Digital Infrastructure and Tax Compliance: Evidence from the Mobile Finance Expansion in Nigeria

Yuxi Li

University of British Columbia

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Abstract

The potential role of digital finance in the formalization of economics in developing economies has become a major focus of contemporary economic research. In this study, I will explore whether the expansion of mobile financial infrastructure has led to increased corporate tax compliance in Nigeria. This paper first investigates and constructs a quasi-natural experiment using the uneven rollout of mobile network infrastructure across Nigerian states between 2009 and 2014 and employs propensity score matching with difference-in-differences (PSM-DID) to address potential endogeneity issues. Unlike optimistic policy narratives and positive macroeconomic correlations in existing research, the experimental results clearly point to a 'null effect,' meaning that the expansion of mobile infrastructure does not lead to firms reporting a larger proportion of taxable sales. I interpret this finding from the perspective of national capacity. While digital footprints can theoretically reduce information asymmetry, in environments with weak national government enforcement (such as the Nigerian government between 2009 and 2014), this is insufficient to alter corporate behavior. Overall, my research suggests that technological progress has the potential to enhance national fiscal capacity, provided it is accompanied by credible enforcement and institutional strength at the national level.

Keywords: Mobile Money, Tax Compliance, State Capacity, Nigeria, PSM-DID

1 Introduction

Perhaps the most fundamental function of any state is to collect taxes. Fiscal capacity, the government's ability to raise revenue, is essential for financing public goods and maintaining the rule of law (Besley and Persson, 2009). However, for many developing countries such as Nigeria, large informal economies severely restrict its fiscal capacity. How to incorporate the widespread informal sector into a formal tax base remains a long-term policy challenge for these states. Policymakers and scholars have increasingly researched digital financial technologies as a potential solution. Aron (2018) argues that as transactions move toward digitization, they leave verifiable "digital trails," which should theoretically reduce opportunities for tax evasion. However, while current research suggests optimistic macro-level evidence linking digitalization to tax revenues (Apeti and Edoh, 2023), we still lack clear micro-level causal evidence of whether digital finance infrastructure could actually change firms' reporting behavior.

I discuss the research question of whether mobile finance infrastructure can be used to enhance corporate tax compliance in Nigeria in this research. I am interested in the 2009-2014 years when the mobile network capacity was increased fast and unevenly in various states. With this variation as a quasi-natural experiment, I utilize firm-level panel data of the World Bank Enterprise Surveys and state-level data of Enhancing Financial Innovation & Access (EFInA) and use a Propensity Score Matching Difference-in-Differences to eliminate endogeneity issues. I would like to create a plausible counterfactual group by uniting the observable baseline traits in the year 2009. The results of my findings indicate that companies in high-growth states were not found to have a high proportion of taxable sales compared to related companies in low-growth states ($p = 0.970$). This is in strong contrast to the positive associations observed in cross-sectional studies, implying that the presence of digital infrastructure is not a sufficient condition to result in better compliance.

I apply theories of political agency and state capacity to interpret my findings. First, infrastructure availability does not guarantee adoption. In a cash-dominated economy, firms may prefer to continue operating off-record to avoid creating paper trails. Second, institutional constraints limit the power of information. When tax authorities lack real-time monitoring capacity or credible enforcement threats, digital trails may be generated yet remain unused. This paper contributes to the research on tax compliance in developing economies by showing that information is ineffective without enforcement, indicating that digital technology complements rather than substitutes for state capacity for further research in fin-tech and economic formalization.

2 Literature Review

There is substantial research on the rise of digital infrastructure, especially mobile money, in developing countries and the resulting economic impacts. Current research is mainly concentrated on three main streams of literature. They are: first, the socio-economic impact of mobile money in developing countries; second, research related to the relationship between digital currency and economic formalization; and third, the state capacity of governments in developing countries regarding tax collection and regulation.

The first stream mainly focuses on the positive impact of mobile money on micro-individuals, specifically households. Existing literature, such as Suri and Jack (2016), indicates that "mobile money has therefore increased the efficiency of the allocation of consumption over time while allowing a more efficient allocation of labor, resulting in a meaningful reduction of poverty in Kenya." Additionally, Lee et al. (2021) find that "in Bangladesh, for active mobile banking users, rural consumption increased by 7.5 percent, and extreme poverty fell, urban migrants experienced less poverty and saved more." Although research regarding the impact of mobile money on households is already relatively complete, research regarding its impact at the firm level is scarce, especially on the topic of tax compliance and regulation.

The second category of literature mainly discusses how the development of mobile money promotes economic formalization. In a review of existing evidence, Aron (2018) clearly suggests that the use of "electronic trails" left by mobile money can increase transparency, thereby playing an important role in widening financial inclusion. At the macroeconomic level, this view is supported by the working paper of Apeti and Edoh (2023); they point out that mobile money significantly increases tax revenue in mobile money countries relative to non-mobile money countries. That is, in cross-country data, there exists a significant positive correlation between the penetration of digital payments and the increase in tax revenue.

The third category of literature explores the core position of state enforcement capacity in taxation and tax regulation; without this capacity, developing countries cannot escape the trap of the informal economy. For example, Besley and Persson (2009) reiterate that policy choices regarding market regulation and taxation are constrained by the state's past investments in legal and fiscal capacities. Building on this, Pomeranz (2015), based on a randomized controlled trial in Chile, emphasizes that the increase in tax compliance levels does not originate from self-enforcement brought about by the VAT paper trail, but originates from the interaction between information structure and enforcement deterrence.

Although these studies highlight the importance of information, they largely assume an environment where the state has the capacity to process this information and enforce compliance. This paper contributes to this literature by interrogating the limits of this mechanism: in a low enforcement setting like Nigeria, does the mere existence of a "digital trail" (via mobile money) automatically translate into tax compliance, or is the credible threat of enforcement a binding constraint?

Based on these studies, this research has critical contributions mainly in two aspects. The first is that existing research generally believes that the digitization process can reduce information asymmetry, thereby leading to formalization. However, these studies all imply an assumption of techno-optimism; this assumption ignores the policy response of firms when facing electronic records but in an environment where the state lacks regulation. The Null Result of this paper reveals an important institutional constraint: in the context of a lack of effective state regulatory capacity, digital technology itself is insufficient to build state tax capacity. The second is innovation in research methodology; previous research mainly focused on showing the positive relationship between the digitization process and tax compliance, relying heavily on cross-sectional correlations prone to selection bias, thereby not well separating the causal relationship between the two. This paper innovatively utilizes a quasi-natural experiment, the PSM-DID method, providing rare micro-causal arguments, challenging the positive correlation findings at the macro level.

3 Institutional Background

To establish a knowledge background for this research, two facts of the Nigerian economy between 2009 and 2014 must be clarified in the first place: the severe constraints on state fiscal capacity, and the rapid but uneven expansion of mobile infrastructure.

3.1 Taxation and State Capacity in Nigeria

Nigeria is a typical case of constrained fiscal capacity. Despite being the largest economy in Africa, during the study period, Nigeria's non-oil tax revenue was only about 4% of GDP, which is significantly lower than the regional average (World Bank, 2015). This structural bottleneck is because of the country's massive informal economy. During the period from 1991 to 2015, the average ratio of Nigeria's shadow economy to official GDP was astonishingly 56.67% (Medina and Schneider, 2018). In terms of the Federal Inland Revenue Service (FIRS), for the dominance of cash transactions, the cost of enforcing compliance among millions of unregistered small enterprises is extremely high. These transactions leave no verifiable paper trail, creating information blind spots for tax authorities. Tax evasion in this sector is not only an anomaly but a long-standing structural equilibrium as well. Such an environment is an ideal testing ground for this research to examine whether digital financial footprints can break this equilibrium and enhance the visibility of the state.

3.2 The Expansion of Mobile Finance (2009-2014)

The period between 2009 and 2014 was one of the significant transition years as far as the digital infrastructure in Nigeria is concerned. Since the liberalization of the telecommunications sector and pilot policies of the digital finance infrastructure by the Central Bank around 2012, there was a boom in the development of mobile networks in the country (EFInA, 2014). What is important to our identification strategy is that the geographical distribution of this expansion was uneven. Telecommunications operators had a focus on investing in commercially viable urban centers and southern states whereas the pace with which infrastructure was being rolled out in rural regions and in the north states was significantly lower. I measured this difference by data obtained in EFInA survey. It was a proxy variable of the availability of local digital finance infrastructure using the difference between the rate of increase in mobile phone ownership within states. Such a geographical and market cost-driven pattern of variation in the supply of infrastructure, and not necessarily the willingness of firms to comply with taxes at the firm level, is what makes the basis of the quasi-natural experiment, applied in this paper.

4 Data Sources and Summary Statistics

4.1 Data Sources

The analysis utilizes firm-level data from the World Bank Enterprise Surveys (WBES) for Nigeria (2009 and 2014). I restrict the sample to panel firms (surveyed in both waves) to enable within-firm comparisons. The dataset provides detailed information on firm attributes, performance, and tax compliance behavior.

To form the treatment and control groups and reflect varying exposure to the emerging mobile finance ecosystem, state-level data from the Enhancing Financial Innovation & Access (EFInA) surveys in 2009 and 2014 are merged. Based on EFInA data, a proxy for the local mobile finance environment is constructed: the change in the percentage of adults with cell phones in each state over the survey years.

Since mobile phones are the primary channel of access to mobile money, this growth rate indicates the pace at which the enabling infrastructure was being built. Using this calculated growth rate, states were grouped into a treatment group (high-growth, upper 30th percentile) and a control group (low-growth, bottom 30th percentile). The analytical dataset combines the state-level treatment indicator from EFInA with firm-level WBES data using state identifiers as the matching key.

4.2 Dependent Variable Definition

The dependent variable of interest is *Tax Compliance*, defined as the percentage of total annual sales that the firm reports to tax authorities. This continuous measure provides a more granular representation of formalization compared to a binary indicator, where 100% reflects complete compliance and values less than 100% indicate tax evasion.

4.3 Summary Statistics

Table 1 presents the summary statistics for the full unmatched sample in the pre-treatment period (2009). The sample consists of 157 firms. The mean tax compliance rate is estimated to be 66.78%, with a standard deviation of 23.48%, indicating significant variation in formalization behavior among firms. Notably, raw sales and employee counts have large standard deviations relative to their means, reflecting a highly skewed distribution. This justifies the application of logarithmic transformation of these variables in the regression analysis to mitigate the influence of outliers.

Table 2 reports the pre-matching balance test between the treatment and control groups. As indicated in the table, firms in high-growth states are systematically different from those in low-growth states. Specifically, treatment firms are significantly younger, smaller in terms of employment, and have lower sales volumes ($p < 0.001$). These differences highlight the necessity of employing Propensity Score Matching to construct a valid counterfactual.

5 Empirical Strategy

5.1 The Treatment and Control Group

The geographic expansion of mobile infrastructure was not strictly random. Telecommunications operators often prioritize infrastructure construction in regions with higher economic development and firm productivity, which implies that high-growth areas may inherently possess an economic environment more conducive to tax compliance.

Therefore, the empirical strategy relies on a key identification assumption: conditional on controlling for firm-level baseline characteristics, the geographic variation in infrastructure is effectively quasi-exogenous to the firm. To support the validity of this assumption, I employ Propensity Score Matching. Given that infrastructure deployment in high-growth states may be correlated with local firms' average size or productivity (a potential source of endogeneity), PSM is used to select a control group of firms in low-growth states that are highly comparable to the treatment group in terms of observable characteristics such as size, age, and industry. This approach assumes that after eliminating these systematic observable differences (selection on observables), the remaining variation in regional infrastructure growth is free from omitted variable bias and can be treated as a plausibly quasi-random policy shock.

5.2 Propensity Score Matching and Difference-In-Differences

However, regional grouping alone is not enough to rule out systematic pre-treatment variations. Firms in high-growth states might differ from those in low-growth states in ways that also affect tax compliance. To address this, I employ a Difference-in-Differences approach augmented with Propensity Score Matching.

In the first stage, I estimate a logit model to predict the probability of a firm being located in a high-growth state based on its 2009 baseline characteristics. I then match treatment firms with control firms that have similar propensity scores. The covariates used for matching include firm size (employees and sales), age, legal status, foreign ownership, managerial experience, and industry.

5.3 Identification Assumption

The validity of the DID strategy relies on the Parallel Trends Assumption. This assumption states that in the absence of the mobile infrastructure shock, the tax compliance trends of firms in the treatment and control groups would have evolved similarly. While the availability of only two periods (2009 and 2014) prevents a formal statistical test of pre-trends, the use of Propensity Score Matching helps to strengthen this assumption. I match firms on a rich set of 2009 baseline characteristics to ensure that the treatment and control groups are comparable in observables before the shock. Conditional on these matched characteristics, it is plausible to assume that their counterfactual trajectories would be parallel.

5.4 Model and Equation

In the DID analysis, I estimate the following model to conduct causal inference:

$$\begin{aligned} TaxCompliance_{it} = {} & \beta_0 + \beta_1 TreatGroup_i + \beta_2 PostPeriod_t \\ & + \delta(TreatGroup_i \times PostPeriod_t) + \Gamma' X_{it} + \varepsilon_{it} \end{aligned} \tag{1}$$

In Equation (1), the dependent variable, $TaxCompliance_{it}$, measures the tax compliance of firm i in year t, expressed as the percentage of total sales reported to the tax authority. The variable $TreatGroup_i$ is a time-invariant binary indicator equal to 1 if the firm is located in a state within the top 30 percent of the mobile phone ownership growth distribution between 2009 and 2014, and 0 otherwise. $PostPeriod_t$ is a time dummy that equals 1 for the post-treatment period (2014) and 0 for the pre-treatment period (2009), capturing broad changes affecting all firms over time.

The coefficient of interest is δ, associated with the interaction term $TreatGroup_i \times PostPeriod_t$. This coefficient captures the Average Treatment Effect on the Treated (ATT), reflecting the differential change in tax compliance for firms in high-growth states relative to those in low-growth states, conditional on covariates. The vector X_{it} includes a set of time-varying firm-level controls, specifically the logarithm of employees, the logarithm of sales, and firm age, to account for observable heterogeneity. Finally, ε_{it} represents the firm-specific error term.

5.5 Limitations and Justification of the Identification Strategy

It is important to acknowledge the limitations of the PSM-DID framework. The fundamental identification assumption of this methodology is based on "selection on observables." In this study, we assume that conditional on matching for observable characteristics (firm size, age, and industry), the treatment and control groups are comparable. However, PSM cannot account for selection bias driven by unobservables—such as regional business culture, unrecorded informal institutional quality, or intrinsic managerial capacity. If these unobservable factors vary over time and simultaneously influence both the deployment of mobile infrastructure and corporate tax behavior, the estimates may remain biased.

Despite these limitations, the PSM-DID approach represents the best feasible strategy for causal inference given the constraints of the available statistical data. Since the WBES dataset provides panel observations for only two periods (2009 and 2014), the lack of long time-series data precludes the use of standard parallel trends tests or the Synthetic Control Method. Under these circumstances, a direct application of OLS or standard DID would likely suffer from severe selection bias, as firms in high-growth states exhibited systematic differences from those in low-growth states at the baseline (as shown in Table 2).

By implementing Propensity Score Matching, we construct a counterfactual control group that is highly balanced across all baseline observable characteristics (as presented in Table 3). While this does not entirely resolve endogeneity issues, it minimizes the bias driven by compositional differences as much as possible. Furthermore, to address concerns regarding unobservables, I conducted Placebo Tests in Section 6. The results indicate that unobserved trends did not confound our main findings. Therefore, the estimates presented in this paper offer the closest approximation to the true causal effect possible within the existing data limitations.

6 Estimation and Results

6.1 Balance Test: Constructing a Valid Control Group

The unmatched sample suffers from serious selection bias. As illustrated in Table 2, in the 2009 baseline period, firms in high-growth states (the treatment group) and in low-growth states (the control group) exhibited significant initial differences in terms of Firm Age, Log(Employees), and Log(Sales), with p-values below 0.001.

This study employed a PSM strategy to mitigate this bias. Propensity scores were estimated using a Logit model, and a stringent 1:1 nearest-neighbor matching algorithm with a caliper of 0.01 was adopted. The results of the post-matching balance test are summarized in Table 3. Before the matching, statistically significant differences existed. In sharp contrast, once matching was performed, the mean differences across all covariates became statistically insignificant. Most importantly, the p-value of the Joint Test ($p > \chi^2$) increased from 0.000 to 1.000, indicating a perfect balance. This confirms that the procedure successfully generated a comparable sample ($N = 128$).

6.2 Estimated Impact on Tax Compliance

These results indicate that the total effect of mobile finance expansion on firms' tax compliance disappears once the initial differences between the treatment and control groups are properly adjusted through PSM. It is very likely that the simple differences observed in the unmatched data reflect selection bias rather than the policy itself. Therefore, this research supports a cautious but firm conclusion: the policy does not meaningfully improve tax compliance.

6.3 Robustness Checks

To verify the robustness of the causal inference, I first examined the sensitivity of the estimates to the choice of matching algorithm. While the main analysis relied on 1:1 nearest-neighbor matching, I again estimated the model using Kernel Matching. Kernel matching constructs the counterfactual using a weighted average of all control units, thereby exploiting more information from the sample. As reported in Column (2) of Table 5 in the Appendix, the coefficient on the interaction term is -1.778 ($SE = 6.156$, $p = 0.773$). This estimate remains statistically indistinguishable from zero, and the magnitude of the coefficient is comparable to that of the main specification (-0.249). This suggests that the null result is not driven by the specific matching technique employed and is robust to alternative specifications.

Second, to address concerns regarding potential omitted variable bias in the treatment assignment, I conducted a placebo test. This test uses the change in Managerial Experience as the dependent variable. Under the identification strategy, the expansion of mobile finance infrastructure is treated as an exogenous shock that should not systematically alter the pre-existing professional experience of firm managers. Column (3) of Table 5 reports the results. The estimated coefficient is -2.791 ($p = 0.108$).

Even though this p-value is not so high as the main estimate, it still does not fall below the traditional mark of statistical significance ($p > 0.10$). We, therefore, do not reject the null hypothesis of parallel trends. It means that there are no significant systematic differences in managerial characteristics that are observed in the treatment and control groups. More importantly, although there may be some residual differences, since there is an estimated negligible and statistically insignificant effect on tax compliance in the main regression ($p = 0.970$), it is quite possible to conclude that such possible background noises did not cause any causal bias to the main results. This also helps to prove the validity of the identification strategy used in this paper.

6.4 Discussion of Mechanisms

The empirical results from this research show no significant impact of mobile finance expansion on firms' tax compliance in Nigeria. This null effect stands in sharp contrast to the positive macro-level correlations that many earlier studies emphasize. Economically, there are two major mechanisms that help explain why the growth of mobile finance infrastructure did not translate into increased tax compliance among these companies: limited adoption despite infrastructure availability, and weak information integration and enforcement capacity.

To begin with, infrastructure availability and business adoption are not the same thing. Though the treatment group witnessed a boom in the number of mobile phone owners (infrastructure), it does not necessarily mean that small business owners actually adopted mobile money for corporate payments.

According to Banerjee and Duflo (2011), the behavioral barriers that limit the adoption of technology are usually marked by the phenomenon known as "inertia." Apparently minor switching costs, like getting used to a new system, paying differently or the fear of making an error in operation, tend to loom large to small business owners. This psychological inertia causes business owners to delay the immediate expenditures on change; in the case of new technologies, which may have long-term benefits, they prefer to stay in the current situation and continue using the old cash transaction models. The effect of such constraints in the context of Nigeria is harsh. According to research by Okoye and Ezejiofor (2013), the high illiteracy rate in Nigeria severely constrains citizens' adoption of electronic technologies. Consequently, a vast number of small business owners, despite being in areas with network coverage and possessing electronic devices, continue to rely on cash because they lack the relevant knowledge or the ability to operate complex POS terminals. As a result, without widespread adoption of digital payments for sales, there is no creation of a digital footprint.

Secondly, despite the possibility of adoption, the absence of regulatory capability at the national level (e.g., the absence of a strict legal framework, policy apparatus, and regulatory tools, and monitoring data and taxes) contains the implication that the existence of records of digital transactions does not change the cost-benefit analysis of a rational company when it comes to tax evasion. Furthermore, Okoye and Ezejiofor (2013) emphasize that during their study period, around 2012, Nigeria was a major center of e-fraud, and the legal framework at that time had significant gaps in consumer protection. This high-risk environment discouraged businesses from conducting transactions through formal digital channels and also reflected the inadequacy of regulatory agencies in monitoring digital financial crimes (including tax evasion). Therefore, given Nigeria's lack of national tax regulatory capacity, resulting in extremely low costs for businesses to evade taxes, coupled with the potential losses from e-fraud, even with the expansion of digital infrastructure, business owners' willingness to pay taxes legally has not increased.

7 Conclusion

This paper examines the causal impact of mobile finance development on corporate tax compliance in Nigeria. I use a PSM-DID strategy that leverages the uneven geographic development of mobile infrastructure between 2009 and 2014 as a quasi-natural experiment to address endogeneity problems and selection bias that are common in earlier literature. My micro-level analysis shows a null result, contrary to the optimism usually reported in macroeconomic research. The effect of increasing mobile infrastructure on firm tax compliance is found to be economically trivial and statistically indistinguishable from zero ($p = 0.970$). Such results indicate that the positive relationships found in prior cross-sectional research are likely driven by selection bias, because firms that are already on a path toward formalization are simply more willing to adopt new technologies, rather than technology being the cause of formalization.

The absence of a substantial effect reveals a key distinction between making digital payments easier and changing compliance behavior. While digital financial technologies can reduce transaction costs for business owners, they don't necessarily change the structural incentives behind tax evasion. Historical political economy literature suggests that written or digital footprints only influence a company's willingness to file taxes when accompanied by a credible threat of prosecution and when this threat is offset by a hostile online fraud environment. In Nigeria, during the research period, there was no effective realtime information integration between national law enforcement or fintech providers and tax authorities; therefore, simply providing digital tools is insufficient to incentivize firms to engage in the formal tax system.

There are several study limitations to consider when interpreting these findings. First, due to panel data attrition, the final regression sample size is relatively small ($N = 128$), which limits statistical power to detect smaller effects. Second, the treatment variable is based on mobile phone ownership growth as a proxy for the mobile finance ecosystem. Although this is a valid measure of infrastructure availability (an "Intent-to-Treat" measure), it is not a direct measure of actual business use of mobile money. Therefore, the null result may partly reflect a gap between access and adoption.

The most direct policy implication of this study is that, for developing countries wanting to improve tax compliance, relying solely on the improvement of infrastructure or technology is severely insufficient; it must be equipped with the enhancement of state capacity, such as more complete legal systems, enhancing the digital monitoring capabilities of tax administration departments, strengthening enforcement mechanisms, and the popularization of business owner education, etc. Future research will benefit from transaction-level data and larger administrative datasets to better distinguish between the presence of technology, its actual usage, and the enforcement structures that make digital footprints effective.

Declaration of Generative AI Use

I acknowledge the use of ChatGPT/Gemini to assist with language refinement (including Chinese-English translation and structural adjustments), LaTeX code formatting, guidance on data processing in Stata, literature search and synthesis, and brainstorming potential mechanisms. All empirical strategies, data analysis (performed in Stata), and final interpretations are my own independent work. I have verified all AI-generated suggestions for accuracy.

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Table 1: Summary Statistics (Full Sample, 2009 Baseline)

Variable	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
Outcome Variable					
Tax Compliance (%)	157	66.78	23.48	2.00	100.00
Firm Characteristics					
Firm Age (years)	157	14.59	9.05	2.00	53.00
Employees (Headcount)	157	34.23	42.05	1.00	264.00
Sales (Millions NGN)	157	203.73	1165.84	0.17	14000.00
Foreign Ownership (%)	157	2.01	12.68	0.00	100.00
Manager Experience (years)	156	10.63	6.95	1.00	45.00

Notes: This table presents summary statistics for the full unmatched sample in the baseline year (2009). The sample consists of 157 panel firms that were successfully matched to state treatment groups. *Tax Compliance* is the percentage of total sales reported to tax authorities. *Sales* are reported in millions of Nigerian Naira (NGN). *Employees* represent the number of full-time permanent workers. Note the large variance in sales, justifying the use of logarithmic transformation in the regression analysis.

Table 2: Pre-Matching Covariate Balance Test (2009 Baseline)

	Control Group		**Treatment Group**		**Difference**	
Variable	**Mean**	**(SD)**	**Mean**	**(SD)**	**Diff**	**p-value**
Outcome Variable						
Tax Compliance (%)	67.840	(24.796)	67.347	(34.695)	0.493	0.876
Firm Characteristics						
Firm Age (years)	13.878	(8.753)	9.884	(8.335)	3.994	0.000
Log Employees	2.819	(1.160)	2.108	(1.257)	0.711	0.000
Log Sales	16.523	(1.968)	15.630	(1.753)	0.893	0.000
Foreign Ownership (%)	1.740	(11.827)	0.000	(0.000)	1.740	0.043
Manager Exp. (years)	10.494	(6.681)	10.142	(7.505)	0.352	0.634
Observations	181		190		371	

Notes: The table shows descriptive statistics of the unmatched sample of 2009 enterprise survey for the panel firms. The sample is restricted to firms in states categorized as Slow Growth (Control group, N=181) or Fast Growth (Treatment group, N=190). P-values correspond to a two-sample t-test testing the equality of means.

Table 3: Post-Matching Covariate Balance Test

	Unmatched Sample			**Matched Sample**		
Variable (2009 Baseline)	**Treat. Mean**	**Ctrl. Mean**	**p-value**	**Treat. Mean**	**Ctrl. Mean**	**p-value**
Firm Age	9.884	13.819	0.000***	11.272	11.011	0.822
Log(Employees)	2.108	2.784	0.000***	2.373	2.361	0.942
Log(Sales)	15.630	16.465	0.000***	15.992	15.935	0.828
Manager Exp.	10.142	10.525	0.608	10.554	10.859	0.756
Legal Status (Partner)	0.126	0.260	0.001***	0.152	0.185	0.557
Industry (Textiles)	0.005	0.045	0.013**	0.000	0.011	0.319
Industry (Retail)	0.062	0.011	0.007***	0.022	0.011	0.563
Industry (Services)	0.389	0.266	0.012**	0.391	0.370	0.763
Industry (Construction)	0.237	0.068	0.000***	0.130	0.098	0.489
Summary Statistics						
Mean Bias	25.2%			5.1%		
Joint Test ($p > \chi^2$)	0.000			1.000		

Notes: Source: Stata 'pstest' output. Match Method: 1-to-1 nearest neighbor matching with a 0.01 caliper. The logit model includes all covariates listed, plus quadratic terms for Age, Log(Employees), and Log(Sales), and a full set of legal status and industry fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: The Effect of Mobile Money Environment on Tax Compliance (2009-2014)

	(1)	(2)	(3)
Method	**Naive OLS**	**Standard DID**	**PSM-DID**
Sample	(Unmatched)	(Unmatched)	(Matched)
Dependent Variable	Tax Compliance (2014)	Δ Tax	Δ Tax
High Mobile Growth × Post	-3.367	-0.774	-0.249
	(2.946)	(5.458)	(6.533)
2009 Baseline Controls:			
Firm Age (2009)	-0.804*	-0.347	0.419
	(0.461)	(0.931)	(1.394)
Log(Employees) (2009)	-3.083	-1.720	-3.284
	(3.275)	(6.696)	(9.882)
Log(Sales) (2009)	20.130**	16.665	36.780
	(9.176)	(18.531)	(24.401)
Manager Exp. (2009)	0.417*	0.337	-0.274
	(0.225)	(0.443)	(0.743)
Covariate Quadratic Terms	Yes	Yes	Yes
Legal Status FE (2009)	Yes	Yes	Yes
Industry FE (2009)	Yes	Yes	Yes
Observations (N)	250	250	128
Adjusted R-squared	0.071	0.058	0.129

Notes: Robust standard errors are in parentheses, clustered at the state level. The Treatment Effect (δ) is the coefficient on the interaction term. Column (1) is a Naive OLS on the 2014 unmatched cross-section, controlling for 2009 baseline characteristics. Column (2) is a standard DID model on the full, unmatched sample. Column (3) is our preferred PSM-DID model, run only on the matched, balanced sample. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Robustness Checks and Placebo Tests

	(1) **Main Result**	(2) **Kernel Matching**	(3) **Placebo Test**
Dependent Variable	Tax Compliance	Tax Compliance	Manager Experience
High Mobile Growth × Post	-0.249	-1.778	-2.791
	(6.533)	(6.156)	(1.725)
P-value	0.970	0.773	0.108
Matching Method	1:1 Nearest Neighbor	Kernel	1:1 Nearest Neighbor
Controls	Yes	Yes	Yes
Observations	128	250	172

Notes: Robust standard errors clustered at the state level are reported in parentheses. Column (1) reports the baseline PSM-DID estimates using 1:1 nearest-neighbor matching. Column (2) employs Kernel matching as a robustness check. Column (3) presents a placebo test using the change in managerial experience as the dependent variable. All specifications include the full set of baseline controls (firm age, size, industry, legal status).