in paratheses. Age is based on the fiscal year 2006; marital status and number of dependents are based on the fiscal year or the first available year in the data set.
*** denotes statistical significance at the 1 % level.

Table A3
Test of pre-treatment differences between monitored high wealth individuals and their synthetic counterparts.

Targeted vs. synthetic control group						
Variables	$N_{targeted}$	N_{SC}	Targeted	Synthetic Control	Difference	*t*-stat.
ln(Earned income)	943	943	21.01 (0.92)	21.05 (0.92)	0.04	1.00
ln(Taxable income)	943	943	20.99 (0.93)	21.03 (0.93)	0.04	0.88
ln(Income tax)	943	943	19.79 (1.06)	19.82 (1.06)	0.03	0.62
ln(Net wealth)	942	942	24.15 (1.16)	24.13 (1.16)	−0.02	−0.35
Age	792	792	51.94 (8.78)	51.49 (8.78)	−0.46	−0.94
Single	943	943	0.07 (0.21)	0.08 (0.21)	0.01	0.81
# of dependents	943	943	1.55 (1.05)	1.37 (1.05)	−0.18**	−3.68

Notes: Targeted refers to monitored individuals, and synthetic control refers to synthetic control units constructed for the targeted individuals. The pre-treatment averages for earned income, income tax, taxable income, and net worth are in 1 billion IDR. Age is based on the fiscal year 2006; marital status and number of dependents are based on the fiscal year or the first available year in the data set. For demographics, we report the pre-treatment differences for the matching for taxable income as the outcome variable, matching based on other outcome variables gives almost identical results.
** denotes statistical significance at the 5 % level.

Table A4
Test of pre-treatment differences between treated and non-treated taxpayers, and non-treated taxpayers by residency.

Targeted HWIs vs. non-targeted taxpayers						
Variables	$N_{targeted}$	$N_{non\text{-}targeted}$	Targeted	Non-targeted	Difference	*t*-stat.
ln(Earned income)	1196	21,995	21.04 (1.35)	20.4 (1.35)	.64***	16.27
ln(Taxable income)	1195	21,996	21 (1.39)	20.36 (1.39)	.65***	15.97
ln(Income tax)	1195	21,997	19.74 (1.65)	19.08 (1.65)	.66***	13.67
ln(Net wealth)	1192	19,682	24.29 (1.24)	20.82 (1.24)	3.47***	90.05
Age	1016	20,778	50.32 (11.44)	46.17 (11.44)	4.15***	11.42
Single	1209	22,000	.09 (0.28)	.09 (0.28)	−0.00	−0.09
# of dependents	1202	21,997	1.4 (1.17)	1.7 (1.17)	−0.31***	−8.90
Non-treated outside Jakarta vs. Non-treated in Jakarta						
Variables	**$N_{non\text{-}JKT}$**	**N_{JKT}**	**Non-JKT**	**JKT**	**Difference**	***t*-stat.**
ln(Earned income)	10,869	11,126	20.5 (0.72)	20.3 (0.72)	−0.21***	−23.26
ln(Taxable income)	10,869	11,127	20.46 (0.73)	20.26 (0.73)	−0.21***	−22.75
ln(Income tax)	10,869	11,128	19.2 (0.84)	18.97 (0.84)	−0.23***	−22.01
ln(Net wealth)	10,187	9495	21.11 (1.68)	20.5 (1.68)	−0.61***	−22.49
Age	10,199	10,579	46.18 (9.12)	46.15 (9.12)	−0.02	−0.20
Single	10,870	11,130	.12 (0.33)	.06 (0.33)	−0.06***	−16.68
# of dependents	10,870	11,127	1.54 (1.06)	1.86 (1.06)	.32***	22.69

Notes: The pre-treatment averages for earned income, income tax, taxable income, and net worth are in log with SD in paratheses. Age is based on fiscal year 2006; marital status and number of dependents are based on fiscal year or the first available year in the data set.
*** denotes statistical significance at the 1 % level.

Table A5
Pre-treatment balance between non-targeted taxpayers by Jakarta and non-Jakarta residence.

Config.	Outcome variable	$N_{Treated}$	N_{SC}	Treated	Synthetic control	Diff.	t-stat.	K–S p-val.
Treated HWIs, panel A								
(Main) Non-JKT w/ demo.	Earn. Inc.	943	943	21.01 (0.92)	21.05 (0.92)	0.04	1.00	0.40
	Tax. inc.	943	943	20.99 (0.93)	21.03 (0.93)	0.04	0.88	0.36
	Inc. tax	943	943	19.79 (1.06)	19.82 (1.06)	0.03	0.62	0.36
	Net wealth	942	942	24.15 (1.16)	24.13 (1.16)	−0.02	−0.35	0.96
Non-JKT w/o demo.	Earn. Inc.	1075	1075	21.11 (1.07)	21.13 (1.07)	0.02	0.42	0.69
	Tax. inc.	1075	1075	21.09 (1.08)	21.10 (1.08)	0.01	0.31	0.62
	Inc. tax	1075	1075	19.89 (1.23)	19.89 (1.23)	0.01	0.10	0.62
	Net wealth	1073	1073	24.27 (1.19)	24.22 (1.19)	−0.05	−0.96	0.83
All IDN w/ demo.	Earn. Inc.	1023	1023	21.09 (1.03)	21.12 (1.03)	0.03	0.56	0.97

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Table A5 (*continued*)

Config.	Outcome variable	$N_{Treated}$	N_{SC}	Treated	Synthetic control	Diff.	t-stat.	K–S p-val.
	Tax. inc.	1023	1023	21.07 (1.04)	21.10 (1.04)	0.02	0.48	0.96
	Inc. tax	1023	1023	19.88 (1.18)	19.89 (1.18)	0.01	0.24	0.97
	Net wealth	1022	1022	24.16 (1.35)	24.12 (1.35)	−0.03	−0.56	0.87
All IDN w/o demo.	Earn. Inc.	1119	1119	21.14 (1.16)	21.15 (1.16)	0.01	0.13	0.99
	Tax. inc.	1119	1119	21.12 (1.18)	21.12 (1.18)	0	0.05	0.88
	Inc. tax	1119	1119	19.91 (1.34)	19.90 (1.34)	0	−0.08	0.64
	Net wealth	1118	1118	24.26 (1.40)	24.21 (1.40)	−0.04	−0.75	0.78
Placebo, panel B								
Non-JKT w/ demo.	Earn. Inc.	4624	4624	20.29 (0.55)	20.31 (0.55)	0.01	1.28	0.46
	Tax. inc.	4624	4624	20.26 (0.56)	20.27 (0.56)	0.02	1.29	0.46
	Inc. tax	4623	4623	18.97 (0.66)	18.99 (0.66)	0.02	1.36	0.49
	Net wealth	4565	4568	20.59 (2.59)	20.57 (2.59)	−0.02	−0.32	0.95
Non-JKT w/o demo.	Earn. Inc.	4782	4782	20.32 (0.60)	20.32 (0.60)	0	0.24	1.00
	Tax. inc.	4779	4779	20.28 (0.61)	20.29 (0.61)	0	0.34	1.00
	Inc. tax	4775	4775	19.00 (0.71)	19.01 (0.71)	0.01	0.36	1.00
	Net wealth	4744	4747	20.73 (2.39)	20.68 (2.39)	−0.04	−0.91	0.81
All IDN w/ demo.	Earn. Inc.	10,138	10,138	20.41 (0.65)	20.41 (0.65)	0	0.35	1.00
	Tax. inc.	10,153	10,153	20.37 (0.67)	20.37 (0.67)	0	0.36	1.00
	Inc. tax	10,129	10,129	19.11 (0.77)	19.11 (0.77)	0	0.36	1.00
	Net wealth	10,024	10,027	20.97 (2.18)	20.93 (2.18)	−0.04	−1.36	0.48
All IDN w/o demo.	Earn. Inc.	10,121	10,124	20.41 (0.67)	20.41 (0.67)	0	0.15	0.99
	Tax. inc.	10,151	10,152	20.38 (0.69)	20.38 (0.69)	0	0.25	0.99
	Inc. tax	10,103	10,104	19.11 (0.79)	19.11 (0.79)	0	0.14	0.99
	Net wealth	10,059	10,065	21.02 (1.95)	20.96 (1.95)	−0.05*	−1.85	0.44
ITT and *Spillover effect (non-treated Jakarta HWIs + treated HWIs)*, panel C								
w/ demo.	Earn. Inc.	1695	1695	21.15 (0.83)	21.20 (0.83)	0.05	1.60	0.21
	Tax. inc.	1695	1695	21.14 (0.84)	21.18 (0.84)	0.04	1.50	0.17
	Inc. tax	1695	1695	19.97 (0.94)	20.01 (0.94)	0.04	1.24	0.19
	Net wealth	1694	1694	23.83 (1.04)	23.83 (1.04)	0	0.04	0.92
w/o demo.	Earn. Inc.	1894	1894	21.25 (0.95)	21.27 (0.95)	0.02	0.61	0.58
	Tax. inc.	1894	1894	21.23 (0.96)	21.25 (0.96)	0.02	0.54	0.40
	Inc. tax	1894	1894	20.07 (1.07)	20.08 (1.07)	0.01	0.34	0.40
	Net wealth	1891	1891	23.94 (1.08)	23.91 (1.08)	−0.03	−0.92	0.95

Notes: Income and wealth variables are in log. *Non-JKT only* = Donor pool includes only non-Jakarta residing taxpayers. *All IDN* = Donor pool includes both Jakarta and non-Jakarta residing taxpayers. *w/ demo.* and *w/o demo.* = List of predictors include and do not include demographic variables (age, marital status, and number of dependents), respectively. Monetary variables are in log.

* denotes statistical significance at the 10 % level.

Table A6

Treated-synthetic control matches excluded due to poor pre-intervention fit.

Config.	Outcome variable	# matches with small MSPE	Total matches	Perc. discarded (MSPE)	Perc. discarded (All)
ATET (Treated HWIs), panel A					
(Main)	Earn. Inc.	916	943	2.86 %	24.23 %
Non-JKT	Tax. inc.	916	943	2.86 %	24.23 %
w/ demo.	Inc. tax	904	943	4.14 %	25.23 %
	Net wealth	864	942	8.28 %	28.54 %
Non-JKT	Earn. Inc.	1025	1075	4.65 %	15.22 %
w/o demo.	Tax. inc.	1030	1075	4.19 %	14.81 %
	Inc. tax	1023	1075	4.84 %	15.38 %
	Net wealth	985	1073	8.20 %	18.53 %
All IDN	Earn. Inc.	987	1023	3.52 %	18.36 %
w/ demo.	Tax. inc.	987	1023	3.52 %	18.36 %
	Inc. tax	979	1023	4.30 %	19.02 %
	Net wealth	942	1022	7.83 %	22.08 %
All IDN	Earn. Inc.	1080	1119	3.49 %	10.67 %
w/o demo.	Tax. inc.	1083	1119	3.22 %	10.42 %
	Inc. tax	1078	1119	3.66 %	10.84 %
	Net wealth	1042	1118	6.80 %	13.81 %
Placebo, panel B					
Non-JKT	Earn. Inc.	4608	4624	0.35 %	14.37 %
w/ demo.	Tax. inc.	4608	4624	0.35 %	14.37 %
	Inc. tax	4603	4623	0.43 %	14.46 %
	Net wealth	4507	4570	1.38 %	16.24 %
Non-JKT	Earn. Inc.	4765	4782	0.36 %	11.45 %
w/o demo.	Tax. inc.	4762	4779	0.36 %	11.50 %
	Inc. tax	4753	4775	0.46 %	11.67 %

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Table A6 (*continued*)

Config.	Outcome variable	# matches with small MSPE	Total matches	Perc. discarded (MSPE)	Perc. discarded (All)
	Net wealth	4693	4749	1.18 %	12.79 %
All IDN	Earn. Inc.	10,109	10,138	0.29 %	7.75 %
w/ demo.	Tax. inc.	10,124	10,153	0.29 %	7.61 %
	Inc. tax	10,093	10,129	0.36 %	7.89 %
	Net wealth	9963	10,029	0.66 %	9.08 %
All IDN	Earn. Inc.	10,101	10,124	0.23 %	7.82 %
w/o demo.	Tax. inc.	10,131	10,152	0.21 %	7.55 %
	Inc. tax	10,085	10,104	0.19 %	7.97 %
	Net wealth	10,028	10,071	0.43 %	8.49 %
ITT (Non-treated Jakarta HWIs + treated HWIs), panel C					
w/ demo.	Earn. Inc.	1650	1695	2.65 %	19.12 %
	Tax. inc.	1650	1695	2.65 %	19.12 %
	Inc. tax	1628	1695	3.95 %	20.20 %
	Net wealth	1604	1694	5.31 %	21.37 %
w/o demo.	Earn. Inc.	1817	1894	4.07 %	10.93 %
	Tax. inc.	1822	1894	3.80 %	10.69 %
	Inc. tax	1812	1894	4.33 %	11.18 %
	Net wealth	1791	1891	5.29 %	12.21 %

Notes: Small MSPE refers to the pre-intervention MSPE of the treated-synthetic control pairs being smaller than the average value of the pre-treatment outcome variable of all treated taxpayers. Last column shows the number of units without a valid synthetic control group based on the total number of individuals. *Non-JKT only* = Donor pool includes only non-Jakarta residing taxpayers. *All IDN* = Donor pool includes both Jakarta and non-Jakarta residing taxpayers. *w/ demo.* and *w/o demo.* = demographic variables (age, marital status, and number of dependents) were included or excluded in the list of predictors, respectively.

Table A7
Average treatment effects on the treated (ATETs) from difference-in-differences method.

Outcome variable	Coefficient	Std. err.	*t*-stat.	*p*-val.	95 % % CIs	$N_{Treated}$	$N_{Control}$	Obs.
Earned Income	0.725***	0.205	3.54	<0.001	[0.324–1.127]	793	3269	26,059
Taxable Income	0.628***	0.204	3.08	0.002	[0.228–1.027]	793	3269	26,059
Income Tax	0.152**	0.061	2.48	0.013	[0.032–0.272]	793	3269	26,059
Net Wealth	3.229	8.847	0.37	0.715	[−14.12–20.57]	792	3269	24,692
S.E. clustered at the provincial level								
Earned Income	0.725***	0.051	14.18	<0.001	[0.62–0.831]	793	3269	26,059
Taxable Income	0.628***	0.048	13.05	<0.001	[0.529–0.727]	793	3269	26,059
Income Tax	0.152***	0.013	11.42	<0.001	[0.125–0.180]	793	3269	26,059
Net Wealth	3.229	5.547	0.58	0.566	[−8.19–14.65]	792	3269	24,692

Notes: Robust standard errors clustered at individual levels (rows 1–4) and provincial levels (rows 5–8). Control groups are identified by the SCM (main result) as donors. All specifications include age, marital, dependant status, individual and year fixed effects as control variables. ** and *** denote statistical significance at the 5 % and 1 % levels, respectively.

Table A8
P-values of average treatment effect (ATET) derived from placebo effects distribution.

Config.	Outcome variable	p-value
(Main)	Earn. Inc.	0.073*
Non-JKT	Tax. inc.	0.083*
w/ demo.	Inc. tax	0.081*
	Net wealth	0.116
Non-JKT	Earn. Inc.	0.068*
w/o demo.	Tax. inc.	0.082*
	Inc. tax	0.077*
	Net wealth	0.254
All IDN	Earn. Inc.	0.077*
w/ demo.	Tax. inc.	0.093*
	Inc. tax	0.081*
	Net wealth	0.114
All IDN	Earn. Inc.	0.090*
w/o demo.	Tax. inc.	0.114
	Inc. tax	0.092*
	Net wealth	0.185

Notes: The average treatment effects (post-treatment counterfactual differences) are compared to the distribution of the placebo SCM with the same configuration, e.g., same outcome variable of interest, whether demographics are included in the list of predictors, and whether only non-Jakarta or all non-treated taxpayers were included in the donor pool.

* denote statistical significance at the 10 % level.

Table A9
Yearly post-treatment counterfactual difference (treatment effects on the treated).

Year	Config.	Earned income			Taxable income			Income tax			Net wealth		
		Post-treatment	Post-pre treatment	Placebo *p*-value	Post-treatment	Post-pre Treatment	Placebo *p*-value	Post-treatment	Post-pre Treatment	Placebo *p*-value	Post-treatment	Post-pre treatment	Placebo *p*-value
2009	1	1.21***	1.07**	0.027**	1.08**	0.94**	0.029**	0.35**	0.31**	0.027**	−5.61	−5.87*	0.925
	2	1.13***	1.07***	0.028**	1.00**	0.93**	0.033**	0.33***	0.31**	0.027**	−10.39**	−9.10***	0.945
	3	1.16***	1.08***	0.032**	1.03**	0.95**	0.037**	0.34***	0.32**	0.031**	2.84	2.96	0.088*
	4	1.04**	1.02***	0.033**	0.91**	0.88**	0.040**	0.32***	0.31***	0.031**	−0.32	1.09	0.588
2010	1	0.45***	0.31**	0.114	0.39**	0.25*	0.133	0.12**	0.08**	0.118	−10.24	−10.5	0.94
	2	0.55***	0.49***	0.088*	0.47**	0.40**	0.106	0.15**	0.13***	0.093*	−12.81**	−11.52**	0.941
	3	0.38**	0.31**	0.135	0.31*	0.24*	0.161	0.11**	0.09**	0.135	−3.47	−3.35	0.814
	4	0.32	0.30*	0.153	0.23	0.2	0.188	0.1*	0.09*	0.14	−8.81*	−7.40*	0.915
2011	1	1.06***	0.92***	0.050**	1.01***	0.87***	0.054*	0.17	0.13*	0.1*	26.89***	26.63***	0.038**
	2	1.02**	0.96***	0.054*	0.93**	0.86***	0.062*	0.11	0.09	0.148	32.76***	34.04***	0.032**
	3	0.92**	0.85***	0.068*	0.79*	0.71***	0.080*	0.22*	0.20***	0.084*	19.32***	19.44***	0.050**
	4	0.82*	0.81***	0.070*	0.63	0.61**	0.097*	0.22*	0.21***	0.081*	25.25***	26.65***	0.040**
2012	1	0.2	0.06	0.275	0.09	−0.05	0.346	0.04	0.00	0.298	32.78	32.53	0.041**
	2	0.26	0.21	0.227	0.15	0.08	0.306	0.08	0.06	0.208	27.77	29.06	0.043**
	3	0.48**	0.40**	0.145	0.33	0.25	0.189	0.14**	0.12**	0.147	17.56	17.68	0.058*
	4	0.18	0.16	0.26	0.01	−0.02	0.388	0.05	0.04	0.264	13.07	14.48	0.062*

Notes: Configuration 1 = main result (non-Jakarta control); Configuration 2 = exclude demographics from the list of predictors; Configurations 3 and 4 are the same as 1 and 2, respectively, with Jakarta sample included in the SCM donor pool. *, **, and *** denote statistical significance at the 10 %, 5 %, and 1 % levels, respectively, based on two-tailed *t*-test. The column Placebo *p*-value shows the p-value of the yearly post-treatment counterfactual difference derived from the placebo effects distribution.

Table A10
Robustness checks on potential delayed impact of the GFC on the treatment group.

Outcome variable	$Year_{Treated}$	$Year_{SC}$	$M_{Treated}$	$SD_{Treated}$	M_{SC}	SD_{SC}	t-stat.	p-val.
Earned Income	2010	2009	2.30	2.83	1.75	1.67	7.03***	<0.001
	2011	2010	2.86	5.55	2.01	3.77	2.62***	0.009
	2012	2011	2.92	4.07	2.07	2.33	4.47***	<0.001
Taxable Income	2010	2009	2.22	2.83	1.72	1.67	6.26***	<0.001
	2011	2010	2.79	5.56	1.98	3.76	2.49**	0.013
	2012	2011	2.85	4.08	2.04	2.32	4.22***	<0.001
Income Tax	2010	2009	0.61	0.83	0.46	0.48	6.76***	<0.001
	2011	2010	0.77	1.62	0.54	1.13	2.33**	0.020
	2012	2011	0.79	1.18	0.68	1.02	1.77*	0.077
Net Wealth	2010	2009	63.38	86.32	60.14	59.68	1.15	0.252
	2011	2010	82.65	100.16	61.47	66.31	4.36***	<0.001
	2012	2011	94.05	115.83	56.86	60.21	6.16***	<0.001

Notes: Two-sample *t*-test on the post-treatment differences between the treated and synthetic control groups, taking into account a one-year lag for the latter. *, **, and *** denote statistical significance at the 10 % and 5 % levels, respectively.

Table A11
Pre- and post-treatment average treated-synthetic control difference and post-pre differences for subsample of HWIs with matching results not affected by missing observations.

Outcome variable	Config.	N	Pre-treatment	Post-treatment	Post-pre difference	*t*-stat.
Earned income	1	829	0.104	0.614***	0.510***	−3.48
	2	829	0.018	0.568***	0.550***	−3.93
	3	829	0.026	0.536***	0.510***	−3.53
	4	829	0.001	0.503***	0.503***	−3.55
Taxable income	1	829	0.104	0.511***	0.406***	−2.82
	2	829	0.021	0.453***	0.432***	−3.12
	3	829	0.028	0.447***	0.419***	−2.94
	4	829	0.002	0.397**	0.394***	−2.83
Income tax	1	829	0.036	0.168***	0.131***	−2.98
	2	829	0.007	0.140***	0.132***	−3.17
	3	829	0.009	0.157***	0.148***	−3.44
	4	829	0.001	0.144***	0.144***	−3.38
Net wealth	1	829	0.061	4.124	4.063	−0.59
	2	829	−1.024	2.269	3.293	−0.51
	3	829	−0.499	2.303	2.801	−0.42
	4	829	−1.29	1.816	3.106	−0.49

Notes: Earned income, income tax, taxable income and net wealth in billion Indonesian rupiah (IDR). Configuration 1 = donor pool restricted to non-Jakarta taxpayers, with demographic variables included in the list of predictors (main result); Configuration 2 = matching only on pre-treatment

income and wealth related variables, with donor pool restricted to non-Jakarta taxpayers; Configurations 3 and 4 are the same as 1 and 2, respectively but the donor pool was not restricted. ** and *** denote statistical significance at the 5 % and 1 % levels, respectively.

Table A12
Estimates of intention-to-treat and spillover effect on non-treated Jakarta HWIs.

Outcome variable	Config.	N	Post-treatment	Post-pre difference	*t*-stat.	Placebo *p*-value
Intention-to-treat						
Earned income	1	1650	0.668***	0.529***	−4.61	0.071*
	2	1817	0.575***	0.525***	−4.35	0.075*
Taxable income	1	1650	0.610***	0.471***	−4.12	0.076*
	2	1822	0.499***	0.436***	−3.6	0.088*
Income tax	1	1628	0.178***	0.137***	−3.95	0.075*
	2	1812	0.133***	0.114***	−3.13	0.090*
Net wealth	1	1604	3.951	3.595	−0.96	0.115
	2	1791	0.556	1.343	−0.39	0.759
Spillover effect						
Earned income	1	734	0.629***	0.492***	−3.70	0.083*
	2	792	0.377**	0.335***	−2.71	0.116
Taxable income	1	734	0.615***	0.477***	−3.59	0.084*
	2	792	0.340**	0.288**	−2.24	0.126
Income tax	1	724	0.173***	0.131***	−3.28	0.086*
	2	789	0.058	0.042	−1.10	0.159
Net wealth	1	740	4.698*	4.226*	−1.88	0.112
	2	806	0.8	0.971	−0.35	0.267

Notes: Configurations 1 and 2 include and exclude demographics from the list of predictors, respectively. *, **, and *** denote statistical significance at the 10 % and 5 % levels, respectively. Column Placebo *p*-value indicates the *p*-values of the average treatment effect derived from the placebo effects distribution.

Table A13
Estimates of spillover effect on non-eligible Jakarta taxpayers.

Outcome variable	Config.	N	Post-treatment	Post-pre difference	*t*-stat.	Placebo *p*-value	% diff. between Treated–SC in Post-treatment
Above pre-treatment net wealth threshold (>10 billion IDR)							
Earned income	1	733	0.068***	0.067***	−3.19	0.319	9.95 %
	2	756	0.065***	0.065***	−3.29	0.311	9.50 %
Taxable income	1	733	0.068***	0.070***	−3.37	0.336	10.36 %
	2	756	0.066***	0.068***	−3.42	0.334	9.99 %
Income tax	1	733	0.018***	0.018***	−3.05	0.334	11.80 %
	2	756	0.015**	0.015***	−2.64	0.332	9.77 %
Above pre-treatment taxable income threshold (>1 billion IDR)							
Earned income	1	2992	0.19	0.115	−0.94	0.219	11.18 %
	2	3061	0.229*	0.194	−1.6	0.191	13.33 %
Taxable income	1	2991	0.2	0.12	−0.98	0.22	11.98 %
	2	3061	0.220*	0.178	−1.47	0.201	12.91 %
Income tax	1	2982	0.026**	0.001	−0.13	0.303	5.84 %
	2	3048	0.037***	0.026**	−2.51	0.268	8.20 %
Below both thresholds							
Earned income	1	2891	−0.059***	−0.058***	5.94	0.47	−9.14 %
	2	2920	−0.072***	−0.070***	6.25	0.474	−11.04 %
Taxable income	1	2895	−0.034***	−0.034***	3.54	0.476	−5.67 %
	2	2931	−0.038***	−0.035***	3.13	0.469	−6.30 %
Income tax	1	2888	−0.008**	−0.008***	2.86	0.458	−5.68 %
	2	2912	−0.010***	−0.009***	3.02	0.453	−7.15 %

Notes: Configurations 1 and 2 include and exclude demographics from the list of predictors, respectively. *, **, and *** denote statistical significance at the 10 % and 5 % levels, respectively. Column Placebo *p*-value indicates the *p*-values of the average treatment effect derived from the placebo effects distribution.

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