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

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**Abstract**

Does the expansion of digital finance infrastructure promote formalization in developing economies? In this paper, causal impact of mobile finance development on corporate tax compliance in Nigeria will be explored. Taking advantage of uneven geographic dispersion of mobile network infrastructure between 2009 and 2014 as a quasi-natural experiment, I use Propensity Score Matching Difference-in-Differences (PSM-DID) approach to endogeneity. Against a backdrop of macroeconomic optimism, firm-level panel data estimation gives an accurate, statistically insignificant null finding: the growth of mobile infrastructure does not have a statistically significant impact on whether firms report taxable sales or not. I explain those results by a set of state capacity: although digital trails are expected to decrease information asymmetry, they do not cause compliance in the case of ineffective enforcement mechanisms. The findings indicate that technology is a requirement that is not sufficient to building fiscal capacities.

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

# 1 Introduction

Imposing taxes is a characteristic feature of a functional state. Fiscal capacity which can be defined as the capacity of the government to raise revenues is a precondition to the provision of fundamental goods in the society as well as upholding the rule of law as argued by Besley and Persson (2009). The state however is limited in many developing economies because of the presence of an informal economy. The process of combating the widespread informal sector to the consummation of formal tax base is a challenge that has resisted policies in Nigeria. The quick spread of digital financial technologies in recent years has been suggested as one of the solutions of this information asymmetry. The rational behind it is simple: with the shift to digital transactions, transactions would leave behind a digital trail that, in theory, would make tax evasion difficult (Aron, 2018).

In this paper, I discuss how far the growth of mobile financial infrastructure reduces the tax evasion by corporations. It is an analysis in the framework of rapid digital growth of Nigeria between 2009 and 2014, which was characterized both by huge and geographically disproportionate investments in mobile network capacities. Although the macroeconomic literature often notes that digitalisation and tax revenue have a positive correlation (Apeti and Edoh, 2023), the actual micro-level mechanisms are still unclear. One prominent empirical issue of this literature is endogeneity: the empirical interest in this literature is that firms which mobile payments use will be nonprobabilistically different with the non-users in size, productivity, and managerial education, which are independent attributes affecting tax compliance probability. In turn, spurious exaggeration of the perceived causal impact of technology may be provoked by naive comparisons between adopters and non-adopters.

I exploit the geographically uneven distribution of mobile financial infrastructure in the Nigerian states as a quasi-natural experiment in order to overcome this identification challenge. I use a combination of firm-level panel data of the World Bank Enterprise Surveys (WBES) and state-level infrastructure data of Electronic Finance Infrastructure Atlas (EFInA). My empirical design is a Propensity Score Matching Difference-in-Differences (PSM-DID) design. I am able to build a convincing counterfactual by balancing a state in the high-growth treatment group with firms in the low-growth control group on a more intensive set of 2009 baseline characteristics.

The findings indicate that the development of the mobile finance environment does not have any statistically significant influence on the tax compliance of firms. In particular, companies in the states where mobile infrastructure was increasing faster were no better off to augment their reported sales to the tax authorities than similar companies in the states with lower growth. The coefficient is considered economically insignificant and statistically non-significant ($p = 0.970$). Such a null finding is consistent across different specifications and very different to the positive correlations reported in cross-sectional studies.

I would explain these results in the framework of political-agency and state-capacity. This fact that digital infrastructure fails to translate into increased compliance is an indication that the binding constraint on formalization is not high transaction cost only, but structural incentives embedded in the tax system. I would suggest two main mechanisms to explain this result. To start with, the situation of availability and adoption of infrastructure is critically different: in the cash-dominated economy, a company might use mobile technology to communicate and keep cash-based business as a precautionary measure, lest they leave any papers. Second, and possibly even more significant, an informational gap between fintech providers and tax collectors continues to exist; without the ability to monitor transactions in real-time or credible threats of enforcement, the potential to evade is not affected by the mere presence of digital potential.

The present paper can be discussed as contributing to two wide areas of scholarship. First, it supplements the literature on the determinants of tax compliance in third-world countries (Pomeranz, 2015; La Porta and Shleifer, 2014). Although some of the previous research has emphasized the critical nature of the third-party reporting and paper trails, my results indicate that the above-mentioned mechanisms do not work in environments where the state lacks the capacity to analyze the ensuing information. Second, it builds on the existing body of knowledge of the actual impact of fintech in emerging markets (Suri and Jack, 2016). Although the welfare implications of mobile money in the households have been well established, this study preempts the constraints of technology in reducing structural institutional shortfalls.

The rest of the paper is structured in the following way. Section 2 gives the literature review. Part 3 provides the institutional background. The empirical strategy is recorded in section 4. Section 5 describes the data. Section 6 reports the results. Section 7 concludes.

## 2 Literature Review

The manuscript builds on and adds to two main streams of the literature, namely the socioeconomic effects of mobile money in less-developed nations, and the connection between digital money and economic formalisation.

The former is concerned with the impact of mobile money on micro-individuals and households. The available literature proves that mobile money could help families a great deal in reducing the level of poverty and leveling consumption as a means of risk sharing (Suri and Jack, 2016). These papers highlight the criticality of mobile money in improving the level of financial inclusion and eliminating friction in making transactions among people. However, whereas the effects on households are well-reported, very little evidence exists on the effects on firms, especially on the subject of regulatory compliance and state relations.

The second one is the study of how mobile money can lead to economic formalisation. In a review of the evidence, Aron (2018) clearly points out that the electronic trails left once mobile money is used can theoretically facilitate formalisation and boost tax collection due to greater transparency. Macroeconomically, this is supported by working papers by Apeti and Edoh (2023) and the BIS (2024) that state that there is a significant positive correlation between digital payment, increased tax revenue, and a reduced informal economy among countries.

Nevertheless, there remains a research gap that is critical. In particular, does availability of digital infrastructure which is the required pre-condition of mobile money adoption trigger business owners to start paying their taxes formally? The available literature has failed to clearly cover this particular behaviour at the micro-level besides it has not solved the issue of identification. The largest part of the prior research is based on cross-sectional correlation, which is prone to selection bias. The current manuscript aims at addressing this gap by offering micro-level empirical data based on a quasi-experimental approach.

# 3 Institutional Background

To understand why the digitization of payments may or may not influence tax compliance, one must contextualize the analysis within the Nigerian economy and regulations as of 2009–2014. In this section, the structural issues of taxation in Nigeria are described and the rapid but disproportionate development of the mobile finance ecosystem is explained.

## 3.1 Taxation and State Capacity in Nigeria

Nigeria offers an ideal case study of how fiscal capacity is limited. The mobilization of non-oil tax revenue has been historically very low, even though this is the largest economy in Africa. At the time of this study, the tax-to-GDP ratio of Nigeria was about 6%, which is far below the Sub-Saharan average. According to the arguments brought out by Besley and Persson (2009), taxation power is not just a policy decision but a capability of state capacity, which is the aggregate investment in administrative infrastructure needed to keep an eye on economic actors.

The main challenge facing the expansion of the tax base in Nigeria is the extensive informal economy. The International Monetary Fund (IMF) estimates that the informal sector contributed about 65 percent of Nigeria's GDP in this period. In the case of the Federal Inland Revenue Service (FIRS), compliance is too costly to be enforced on millions of unregistered micro-enterprises. These companies are almost completely cash-based, have no accounting records, and leave no paper trail for auditors to trace. In that regard, tax evasion is not a criminal deviation but rather a structural attribute of the business environment. In theory, digital payment introduction should be a technology shock to this balance, producing verifiable transaction records, but the strength of this shock largely depends on the existing capacity of the state to enforce this policy.

## 3.2 The Mobile Finance Expansion (2009–2014)

The years 2009 to 2014 are found to be the turning point for the financial arena in Nigeria. The second wave of digital integration was the shift from simple voice connectivity into mobile-enabled financial services following the liberalization of the telecommunications sector in the early 2000s.

One of the major contributors to this shift was the policy of the Central Bank of Nigeria, specifically "Cashless Nigeria." This policy was piloted in Lagos in 2012 and later implemented in other states across the country to curtail physical cash in circulation by charging handling fees on big cash withdrawals and encouraging the use of electronic payments. Although the policy requirement was federal, market forces and the availability of infrastructure predetermined the de facto extension of the mobile money ecosystem. Telecommunication operators invested heavily in network towers, but not in a balanced manner. Urban centers and commercially viable states experienced a fast-paced expansion of mobile network coverage and agent banking networks, whereas rural and northern states lagged behind.

This geographic difference in infrastructure development serves as the quasi-natural experiment for this study. To quantify this growth, I use data from Enhancing Financial Innovation & Access (EFInA). EFInA is a specialized development agency funded by the UK's Department for International Development (DFID) and the Bill & Melinda Gates Foundation. Their financial services survey, conducted bi-annually, is generally considered the most stringent source of information on financial inclusion in Nigeria. By relying on EFInA's state-level information regarding mobile phone user counts, I construct a solid proxy for the presence of digital finance infrastructure, independent of any individual company's choice to use the technology.

# 4 Empirical Strategy

## 4.1 The Treatment and Control Group

To identify the causal effect, I utilize the uneven spread of network infrastructure across Nigerian states during the development of mobile payment services (2009-2014) as a quasi-natural experiment. Using the EFInA survey data, I build a key proxy of the local mobile finance environment: the growth rate of adult mobile phone ownership in each state. Since mobile phones are the primary gateway to the mobile money ecosystem, this growth rate serves as a powerful indicator of the local digital finance infrastructure.

I divide states based on this growth rate to sharpen the comparison. States in the upper 30% of the growth distribution form the Treatment Group (High Growth), while states in the bottom 30% form the Control Group (Low Growth). The middle 40% are excluded to ensure a distinct separation between the treatment and control environments.

This geographical assignment is relatively exogenous. The impact of the individual firm on the overall development of mobile infrastructure across the state is insignificant, which means that the location of a firm in a fast-growth state may be viewed as an exogenous environmental shock. The plan therefore attempts to reduce the main endogeneity bias that is caused by self-selection of firms.

## 4.2 Propensity Score Matching and Difference-In-Differences

However, regional grouping alone does not 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 (DID) approach augmented with Propensity Score Matching (PSM).

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.

## 4.3 Identification Assumption

The validity of the DID strategy relies on the Parallel Trends Assumption, which posits 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 significantly strengthens this assumption. By matching firms on a rich set of 2009 baseline characteristics, we ensure that the treatment and control groups are comparable in observables before the shock. It is therefore plausible to assume that, conditional on these matched characteristics, their counterfactual trajectories would be parallel.

## 4.4 Model and Equation

Ultimately, we will estimate the following core DID regression model:

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

Where:

- $TaxCompliance_{it}$ is the measure of tax compliance for firm $i$ in year $t$.
- $TreatGroup_i$ is a dummy variable equal to 1 if firm $i$ is in a high-growth state (Treatment Group), and 0 if in a low-growth state (Control Group).
- $PostPeriod_t$ is a time dummy variable, equal to 1 if the year $t$ is 2014 (post-treatment), and 0 if it is 2009 (pre-treatment).

- The coefficient $\delta$ on the interaction term $(TreatGroup_i \times PostPeriod_t)$ is our parameter of interest. It captures the Average Treatment Effect on the Treated (ATT).
- $X_{it}$ is a vector of time-varying firm-level controls (e.g., log of employees, log of sales, age).
- $\varepsilon_{it}$ is the error term.

# 5 Data Sources and Summary Statistics

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 analysis. The dataset provides detailed information on firm attributes, performance, and tax compliance behavior.

To form both the treatment and control groups, which reflect the varying exposure of firms to the emergence of the mobile finance ecosystem, state-level data of surveys on Enhancing Financial Innovation Access (EFInA) Access to Financial Services in Nigeria 2009 and 2014 are used. Based on the EFInA survey data, a major substitute of the local mobile finance setting is developed: the increase rate of the proportion of adults possessing a cell phone in each of the states over the two years of the survey.

Given mobile phones are the main channel of access to the mobile money ecosystem, this growth rate is a strong pointer to the rate at which the local mobile finance enabling infrastructure is being built. Using this calculated growth rate, the states in Nigeria were grouped into treatment group (high-growth, upper 30th percentile) and control group (low-growth, bottom 30th percentile). The analytical data is then built using the state level treatment group indicator that is based on the EFInA data, and the firm level WBES data, where the state identifier is used as the matching key.

## 5.1 Dependent Variable Definition

The primary dependent variable is Tax Compliance, measured as the percentage of total sales that the firm reports for tax purposes. This continuous measure provides a more granular view of formalization than a simple binary indicator.

## 5.2 Summary Statistics

Table 1 (see Appendix) presents the descriptive statistics for the full sample in the pre-treatment period (2009) before matching. The sample consists of 371 firms (181 in the Control Group and 190 in the Treatment Group). As shown in the table, there are significant pre-existing differences between the groups, justifying the use of PSM.

# 6 Estimation and Results

## 6.1 Balance Test: Constructing a Valid Control Group

The unmatched sample suffers from serious selection bias. To illustrate this, 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 balance test are summarized in Table 2. 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 ($N = 184$).

## 6.2 Estimated Impact on Tax Compliance

Table 3 demonstrates the key regression results. Three models are compared: (1) a Naive OLS model using the 2014 unmatched data; (2) a conventional Standard DID model using the complete unmatched sample; and (3) the desirable PSM-DID model using the matched, balanced sample.

The OLS model in Column (1) and the standard DID model in Column (2) show negative but statistically insignificant coefficients. However, these estimates are undermined because they rely on unbalanced samples.

The specification of choice, the PSM-DID estimate in Column (3), corrects for both selection bias and common time trends. The Treatment Effect ($\delta$) coefficient is -0.249, and the robust standard error is 6.533. The p-value of 0.970 implies that the effect is not statistically significant at any standard level. The economic magnitude of this coefficient (-0.25 percentage points) is also trivial.

These results indicate that the total effect of mobile finance expansion on SME tax compliance fades out once the initial variations between the treatment and control groups are properly adjusted through PSM. It is highly probable that simple differences observed in the unmatched data are caused by selection bias rather than the policy itself. This research thus derives a methodologically strong conclusion: the policy has no substantive impact on tax compliance.

## 6.3 Discussion of Mechanisms

The result of the empirical analysis is a null result, which is in contrast to positive correlations of the macro-level studies. Economically, there are probably two major mechanisms that would have provided explanations as to why growth of mobile finance infrastructure did not translate to increase in tax compliance among these companies.

To begin with, infrastructure availability and business adoption have a critical difference. Though the treatment group witnessed a boom in the number of mobile phone owners (infrastructure), it does not necessarily mean that small business owners turned to the use of mobile money in making corporate payments. In an informal sector such as Nigeria, where the economy is more cash-driven, companies might be in possession of phones to maintain personal communication; however, they will stay in cash transactions so as not to leave any paper trail. In the absence of formalization created by direct adoption of digital payments to make sales, there is no creation of the digital footprint.

Second, even if adoption occurred, there is likely an informational disconnect between fintech platforms and tax authorities. The theoretical argument for formalization relies on the premise that digital transactions increase visibility to the state. However, during the period of study (2009-2014), Nigeria's Federal Inland Revenue Service (FIRS) lacked the technological integration to monitor mobile money transactions in real-time. Without a credible threat of enforcement or an automated tax-collection mechanism embedded in the payment system, the mere existence of digital transaction logs does not alter the cost-benefit analysis of tax evasion for a rational firm.

# 7 Conclusion

The paper examines the causality impact of mobile finance development on corporate tax compliance in Nigeria. I use a Propensity Score Matching Differences (PSM-DID) strategy, which also takes advantage of the uneven geographic development of mobile infrastructure in 2009-2014 as a quasi-natural experiment to overcome the endogeneity problems and selection bias, which are common in earlier literature.

My micro-level analysis shows a null result, unlike the optimism that is usually reported in macroeconomic reports. The economic impact of increasing mobile infrastructure on tax compliance of firms is found to be economically insignificant and statistically no significant than zero ($p = 0.970$). Such results indicate that the positive relationships found in prior cross-sectional research are probably caused by selection bias because firms already headed towards a formalization path are merely more apt to implement new technologies, and not the technology itself is the cause of the formalization.

The absence of a substantial effect of treatment reveals an essential line between the method of payment and supplies of compliance. Although digital financial technologies have the effect of reducing the transaction costs incurred in payment, it will not necessarily change the structural cost-benefit analysis of tax evasion. According to the political economy literature, paper trails or digital footprints are effective only at the time when the threat is accompanied by credible threat of detection. Contextually speaking, in the context of Nigeria at the time of the study, without an effective state enforcement system or informational integration in real-time between fintech solutions and tax agencies, the simple provision of the digital tool was not enough to motivate rational companies to move to the formal tax net.

There are a number of study weaknesses that should be considered when explaining these findings. First, because of the panel data attrition, the final regression sample size is not that large ($N = 128$), which restricts the statistical power to find smaller marginal effects. Second, the variable of the treatment depends on the increase of the mobile phone ownership as a proxy of the mobile finance ecosystem. Although this is a measure of the availability of the required infrastructure (an "Intent-to-Treat" measure), it is not a direct measure of the actual transfer of mobile money into business transactions. Therefore, the null result can be an indication of a disconnect between access to infrastructure and its application in business to some extent.

The policy implication is simple: technology is not a silver bullet. To achieve the formalization goal successfully, fintech will have to be supplemented with active policy initiatives, including increasing the technological capacity of tax administrations to trace digital transactions and enforce the current tax regulations. Future studies would be useful in including direct transaction-level data and larger administrative data sets in investigating these dynamics in greater detail, differentiating between the presence of technology and its application, associated with enforcement.

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Table 1: Descriptive Statistics for Pre-treatment Period (2009, Pre-Matching)

| | **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 2: PSM Covariate Balance Test (2009 Sample)

| | **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 3: 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) | $\Delta$ Tax | $\Delta$ Tax |
| **Treatment Effect ($\delta$)** | -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 ($\delta$) 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.