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

Abstract

We examine two Pakistani programs to explore the role of deterrence as well as social and psychological factors in the tax compliance behavior of agents. In the first of these programs, the government began revealing income tax paid 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. We find that both public disclosure and social recognition of top taxpayers caused a substantial increase in tax payments. We explore the drivers of this behavior, including the shift of social norms toward compliance.

JEL-Codes: H240, H250, H260.

Keywords: tax evasion, income tax, social norms.

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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 how important they are and whether 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 paid 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 paid 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 dinners 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 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. The numeric identifier is effectively private information, known primarily to the agent and tax administration. 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 program. We provide two sets of tests to rule out this and related concerns. First, we show through both visual and regression-based evidence that the tax payments of the compared groups were trending similarly in the six pre-program periods: the relative difference in the outcome was indistinguishable from zero for virtually all these 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 9 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). Along the extensive margin, the program caused a 1-2 log points increase in tax filing by individuals with less common names relative to others. Second, the disclosure had a far stronger impact on MPs. The tax liability reported by them surged by more than 40 log points, and their tax filing rate jumped up by around 60 percentage points, from around 30% to more than 90%. The stronger response from MPs is not surprising. 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 more 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 17 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 analysis convincingly shows that the public disclosure caused a substantial increase in tax compliance. This increase could be due to a perception that the chance of evasion being detected has increased. It could also be due to a shift in social norms towards compliance. We evaluate the latter possibility in two ways. First, we exploit the spatial element of our data and categorize neighborhoods on the basis of their demographic makeup as "more compliant" or "less compliant". We then investigate how the dynamics of the public disclosure response varies across the two categories. We find that both types of neighborhoods were very similar initially, but over time the tax payments of bottom taxpayers in the more compliant neighborhoods started growing relative to bottom taxpayers in the less compliant neighborhoods, while the tax payments of top taxpayers stayed almost the same.[1] This catch-up pattern of response is consistent with a model in which the presence of compliant peers creates pressure on noncompliant ones to conform (Bursztyn & Jensen, 2015). Second, we investigate if the electorate rewarded or punished MPs in the next general election, held in July 2018, on the basis of their tax payments, which became public information from 2012. We find a strong, positive association between tax payment and electoral success. Taken together, the two pieces of evidence suggest that the public disclosure and TPHC programs may also have initiated a shift of the social equilibrium toward compliance.

[1] We define top taxpayer as someone who is in the top quartile of the tax liability distribution in the baseline year i.e. 2012. The rest of the taxpayers are treated as bottom taxpayers.

Our results have important policy implications. The programs we study cost little in terms of economic resources. Thus, to the extent that they are effective in influencing both private and social behavior, they potentially offer a cost-effective complement to the standard measures 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.[2]

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 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, 2018b,c), to our knowledge there still does not exist any study of social and psychological motivations in tax payments from a developing country perspective.

[2]Issues created by the public disclosure of tax payments are discussed at more length in Lenter *et al.* (2003); Blank (2014). In addition to privacy, public disclosure can expose affluent individuals to unwarranted and even dangerous attention, as well as pleas from relations and peers to share their affluence.

The Pakistani public tax disclosure program has been studied in one recent political science paper. Malik (2017) investigates the impact of the program 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 income tax paid by every taxpayer in the country.[3] 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

[3]Tax paid here refers to the self-assessed tax liability reported by a taxpayer in its annual income tax return, which includes any tax withheld at source. Pakistani tax code requires that this self-assessed tax liability should be deposited into the treasury at the time of filing of return. For this reason, we use the terms tax paid and tax liability interchangeably in this paper.

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.[4]

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.[5] 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 corporations, 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 with the 2013 tax year data and thereafter, both of which are effectively private information of agents.[6] Therefore, the only information through which an observation in the directory can be readily linked to a taxpayer is the name.[7] In contrast, the direc-

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

[5]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".

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

[7]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 may still not be sufficient. A taxpayer's address may have changed since they first registered for an NTN or it may not be public information. Additionally, there is a significant effort cost of obtaining the information and it is increasing in the commonness of the taxpayer's name. 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. The effective disclosure intensity therefore is still linked

tory 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 from 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.[8] 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.[9]

As we note above, the MPs' directory also contains the constituency number they serve. Table A.I reports the composition of the Pakistani legislature. Because the country has a limited number of MPs, their identities are well known, especially in their electoral constituencies. Their exposure to the program therefore must be independent of how common their name is. We use this feature of the program as a specification check on our empirical strategy.

Both sets of directories receive wide coverage in the Pakistani media, especially at the time they are released. For example, simple Google searches of "FBR Tax Directory" and "Parliamentarians Tax Directory" looking for the occurrence of these words as exact phrases return 1,010 and 8,560 results.[10] It means that there are at least 1,010 (and potentially many more)[11] active web pages that discuss the two sets of Pakistani tax directories. This profusion of information creates a strong first stage in our setting in the sense that many Pakistani taxpayers are aware that their dis-

primarily to the commonness of the taxpayer's name.

[8] Generally, a majority of tax returns are filed in the last few weeks before the due date. Consistent with this trend, more than 90% of the 2012 returns in our data were filed in or after October 2013.

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

[10] This data was accessed on May 28, 2019 in Manchester, UK.

[11] Similar Google searches looking for the occurrence of "FBR Tax Directory" and "Parliamentarians Tax Directory" not as exact phrases return 169,000 and 160,000 results, suggesting that there are potentially many more active web pages that discuss the two sets of directories.

closed tax data would remain available online for the foreseeable future and could be accessed anytime by their peer networks. Note that the income tax exemption threshold in Pakistan, like other developing countries, is quite high, set at around the 80th percentile of the income distribution (Waseem, 2018a). Income taxpayers in the country are a richer segment of the population and therefore they and their peer networks are extremely likely to be exposed to the disclosed information, be it online or in other formats.

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) wage-earners, (b) self-employed 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 dinners held on Pakistan Day (23rd March) and Independence Day (14th August); (3) fast-track immigration through special counters (Figure A.I 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,[12] 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 also focus on who are the top tax-

[12] 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."

payers 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.

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's (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 adjusting these spelling variations does not change our results materially, we provide the corresponding raw distributions in Figure A.II (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.I 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.

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 (2018a), for example, estimates the compliance rate of self-reported income in Pakistan to be around 50%,[13] considerably

[13] Waseem (2018a) reports the evasion rate as a fraction of reported income. Here we convert the

larger than the 13% compliance rate predicted by the model for a plausible estimate of the coefficient of relative risk aversion ($\gamma = 3$).[14] 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 often use quite sophisticated selection models to target their audits. These 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.[15] 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 are behavioral biases whereby 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:

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

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. We decompose the detection probability

estimate into a fraction of real income to make it comparable with the simulations of the Allingham & Sandmo Model.

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

[15]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."

perceived by the agent p into two parts: ρ represents the real probability of detection and φ the factor by which the real detection probability is mis-estimated. The FOC of the extended model is:

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

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 composite detection probability $\varphi\rho$ now reflects the behavioral biases φ. The comparative statics of the problem are straightforward. Evasion decreases with φ, ρ, g, and s as long as the marginal utility of consumption is diminishing (risk aversion).[16]

The public disclosure program we examine potentially affects each of these four parameters. By facilitating whistle-blowing, it arguably raises both the real and perceived likelihood of detection. 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. Pakistan, however, did not have a formal whistleblowing regime for most of the period we study. Whistleblowing was incorporated into the Pakistani tax code in July 2015 and the necessary rules for this purpose were issued on May 5, 2016. While we cannot rule out *informal* whistleblowing, the absence of a formal whistleblowing regime in our setting considerably weakens the possibility that the real detection probability ρ rises as a result of the disclosure. We therefore believe that any response arising out of the disclosure would largely reflect moral costs (g and s) and the behavioral bias (φ). These factors reinforce each other. We, therefore, expect the public disclosure 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 position (Veblen) good, deriving utility from being seen as one of the richest in the country (Akerlof & Kranton, 2000).[17] 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 pro-

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

[17] 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).

gram. 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 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

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

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. In this and all subsequent specifications, we cluster standard errors at the individual level, the most aggregate level feasible in our setting (Abadie *et al.*, 2017; Bertrand *et al.*, 2004).

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

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

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. In our final test, we estimate equation (5) on the pre-program periods only (2006-2011), pretending as if the program 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 detection. 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.

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, 2018a). If the public disclosure program curtails tax evasion, the effect would be the strongest for this section of the population. 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:

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

where λ_t are the 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. The TPHC program was announced before the beginning of the tax year 2012. To respond to the program, however, the taxpayers needed to know their rank. We assume this was not possible before the publication of the first set of public disclosure data. For this reason, we consider 2013 as the first post-program year. We estimate equation (7) 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.

III.D 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 (corporation, 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 tax paid at source (called "final tax paid" in the Pakistani tax code). 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.

Table II presents summary statistics of our sample of self-employed individuals. Treatment group comprises individuals whose Name Frequency does not exceed 40. We first compare five moments of the distributions of taxable income, tax paid on taxable income, and tax paid at source for the two pre-program years across the treatment and control samples. In subsequent rows, we compare the mean of nine taxpayer traits across the two groups. Traits in rows 4-6 capture intensity of the program. Since the program was rolled out electronically, taxpayers in cities with greater internet access were more exposed to it. On the other hand, taxpayers with multiple businesses or with business in a city different from the city of residence were less exposed as linking the disclosed tax to the observed lifestyle is harder in such situations. Rows 7-9 of the table explore variation in risk aversion across the two groups. Early filers are expected to be more and males and young less risk averse than their counterparts (Borghans *et al.*, 2009; Albert & Duffy, 2012). And finally, rows 10-12 compare the knowledge of and responsiveness to taxation among the two groups.

Rows 1-3 of the table show that the two groups are fairly evenly distributed across the taxable income and the two tax-paid distributions. But, as expected, taxpayers with more unique names are different from the others along a few dimensions. For example, they are more likely to reside in a major city and less likely to be male or old. In our empirical strategy, these fixed traits are absorbed by the individual fixed effects. Table III explores if conditioning on these fixed effects removes the correlation between the treatment and the outcome of interest. We estimate a triple-difference version of model (5) on the pre-program years (2006-2011) only, pretending 2010-11 to be the post-program years. Clearly, the outcome is not correlated with the name-uniqueness once the individual fixed effects are included in the model. None of the triple-interaction coefficients in the nine specifications is significant at the conventional level in either the complete or the balanced panel sample.

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.

IV Effects of the Public Disclosure Program

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

IV.A All Taxpayers

IV.A.1 Intensive Margin

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 from the equation. 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 strongly support our empirical strategy. There are almost no pre-existing 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 treatment effect in 2013, the second year of the program. Although we cannot test it formally, we believe that the dip results from a mass media campaign launched by the Pakistani tax administration in 2014 to increase voluntary tax compliance in the country. The campaign began in mid-September and continued till October 31st, shortly before the deadline to file the 2013 tax return (Cyan *et al.* 2017).[18] During the campaign, the administration took out advertisements in television, radio, and newspapers and sent out mobile phone text messages telling prospective taxpayers how easy it was to file taxes and how important doing so was for national development. We feel that this campaign could conceivably have nudged even the control group taxpayers to increase their tax payments, reducing the gap between the two groups. No campaign of comparable intensity was launched in any other tax year.

Regression Results—Table IV 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 (Bertrand *et al.*, 2004).

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.

[18]The tax year 2013 in our paper refers to the year that runs from July 2013 to June 2014. Cyan *et al.* (2017) refer to it as the tax year 2014 in their paper.

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 pre-existing 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 V 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) illustrate 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 self-employed taxpayers having more distinct names remitting significantly more tax. This effect is strongest at the left tail of the distribution, containing 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 V 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 another check on our empirical strategy, we now 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, publicly-known identifiers. Table A.II presents the results. We replicate Table IV, 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.

Our baseline specification defines Name Frequency as the number of times a full name appears in the four years of disclosed data (2012-2015). There is a concern that this definition may conflate the true population measure of the uniqueness of a name with the return filing behavior. For example, the definition assigns the same value to a full name appearing four times in a single year or once every post-reform year. While this concern is mitigated by the fact that the distribution of names in our sample is extremely stable across years (see Figure I-B), we address it more directly in Table A.III. We now define Name Frequency as 4 $\times$ the number of times a full name appears in a given year's data. We multiply the number of occurrences of a full name in a given year's data by four to make this alternative definition more compatible with the one in our baseline specification. Unsurprisingly, we obtain very similar results.

Table A.IV shows the results of our final robustness check. We estimate equation (5) restricting the sample to self-employed taxpayers whose taxable income for the baseline year (2011) falls in the window indicated in the heading of the column. This check addresses the potential concern that taxpayers with common and uncommon names might be located in different areas of the income distribution and thus would be subject to different shocks. We have already shown in Table II that this is not the case, and that our treatment and control taxpayers are distributed fairly evenly across the taxable income distribution. The results in Table A.IV confirm this. Even when taxpayers having baseline income within a window of PKR 100k are compared, the tax paid by unique-named taxpayers goes up significantly after the program relative to the others, although no such difference existed prior to the program (see the placebo exercise in Panel B of the table). Another important finding shown in the table is that the response declines as we move up the taxable income distribution, becoming insignificant as the income approaches PKR 400k. This finding is consistent with the recent theoretical literature that argues that large/high-income taxpayers have far less ability to engage in tax evasion (see Gordon & Li, 2009; Kopczuk & Slemrod, 2006; Kleven *et al.*, 2016).[19]

[19]Existing empirical results are also consistent with these theoretical models. Waseem (2018a), for

Heterogeneity—Table A.V estimates a triple-difference version of model (5), exploring how the response varies across self-employed taxpayers with the nine traits listed in Table II. The first three of these traits, as we mention above, capture program intensity. The results are consistent with our expectations. Major-city residents with greater access to the internet and hence to the disclosed data respond more aggressively; multiple businesses owners, for whom there is greater ambiguity about their earnings, respond less aggressively. We do not observe either the residence or business city for roughly one-third of the population and very likely for this reason the triple-interaction coefficient in the second column, although of the expected sign, is insignificant. The next three columns of the table explore if the response varies with the likely correlates of the degree of risk aversion of a taxpayer.[20] The results of this exercise are inconclusive: all the triple-interaction coefficients are of the expected sign but insignificant. The last three columns of the table look for any variation in response across taxpayers with a varying degree of knowledge of or attention to the tax system or the ability to game the tax system. We find no differential response along these margins.

IV.A.2 Extensive Margin

Event Study—Public disclosure can also encourage tax filing by individuals with less common names. To probe this, we first present visual evidence. Figure III plots the log of number of self-employed filers in the treatment and control groups from year 2006 to 2015. We normalize the outcome variable in both groups to 1 in 2006 and track its evolution in the later years. As earlier, we consider four definitions of treatment indicated in the heading of each panel. To make the comparison more stark, we drop the middle portion of the distribution in Panels C-D as we did in Figure II. Plots show that the program did result in more filing by less-common-named taxpayers. This effect is qualitatively very similar to the intensive margin effect, although it is smaller in magnitude. The next section formalizes this result

example, finds that the evasion rate for the self-employed in Pakistan is around 74% at the bottom of the taxable income distribution but reduces to 6% as the income approaches PKR 350k. Because the response to the public disclosure program captures a reduction in tax evasion, it is not surprising that it becomes insignificant at the higher income levels.

[20]There is some evidence in literature that men and young are less risk-averse than their counterparts (Borghans *et al.*, 2009; Albert & Duffy, 2012). Similarly, individuals who habitually file their tax returns earlier than others are expected to be more risk averse.

using the regression framework.

Regression Results—Table VI reports the results from the following regressions

$$\log \mathrm{N}_{gt} = \alpha + \beta \text{ treat}_g + \gamma \text{ treat}_g \times \text{after}_t + \lambda_t + u_{gt}, \tag{8}$$

where N_{gt} is the log number of filers of group $g \in \{\text{treat}, \text{control}\}$ in year t. Columns (1)-(4) are constructed similarly to the corresponding columns of Table IV, while columns (5)-(7) correspond to the three specifications in Figure IIIB-D. Panel B of the table conducts a placebo exercise, where we estimate the above equation on the pre-program periods only, treating 2010-11 as the two post-program years. Consistent with the visual evidence, none of these placebo coefficients is significant at the conventional level, illustrating that tax filing was evolving similarly in the compared groups. After the program, however, the tax filing of less-common-named taxpayers goes up relative to the more-common-named taxpayers. The DD coefficient is statistically different from zero in all specifications, showing that the program increased filing by around 1-2%.

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 fear of detection. A priori, therefore, the program should have a stronger pro-compliance effect on them.

Intensive Margin Response—Table VII estimates the effect of the public disclosure on the tax liability reported by MPs. 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 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 V, 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.[21] In addition to this, MPs may also receive income 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.[22]

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.[23] 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

[21]The salary was fixed initially by the The Members of Parliament (Salaries and Allowances) Act, 1974. It is revised from time to time using the procedure laid down in the statute.

[22]One other event that occurred during our sample period and could affect the tax payments of MPs was the release of the Panama Papers in April 2016. In these papers, close family members of the sitting Prime Minister were named as owners of offshore companies. Although no other MP was named, the release could have led to increased tax payments by MPs fearing greater tax scrutiny. The tax directories for the years 2014 and 2015 were published after the release of the Panama Papers. We, however, do not see any evidence of a jump in the DD coefficients for these years, suggesting that the effect of the papers, if any, was not large (see Figure IV).

[23]One other consequence of the change in the composition of the sample in 2012 (see Figure V) 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.

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.

Table A.VI explores heterogeneity in MPs' response across five traits. Table A.VII reports the results from parallel regressions run on balanced panel samples, and Table A.VIII from placebo regressions run on the pre-program periods only. MPs belonging to the ruling party, serving the federal legislature, facing tight races, and holding federal cabinet positions are expected to be more sensitive to the disclosure than others. But the evidence on this point is not conclusive: only one of the four triple-interaction terms—federal minister—is statistically significant; the placebo regression, however, shows that the tax liability of this group of MPs was growing faster than others even before the disclosure program. MPs in our sample were elected in May 2013. Their response to the disclosure therefore potentially conflates the program effect with the effect of becoming an MP (income and hence tax liability of an MP may grow faster than others). To rule out this concern, we examine any differential response by individuals who were also MPs in the previous parliament. The triple-interaction coefficient in column (6) is negative but statistically indistinguishable from zero.

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 filing rate. The results of this investigation are shown in Figure V. It illustrates that only around 30% of MPs were filing their returns prior to the program. 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.IX, show that the filing rate on average increased by nearly 60 percentage points. The increase was significantly higher for MPs in more competitive races and lower for federal legislators and cabinet members; but these differences mostly reflect differences in the pre-treatment levels, as post-treatment all of the filing rates are very close to 100%.

V Effects of the TPHC Program

Figure VI provides non-parametric evidence on the effects of the TPHC program. The sample for this diagram includes corporations, partnerships, self-employed and wage-earning 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 three-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, 2012 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 VIII 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$\times 1.$(year $\in \{2010, 2011\}$) interaction term into it. The coefficient on the term loosely captures any differences in the pre-existing 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 (2006-2011), pretending that the program 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 A.X 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.XI 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 brands by advertising their status as one of the top taxpayers, translating the social recognition into higher sales and profits. Table A.XII evaluates this explanation by exploring response heterogeneity across firms. Strikingly, firms that are likely to be more sensitive to public opinion—public-limited firms[24] and firms engaged in consumer sectors such as banking, food, and textile—respond aggressively to the program. In contrast, firms who are foreign-owned, face inelastic demand (pharma), or do not operate in the consumer sector (construction) seem unaffected. Although not all of the estimated interaction terms are statistically significant, the overall pattern is consistent with both our expectations and similar evidence from other contexts showing that big firms, in particular those in the consumer sector, are relatively more sensitive to their public image, especially in issues involving social responsibility and taxes (see for example Hanlon & Slemrod, 2009; Bénabou & Tirole, 2010; Graham *et al.*, 2013).[25]

[24]Public limited firms are corporations whose shares can be bought and sold by the general public through the stock exchange. They are therefore more likely to care about their public image than private limited firms whose shares are not available to public.

[25]One complementary mechanism driving the higher response by corporations could be the fol-

VI Did the Two Programs Affect Social Norms?

Arguably one motivation of the government in introducing the public disclosure and TPHC programs was to instill and strengthen a culture of compliance, inculcating tax payment as a social virtue and tax evasion as a vice. We have shown above that the two programs influenced private behavior substantially. In this section we look at whether part of the response was due to a shift in social norms toward compliance.

VI.A Heterogeneous Social Pressure to Comply

To examine this question, we first exploit spatial heterogeneity in the public disclosure response, exploring if the dynamics of the response varies across more and less compliant neighborhoods. Neighborhood here denotes the subdistrict a taxpayer resides in. There are 1,145 subdistricts in our data, with a typical subdistrict containing 470 taxpayers. We define a compliant neighborhood in two different ways. First, columns (1)-(4) of Table IX treat a neighborhood with an above-median proportion of less-common-named wage earners as compliant. Because wage income is third-party reported, the extent of evasion on this type of income is severely constrained.[26] Neighborhoods with a large proportion of less-common-named wage-earners contain a large proportion of recognizably compliant taxpayers, and therefore arguably create stronger social pressure for compliance. In an alternative formulation, columns (5)-(8) of the table treat neighborhoods with an above-median proportion of top,[27] less-common-named self-employed as compliant.

Column (1) of the table shows that the compliant neighborhoods were quite similar to the others initially, but over time tax payments there started trending upwards. Column (2) demonstrates that this result is not driven by the neighborhoods in three major cities of the country, where the public disclosure response was

lowing. 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 benefit at the cost of shareholders.

[26] Waseem (2018a) estimates that the evasion of wage income in Pakistan is less than 1% of the reported income. This estimate is in line with the similar estimates from other countries (Slemrod, Forthcoming).

[27] We define top taxpayer as someone who is in the top quartile of the tax liability distribution in the baseline year i.e. 2012. The rest of the taxpayers are treated as bottom taxpayers.

stronger anyway (see column 1 of Table A.V). Columns (3)-(4) decompose the aggregate effect into two, looking at the behavior of top and bottom taxpayers within each neighborhood separately. The decomposition suggests that compliance indeed has an infectious element to it: tax payments of bottom taxpayers in more compliant neighborhoods increase over time, while those of top taxpayers stay almost the same. We see a similar *catch-up* pattern of response for our alternative definition of compliant neighborhood in columns (5)-(8).

VI.B Public Disclosure and Electoral Success

Recall that the public disclosure program was precipitated by the revelation that the country's political elites were delinquent in fulfilling their tax obligations. We next explore how the electorate reacted to the tax histories of politicians, which became public information as a result of the program. Pakistan had a general election in July 2018 in which 664 of the 915 directly-elected MPs in our sample took part. Nearly one-half of these MPs succeeded in retaining their seats. Table X investigates if this electoral success is associated with the disclosed tax payments. We regress an indicator that an MP wins the 2018 election on her tax payments as disclosed through the directories of 2012-2015. The sample includes only the directly-elected MPs and we normalize the RHS variable by its standard deviation. The results show that electoral success is indeed strongly positively associated with the level of tax payments (columns 1-7), although not with the change (column 8). The coefficient in column (1), for example, shows that paying one standard deviation higher tax in 2012 is associated with a roughly six percentage points higher probability of winning the 2018 election. This strong positive correlation persists if, instead of 2012, later years' tax payments or the maximum, minimum, or sum of the tax payments are used as regressors (columns 2-7). Because all the post-2012 tax payments are of a similar level (see Figures IV and V), it is not surprising that the coefficient on the change variable is smaller and insignificant (column 8).[28]

The association documented above could simply reflect heterogeneity in MP characteristics (richer MPs pay more tax and are more likely to win), program intensity (disclosure was more salient for federal and ruling party MPs), or electoral

[28] One other manifestation of this result is that if we put tax paid in each year from 2012 to 2015 as four separate regressors the coefficient on only one of the regressors is statistically different from zero.

swing. Table XI seeks to control for these potential confounders by introducing four sets of covariates into the baseline specification. The results show that the correlation remains strong even if we compare MPs belonging to the same political party, serving the same legislature, and having similar electoral performance in the 2013 election. Table A.XIII explores the influence of tax payments on five other electoral outcomes. While the success-related outcomes are all positively correlated with the disclosed tax payments (columns 1-3), non-success ones are not (columns 4-5). Finally, Table A.XIV investigates if reporting zero tax payment in any post-disclosure year reduces an MP's re-election probability. We generally obtain negative coefficients from these regressions, but because zero tax payments are rare (other than in the tax year 2012)[29], most of these coefficients are indistinguishable from zero.

Overall, the evidence in this section (Tables X-XI, and A.XIII-A.XIV) is consistent with the notion that the electorate rewarded higher tax payments. In combination with the evidence in the last section, it also suggests that the public disclosure and TPHC programs may have initiated a gradual shift of the social equilibrium toward compliance.

VII 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 tax liability reported 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 may also exacerbate the guilt and shame felt by 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 9 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

[29]MPs in our sample were elected to office in May 2013. The 2012 tax year runs from July 2012 to June 2013. Because MPs did not receive salary from the government for the complete year, they were more likely to report zero tax payments in 2012 than in any other year.

result of the program, the tax liability reported by treated taxpayers in the neighborhood of the program threshold went up by approximately 17 log points. Exploiting spatial dimension of our data, we document a catch-up pattern of response to the public disclosure, whereby tax payments of bottom taxpayers in more compliant neighborhoods increased over time. We also document a strong, positive correlation between the electoral success of MPs and their disclosed tax payments. These two pieces of evidence suggest that the public disclosure and TPHC programs may also have initiated a gradual shift of the social equilibrium toward compliance.

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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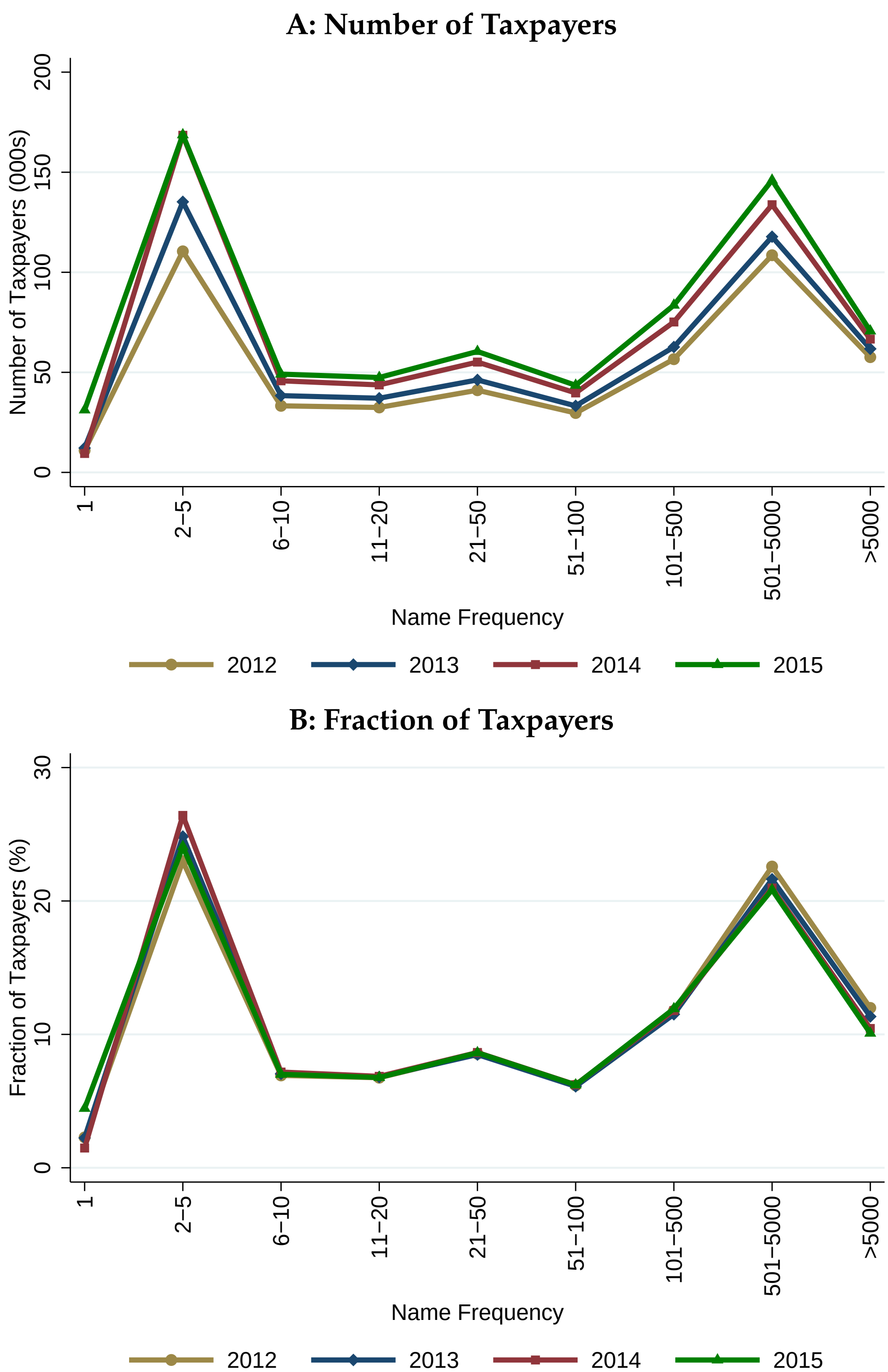
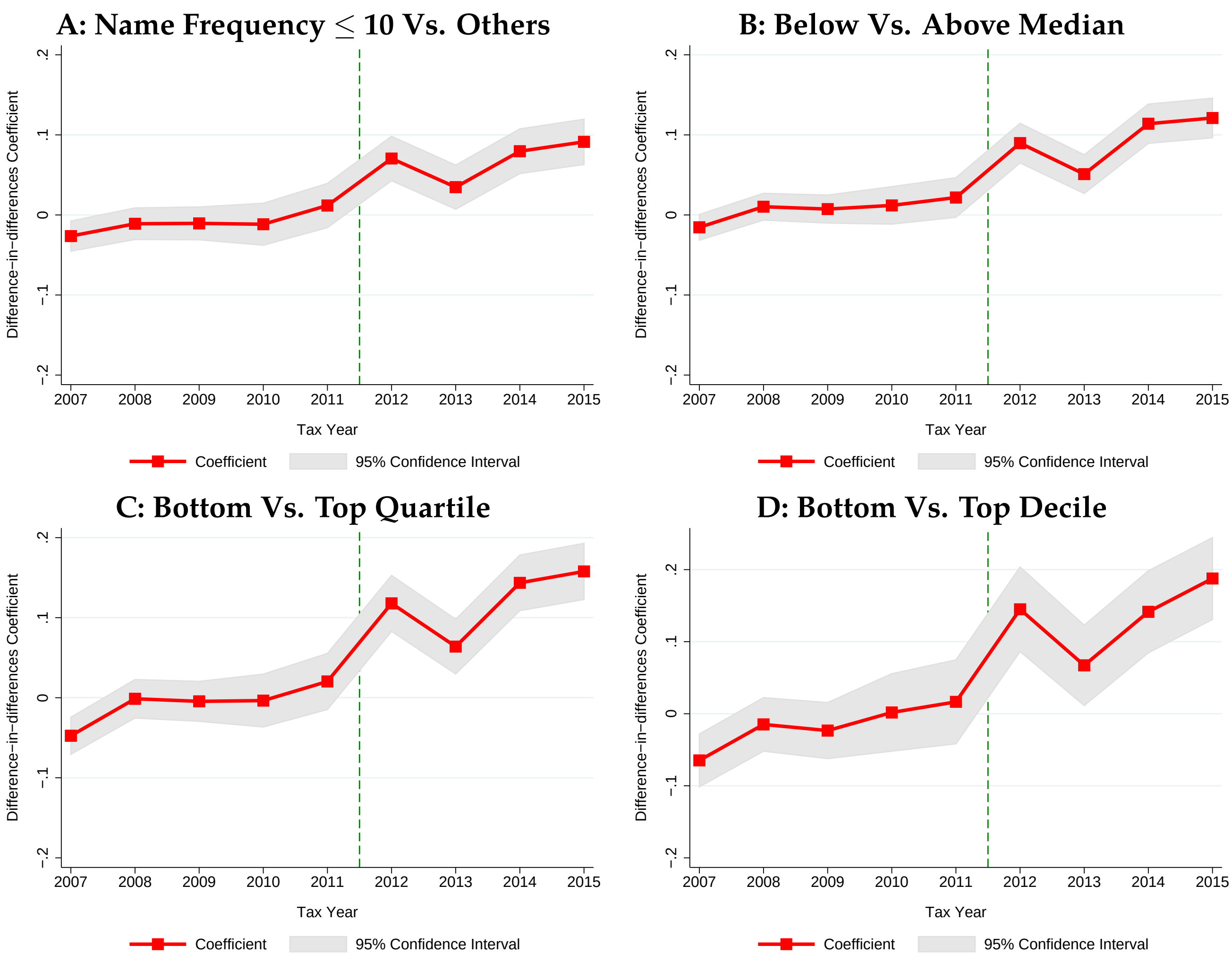
Figure I: **Distribution of Names**



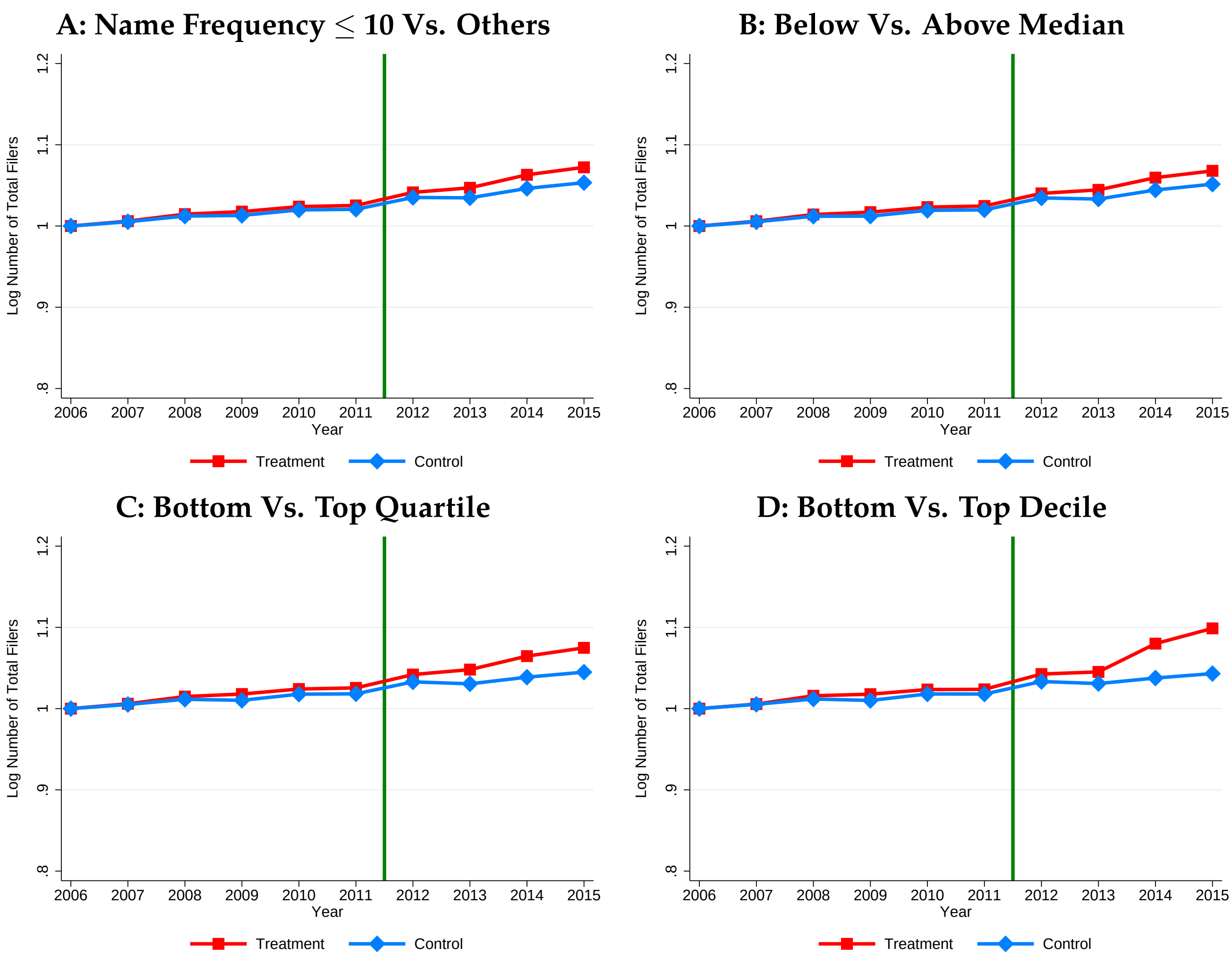
Notes: The figure illustrates the distribution of full names in Pakistan. We define Name Frequency as the number of times a full name appears in the disclosure data for the years 2012-2015. The Name Frequency of 4, for example, means that the full name appears four times in four years of data. The two 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 of taxpayers 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: Intensive Margin Response to the Public Disclosure Program



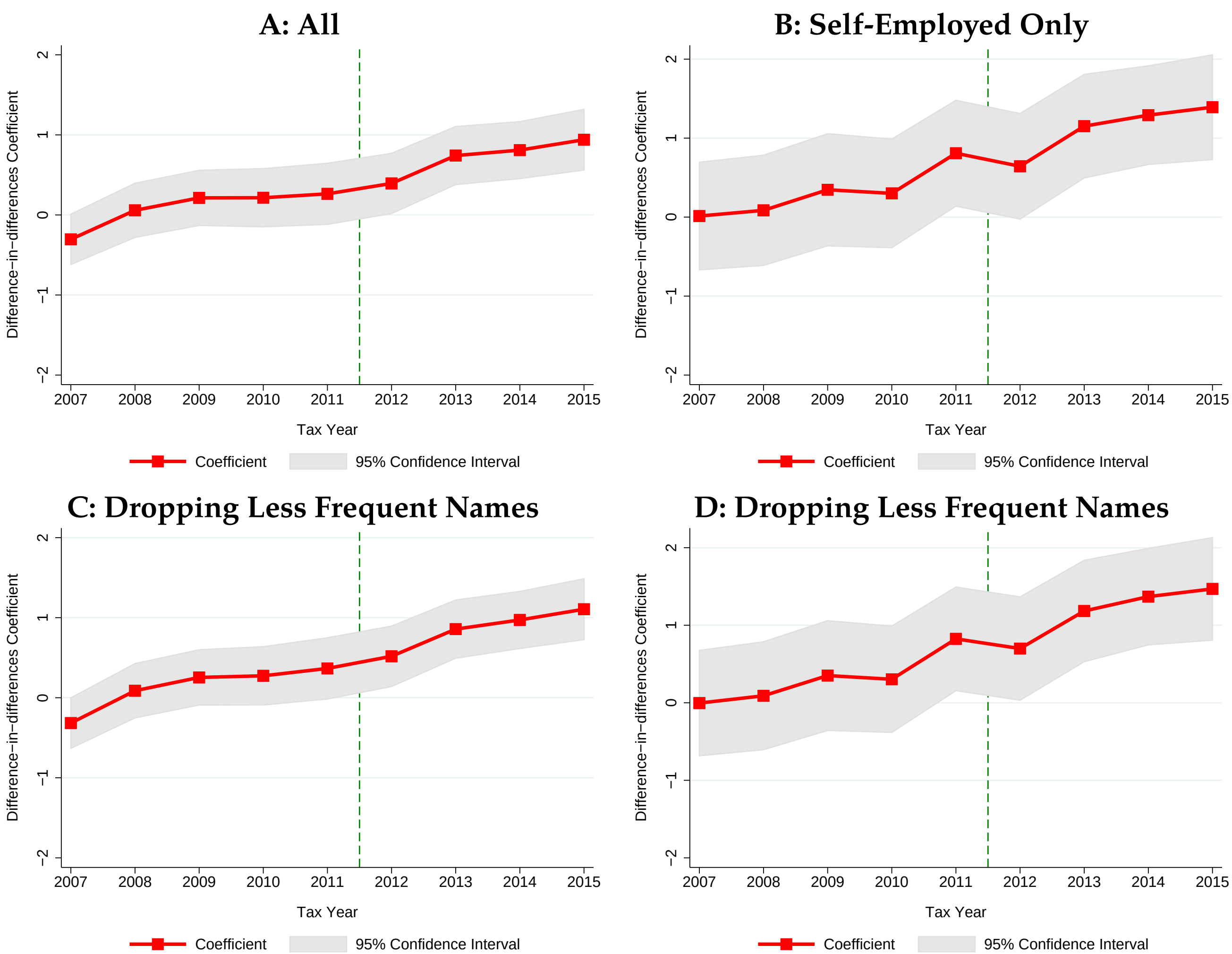
Notes: The figure plots the coefficients γ_js and 95% confidence interval around them from the event study equation (6). We estimate the equation on a balanced panel sample of self-employed taxpayers, who file in all years from 2006 to 2015. The definitions of the treatment and control groups are provided 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. The first decile, first quartile, median, third quartile, and top decile of the Name Frequency distribution are 4, 6, 76, 1853, and 6091, respectively. 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: EXTENSIVE MARGIN RESPONSE TO THE PUBLIC DISCLOSURE PROGRAM



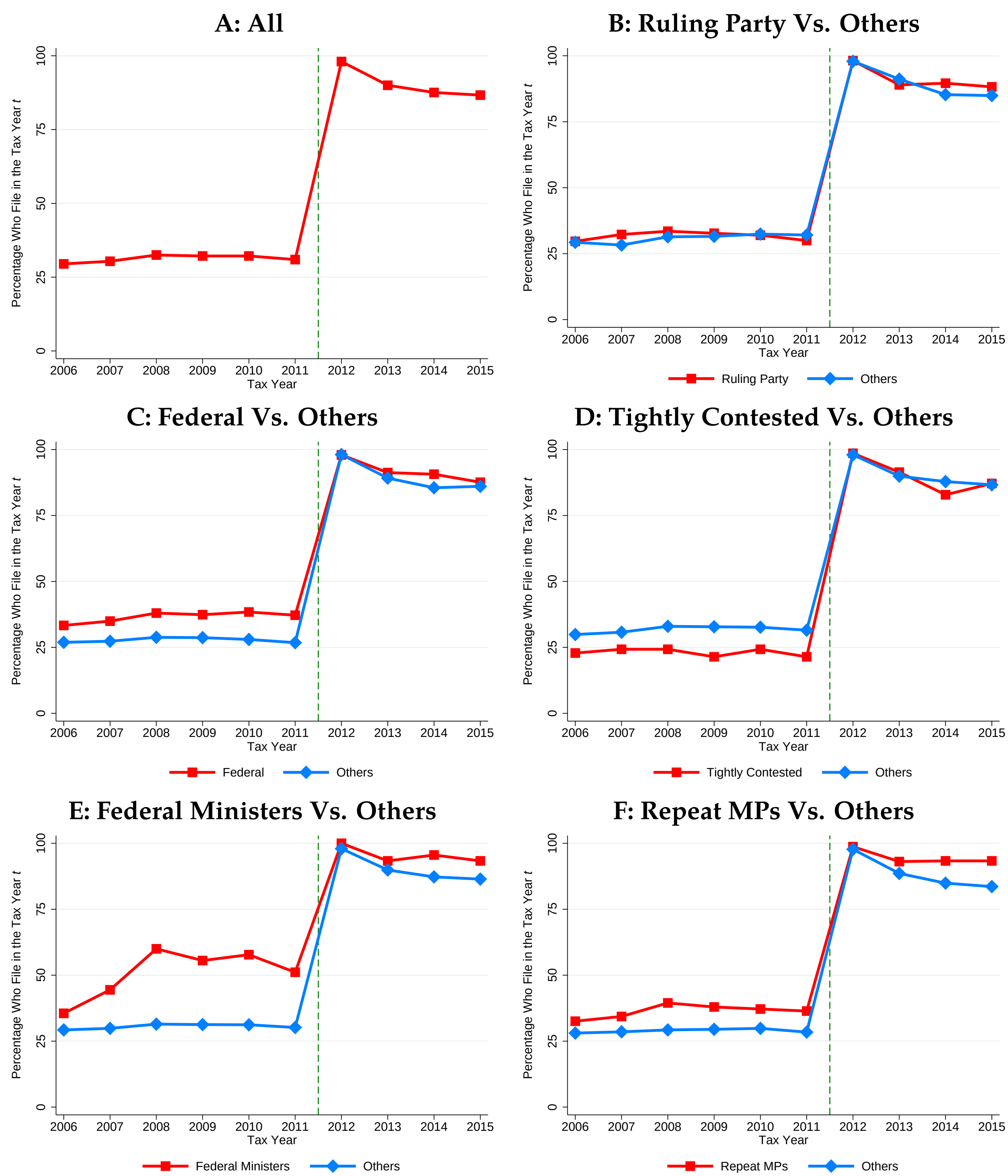
Notes: The figure plots the log of number of treatment and control self-employed tax filers from 2006 to 2015. We normalize the log of number of filers in each group to one in 2006 and track its evolution in the next nine years. The definitions of the treatment and control groups are provided in the 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 are considered as the control group. The first decile, first quartile, median, third quartile, and top decile of the Name Frequency distribution are 4, 6, 76, 1853, and 6091, respectively. 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. Vertical lines demarcate the time from which the public disclosure begins to have an effect on the tax paid by individuals.

Figure IV: Intensive Margin Response to the Public Disclosure Program – MPs



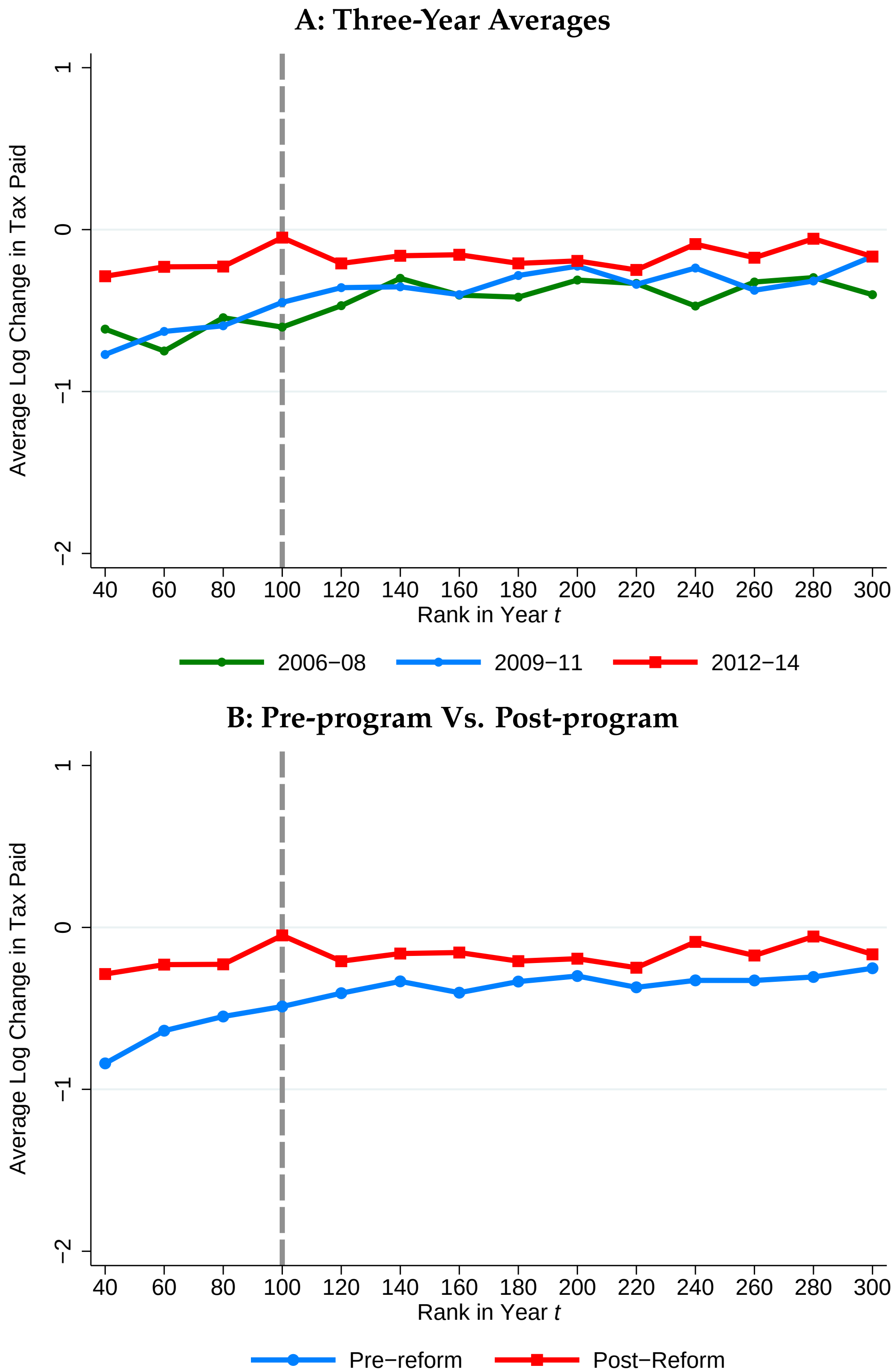
Notes: The figure displays how the tax paid by MPs reacts to the public disclosure program. The figure plots the coefficients γ_js and 95% confidence interval around them from the event study equation (6). We estimate the equation on a balanced panel sample of individuals who file in all years from 2006 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: Extensive Margin Response to the Public Disclosure Program – MPs



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. Federal Minister is an MP who has been a minister in the federal cabinet at any time during the period 2013-2018, including the Speaker and Deputy Speaker of the National Assembly. Repeat MP denotes an individual who has been a member of both the 2008-2013 and 2013-2018 parliaments of Pakistan. Vertical lines demarcate the time from which the public disclosure begins to have an effect on the tax filing of MPs.

Figure VI: **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 average over three-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 published. It publishes the data that formed the basis of earlier investigative reports, cataloging tax noncompliance of MPs elected 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	MPs' directory for tax year 2012 published
April 15, 2014	All taxpayers' directory for tax year 2012 published
June 30, 2014	Tax year 2013 ends
April 10, 2015	MPs' and all taxpayers' directories for tax year 2013 published
June 30, 2015	Tax year 2014 ends
June 30, 2016	Tax year 2015 ends
September 9, 2016	MPs' and all taxpayers' directories for tax year 2014 published
July 27, 2017	MPs' directory for tax year 2015 published
August 11, 2017	All taxpayers' directory for 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: Summary Statistics

	2011		2010	
	Treatment	Control	Treatment	Control
	(1)	(2)	(3)	(4)
1. Taxable Income:				
25th percentile	12.281	12.255	12.044	12.017
Median	12.560	12.516	12.304	12.255
Mean	12.505	12.459	12.306	12.248
75th percentile	12.723	12.680	12.554	12.497
90th percentile	12.899	12.766	12.766	12.612
2. Tax on taxable income:				
25th percentile	10.271	10.244	10.091	10.070
Median	10.521	10.494	10.337	10.264
Mean	11.064	11.015	10.737	10.567
75th percentile	11.845	11.884	11.081	10.531
90th percentile	12.848	12.613	12.520	12.155
3. Tax at source:				
25th percentile	9.502	9.517	9.287	9.259
Median	10.917	10.943	10.625	10.540
Mean	10.915	10.984	10.678	10.687
75th percentile	12.411	12.475	12.132	12.162
90th percentile	13.699	13.804	13.450	13.526
4. Major city	0.462	0.336	0.458	0.334
	(0.001)	(0.001)	(0.001)	(0.001)
5. Business in other city	0.123	0.123	0.123	0.123
	(0.001)	(0.001)	(0.001)	(0.001)
6. More than one businesses	0.158	0.131	0.157	0.129
	(0.001)	(0.001)	(0.001)	(0.001)
7. Male	0.919	0.986	0.924	0.986
	(0.001)	(0.000)	(0.001)	(0.000)
8. Early filer	0.615	0.642	0.554	0.543
	(0.001)	(0.001)	(0.001)	(0.001)
9. Young	0.545	0.507	0.521	0.485
	(0.002)	(0.002)	(0.002)	(0.002)
10. Buncher	0.049	0.054	0.044	0.046
	(0.000)	(0.000)	(0.000)	(0.000)
11. Strictly dominated choice	0.018	0.016	0.022	0.019
	(0.000)	(0.000)	(0.000)	(0.000)
12. Revised return	0.002	0.002	0.003	0.003
	(0.000)	(0.000)	(0.000)	(0.000)

Notes*:* The table presents summary statistics for the treatment and control groups of self-employed taxpayers. Treatment group comprises individuals whose Name Frequency does not exceed 40. We first compare five moments of the log of taxable income, tax paid on taxable income, and tax paid at source distributions for the two pre-program years across the two groups. Rest of the rows present the mean and standard error of nine taxpayer traits, all defined as dummy variables. The definitions of these dummy variables are provided in Appendix A.2 of the paper.

Table III: **Balance of Treatment Control Samples**

	Major City	Business in Other City	Multiple Businesses	Male	Early Filer	Young	Buncher	Dominated	Revised Return
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<u>A: Complete Panel (2006-2011)</u>									
treat $\times$ 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 $\times$ trait $\times$ 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)
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
<u>B: Balanced Panel (2006-2011)</u>									
treat $\times$ after	-0.007	-0.004	0.007	0.007	-0.001	-0.010	0.004	0.009	0.009
	(0.010)	(0.011)	(0.008)	(0.008)	(0.011)	(0.017)	(0.011)	(0.008)	(0.008)
treat $\times$ trait $\times$ after	0.023	-0.020	-0.016	-0.028	0.016	-0.038	0.010	0.027	0.060
	(0.016)	(0.034)	(0.024)	(0.058)	(0.016)	(0.026)	(0.015)	(0.034)	(0.064)
Observations	837,536	486,993	837,550	837,147	807,171	288,788	840,469	840,469	840,469
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes*:* This table illustrates that conditional on the individual fixed effects the evolution of our outcome variable is independent of taxpayer traits shown in the column headings, listed in Table II, and defined in A.2. We estimate a triple-difference version of model (5) on the pre-program years 2006-2011, defining the last two years as the $after$ years. The sample is all self-employed taxpayers. Treatment here is defined as an individual whose Name Frequency does not exceed 40. To avoid making strong functional form assumptions all traits are introduced into the equation nonparametrically, as dummy variables. The model includes a full set of double-interaction terms. Panel B reports the results for a balanced panel sample, where we include only the taxpayers who file in all years included in the sample.

Table IV: Intensive Margin Response to the Public Disclosure Program

	Treat: Name Frequency							
	≤ 10		≤ 20		≤ 30		≤ 40	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Main Regression (2006-2015)								
treat $\times$ 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 $\times$ 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 V: **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 intensive margin 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 VI: **EXTENSIVE MARGIN RESPONSE TO THE PUBLIC DISCLOSURE PROGRAM**

	Treat: Name Frequency						
	≤ 10	≤ 20	≤ 30	≤ 40	$\leq$ Median	$\leq$ 1st Quartile	$\leq$ 1st Decile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A: Main Regression (2006-2015)							
treat $\times$ after	0.0117	0.0106	0.0101	0.0097	0.0094	0.0163	0.0265
	(0.0027)	(0.0024)	(0.0023)	(0.0022)	(0.0022)	(0.0041)	(0.0089)
B: Placebo Regression (2006-2011)							
treat $\times$ after	0.0027	0.0027	0.0026	0.0025	0.0024	0.0038	0.0026
	(0.0018)	(0.0017)	(0.0017)	(0.0016)	(0.0016)	(0.0026)	(0.0027)

Notes: The table reports the estimates from equation (8). The equation is estimated on a sample of all self-employed individuals. The outcome variable here is the log number of filers in group g in year t. Panel A estimates the equation on 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 columns (1)-(4), we drop taxpayers with the Name Frequency between 10 and 40 in columns (1) to (3). In columns (6) and (7) we drop the middle part of the distribution: the middle two quartiles in column (6) and the deciles 2-9 in column (7). 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.

TABLE VII: **INTENSIVE MARGIN RESPONSE TO THE PUBLIC DISCLOSURE PROGRAM – 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 VIII: **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	0.166	0.138	0.171	0.161	0.136	0.126	0.140	0.128
	(0.075)	(0.077)	(0.062)	(0.064)	(0.054)	(0.055)	(0.048)	(0.049)
treat × 1.(year ∈ {2010,2011})		-0.163		-0.060		-0.058		-0.070
		(0.151)		(0.126)		(0.115)		(0.105)
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 2012. 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 $treat \times 1.(year \in \{2010, 2011\}$ interaction into the regression. Standard errors are in parenthesis, which have been clustered at the individual level.

Table IX: **Public Disclosure and Social Norms**

	Trait: Neighborhoods							
	With Proportion of Less-Common-Named Wage-earners Above the Median				With Proportion of Less-Common-Named Top Taxpayers Above the Median			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treat × after	0.080	0.079	-0.013	0.030	0.069	0.069	-0.022	0.031
	(0.007)	(0.008)	(0.013)	(0.009)	(0.007)	(0.007)	(0.014)	(0.008)
treat × trait × 2012	-0.005	-0.025	-0.043	-0.155	0.010	-0.034	-0.031	-0.256
	(0.011)	(0.012)	(0.016)	(0.015)	(0.011)	(0.012)	(0.017)	(0.016)
treat × trait × 2013	-0.027	-0.022	-0.040	-0.008	-0.028	-0.016	-0.008	-0.026
	(0.011)	(0.012)	(0.018)	(0.014)	(0.012)	(0.012)	(0.019)	(0.015)
treat × trait × 2014	0.037	0.040	0.012	0.081	0.047	0.056	0.027	0.115
	(0.011)	(0.012)	(0.018)	(0.014)	(0.012)	(0.012)	(0.019)	(0.015)
treat × trait × 2015	0.038	0.044	-0.011	0.104	0.048	0.058	-0.006	0.158
	(0.012)	(0.012)	(0.019)	(0.014)	(0.012)	(0.013)	(0.020)	(0.016)
Included Taxpayers	All	All	Top	Bottom	All	All	Top	Bottom
Major Cities Dropped	No	Yes	No	No	No	Yes	No	No
Observations	2,131,611	2,043,533	657,201	1,474,410	2,045,955	1,962,510	649,939	1,396,016

Notes: The table explores if the public disclosure program caused a shift in social norms towards compliance. We estimate an augmented version of equation (5) by adding the four $trait \times year$ interactions into the model. The dummy variable $trait$ represents a neighborhood with the characteristic shown in the column heading. Neighborhood denotes the subdistrict a taxpayer resides in. There are 1,145 subdistricts in our data with a typical subdistrict containing 470 taxpayers of whom 92 are less-common-named wage-earners. We define a taxpayer as a *top* taxpayer if it falls in the top quartile of the tax-paid distribution of the baseline year i.e. 2012. All even-numbered columns drop subdistricts located in Karachi, Lahore, and Islamabad. Columns (3)-(4) and (7)-(8) rerun the baseline model on the sample of top and bottom taxpayers only. Standard errors are in parenthesis, which have been clustered at the individual level.

Table X: **Public Disclosure and Electoral Success**

	Definition of Tax Paid:							
	Tax Paid in 2012	Tax Paid in 2013	Tax Paid in 2014	Tax Paid in 2015	Max Tax Paid	Min Tax Paid	Sum of Tax Paid	Diff of 2015 & 2012
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tax Paid	0.064	0.064	0.043	0.043	0.068	0.057	0.062	0.025
	(0.021)	(0.018)	(0.018)	(0.018)	(0.016)	(0.017)	(0.016)	(0.016)
Observations	478	702	734	738	838	838	863	863

Notes: The table explores if the electoral performance of MPs is correlated with their disclosed tax payments. We report results from Linear Probability Models where the outcome variable is an indicator denoting that MP i won a legislative seat in the 2018 election. The RHS variable is the log of tax paid by the MP (the exact definition of the variable is in the heading of each column). We normalize the RHS variable by its standard deviation. The sample includes all individuals who were directly elected to a legislative house in the 2013-2018 parliament of Pakistan. Robust standard errors are in parenthesis.

Table XI: **Public Disclosure and Electoral Success**

	Definition of Tax Paid:											
	Tax Paid in 2012						Tax Paid in 2015					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Tax Paid	0.064	0.061	0.058	0.051	0.053	0.050	0.043	0.059	0.071	0.061	0.064	0.060
	(0.021)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.018)	(0.019)	(0.021)	(0.021)	(0.022)	(0.022)
Observations	478	478	478	477	475	475	738	738	738	737	732	732
Controls:												
Party Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
House Fixed Effects	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
% Votes Obtained in 2013	No	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Winning Margin in 2013	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Federal Minister	No	No	No	No	No	Yes	No	No	No	No	No	Yes

Notes: The table replicates the specifications in column (1) and (4) of the above table (Table X). We introduce MP characteristics indicated in the last four rows sequentially into the model. Robust standard errors are in parenthesis.

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 standard issues in transliterating Arabic script 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.

In Urdu, 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 in English as either U OO OU or UO.

Secondly, in Urdu 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 repeated sound 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 on 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.

A.2 Definition of Variables

(*i*) **Major city.** The taxpayer reports an address in one of the three major cities—Karachi, Lahore, and Islamabad—of Pakistan.

(*ii*) **Business in other city.** The taxpayer conducts business in a city different from where he or she resides.

(*iii*) **Multiple businesses.** The taxpayer owns more than one businesses.

(*iv*) **Early filer.** The taxpayer files their return relatively early. The dummy variable takes the value 1 if the taxpayer filed their return for year t before the median filing date for the year.

(*v*) **Young.** If the taxpayer is younger than the median income tax filer for the year t.

(*vi*) **Buncher.** If the taxpayer reported income at or within a window of ten thousand PKR below any notch in the corresponding tax schedule.

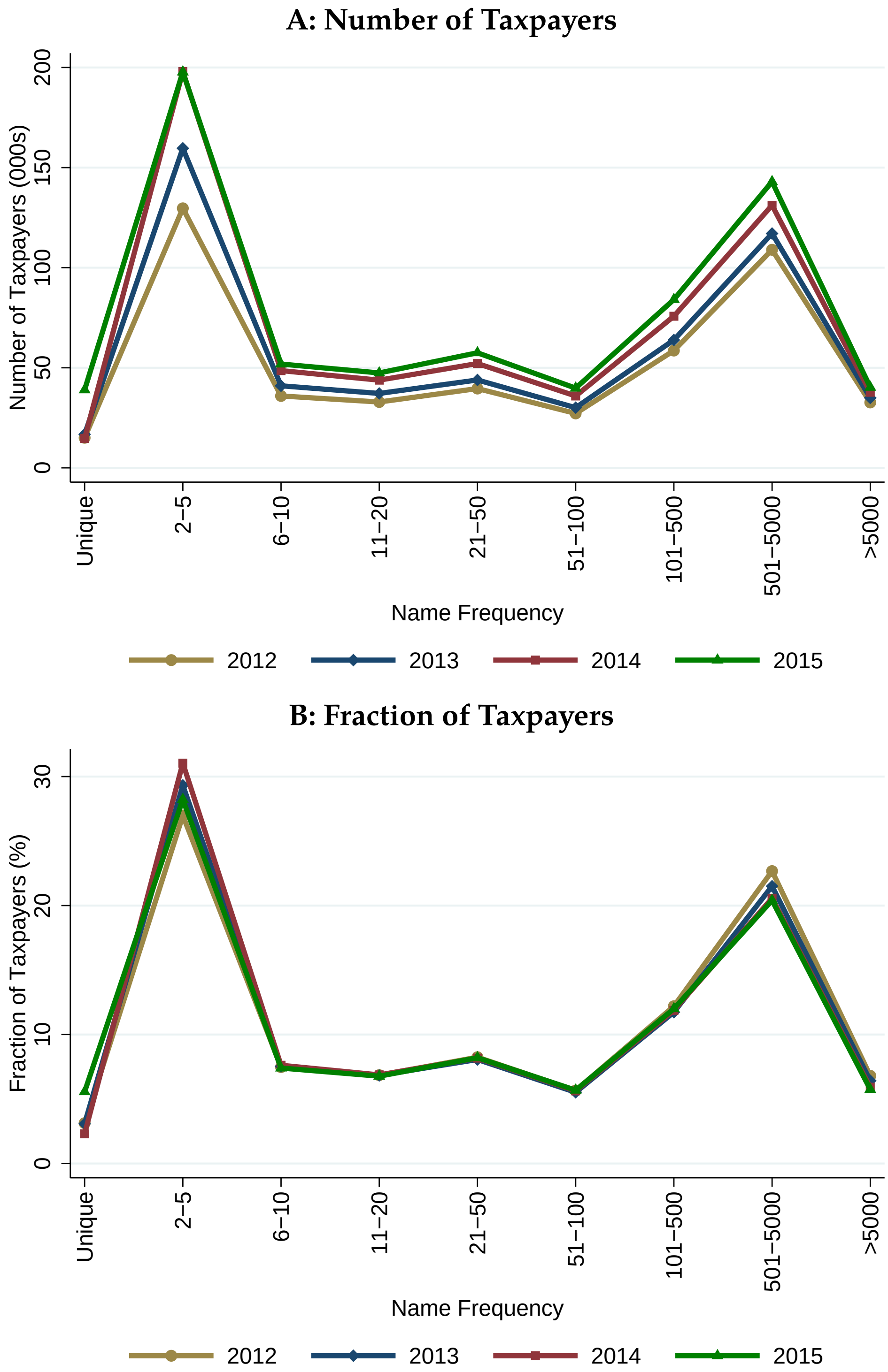
(*vii*) **Strictly dominated choice.** If the taxpayer reported income within the strictly dominated region above any notch in the corresponding tax schedule.

(*viii*) **Revised return.** If the taxpayer filed a revised return for the given tax year t.

Figure A.I: **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.

Figure A.II: **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.

Table A.I: **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.II: **Intensive Margin Response to the Public Disclosure Program – Placebo**

	Treat: Name Frequency							
	≤ 10		≤ 20		≤ 30		≤ 40	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: 2006-2015								
treat × after	-0.255	-0.302	-0.221	-0.227	-0.230	-0.254	-0.226	-0.235
	(0.181)	(0.233)	(0.179)	(0.229)	(0.179)	(0.228)	(0.178)	(0.227)
Observations	4,818	1,345	5,147	1,469	5,334	1,507	5,452	1,544
B: 2006-2011								
treat × after	-0.178	-0.183	-0.131	-0.093	-0.148	-0.119	-0.148	-0.121
	(0.183)	(0.245)	(0.182)	(0.245)	(0.180)	(0.243)	(0.179)	(0.242)
Observations	1,521	770	1,621	838	1,680	862	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 IV 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 A.III: **Intensive Margin Response to the Public Disclosure Program – Alternative Definition of Name Frequency**

	Treat: Name Frequency							
	≤ 10		≤ 20		≤ 30		≤ 40	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Main Regression (2006-2015)								
treat $\times$ after	0.098	0.094	0.093	0.092	0.092	0.091	0.091	0.088
	(0.006)	(0.009)	(0.005)	(0.008)	(0.005)	(0.008)	(0.005)	(0.008)
Observations	2,394,847	764,796	2,621,675	837,306	2,704,406	863,405	2,792,270	891,420
B: Placebo Regression (2006-2011)								
treat $\times$ after	0.014	0.010	0.018	0.014	0.018	0.014	0.017	0.013
	(0.007)	(0.008)	(0.006)	(0.008)	(0.006)	(0.008)	(0.006)	(0.008)
Observations	1,288,038	723,868	1,406,460	789,856	1,449,905	814,280	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). We replicate Table IV using an alternative definition of the variable Name Frequency. Instead of defining Name Frequency as the number of times a full name appears in the four years of disclosed data (2012-2015), we define it as 4 $\times$ the number of times a full name appears in the 2012 disclosed data. We multiply the number of occurrences of a name in 2012 by four to make this alternative definition of Name Frequency more compatible with the one in our baseline specification. Other than this change of definition, the table is constructed exactly similar to Table IV. We obtain similar results if we use any other post-disclosure year 2013-2015 in place of 2012 used here to define Name Frequency.

Table A.IV: **Intensive Margin Response to the Public Disclosure Program – By Baseline Taxable Income**

	Baseline Taxable Income:					
	$\in (0,\ 100k]$	$\in (100k,\ 200k]$	$\in (200k,\ 300k]$	$\in (300k,\ 400k]$	$\in (400k,\ 500k]$	$\in (500k,\ 600k]$
	(1)	(2)	(3)	(4)	(5)	(6)
A: Main Regression (2006-2015)						
treat $\times$ after	0.075	0.083	0.061	0.058	0.014	-0.026
	(0.059)	(0.018)	(0.009)	(0.010)	(0.028)	(0.056)
Observations	26,071	197,583	575,312	447,856	60,784	14,442
B: Placebo Regression (2006-2011)						
treat $\times$ after	0.058	0.019	0.005	-0.029	-0.072	-0.069
	(0.046)	(0.010)	(0.021)	(0.024)	(0.036)	(0.078)
Observations	44,234	760,496	104,403	38,149	21,214	5,214
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes*:* The table explores how the intensive margin response to the public disclosure program varies across the taxable income distribution. We replicate the specification in Column (7) of Table IV restraining the sample to taxpayers whose taxable income in the baseline year (2011) was within the interval indicated in the heading of each column. The treatment variable takes the value 1 if the Name Frequency of an individual does not exceed 40. 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. The baseline year for these regression is 2009. Standard errors are in parenthesis, which have been clustered at the individual level.

Table A.V: **Heterogeneity in Intensive Margin Response to the Public Disclosure Program**

	Major City	Business in Other City	Multiple Businesses	Male	Early Filer	Young	Buncher	Dominated	Revised Return
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
treat × after	0.066	0.068	0.090	0.137	0.075	0.050	0.083	0.088	0.089
	(0.006)	(0.007)	(0.005)	(0.038)	(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 the intensive margin 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 IV. 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 male, 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-program periods. We do not observe some of the traits for the whole sample. The Baseline Coefficient reports 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.VI: **Intensive Margin Response to the Public Disclosure – MPs**

	(1)	(2)	(3)	(4)	(5)	(6)
treat × after	0.407	0.489	0.401	0.399	0.371	0.491
	(0.069)	(0.108)	(0.100)	(0.070)	(0.072)	(0.091)
treat × after × ruling party		-0.154				
		(0.140)				
treat × after × federal			0.012			
			(0.138)			
treat × after × tightly contested				0.181		
				(0.406)		
treat × after × federal minister					0.514	
					(0.220)	
treat × after × repeat MP						-0.197
						0.137
Observations	5,832,527	5,832,527	5,832,527	5,832,527	5,832,527	5,832,527
Individual Fixed Effects	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) reproduces the corresponding column in Table VII. The other columns add interaction terms to these baseline specifications. 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. Federal Minister is an MP who has been a minister in the federal cabinet at any time during the period 2013-2018, including the Speaker and Deputy Speaker of the National Assembly. Repeat MP denotes an individual who has been a member of both the 2008-2013 and 2013-2018 parliaments of Pakistan. Standard errors are in parenthesis, which have been clustered at the individual level.

Table A.VII: **Intensive Margin Response to the Public Disclosure – MPs (Balanced Panel)**

	(1)	(2)	(3)	(4)	(5)	(6)
treat × after	0.651	0.653	0.626	0.656	0.581	0.705
	(0.097)	(0.164)	(0.136)	(0.099)	(0.099)	(0.126)
treat × after × ruling party		-0.003				
		(0.200)				
treat × after × federal			0.054			
			(0.193)			
treat × after × tightly contested				-0.141		
				(0.522)		
treat × after × federal minister					1.082	
					(0.323)	
treat × after × repeat MP						-0.136
						0.193
Observations	1,304,247	1,304,247	1,304,247	1,304,247	1,304,247	1,304,247
Individual Fixed Effects	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. The regressions are run on a balanced panel containing only the taxpayers who file in all years included in the sample. Columns (1) reproduces the column (5) in Table VII. The other columns add interaction terms to these baseline specifications. 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. Federal Minister is an MP who has been a minister in the federal cabinet at any time during the period 2013-2018, including the Speaker and Deputy Speaker of the National Assembly. Repeat MP denotes an individual who has been a member of both the 2008-2013 and 2013-2018 parliaments of Pakistan. Standard errors are in parenthesis, which have been clustered at the individual level.

Table A.VIII: **Intensive Margin Response to the Public Disclosure – MPs (Placebo)**

	(1)	(2)	(3)	(4)	(5)	(6)
treat × after	0.033	-0.065	0.033	0.043	-0.003	0.057
	(0.082)	(0.126)	(0.105)	(0.084)	(0.087)	(0.112)
treat × after × ruling party		0.192				
		(0.164)				
treat × after × federal			0.001			
			(0.164)			
treat × after × tightly contested				-0.282		
				(0.229)		
treat × after × federal minister					0.465	
					(0.222)	
treat × after × repeat MP						-0.051
						0.164
Observations	3,098,528	3,098,528	3,098,528	3,098,528	3,098,528	3,098,528
Individual Fixed Effects	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. The regressions are run on the pre-program periods only, defining 2010 and 2011 as the "after" years. Columns (1) reproduces the corresponding column in Table VII. The other columns add interaction terms to these baseline specifications. 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. Federal Minister is an MP who has been a minister in the federal cabinet at any time during the period 2013-2018, including the Speaker and Deputy Speaker of the National Assembly. Repeat MP denotes an individual who has been a member of both the 2008-2013 and 2013-2018 parliaments of Pakistan. Standard errors are in parenthesis, which have been clustered at the individual level.

Table A.IX: **Extensive Margin Response to the Public Disclosure – MPs**

	Dependent Variable: Filed in year t					
	(1)	(2)	(3)	(4)	(5)	(6)
1.(year $\geq$ 2012)	0.592	0.588	0.618	0.587	0.598	0.596
	(0.007)	(0.010)	(0.009)	(0.007)	(0.007)	(0.008)
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)		
1.(year $\geq$ 2012) $\times$ federal minister					-0.149	
					(0.035)	
1.(year $\geq$ 2012) $\times$ repeat MP						-0.013
						(0.014)
Constant	0.313	0.309	0.278	0.318	0.306	0.290
	(0.005)	(0.008)	(0.007)	(0.006)	(0.005)	(0.006)
Observations	12,300	12,300	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 significantly higher for MPs facing tight contests and lower for MPs of federal MPs and federal cabinet ministers. 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. Federal Minister is an MP who has been a minister in the federal cabinet at any time during the period 2013-2018, including the Speaker and Deputy Speaker of the National Assembly. Repeat MP denotes an individual who has been a member of both the 2008-2013 and 2013-2018 parliaments of Pakistan. Robust standard errors are in parenthesis.

Table A.X: 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	-0.001	0.027	0.054	-0.004	0.019	-0.021	-0.003
	(0.068)	(0.076)	(0.065)	(0.072)	(0.058)	(0.065)	(0.066)	(0.071)
treat × 1.(year ∈ {2010,2011})		0.079		0.083		0.065		0.054
		(0.098)		(0.085)		(0.081)		(0.093)
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 VIII, 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 2012. 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 $treat \times 1.(year \in \{2010, 2011\}$ interaction into the regression. Standard errors are in parenthesis, which have been clustered at the individual level.

Table A.XI: **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	-0.033	0.013	0.215	0.276	0.036	0.089	0.412	0.267
	(0.205)	(0.241)	(0.143)	(0.172)	(0.105)	(0.114)	(0.115)	(0.129)
treat × 1.(year ∈ {2010,2011})		0.130		0.176		0.144		-0.444
		(0.221)		(0.254)		(0.102)		(0.206)
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 2012. 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 $treat \times 1.(year \in \{2010, 2011\}$ interaction into the regression. Standard errors are in parenthesis, which have been clustered at the individual level.

Table A.XII: **Heterogeneity in Response to the TPHC Program**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treat × after	0.412	0.356	0.501	0.369	0.399	0.369	0.462	0.427
	(0.115)	(0.214)	(0.124)	(0.115)	(0.116)	(0.119)	(0.124)	(0.119)
treat × after × public		0.091						
		(0.255)						
treat × after × foreign owned			-0.793					
			(0.295)					
treat × after × banking				1.241				
				(0.718)				
treat × after × food					0.389			
					(0.583)			
treat × after × textile						0.114		
						(0.272)		
treat × after × pharma							-0.573	
							(0.233)	
treat × after × construction								-0.342
								(0.394)
Observations	8,329	8,329	8,329	8,329	8,329	8,329	8,329	8,329

Notes: The table explores heterogeneity in corporate firms' response to the TPHC program. We estimate the triple-difference version of model (7), adding the interaction terms shown above. Columns (1) reproduces column (7) of Table A.XI. The other columns add interaction terms to this baseline specification. The dummy variable *public* denotes a public-limited corporation; *foreign owned* a completely-owned subsidiary of a foreign firm; and *food*, *textile*, *pharma*, and *construction* the industry the firm operates in. Standard errors are in parenthesis, which have been clustered at the firm level.

Table A.XIII: **Public Disclosure and Electoral Outcomes**

	Outcome				
	Contests Next Election	Finishes in Top Two	Increases Vote Share	Changes Party	Contests More Than One Constituency
	(1)	(2)	(3)	(4)	(5)
A: 2012					
Tax Paid in 2012	0.030	0.053	0.013	-0.021	-0.009
	(0.020)	(0.022)	(0.018)	(0.017)	(0.019)
Observations	475	475	475	475	475
Unconditional Mean	0.731	0.565	0.205	0.208	0.144
B: 2015					
Tax Paid in 2015	0.040	0.067	0.030	-0.028	0.012
	(0.019)	(0.021)	(0.016)	(0.015)	(0.016)
Observations	732	732	732	732	732
Unconditional Mean	0.745	0.600	0.202	0.204	0.149
Controls:					
Party Fixed Effects	Yes	Yes	Yes	Yes	Yes
House Fixed Effects	Yes	Yes	Yes	Yes	Yes
% Votes Obtained in 2013	Yes	Yes	Yes	Yes	Yes
Winning Margin in 2013	Yes	Yes	Yes	Yes	Yes

Notes*:* The table investigates if the electoral outcomes of MPs are correlated with their tax payments. We report results from Linear Probability Models where the outcome variable is a dummy, which takes the value 1 if the statement in the heading of each column is true for an MP. The RHS variable of interest is the log of tax paid by the MP, in 2012 for Panel A and in 2015 for Panel B. We normalize this variable by its standard deviation of the corresponding year. The models include the four set of covariates indicated in the last four rows. We also report unconditional mean of the dependent variable in each column. The dummy variable in the fourth column takes the value 1 if the MP contested the 2013 and 2018 elections on two different political parties' tickets. The Pakistani law allows individuals to contest from multiple constituencies simultaneously. The outcome in the final column takes the value 1 if an MP does so.

Table A.XIV: **Public Disclosure and Electoral Outcomes**

	Year							
	2012	2012	2013	2013	2014	2014	2015	2015
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zero Tax Paid	-0.028	0.008	0.019	-0.015	-0.258	-0.301	-0.151	-0.119
	(0.032)	(0.035)	(0.059)	(0.064)	(0.058)	(0.081)	(0.085)	(0.123)
Observations	899	845	811	766	801	754	788	750
Pr {Tax Paid=0}	0.444	0.444	0.091	0.091	0.042	0.042	0.033	0.033
Controls:								
Party Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
House Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
% Votes Obtained in 2013	No	Yes	No	Yes	No	Yes	No	Yes
Winning Margin in 2013	No	Yes	No	Yes	No	Yes	No	Yes

Notes*:* The table investigates if reporting zero tax payment is associated with the re-election probability of an MP. We regress an indicator that a sitting MP wins the 2018 election on a dummy denoting that the MP reported zero tax payment for the given year. The even-numbered columns include the four sets of covariates mentioned in the last four rows into the model. The fraction of MPs reporting zero tax is shown in the third row of the table. Note that this fraction is higher in 2012 than in other years. MPs in our sample were elected to office in May 2013. The 2012 tax year runs from July 2012 to June 2013. Because MPs did not receive salary from the government for the complete year, they were more likely to report zero tax payments in 2012 than in any other year. Robust standard errors are in parenthesis.