Pecuniary and Non-Pecuniary Motivations for Tax Compliance: Evidence from Pakistan

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Abstract

We examine two Pakistani programs to explore the extent to which deterrence, as well as social and psychological factors, play a meaningful role in the compliance behavior of agents. In the first of these programs, the government began publishing online the income tax liability reported by every taxpayer in the country. The second program publicly recognizes and rewards the top 100 tax paying corporations, partnerships, self-employed individuals, and wage-earners in the country. We find that both programs induced a significant compliance response. The public disclosure caused a 10 log point increase in the tax liability reported by agents exposed to the program. The social recognition of top taxpayers caused a further 15 log point increase in the tax liability reported by the treated agents.

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I Introduction

Tax evasion, a pervasive problem in developing countries and a non-trivial one in developed countries, is constrained by the threat of detection and punishment, as highlighted in the canonical deterrence model of tax evasion due to Allingham & Sandmo (1972). It may also be constrained by social and psychological factors (Andreoni *et al.*, 1998). Some individuals may feel guilt or shame from evading or pride from fulfilling their civic duty, while others may be influenced by peer behavior and the possibility of approval or sanctions from peers (Luttmer & Singhal, 2014). Similarly, individuals may have intrinsic motivation to pay taxes (Dwenger *et al.*, 2016). While the existence of these social and psychological factors is increasingly being recognized, there is still little empirical work on the relative importance of the pecuniary and non-pecuniary constraints on evasion and the extent to which governments can prime them for resource mobilization.

In this paper, we examine the consequences of two Pakistani programs to study these issues. In the first of these programs, the government began revealing the amount of income tax liability reported by every taxpayer in the country. The public disclosure program was instigated by a series of press reports documenting that the majority of lawmakers of the country had not been fulfilling their tax obligations. It began in tax year 2012 and has continued since then. Each year, two tax directories are published, one for the Members of Parliament (MPs) and one for all taxpayers. The directories are available online in a searchable PDF format and can be downloaded freely by anyone. The directory for general taxpayers reveals the name, a numerical tax identifier, and the tax liability reported by each taxpayer. The directory for MPs also lists the constituency they serve.

The second program we examine publicly recognizes and rewards top taxpayers of the country. The Taxpayers Privileges and Honour Card (TPHC) program began concurrently with the public disclosure program. It acknowledges the top 100 taxpayers in each of four categories—self-employed individuals, wage-earners, partnerships, and corporations—and grants them certain privileges. The Honour Card holders are invited to a special ceremony hosted by the Prime Minister each year to "recognize their services to the nation," as well as to the State Dinner held on the Pakistan and Republic Day. In addition, they are eligible for benefits such as fast-track immigration and gratis passports. The personal benefits of the program

are conferred on the partner with the maximum capital contribution in case of a partnership and on the CEO in case of a corporation.

These programs can influence tax compliance through a number of channels. The disclosed information can expose an agent as a tax cheat if the tax payment does not conform with the level of income, consumption, or wealth observed by neighbors, friends, and other peer networks. The information can encourage whistleblowers to come forward, increasing the expected costs of evasion through the conventional channel of the deterrence framework. The shame and guilt resulting from the disclosure can also induce greater tax compliance. On the other hand, the programs may stimulate feelings of pride and positive self-image if one is revealed to be a compliant or top taxpayer. The programs also let agents signal their types. Some individuals may obtain higher utility from the public appreciation of their level of affluence (Akerlof & Kranton, 2000; Glazer & Konrad, 1996), while others may fear that wide knowledge of their affluence could attract unwarranted and even dangerous attention, as well as pleas from relations and peers to share their affluence. And some agents may monetize the goodwill offered by the programs, translating the social recognition into higher sales and profits.

The tax directory, as we note above, lists the name and a numerical identifier of each taxpayer that is effectively private information. Thus, the only publicly-disclosed information that can link an observation in the directory to a particular taxpayer is the name. Pakistani names do not follow the standard Western syntax of given name+middle name+surname. Instead, a typical Pakistani name is composed of two or more given names. One of these given names—usually the most-called name of the father or husband—serves as the surname. Surnames in this way are usually not fixed across generations and vary even within the nuclear family. Because of these naming conventions, it is quite common for people to have the same full name. For example, the most frequent name in our data, Muhammad Aslam, appears 15,598 times in four years, with a typical year's directory containing more than 60 pages listing the name Muhammad Aslam alone. On the other hand, about one-third of taxpayers have unique names. This variation in name commonness implies that the intensity of the disclosure varies considerably across individuals depending upon how common their name is. Taxpayers with very frequent names enjoy virtual anonymity in the disclosed records; uniquely-named taxpayers, on the other hand, are exposed perfectly. We exploit this variation in treatment

intensity in our empirical strategy, comparing the change in tax payments across taxpayers with frequent and unique names.

Of course, names are not randomly assigned. Instead, they are chosen by parents and hence may be correlated with parental traits such as income, education, and ethnicity. We always include individual fixed effects in our empirical models, implying that parental traits will influence our estimates only if their effect changes over time, in particular contemporaneously with the programs. We provide two sets of tests to rule out this and related concerns. First, we show through visual and regression-based evidence that the tax payments of the compared groups were trending similarly in the pre-program period: the relative difference in the outcome was indistinguishable from zero for a number of pre-program years. Second, we show that the name of a taxpayer bears no association with the outcome in the sample of taxpayers (MPs) where the disclosure intensity is independent of the name commonness.

The TPHC program applies only to the top 100 taxpayers of each category. We leverage this discontinuity in program eligibility to estimate its impacts. If social recognition and related benefits offered by the program are valued, taxpayers close to the eligibility cutoff will increase their tax payments in order to remain in, or enter into, the top 100 club. We test this by comparing the yearly growth in tax liability reported by agents close to the cutoff with other top taxpayers. To show that our estimates are not driven by factors unrelated to the program such as rising inequality at the top, we run placebo regressions estimating the program effects in pre-intervention periods and on unaffected groups.

We combine the disclosed data of the years 2012-2015 with administrative tax return data from 2006-2012 to create a long panel of tax records from 2006 to 2015. We document four key findings. First, the exposure of tax information induced a substantial response from the treated taxpayers. The tax liability reported by taxpayers with less common names on average increased by around 10 log points as a result of the program. Consistent with our expectations, the estimated effect varies directly with the program intensity. It is strongest in the left-tail of the name-frequency distribution, declines monotonically as we move rightward, and becomes insignificant as the name-frequency approaches 300 (i.e., the name of the taxpayer appears at least 300 times in the four years of disclosed data). Second, the disclosure had a far stronger impact on MPs. Their tax filing rate jumped up by around 60 percent-

age points, increasing from around 30% to more than 90%. Along the intensive margin, their reported tax liability surged by more than 40 log points. The stronger response from MPs is not surprising, as they are likely to be more sensitive to the revealed information because in addition to inducing shame and guilt it can reduce their re-election probability. The disclosure was also more salient for them. They were explicitly identified in the disclosed records through their constituency numbers, and the media was likely to pick on their noncompliance. Third, the TPHC program also had a large impact. In a sample containing top 1000 taxpayers of each category, the tax liability reported by 70-130 ranked taxpayers grew by nearly 18 log points faster than others as a result of the program. This estimate declines slightly as we widen the treatment window, suggesting that, as hypothesized, the effect is concentrated around the eligibility cutoff of the program. Finally, we document that our estimates are highly robust to alternative specifications and the identification concerns noted above.

Our results have important policy implications. The programs we study cost little in terms of economic resources. Thus, if they are effective, they potentially offer a cost-effective complement to the standard measures that governments undertake to deter tax evasion such as audits and information reporting requirements. Of course, any such policy needs to balance the pro-social impacts against concerns such as privacy.[1]

This paper contributes to a small but growing literature that assesses the influence of factors both within and outside the standard expected utility framework on tax compliance (see Slemrod, Forthcoming and Luttmer & Singhal, 2014 for surveys). More specifically, Hasegawa *et al.* (2012), Bø *et al.* (2015), and Hoopes *et al.* (Forthcoming) study the impact of public disclosure on tax compliance in Japan, Norway, and Australia, respectively. The studies of the Australian and Japanese programs, which both had thresholds, revealed that some individuals and businesses take actions to avoid disclosure, but no evidence was found that the programs enhanced compliance. In Norway, though, a novel identification strategy suggested that increasing the ease of access to the tax data via the Internet significantly increased reported self-employment income. Dwenger *et al.* (2016) conduct a field experiment in Germany to assess the importance of intrinsic motivation in tax

[1]Issues created by the public disclosure of tax payments are discussed at more length in Lenter *et al.* (2003); Blank (2014).

compliance. In addition, a strand of literature runs field/lab experiments to study social motivations in tax payments (see, for example, Slemrod *et al.*, 2001 and Fellner *et al.*, 2013). A few of these experiments employ treatment arms that reward taxpayers for compliant behavior, but we are not aware of a national program like Pakistan's that has been heretofore studied.

Remarkably, all the aforementioned studies have developed-country settings. Taxation capacity in developing countries is limited and evasion pervasive, and it is likely that collective and individual attitudes toward evasion hence would not be the same there as in developed countries. While a robust public finance literature is emerging in developing countries (see, for example, Kleven & Waseem, 2013; Pomeranz, 2015; Waseem, 2018a,b), to our knowledge there still does not exist any study of social and psychological motivations in tax payments from a developing country perspective.

The Pakistani public tax disclosure program has been studied in one recent political science paper Malik (2017) which investigates its impact on the tax reporting behavior of MPs. She uses two years' publicly available data to assess if MPs in more competitive races respond more aggressively to the program than others and similar political economy questions. As we note above, the primary focus of our paper is the universe of tax filers and is not limited to MPs. Even for the subsample of MPs, our analysis differs substantially from Malik's in both focus and methodology. We analyze a long panel spanning ten years, include both individual and time fixed effects in our empirical models, employ a novel identification strategy based on name commonness, and study a distinct set of questions.

II Context

In this section, we describe features of the Pakistani environment that are important for our empirical analysis.

II.A Public Disclosure Program

In the first of two programs we study, the Pakistani government started publishing a tax directory each year, revealing reported income tax liability, including final withholding tax, of every taxpayer in the country. The policy change (in large part)

was instigated by a string of investigative reports that began appearing in the Pakistani press in the latter half of 2012. The reports focused primarily on the tax affairs of lawmakers of the country, documenting that a majority of them had apparently not been fulfilling their tax obligations. Combining data leaked by whistle-blowers with the official data obtained through the Election Commission of Pakistan, the reports painted quite a bleak picture of tax compliance among the MPs of the country. It was reported that around 66% of them—including 34 out of 55 federal ministers—had not filed their tax return for the latest year; in fact, about 20% of them had not even obtained the National Tax Number, which is the first requirement for tax filing (Center for Investigative Reporting in Pakistan, 2012). These revelations, compiled into two papers published by the Center of Investigative Reporting in Pakistan (CIRP), generated strong reaction. The Federal Tax Ombudsman, upon a representation filed by a citizen, ordered the government to begin disclosing the tax remitted by every public office holder in the country. The leading opposition party at the time went even further, pledging to publish the amount of tax remitted by all taxpayers in the country if elected to power. This party won the next elections and formed the federal government in May 2013. It fulfilled its election promise and began publishing the tax records for the tax year 2012 onward, which were due to be filed by December 15, 2013.[2]

Since the institution of the program in 2012, two tax directories are published each year, one for MPs and the other for all taxpayers. These directories are posted online on the Federal Board of Revenue (FBR)'s website in a searchable PDF format.[3] They can also be downloaded freely by anyone. The directory for general taxpayers reveals the name, tax identifier, and tax liability of each taxpayer. This information—sorted alphabetically on the full name —is provided separately for companies, partnerships, and individuals. The tax identifier is either the nine-digit National Tax Number (NTN), disclosed with the tax year 2012 data, or the 13-digit Computerized National Identity Card Number (CNIC), disclosed withe 2013 tax year data and thereafter, both of which are effectively private information of agents.[4] Therefore, the only information through which an observation in the

[2]The Pakistani tax year runs from July to June. Any year t in this paper denotes the tax year from July t to June $t+1$.

[3]In fact, the title page of the directory contains the following direction in a very salient yellow box: "Please press CTRL + F Key to Search the Record".

[4]The NTN is used exclusively for tax filing. The CNIC is the primary identification document

directory can be readily linked to a taxpayer is the name.[5] In contrast, the directory of parliamentarians also contains the constituency number an MP serves and therefore the disclosed information can be linked to them fairly easily.

Table I lists important events in the public disclosure program. The timing of these events is important for our empirical analysis, in particular in deciding in which period the program would begin affecting behavior. As we note above, the political party committed to the full public disclosure had come into power in May 2013. The last date for filing the 2012 tax return was December 15, 2013. Thus, by the time the 2012 returns were filed, it was clear that the tax remitted through them would be made public. We accordingly treat tax year 2012 (which covers July 2012 - June 2013) as the first post-program year in our analysis. Although the exact format of the disclosure was not known at the time, it was clear that it would, at a minimum, include the name of the taxpayer. The name is a primary, and to some extent the only, information through which the public can link a tax return to a taxpayer, and therefore there could be no meaningful disclosure without it.[6]

As we note above, the MPs' directory also contains the constituency number they serve. Table A.III reports the composition of the Pakistani legislature. Because the country has a limited number of MPs, their identities are well known, especially in their election districts. They are also identified in the disclosed data through their constituency numbers. Their exposure to the program therefore must be independent of how common their name is. We use this feature of the program

and proof of citizenship in Pakistan. It is required for most official services including obtaining a passport, driving license, utility connection, opening and operating bank accounts. The NTN was issued sequentially beginning in 1995, so the number reveals some information about how long a taxpayer has been in the tax net. The first few digits of the CNIC indicate in which district (of 128 in Pakistan) a taxpayer resided at the time of initial registration.

[5]FBR provides an online taxpayer verification service through which tax identifiers can be used to obtain additional taxpayer information, namely address (at the time of registration), registration date and regional tax office. This additional information may improve the chances of linking an observation in the directory to a taxpayer, but there is a significant effort cost associated with obtaining the information. The tax identifiers of all taxpayers with a particular name would have to be manually entered one at a time to obtain the additional information and online security features prevent the process from being automated. Therefore, the cost of obtaining the information increases with the commonness of the taxpayer's name: the more common a name, the more observations have to be individually verified. In addition, the additional information may still not be sufficient to identify an individual. A taxpayer's address may have changed since they first registered for an NTN or it may not be public information. The effective disclosure intensity therefore is still linked primarily to the taxpayer's name.

[6]The CIRP reports that precipitated the full public disclosure program always used the name as the primary identifier of a taxpayer.

as a specification check on our empirical strategy.

II.B Taxpayer Privileges and Honour Card Program

The second program we examine is the Taxpayer Privileges and Honour Card (TPHC) scheme. The program was announced at the beginning of the tax year 2012,in July 2012. It acknowledges and grants special privileges to the top 100 taxpayers in each of the following four categories: (a) salaried individuals, (b) nonsalaried individuals, (c) partnerships, and (d) corporations. The special privileges granted by the program include: (1) automatic invitation to the Annual Excellence Awards hosted by the Prime Minister; (2) automatic invitation to the State Dinner held on Pakistan Day (23rd March) and Independence Day (14th August); (3) fast-track immigration through special counters (Figure A.II provides a photograph of such an immigration counter at the Lahore airport); (4) issuance of gratis passports; (5) access to VIP lounges at Pakistani airports; and (6) an increased baggage allowance. These privileges last one complete year, until the new set of recipients are announced. The personal benefits of the program are conferred on the partner with the highest capital contribution in the case of partnerships, and on the CEO in case of corporations.

Two features of the program need emphasizing. First, while the principal element of the program is to honor and recognize top taxpayers,[7] it provides some material benefits as well. To the extent that these benefits are valued, the response to the program would also reflect the willingness to pay of top taxpayers for these benefits. Second, the program has some overlap with the public disclosure, as the latter also identifies top taxpayers, albeit indirectly. In fact, most of the news items that report on the public disclosure program focus on who are the top taxpayers in the disclosed data. This media recognition, however, is indirect, usually limited to the very top taxpayers (say top 10), and is not as salient or meaningful as one offered by the TPHC program. But to the extent that the two programs overlap, our estimates will capture the combined effects of the two.

[7] Addressing the first batch of the Honour Card recipients, the Prime Minister said that the "ceremony has been convened to acknowledge your services for the nation."

II.C Pakistani Naming Conventions

Pakistani names generally do not conform to the standard Western syntax of given name + middle name + surname. Instead, a typical Pakistani name consists of one or more given names and a surname. The given names are usually derived from Persian, Arabic, or Turkish, and it is quite common for people to have more than one given name. If a person has two or more given names, the less common one serves as the *most-called* name (the person is informally referred to by this given name). For example, if Muhammad is one of the multiple given names, it is usually not the person's most-called name, as being so common it does not serve as a useful identifier. Unlike the Western practice, surnames in Pakistan are usually not fixed across generations. The most popular convention is to adopt the most-called given name of father (husband) as the child (married woman's) surname. As a result, surnames vary even within the nuclear family (father/husband has a different surname). In cases where the surname does not vary within the family, it is rarely unique. For example, virtually all people of Pashtun origin use Khan as their surname.

Because of these conventions, many full names are widely shared in Pakistan. Figure I illustrates this formally. We plot the distribution of full names contained in the public disclosure data for the tax years 2012-2015. To construct the diagram, we treat all English variants of an Urdu name as one. For example, Muhammad spelled as Mohammad, Muhammed or Mohammed is treated as one name (to an Urdu speaker, they would be indistinguishable). To show that cleaning these spelling variations does not change our results materially, we provide the corresponding raw distributions in Figure A.I (the details of our cleaning algorithm are presented in Appendix A.1). A total of 526,425 unique names appear in the publicly disclosed data during the four years. Of these, Muhammad Aslam is the most frequent, appearing 15,598 times. Because a single page of the directory on average consists of 60 rows, a given year's directory contains about 65 (15,598/(4*60)) pages listing the name Muhammad Aslam alone. There are other such very frequent names. In fact, nearly one-third of taxpayers share their full name with at least 500 others. The distribution has a thick tail at the other end as well. Approximately 35% of taxpayers have names that appear fewer than ten times in the four years of data; about 4% appear only once, while 24% of names appear between 2-5 times.

As we note above, the directory carries no publicly-known identifier other than the name. The wide variation in name frequency thus translates into a wide variation in the effective intensity of disclosure. Note that we do not expect, and do not assume, that taxpayers know precisely how common their name is. However, persons with very frequent names such as Muhammad Aslam would very likely have come across numerous other people of the same name in their lives and would have—through a conscious or subconscious process—formed a belief that their name grants virtual anonymity to them. On the other hand, unique-named individuals would likely have a sense that any information with their name on it can be linked to them directly. Once the public disclosure lists became available, it was straightforward to acquire more concrete information about how common one's name is.

II.D Structure of Pakistani Legislature

Pakistan is a federation composed of a center and four provinces. The federal legislature, called the Parliament, consists of two houses: the National Assembly and Senate. The National Assembly has 342 seats, of which 272 are directly elected through a first-past-the-post system. These directly-elected seats are divided between the provinces on the basis of their population in the latest census. The other 70 seats are reserved for women and religious minorities. The reserved seats are filled through the proportional representation system based on the party position in the national and four provincial assemblies. The Senate gives equal representation to all four provinces. It has 104 seats, all of which are filled through the proportional representation system described above. Provincial legislatures are unicameral, structured similarly to the National Assembly. Table A.III shows the composition of the Pakistani legislature. Members of the national and four provincial assemblies are elected for five years. The last four general elections were held in July 2018, May 2013, February 2008, and October 2002. Senators, on the other hand, are elected for six years in a three-year election cycle. There are no term limits on any house membership, and MPs can continue standing for reelection as long as they chose to.

II.E Data

We use data from three different sources for our empirical analysis. First, we access the public disclosure data from the FBR's website. As we note above, this data set contains the name, numerical identifier, and tax paid by every taxpayer in Pakistan for the tax years 2012-2015. The data set for MPs includes the additional identifier of the constituency number. Second, we utilize administrative tax return data from the FBR. We have access to this data for the tax years 2006 to 2012 only (the FBR stopped providing researchers access to the data after that). The administrative data contains all the line items in the tax return form. It also includes a few taxpayer characteristics such as name, tax identifier, type (company, partnership, self-employed, wage-earner), and date of registration. Combining the two sources of data, we are able to construct a panel of all taxpayers in Pakistan from 2006 to 2015.

Pakistan runs an elaborate system of what is called tax withholding. A tax remittance responsibility is triggered by a number of transactions including wage payments. For some of such transactions (not including, e.g., employer withholding), the withheld tax is treated as the final discharge of liability. For example, income tax at the rate of 1% of the value is owed on all export transactions. The remittance is due at the time the payment is received and the withheld tax is deemed as the final discharge of liability: the taxpayer does not include income from the transaction in computing taxable income, nor is he or she allowed any refund or credit for the withheld tax. Tax payments reported in the disclosure data are the sum of the tax paid on taxable income and the final tax paid. We observe both these types of tax paid in the administrative data, and are thus able to construct a consistently-defined variable that captures tax payment of each taxpayer in all years included in the panel.

For our analysis of MPs' behavioral responses, we collect data on all elections held during the 2013-2018 election cycle of Pakistan. This data set include variables such as the date of election, type of constituency (reserved or directly contested), total votes cast, votes obtained, and party affiliations. We collect this data from the websites of the Election Commission of Pakistan, National Assembly, Senate, and the four Provincial Assemblies of the country.

III Conceptual Framework

III.A Social and Psychological Motivations in Tax Compliance

Economists have traditionally modeled tax evasion as if it were a choice under uncertainty (Allingham & Sandmo, 1972). Successful evasion provides additional disposable income, but evasion also entails the risk that the evaded amount will be recovered along with penalty in case of detection. Assume a taxpayer earns real income z but reports $\underline{z} \leq z$ with $e \equiv z - \underline{z}$, paying a tax $T \equiv \tau(z - e)$. The taxpayer perceives that evasion will be detected with probability p, triggering a proportional penalty of θ applied to the evaded income upon detection. The taxpayer chooses e to maximize the expected utility of the gamble denoted by

$$\max_{e} \quad (1-p)\,.u\left[(1-\tau)z + \tau e\right] + p.u\left[(1-\tau)z - \theta e\right]. \tag{1}$$

In this model evasion is deterred solely by the fear of penalty. A risk-averse taxpayer balances the disutility of income loss in the detected and penalized state against the utility of extra income in the undetected state.

$$\frac{u'(c_A)}{u'(c_{NA})} = \frac{(1-p)\,\tau}{p\theta}, \tag{2}$$

where c_A and c_{NA} denote consumption in the detected and undetected states.

The model has been criticized for its lack of realism. Indeed, if one measures the probability of detection and punishment by the average audit rate, one would have to assume implausibly large levels of risk aversion to fit the model to data about observed levels of evasion. Waseem (2018c), for example, estimates the compliance rate of self-reported income in Pakistan to be around 50%, considerably larger than the 13% compliance rate predicted by the model for a plausible estimate of the coefficient of relative risk aversion ($\gamma = 3$).[8]

However, proxying the detection probability by the fraction of returns audited by a tax authority ignores that audit is not a scatter gun operation. Instead, tax authorities use often quite sophisticated selection models to target their audits. These

[8] See the calibrations in Alm *et al.* (1992) and related discussion in Luttmer & Singhal (2014).

models are helped by information reports from sources such as employers and financial institutions. Large scale cross-matching allowed by these reports means that the detection probability faced by taxpayers on income covered by third-party reports can be close to one even if only a small percentage of tax returns are actually audited (Slemrod, 2007; Kleven *et al.*, 2011). Incorporating this feature of the environment improves the deterrence model's fit considerably. In fact, it is able to explain the first-order pattern of evasion across income from various sources, notably that the noncompliance rate of employee income is considerably lower than that for self-employment income, estimated in the United States to be 1% and 63%, respectively.

The Allingham-Sandmo deterrence model does not, though, explain all aspects of tax evasion, and does not take into account social and psychological factors.[9] These factors can be divided into three classes. First, there are factors that reduce utility in both states of the world. Guilt, for example, may cause psychological and emotional distress to a tax cheat even if the act of cheating remains undetected. Second are factors such as shame that reduce utility only if cheating gets detected (Erard & Feinstein, 1994). And, third, there may be behavioral biases if the detection probability and penalty are systematically mis-estimated by taxpayers (Scholz & Pinney, 1995; Chetty, 2009). The simplest manner in which these factors can be incorporated into the model is to rewrite the maximization problem as follows:

$$\max_{e} \quad (1-p)\,.u\left[(1-\tau)z+\tau e-ge\right]+p.u\left[(1-\tau)z-\theta e-ge-se\right]. \tag{3}$$

Here, g (denoting guilt) and s (denoting shame) represent the moral costs of evasion. For simplicity, we introduce these costs as proportional terms, but the results are robust to plausible functional forms. The FOC of the extended model is:

$$\frac{u'(c_A)}{u'(c_{NA})}=\frac{(1-p)\,(\tau-g)}{p(\theta+g+s)}. \tag{4}$$

Intuitively, s enters the problem in a similar way as the pecuniary penalty θ; on the other hand, g acts like a negative tax rate in the undetected state—reducing the benefit of evasion—and like a penalty in the audited state. The detection probabil-

[9]For example, in an influential survey of the tax compliance literature, Andreoni *et al.* (1998) write that "factors such as a moral obligation to be truthful, or the social consequences of being a known cheater, may add further enforcement incentives that are not accounted for in our models."

ity p now reflects the behavioral biases. The comparative statics of the problem are straightforward. Evasion decreases with p, g, and s as long as the marginal utility of consumption is diminishing (risk aversion).[10]

The public disclosure program we examine potentially affects each of these three parameters. By facilitating whistle-blowing, it arguably raises the likelihood of detection perceived by agents. It may also intensify the guilt and shame felt by tax cheats, especially if reported income does not match consumption or wealth observed by peers. The three factors reinforce each other. We, therefore, expect the public disclosure program to reduce evasion and increase tax payments.

The other program we study (TPHC) promotes compliance as well. Social recognition of top taxpayers can induce pride and sense of accomplishment. Individuals may also treat taxation as a status (Veblen) good, deriving utility from being seen as one of the richest in the country (Akerlof & Kranton, 2000).[11] The goodwill offered by the TPHC program can be monetized too. Individuals and firms may advertise their status as a top taxpayer to gain more consumers and sales. Due to these mechanisms, the costs of evasion jump up at the eligibility cutoff of the program. The resulting notch will induce taxpayers to locate on the eligible side of the cutoff, increasing the tax paid by agents close to the cutoff. Working in the opposite direction, some taxpayers may place negative value on the attention the program provides.

III.B Empirical Strategy

We use difference-in-differences research designs to estimate the effects of the two programs on tax compliance. These designs are explained in greater detail below.

III.B.1 Public Disclosure Program

The public disclosure program was rolled out nationally, all at once. Therefore, the principal identification challenge in estimating its effects is to control for any trends or shocks that might affect tax reporting at the aggregate level and may coincide with the program. We achieve this by exploiting the variation in exposure to

[10] See Slemrod & Yitzhaki (2002) for details.

[11] It has been found that consuming goods associated with wealth provides utility to individuals even if the consumption remains invisible to others (Bursztyn *et al.*, 2018).

the program caused by the degree of uniqueness of a taxpayer's name. We define Name Frequency as the number of times a full name appears in the four years of the disclosed data. For example, the Name Frequency of the most frequent name in the data—Muhammad Aslam—is 15,598. Taking advantage of the observable differences in program intensity across taxpayers with different Name Frequency, we estimate regressions of the form

$$\text{(5)} \qquad \log \text{TaxPaid}_{it} = \alpha_i + \beta \text{ treat}_i \times \text{after}_t + \lambda_t + u_{it},$$

where α_i and λ_t are individual and year fixed effects, after$_t$ is a dummy indicating 2012 or a later year, and treat$_i$ is an indicator of the Name Frequency of individual i. We experiment with different Name Frequency cutoffs in our empirical specifications. The difference-in-differences (DD) coefficient of interest β captures the differential effect of the program, denoting the average additional tax paid in the post-program years by individuals with relatively low Name Frequency.

For β to have a causal interpretation, it must be shown that the interaction variable and the error terms are uncorrelated. Our treatment variable captures how unique a taxpayer's name is. But names are not randomly assigned. Instead, they are chosen by parents, perhaps with the help of close relatives and friends. Any measure of name uniqueness, therefore, could be correlated with parental traits such as income, education, and ethnicity. To control for such correlations, we always include individual fixed effects in our regressions. The parental traits, therefore, would influence our estimates only if their effect changes over time, in particular in 2012.

We offer three pieces of evidence to rule out this concern. First, exploiting the panel nature of data we show that there were no systematic differences between the compared groups in terms of their tax payments in the pre-program years. We show this through the following event-study regressions

$$\text{(6)} \qquad \log \text{TaxPaid}_{it} = \alpha_i + \sum_{j=2007}^{2015} \gamma_j \text{ treat}_i \times 1.(\text{year}{=}j)_t + \lambda_t + u_{it}.$$

The coefficients γ_js here capture the average difference in tax payment between the two groups in year j relative to the reference year 2006. For a variety of definitions

of treatment, we show that the estimated γ_js remain trivial/insignificant in the pre-program years but become large and significant in the post-program years. While validating our empirical strategy, these results do not expressly rule out a contemporaneous macro event that affects the tax payments of more-uniquely-named individuals. Note that in most difference-in-differences setups this assumption remains untested and is presumed satisfied if the preexisting trends are parallel. But in our setting we can go one step further than the parallel-trends assumption to rule out this possibility more directly. As we note above, MPs in Pakistan are prominent in their communities and their constituencies are listed in the directory. The effectiveness of the disclosure is therefore plausibly independent of how conspicuous or obscure their name is. We show that β remains statistically indistinguishable from zero when equation (5) is estimated on the sample of MPs only. This result is consistent with our assertion that the estimated coefficient of interest is driven by the causal impact of disclosure, rather than by any residual correlation between the name and tax payment. Had any contemporaneous, non-disclosure event been responsible for higher tax payments by more-unique-named taxpayers in 2012 and thereafter, it would have shown up in this falsification exercise as well. In our final test, we estimate equation (5) on the pre-program periods only (2006-2011), pretending as if the reform occurred in 2010 rather than the actual date of 2012. These placebo regressions always return trivial/statistically insignificant coefficients on the interaction term of interest.

The response to the public disclosure program is principally driven by three forces: shame, guilt, and fear of whistle-blowing. Of these, guilt is entirely internal to a person. A tax cheat may feel cognitive or emotional distress even if the act of cheating is never exposed. The disclosure program can intensify these feelings. A tax cheat may now experience greater disutility from guilt because they can compare their tax payments to their peers. Given that guilt is independent of the ease with which an individual can be identified in the disclosed records (on average the control group experiences the same level of guilt as the treatment group), our estimates do not capture the response driven by it. In this sense, our estimates represent a lower bound on the total effect of the disclosure program.

Our primary population of interest are the self-employed individuals. The Pakistani tax code and our administrative data defines a taxpayer as self-employed if their salary income does not exceed 50% of their taxable income. Self-employment

income, being self-reported and not subject to substantial cross-checking with third-party information reports, is the most amenable to manipulation. Tax compliance studies from around the globe show that the incidence and extent of noncompliance is the highest for the self-employed (see for example Slemrod, Forthcoming and Waseem, 2018c). If the public disclosure program curtails tax evasion, the effect would be the strongest for this section of the population. Wage income in Pakistan, as in other countries, is third-party-reported and some tax is withheld (i.e., remitted) by the employer. There is therefore little scope of evading wage income. Waseem (2018c) shows that the extent of evasion on this type of income in Pakistan is less than 1%. Given this, the program is unlikely to affect tax paid by wage earners. Our secondary population of interest are MPs. In regressions relating to them, the dummy variable treat_i indicates an individual who has been an MP in the 2013-2018 election cycle of Pakistan. Control groups in these regressions are either all individuals or individuals with relatively common names.

III.C TPHC Program

The TPHC program recognizes and rewards the top 100 taxpaying corporations, partnerships, self-employed individuals, and wage-earners. If the incentives and recognition offered by the program are valued, taxpayers ranked just below 100 would attempt to get into the top 100 in the next year and taxpayers just above the cutoff would attempt to stay there. The discontinuous treatment would thus cause a spike in the growth of tax paid from year t to $t+1$ by taxpayers ranked around the eligibility cutoff of the program in year t. We test this hypothesis by estimating regressions of the following sort:

$$\Delta\text{log TaxPaid}_{it} = \alpha_i + \beta\ \text{treat}_i \times \text{after}_t + \lambda_t + u_{it}, \tag{7}$$

where α_i and λ_t are the individual and year fixed effects and treat_i is a dummy indicating that taxpayer i was ranked in a window around the cutoff in year t. We begin with a narrow window around the cutoff and gradually widen it to determine whether, as expected, the effects of the program are concentrated close to the cutoff. We estimate this equation on a sample of the top 1000 taxpayers in each of the four categories. The principal identification concern in this setting is that income, and

therefore tax liability, of top taxpayers may be trending differently than others for non-program reasons such as rising inequality. We rule out this concern through non-parametric event studies and placebo falsification exercises.

IV Effects of the Public Disclosure Program

We first report the effects of the program on the general population of taxpayers and later for MPs.

IV.A All Taxpayers

Event Study—Figure II shows the results from the estimation of equation (6). We restrict the sample to a balanced panel of self-employed individuals who file in every year from 2006 to 2015. The figure plots the estimated values of the γ_js. Panels A-D feature four different definitions of treatment as indicated in the title of the panel. The first decile, first quartile, median, third quartile, and top decile of the Name Frequency distribution are 4, 6, 76, 1853, and 6091, respectively. Taxpayers in the first decile of the distribution, therefore, have literally unique names: their name appears 4 times in 4 years of data. To accentuate the comparison, we drop the middle part of the distribution in Panels C-D: second and third quartiles in Panel C and deciles 2-9 in Panel D. The results of the exercise strongly support our empirical strategy. There are almost no preexisting differences between the compared groups in terms of tax payments: for all the definitions of treatment, the γ_js are indistinguishable from zero for at least four of the five pre-program years. The tax payments of the two groups diverge exactly from the time the program takes effect. This divergence is sharp and persistent. It is also larger, the larger is the difference in exposure to the program. For example, the relative differences in Panel D (bottom vs. top decile) are almost double those in Panel B (below vs. above median).

All of the specifications show evidence of a dip in the timestamped treatment effect in 2013, the second year of the program. We can offer, but cannot formally test, two reasons for this dip. It may be that, on average, taxpayers were concerned about the program at first, but once it was rolled out they adjusted downward their perception of how much it actually increased their detection probability. In addi-

tion, in the second year of the program the disclosed identification number changed from the national tax number (NTN) to the CNIC; as discussed earlier, the CNIC is somewhat more visible, so this might have decreased the difference in effective disclosure between commonly and uncommonly named taxpayers.

Regression Results—Table II reports the regression results. We estimate equation (5) on the sample of self-employed individuals using four different definitions of treatment. To keep the control group fixed across all specifications, columns (1)-(6) drop taxpayers whose Name Frequency falls between the upper bound of the treatment and 40. All specifications include individual fixed effects and allow an unrestricted variance-covariance structure at the individual level.

Note that the public disclosure program can spur the entry of new taxpayers. If such entry is correlated with our measure of exposure to the program, the post-program sample would have a different composition than the pre-program one. Although the individual fixed effects mitigate this concern, we rule it out even further by estimating each specification on the balanced panel sample as well (even-numbered columns). Panel B provides a direct test of the validity of the research design, estimating each specification on the pre-program periods 2006-2011 only. We define the last two years in these placebo regressions as the post-program years.

The details of the regression results affirm the visual evidence presented above. The public disclosure induces individuals with relatively unique names to report on average around 9 log points more tax liability than others. This effect is statistically significant and remarkably stable across all specifications. As expected, it drops slightly as we widen the treatment window, allowing less distinctly named individuals to enter the treatment window, a finding we explore further in the next set of results. Panel B provides evidence that validates the empirical strategy, showing that the placebo coefficient capturing any preexisting trends in tax payments across the compared groups is trivial/insignificant in all specifications. This indicates that leveraging the variation in exposure to the program based on name uniqueness indeed isolates the treatment effect of the program.

The evidence we have presented so far is consistent with our premise that the program intensity varies proportionally with the uniqueness of a person's name. Table III explores this idea further. We now use a more continuous definition of treatment instead of a dichotomous one, exploring how the response varies across the Name Frequency distribution. The placebo specifications in columns (3)-(4) il-

lustrate that no systematic relationship existed between the tax payment and name of an individual before the program. However, a strong relationship appears after the program (columns 1-2), with taxpayers having more distinct names remitting significantly more tax. This effect is strongest at the left tail of the distribution, the most unique names. It declines monotonically as we move rightward and becomes indistinguishable from zero as the Name Frequency approaches 300. As we note above, we do not presume that taxpayers have a precise, objective idea of how common their name is. But life experiences of persons with very common name such as Muhammad Aslam would have instilled subjective beliefs that their name affords virtual anonymity to them. The results in Table III show that this threshold is apparently reached at about 300. Persons with such frequent names behave as if they are aware of the objective reality that linking the disclosed information to them through their name is virtually impossible.

In a final check on our empirical strategy, we show that no significant association exists between the name and tax payment for the sample of taxpayers who are (i) well-known and (ii) identified in the disclosed records through additional, unique identifiers. Table IV presents the results. We replicate Table II, estimating equation (5) on the sample of MPs only. Because MPs fulfill conditions (i) and (ii), we do not expect the regressions to return significant DD coefficients. Reassuringly, the results are consistent with our expectations: the uniqueness of the name of an MP is not associated with a significantly higher or lower tax payment after the program in any of the eight specifications.

IV.B MPs

We now turn our attention to MPs. For this group a disclosure suggesting noncompliance can be particularly stigmatic and damaging. If constituents negatively view suspiciously low tax payments and non-filing, it could influence an MP's election probability in addition to triggering mechanisms such as guilt, shame, and whistleblowing. A priori, therefore, the program should have a stronger pro-compliance effect on them.

Extensive Margin Response—One advantage we have in analyzing MPs' response that we did not have with all taxpayers is that we know the population of MPs who should be filing, which allows us to measure the effects of the program along

the extensive margin. More specifically, we can investigate if the disclosure causes an increase in the filing rate. The results of this investigation are shown in Figure III. It illustrates that only around 30% of MPs were filing their returns prior to the program. In fact, this apparently large level of noncompliance was what caused the public outrage that culminated in the public disclosure of all tax payments. Following the program, the filing rate increased to almost 100% in 2012, declining a little thereafter to the 85-90% mark. The corresponding LPM regressions, reported in Table A.IV, show that the filing rate on average increased by nearly 60 percentage points. The increase was slightly higher for MPs in the ruling-party and federal legislators (who are relatively more visible), but not for ones in more competitive races; these differences mostly reflect differences in the pre-treatment levels, as post-treatment all of the filing rates are very close to 100%.

Intensive Margin Response—Note that not filing a tax return does not unambiguously signal tax evasion. As we mention above, income tax due on certain transactions is withheld at source. Income from agriculture is not subject to the federal income tax. Non-filer MPs who earn all their income from such sources would not have underpaid taxes, even though their non-filing constitutes a clear breach of law. We now focus on the tax evasion margin, comparing the tax paid by MPs with others.

Table V estimates this response. We use two control groups: all non-MP taxpayers in columns (1)-(2) and (5)-(6), and common-named, non-MP taxpayers in other columns. Each specification has its own merits. The first control group includes taxpayers who are themselves affected by the program. The specification therefore isolates the additional response of MPs, i.e. their response relative to the if-they-were-ordinary-taxpayers counterfactual. This additional response reflects that MPs are perhaps more sensitive to the disclosure (because of its effect on their reelection probability) and that the disclosed information is more salient for them. The second control group excludes taxpayers whose Name Frequency exceeds 300. These taxpayers, as shown in Table III, enjoy effective anonymity in the disclosure and are therefore less responsive to it. The specification accordingly captures MPs response relative to the no-effective-disclosure counterfactual.

All MPs receive a salary from the government of Pakistan, in an amount that is fixed by the relevant legislature.[12] In addition to this, MPs may also receive in-

[12]The salary was fixed initially by the The Members of Parliament (Salaries and Allowances) Act,

come from businesses they own or assets they hold. We expect the effects of the program to be concentrated on this part of income. To capture this, we run parallel regressions for each specification where we restrict the sample to individuals whose non-salary incomes constitutes more than 50% of their taxable income.

The results show that the program had a far stronger compliance effect on MPs than on non-MPs. Their tax payments went up on average by 40 log points relative to the first control group and by 50 log points relative to the second. Consistent with our expectations, this response is primarily driven by the non-salary income. The estimates in even-numbered columns (which focus on non-salary income) are nearly double those in the odd-numbered columns.

Note that the placebo regressions return statistically significant coefficients in three out of eight specifications. We suspect that this is due mainly to the lack of power we face in these specifications. The 2013-2018 Pakistani legislature had 1174 members. Only one-third of these were filing tax returns prior to the program. This leads to small treatment samples in our regressions, especially when we do not work with the complete panel.[13] Another plausible reason for this are the pre-program effects. MPs in our sample won their seats in the election of May 2013. As a requirement of running for office, they had to report the tax paid by them during the tax year 2011 to the Election Commission of Pakistan. They also almost certainly knew that their tax declaration would receive increased attention from the media due to the ongoing investigation of the CIRP (as discussed in section II.A). They therefore might have remitted higher tax for the year 2011 to create a favorable impression on their constituents. We find some evidence of this in the event study diagrams displayed in Figure IV. The DD coefficient for 2011 is significantly higher than the pre-program trend in specifications with the restricted samples. Finally, Table A.V explores response heterogeneity across MPs. None of the three triple-interaction terms we explore is statistically significant.

1974. It is revised from time to time using the procedure laid down in the statute.

[13]One other consequence of the change in the composition of the sample in 2012 (see Figure III) is that the balanced-panel estimates in column (5) and (7) are larger than the corresponding complete panel estimates. The balanced-panel estimates here capture the average response of MPs who were filing their tax returns even in the pre-program years.

V Effects of the TPHC Program

Figure V provides non-parametric evidence on the effects of the TPHC program. The sample for this diagram includes corporations, partnerships, salaried and non-salaried taxpayers. We group taxpayers into 20-rank bins on the basis of their rank in year t. The upper bound of a bin is included in the bin so that, for example, the bin denoted by 40 in the horizontal axis includes the taxpayers ranked between 21 and 40 in each of the four categories. We then plot the average log change in tax paid from year t to $t+1$ in the bin. To increase the power of our analysis, we take the averages over two-year periods in Panel A and over the entire pre- and post-program periods in Panel B. Because we are plotting changes rather than levels, 2011 is the first post-program year in this analysis. If the program influences behavior, the post-program curves should be significantly higher than the pre-program ones around the cutoff of 100. The evidence in the diagram is consistent with this a priori reasoning. It suggests that at least some taxpayers near the eligibility cutoff of the program increase their tax payments in order to receive or continue to receive the benefits of the program.

Table VI formalizes this analysis. We estimate equation (7) on a sample of the top 1000 taxpayers in each of the four categories. We define taxpayers in a window around the eligibility cutoff of the program as treated, and look for any differential growth in tax liability reported by them relative to the other taxpayers. In line with the visual evidence, the growth rate does spike up around the cutoff. For example, the DD coefficient in the first column shows that compared to the others, the yearly growth in tax liability reported by the 81-120 ranked taxpayers was on average 17 log points higher in the post-program years than it was in the pre-program years. The coefficient declines slightly as we widen the window, suggesting that the effect is stronger closer to the cutoff.

To establish that our DD coefficient captures the causal effect of the program, we need to ensure that it is not driven by any differential trends resulting from, for example, rising inequality at the top. We take three steps to achieve this. First, we re-estimate each specification in the table by adding a treat$\times 1.(\text{year} \in \{2009, 2010\})$ interaction term into it. The coefficient on the term loosely captures any differences in the preexisting trends across the compared groups. It is small and statistically insignificant in all the specifications. Second, we estimate our model on the pre-

program period only, pretending that the reform occurred in 2010. These placebo regressions, shown in Panel B, always return insignificant coefficients. Finally, we look for the effect of the program on very similar taxpayers unaffected by it. Table VII conducts this exercise. The treatment window now contains taxpayers who are relatively far away from the eligibility cutoff of the program, on whose behavior we expect the program to have no influence. The results confirm this. None of the coefficients in the table is distinguishable from zero at the conventional level.

To increase the power of our analysis, we have so far combined all four categories of taxpayers in our estimation samples. Table A.VI decomposes the aggregate response. We now estimate our baseline specification (7) separately on the sample of top 1000 taxpayers of each of the four categories. The results show that the aggregate effect we report above is driven almost entirely by the behavior of corporations. Compared to the large and statistically significant effect on corporations, the program's effect on the other three categories of taxpayers is not different from zero.

These heterogeneous findings are perhaps not surprising. Of the four taxpayer types, corporations are perhaps in the best position to monetize the goodwill offered by the program. They can build their brand by advertising their status as one of the top taxpayers, translating social recognition into higher sales and profits. Anecdotal evidence from other jurisdictions also shows that big firms are considerably sensitive to the public discussion of their taxes, both favorable and adverse. Second, as we note above, personal benefits of the program such as fast-track immigration are conferred on the CEO of the corporation. The burden of higher tax payments, on the other hand, falls on shareholders. If the oversight by the board of governors is weak, the agency problem can also result in a situation where the CEOs obtain benefits at the cost of shareholders.

VI Conclusion

We analyze two Pakistani programs to explore the roles of both deterrence as well as social and psychological factors in the tax compliance choice of agents. In the first of these programs, the government began revealing the reported tax liability by every taxpayer in the country. The disclosure program exposes tax evaders to the fear of whistle-blowing from peer groups in case the tax payments do not match

the level of consumption and wealth observed by them. It also may exacerbate the guilt and shame felt by some potential evaders.

We find that, relative to those unexposed to the program, the tax paid by individuals exposed to the program on average went up by about 10 log points. The increase was far greater for the subsample where the exposure was more salient and peers more responsive. In the second of these programs, the government began acknowledging and honoring top taxpayers in the country. We find that, as a result of the program, the tax liability reported by treated taxpayers in the neighborhood of the program threshold went up by approximately 15 log points.

That these programs produce significant response has important implications. It shows that fear of detection and punishment as well as shame and pride may, in some settings, be meaningful determinants of behavior that economic models need to take into account. From a policy standpoint, the results show that public disclosure and social recognition of top taxpayers can be effective enforcement instruments. To the extent that fear, shame, and pride motivate humans toward pro-social behavior, the governments can leverage them to promote compliance and hence welfare. These programs cost little resources, and therefore can be a cost-effective complement to the other costly measures the governments undertake to deter noncompliance.

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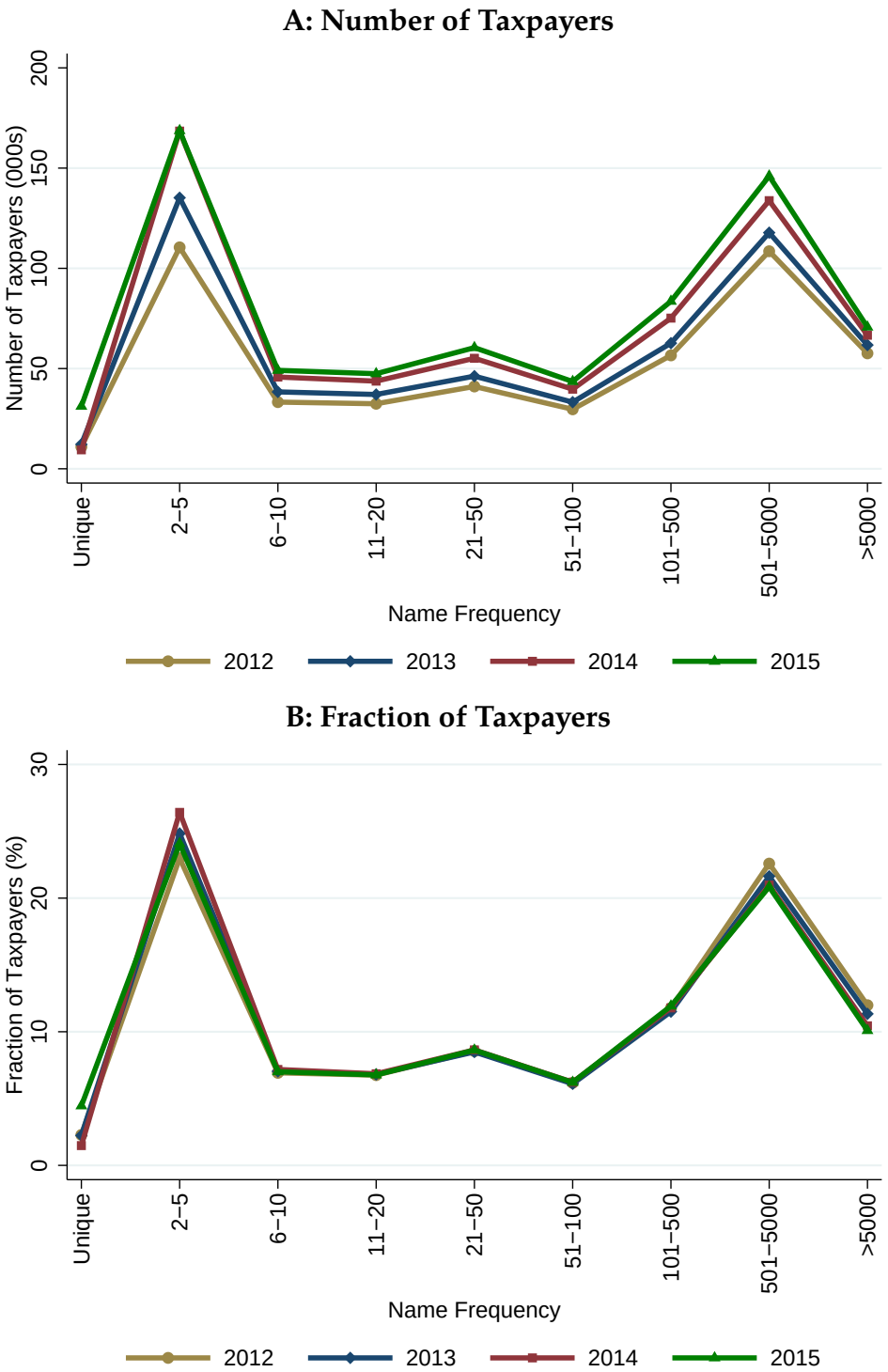
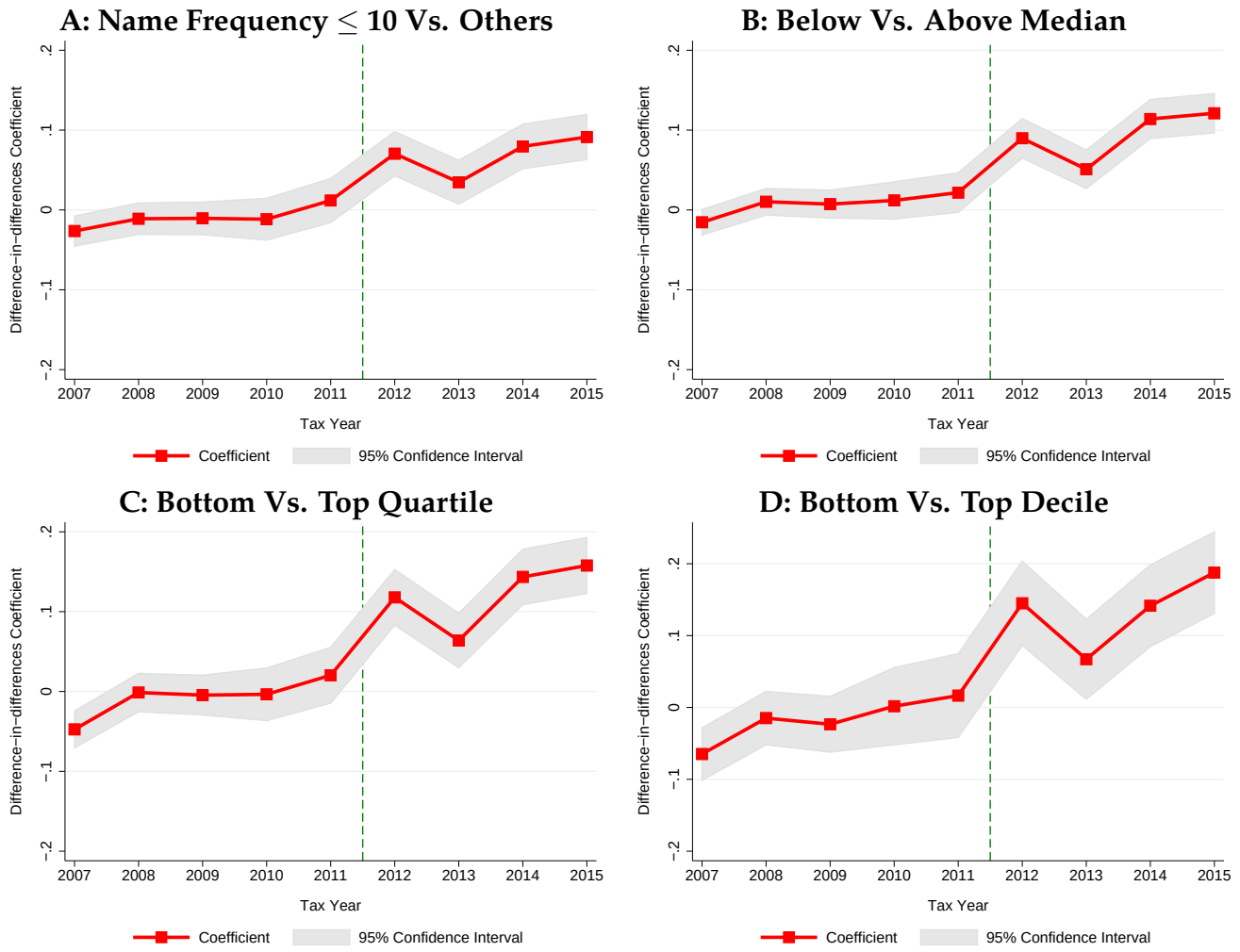
Figure I: Distribution of Names



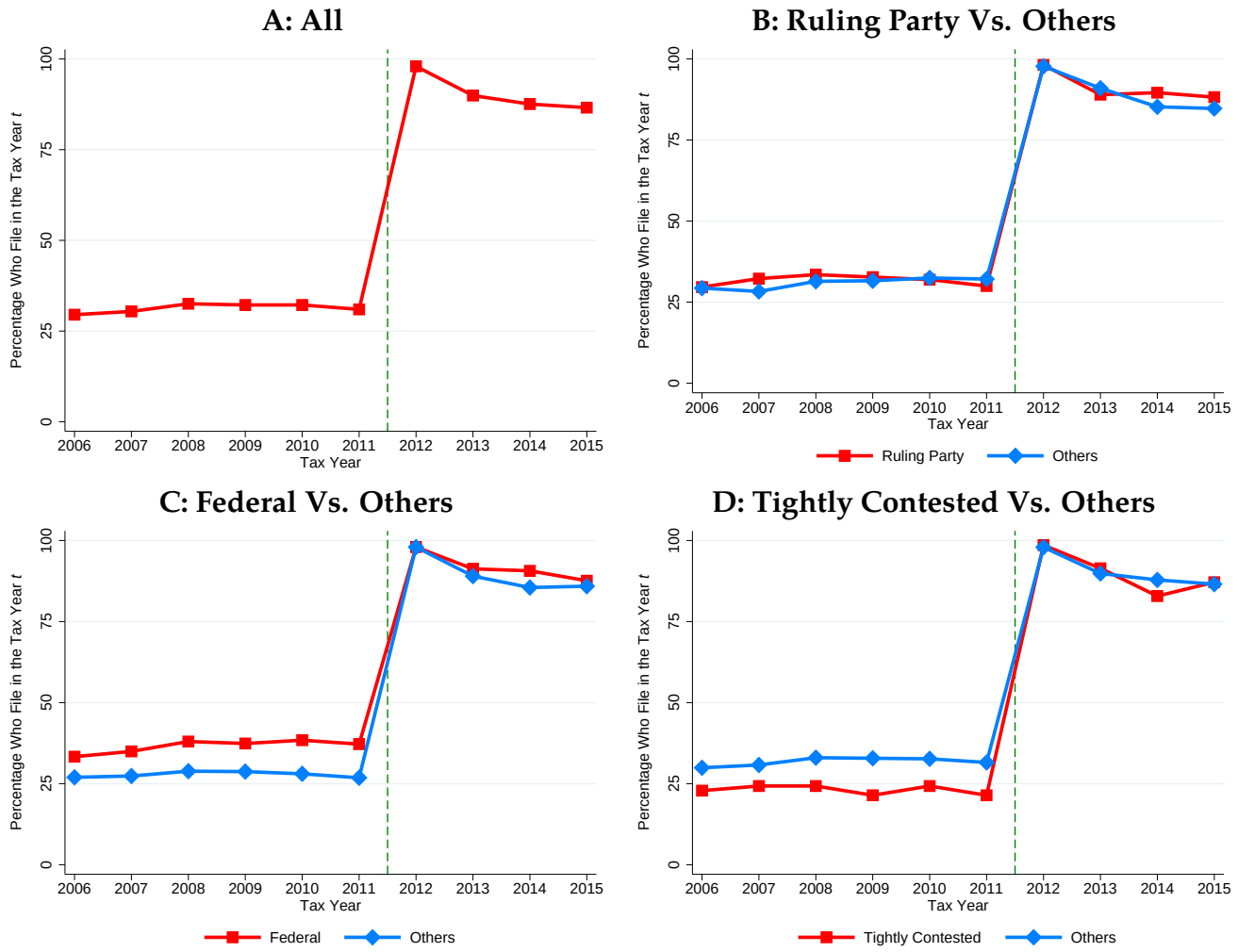
Notes: The figure illustrates the distribution of full names in Pakistan. We define Name Frequency as the number of times a full name appears in the disclosure data for the years 2012-2015. The Name Frequency of 4, for example, means that the full name appears four times in four years of data. The two panels plot the distribution of the variable. Each marker in panel A denotes the number of individuals in year t whose Name Frequency falls in the interval indicated in the horizontal axis. Panel B plots the fraction in place of the number. We treat all English variants of an Urdu name as one. For example Muhammad spelled as Mohammad, Mohammed, or Muhammed is treated as one name. The algorithm we use to clean such spelling variations is described in Appendix A.1.

Figure II: Response to the Public Disclosure – All Taxpayers



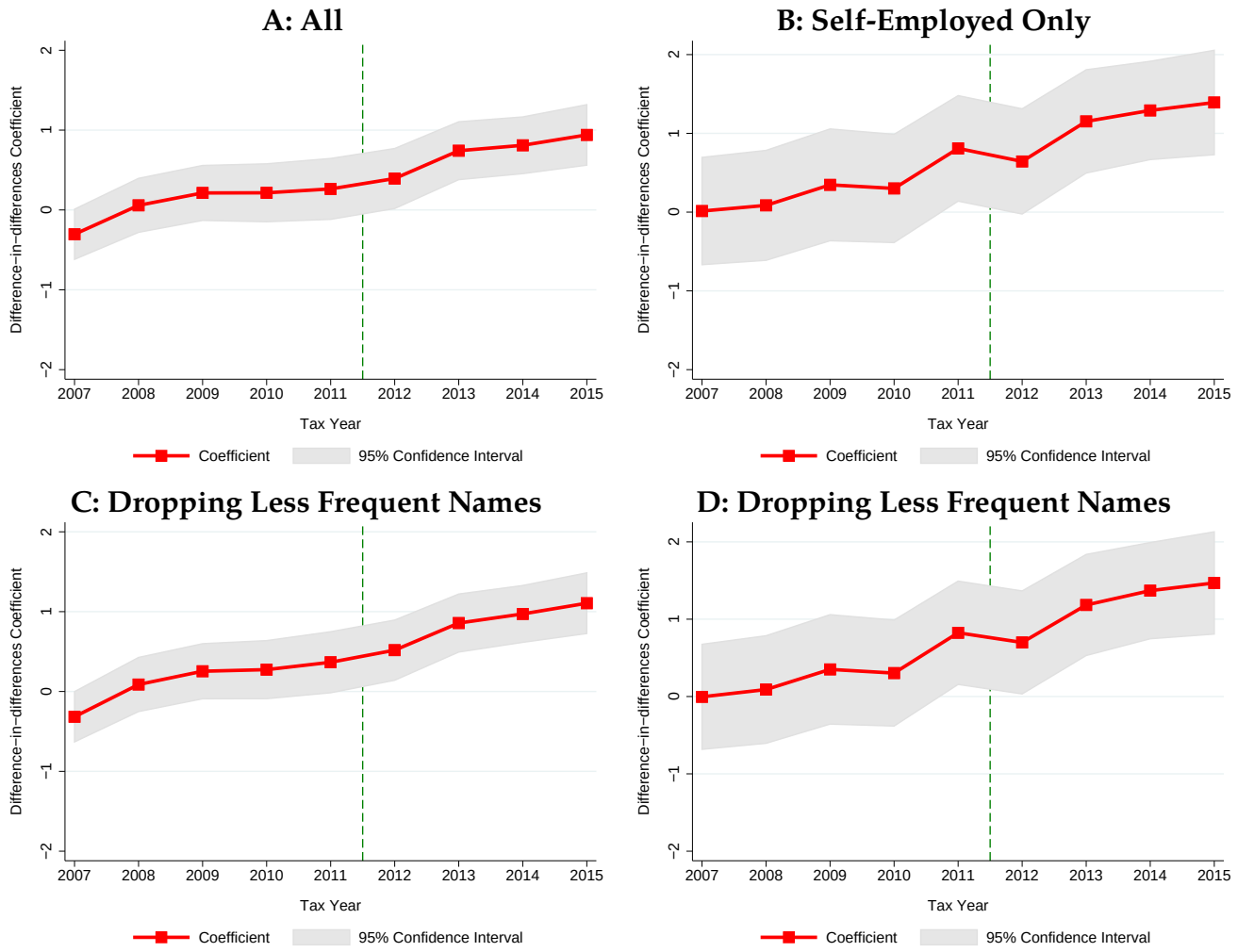
Notes: The figure plots the coefficients γ_js and 95% confidence interval around them from the event study equation (6). We estimate the equation on a balanced panel sample of self-employed taxpayers, who file in all years from 2006 to 2015. The definitions of the treatment and control groups are provided in the title of each panel. For example, for Panel A all observations where full name of the taxpayer appears at the most ten times in the four years' disclosure data are considered as treated; the rest of the taxpayers serve as the control group. For Panels C-D, we drop observations in the middle of the distribution: the middle two quartiles in Panel C and the middle eight deciles in Panel D. The standard errors have been clustered at the individual level. Vertical lines demarcate the time from which the public disclosure begins to have an effect on the tax paid by individuals.

Figure III: Response to the Public Disclosure – MPs (Tax Filing)



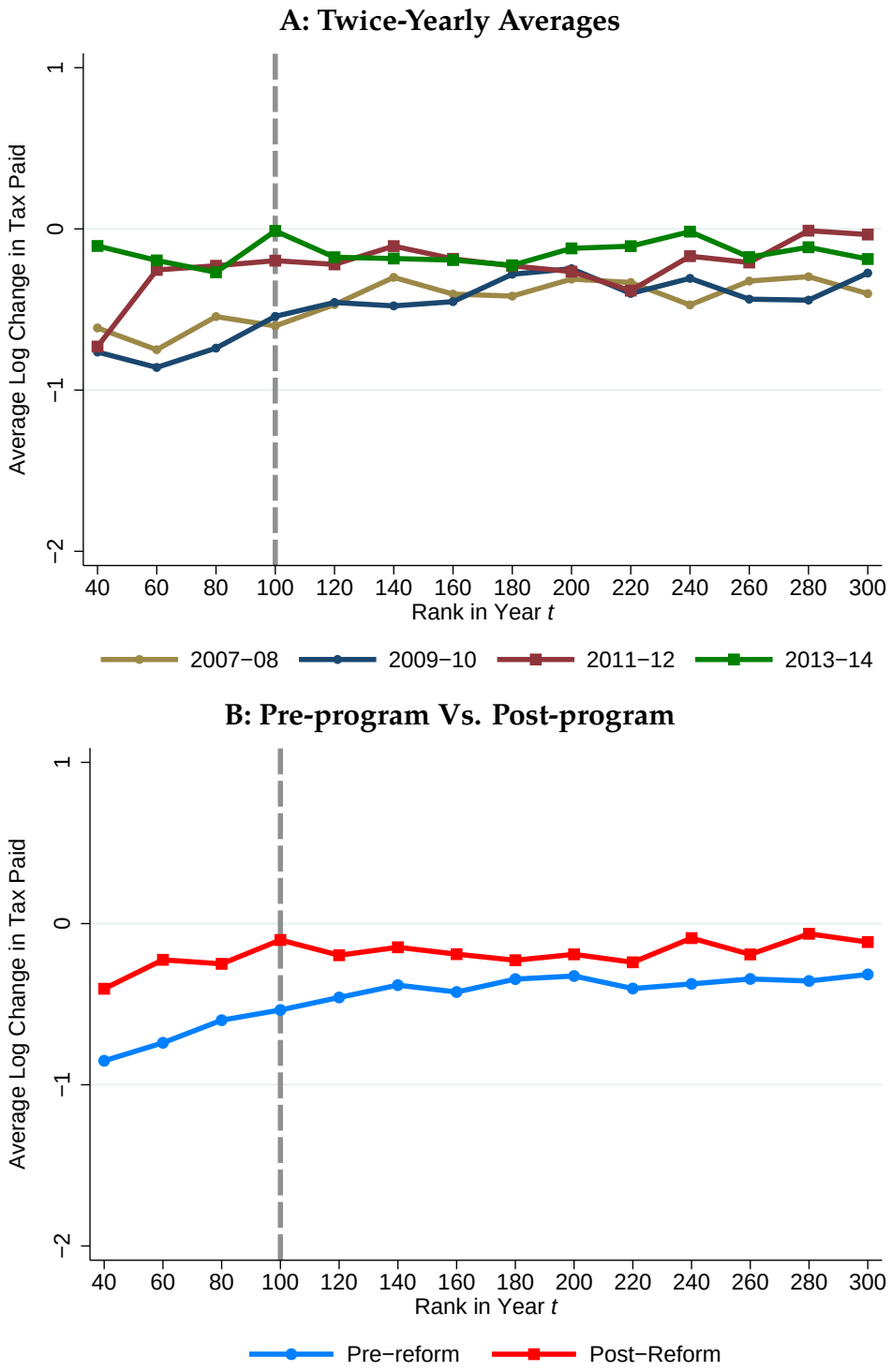
Notes: The figure plots the fraction of MPs who file their tax return in the year indicated in the horizontal axis. MP denotes an individual who has been a member of a federal or provincial legislature in the 2013-2018 election cycle of Pakistan. Ruling Party denotes the party that formed the federal or provincial government the MP belongs to. The dummy Tightly Contested takes the value 1 if the difference between the winning and runner-up candidates is less than 2% of the valid votes. Vertical lines demarcate the time from which the public disclosure begins to have an effect on the tax filing of MPs.

Figure IV: Response to the Public Disclosure – MPs (Tax Paid)



Notes: The figure displays how the tax paid by MPs reacts to the public disclosure program. The figure plots the coefficients γ_js and 95% confidence interval around them from the event study equation (6). We estimate the equation on a balanced panel sample of individuals who file in all years from 2006 to 2015. For these regressions, the dummy variable treat_i indicates that the individual has been an MP during the 2013-2018 election cycle of Pakistan. Panel A compares all MPs to all other individuals. Panel B restricts the comparison to the self-employed individuals only. Panel C-D replicate Panels A-B but drop non-MP taxpayers with Name Frequency up to 300. The standard errors have been clustered at the individual level. Vertical lines demarcate the time from which the public disclosure begins to have an effect on the tax paid by individuals.

Figure V: Response to the TPHC Program



Notes: The figure explores the response to the TPHC program. We rank taxpayers in each of the four categories—self-employed, wage-earners, partnerships, and corporations—on the basis of tax paid by them in period t, group them into 20 rank bins, and plot the average log change in tax paid from period t to $t+1$ in the bin as a function of the rank in period t. Panel A takes the overage over two-year periods; Panel B over the entire pre- and post-program periods. The upper bound of the bin is always included in the bin. For example, the bin indicated by 40 includes 21-40 ranked taxpayers of each category. The vertical line demarcates the eligibility cutoff of the program.

Table I: Timeline of the Public Disclosure Program

Date (1)	Event (2)
Sep-Dec 2012	Investigative reports alleging tax noncompliance by MPs begin appearing in the press
December, 2012	First CIRP report released. It publishes the data that formed the basis of earlier investigative reports, cataloging tax noncompliance of MPs in the 2008-2013 election cycle of Pakistan
December, 2012	The Federal Tax Ombudsman orders the FBR to begin disclosing the tax paid by every public office holder in the country
January, 2013	The leading opposition party and eventual election winner, PML-N, issue election manifesto, pledging the public disclosure of tax paid by all taxpayers in the country
May 11, 2013	General elections
June 30, 2013	Tax year 2012 ends
December 15, 2013	Final date for filing of 2012 tax return
December, 2013	Second CIPR report published. It documents the tax payments of MPs who won during the 2013 elections
February 28, 2014	The directory of MPs for the tax year 2012 published
April 15, 2014	The directory of all taxpayers for the tax year 2012 published
June 30, 2014	Tax year 2013 ends
April 10, 2015	Directories of both MPAs and general taxpayers for the tax year 2013 published
June 30, 2015	Tax year 2014 ends
June 30, 2016	Tax year 2015 ends
September 9, 2016	Directories of both MPAs and general taxpayers for the tax year 2014 published
July 27, 2017	MPs' directory for the tax year 2015 published
August 11, 2017	All taxpayers' directory for the tax year 2015 published

Notes: The table report the timeline of important events in the public disclosure program. The date each event listed in column (2) occurred is given in column (1). Pakistani tax year runs from July to June. Tax year indicated by t in this paper runs from July t to June $t+1$. The first CIRP report indicated in the second row is available here; the second report indicated in the eighth event is available here. Tax directories of all years can be downloaded from here.

Table II: Response to the Public Disclosure Program – All Taxpayers

	Treat: Name Frequency							
	≤ 10		≤ 20		≤ 30		≤ 40	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Main Regression (2006-2015)								
treat × after	0.094	0.093	0.090	0.089	0.089	0.086	0.088	0.086
	(0.006)	(0.009)	(0.005)	(0.008)	(0.005)	(0.008)	(0.005)	(0.008)
Observations	2,430,002	773,038	2,614,754	833,675	2,720,267	868,250	2,792,270	891,420
B: Placebo Regression (2006-2011)								
treat × after	0.009	0.005	0.013	0.009	0.013	0.010	0.014	0.010
	(0.007)	(0.008)	(0.006)	(0.008)	(0.006)	(0.008)	(0.006)	(0.008)
Observations	1,307,541	734,269	1,403,240	787,845	1,458,457	818,942	1,496,374	840,469
Sample:								
Balanced Panel	No	Yes	No	Yes	No	Yes	No	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports the estimates from equation (5). For Panel A, we estimate the equation on a sample containing all self-employed individuals for the period 2006-2015. The definition of the treatment variable is provided in the title of each column. The dummy variable takes the value 1 if the Name Frequency of an individual does not exceed the cutoff indicated in the title. To maintain a fixed control group across all columns, we drop taxpayers with Name Frequency between 10 and 40 in Columns (1) to (6). Panel B reports the results from parallel placebo regressions, where the sample is restricted to tax years 2006 to 2011, with the last two years defined as the post-program years. Even-numbered columns restrict the sample to a balanced panel of taxpayers, who file in all years included in the sample. Standard errors are in parenthesis, which have been clustered at the individual level.

Table III: Public Disclosure Response Across the Name Distribution

	Baseline Specification (2006-2015)		Placebo Specification (2006-2011)	
	(1)	(2)	(3)	(4)
Name Freq $\in (0,\ 50] \times$ after	0.107	0.105	0.020	0.013
	(0.005)	(0.008)	(0.007)	(0.008)
Name Freq $\in (50,\ 100] \times$ after	0.067	0.069	0.014	0.003
	(0.011)	(0.016)	(0.014)	(0.016)
Name Freq $\in (100,\ 150] \times$ after	0.061	0.080	0.027	0.036
	(0.015)	(0.023)	(0.019)	(0.023)
Name Freq $\in (150,\ 200] \times$ after	0.050	0.046	0.029	0.034
	(0.019)	(0.029)	(0.025)	(0.030)
Name Freq $\in (200,\ 250] \times$ after	0.043	0.011	0.014	-0.005
	(0.021)	(0.031)	(0.026)	(0.032)
Name Freq $\in (250,\ 300] \times$ after	0.045	0.022	-0.014	-0.027
	(0.022)	(0.033)	(0.028)	(0.036)
Name Freq $\in (300,\ 350] \times$ after	0.047	0.086	0.032	0.042
	(0.025)	(0.038)	(0.032)	(0.039)
Name Freq $\in (350,\ 400] \times$ after	0.037	0.039	0.028	0.021
	(0.027)	(0.041)	(0.037)	(0.043)
Name Freq $\in (400,\ 450] \times$ after	0.035	0.017	0.017	0.029
	(0.026)	(0.039)	(0.033)	(0.041)
Observations	2,792,270	891,420	1,496,374	840,469
Sample:				
Balanced Panel	No	Yes	No	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table explores how the response to the public disclosure program varies across the name distribution. We estimate an augmented version of equation (5), including the nine interaction terms shown above. The equation is estimated on a sample of all self-employed individuals. The control group in these regression are the self-employed whose Name Frequency exceeds 450. The coefficient on each interaction terms accordingly captures the average additional tax paid (in log points) by the self-employed with Name Frequency falling in the interval as a result of the program. Columns (1) and (2) report the results for the baseline specification containing periods 2006-2015, both for the complete and balanced panels. Columns (3) and (4) estimate the specifications on the pre-program years only, defining the years 2010 and 2011 as the post-program period. Standard errors are in parenthesis, which have been clustered at the individual level.

Table IV: Response to the Public Disclosure Program – MPs (Placebo)

	Treat: Name Frequency							
	≤ 10		≤ 20		≤ 30		≤ 40	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: 2006-2015								
treat × after	-0.240	-0.349	-0.166	-0.159	-0.225	-0.293	-0.226	-0.235
	(0.151)	(0.196)	(0.166)	(0.212)	(0.170)	(0.218)	(0.178)	(0.227)
Observations	5,452	1,544	5,452	1,544	5,452	1,544	5,452	1,544
B: 2006-2011								
treat × after	-0.190	-0.268	-0.048	0.024	-0.134	-0.089	-0.148	-0.121
	(0.173)	(0.240)	(0.169)	(0.233)	(0.178)	(0.240)	(0.179)	(0.242)
Observations	1,713	883	1,713	883	1,713	883	1,713	883
Sample:								
Balanced Panel	No	Yes	No	Yes	No	Yes	No	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table illustrates that the name of a taxpayer does not influence their tax payment as long as the effectiveness of the disclosure is independent of the name. We replicate Table II on a sample of MPs only. As MPs are (i) well-known and (ii) identified in the disclosed data directly through their constituency numbers, their exposure to the program does not depend upon how common their name is. As earlier, the definition of the treatment variable is provided in the title of each column. The dummy variable takes the value 1 if the Name Frequency of the MP does not exceed the cutoff indicated in the title. To maintain a fixed control group across all columns, we drop MPs with Name Frequency between 10 and 40 in Columns (1) to (6). Panel B reports the results from a parallel placebo regression, where the sample is restricted to tax years 2006 to 2011, with the last two years defined as the post-program years. Even-numbered columns restrict the sample to a balanced panel of MPs, who file in all years included in the sample. Standard errors are in parenthesis, which have been clustered at the individual level.

Table V: Response to the Public Disclosure – MPs

	Complete Panel				Balanced Panel			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Main Regression (2006-2015)								
treat × after	0.407	0.900	0.519	0.966	0.651	0.906	0.756	0.965
	(0.069)	(0.117)	(0.070)	(0.115)	(0.097)	(0.165)	(0.097)	(0.165)
Observations	5,832,527	2,968,236	1,747,719	1,105,038	1,304,247	971,216	454,364	379,390
B: Placebo Regression (2006-2011)								
treat × after	0.033	0.374	0.089	0.385	0.173	0.368	0.243	0.384
	(0.082)	(0.151)	(0.082)	(0.148)	(0.114)	(0.203)	(0.114)	(0.202)
Observations	3,098,528	1,670,694	963,113	646,461	800,475	610,799	286,013	243,515
Sample:								
Wage-earners Dropped	No	Yes	No	Yes	No	Yes	No	Yes
Control Group:								
Less-Common Names Dropped	No	No	Yes	Yes	No	No	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports the results from equation (5). We estimate the equation on a sample containing all individuals (both MPs and non-MPs). The dummy variable $treat_i$ denotes an individual who has been an MP in the 2013-2018 election cycle of Pakistan. Even-numbered columns drop wage-earners; columns (3)-(4) and (7)-(8) drop individuals with Name Frequency up to 300, and columns (5)-(8) restrict the sample to a balanced panel of individuals who file in all years included in the sample (2006-2015 in Panel A and 2006-2011 in Panel B). Panel B reports the results from parallel placebo regressions, where the sample is restricted to tax years 2006 to 2011, with the last two years defined as the post-program years. Standard errors are in parenthesis, which have been clustered at the individual level.

Table VI: Response to the TPHC Program

	Treat: Rank							
	$\in (80,\ 120]$		$\in (70,\ 130]$		$\in (60,\ 140]$		$\in (50,\ 150]$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Main Regression (2006-2014)								
treat × after	0.165	0.173	0.178	0.182	0.144	0.108	0.143	0.105
	(0.077)	(0.095)	(0.063)	(0.078)	(0.055)	(0.065)	(0.049)	(0.059)
treat × 1.(year ∈ {2009,2010})		0.019		0.010		-0.086		-0.090
		(0.120)		(0.102)		(0.091)		(0.081)
Observations	32,047	32,047	32,047	32,047	32,047	32,047	32,047	32,047
B: Placebo Regression (2006-2010)								
treat × after	0.019		0.010		-0.086		-0.090	
	(0.120)		(0.102)		(0.091)		(0.081)	
Observations	17,208		17,208		17,208		17,208	

Notes: The table reports the results from the equation (7). We estimate the equation on a sample containing top 1000 taxpayers of each of the four categories of taxpayers, corporations, partnerships, self-employed, and wage-earners. The treatment variable here denotes taxpayers ranked in period t in a window around the eligibility cutoff of the program. The exact length of the treatment window is indicated in the title of each column. Given that we measure the outcome variable here in changes rather than levels, the first post-program year is 2011. Panel A estimates the equation on years 2006-2014. Panel B runs parallel placebo regressions on years 2006-2010, with the last two years defined as the post-program years. Columns (2), (4), (6) and (8) test the parallel trend assumption by including a $treat \times 1.(year \in \{2009, 2010\}$ interaction into the regression. Standard errors are in parenthesis, which have been clustered at the individual level.

Table VII: Response to the TPHC Program (Placebo)

	Treat: Rank							
	$\in (150,\ 200]$		$\in (200,\ 250]$		$\in (250,\ 300]$		$\in (300,\ 350]$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Main Regression (2006-2014)								
treat × after	-0.029	0.006	0.015	0.026	0.049	0.032	-0.000	0.025
	(0.069)	(0.086)	(0.063)	(0.077)	(0.059)	(0.069)	(0.065)	(0.078)
treat × 1.(year ∈ {2009,2010})		0.084		0.025		-0.040		0.058
		(0.100)		(0.092)		(0.094)		(0.094)
Observations	32,047	32,047	32,047	32,047	32,047	32,047	32,047	32,047
B: Placebo Regression (2006-2010)								
treat × after	0.084		0.025		-0.040		0.058	
	(0.100)		(0.092)		(0.094)		(0.094)	
Observations	17,208		17,208		17,208		17,208	

Notes: The table tests the validity of the research design used to estimate the TPHC response. We estimate equation (7) on a sample containing top 1000 taxpayers of each of the four categories of taxpayers, corporations, partnerships, self-employed, and wage-earners. But in distinction to Table VI, the treatment variable here denotes taxpayers who are not affected by the program, being too far away from its eligibility cutoff. The exact length of the treatment window used here is indicated in the title of each column. Given that we measure the outcome variable here in changes rather than levels, the first post-program year is 2011. Panel A estimates the equation on years 2006-2014. Panel B runs parallel regressions on years 2006-2010, with the last two years defined as the post-program years. Columns (2), (4), (6) and (8) test the parallel trend assumption by including a $treat \times 1.(year \in \{2009, 2010\}$ interaction into the regression. Standard errors are in parenthesis, which have been clustered at the individual level.

A Online Appendix

A.1 Name Cleaning Algorithm

Identifying Potential Spelling Variations in Pakistani Names

Most Pakistani names are derived from Arabic, Persian or Turkish. Like Urdu, these languages are (or were) written in variants of the Arabic script. As a result the spelling variations in Pakistani names arise mainly because of some standard issues in transliterating Arabic script words into English.

The most common issue is the spelling of transliterated vowel sounds. As there are no standardized rules for transliteration each vowel sound can be spelled in many different ways. In Urdu, shorter vowel sounds are not indicated through separate letters. So for example, the name Muhammad in Urdu is spelled with only four letters - MHMD. In transliterating the name to English there is considerable discretion as to what English vowels will be used for the sound in each syllable. The first syllable can be spelled as M, MA, MO, MU, MUA, MOU, MU; the second syllable as HAM, HUM, HOM, and the third syllable as MED, MAD, MD. The various combinations of these syllables generates multiple spellings for the same name.

Some longer vowel sounds are indicated through specific letters. However the spelling issue still persists in these cases because of a lack of transliteration rules. For example the name Mehmood in Urdu is spelled with five letters - MHMUD. The added vowel represents the "oo" sound as in "rude" but it can be spelled as either U OO OU or UO.

Secondly, elongated sounds or sounds that are repeated across syllables are not indicated through double letters (as is often the case in English) but are also expressed through accent marks. Again taking the case of the name Muhammad, the middle "m" sound is repeated but spelt with a single letter in Urdu. In English the sounds can be spelled as M or MM depending on whether the spelling is based on the Urdu spelling or the phonetic sound.

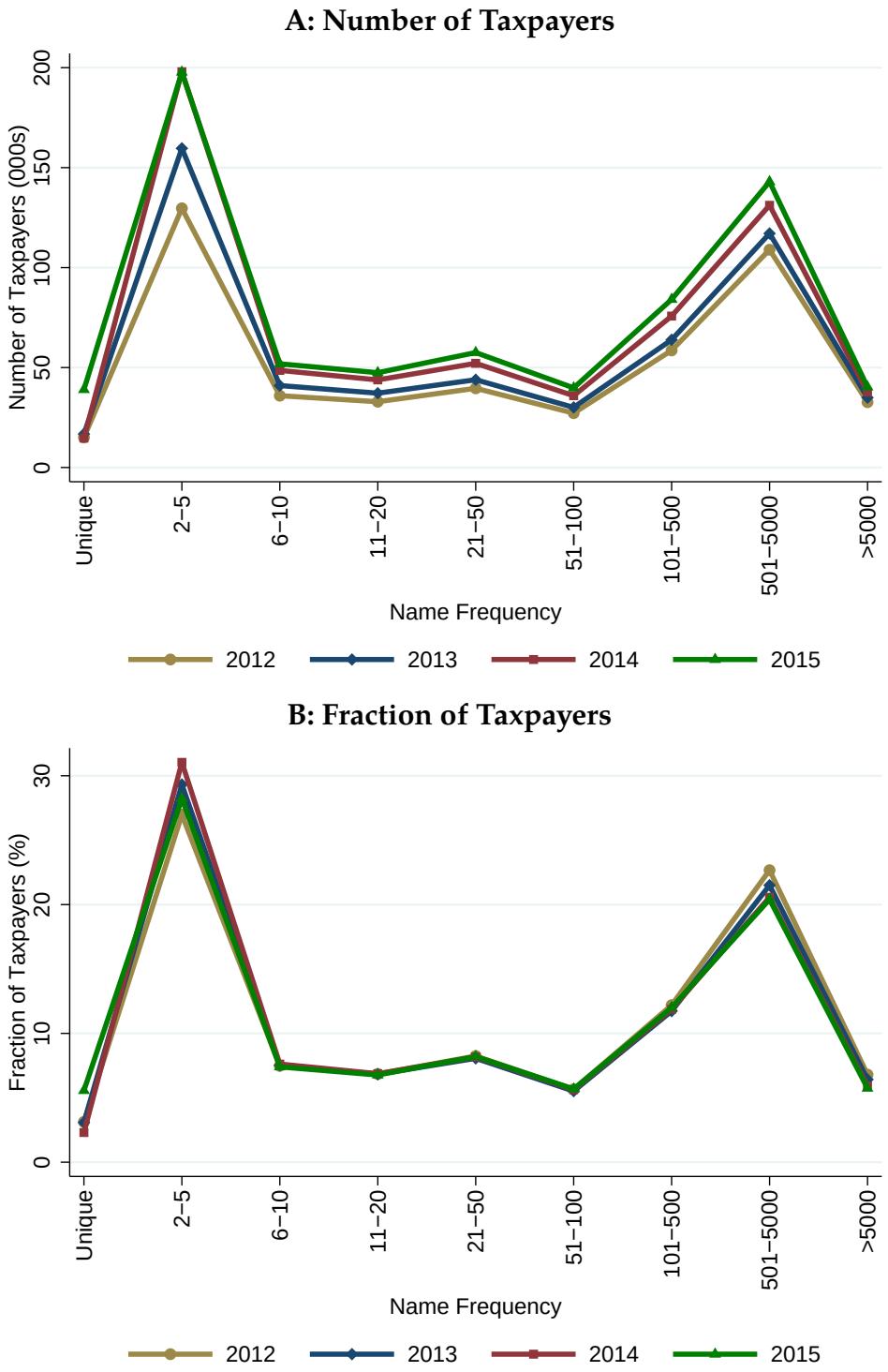
So for a given Urdu name, the vowel and repeated sounds imply potential spelling variations which we use to identify variants of the same name.

Standardizing Full Names

The tax directory published by the Federal Board of Revenue (FBR) lists each taxpayer's full name. We combine the tax directories for all "Individual" taxpayers for 2012-2015 to get an exhaustive list of all full names that have ever appeared in the disclosure data. We then split the full names, based of spaces or hyphens, into the different (given or family) single names they constitute. This gives us a master list of all distinct single names in the data.

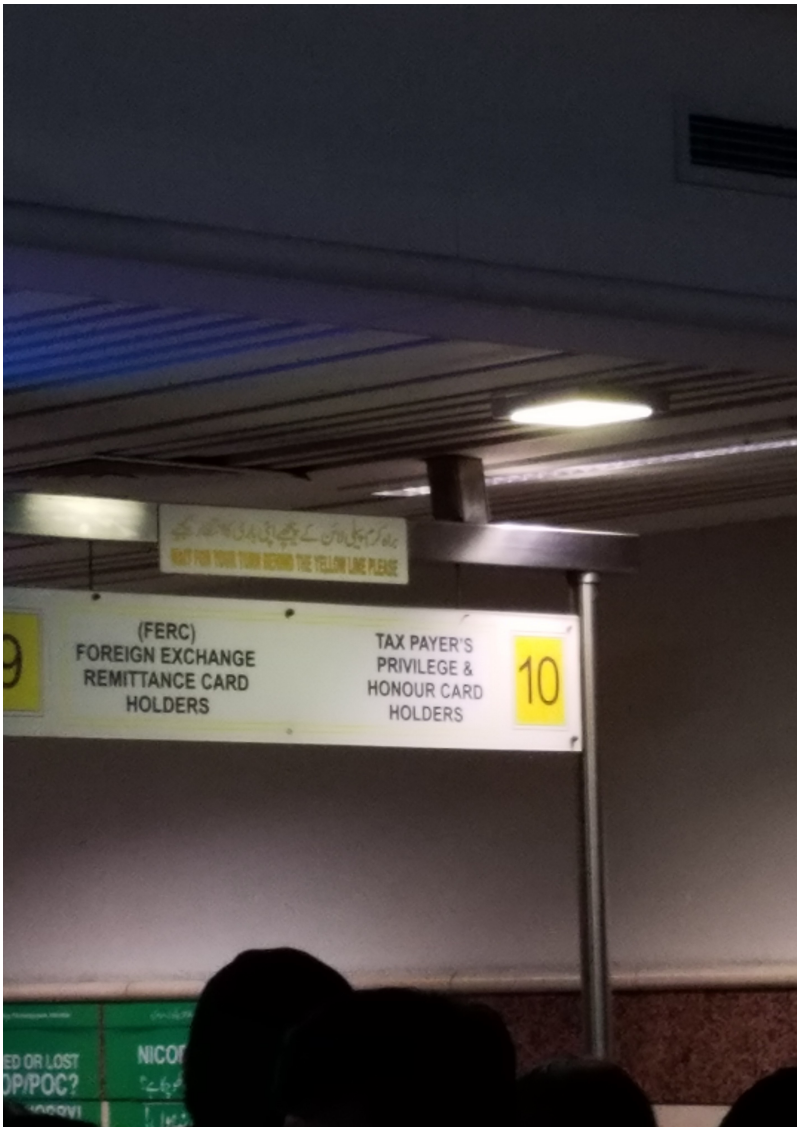
Given the possible spelling variations we manually work through this master list to identify the English variants of the same Urdu names. By convention, certain spellings of names have become more common and widely used. Each name variant is standardized to the most common spelling used for that name in the data. After the spellings of the single names are standardized we combine them back again to create standardized full names. The name frequency measures we use in the analysis are based on these standardized full names.

Figure A.I: Distribution of Names – Original Spelling



Notes: The figure illustrates the distribution of full names in Pakistan. We define Name Frequency as the number of times a full name appears in the disclosure data for the years 2012-2015. The Name Frequency of 4, for example, means that the full name appears four times in four years of data. The two panels plot the distribution of the variable. Each marker in panel A denotes the number of individuals in year t whose Name Frequency falls in the interval indicated in the horizontal axis. Panel B plots the fraction in place of the number. Here, we treat all English variants of an Urdu name as distinct names. For example Muhammad, Mohammad, Mohammed, and Muhammed are treated as distinct names.

Figure A.II: Special Immigration Counter for TPHC Holders



Notes: The figure shows the picture of special immigration counter at the Allama Iqbal International Airport, Lahore. The picture was taken in the summer of 2018.

Table A.I: Heterogeneity in Response to the Public Disclosure Program

	Top City	Business in Other City	No. of Businesses	Female	Early Filer	Young	Buncher	Dominated	Revised Return
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
treat × after	0.066	0.068	0.090	0.085	0.075	0.050	0.083	0.088	0.089
	(0.006)	(0.007)	(0.005)	(0.005)	(0.009)	(0.011)	(0.007)	(0.005)	(0.005)
treat × trait × after	0.032	-0.007	-0.068	0.052	0.017	-0.018	0.004	0.003	-0.019
	(0.010)	(0.021)	(0.016)	(0.038)	(0.014)	(0.017)	(0.010)	(0.025)	(0.051)
Baseline Coefficient	0.088	0.068	0.088	0.088	0.081	0.049	0.088	0.088	0.088
	(0.005)	(0.007)	(0.005)	(0.005)	(0.007)	(0.008)	(0.005)	(0.005)	(0.005)
Observations	2,767,938	1,780,777	2,767,995	2,763,734	1,628,762	1,329,391	2,792,270	2,792,270	2,792,270
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table explores heterogeneity in response to the public disclosure program. We estimate a triple-difference version of equation (5) to see how the response varies across taxpayers of different traits. Treatment here is defined as an individual whose Name Frequency does not exceed 40, so the estimates correspond to the specification in Column (7) of Table II. To avoid making strong functional form assumptions all traits are introduced into the equation nonparametrically, as dummy variables. The dummy variable in the first column indicates if the taxpayer belongs to Karachi, Lahore, or Islamabad; in the second column if the taxpayer has business in a city different from the one he resides in; in the third column if the taxpayer has more than one businesses; in the fourth column if the taxpayer is a female, in the fifth column if the taxpayer routinely files her return before the median filing date; in the sixth column if the taxpayer is younger than the median tax filers; in the seventh column if the taxpayer bunched at any of the notches in the 2006-09 tax system of Pakistan; in the eighth column if the taxpayer was in a dominated region above any of the notches; and in the final column if the taxpayer filed a revised return in any of the pre-reform periods. We do not observe some of the traits for the whole sample. The Baseline Coefficient reports the the treat × after coefficient in equation (5) for the restricted sample for which we observe the trait. Standard errors are in parenthesis, which have been clustered at the individual level.

Table A.II: Heterogeneity in Response to the Public Disclosure Program (Placebo)

	Top City	Business in Other City	No. of Businesses	Female	Early Filer	Young	Buncher	Dominated	Revised Return
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
treat × after	0.002	0.001	0.010	0.012	-0.000	-0.016	0.002	0.014	0.014
	(0.008)	(0.009)	(0.007)	(0.006)	(0.009)	(0.014)	(0.008)	(0.006)	(0.006)
treat × trait × after	0.003	-0.011	-0.012	-0.001	0.021	-0.017	0.025	-0.001	0.070
	(0.013)	(0.026)	(0.019)	(0.044)	(0.013)	(0.021)	(0.013)	(0.030)	(0.058)
Baseline Coefficient	0.015	-0.001	0.015	0.015	0.011	-0.019	0.014	0.014	0.014
	(0.006)	(0.008)	(0.006)	(0.006)	(0.007)	(0.010)	(0.006)	(0.006)	(0.006)
Observations	1,484,133	917,213	1,484,174	1,482,108	1,430,873	574,137	1,496,374	1,496,374	1,496,374
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table illustrates the internal validity of our response heterogeneity analysis. We replicate Table - - but estimate our model on the pre-reform years (2006-2011) only, defining the last two years as the $after$ years.

Table A.III: Structure of Pakistani Legislature

House	Total Seats	Directly Elected	Reserved		
			Women	Minorities	Technocrats
(1)	(2)	(3)	(4)	(5)	(6)
National Assembly	342	272	60	10	-
Senate	104	66	17	4	17
Punjab Assembly	371	297	66	8	-
Sind Assembly	168	130	29	9	-
KP Assembly	124	99	22	3	-
Balochistan Assembly	65	51	11	3	-
Total	1174	915	205	37	17

Notes: The table shows the composition of the Pakistani legislature. National Assembly and Senate are the two houses at the Federal level. Pakistan has four provinces: Punjab, Sind, Khyber Pakhtoonkhwah (KP), and Balochistan. Each province has its own legislature. The legislative powers are divided between the federation and provinces by the constitution. Seats are reserved for women and religious minorities (non-Muslims) in every house and for technocrats in Senate. Reserved seats are filled through a proportional representation system.

Table A.IV: Response to the Public Disclosure – MPs (Tax Filing)

	Dependent Variable: Filed in year t			
	(1)	(2)	(3)	(4)
1.(year $\geq$ 2012)	0.592	0.588	0.618	0.587
	(0.007)	(0.010)	(0.009)	(0.007)
1.(year $\geq$ 2012) $\times$ ruling party		0.008		
		(0.014)		
1.(year $\geq$ 2012) $\times$ federal			-0.065	
			(0.014)	
1.(year $\geq$ 2012) $\times$ tightly contested				0.082
				(0.028)
Constant	0.313	0.309	0.278	0.318
	(0.005)	(0.008)	(0.007)	(0.006)
Observations	12,300	12,300	12,300	12,300

Notes: The table explores heterogeneity in MPs' extensive margin response to the public disclosure program. We estimate a linear probability model. The outcome is a dummy variable, indicating if MP i files a tax return in period t. In a world with full compliance, every MP files a tax return and the coefficient on the post-program dummy $1.(\text{year} \geq 2012)$ would be insignificant. Column (1), however, shows that only around one-third of MPs were filing tax returns prior to the disclosure. The filing rate jumped by around 60 percentage points after the disclosure. The jump was marginally higher for the ruling party and federal MPs but not for ones facing tight contests. Ruling Party here denotes the party that formed the federal or provincial government the MP belongs to. The dummy Tightly Contested takes the value 1 if the difference between the winning and runner-up candidates is less than 2% of the valid votes. Robust standard errors are in parenthesis.

Table A.V: Response to the Public Disclosure – MPs (Tax Paid)

	Complete Panel				Balanced Panel			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treat × after	0.407	0.489	0.401	0.399	0.651	0.653	0.626	0.656
	(0.069)	(0.108)	(0.100)	(0.070)	(0.097)	(0.164)	(0.136)	(0.099)
treat × after × ruling party		-0.154				-0.003		
		(0.140)				(0.200)		
treat × after × federal			0.012				0.054	
			(0.138)				(0.193)	
treat × after × tightly contested				0.181				-0.141
				0.406				0.522
Observations	5,832,527	5,832,527	5,832,527	5,832,527	1,304,247	1,304,247	1,304,247	1,304,247
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table explores heterogeneity in MPs' intensive margin response to the public disclosure program. We estimate a triple-difference version of model (5), adding the interaction terms shown above. Columns (1) and (5) reproduce the corresponding column in Table V. The other columns add interaction terms to these baseline specifications. None of these interaction terms, however, returns a statistically significant coefficient. Ruling Party here denotes the party that formed the federal or provincial government the MP belongs to. The dummy Tightly Contested takes the value 1 if the difference between the winning and runner-up candidates is less than 2% of the valid votes. Standard errors are in parenthesis, which have been clustered at the individual level.

Table A.VI: Response to the TPHC Program (By Taxpayer Category)

	Treat: Rank $\in (80,\ 120]$							
	Self-Employed		Wage-Earners		Partnerships		Corporations	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Main Regression (2006-2014)								
treat × after	0.067	0.169	0.137	0.209	-0.025	0.029	0.452	0.300
	(0.196)	(0.276)	(0.159)	(0.188)	(0.098)	(0.119)	(0.127)	(0.157)
treat × 1.(year ∈ {2009,2010})		0.231		0.173		0.120		-0.387
		(0.278)		(0.258)		(0.116)		(0.225)
Observations	7,619	7,619	7,914	7,914	8,185	8,185	8,329	8,329
B: Placebo Regression (2006-2010)								
treat × after	0.231		0.173		0.120		-0.387	
	(0.278)		(0.258)		(0.116)		(0.225)	
Observations	3,993		4,241		4,420		4,554	

Notes: The table breaks down the TPHC response by taxpayer category. We estimate equation (7) separately for each category of taxpayers. These categories are indicated in the title of each column. The sample for each regression includes top 1000 taxpayers of the corresponding category in each year included in the sample. The treatment variable here denotes taxpayers of the category ranked 81-120 in the given year. Given that we measure the outcome variable here in changes rather than levels, the first post-program year is 2011. Panel A estimates the equation on years 2006-2014. Panel B runs parallel placebo regressions on years 2006-2010, with the last two years defined as the post-program years. Columns (2), (4), (6) and (8) test the parallel trend assumption by including a $treat \times 1.(year \in \{2009, 2010\}$ interaction into the regression. Standard errors are in parenthesis, which have been clustered at the individual level.