Economics Letters 263 (2026) 112931

Contents lists available at ScienceDirect

Economics Letters

journal homepage: www.elsevier.com/locate/ecolet

Open banking and digital financial inclusion: Early evidence

Michael Ryan *, Abhishek Mukherjee

Division of Management, University of Waikato, Tauranga, New Zealand

ARTICLE INFO

Dataset link: https://data.mendeley.com/datasets/mfmjkm2zxk/2

JEL classification:
C23
D14
G28

Keywords:
Open banking
Digital financial inclusion
Fintech regulation
Difference-in-differences

ABSTRACT

Using a large panel of countries, we study whether open-banking adoption expands digital financial inclusion. Applying difference-in-differences methods, we find that open banking increases the share of adults making or receiving a digital payment, with the largest effect among less-educated adults. We find, however, no statistically significant effect of open banking on the number of mobile/internet banking transactions per 1000 adults. One interpretation of our results is that open banking benefits may accrue through non-bank channels to those who previously did not use digital means of payment.

1. Introduction

Financial inclusion, the availability and equality of opportunities to access financial services, matters for economic development and poverty reduction (Demirguc-Kunt et al., 2018). Open banking is seen as a route to financial inclusion, particularly in developing and emerging economies (OECD, 2023, p.6). Indeed, one-third of countries adopting open banking explicitly cite financial inclusion as a motivation (Babina et al., 2025, Table 1). An OECD report notes open banking can serve the unbanked (OECD, 2023, p.5), while Plaitakis and Staschen (2020, p.6) state that open banking may improve services for the underbanked by enhancing the quality and availability of financial offerings. No prior study, however, empirically evaluates whether open-banking adoption expands financial inclusion; we do so using a difference-in-differences approach.

2. Data and estimation strategy

We use the year in which data-sharing mandates came into effect, as reported by Babina et al. (2025), to define each country's open banking adoption year. Data sharing among financial institutions may intensify competition, improving the cost, quality and variety of digital financial services available to customers, thereby encouraging their use (Babina et al., 2025; He et al., 2023). We focus on digital financial inclusion as this is where any financial inclusion gains from data sharing should be most evident. Our two measures of digital financial inclusion are: the (log of the) number of mobile and internet banking transactions per 1000 adults sourced from the IMF's Financial Access Survey, and the percentage of the population aged 15 and over that made or received a digital payment in the past year according to the World Bank Global Findex database.[1] The first metric covers formal banking; the second also captures non-bank transactions (and debit or credit card use). Our balanced panels include 48 countries for the banking transactions measure (Panel A) and 111 countries (through to 2021) or 71 countries (through to 2024) for the digital payments percentage measure (Panel B); see Table 1.

Our main specification uses the doubly-robust difference-in-differences estimator of Callaway and Sant'Anna (2021) (hereafter CS-DR), which combines inverse probability weighting and regression adjustment; we prefer it for its robustness against model misspecification (Callaway and Sant'Anna, 2021, p. 212).

3. Results

3.1. Outcome: the number of digital (mobile and internet) banking transactions per 1000 adults

In panel A there is one large cohort of countries that adopt in 2019 and then some single country cohorts that adopt in 2018 and after 2020 (see Table 1, third column). Initially we restrict our dataset to countries

* Corresponding author.
E-mail address: michael.ryan@waikato.ac.nz (M. Ryan).

[1] Digital payments include payments made using mobile money, a debit or credit card, or payments made from an account using the internet or a mobile phone; see the Glossary to the 2021 Global Findex report.

https://doi.org/10.1016/j.econlet.2026.112931
Received 29 September 2025; Received in revised form 23 February 2026; Accepted 9 March 2026
0165-1765/© 2026 The Authors. Published by Elsevier B.V. This is an open access article under the CC BY-NC license (http://creativecommons.org/licenses/by-nc/4.0/).

Table 1
Counts of open banking adoption in various samples.

Adoption year	Babina et al. (2025) sample	Panel A The number of digital banking transactions per 1000 adults	Panel B The percent of adults (15+) making digital payments	
			Panel ending 2021	Panel ending 2024
Not yet	122	26[a]	70[b]	61[b]
2018	3	1	3	1
2019	32	19	28	4
2020	2	0	2	0
2021	5	1	4	2
2022	2	1	2	2
2023	1	0	1	0
2025	1	0	1	1

Notes: Adoption year is the year of the data sharing date reported by Babina et al. (2025).
[a] Lebanon was dropped from matched sample owing to missing GDP data.
[b] 12 countries were dropped from the original matched sample owing to missing covariates; 12 more were dropped because the percentage of digital payments was recorded in 2022 not 2021.

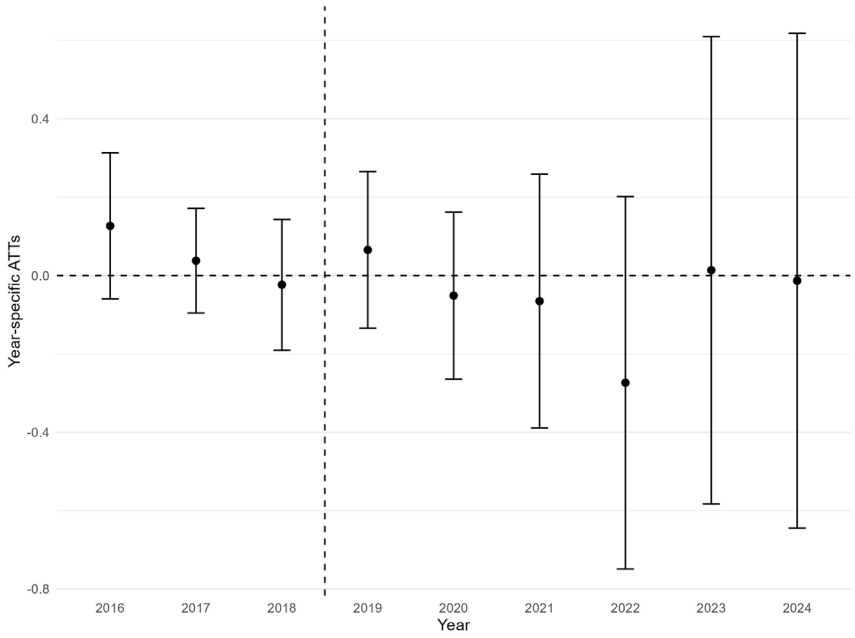

Fig. 1. Year-specific estimates of the Average Treatment Effect on the Treated of open banking on the number of digital transactions per 1000 adults. Note: 95% confidence intervals shown.

that adopted in 2019 (our 'treated' group; country count = 19), and to countries that have not yet adopted open banking (our 'control' group; country count = 26). With this slightly smaller panel, we use the CS-DR estimator to estimate the yearly Average Treatment Effect on the Treated (ATT). Fig. 1 shows that the yearly ATTs before 2019 are not statistically significantly different from zero, supporting the critical conditional parallel trends assumption.[2] Further, the estimated ATTs after the adoption of open banking in 2019 are not statistically different from zero; this suggests open banking does not have a statistically significant impact on the number of digital banking transactions per 1000 adults.

Four robustness tests are carried out. First, we include the log of population as a covariate; including it caused the weighting step to fail in the CS-DR model, so we estimate the regression-adjustment-only version of the Callaway and Sant'Anna (2021) estimator (CS-RA). We also use the Lee and Wooldridge (2025) estimator, the simple Two-Way Fixed Effects estimator and the Extended Two-Way Fixed Effects estimator (Wooldridge, 2023, 2025). Table 2 shows in all of these specifications the overall ATT (the mean of the yearly ATTs from 2019 to 2024) is not statistically different from zero at a 5 percent level of statistical significance.

The countries that adopted open banking in 2019 in our sample are all high-income countries.[3] The remaining adopters in this panel are India (adoption date 2018), Brazil (2021), and Indonesia (2022). We examine the impact of open banking in these economies to test if open banking is more beneficial in lower income economies. Lee and Wooldridge (2025, p. 1) note their approach to difference-in-differences (combined with randomisation inference) can be used when the treated number of units is small (even one). In separate estimations, we compare India, Brazil and Indonesia to the non-adopting countries in our panel using the heterogeneous trends method of Lee and Wooldridge (2025). Again, we see no statistically significant effect from open banking in any of these countries, with the overall ATTs being −0.548 (exact p-value 0.26) for India, −0.016 (exact p-value 0.93) for Brazil and 0.548 (exact p-value 0.37) for Indonesia.

[2] Results from formal testing of the pre-trends are in the Supplementary Materials (Table 3).

[3] See the Supplementary Materials (Table 2).

Table 2
Overall ATTs for the impact of open banking on the number of digital banking transactions per 1000 adults.

Estimator	Covariates included				Overall ATT estimate
	log(GDP per capita)	Mobile	Internet	log(Pop.)	
2019 adopters (treated) vs. non-adopters (controls)					
CS-DR	×	×	×		−0.05 [−0.35, 0.24]
CS-RA	×	×	×	×	−0.21 [−0.56, 0.20]
LW					−0.22 [−0.69, 0.24]
TWFE	×	×	×	×	−0.29 [−0.73, 0.15]
ETWFE	×	×	×	×	0.03 [−1.34, 1.39]

Notes: CS-DR = Callaway–Sant'Anna doubly robust; CS-RA = Callaway–Sant'Anna regression adjustment; LW = Lee–Wooldridge; TWFE = two-way fixed effects; ETWFE = extended TWFE. In the ATT column, 95 per cent confidence intervals are reported (in the square brackets) based on standard errors clustered at a country level. The exception being the 'LW' estimator where the confidence intervals are based on 'HC3' standard errors; see Lee and Wooldridge (2025, p.37). The covariates were sourced from the World Bank. Pop. = population. Mobile = the number of mobile cell phone subscriptions per 100 people. Internet = percent of individuals using the internet. In some country-year pairs we extrapolate missing values for the internet/mobile phone variables; see Table 1 of the Supplementary Materials; we do this via the Kalman filter. With the LW model unit-specific trends are removed, we therefore did not feel the need to add covariates.

3.2. Outcome: Percent of adult population making or receiving digital payments

Data on the percent of adult population making or receiving digital payments were matched for 2014, 2017, and 2021 for 111 countries in the Babina et al. (2025) sample; further, 2024 data are available for 71 of these countries (see Table 1).[4] We define the treated countries as those adopting open banking between 2018 and 2021, and restrict the control group to countries that have yet to adopt. For the panel ending 2021 this means there is one adopting cohort of 37 countries and the sample consists of 107 countries; in the panel ending 2024 there are seven adopters out of a sample of 68 countries.[5] Unless otherwise stated, in the estimations reported below we use the CS-DR estimator with the log of GDP per capita, log of population, the number of mobile cell phone subscriptions per 100 people and percent of individuals using the internet as covariates.

For the panel ending in 2021, Table 3, fourth column, shows that the estimated ATT for 2021 is positive and statistically significant. Specifically, open banking adoption leads to a 3 percentage point increase in digital payment percentages; for context, the mean percent of the adult population who made a digital payment in non-adopting countries in 2017 was 44 percent. If the Lee and Wooldridge (2025) estimator is used, we obtain an ATT estimate of 6.9 percentage points, which is also statistically significant at the 5% level. The two-way fixed effects estimator ("TWFE") does not, however, return statistically significant an ATT. We, however, put little weight on the TWFE result as conditional parallel trends does not hold for this specification (see the Table 4 of the Supplementary Materials).

If open banking lowers barriers to digital payment adoption, its effects should be concentrated among groups that previously faced the greatest barriers. For each country, the Global Findex survey provides digital payment percentages for two income groups: the poorest 40% and richest 60% of households. In the panel ending 2021, the estimated ATT is statistically insignificant for both the poorest 40% and richest 60% income groups. The Findex income classification, however, reflects relative within-country positions meaning the "poorest 40%" potentially represents very different absolute digital financial disadvantage across countries. We therefore examine heterogeneity by educational attainment (primary education or less vs. beyond primary) as low education may be a more comparable indicator of digital financial disadvantage across countries than relative income thresholds. We find that open banking adoption is associated with an 8.5 percentage point increase in the digital payment percentages among less-educated adults, relative to less-educated adults in non-adopting countries. The estimated effect of open banking adoption for adults with secondary education or above is near zero (+0.4 pp) and statistically insignificant. These results suggest that open banking's aggregate effect on digital payments operates primarily through the inclusion of less-educated populations, the group most likely to have been excluded from traditional digital banking, strengthening the support for open banking as a financial inclusion policy.

In the panel ending in 2024 (with fewer countries), we find no statistically significant effect of open banking on the digital-payments percentage for the full sample (across all specifications) or for any subgroup (Table 3, column 5). This contrasts with the statistically significant ATTs observed when the panel ends in 2021. To distinguish between the effect of extending the sample period and the effect of the smaller number of adopters available in the 2024 panel, we re-estimate the ATT for the panel ending in 2024 but excluding the 2024 data point (Table 3, row *2024 respondents*). The estimate remains insignificant, indicating that the difference relative to the 2021 results is driven by sample composition rather than the inclusion of 2024 as a data point. The 2024 panel contains only seven adopting countries (i.e., seven treated units), which makes the ATT estimates sensitive to country-specific events in these adopting countries; we therefore put more weight on the results from the panel ending 2021 which has 37 adopters.

4. Discussion

Three readings of the evidence presented here are possible. First, open banking raises financial inclusion: we find that open banking increases the share of adults making a digital payment in the panel with more adopting countries (Panel B ending in 2021), with effects concentrated among less-educated adults. The absence of a statistically significant effect of open banking on the number of banking transactions may reflect that Panel A (where transactions are the outcome)

[4] After the initial matching, some countries were dropped owing to other data reasons; see the notes to Table 1, the counts in the text do not count these exclusions.

[5] In the panel ending 2024, a second cohort of open banking adopters could have been analysed: those that adopted in 2022, but this two member cohort was deemed too small.

Table 3
ATT estimates for the effect of open banking on the percentage of the population making digital payments.

			Overall ATT	
Sample	Estimator	SE	Panel ending 2021	Panel ending 2024
Baseline				
Baseline	CS–DR	Clust.	0.030	−0.000
			[0.005, 0.055]	[−0.054, 0.054]
Baseline	LW	HC3	0.069	0.043
			[0.020, 0.118]	[−0.133, 0.219]
Baseline	TWFE	Clust.	−0.037	0.030
			[−0.086, 0.012]	[−0.038, 0.098]
Subgroup estimates (CS–DR)				
Primary education or less	CS–DR	Clust.	0.085	0.034
			[0.005, 0.165]	[−0.040, 0.108]
Secondary education or more	CS–DR	Clust.	0.004	−0.003
			[−0.016, 0.024]	[−0.072, 0.066]
Poorest 40%	CS–DR	Clust.	0.019	0.012
			[−0.015, 0.053]	[−0.069, 0.093]
Richest 60%	CS–DR	Clust.	0.029	−0.005
			[−0.003, 0.061]	[−0.043, 0.033]
Robustness				
2024 respondents	CS–DR	Clust.	0.002	–
			[−0.050, 0.054]	
Panel A countries (2019 adopters)	CS–DR	Clust.	0.025	–
			[−0.018, 0.068]	
Panel A countries (2019 adopters)	LW	HC3	0.043	–
			[−0.024, 0.11]	

Notes: See the notes to Table 2. For India (2021), Kenya (2021), Uganda (2017), and for 50 out of 68 2024 values we impute internet penetration using the last observed value. All models except LW include log GDP per capita, log population, and mobile and internet penetration as covariates.

includes fewer countries, yielding imprecise estimates. Consistent with this, when we examine Panel B's digital-payments outcome but restrict the sample to Panel A countries, the estimated effects remain positive but are statistically insignificant: 2.5 and 4.3 percentage points using the CS-DR and Lee and Wooldridge (2025) estimators, respectively (Table 3, *Panel A countries (2019 adopters)*). These point estimates are only modestly smaller than those in the full Panel B sample (3.0 and 6.9 percentage points).[6]

A second reading accepts the finding that open banking does not affect the number of banking transactions at face value. If this result is accepted, reconciling the divergence in results between the two financial inclusion measures is required. This could be done by noting that the digital payment percentage measure captures both bank and non-bank activity, whereas the number of banking transactions reflects activity only the formal banking sector.[7] Open banking may therefore be facilitating the use of non-bank digital financial services by those who previously did not use digital payment methods. This is consistent with findings by Fang and Zhu (2023), who found in India and Brazil, open banking adoption increased the propensity for customers to take loans from informal financial institutions. A third reading prioritises the Panel B (ending 2024) null results, concluding open banking has no effect on either measure. We discount this reading given the small number of adopters in this panel.

Declaration of Generative AI and AI-assisted technologies in the writing process

During the preparation of this work one author used ChatGPT in order to review selected sentences to improve clarity. Claude code also checked the analysis. We also asked ChatGPT to proofread the final document. After using this tool/service, the authors reviewed and edited the content as needed and takes full responsibility for the content of the published article.

[6] A caveat is that the parallel-trends assumption does not appear to hold for the CS-DR model in this subsample; see Table 4 of the Supplementary Materials.

[7] The digital payment percentage measure also captures card use, so it may be open banking has increased electronic card use.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.econlet.2026.112931.

Data availability

The code and the data used in this study are available at Mendeley: https://data.mendeley.com/datasets/mfmjkm2zxk/2.

References

Babina, T., Bahaj, S., Buchak, G., De Marco, F., Foulis, A., Gornall, W., Yu, T., 2025. Customer data access and fintech entry: Early evidence from open banking. J. Financ. Econ. 169, 103950.

Callaway, B., Sant'Anna, P.H.C., 2021. Difference-in-differences with multiple time periods. J. Econometrics 225 (2), 200–230.

Demirguc-Kunt, A., Klapper, L., Singer, D., Ansar, S., 2018. The Global Findex Database 2017: Measuring Financial Inclusion and the Fintech Revolution. World Bank Publications, Washington, DC.

Fang, J., Zhu, J., 2023. The impact of open banking on traditional lending in the BRICS. Financ. Res. Lett. 58, 104300.

He, Z., Huang, J., Zhou, J., 2023. Open banking: Credit market competition when borrowers own the data. J. Financ. Econ. 147 (2), 449–474.

Lee, S.J., Wooldridge, J.M., 2025. Simple approaches to inference with difference-in-differences estimators with small cross-sectional sample sizes. Available at SSRN No. 5325686.

OECD, 2023. Data portability in open banking: Privacy and other cross-cutting issues. OECDDigital Economy Papers 348, OECD Publishing, Paris.

Plaitakis, A., Staschen, S., 2020. Open banking: How to design for financial inclusion. Working Paper. Consultative Group to Assist the Poor (CGAP), World Bank.

Wooldridge, J.M., 2023. Simple approaches to nonlinear difference-in-differences with panel data. Econom. J. 26 (3), C31–C66.

Wooldridge, J.M., 2025. Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators. Empir. Econ. 1–43.