Final report UGA-24179 May 2025

# Mobile money tax: Financial inclusion versus financial development

Evidence using transaction-level data: The case of Uganda

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# Final Report

| Project title | |
|---|---|
| Mobile Money tax: financial inclusion VS financial development | |
| **Project code** | **Name of PI completing this form** |
| UGA-24179 | Lorenzo Spadavecchia |
| **Date** | |
| 15/05/2025 | |

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# Evidence Using Transaction-Level Data: The Case of Uganda

## Introduction

The expansion of mobile money in Sub-Saharan Africa has transformed the financial inclusion landscape by offering basic transactional and storage services to previously unbanked populations. In countries like **Uganda**, mobile money became the dominant financial tool for day-to-day transactions, domestic remittances, and liquidity management. The system's widespread penetration—particularly in rural areas—was driven by its low transaction costs, proximity through agent networks, and the flexibility it offered to informal sector users.

However, this ecosystem was disrupted in **July 2018**, when the Ugandan government introduced a **tax on mobile money transactions**. Initially set at **1% on all transactions**—including deposits, withdrawals, and transfers—the tax was rapidly revised to **0.5% on withdrawals only** following public backlash. Despite this policy adjustment, the tax generated significant uncertainty and introduced frictions in a system that had become central to household financial behavior.

This section presents **empirical evidence on the effects of the mobile money tax**, using a unique combination of **transaction-level administrative data** from mobile money providers and **survey-based panel data** from the Uganda National Panel Survey. The analysis explores how users responded to the introduction of the tax, particularly in terms of reduced mobile money activity and **substitution toward alternative financial services** such as commercial banks and banking agents. It also highlights the **heterogeneous impact** of the policy across rural and urban areas, illustrating its regressive implications for populations with limited access to financial infrastructure.

Uganda provides a compelling setting to analyze the **mechanisms and behavioral margins** through which users adapt to transaction-based mobile money taxes. The **findings from Uganda**—particularly regarding substitution to bank accounts and cash, the heterogeneity of tax effects, and the role of infrastructure—offer timely and policy-relevant lessons for **other low-income countries** navigating the balance between digital financial expansion and fiscal policy.

## Data and Empirical Framework

This section outlines the empirical framework used to estimate the impact of Uganda's mobile money tax on user behavior, focusing on both the **intensive** and **extensive** margins of mobile money usage. It also discusses the substitution towards formal banking and agent-based financial services following the introduction of the tax. The analysis relies on two primary data sources: **transaction-level administrative data** obtained directly from mobile money providers, and repeated cross-sectional data from the **Uganda Panel Survey (UPS)**. Together, these sources allow for a robust identification strategy and the estimation of causal impacts using a difference-in-differences framework.

### Data Sources and Structure

The first dataset comprises **high-frequency, transaction-level administrative data** from mobile money operators in Uganda. These records include detailed information on individual transactions — such as type (withdrawal, deposit, P2P transfer), value, and timestamp — across millions of user accounts. These data cover both individual and business customers and are collected at daily frequency, making it possible to detect immediate behavioral changes around policy shocks.

We have access to the universe of mobile money transactions from one of the two major companies in Uganda. MTN and Airtel share the mobile money market equally, have similar coverage and set extremely similar prices on mobile money transactions. We expect no major differences in individual level usage between the two comapnies, indeed it is estimated that at least 30% of the Ugandan population with access to a a mobile phone has a SIM subscription with both operators[1]. For the only year 2018, we have access to more than 50 million transactions, divided by person-to-person transfers (P2P), cash-in (deposits) and cash-out (withdrawals). We are able to access both the sender, the receiver or the mobile money agent identifier, hence allowing us to reconstruct the whole network of mobile money transactions. We have access to the type of transaction, to its value in Ugandan Shillings (UGX), to the fees applied on the transactions, as well as on the time and day it was performed.

Out of the 5.5 million mobile money users active before the introduction of the tax, we are able to identify the district of residence for a random sample of about 1.5 million users. This allows us to present evidence of heterogeneity in mobile money usage elasticity between different district, depending on local characteristics.

These administrative data offer several advantages: (i) they eliminate concerns related to recall bias or misreporting common in survey-based studies; (ii) they allow for real-time analysis of user behavior; and (iii) they permit the estimation of heterogeneous treatment effects by transaction type and user characteristics.

The second dataset comes from the **Uganda Panel Survey (UPS)**, a nationally representative household survey conducted in multiple waves. The UPS provides repeated observations on households' access to and use of various financial services, including mobile money, commercial banks, and agent banking. It includes detailed demographic and geographic controls, which are essential for exploring **heterogeneity** in response to the tax across rural and urban households.

## Identification Strategy and Research Design

The core identification strategy exploits the **exogenous timing of the mobile money tax**, which was introduced on July 1, 2018. The initial policy applied a 1% tax on all mobile money transactions, including deposits and transfers. Due to public backlash, the tax was quickly revised to 0.5% on withdrawals only, yet the reform introduced a **highly salient cost shock** that permanently altered user expectations and behaviors (Spadavecchia, 2024).

We develop our analysis adopting two empirical approaches.

For our main results, we first develop an event study design meant to test for pre-trends and to investigate the dynamics of the treatment effect. Second, we implement a difference-in-differences specification using two-way fixed effects regressions. Our main assumption is that individuals substitute mobile money with other means of payment and money storage (namely cash and deposits) depending on the conveniency or the easiness of access to them. For our identification strategy, we employ a quasi-experimental design, leveraging the temporal variation introduced by the Mobile Money tax and the variation at geographical level coming from the heterogeneous access to Mobile Money alternatives, proxied by the density of ATMs.

[1] National IT Survey Uganda (NITA), 2018. See https://www.nita.go.ug/reports/national-it-survey-2018-final-report

We provide evidences at the user's, district's and bank's level.

## Econometric Specification

We hypothesize that the impact of the mobile money tax is heterogeneous across users, depending on the availability of alternative financial infrastructures — particularly access to formal banking services. To empirically test this hypothesis, we exploit variation in the **density of ATMs** across Ugandan districts as a proxy for the availability of substitutes to mobile money. Specifically, we implement a difference-in-differences (DiD) strategy that compares mobile money usage before and after the tax, between areas with high and low ATM penetration.The difference-in-differences design we exploit is the following:

$$Y_{idt} = \alpha_i + \alpha_t + \beta Post\ Tax_t \times I[High\ ATM\ density]_d + \varepsilon_{idt}$$

Where we define individual i in district d in the pre o post policy period defined by t, and where

- $Y_{idt}$ is the outcome variable for user i at time t, such as transaction value, number of withdrawals, etc.
- $Post\ Tax_t$ is a binary indicator equal to 1 for dates after the tax implementation, and 0 before.
- $I[High\ ATM\ density]_d$ indicates whether the individual resides in a district in the upper quartile of the ATM density distribution. We assign to each user the ATM density (calculated as number of ATMs over the districts area) of the district where she resides. We define $I[High\ ATM\ density]_d$ as a dummy indicating whether the users i in district d is in the highest 25 percentile of the users' distribution of ATM density. We use the subscript d as there are no users in the same district assigned to a different value of the dummy variable.
- $\alpha_i$ are **user fixed effects**, controlling for time-invariant heterogeneity across individuals or accounts.
- $\alpha_t$ are **time fixed effects**, capturing aggregate shocks, seasonality, or temporal trends.
- $\varepsilon_{idt}$ is the idiosyncratic error term.

The coefficient β captures the **average treatment effect of the tax**, identifying the discontinuity in behavior coinciding with the tax policy change. It has to be red as the difference in the effects of the tax on urban users (high ATM density) with respect to rural users (low ATM density). It captures the **differential impact of the tax in urban against rural areas**, where substitution options may be more limited and mobile money usage more critical for financial access

The coefficient β captures the differential effect of the mobile money tax on users residing in districts with high ATM density relative to those in areas with low ATM availability. A statistically significant estimate of ββ would indicate that the behavioral response to the tax — such as reductions in usage or shifts to alternative services — is conditional on the ease of substituting mobile money with formal financial infrastructure. In other words, it quantifies whether users in urban areas, where access to ATMs is typically higher, are more resilient to the tax shock compared to rural users, for whom mobile money often represents the only accessible financial service.

In addition to individual-level analysis, we replicate this framework at the district level, using aggregate outcomes computed over time for each district as the unit of observation. This enables us to triangulate the results and test whether the aggregate effects mirror the individual-level dynamics, providing a more robust picture of substitution behavior across Uganda's financial landscape.

To complement the baseline difference-in-differences analysis, we also implement an **event study approach** to trace the dynamic effects of the mobile money tax over time. This specification allows us to visualize the trajectory of treatment effects across months and to assess the validity of the parallel trends assumption. The estimating equation is:

$$Y_{idt} = \alpha_i + \alpha_t + \sum_{\tau=1,\tau\neq 5}^{T} \beta_\tau \text{Month}_\tau \times \mathbf{I}[\text{High ATM density}]_d + \epsilon_{idt}$$

In this framework:

- $Month\_\tau$ is a set of **binary indicators for each month** in the observation window, where the month immediately preceding the introduction of the tax is omitted and serves as the **reference period**.
- The coefficients $\beta_\tau$ of the interaction terms $Month_\tau \times I[High\ ATM\ density]_d$ capture the **time-varying differential effect** of the tax in high-ATM districts relative to low-ATM districts.

This model permits the construction of a dynamic treatment effects plot, showing the evolution of $\beta_\tau$ over time. If the pre-treatment coefficients are close to zero and statistically insignificant, this provides support for the parallel trends assumption. Post-treatment coefficients, on the other hand, illustrate the monthly adjustment in user behavior following the tax, allowing us to identify both the magnitude and persistence of the policy's impact.

We estimate this specification using individual-level panel data and replicate the analysis at the district level to examine whether the dynamics aggregate similarly at higher levels of geographic aggregation.

# Econometric analysis

This section presents the empirical evidence on how the mobile money tax reshaped financial behavior across different segments of the population and institutional infrastructure. We organize the analysis into three subsections, each addressing a distinct mechanism of substitution away from mobile money.

In the first subsection, we demonstrate that mobile money usage significantly declined among individuals residing in districts with greater access to formal banking infrastructure, as proxied by high ATM density. This result is consistently observed in both the transaction-level administrative data covering the full universe of mobile money activity and in the Ugandan National Panel Survey, which allows us to validate behavioral changes at the household level.

The second subsection explores how users respond to the mobile money tax by adopting agent banking services, a complementary financial innovation that facilitates cash deposits into bank accounts through a decentralized network of agents. We find that take-up of agent banking is disproportionately concentrated in districts with higher ATM density — areas that already enjoy easier physical access to formal financial services.

Finally, in the third subsection, we provide evidence that the demand for cash increases in precisely those areas where banks are more accessible. Specifically, we show that financial institutions with a larger market share of ATMs experience a notable rise in customer cash withdrawals following the introduction of the tax. This finding aligns with our overall narrative: mobile money services — previously used both for money storage and daily transactions — are now being substituted by formal bank accounts for savings, and by cash for payments.

## Mobile Money Usage Before and After the Tax

### Transaction level data

The initial evidence highlights a sharp decline in mobile money usage following the introduction of the tax. The administrative transaction data reveal a **significant and persistent drop in the average balances held in mobile money accounts**, indicating a shift in user behavior toward reduced liquidity holdings on digital wallets. The decline in balances begins immediately after the tax's introduction and remains below pre-tax trends throughout the observation period.

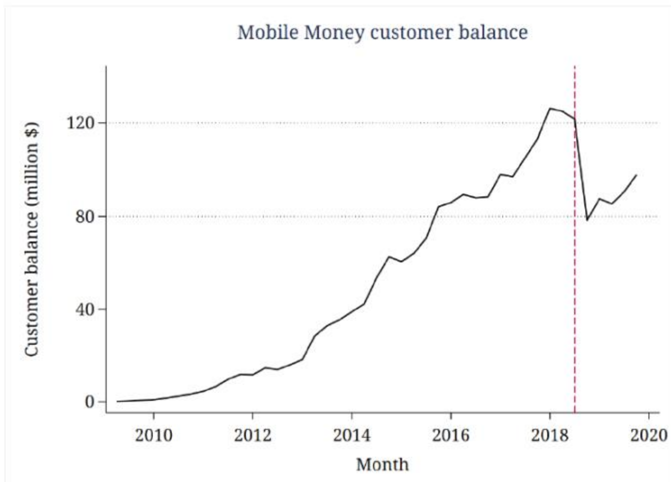


**Figure 0-1 Mobile Money customer balance**
Notes: This figure plots the quarterly customer balance of mobile money, expressed in US $. It represents the value of mobile money detained by users.

Alongside declining balances, the data also show a contraction in the **number and value of mobile money transactions**. We here presents results from the econometric model specified in previous section and using as outcome variable the individual's average daily amount of a given type of transaction, the number of times and the share of days in which that type of transaction was performed in a given month. We express all outcomes in log. We however restrict the sample to those users that perform a given type of transaction both in the pre-tax and the post-tax period. We hence interpret the coefficients as percentage change. We also show results for an additional measure, net deposits, i.e. the difference between deposits and withdrawals. This measures the money that a given individual deposits in the mobile money network net of the money she withdraws. Since the difference between deposits and withdrawals can take negative value, we cannot log transform the outcome variable: we hence standardize it, and the interpretation changes accordingly. Again, we do not include time fixed effects in order to show the generalized negative impact of the tax on mobile money usage. As before, the $Post\ Tax_t$ dummy represents the time fixed effect.

| | Sent | Received | Deposits | Withdrawals | Net |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Tax dummy$_t$ | -0.689*** | -0.607*** | -0.662*** | -0.256*** | -0.035*** |
| | (0.006) | (0.004) | (0.003) | (0.002) | (0.000) |
| Tax dummy$_t$ × High ATM density$_d$ | -0.103*** | -0.117*** | -0.040*** | -0.060*** | -0.004*** |
| | (0.009) | (0.007) | (0.004) | (0.003) | (0.001) |
| User FE | Yes | Yes | Yes | Yes | Yes |
| N. of users | 142522 | 225365 | 585690 | 691428 | 768061 |
| Obs. | 285044 | 450730 | 1171380 | 1382856 | 1536122 |
| Adj. R sq. | 0.438 | 0.349 | 0.407 | 0.448 | 0.225 |
| Mean Dep. Var. High ATM | 2900.033 | 2497.483 | 5883.921 | 5804.549 | -220.917 |
| Mean Dep. Var. Low ATM | 2253.829 | 1849.746 | 4292.215 | 4178.469 | -171.947 |

**Table 0-1 Intensive margin: performed transactions**
In this table, we use our difference-in-differences approach and we show how mobile money users in high ATM density districts respond to the introduction of the mobile money tax at the intensive margin, relatively to users in low ATM density districts. High-ATM-density users transact between 4% and 12% less with respect to low-ATM-density users, after the tax. We estimate the effect on the sample of users that performed transactions of a given type before and after the tax. Column (1) show the effects on the amount of mobile money sent, column (2) on the amount received, column (3) on the amount deposited, column (4) on the amount withdrawn. For columns (1)-(4) outcome variables are the log of the average daily amount. In column (5) we use as outcome variable the standardized value of the difference between deposits and withdrawals. Standard errors are clustered at the individual level. $***$, $**$ and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

We complement the analysis on the intensive margin adopting a difference-in-differences and an event study approach using monthly level data at the individual level. In the Figure below, we show results for the event study on the log average daily value transacted in a month. This event study specification does not lend themselves to immediate interpretation, and thus merits a detailed explanation of the dataset structure and the meaning of the estimated coefficients.

As with the rest of the empirical analysis, these models estimate effects at the **intensive margin** of mobile money usage. That is, we only observe non-zero transaction values in months when the user engages in a transaction; for all other months, the observation is recorded as missing rather than zero. Users are not required to transact every month, which implies **unbalanced timing of transactions** across individuals. For example, user *i* might transact in April and August, while user *j* does so in May, September, and November.

Including **individual fixed effects** helps address this source of heterogeneity by controlling for user-specific transaction patterns over time — we are thus estimating the tax effect **within individuals**, comparing only periods when a given individual is active. Similarly, **month fixed effects** capture aggregate, time-specific shocks or seasonal variation common across all users.

In the **difference-in-differences specification**, the coefficient β captures the **average treatment effect** of the tax for users in high-ATM-density districts relative to those in low-ATM-density districts. It reflects how otherwise comparable individuals respond differently to the tax, depending on the availability of mobile money substitutes.

In the **event study framework**, the monthly coefficients $\beta_\tau$ represent the **differential change** in the outcome for users in high-ATM-density areas relative to their counterparts in low-density districts, **for each month τ**, compared to the **reference month of May** (just before the tax implementation). These coefficients allow us to track the dynamics of behavioral adjustment over time and assess whether treatment effects were already emerging prior to the tax (violating parallel trends) or materialize only after its introduction.

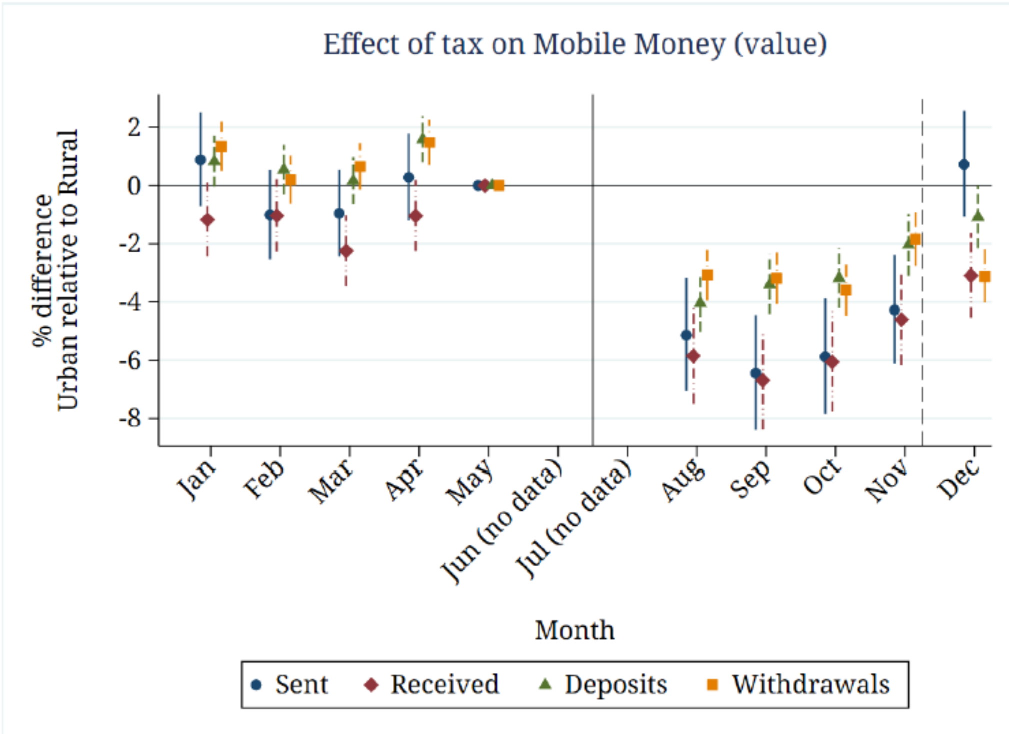


**Figure 0-2 Differential effect of the tax on users in high ATM density districts**
This figure plots the coefficients $\beta$ of the event study approach. We use as outcome variable the log of average daily value of mobile money transactions in a month at the individual level. We differentiate between type of transactions. We already express the y axis in terms of % change. We use May as the baseline month. Data for June and July are excluded due to issues with data collection. Standard errors are clustered at the individual level, and the figure reports 95% confidence interval.

The pattern of usage suggests both a **mechanical effect of the tax (price sensitivity)** and a **psychological effect of policy uncertainty**. Although the tax was revised just days after its initial implementation, the policy reversal did not fully restore user behavior to pre-tax levels. This persistence indicates a loss of trust and a shift in the perceived cost of using mobile money — a point consistent with theoretical predictions of salience and reference dependence in behavioral responses to policy changes (Chetty et al., 2009; Dupas et al., 2018).

## Survey data

To further confirm our previous results, we analyzed data from the Ugandan National Panel Survey (UNPS). The UNPS is carried out by the Ugandan Bureau of Statistics over a twelve-month period (a "wave") on a nationally representative sample of individuals/households, for the purpose of accommodating the seasonality associated with the composition of and expenditures on consumption. The UNPS set out to track and interview more than 5'000 individuals.

We employ data from the 2018/2019 wave, focusing on the outcomes related to mobile money usage. We adopt the identification proposed by Bassi and Rasul (2017), where the identification comes from the timing of the interview, before or after the tax. Controlling for individuals' characteristics, district and time FEs. To notice, as the authors propose, we cluster standard error at the week level. This clustering reflects that identification in our research design is based on time variation.

We provide further evidence of the drop of mobile money usage in districts with high ATM density after the introduction of the tax, and exploit the following:

$$Y_{idt} = \alpha_d + \alpha_t + \beta \mathbf{I}[\text{High ATM density}]_d + \gamma \mathbf{X}_i + \epsilon_{idt}$$

where the outcome is referred to individual i in district d at time t. We control for the individual's characteristics, and include district and time FEs. Since during one wave individuals cannot be tracked (as they answer questions on mobile money just once), our source of variation comes from the timing of their interview, before or after the introduction of the tax. In the Table below we report the results of the linear probability model described above, where outcome variables are dichotomous as they indicate whether the individual used a given mobile money service or not in the last week. For all measures, we find that individuals in high ATM density areas are up to 9% less likely to use mobile money.

| | Send | Transfer cash | Withdraw | Pay utilities | Pay school |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Tax dummy$_t$ × $\mathbf{I}$ [High ATM density] $_h$ | -0.061* | -0.019* | -0.093*** | -0.036** | -0.019* |
| | (0.034) | (0.010) | (0.030) | (0.015) | (0.010) |
| District FE | Yes | Yes | Yes | Yes | Yes |
| Date FE | Yes | Yes | Yes | Yes | Yes |
| Obs. | 5044 | 5047 | 5060 | 5043 | 5044 |
| Adj. R sq. | 0.224 | 0.117 | 0.246 | 0.160 | 0.046 |
| Mean Dep. Var. | 0.336 | 0.021 | 0.320 | 0.030 | 0.010 |

**Table 0-2 Mobile Money usage - Survey data**
This table reports the coefficients of the difference-in-differences approach on the survey data of UNPS. The outcome variables are dummy variables taking value 1 if the individual used a given mobile money service in the past week. We control for individual's characteristics such as gender, age and marital status. Time and district FEs are included. Standard errors are clustered at the week level, as suggested by Bassi and Rasul (2017). ∗∗∗, ∗∗ and ∗ indicate significance at the 1%, 5% and 10% level, respectively.

## Evidence of Substitution: increased bank deposits

The introduction of the tax lowered the conveniency of Mobile Money with respect to other technologies that facilitate the exchange of money. Corroborating the findings of Crouzet et al. (2019), consistent with the predictions of a technology adoption model with complementarities, we show that the adoption of Banking Agents increased persistently as a response to the contraction registered by mobile money after the tax. As explained, banking agents are a technology that allows the execution of bank-related activities, such as deposits, in the fashion of branchless banking. The adoption of this technology is highly demand driven: indeed, it is not the bank who decides where to open a new banking agents. Like mobile money agents, it is merchants or individuals themselves who decide whether to start offering this service. While they bear the fixed costs needed to start such activity, they earn a fee on each transaction they perform.

In this subsection, we present evidence that the spread of banking agents spurred after the introduction of the mobile money tax. This is particularly true in districts with high ATM density and for banks with a high ATM market share. These results are justified by the complementary that arises between banking agents and ATMs. Indeed, banking agents have more incentive to start their activity where the users are already acquainted to the banking system or where there is a pervasive access to ATMs, that facilitate the withdrawal of deposited cash. Moreover, banking agents also have an incentive to provide the service for banks which are more pervasive: being the fixed costs of becoming a banking agent the same for any bank (consisting it in learning how to use the technology, which is shared between all banks), agents surely want to serve the highest possible number of customers. Similarly, we will also show that the number of banking agents grow relatively more for those banks who have a higher market share of ATMs.

In the Figure below we present the result of the event-study which uses as outcome variable the number of banking agents over time. Again, the coefficients estimate the differential between urban and rural districts.

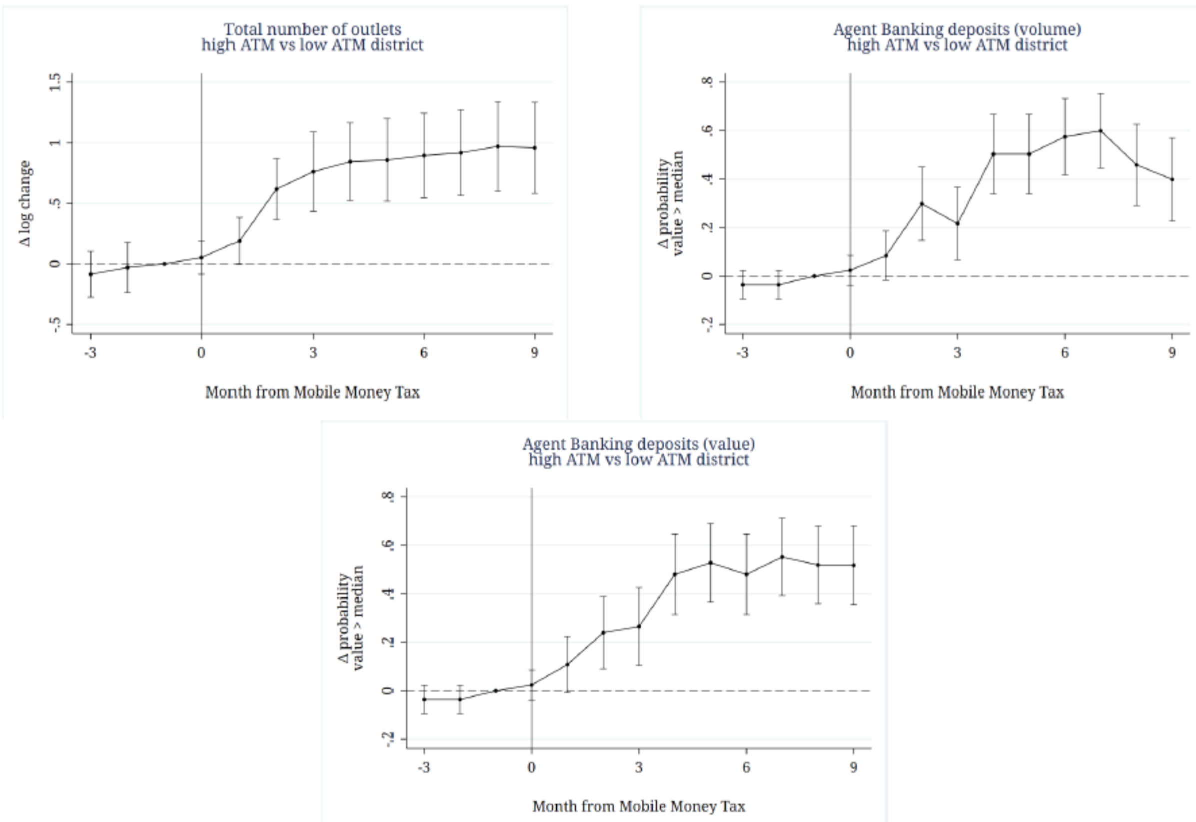


**Figure 0-3 Banking agents: high- vs low-ATM density**
In this panel we plot the coefficients of the event-study approach, where we use as outcome variable the log number of banking agents (top left), a dummy for banking agents' deposits volume (top right) and value (bottom) above median. All outcome variables are at the district level. The plotted coefficient represents the differential between high- and low-ATM density district, with respect to the reference period. We use as reference the month before the introduction of the mobile money tax. Standard errors are clustered at the bank level and we report 90% confidence intervals.

We also present results of the difference-in-differences approach:

| | Volume | | | Value | | |
|---|---|---|---|---|---|---|
| | Δ Level ('000) (1) | Δ Log (2) | Δ Pr > median (3) | Δ Level ('000) (4) | Δ Log (5) | Δ Pr > median (6) |
| Tax dummy$_t$ × High ATM density$_c$ | 0.323** | 2.164*** | 0.390*** | 0.098* | 6.748*** | 0.395*** |
| | (0.142) | (0.369) | (0.064) | (0.050) | (1.271) | (0.066) |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Obs. | 1495 | 1495 | 1495 | 1495 | 1495 | 1495 |
| Adj. R sq. | 0.484 | 0.683 | 0.528 | 0.500 | 0.664 | 0.539 |
| Mean Dep. Var. | 0.076 | 1.098 | 0.146 | 0.023 | 4.863 | 0.157 |

**Table 0-3 Banking agents deposits**
This table reports the coefficients of the difference-in-differences approach. The outcome variables are the number and the value of deposits made by customers to Banking Agents. They are expressed in level, log, or as a dummy indicating whether the value is below or above the median as proposed in Chen and Roth (2023). Time and district FEs are included. Standard errors are clustered at the district level. ∗∗∗, ∗∗ and ∗ indicate significance at the 1%, 5% and 10% level, respectively.

## Evidence of substitution: increased use of cash

In this last subsection, we show evidence of the increased usage of cash. We provide two levels of analysis, at the district and at the bank level. We first show that the request for cash at the district level increases relatively more in districts with high ATM density after the tax. Then, we also provide evidence that banks in the top quartile of the ATM market share distribution witness a significant increase in cash withdrawn through ATMs. Again, this corroborates our thesis that users use cash for payments, and hence constantly withdraw the cash deposited in banks through ATMs.

### Increased cash demand at the district level

We present evidence that district with high ATM density present an increased demand for physical cash. These results further corroborates the hypothesis that mobile money is substituted by bank deposits and cash after the introduction of the tax: banks are used for money storage through banking agents, ATMs register an increase in withdrawals, and physical cash is now used for transaction.

We present evidence that district with high ATM density present an increased demand for physical cash. These results further corroborates the hypothesis that mobile money is substituted by bank deposits and cash after the introduction of the tax: banks are used for money storage through banking agents, ATMs register an increase in withdrawals, and physical cash is now used for transaction.

We use data from total issuance of physical cash. While data on cash withdrawals at the individual branch do not exist, we exploit data at the bank-district level. We use data from 26 banks in 10 different districts. We define the bank-district pairs as branches. We use monthly data spanning from 2017 to 2022.

We exploit the following difference-in-differences specification, where we include the interactions between the post tax dummy and a dummy identifying those districts in the highest quartile of the ATM density distribution. This means that all branches within the same district will be assigned the same ATM density. We exploit the following difference-in-differences:

$$\mathrm{Y}_{bdmy} = \alpha_{bd} + \alpha_{my} + \beta \text{Post Tax}_{my} \times \mathbf{I}[\text{High ATM density}]_d + \epsilon_{bdmy}$$

where the outcome variable is the log value of notes issued by bank b in district d. Our preferred specification contains district-month FE that account for seasonality and bank-district FE that allow comparison of the same branch.

| | Log cash withdrawn | |
|---|---|---|
| | (1) | (2) |
| Post $\text{Tax}_t \times$ High ATM density $_d$ | 0.304*** | 0.231*** |
| | (0.061) | (0.055) |
| Branch FE | Yes | Yes |
| Time FE | Yes | Yes |
| District × Month FE | | Yes |
| Obs. | 2622 | 2622 |
| Adj. R sq. | 0.543 | 0.542 |
| Mean Dep. Var. | 21.745 | 21.745 |

**Table 0-4 Cash issuance**
This table reports the coefficients of the difference-in-differences approach. The outcome variable is the log value of cash issued by the Central Bank to private banks. The unit of observation is the private bank-district pair, that we define as branch. We control for branch and time FEs in column (1), and add branch-month FEs in column (2) to account for seasonality. Standard errors are clustered at the district level. $***$, $**$ and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

## Increased usage of ATMs at the bank level

Eventually, we provide evidence of the increased ATM withdrawals for those banks in the higher quartile of the ATM market share distribution. We use quarterly data at the bank level on the value of ATM withdrawals.

We estimate the following:

$$Y_{bqy} = \alpha_b + \alpha_{qy} + \beta \text{Post Tax}_{qy} \times \mathbf{I}[\text{ATM market share}]_b + \epsilon_{bqy}$$

where the unit of observation is bank b in quarter q in year y. The coefficient $\beta$ expresses the differential change in the outcome after the tax for banks in the highest quartile of the ATM market share. The independent variable *I[ATM market share]* is defined at the bank level in the pre-policy period. It is interacted with a post-policy dummy. Bank and time FEs are included, hence all individual terms are absorbed. We report the results in the table below, and also include the results when using as independent variable the ATMs market share of the bank.

| | ATM withdrawals | |
|---|---|---|
| | Log | Log |
| | (1) | (2) |
| Post Tax × **I**[ATM Market share] | 0.029** | |
| | (0.012) | |
| Post Tax × Market share of urban ATMs | | 0.003*** |
| | | (0.000) |
| Bank FE | Yes | Yes |
| Time FE | Yes | Yes |
| Obs. | 263 | 263 |
| Adj. R sq. | 0.984 | 0.992 |
| Mean Dep. Var. | 0.025 | 0.025 |

*Table 0-5 ATM withdrawals*
This table reports the coefficients of the difference-in-differences approach. The outcome variables are the value of ATM withdrawals (in billion UGX). The unit of observation is the private bank at quarterly level. We control for bank and time FEs. Standard errors are clustered at the bank level. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively.

# Interpretation in the Policy Context

Uganda's mobile money tax was among the first of its kind in Africa and triggered widespread debate about the taxation of digital financial services. While the government's objective was to broaden the tax base and capture value from a rapidly growing sector, the reform exposed the **fragility of digital ecosystems to policy shocks**, especially when applied regressively or without infrastructure buffers.

The evidence presented here illustrates that **even small frictions** can cause large and persistent behavioral changes, especially when they affect key services like withdrawals. It also confirms that **behavioral and policy responses** are highly context-dependent — driven by access, alternatives, and user expectations.

Similar patterns have been observed in other countries, including Ghana (UNCDF, 2022; GSMA, 2022), where digital levies triggered temporary or permanent reductions in usage. However, Uganda remains unique in the depth of administrative and survey data available to analyze the effects, making it a critical case study for other countries considering similar fiscal measures.

## Digital Infrastructure, Platform Competition, and Design Risks

The Ugandan case illustrates how digital financial systems — while appearing robust — can be **highly sensitive to cost structure changes**, especially when those costs are applied at critical nodes such as withdrawals. The institutional architecture of mobile money is based on an ecosystem of **agents, users, and digital balances**, all of which rely on predictable rules and cost structures. When any component is perturbed — particularly by state-imposed frictions — the network re-optimizes in ways that may reduce inclusion or shift users back to older models of banking and liquidity management.

Mobile money's comparative advantage in Uganda rested on three pillars: low transaction costs, high geographic reach via agents, and minimal onboarding friction. By introducing a tax that undermined these features — even temporarily — the government disrupted users' expectations and **provoked reallocation across platforms**. The evidence from transaction-level data and household panels shows that users **do not respond marginally**: they shift dramatically when the structure of costs or convenience changes.

Moreover, the system's fragility was exacerbated by Uganda's **dual financial architecture**, in which mobile money and formal banks coexist but are **not seamlessly integrated**. While substitution toward banks was feasible for many urban users, the **lack of interoperability**, high fixed costs of banking, and geographic limitations meant that rural users were left with few viable options. This creates a bifurcated system in which certain policy shocks can simultaneously formalize and exclude, depending on local conditions.

This phenomenon resonates with broader concerns about **platform design and digital public infrastructure**. As more countries build financial inclusion strategies on mobile-first platforms, questions emerge about **how to tax, regulate, and stabilize** these ecosystems without generating large-scale exclusion or behavioral reversals. The Ugandan experience is instructive: even partial or short-lived cost increases, when applied to essential transactions, can unravel years of inclusion gains.

### Policy Implications and Recommendations

The findings presented in this section lead to several clear policy takeaways, relevant not only for Uganda but for any country considering similar taxation or regulatory reforms in the digital finance space:

The mobile money tax imposed higher effective costs on users transacting in small amounts — disproportionately low-income and rural households. This regressive structure led to greater costs among the most vulnerable, undermining the financial inclusion goals the digital finance sector was designed to serve.

The initial imposition of a broad-based 1% tax on all mobile money transactions, and its swift revision to a narrower 0.5% withdrawal-only tax, created confusion and **eroded trust in the system**. As documented in the administrative data, usage patterns did not recover even after the policy change. This suggests that clear, gradual, and well-communicated reforms are less likely to destabilize usage.

While some users successfully shifted to formal banks and banking agents, this was **contingent on access**. In rural areas, substitution was limited by infrastructure and distance to service points. Any attempt to manage digital finance through fiscal tools must therefore account for **geographic and institutional constraints** on substitution.

The use of transaction-level mobile money data and panel survey evidence in this analysis demonstrates the value of granular, high-frequency information for **evaluating financial policy interventions**. Similar data-sharing frameworks should be promoted across regulators and providers to enable real-time monitoring of system-level impacts.

Finally, the case reinforces the need for **policy-resilient infrastructure**: systems that maintain core usage even under moderate cost shocks. This may involve building redundancy through interoperability, preserving access to low-cost channels, and ensuring clear lines of accountability when transaction costs are altered.

## Conclusion

Uganda's 2018 mobile money tax offers a rare natural experiment in platform-level taxation and user response in a low-income setting. Leveraging high-frequency transaction-level administrative data and rich household survey evidence, this section has documented the significant and persistent decline in mobile money usage following the introduction of the tax, as well as a partial substitution toward banks and banking agents, particularly in urban areas.

The analysis reveals a regressive effect in rural regions, where substitution options were limited and dependence on mobile money was highest. The response underscores the importance of cost-sensitive design in digital financial ecosystems and the fragility of inclusion gains when policy reforms are introduced without structural buffers.

As other countries explore fiscal measures to capture revenue from growing digital sectors, Uganda's experience provides a critical warning: small taxes, poorly structured, can have large and lasting consequences for financial access. Future research and policy design must integrate behavioral insights, spatial heterogeneity, and infrastructural realities to ensure that digital financial systems remain inclusive, stable, and resilient.

This report has examined the consequences of Uganda's 2018 mobile money tax through a rich combination of administrative, geospatial, and survey data. The findings show that the tax triggered immediate and profound shifts in user behaviour, liquidity flows, and bank lending strategies. Mobile money usage declined markedly, particularly among users in better-connected districts. In response, many individuals substituted toward agent banking and ATM withdrawals. However, this reallocation was uneven, favouring regions with pre-existing infrastructure and disadvantaging more remote or underserved populations.

While these behavioural changes led to short-term liquidity inflows into the banking system, the deposits were volatile and transitory. This instability prompted banks to reassess their lending strategies, resulting in a reallocation of credit toward safer borrowers. Riskier clients—particularly those without prior credit histories—faced higher interest rates and shorter loan maturities. Consequently, rather than enhancing financial access, the tax catalyzed exclusionary dynamics that disproportionately affected vulnerable groups.

These outcomes underscore a broader message for policymakers. Regulatory changes to digital financial systems—even when seemingly narrow in scope—can generate ripple effects across the economy. The Ugandan case illustrates how user behaviour, institutional response, and infrastructure interact in complex ways, producing outcomes that defy linear predictions. Taxes on digital services should therefore be evaluated not only in fiscal terms but also with regard to their systemic implications for financial access, trust, and resilience.

More broadly, our analysis contributes to global conversations about the future of money and the role of governments in shaping payment systems. As many countries contemplate introducing CBDCs, expanding agent networks, or taxing platform-based finance, the Ugandan experience offers a clear and timely lesson: financial inclusion is not only about expanding access, but also about maintaining stability and designing policies that reinforce—rather than disrupt—the pathways through which individuals and institutions manage their financial lives.

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