

Mobile Money and the Economy: A Review of the Evidence

JANINE ARON¹

Mobile money is a recent innovation giving financial transaction services via a mobile phone, including to the unbanked global poor. The technology has spread rapidly in the developing world, “leapfrogging” the provision of formal banking services by solving the problems of weak institutional infrastructure and the cost structure of conventional banking. This article examines the evolution of mobile money and its important role in widening financial inclusion. It explores the channels of economic influence of mobile money from a micro perspective, and critically reviews the empirical literature on the economic impact of mobile money. The evidence suggests convincingly that mobile money fosters risk sharing, but direct evidence of the promotion of welfare and saving is mostly rather less robust.

¹ *Affiliations:* Institute for New Economic Thinking at the Oxford Martin School, University of Oxford, UK, Centre for the Study of African Economies, Department of Economics, Manor Road Building, Oxford OX1 3UQ.

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“Leapfrog”: to improve a position by going past others quickly or by missing some stages of an activity or process.” [Cambridge Business English Dictionary, CUP]

1. Introduction

Mobile money is *novel*: it was barely heard of a decade ago.² Yet it has transformed the landscape of financial inclusion, spreading rapidly in developing and emerging market countries (see Figure), and “leapfrogging” the provision of formal banking services. The poor are especially vulnerable to risk (e.g. from illness, unemployment, death of family members, or natural disasters). Enhancing financial inclusion of the unbanked urban and rural poor, a goal of the G20 group of countries, can help to diversify risk. Financial inclusion policy has focussed on extending access to *formal* banking services, but progress has been thwarted by cost and market failure challenges.

The new technology helps overcome problems from weak institutional infrastructure and the cost structure of conventional banking. Small size, volatility, informality and poor governance place constraints on the commercial viability of financial institutions in developing countries (Beck and Cull, 2013), see Figure 2. The poor mostly cannot afford the minimum balance requirements and regular charges of typical bank accounts. Mobile phone technology has the advantage that consumers themselves invest in a mobile phone handset, while the (scalable) infrastructure is already in place for the widespread distribution of airtime through secure network channels, see Figure 3. By adopting mobile money, under-served citizens gain a secure means of transfer and payment at a lower cost, and safe and private storage of funds. Mobile money has filled a lacuna, and has “changed the economics of small accounts” (Veniard, 2010).³

The technological innovation helps ameliorate the perennial asymmetric information constraint faced by conventional banks in lending to the collateral-less poor.⁴ The movement of cash into electronic accounts gives a record, for the first time for the unbanked, of the history of their financial transactions in real time. By using algorithms⁵, these records can provide evolving individual credit scores for the unbanked. After a designated period of usage and once a score is available, registered users of mobile money may obtain a pathway to *formal* banking services accessed only through a mobile phone: to interest-bearing savings accounts that can protect assets; to credit extension to invest in livelihoods; and insurance products that reduce risk.

Apart from reducing asymmetric information, the impact is far-reaching of enhancing *transparency* through electronic records. Tax collection could be improved by the rise of more visible spending, quite apart from the greater ease of tax collection via mobile money payments. The increased transparency of records protects customers’ rights and fosters trust in business, promoting the growth of efficient payments networks. Mobile money should make international transactions more readily traceable and therefore facilitate identification and better control of money laundering. If

² The phenomenal growth since 2007 of Kenya’s M-Pesa system has brought mobile money to international prominence (“M” is for mobile, and “pesa” is Swahili for money), see [Box 1](#).

³ Prior to mobile money in Kenya, there were fewer than three bank branches per 100,000 people. Saving was mostly as cash under the mattress. Domestic transfers used scarce post office branches, or insecure intermediaries such as bus-drivers. International remittances were received expensively via money transfer companies or Hawala.

⁴ Rotating savings and credit associations and cooperatives address the problem of asymmetric information, allowing small accumulated sums by groups to help individual members spread risk. The related micro-credit movement offers collateral-free loans to marginalised borrowers at near-market interest rates. However, assessing such micro-finance in a long-running evaluation in India, Banerjee et al. (2015) conclude it has had limited success.

⁵ The FICO scores in the US, decisive in 90 percent of US lending decisions by 2015, are created in a similar manner (Financial Times, 5th February, 2015, p.9).

the high cost of remittances were reduced by mobile money, this could attract more official remittances, and re-channel “informal” remittances through official channels, raising recorded remittances.⁶ In essence, mature mobile money systems and the records they produce help foster the “formalisation” of the economy, integrating informal sector users into business networks, formal banking and insurance, and linking to government through social security, tax and secure wages payments. However, there are legal data privacy considerations concerning access to and use of mobile money records which have not yet begun to be addressed.

The channels through which mobile money can affect the economy are many and complex, and not necessarily well-understood. A burgeoning body of empirical literature has attempted to quantify the possible economic gains for different countries of access to secure financial services through mobile money (e.g. improved risk-sharing, food security, consumption, business profitability, saving and use of cash transfers), and the factors driving the adoption of mobile money. Demonstrating welfare and risk-sharing gains from mobile money across countries could bolster the case for significant government and donor support, and investment.

Unfortunately, interpreting the evidence on the economic impact of mobile money is not straightforward. The empirical literature is burdened by a range of sometimes serious problems with data, methodology, and identification, which some authors underestimate or choose to ignore. Work on mobile money faces “selection” problems, since both the “roll-out” of mobile money by Mobile Network Operators (MNO) and their agents, and adoption or usage of mobile money by individuals, may be influenced by other factors such as education, wealth, and changes in technology preference. There is mixed success using various methods and data sets in dealing with the resultant ambiguous causality. Although various studies establish statistically significant relationships, they frequently do not test the robustness of their results to different model specifications, measurement errors, and bias due to the possible omission of variables. Furthermore, in practice it is difficult to generalise from these models.

This article introduces the phenomenon of mobile money and its role in financial inclusion. It examines possible channels for the economic influence of mobile money, and reviews the new empirical literature on mobile money, both to obtain a better understanding of the linkages involved and to assess critically the sometimes strong claims made by the authors.

2. Some background on mobile money and its role in financial inclusion

In economies like the US with deep financial markets, mobile payments or transfers are predominantly linked with *pre-existing* bank accounts; mobile payments are rapidly gaining market share after a slow start, catalysed by new technology and commercial partnerships (e.g. Apple Pay). This is distinct from *mobile money* payments or transfers in largely cash-based developing or emerging countries, where most users are unbanked. Yet, as mobile money systems evolve, and smart-phones become ever cheaper in less advanced countries, the range of financial services could expand to link with products managed by formal financial institutions, such as banks and insurance companies. This will ultimately *blur* the distinctions between mobile banking and mobile money. Survey evidence suggests that security concerns about mobile payments have diminished in the US, shaped by industry efforts to enhance security (e.g. Federal Reserve, 2016). There may be a technological spill-over to less advanced economies, and biometrics may allay security concerns

⁶ The official statistics could improve and also the economic management of remittances. In highly dollarised economies (see Corrales et al. (2016) for the extent of this phenomenon in Africa), mobile money through lower transactions costs may reduce currency substitution, thereby deepening their financial systems.

(though there are caveats about their use in poor countries). This could catalyse a transformation to a virtually cashless⁷ economy, and possibly a new role for some banks beyond traditional payments.

The term “financial inclusion” is of recent vintage, and has gained currency with policymakers, most prominently in the Maya Declaration of 2011, when 80 regulatory institutions from 76 countries collectively endorsed a set of financial inclusion principles. The G20 has backed the Maya declaration, promoted indicators to measure “financial inclusion”, and the G20 Summit in 2017 prominently endorsed digital approaches to financial inclusion. Mainstream definitions of financial inclusion share the goal of participation in the *formal* financial sector, which has severely constrained progress to inclusion.⁸ Until recently, use of *electronic* mobile money has *not* been counted as part of financial inclusion under most definitions. Mobile money’s role is seen as a *pathway* for registered users to formal sector financial inclusion via products (insurance, credit and a bank savings account) accessed through a mobile phone.

Aron (2017) argues that a revised definition of financial inclusion should encompass *tiers* of semi-formal inclusion, and not focus on comprehensive formal banking sector inclusion. Mobile money has transformed the lives of poor consumers who can hold recorded cash privately in non-bank electronic accounts and perform financial transfers, easily and cost effectively. Fast-spreading and cheaper smartphones (and recycled smartphone handsets) potentially offer access to sophisticated features and a spectrum of financial services for huge numbers of illiterate people through well-designed applications (Villasenor, 2013). Such users may not embrace the formal sector products even if they become available, e.g. if they qualify for credit, the loans may be small and not adequate to purpose, creating a disincentive to participate. Moreover, the actual number of informal users may be far higher than is formally reported. South Asia has close to 90 percent of the global *unregistered* mobile money customers, using an over-the-counter (OTC)⁹ model where the challenges and costs of establishing identity in registering were circumvented in favour of a drive for early market share (Scharwatt et al., 2015). In practice, the proliferation of mobile money services and the sheer numbers of new users actively signed up has become integral to achieving ambitious targets under the 2011 Maya Declaration. A revised set of G20 indicators in 2016 has raised the prominence of mobile money, reducing the bias to formality.

In Box 1, the Kenyan mobile money system, M-Pesa, is summarised and serves to explain the “nuts and bolts” of a profitable mobile money system. Instead of bank branches, mobile money systems rely on a large network of agents. These are linked under various contractual arrangements with a parent MNO, usually in partnership with a prudentially-regulated bank.¹⁰ The nature of agent network structures and the design of the individual agent contracts are crucial for the successful development of mobile money systems (Aron, 2017, Section 9). The typical authorized agents¹¹ of the mobile money services provider are shops or outlets staffed by small business owners. Mobile money systems were initially dominated by domestic money transfers, but have expanded into a broader payments platform for utility bills, rent, taxes, school fees and retail payments. Business usage is expanding rapidly through special networks for the payment of suppliers, wages payments and potentially pensions. Government usage for the payment of wages and social security has lagged, though the cost savings or reductions, especially in insecure environments, could be significant.

⁷ On the merits of a cashless economy, including fighting corruption and money-laundering, see Rogoff (2016).

⁸ This bias to formality is challenged by Porteous (2013).

⁹ Registration aids financial inclusion toward formal sector products. By contrast, an OTC transaction is conducted through an agent’s account on behalf of the customer.

¹⁰ Regulation of mobile money is discussed in detail in Aron (2017), especially prudential regulation by the central banks; see also Di Castri (2013).

¹¹ Third party merchants are not “agents” in a strict legal sense of having the legal authority to act for the service provider – this depends on the local regulation requirements.

A fast-growing product is international remittance through mobile money channels. The size of officially recorded remittance flows to developing countries and the high transactions costs, suggests the potential gains from transparent and cheaper methods of remittance are significant.¹² Security concerns present a challenge because of poor compliance to international law at the *receiving* end. If the local compliance challenge can be overcome, mobile money (bound by “know your client” legislation and electronic recording of transactions) should facilitate remittances to war-torn countries with weak governance like Somalia, with limited or no functional banking.

3. The economics of mobile money: the micro-view

The novelty of mobile money and its recent introduction in many countries means few studies have examined the economics of mobile money.¹³ The mobile money storage and payments system, and its further linkages to bank savings accounts, micro-insurance, and credit via algorithmic credit scores, could affect households and businesses through several different channels. Mobile money potentially helps ameliorate several areas of market failure in developing economies.¹⁴

3.1 Reducing transactions costs

Mobile money reduces the transactions costs of sending and receiving money, especially with inadequate and expensive transport infrastructure. Jack and Suri (2014) observe that in Kenya, where families and social networks are widely-dispersed from internal migration, remittances on average travel 200km.¹⁵

Transactions costs include the *transport costs* of travel e.g., to a bank, utility company or government office; the *travel time* and the *waiting time* in long queues; the *coordination costs* between individuals, between firms and suppliers or customers, and between government and individuals, which can be extensive in time and money lost; and the costs of *delays* and “*leakages*” through corruption or middlemen, acting like a *tax* (or complete loss through *theft* from insecure methods of money transfer). There is an *opportunity cost* to lost money and time. The money could have been invested, spent or saved; the time could have been spent in productive activities. The automated delivery of cash transfers, wages, social security funds and private remittances by electronic transfer increases the *certainty* of the *timing* of cash receipts which helps *planning*. This further reduces coordination costs, the costs of delays and hence the opportunity costs.

3.2 Reducing asymmetric information and improved transparency

Recording financial transactions creates greater financial transparency, and it reduces asymmetric information. Asymmetric information and the fixed costs of servicing an account lie at the heart of the failure of the formal banking sector to advance credit to poor customers who lack collateral and financial histories. Moving cash from under the mattress into an electronic account turns it into *recorded* cash. Every deposit, withdrawal, transfer or payment transaction through mobile money

¹² Remittances to developing countries are projected to reach US\$444 billion in 2017. The *true* size of remittances, including unrecorded flows, is likely to be significantly larger (World Bank Migration and Development Brief no. 27).

¹³ The following authors have examined aspects of the economics of mobile money: Mas and Klein (2012), Jack et al. (2010), Jack and Suri (2011) and Weil et al. (2012).

¹⁴ See Karlan et al. (2016) on market failure in a more general context of financial services.

¹⁵ Mobile money halves the cost of sending compared to Western Union, and is about a third lower than the postal bank or bus delivery cost, excluding transportation or time costs (see also Morawczynski (2009)).

creates a recorded financial history. Linking algorithmic credit scores and the granting of small loans was discussed above (see [Box 1](#)).

An electronic record of payments potentially protects consumers against theft, fraud and misinformation. Such protection can reduce transactions costs for consumers and increase the use of business through trust (Radcliffe and Voorhies (2012) note how the “anonymity of cash” may inhibit trust between traders and new vendors). Greater transparency through records can help regulate the service, including the dissemination and posting of information on transactions costs, and to promote competition. Recorded transfers with appropriate ID documentation (“know your customer”) also facilitates cheaper international remittance transfers.

3.3 Changing the nature of saving and increasing saving through digital means

There are several motives for saving. Life-cycle motives compensate for differences in timing between income and expenditure streams, and these include saving for education, leisure, marriage, consumer durables, housing purchases, retirement and funeral expenses. Precautionary motives (buffer stock saving) reflect the uncertainties of future income and expenditures, and include saving for unemployment, illness, accidents, natural disasters and risks associated with old age. Finally, there is saving for a bequest motive, to give gifts in one’s lifetime or to leave a legacy to heirs. Saving thus helps to allocate consumption over time, and to reduce risk.

For the unbanked poor, their “immersion in physical cash creates considerable frictions in their financial lives” (Radcliffe and Voorhies, 2012). Cash-based households have informal savings options, which carry risks of theft or “liquidation”: cash under the mattress; accumulation of assets such as jewellery or livestock; and storing savings with informal savings groups. Loss of savings in this manner is common. Mobile money electronic accounts offer safe storage of cash, though without the payment of interest.

Another advantage is privacy. Compared with cash receipts, the reduced observability of the timing and sizes of mobile transfers and the accumulated electronic balances, could protect savings for the recipient (Aker et al., 2016). Related to this is an economic psychology literature on how the poor could be encouraged to accumulate savings, e.g. use of “commitment” savings accounts (Dupas and Robinson, 2013).

3.4 Risk and insurance

Living standards of the poor are at risk of multiple *communal shocks* including flooding, droughts, pestilence, other natural disasters, sometimes conflict, and medical epidemics; and *idiosyncratic shocks* including theft, damage to the homestead, illness and death in the family. There are very limited opportunities for insuring against these risks. Formal insurance is typically absent, but family, clan and network ties can create informal insurance networks, ameliorating such risks by periodic transfers and monitored by trust relationships amongst members of the network (De Weerd and Dercon 2006). Jack and Suri (2011) suggest several ways by which mobile money can facilitate risk-spreading. The geographic reach of networks can enlarge. Timely transfers of money can arrest serious declines that may be impossible or hard to reverse. The mobile money technology allows small and more frequent transfers of money that allow a more flexible management of negative shocks. Thus, informal insurance networks may function more effectively. In turn, more efficient investment decisions can be made, improving the risk and return trade-off. Where mobile money develops sufficiently to allow access to micro-insurance (Box 1), there is potentially an additional buffer against negative shocks.

3.5 Incomplete property rights, changing family dynamics and changing social networks

Women or minority groups may face limitations in their opportunities and their access to property, an aspect of inequality, and often resulting in more widespread economic inefficiencies. Mobile money could change bargaining power within the family. Greater privacy may influence both inter-household allocations (Jakiela and Ozier, 2016) and intra-household allocations (Duflo and Udry, 2004). If the nature of expenditure by gender differs (Chattopadhyay and Duflo, 2004), there could be welfare changes in the household (Aker et al., 2016).

There is in general little research on network formation or dissolution, and on migration and remittance decisions using network data (Chuang and Schechter, 2015). Mobile money could change the nature of social networks. The cohesion of a network could be strengthened or weakened. The size of networks could be expanded with the greater geographical reach of the transfer mechanism. Morawczynski and Pickens (2009) note the greater autonomy of rural Kenyan women as they could more easily solicit funds from their husbands and other contacts in the city. The reduced transactions costs of remittances might create a more liberal attitude to migration from the homestead (Jack and Suri, 2011), though distant migrants are also less observable and accountable. Johnson (2014) stresses the continued importance of rotating credit schemes for perpetuating trust and coordination in communities. There is evidence of substitution away from these schemes due to mobile money (Mbiti and Weil, 2016), but equally, evidence that the mobile money transfer and storage mechanism is used by them (Wilson, 2010).

3.6 Improving other aspects of economic efficiency

The combination of better communication and coordination with mobile phones and instantaneous mobile payments could improve business planning and efficiency. Mobile payments facilitate trade. Access to credit, informally and through banking services linked to mobile money, can improve investment decisions. Improved risk sharing and cheaper, secure, long-range remittances can expand the scope of labour decisions to encompass higher-risk but higher-return occupations, or migration to higher-return labour markets (Jack and Suri, 2016). There could be better allocation of savings and labour within the household and in businesses, and more efficient investment decisions affecting agriculture and business, and education and skills. Returns to investment could rise, with a feed-back to greater savings.

4. Empirical research

“Perhaps the ‘holy grail’ of demand side data is the impact question. How can we understand whether branchless banking services are making a positive difference in client’s lives?” McKay and Kendall (2013)

The rapid global growth of payments, transfers and international remittances, speaks of mobile money providers satisfying a demand for financial services not previously adequately met. This *revealed preference* suggests a net welfare improvement. Moreover, positive externalities imply a *larger* total than private benefit, as greater connectedness in the system occurs with each adoption. *But are empirical studies able to measure the economic benefits and local if not system-wide externalities?*

Given its novelty, few academic studies have examined the economics of mobile money. The bulk of empirical work employs survey data at the household or firm level. To reach robust conclusions on the economic benefits, the bar is set very high for empirical analysis. First, it is important to *analyse the appropriate data*, but often this is hard to achieve. Second, there are

considerable *methodological challenges* in the empirical work, so that results need to be carefully assessed, and *not* taken at face value. An analytical typology table summarises the empirical studies ([Table 1](#)). A more in-depth analysis of the studies is presented in Aron (2017).

4.1 Challenges for data

Definitional ambiguities could cause mis-counting when measuring mobile money “usage”. If the precision of the variable is compromised, measurement bias is introduced into regressions (see [Table 1](#), column 1). Using the *number of mobile money accounts* or the *number of registered customers* may induce multiple counting of the same individual if several accounts are held with different providers. If registered customers are inactive (and globally two-thirds of registered accounts are inactive with a generous 90 day definition), this will exaggerate the true participation, Figure 4. Where *unregistered* customers intensively use the service, as in over-the-counter (OTC) services, overall usage will be under-estimated.

Some data are unobservable. Empirical regressions will be mis-specified when omitting hard-to-measure variables linked to mobile money, such as spill-over learning effects in the community, and technological and quality changes. Important “observables”, such as education (where quality is not assessed) and wealth, are typically poorly measured in household surveys which may exacerbate the biases.

Institutional and political regime changes affect the uptake of mobile money. For example, adoption is enhanced with more liberal registration requirements below a low threshold of use. In Côte d’Ivoire, the cessation of conflict and onset of greater growth and stability from 2012 was a key to driving mobile money adoption (Pénicaud and Katakam, 2014). There are likely to be shifts over time in the relevance of particular determinants e.g. cheaper, more capable smartphones widen access and ownership. Shifts can be proxied by carefully dated dummy variables; interaction of these dummies with explanatory variables introduces non-linearities and tests whether the effects of the variables alter with regime changes.

Data may be proprietary, and it may be difficult to design surveys optimally in advance. Against these difficulties, if privacy concerns can be overcome, new access to a rich seam of “big” data, the administrative mobile money transactions from business and individuals, represents an enormous research opportunity. Mobile money payments data could be used to help forecast hard-to-gauge household assets and expenditure that otherwise rely on self-reported data (as for mobile phone data, Blumenstock et al., 2015a), to derive proxies for migration patterns from geotagged data (Blumenstock, 2012) or to link GPS data with administrative data to examine profitable incentive-compatible price discrimination schemes (Economides and Jeziorski, 2016), and explore social networks, relevant for work on remittances (Aron, 2017; Aker and Blumenstock, 2015).

4.2 Challenges for empirical methods

The quantitative empirical work on mobile money falls into two categories: studies which assess the determinants of the *adoption* of mobile money (i.e. where a proxy for usage of mobile money is the dependent variable) and studies of the *effects of mobile money on micro-economic outcomes* (i.e. where usage of mobile money is *not* the dependent variable). Examples of the latter include whether mobile money promotes improved risk-sharing, food security, consumption, business profitability, saving and effective use of cash transfers.

Research on mobile money faces two “selection” problems, raising the problem of endogeneity¹⁶ in empirical analysis. The “roll-out” of mobile money by MNOs and their agents may not be random if they select into areas on the basis of household and village characteristics. For instance, there will be an upward bias on the effect of mobile money on consumption if the wealth of a village determines agent selection into that village (and that wealth is not controlled for in regressions). It is difficult to disprove self-selection by the agents toward more profitable locations. Several authors contend there is little statistical correlation between agent “roll-out” and household observable characteristics that might have been associated with future outcomes; but they use *partial* correlates only, which is not decisive. In Jack and Suri (2014), such bivariate correlations between agent density at 1, 2 or 5km and a range of observables¹⁷ also include location-by-time and rural-by-time fixed effects. But this is rather different from trying to explain agent density with a full range of the variables and all relevant interaction effects, to prove it is exogenous or “unpredictable”. Moreover, it does not rule out correlation between agent roll-out and unobservables or poorly-measured observables (such as wealth) that also affect outcomes.

One factor suggesting roll-out may have been non-random is that Jack and Suri (2014) themselves suggest: “...many of the agents had business relationships with Safaricom prior to the advent of M-PESA, and about 75 percent report sales of cell phones or Safaricom products as their main business.” As Aker and Blumenstock (2015) imply for the prior telecom infrastructure: “...decisions regarding expansion of ICT infrastructure and ICT-based programs are typically driven by private sector or policy criteria.” Thus, even if the bias is likely to be low for Kenya, there may be greater selectivity biases in countries such as Niger, Tanzania and Uganda with relatively less developed technological infrastructure.

A second selection problem is undisputed: adoption of mobile money by individuals is influenced by factors both observable (e.g. education, wealth, urban dwelling and the use of banking services) and unobservable (e.g. susceptibility to risk, community learning spill-over effects and changes in technology preference) that may be correlated with mobile money use

Given the selection problems, the dominant empirical methodologies are Randomised Controlled Trials (RCT), quasi-experiments with a Difference-in-Differences estimation strategy or the non-parametric method of Propensity Score Matching, and Instrumental Variables (Box 2). The choice amongst methods is not uncontroversial. The methods have differing degrees of success in dealing with heterogeneity¹⁸ at the individual or household level. A consideration is whether results can be “scaled-up” or “transported” to allow generalisation to other contexts. Since institutional structures, regulation and demand patterns differ across countries, generalisations of evidence need to be made cautiously (e.g. generalisability may depend on the extent and quality of the agent network). Econometric modelling difficulties imply that the conclusions drawn are often suggestive only.

Many studies fail to “disentangle” the adoption of the technology (the phone) from adoption of the service (mobile money) it provides (Aker et al., 2016). How and whether the different studies address this, to reduce bias, is explicitly clarified in [Table 1](#) (column 4), and also whether clustered standard errors are reported (Bertrand et al. (2004)).

¹⁶ An endogeneity problem in econometrics occurs when an explanatory variable is correlated with the error term, as a result of simultaneous causality, omitted variables and/or measurement error. There are several statistical methods that aim to correct the resulting bias in the regression estimates (Box 2).

¹⁷ The log of wealth is one of the observables and there is weak evidence for a *correlation* with wealth.

¹⁸ Heterogeneity refers to variation across individual units of observation, some of which can be observed (e.g. age and education), and some of which is difficult to measure (e.g. changing technological preferences). Thus, omitted heterogeneity is an omitted variable, and hence a kind of endogeneity (see [Box 2](#)).

4.3 Adoption¹⁹

To explore the factors that determine the adoption of mobile money (i.e. where a proxy for usage is the dependent variable), Probit or Tobit regressions or OLS regressions are commonly used. The principal empirical problem is the identification of causal relationships. This encompasses biases introduced by poorly measured determinants, omitted observable variables and omitted unobservables. Examples of hard-to-measure unobservables are: spill-over effects; technological and quality changes of the handset and services; the quality of agents²⁰ and trust in the system; and the effects of advertising campaigns and incentives to register.²¹ Non-linearities are crucial in adoption empirics (e.g., adoption can be catalysed by the cessation of conflict), but are typically ignored. Network effects also matter since a critical mass of users and a critical mass of reliable agents fosters sustainable adoption.

Given these challenges, it is unsurprising that studies of adoption in different countries have been conducted by *non-economists* focused largely on qualitative aspects, or have examined mobile money adoption *correlations* with firm and household surveys (Aker and Mbiti, 2010). These find that adopters of mobile money are more likely to be younger, wealthier, better educated, have a bank account, own a mobile phone and reside in urban areas. One convincing econometric study has supported these links (Munyegera and Matsumoto, 2016a) and deserves attention. This panel study removes *time-invariant* household heterogeneity with household fixed effects and some *time-variant* household heterogeneity with location-by-time dummies in a panel context in rural Uganda.²² It includes many individual controls (e.g. control for ownership of a mobile phone, distance to the nearest mobile money agent²³ and a migrant worker in the family) further helping to reduce endogeneity. They find no gender effect or age effect for rural adopters, but distance to the nearest mobile money agent proved important, as did education and wealth; and both the dummies for the ownership of the phone and the migrant worker are significant (all with 1 percent significance). It is still possible that there is some *time-variant* household heterogeneity that is not controlled for, as location-by-time dummies only address an average over households in a location.²⁴

4.4 Private mobile money transfers and risk sharing

Amongst the most convincing analyses of the impact of mobile money are the *panel data studies* using a Difference-in-Differences approach that explore how mobile money has fostered improved *risk-sharing* amongst informal networks after large shocks. The proposed mechanism operates via lower transaction costs (compared to alternatives) for money transfer, influencing the size, frequency and (sender) diversity of domestic remittances. The intervention is a negative shock, and such shocks

¹⁹ Not on adoption *per se*, Economides and Jeziorski (2016) match administrative transactions data with GPS data in Tanzania, quantifying motivations for usage, such as willingness to pay to avoid walking with cash or storing money at home to alleviate criminal risk.

²⁰ See Balasubramanian, K. and D.F. Drake (2015).

²¹ Work in progress by Blumenstock and co-authors explores the negative effects of violence on the adoption of mobile money in Afghanistan, <http://www.jblumenstock.com/>.

²² They take two approaches, and find similar results, using first a Probit regression, and then a linear probability model with fixed effects. The mobile money “usage” measure in the dependent variable does not match the preferred definition of *active* (90-day) users, however, and this could bias the results.

²³ Note that agent density may not be exogenous, see [Section 4.2](#).

²⁴ The results of a related study on adoption by Weil et al. (2012) should be regarded as suggestive, and of supporting correlations, see Aron (2017) and [Table 1](#). The study cannot control for individual fixed effects and suffers from an omission of controls.

are probably random²⁵. The focus is not on the *direct effect* of mobile money usage on outcome variables like consumption, but rather on the interaction of mobile money usage with the shock (while controlling for household characteristics to interact with the shock). This puts less emphasis on the endogeneity of the mobile money usage dummy. The best of these studies fully exploit the panel data to remove sources of unobserved *time-invariant* household heterogeneity using household fixed effects ([Box 2](#)), include location-by-time dummies and rural-by-time dummies to help control for time-varying heterogeneity according to location or the rural-urban divide, and (mostly) include appropriate controls.

All the reviewed risk-sharing studies disentangle the impact of the mobile phone technology from the transfer mechanism, either by considering only participants with a mobile phone number (though this introduces a new selection criterion); or by introducing a dummy for ownership of a mobile phone into the regressions.

A sophisticated study by Blumenstock et al. (2016) uses a Difference-in-Differences approach to analyse the transfer of airtime: the authors call it a “rudimentary form of mobile money” but it is not convertible for cash. They exploit the random timing and location of earthquakes in Rwanda in a natural experiment to identify covariate economic shocks²⁶. Their study relies solely on administrative telecoms data and lacks survey measures of welfare or wealth.²⁷ The link between risk-sharing and money transfer is instead implied, given the consistency between observed patterns of transfers and the characteristics of their theoretical models of reciprocal risk sharing. All regressions include a shock dummy and time fixed effects. Location fixed effects in regional-level regressions are replaced by recipient fixed effects in individual-level regressions, and by a fixed effect controlling for the average intensity and direction of transfer flows between two users in dyadic regressions. In extended regressions they allow for heterogeneity between individuals and different types of sender-recipient pairs, and cross the characteristics with shock dummies ([Table 1](#)).

They find, perhaps surprisingly, that as well as geographical proximity, transfers to victims near the epicentre after the Lake Kivu earthquake of 2008 are determined by a past history of reciprocity between individuals, and the transfers decrease in the wealth of the sender and increase in the wealth of the recipient. The opposite would be obtained in the case of charity or altruism. There are possible selection issues. Selection is induced because wealth itself determines the ownership of phones in Rwanda in 2008 (Blumenstock and Eagle, 2012). Further, the wealth of the recipient is likely to be correlated with the size of his or her geographical network. Ideally, the differences in such networks should be controlled for, as airtime does not in this sense have the same utility in times of disaster for the wealthy and the poor.

A path-breaking study by Jack and Suri (2014) exploring risk sharing and mobile money, finds total consumption²⁸ of Kenyan mobile money users is unaffected by a range of negative (self-reported) income shocks while that of non-users drops by 7 percent (with 10 percent significance). The effect is more evident for the bottom three quintiles of the income distribution. A similar result is found when isolating the impact of health shocks on total consumption.²⁹ A Difference-in-Differences

²⁵ This is a reasonable assumption if *unexpected* shocks are reported, and not systematically correlated with most household characteristics. Though unlikely in a short time frame, if shocks are correlated with changes in unobservable household characteristics then they would not be random.

²⁶ Idiosyncratic shocks affect individuals or households; covariant shocks affect groups of households, communities, regions or even entire countries.

²⁷ The average amount transferred over the two month period is small at around US \$1; the total additional influx (explicit transfers to all 15 cellular towers within 20km of the epicentre) measured about US \$84.

²⁸ Food consumption, however, appears to be equally well-smoothed by both users and non-users in the sample.

²⁹ User households can finance health care expenditures from remittances without compromising other consumption, but non-users must reduce non-medical spending for this; see also Suri et al. (2012).

approach is applied to a panel specification controlling for household fixed effects, location-by-time dummies and rural-by-time dummies. There is a dummy for a negative shock to income in the last six months, and a dummy for an M-Pesa user in the household, and the two dummies are crossed to test whether M-Pesa users are better able to smooth risk. An included vector of controls (though not including wealth, see [Table 1](#)) is crossed with the shock dummy to help control for correlations of M-Pesa with observables that might help smooth risk.

For Tanzanian mobile money users, a very similar set-up by Riley (2016) takes matters a stage further, examining the potential beneficial *spill-over effects* (local externalities) of mobile money to the village community³⁰ (which includes non-users) following an aggregate shock (either a self-reported shock such as droughts or floods, or a measure of rainfall deviations from a long-term mean, see [Table 1](#)). The regressions include a dummy for mobile money use by an individual in a village, and one for the proportion of mobile money users in a village, so that there are three interaction effects with the shock dummy, including its interaction with the vector of controls. Unlike in Jack and Suri (2014), wealth, expected to be time-varying, *is* here included as a control.

She finds that there are spill-over effects in the absence of a shock, as mobile money users share remittances with the village resulting in per capita consumption of everyone in the village increasing. After an aggregate shock, however, households using mobile money benefit from an 8-14 percent increase in consumption (with 5 percent significance) compared with non-users, cancelling the effect of the negative shock on users; but there are *no* spill-over effects to the community of non-users. The benefits to users and to communities (in the absence of a shock) are found to be highest in rural areas and to decrease sharply with distance to the nearest mobile money agent. The included district-by-time dummies are important in helping to control for heterogeneity from the self-selection into districts by mobile money services providers, for localised spill-over effects, and unobservable differential effects of rainfall (e.g., for different occupations by district).

All three studies conduct placebo tests supporting the common trends assumption of the DD specification. In Riley (2016), Propensity Scoring was used to try to match users and non-users with similar characteristics, confirming results. Attempts by both Riley (2016) and Jack and Suri (2014) to apply the IV technique (see [Box 2](#)) and *instrument* the usage dummy and its interaction with the shock are less successful, typically with weak instruments based on agent rollout data such as agent density (see [Section 4.2](#) and [Box 2](#)). The IV regressions do not contradict the conclusions, but in Riley (2016), although a Sargan-Hansen test determines the instruments are valid (exogenous), they are found by Cragg-Donald Wald F statistic tests to be statistically weak which may potentially introduce a large bias. The former test is missing in Jack and Suri (2014).

Using data from a survey of nearly 7700 M-Pesa agents, Jack and Suri (2014) also compare consumption responses in reduced form panel regressions with fixed effects, substituting “access to an agent” for M-Pesa usage, and claim the results reinforce their conclusions. However, the crucial assumption of exogeneity of the agent density proxy rests on bivariate correlations and these partial correlations were critically discussed in [Section 4.2](#).

It remains possible that *time-variant* household heterogeneity (e.g., changing risk preference or changing technology preference) may still confound the results. One specific example of time-variation in characteristics would be where in the first wave of the panel, a fifteen year old is not in work, but by the second wave, three years later, she is working, which affects her ability to purchase a mobile phone and use of mobile money. It would be important to control properly for age structure in this case. More difficult to deal with is systematic unobserved heterogeneity from interaction effects.

³⁰ A broadening of networks is likely (Chuang and Schechter, 2015), but Riley (2016) more restrictively assumes the sharing social network is village-wide, rather than across villages by lineage, for instance, and is constant over time.

If there are missing interaction effects from time-varying unobservables or time-varying excluded observables (e.g. wealth) that could help households to smooth risk, then the effect of M-Pesa in smoothing consumption could be exaggerated. For instance, there could be an upward bias if a household that is wealthier in the second period is better placed to withstand a negative income shock; or if households wealthier in the second period than the first tend to experience smaller negative income shocks.

4.5 Mobile money transfers and welfare

Far less satisfactory are the (non-RCT) welfare studies reviewed, where results are generally judged unreliable by this survey. Endogeneity problems for the usage dummy are centre-stage, and the use of instrumentation and other methods to mitigate it by removing as many sources of heterogeneity as possible are not always convincing.

Of the six studies, only three disentangle the impact of the mobile phone technology from the transfer mechanism by including a dummy for ownership of a mobile phone into regressions (Munyegera and Matsumoto (2016a), Murendo and Wollni (2016) and Sekabira and Qaim (2016)). One cross-sectional study faces serious problems of controlling for unobserved heterogeneity (Murendo and Wollni, 2016). Two panel studies use inappropriate linear specifications that are likely to introduce heavy biases (Sekabira and Qaim (2016) and Kikulwe et al. (2014)), see discussion in Aron (2017), Section 8.1.4. A fourth study employs propensity scoring with a very small cross-sectional sample, but is subject to unobserved heterogeneity (Kirui et al., 2013). The full critical analyses of these studies can be found in Aron (2017), and details are summarised in [Table 1](#).

The two remaining studies use panel data. Of these two, one fully exploits Ugandan panel data to control for heterogeneity where possible (see [Table 1](#)), and claims an increase of 9.5 percent (with 5 percent significance) in the monthly real per capita household consumption for mobile money users (Munyegera and Matsumoto, 2016a). The Difference-in-Differences specification requires the mobile money intervention to be random, which is questionable. Their IV regression to address this problem shows the above coefficient in the regression for consumption increasing *four-fold*, which casts doubt on the results. Similar to Jack and Suri (2014), the authors rely on *bivariate* correlations only to validate the agent density-based instrument (see [Section 4.2](#)). Using fixed effects regressions the authors find a similar coefficient for food consumption as for total consumption, but greatly higher coefficients for non-food. Given the ambiguous results, propensity score methods are applied to try to match comparable households, and weighted regressions are run for total and food consumption. This recovers a coefficient of around 7 percent (at the 5 percent level) for overall consumption, but the coefficient for food consumption is poorly measured. Too little information is given properly to evaluate the method, however (see [Box 2](#)).

The other panel study, by Suri and Jack (2016), argues strongly for a *causal* role for mobile money on welfare.³¹ The effect of mobile money in Kenya is explored for categories of outcomes, measured in 2014 (see [Table 1](#)). Unlike the other studies in this sub-section, they use the *change in* agent density³² between 2008 and March 2010 to proxy or substitute for mobile money usage (i.e. they are not using agent density as an instrument in an IV regression). By pre-dating the proxy relative to 2014 outcomes, the authors hope to make their proxy exogenous. There are two problems with this. The measure may not be highly correlated with *later* usage (which is like having a weak instrument in an IV regression). Second, the crucial assumption of exogeneity of the agent density

³¹ “...Thus, although mobile phone use *correlates* well with economic development, mobile money *causes* it.” (Suri and Jack (2016), p.1292, final sentence of article, with italics added).

³² Agent density is defined as the number of agents within 1 km of the household. This change variable approximates to the level of agent density in 2010, as agent density would have been low in 2008.

proxy rests on bivariate correlations conducted in Jack and Suri (2014), see discussion in [Section 4.2](#). That said, placebo tests support the common trends assumption of the DD specification.

To estimate the marginal effect of an increase in agent density for females, a gender dummy and the change in agent density are crossed. The change in agent density is also crossed with household (or individual) characteristics to rule out cases where the gender effect was in fact driven by these other characteristics.

They do not use household fixed effects or location-by-time dummies, but control only for location fixed effects - on which a great deal then rests to try to mop up household heterogeneity. There are controls for age and gender, but controls such as dummy for ownership of a mobile phone, household physical and financial wealth, education and possession of a bank account are excluded. Their analysis is at its most convincing in a *differenced* specification for consumption (their Table 1), which at least then effectively excludes household time-invariant fixed effects through differencing (the *level* regressions are likely to have considerable unexplained heterogeneity). Nevertheless, even in the differenced specification, time-varying heterogeneity from unobservables (and omitted wealth) may still introduce bias. With these caveats in mind we present their results for consumption. They find that for households using mobile money, consumption growth for male-headed households was negative, while that of female-headed households was positive and statistically significant. They suggest the latter could be driven by increased labour or capital income, or by transfers between individuals with different propensities to consume. They draw implications for the reduction of poverty (affecting 2 per cent of Kenyan households), and shifts in occupations out of farming, particularly for female-headed households. However, if there is unobserved heterogeneity of the type discussed above, e.g. if wealth which is not controlled for is correlated with mobile money services, then they may be over-estimating the reduction in poverty.

Of the few RCT studies reviewed, see [Table 1](#), some deal with very small transfers and small and specialised samples, and results are not easily generalisable. Two papers exploring impacts of public or employer mobile money cash or wages transfers are Aker et al. (2016) and Blumenstock et al. (2015b). Both identify cost savings from reduced transactions costs for the disbursing party. But there are different results for the recipient: there are cost savings in Aker's study based in Niger, and possible cost increases in the Blumenstock et al. study in the more insecure environment in Afghanistan. Both disentangled mobile money delivery from ownership of a mobile phone, providing new phones to treatment and control groups.

The impressive RCT study on household welfare by Aker et al. (2016) finds improvements in household welfare after drought for the recipients of cash transfers through mobile money accounts in Niger, one of the poorest countries. Intra-household bargaining power for women was promoted³³ and their productivity improved by reduced transport costs, travelling and queuing time. Recipients were more likely to cultivate and market cash crops conventionally grown by women, and had fewer depleted durable and non-durable assets. Household and child diet diversity was 9-16 percent higher among households who received mobile transfers, mostly due to increased consumption of beans and fats (1 percent significance level), and children consumed a third more of a meal per day (5 percent significance level). They emphasise that the mobile money "infrastructure" has to be working well to reap the benefits. Repeating such RCT studies across many locations, cultures, continents and time periods may help reinforce the conclusions and generalisability.

Given the short time period of observation and the small sample size, the Blumenstock et al. (2015b) study, which was able to distinguish changes in the savings behaviour of recipients of wage transfers in Difference-in-Differences estimates of the treatment effect, see below, was *not* able to find improvements in welfare indicators such as consumption and self-reported satisfaction.

³³ Cash-transfer recipients were able temporarily to conceal the arrival of the transfer.

4.6 Analyses of saving behaviour

There are several qualitative studies with localised implications for savings behaviour. For instance, Wilson et al. (2010, Chapter 9) describe how members of informal savings groups in Nairobi find it cost and time effective to move their cash (especially with larger savings) into a group M-Pesa account each week from the deposit collector's own account. Jack and Suri (2011) find that by 2009, 90 percent of early adopters used M-Pesa for saving (amongst other savings instruments and use of cash) for reasons of improved security, greater privacy, increased ease of use, reduced transactions costs and precautionary saving against emergencies.

Three non-RCT studies encompassing a variety of techniques all suggest the *beneficial* influence of mobile money on reported savings by method, and on saving flows (Table 1). Two use cross-sectional survey data (Demombynes and Thegeya (2012) and Munyegera and Matsumoto (2016a)), and one makes a balanced panel of locations not individuals (Mbiti and Weil, 2016). None disentangles the technology from the service it provides by controlling for the ownership of a mobile phone. Attempts to instrument the mobile money dummy are not successful in these studies, but an approach employing the residual of an adoption regression by Munyegera and Matsumoto (2016a) is supportive though in a cross-sectional context. No robust and conclusive results are reached, therefore. There are serious concerns with how the saving flow is measured and from the implications of the use of log specifications (details in Aron (2017), Section 8.1.6).

Probit regressions for methods of saving by Demombynes and Thegeya (2012) with various controls (Table 1), finds reported saving is more likely for older individuals who are male, rural, married, and with higher levels of education, reported income and wealth. With these controls, and instrumenting for M-Pesa usage, M-Pesa users are 20 percent more likely to report having savings (1 percent significance). The instrument (the fraction of respondents in the sub-location registered with M-Pesa) averages over individuals within locations, and eliminates only *some* unobserved district-level heterogeneity. This caveat suggests that the result is indicative. The authors apply IV estimation to the log of average monthly savings (a flow) amounts on similar controls and with the same instrument (see Table 1). The coefficient for M-Pesa usage is not statistically significant. It is unclear whether the endogeneity is severe and the instrument is so successful in dealing with it that mobile usage is not relevant to saving, or whether it is simply a poor instrument for M-Pesa usage.

A related exercise for Uganda using Probit regressions for reported saving yields no significant variables at the 1 percent significance level, save for the mobile money usage dummy (Munyegera and Matsumoto, 2016b). The specification is not comparable to that of Demombynes and Thegeya (2012), which included log income (highly significant), wealth quintiles and marital status for a far larger survey (Table 1). Whether the significance of mobile money usage for Uganda is indeed important or whether the coefficient is biased strongly upwards as it proxies for unobservables is unclear. The log of annual savings (a flow) is modelled in Tobit regressions³⁴, with similar controls. Two approaches are adopted to help address endogeneity (though not the IV approach). A residual from a first stage Probit regression for mobile money adoption is added to the Tobit and is significant at the 1 percent level. The coefficient on the mobile money usage dummy remains fairly stable, and is positive and significant, which is a supportive test. Second, to *reduce* observable (time-invariant) household heterogeneity, propensity score matching is applied (though with scant information on methods used and robustness). They run OLS regressions weighted by the propensity score with various controls (Table 1), but nothing proves significant except the mobile money usage dummy and the value of assets (at the 5 percent level). The authors suggest this is because heterogeneity has been

³⁴ This technique serves to censor observations at zero as the lower limit, since households not using financial services will not yield an outcome.

successfully removed and suggest a role for mobile money in encouraging savings. The conclusions require the proverbial “large pinch of salt”, because despite the authors’ heroic attempts, in cross-section it is very difficult to control for unobserved heterogeneity, and the propensity result is also subject to unobserved heterogeneity concerns ([Box 2](#)).

A potentially interesting finding from the quantitative work of Mbiti and Weil (2016) is that adoption of M-Pesa reduces both the use of informal savings groups and the need to hide cash in secret places. They use a first-differenced IV regression for saving methods with various controls ([Table 1](#)), the differenced specification removing biases due to any time-invariant unobservables. However, it is difficult to draw firm conclusions as the set of instruments used is not intuitive (see Aron (2017), Section 8.1.6, for details); and biases might arise from correlation with unobserved, time-varying characteristics of households.

Two RCT studies were the only saving studies that disentangled the mobile technology from the service it provides ([Table 1](#)). One RCT experimental study (Batista and Vicente, 2016) uses cross-sectional data and narrows the type of population tested in its selected sample; it is subject to the problem of interpreting a treatment effect when intervention depends also on the type of training information provided. Both aspects limit the generalisability of the finding that mobile money increases the willingness to save, though the narrowing of selection helps deal with heterogeneity. A second RCT panel study controlling for individual and survey wave fixed effects, based in Afghanistan (Blumenstock et al., 2015b), was applied to a small and specialised sample. Increased usage of mobile savings differed by the prior banking status and size of salary of recipients, and liquidity preference and savings withdrawal increased with perceptions of physical insecurity. However, recipients had to incur the costs of finding liquid agents (where adequate mobile network and agent coverage actually existed), and some had privacy concerns for security reasons. Again, the results are suggestive but not generalisable.

4.7 Regulation

One cross-country study tries to relate “enabling” regulation to the usage of mobile money for 35 countries. Gutierrez and Singh (2013) use self-constructed (*de jure*)³⁵ regulatory indices in a logit regression controlling for both country characteristics and individual (micro-) characteristics.³⁶ By using location (country) fixed effects to *reduce* omitted variable bias they are unable to include the indices³⁷ themselves, but only their interaction with individual characteristics. The interaction effects nevertheless yield some plausible insights. A regulatory framework that supports interoperability appears to promote higher usage among the poorest. Stronger consumer protection appears to reduce usage by the poorest, perhaps through raised costs, while amongst the educated, greater consumer protection promotes usage. But heterogeneity remains present in cross-section, and the *direct* effect of regulation could only be tested if a *panel* of Global Findex usage data should become available.

5. Conclusion

The main contribution of this survey has been to explore the channels of economic impact and to survey critically a new body of economic research in order to answer the question: *are empirical studies able to measure the economic benefits and local if not system-wide externalities?* As a reality

³⁵ *De facto* rather than *de jure* regulations should enter an index, so that it is the quality or performance of the existing regulations that matter rather than merely their existence (Aron, 2000).

³⁶ The data are from Global Findex, and regulatory categories favour openness and certainty (Porteous, 2009).

³⁷ The indices may be correlated with omitted country characteristics; most possible instruments have the same problem.

check for policy-makers, there is an important role for micro-studies in evaluating the often optimistic assumptions underlying macro-studies that link digital finance and economic growth and inequality. These include assumptions about the barriers to adoption, the welfare impact, the take-up of diversified services including credit, and the government's tax take. For instance, a highly optimistic study by McKinsey (2016) applies a proprietary general equilibrium macroeconomic model to macro-data for seven countries, extrapolating the results globally for all emerging market countries; they predict that adoption and use of digital finance (banking in general) could increase the GDP of all emerging economies by 6 percent, or \$3.7 trillion, by 2025.

The survey has distilled lessons for the best practice (and the way forward) in empirical analysis of mobile money. Studies should demonstrate that they take the data issues seriously, including correctly measuring the usage of mobile money, or providing caveats. It is important to disentangle phone ownership from usage of its services, such as mobile money. The survey suggests that studies do grapple with unobserved heterogeneity but often not sufficiently. The wary policy-maker should give the greater weight to micro-studies using balanced *panel data* and which apply their considerable potential advantages for control of time-invariant and some time-variant (e.g. by location) heterogeneity (Box 2). Ideally these should include appropriate controls for potentially time-variant household characteristics (e.g. demography, *wealth*, having a migrant worker in the family and being formally banked) and location-by-time dummy proxies. Such a panel approach is probably “as good as it gets” in terms of ameliorating biases from unobserved heterogeneity. Some residual time-variant *unobservable* heterogeneity may still confound results, but in shorter time periods, the bias is likely to be small. In areas where mobile money is fairly new, panel survey data collection should be encouraged (see “on concerted action” below). Controlling for heterogeneity and finding exogenous instruments in *cross-sectional* studies is a heroic exercise: these studies are likely to be compromised and unreliable.

Finding credible exogenous instruments for the endogenous mobile money usage measure in instrumental variable (IV) methods has proved highly challenging. Most are based on agent density and network connectivity, assuming the “random roll-out” of mobile money and of network coverage. Statistical F tests often find the instruments weak, leading to potentially biased results. An increasing trend is to present propensity score analysis to reinforce the results when IV results prove ambiguous. However, more detail and clarity on evaluation and assumptions is required given the debate and controversy in the literature, so that the propensity score application is transparent and not a black box result.

Given drawbacks with all the techniques, it would be most satisfactory if studies could apply and contrast a range of techniques.³⁸ Applying a best practice approach to panel data both with *and* without fixed effects can ascertain the size and direction of the bias of OLS methods. The bias may be positive or negative; authors need to consider the direction of the bias, since then OLS methods can give useful upper or lower bounds on estimates. Not controlling for unobserved heterogeneity and lack of instrumenting or weak instruments probably results in an upward bias of the importance of mobile money for the level of consumption or saving. But, if looking at interactions with a negative shock, there is more likely to be a bias to zero³⁹; hence the micro-studies could be *under-stating* the absolute size of the beneficial effect of mobile money on risk-sharing. And while Suri and Jack (2016) characterise the risk-sharing result as more short-term in nature, if illness and death are prevented by improved insurance of this type, then there are long-term implications too. With a range of techniques, the potential biases of IV methods and of the propensity score matching can also be

³⁸ Several authors apply a range of techniques, e.g. Riley (2016).

³⁹ For instance, if wealthy households are more likely to adopt mobile money but have less need of the insurance than the poor when a negative shock strikes or are less likely to experience a large negative shock than the poor, then there is a bias toward zero.

ascertained. Where there is an *under-statement* of the bias, this qualitatively strengthens policy conclusions from noisy micro-studies.

Another problem, universally neglected by the surveyed studies, is non-constant parameters, e.g. because of spill-over effects and technological improvements. By its nature, the evolution of mobile money entails regime changes. These shifts introduce potential non-linearities that need to be tested for in both micro- and macro-work. The changes could result in earlier estimates being an *underestimate* of later effects. Structural breaks can mean the findings of studies can be hard to generalise. The micro-studies ignoring spill-over effects may be picking up only part of an effect, and hence may be a poor guide as to the economy-wide effect of a policy.

Robustness testing and testing of the validity of instruments (their strength and exogeneity) are patchy over the studies.⁴⁰ Researchers should try harder to illuminate those dimensions where welfare improvements are greatest by checking for differences in responses between more and less affluent households and other types of non-linearity (e.g. urban versus rural, by occupation, and by education level), and by gender (Suri and Jack (2016)). Areas for future research, where there has been little quantitative work as yet, include building on Riley (2016) in exploring community spill-over effects and on Jack et al. (2013) and Blumenstock et al. (2016) on little studied network effects; and a timely investigation of the new products of digital credit (Francis et al., 2017) and insurance through mobile money channels.

Focussing on the studies that apply best practice, the most convincing evidence is from the panel studies of Riley (2016) and Jack and Suri (2014), suggesting that mobile money fosters improved risk-sharing amongst informal networks in Kenya and Uganda after large shocks, through lower transaction costs of domestic transfer. On mobile money adoption, the Ugandan panel study of Munyegera and Matsumoto (2016a) deserves attention, supporting widespread qualitative evidence that education and wealth matter, but they found no gender or age effect for rural adopters. Generalisability of all these results may depend on the extent and quality of the agent network. Though all the non-RCT studies claim the *beneficial* influence of mobile money on reported savings (by saving method), and on saving flows, the results are compromised by lack of balanced panel data and appropriate instruments, and no robust and conclusive results can be reached. RCT studies in Mozambique and Afghanistan suggest saving did not increase though the saving method switched to mobile money; these studies use small and specialised samples and are probably not generalisable. Far less satisfactory are the (non-RCT) welfare studies reviewed, where results are generally judged unreliable by this survey. A Ugandan panel study suggests an improvement in consumption for mobile money users (Munyegera and Matsumoto, 2016a); the IV regression casts doubt on the claimed result, but it is supported by a propensity score analysis. A panel study for Kenya by Suri and Jack (2016) is at its most convincing in a differenced specification for consumption; consumption growth for male-headed households was negative and of female-headed households was positive with access to mobile money, but the result is tempered by probable bias from the limited control of heterogeneity (Section 4.5). The RCT study by Aker et al. (2016) found the receipt of cash transfers through mobile money accounts promoted intra-household bargaining power for women and their productivity in Niger, with reduced transactions costs. Child nutrition improved and increased diet diversity for the household, with fewer depleted durable and non-durable assets than for control groups. The generalisability of this study is uncertain and depends on a functioning agent network.

⁴⁰ Riley (2016), Blumenstock et al. (2016) and Jack and Suri (2014) are amongst rarer examples that test robustness, and present clear assumptions and caveats for the techniques.

Repeating such RCT studies across many locations, cultures, continents and time periods may help reinforce the conclusions and generalisability.⁴¹

Digital finance is one of few areas where there has been a *real* revolution in services and leapfrogging over deficient traditional infrastructure. However, improved access to financial services is compromised by economic obstacles, significant amongst which are corruption, lack of electricity generation, and appalling road infrastructure.⁴² Complementary action is required to address such problems. The micro-studies show how difficult it is to quantify outcomes accurately to extrapolate from individual studies of different countries, scaling up the effects to make policy pronouncements. Given the lack of complementary inputs, there could be sharp returns to scale in the short-run from mobile money, but not in the long-run, given the constraints. On the other hand, the micro-benefit established by several studies could be multiplied greatly through spill-over effects in the presence of well-functioning general infrastructure and transparency (lack of corruption) – especially if mobile money itself reduced corruption.

Atkinson (2015) has argued that economic inequality is often aligned with differences in access to, use of, or knowledge of information and communication technologies. He stressed that researchers, firms, policymakers and governments have the possibility to shape the direction and path of technological change. Aid agencies, other donors, charitable foundations and international agencies have played a key role in the beneficial growth of mobile money and the associated financial inclusion (Aron, 2017). Creative coalitions and the investment in multi-stakeholder partnerships can prompt deeper change, learning and practical action. An important application is for academic research on mobile money. Poor quality data and sub-optimal data collection and analysis severely compromise the conclusions that can be reached from empirical work. A *concerted* attempt by donors, regulators such as central banks, the regulated MNOs and academics could harness the appropriate data for timely best practice analysis. If anonymising procedures were accepted, then the benefits from research analysis using anonymised disaggregated data could be reaped. The survey has highlighted the best practice techniques that when applied to empirical analysis could reach more reliable conclusions and bolster the case for significant government and donor support, and commercial investment.

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⁴¹ The challenge of scalability for RCT studies is addressed in Banerjee et al. (2016). Deaton and Cartwright (2016) recommend a route to precision through prior information (which is excluded by randomisation) and controlling for those factors that are likely to be important. Then, they argue, there is a better chance of "transporting" results more generally to other contexts.

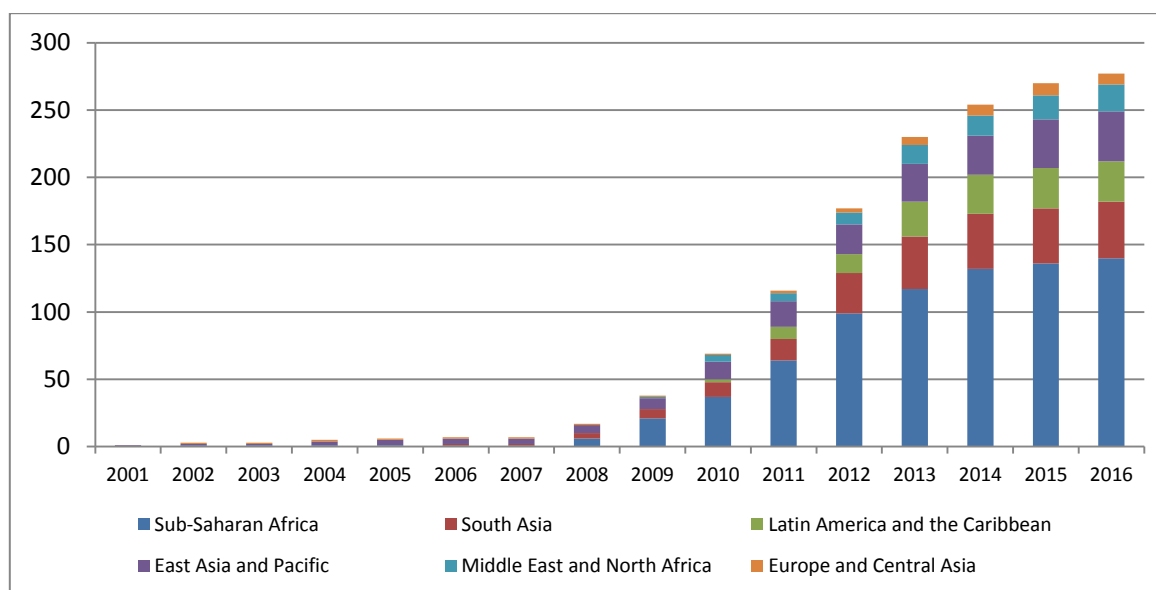
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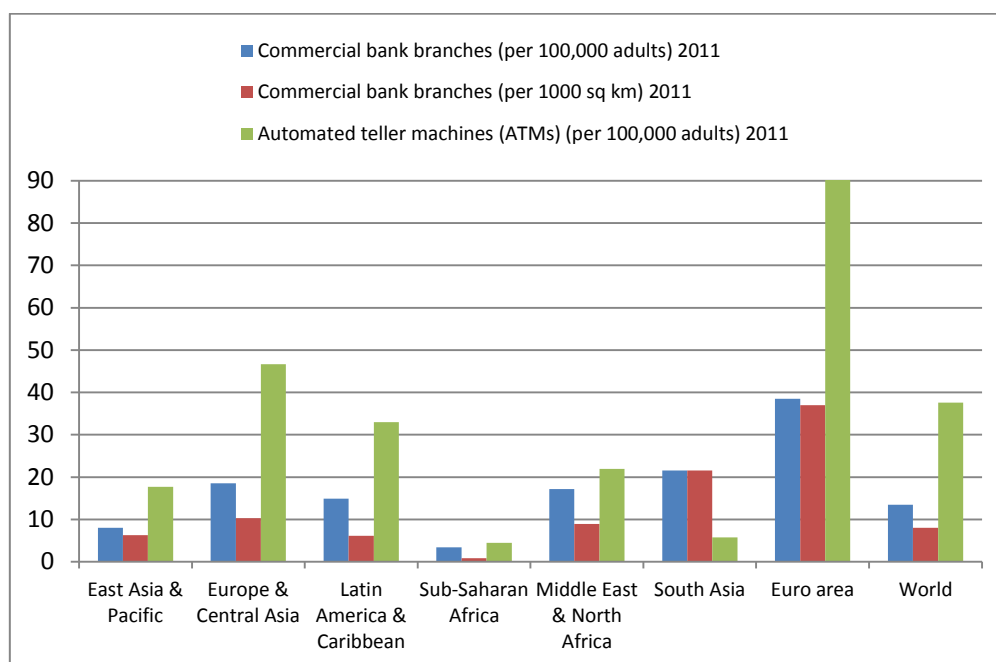
Figure 1: Number of live mobile money services for the unbanked by region



Source: Data from the GSMA State of the Industry report (2016).

Notes: The first mobile money system was launched in the Philippines in 2001, and M-Pesa in 2007.

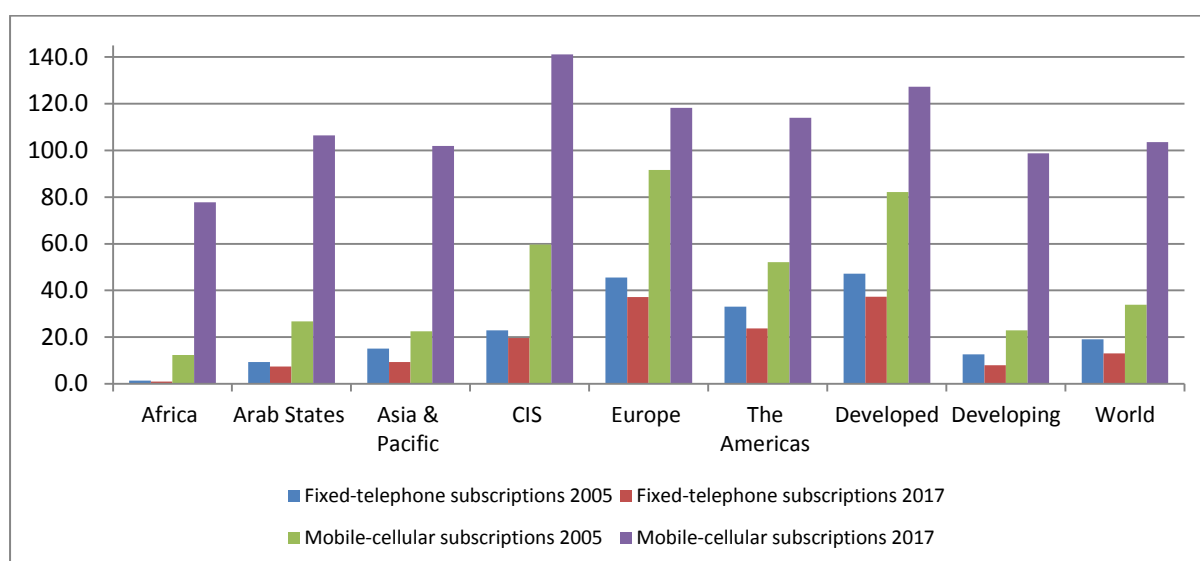
Figure 2: Provision of banking infrastructure



Source: G20 Financial Inclusion Indicators database, World Bank.

Notes: This shows the position shortly after the adoption of mobile money in Kenya.
The first five regions refer to “developing only”.

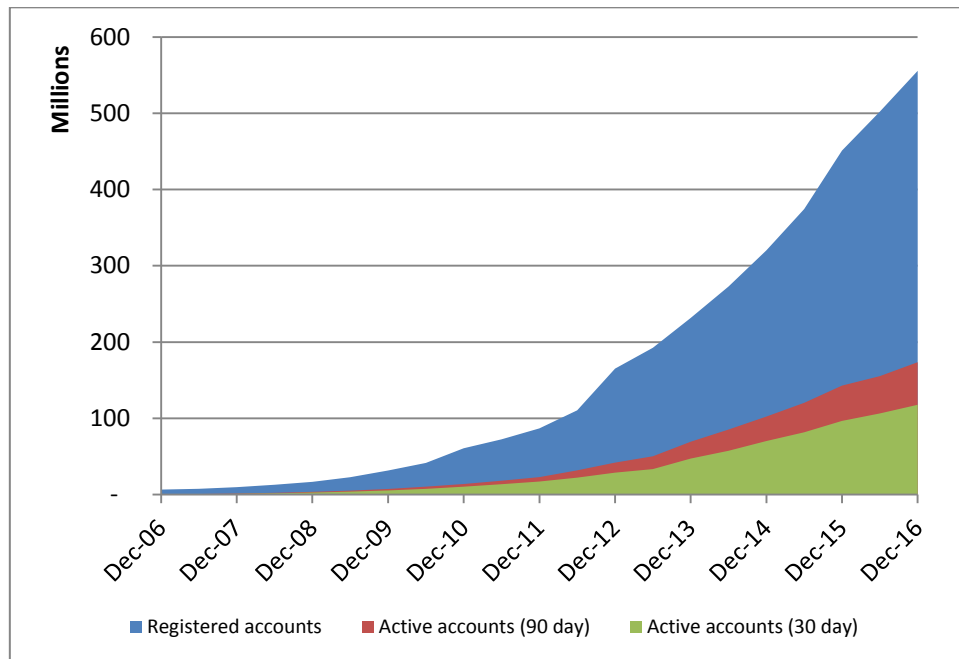
Figure 3: Fixed telephone and mobile-cellular subscriptions: 2005 and 2017



Source: ITU World Telecommunication, ICT Indicators database.

Notes: Subscriptions are per 100 inhabitants. "Mobile phone subscribers" refer to active SIM cards rather than individual subscribers.

Figure 4: Registered and active total accounts



Source: Data from the GSMA State of the Industry report (2016).

BOX 1: An anatomy of Kenya's mobile money system: 2006-2017.

Origin and Size:

Kenya's mobile money system originated in 2005 as an experiment for loan payments via mobile phones in micro-credit schemes, in a public-private partnership between DFID (UK), the Kenyan Government and Vodafone. In March, 2007, Safaricom, the Kenyan subsidiary of Vodafone, launched a commercial payments service, M-Pesa, with the slogan "send money home", exploiting the proliferation of mobile phone ownership. A decade later, there are six operators, though Safaricom controls 65% of the market. The FinAccess (2013) survey revealed that 67% of the adult population used financial services in 2013 versus 41% in 2009, driven by mobile money. There were 27 million registered M-Pesa customers by 2017 of whom 19 million were (30-day) "active". M-Pesa revenue grew by 33% to Kshs 55bn (US\$536m) in the year to Mar. 2017, over a quarter of Safaricom's total service revenue. The Bank of Kenya recorded in 2015, for all operators, a monthly value of transactions of Kshs 227.9bn (US\$2.2bn), or about half of average monthly GDP.

Systems structures and regulatory regimes:

In August, 2014, the National Payment System Regulations were issued under the National Payment System Act, providing a legal framework for mobile money. This formalised and extended prudential and market conduct requirements for mobile money providers as previously articulated in simple letters of no-objection from the Central Bank of Kenya (CBK). The CBK has duties of oversight, inspection and enforcement. There are mechanisms for consumer protection, redress and confidentiality of data.

In Kenya, banks and non-banks, including mobile network operators (MNOs), may provide mobile money services. The net deposits from customers have to be invested in prudentially-regulated banks for safe-keeping in "Trust" accounts, which back 100% of the money of the participants in the mobile money service; the banks are required to satisfy fiduciary responsibility in all transactions concerning the Trust funds. No investment of Trust funds is allowed; the funds are strictly separated from the service provider's own accounts and safeguarded from claims of its creditors. Safaricom's Trust account interest income is covenanted to charity.

The early agent exclusivity arrangement for M-Pesa was formally outlawed in July, 2014; the CBK ordered Safaricom to open the agent network to other operators to improve competition and lower fees for customers. There is not yet interoperability of platforms though it is expected in 2017; users of mobile money services currently have to affiliate with multiple mobile providers.

Agency network:

By 2017, there were 136,000 M-Pesa agents countrywide (compared with about 2.43 commercial bank branches per 1000km² in 2013, or 1410 total branches). Establishing an agency network and the training and payment of agents is a considerable early investment by operators to develop the market. Retail cash agents transact with their own cash and electronic money in their own M-Pesa accounts to meet customer demand. Wholesale agents (banks or non-bank merchants) are allowed higher limits on electronic money stored in their M-Pesa accounts; they perform a liquidity management service for retail agents, who typically transact daily with wholesalers. Retail agents open accounts observing identity checks required by anti-money laundering legislation, and the cash provision function spans in-store cash merchants to street-based merchants. M-Pesa agents are compensated from transaction fees charged to customers.

A payments platform for individuals:

Mobile phone users purchase a SIM card with the mobile money "app" for their phone, register with a retail agent using a national identity card and acquire an electronic mobile money account. They deposit money into the account by giving cash to the agent, and receive, in return, equivalent value "electronic money" via their mobile phone. To withdraw money, they transfer electronic money via their mobile phone to the cash merchant's mobile money account, and receive cash in return. Electronic money can be transferred instantly from a customer's account to any other individual, whether registered or not, without using formal bank accounts. The transactions are authorised and recorded in real time. A secure text message (SMS) with a code is sent to the recipient, authorising a retail agent to transfer money from the remitter's account into cash for the

designated recipient. The maximum allowed account balance is Ksh100,000 (US\$970), the maximum daily transaction is Ksh140,000, the maximum per transaction is Ksh70,000, and the minimum allowed transfer is Ksh1 (US10cents). The main transactions are non-bank payments services such as buying airtime, paying bills and school fees, and domestic transfers.

Transactions costs:

Depositors do not receive interest on their electronic accounts and bear the risk of loss of value through inflation. They pay the cost of transferring and withdrawing money, but there is no charge for depositing money. The graduated withdrawal fee pays for the cost of the M-Pesa account, ranging from about 0.5% for large transfers to 20% for the smallest. The costs of transfer are 10% for the smallest transfers, falling to 0.5% at transfers of Kshs 20,000, and to 0.16% for Kshs 70,000. Costs are greater to transfer to unregistered users.

Expanding to business usage and payments platform

Safaricom has pioneered a business payments platform and this is an important growth area for the company. The “Lipa na M-Pesa” business network has built a critical mass of consumers using retail payments providing dedicated business till numbers and low transaction fees, and it enables bulk disbursements such as promotional payments or salary payments. For Safaricom, customer-to-business payments accounted for 10.5 percent of the average monthly value of all payments in 2016.

Expanding to a savings and micro-credit platform

M-Shwari is a savings and loan product operated entirely from the mobile phone, launched in 2012 by partners Safaricom and Commercial Bank of Africa. By 2016, (30-day) active customers numbered 3.9m, with Kshs 8.1bn on deposit. Customers can move funds between their M-Pesa account and M-Shwari bank savings account (with no minimum balances or charges, and paying graduated interest rates of 2-5%). The new Lock Box service pays higher interest rates for *fixed* deposits. M-Pesa subscribers of 6 months standing can apply for an M-Shwari loan without fees or paperwork. An initial credit score and loan limit is calculated using an algorithm from the stream of recorded financial actions. Loan disbursement and repayment is via M-Pesa, without loan interest charges, but with a facility fee of 7.5%. Loan sizes range from US\$1 to US\$235 with a 30-day term but can be rolled over at a monthly fee of 7.5% (this resembles an interest rate at a high annual compounded rate of 138 percent). Progressively larger loans can be extended when a loan is successfully repaid. By 2016, there was Kshs 7.4bn on loan; non-performing loans numbered 1.93% of the portfolio, with an average loan size of Ksh4000 (\$39).

Expanding to a micro-insurance platform

In 2015, an M-Pesa health micro-insurance product, launched the previous year, was discontinued through failure to gain traction. The annual premium (Kshs12,000) had bought family cover worth Kshs 290,000 for maternity, dental and optical care, and hospital and funeral expenses. In late 2015, M-Tiba (“mobile care”), a dedicated health savings “wallet” to pay for care at selected affordable health providers, was launched by Safaricom with two partners, enabling users to save and pay for healthcare. Donors and insurers can use M-Tiba for targeted products, including vouchers, managed funds and low cost health insurance.

Expanding to international (diaspora) remittances

Kenya received an estimated US\$1.7bn of international remittances in 2016 (World Bank Migration Brief 27). In 2014, Safaricom partnered with MoneyGram, to enable remittances from over 90 countries worldwide to be sent to M-Pesa users, and now similar agreements with Western Union and several other partners. In 2015, Vodafone and MTN announced an interconnection of mobile money services enabling affordable regional remittances between M-Pesa customers in Kenya, Tanzania, Democratic Republic of Congo and Mozambique, and MTN Mobile Money customers in Uganda, Rwanda and Zambia. In 2016, Vodafone partnered with HomeSend (a joint venture created by MasterCard, eServGlobal and BICS) to extend remittances for M-Pesa users in Africa, Albania and Romania.

Government and donor usage of mobile payments

Governments could securely pay policemen and other officials their wages; the national revenue authority could accept payments for taxes, licences and fines, and municipalities for parking payments; and public transport could use mobile money payments. Delivery of social welfare or aid with mobile payments could reduce “leakage” and ghost recipients. Some of these are a reality in Kenya, with M-Pesa and Airtel, through pilots or fully-functioning systems, but government salary and social payments have lagged relative to Afghanistan, Tanzania and Malawi. Donor and commercial initiatives increasingly use the technology; e.g. affordable solar energy-powered electricity systems in rural areas can be fully purchased remotely on a pay-as-you-use basis using mobile payments (M-Kopa Solar launched in 2014 in Kenya.)

Geographical expansion of the successful M-Pesa model

Vodafone has concentrated on proliferation of its mobile money platform in markets that are heavy cash users. M-Pesa is used in ten countries other than Kenya, by order of roll-out: Tanzania, Fiji, South Africa, Fiji, DRC, India (launched in 2013), Mozambique, Egypt, Lesotho, Romania (2014), Albania (2015) and Ghana (2015).

BOX 2: RCT, Difference-in-Differences, and other empirical approaches used in mobile money studies

Randomised Controlled Trials (RCT)

Common in medical research, RCT was little used in economics before 2003, and has generated heated debate. This critique is pertinent to the reliability and generalisability of mobile money RCT studies. An RCT evaluates whether a specific, controlled change has a discernible impact on a treated group relative to a control group. RCTs focus on small interventions that apply in certain contexts so that inferences for other settings, or even scaling up based on the results, may be invalid. Identifying a causal connection in one situation might be specific to that trial and not a general principle; even the direction of causality can depend on the setting. Deaton (2010) argues that there are actually two stages of selection. In the first, a group is chosen from the entire population that will in the second stage be randomly divided into the treated and control groups. The first stage is not random, but may be determined by convenience or politics, and therefore may not be representative of the entire population. Deaton and Cartwright (2016)^a further argue that randomisation does not *guarantee* that the treatment and control groups are identical except for the treatment, i.e. it does not guarantee that *other* causal factors are balanced across the groups at the point of randomisation. The studied populations in RCTs are typically very small, so that an outlier in the experimental group can have a large distortionary effect. Further, the trial or intervention itself (Gillespie, 1991), and the nature and quality of information provided about the intervention, can affect behaviour. Standard errors are often erroneously computed and spurious inferences made, as t-statistics for estimated average treatment effects from RCTs do not in general follow the t-distribution.

Difference-in-Differences (DD)

A second approach, more widely-used in mobile money research, tests specific theoretical hypotheses using a Difference-in-Differences (DD) estimation, which mimics an experimental approach by comparing differences in the changes of a control and a treated group after an intervention (here, the adoption of mobile money). The restrictive assumption is made that in the absence of the intervention, the average change in the outcome for the affected and control groups would have been the same. This is the “parallel or common trends” assumption. The DD estimates typically derive from an Ordinary Least Squares (OLS) regression for repeated cross-sections or for a panel of data on individuals (appropriately sampled to avoid selection bias) for one or more periods before and after an intervention. A dummy variable is included for the intervention and a set of control variables. The method has the appeal of simplicity, and when the interventions are approximately random, conditional on the time and location fixed effects, and also on household fixed effects^b in the context of household panels, it can reduce the (time-*invariant*) endogeneity problems from comparing heterogeneous individuals. What remains is time-*variant*, unobserved household heterogeneity. This may be partially mitigated with appropriate controls for time-variant household characteristics (demographics, for instance) and location-by-time fixed effects^c (accounting for only part of the time-*variant*, unobserved heterogeneity, since these dummies average over households in a location). Further problems arise when the intervention is not random, when the linear assumption under OLS is inappropriate, and from serial correlation problems exaggerating levels of significance in standard errors when several years of data are involved (Bertrand et al., 2004). One useful test of the DD strategy is the placebo test; it uses data from prior periods before the intervention, and the DD is redone aiming for a close-to-zero placebo effect for the included intervention.

Propensity Score methods

Several mobile money studies present supplementary evidence from Propensity Score matching methods. These methods mimic characteristics of an RCT in the context of an observational (or non-randomised) study, using non-parametric rather than regression techniques to estimate the effects of an intervention (e.g. use of mobile money) on outcomes between treated and control groups. Where baseline characteristics of treated subjects often differ systematically from those of untreated subjects, Propensity Score matching can match samples of subjects who are as similar as possible on observed (pre-treatment) characteristics. Differences in post-treatment outcome variables between the matches are averaged and are attributed to the treatment. There are two crucial assumptions for the validity of the technique. There should be no hidden bias from unobserved heterogeneity

and the criteria for adequate balance should be clear and satisfied. However, conditioning on the Propensity Score need not balance *unmeasured* covariates; and even the balance-checking between *measured* co-variables is problematic because the criteria for adequate balance are ill-defined (see Hill (2008) and Austin (2011)).

Instrumental Variables (IV) methods

IV can be used for consistent estimation when correlation between explanatory variable/s and the error term is suspected. An endogenous variable is replaced by the predicted value from a set of instruments which are strongly correlated to the explanatory variable (informative or strong), but uncorrelated with the errors (valid or exogenous). Finding credible exogenous instruments for mobile money usage is a challenge. Several instruments have been used in the mobile money empirical literature^d but statistical tests tend to find them weak which may introduce bias. Instruments based on agent density and network connectivity assume that the roll-out of mobile money and network coverage itself was “random”.

^a See a non-technical version at: <http://voxeu.org/article/limitations-randomised-controlled-trials>, Nov. 2016.

^b A dummy variable is included for every household or entity (bar one entity).

^c A *national* time effect is a common effect across time experienced by all *regions*, e.g. from macro-fluctuations. But disaggregating to two regions, North and South say, where North is less affected by drought, then interacting both regional dummies with time allows their differential response over time to be captured. With location-by-time fixed effects (without a national time effect), there is a location (e.g. district, region or country) dummy for each year (bar one location and one year).

^d Instruments used for mobile money usage (Table 1) are: the log of the distance to the closest agent and the number of agents within 5km of the household (Jack and Suri, 2014), the distance to and cost of reaching the nearest mobile money agent (Riley (2016), and the log of the distance to the nearest mobile money agent (Munyegera and Matsumoto, 2016a); the fraction of respondents in the sub-location registered with M-Pesa (Demombynes and Thegeya, 2012) and the proportion of households using mobile money and for those owning a mobile phone at the village level (Kikulwe et al., 2014); household-specific mobile phone network connectivity and the size of the information exchange network of the household (Murendo and Wollni, 2016); and 2006 survey responses (before M-Pesa was introduced) about riskier, slower and more costly transfer methods (Mbiti and Weil, 2016).

Table 1: A typology of micro-empirical studies on the economic effects of mobile money

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
Adoption				
<p>Munyegera and Matsumoto (2016a)</p> <p>Dependent variable: <u>Probit/FE</u>: Zero-1 dummy: for whether household <i>i</i> living in village <i>j</i> in district <i>d</i> uses mobile money services at time period <i>t</i>.</p> <p>Definition of M-money usage: Exact definition of “use” unclear.</p>	<p><u>Uganda</u></p> <p>Balanced panel of 838 households generated from the 3rd & 4th rounds of household and community surveys in Uganda, 2009 & 2012 (RePEAT) project.</p>	<p><u>Probit regression; and linear probability model with household fixed effects</u></p> <p><i>Controlling for:</i> district-by-time dummies; dummy for ownership of a mobile phone; and vector of household characteristics (age (and age squared), gender and education (years of schooling) of household head, dummy for migrant worker in household, distance to nearest mobile money agent, size of household, and household wealth (land size and total assets)).</p> <p>[Robust standard errors]</p>	<p>Household fixed effects and location-by-time dummies are used in a panel context, and many individual controls (including control for ownership of a mobile phone and a migrant worker) reducing potential endogeneity; possibly some <i>time-variant</i> household heterogeneity may remain.</p> <p>Disentangle technology/service? Yes. Mobile phone dummy used.</p>	<p><i>Adoption of mobile money</i></p> <p>Cannot find a gender effect or an age effect for these rural adopters; distance to the agent is important as is wealth; and dummies for ownership of the phone and migrant worker are significant.</p>
<p>Weil et al. (2012)</p> <p>Dependent variables: <u>OLS</u>: Zero-1 dummy: for whether an individual uses mobile money; Frequency of mobile money transactions per user.</p> <p>Definition of M-money usage: Exact definition of “use” unclear.</p>	<p><u>Kenya, Tanzania and Uganda</u></p> <p>Repeated cross-sections. FinAccess data from Kenya (2006 and 2009); Finscope data for Tanzania and Uganda (2006 and 2009). (These are not panel data.)</p>	<p><u>OLS regressions</u></p> <p><i>Controlling for:</i> vector of individual characteristics (dummies for urbanisation and the level of poverty, 3 age cohorts, education (primary/secondary/ tertiary), marriage, and gender).</p> <p>[Robust standard errors]</p>	<p>There are endogeneity problems. Omission of measurable controls e.g. banking status, wealth and mobile phone ownership. Unobservables like spill-over effects cannot be controlled for. But location-by-time fixed effects were not included for repeated cross-sections to control for (some) <i>time-variant</i>, unobserved regional-level heterogeneity. The results are thus only suggestive.</p> <p>Disentangle technology/service? No.</p>	<p><i>Adoption of mobile money</i></p> <p>They deduce for all three countries (limited significance in the less well-developed markets of Tanzania and Uganda) that adopters are younger, wealthier, better educated and urban dwellers. Analysis of frequency of mobile money transactions per user, yields similar findings. Cannot find a gender effect.</p>
Private domestic remittances using mobile money or an early version of mobile money (prepaid airtime): risk-sharing studies				
<p>Batista and Vicente (2016)</p> <p>Dependent variables: <u>OLS</u>: log consumption per</p>	<p><u>Mozambique</u></p> <p>Panel data (some analysed as cross-section) generated in rural</p>	<p>Randomised Controlled Trials (RCT).</p> <p><i>Random intervention:</i> treated individuals receive training about a new mobile money product.</p>	<p>The first stage of selection may not be random, and there are other problems of potential heterogeneity (see Deaton’s critique, <u>Box 2</u>). Other selection</p>	<p><i>Total Consumption</i></p> <p>No significance for the treatment dummy for consumption in the absence of shocks.</p> <p><i>Total consumption after idiosyncratic,</i></p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
<p>capita; DD: Binary variables for investment in agricultural categories (e.g. active farm or pesticides) or business categories (e.g. cattle trading). Definition of M-money usage: Treated individuals receive training about a new mobile money product, M-Kesh.</p>	<p>provinces: Maputo-Province, Gaza, and Inhambane, March 2012 (102 rural Enumeration Areas: 51 locations in 3 regions randomly selected as treatment areas; the residual is control group). Administrative mobile money records combined with household survey data (3 years, 2012-14). [<i>Selection criteria:</i> rural treatment locations required mCel coverage & 1 or more commercial banks; targeted individuals required a mobile phone number and a migrant family member in Maputo with mobile phone number.] [<i>Shock index:</i> simple average of zero-1 indicators for mix of negative shocks: deaths, job loss, health problems, loss of valuables, agricultural losses.]</p>	<p><u>OLS regression specification</u> for consumption, comparing differences in outcomes for targeted and control individuals for 2013, 2014 and these years pooled. <i>Controlling for:</i> treatment dummy variable; province dummies; year dummies; and individual controls for age and gender. <u>OLS regression specification</u> for consumption and risk sharing, comparing outcomes for a cross-section in mid-2014. <i>Controlling for:</i> treatment dummy variable; a shock index; locational dummies; and individual controls for age and gender. <ul style="list-style-type: none"> The shock dummy and M-money dummy are crossed to test if M-money users are better able to smooth risk. <u>OLS Difference-in-Differences (DD) regressions</u> for investment outcomes, comparing outcomes for 2013 and for 2014. <i>Controlling for:</i> treatment dummy variable; locational dummies; year dummies; and individual controls for age and gender. [Clustered standard errors]</p>	<p>criteria (see LHS) narrow the type of population which reduces generalisability. There is a problem of interpreting a treatment effect when intervention depends also on the type of training information provided (see Aker and Blumenstock (2015)). The constructed shock index is misleading as it conflates shocks that raise and those that lower expenditure; a simple average is used. Absence of time-by-location dummies: yet are critical to control for heterogeneous effects across locations of the 2013 flood. They do not cross individual characteristics with the shock index (as in Riley (2016) and Jack and Suri (2014)). Disentangle technology/service? Yes. Only individuals with phone numbers are selected.</p>	<p><i>reported negative shocks</i> The treated group increases consumption in response to a negative shock (e.g. health or funeral expenditures drawing on remittances); the control group has to reduce other expenditure. The negative coefficient for the treatment dummy suggests the treated group is spending less (perhaps because they are sending remittances to relatives or if there is a systematic difference between treated and untreated groups e.g. are poorer). Suggests improving rural households' welfare as mobile money contributes to household consumption smoothing. <i>Investment</i> No productive effects of remittances: for mobile money users, active farm investment and investment in cattle trading falls significantly, but household ownership of "safe asset" livestock is higher. Interpret as evidence that (informal) insurance from mobile money reduced the incentives for risky investment (given credit constraints).</p>
<p>Blumenstock, Eagle, and Fafchamps (2016) Dependent variable: DD: three degrees of disaggregation: (i) total gross transfers of airtime received by all users in location r at time t. (ii) total gross transfers</p>	<p><u>Rwanda</u> Panel data. 2005-09, daily primary telecom operator's log of activity (50 billion transactions: calls, text messages, and airtime transfers and purchases), 1.5 million subscribers; 2005 Rwanda Demographic and Health</p>	<p><u>Panel Difference-in-Differences (DD) regressions</u> <i>Random intervention:</i> an earthquake shock <i>Controlling for:</i> (i) shock dummy equal to 1 for location r receiving a shock at time t and 0 otherwise; time dummies; and location fixed effects. (ii) shock dummy equal to 1 for user i in location r receiving a shock at time t and 0</p>	<p>The earthquake shock is exogenous if unpredictable. Potential time variance in location could be tested for with broader location-by-time dummies than the epicentre-by-time dummy. There is imaginative use of fixed effects, and interaction effects with innovative wealth and social</p>	<p><i>Airtime transfers after covariate negative earthquake shock</i> As well as geographical proximity, transfers to victims near the epicentre after the Lake Kivu earthquake of 2008 are determined by a past history of reciprocity between individuals, and the transfers decrease in the wealth of the sender and increase in the wealth of the recipient. The magnitude of these</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
<p>received by user i in region r at time t. (iii) total gross transfer of airtime sent to an individual i, located in region r at time t, from another individual j.</p> <p>Definition of M-money usage: MNO record of pre-paid airtime (a precursor of mobile money) transferred.</p>	<p>Survey; 2009/2010 phone survey of 1,000 individuals on household asset ownership and housing characteristics.</p>	<p>otherwise; epicentre dummy for user i near epicentre at any time; time dummies; and recipient fixed effects.</p> <p>(iii) as in (ii), but replacing the fixed effects by a fixed effect controlling for average intensity and direction of transfer flows between two users.</p> <p><u>Heterogeneity amongst individuals:</u> add in (ii), the interactions of predicted measures of expenditure (to proxy for wealth) and of social connectedness with the shock dummy, the epicentre dummy and a dummy capturing the day of a severe shock.</p> <p><u>Heterogeneity amongst sender-recipient pairs:</u> add in (ii), the interactions of information on the geographic distance between i and j, and the history of transfers between them with the shock dummy, the epicentre dummy and a dummy capturing the day of a severe shock.</p> <p>[Clustered standard errors]</p>	<p>connectedness measures and others, to control for types of heterogeneity. There may be selection problems associated with social networks, see Section 4.4. Selection is also induced when wealth itself determines the ownership of phones as in Rwanda in 2008, though in a sharing culture some may own only the SIM card and borrow a phone.</p> <p>Disentangle technology/service? Yes. Only individuals with phone numbers are selected.</p>	<p>transfers is small in absolute terms.</p>
<p>Riley (2016)</p> <p>Dependent variable: <u>DD/ IV:</u> log of consumption per capita</p> <p>Definition of M-money usage: Households that used mobile money services at least once in the previous year.</p>	<p><u>Tanzania</u></p> <p>Panel data. Tanzania National Panel household panel survey (NPS) for 2008-9, 2010-11 and 2012-13, covers 3,265 households in 26 districts containing 409 Enumeration Areas: 3 waves of data and a low attrition rate; and Finscope (2013) data.</p> <p>[Treatment groups are villages where mobile money is available.]</p> <p>[Shocks: self-reported aggregate income shocks e.g. droughts or floods; or</p>	<p><u>Panel Difference-in-Differences (DD) regressions</u></p> <p><i>Random intervention:</i> a negative income shock</p> <p><i>Controlling for:</i> M-money dummy equal to 1 for households that used mobile money services and 0 otherwise; a dummy for aggregate shock; household fixed effects, location-by-time dummies, a dummy for the proportion of mobile money users in a village; and household characteristics.</p> <ul style="list-style-type: none"> ○ The shock dummy and M-money dummy are crossed to test if M-money users are better able to smooth risk. ○ The shock dummy and village M-money dummy are crossed to test if there are spill-over effects. ○ The vector of household characteristics is 	<p>The specification requires the shock to be random. If correlated with changes (given fixed effects) in <i>unobservable</i> household characteristics, shocks would not be random.</p> <p>A more precise rainfall measure would separate large positive from large negative deviations.</p> <p>Possibly restrictive to assume the social network for sharing is only village-wide, and constant.</p> <p><i>Time-invariant</i> unobservables are controlled for by household fixed effects. Village-by-time dummies average over individuals in villages, and eliminate some (not all)</p>	<p>This study examines potential beneficial spill-over effects of mobile money to the village community (which includes non-users) following a aggregate (co-variate) shock.</p> <p><i>Effect of shock on consumption</i> The rainfall (or other) shock causes a drop in consumption of 6-11% for all households without mobile money use.</p> <p><i>Effect on consumption without shock</i> For villages where at least one person uses mobile money, average village consumption is 4-10% higher (1% significance level and robust to the inclusion of fixed effects); signals positive spill-over effects of mobile money to non-users in the village;</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
	a constructed measure of rainfall deviations (> 1 standard deviation) from a 40 year mean, expressed as an absolute value.]	<p>crossed with the shock dummy.</p> <p>[<i>Household characteristics</i>: a rural dummy, age and education (years) of the household head, the size of household, a dummy for ownership of a mobile phone, some financial indicators, a wealth index constructed using principal component analysis, and a household head occupational dummy.]</p> <p><u>Instrumental Variables</u>;</p> <p><i>Controlling for</i>: <u>as above</u></p> <p>[<i>Instruments for mobile money and for its interaction with the income shock</i>: distance to and cost of reaching the nearest mobile money agent, and the interactions of each with the shock]</p> <p><u>Propensity score model</u></p> <p>Matched users and non-users with similar characteristics.</p> <p>[Standard errors are clustered, village level]</p>	<p>unobserved, village-level, time-varying heterogeneity (e.g. self-selection into villages by providers; localised “herd” effects and learning spill-over; differential effects of rainfall by occupation across districts). But time-varying, unobservable, household heterogeneity may remain.</p> <p>The IV results do not reject their findings; but though the instruments are statistically exogenous they were found to be weak, introducing bias.</p> <p>Disentangle technology/service? Yes. A mobile phone dummy used.</p>	<p>For households with mobile money users (fixed effects included), their consumption is unaffected.</p> <p><i>Effect on consumption after shock</i> There is no spill-over benefit to the community for non-users. But for households using mobile money, consumption increases by 8-14% i (5% significance level), cancelling the effect of the negative shock, helping these households to smooth consumption.</p> <p>Benefits to both the users and community are highest in rural areas and decrease sharply with distance to the nearest mobile money agent.</p>
<p>Jack and Suri (2014)</p> <p>Dependent variable: <u>DD/IV</u>: log annual per capita consumption for a household at a particular location and time.</p> <p>Definition of M-money usage: M-Pesa registrations from the telecommunications firm (at least one per household).</p>	<p><u>Kenya</u></p> <p>Panel data. Household panel survey conducted in Sep. 2008 (3000 HHs), Dec. 2009 (2017 of these HH) and Jun. 2010 (1,595 HHs from 2008 sample, but 265 not interviewed in 2009). They construct a 2-period balanced panel of 2,282 (or 2017+265) HHs, with attrition rate of ~24%, controlling for round (time) dummies in regressions. Excluding Nairobi lowers the attrition rate to ~18%.</p>	<p><u>Panel Difference-in-Differences (DD) regressions</u></p> <p><i>Random intervention</i>: a negative income shock</p> <p><i>Controlling for</i>: M-money dummy equal to 1 for an M-Pesa user in the household in survey and 0 otherwise; a dummy for negative shock to income in last 6 months; household fixed effects; location-by-time dummies; rural-by-time dummies; and household characteristics.</p> <ul style="list-style-type: none"> ○ The shock dummy and M-Pesa dummy are crossed to test if M-Pesa users are better able to smooth risk. ○ The vector of household characteristics is crossed with the shock dummy. <p>[<i>Household characteristics</i>: household demographics, household head years of</p>	<p>The specification requires the shock to be random. If correlated with changes (given fixed effects) in unobservable household characteristics, shocks would not be random.</p> <p>Self-reported wealth is not in the vector of characteristics.</p> <p>Time-invariant unobservables are controlled for by household fixed effects. Location-by-time dummies average over individuals within locations, eliminating some (not all) unobserved, location-level, time-varying heterogeneity. Ditto the inclusion of rural-by-</p>	<p><i>Total, food and health consumption after idiosyncratic, reported negative shocks</i> For Kenyans with access to mobile money, total consumption is unaffected negative income shocks, while the consumption of non-users drops by 7% (significant at the 10% level). The effect is more evident for the bottom three quintiles of the income distribution. Same result for the impact of health shocks on total consumption; but food consumption is equally well-smoothed by users and non-users.</p> <p>Transactions cost savings mean users are better able to smooth consumption following negative income shocks, from the greater frequency, geographical diversity and size of mobile money</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
	<p>A March 2010 survey of nearly 7,700 M-Pesa agents, who also reported when they began business.</p> <p>[<i>Shocks</i>: negative shock could be covariate like a drought; or idiosyncratic like severe illness, job loss, fire, livestock death, and harvest or business failure.]</p>	<p>education and occupation dummies (for farmer, business operator and professional), use of financial instruments (bank accounts, savings and credit cooperatives and rotating savings and credit associations), and a dummy for cell phone ownership.] Note that wealth is not included.</p> <p><u>Reduced form regressions</u></p> <p><i>Controlling for</i>: <u>as above</u>, but without crossing vector of household characteristics with the shock dummy.</p> <ul style="list-style-type: none"> Simply substitute “access to an agent” for M-Pesa usage. <p><u>Instrumental Variables</u></p> <p><i>Controlling for</i>: <u>as above</u>.</p> <p>[<i>Instruments for M-Pesa user in the household at the time of the survey and for its interaction with the income shock</i>: distance to the closest agent, the number of agents within 5 km of the household, and the interactions of each with the shock]</p> <p>[Standard errors are clustered, village level]</p>	<p>time dummies. But time-varying unobservable <i>household</i> heterogeneity may remain; also, if there are missing interaction effects from time-varying unobservables (e.g. wealth) that could help households to smooth risk, this may bias the role of M-Pesa in smoothing consumption.</p> <p>Their claim for validity of instruments relies on lack of systematic correlation between agent density and observable household characteristics that may help households to smooth risk (their Table 6C uses only <i>bivariate</i> correlations, however; see text on more comprehensive testing). There may still be correlation with <i>unobservables</i> or poorly-measured observables (e.g. wealth) that may help households to smooth risk. F tests suggest instruments are not weak; no tests are reported for whether they are exogenous. They do successfully conduct placebo tests.</p> <p>Disentangle technology/service? Yes. A mobile phone dummy used.</p>	<p>remittances.</p> <p>Evidence suggests higher expenditure after negative shocks, rather than “stable” consumption, perhaps on repairs and medical treatment.</p> <p>The IV regressions reinforce the conclusions: improved access to agents improves a household’s ability to smooth risk. The agent roll-out proved statistically to be uncorrelated with observables including self-reported wealth (though using only <i>partial</i> correlates, see LHS); in principle instrumenting could help to control for endogeneity.</p>
Private domestic remittances using mobile money: welfare studies				
<p>Suri and Jack (2016)</p> <p>Dependent variable:</p> <p><u>OLS</u>: the outcome (measured in 2014) for household (or individual) i</p>	<p><u>Kenya</u></p> <p>Panel data. Household panel survey conducted across 118 locations, in Sep. 2008 (3000 HHs),</p>	<p><u>Panel OLS regressions</u></p> <p><i>Controlling for</i>: the <u>change in</u> agent density between 2008 and 2010; location fixed effects; a dummy for gender of the household head in household level regressions (or for the</p>	<p><i>Proxying mobile money usage</i></p> <p>Pre-dating the agent density proxy relative to 2014 outcomes intends to make it exogenous. There are 2 problems. It may be</p>	<p><i>Consumption, growth in consumption, poverty</i></p> <p>Prior agent density (proxies access to M-Pesa) increased per capita consumption levels (2014) and reduced the level of</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
<p>in location <i>j</i> for 3 categories of variable:</p> <p>(i) the log of average consumption per person in a household, the change in this variable, and the level of household poverty rates (consumption pc below \$1.25 per day or “extreme poverty”, and below \$2 per day);</p> <p>(ii) physical and financial wealth: the log of assets, the log of total financial savings, and presence of a bank account; and</p> <p>(iii) occupational choices: farming, business and sales, or secondary occupations.</p> <p>Definition of M-money usage: they proxy usage by the change in agent density (i.e., the number of agents within 1 km of the HH) between 2008 and 2010.</p>	<p>Dec. 2009, Jun. 2010, 2011 and 2014 (1608 HHs); the 2011 survey was targeted specifically toward attrited households from earlier rounds; Nairobi was dropped from the sample after 2011 (480 HHs).; attrition from the original non-Nairobi sample, 2008- 2014, was 35%.</p> <p>A March 2010 survey of nearly 7,700 M-Pesa agents, who also reported when they began business.</p>	<p>individual in individual level regressions); and household (individual) characteristics.</p> <ul style="list-style-type: none"> ○ The gender dummy and the <u>change in</u> agent density are crossed to estimate the marginal effect of an increase in agent density for females. ○ The <u>change in</u> agent density is crossed with household (or individual) characteristics to rule out cases where the gender effect was in fact driven by these other characteristics. <p>[Household (individual) characteristics used in the regressions (measured in 2008): age and age squared of the household head.]</p> <p>[Household/individual characteristics used in the interaction effect (measured in 2008): (i) for individual regressions: education; (ii) for household level regressions: education, wealth, and a dummy for the household being unbanked (education and wealth are dummy variables for whether the household is below the median value in the sample).]</p> <p>[Standard errors are clustered, location level]</p>	<p>a poor proxy for <i>later usage</i> of mobile money, as usage growth is catalysed 2010-14 (see text on statistics). The exogeneity assumption relies on lack of systematic correlation (using only <i>bivariate</i> correlations) with observable household characteristics possibly associated with future outcomes (see text on testing more comprehensively). There may also still be correlation with unobservables or poorly-measured observables (e.g. wealth) that affect outcomes.</p> <p><i>Unexplained heterogeneity</i> There is probably considerable unexplained heterogeneity in the <i>levels</i> regressions. Household fixed effects, location-by-time dummies, ownership of a mobile phone, wealth, education and possession of a bank account are excluded. More weight should be placed on regression of the <i>change in the log level of consumption</i> which serves to remove household fixed effects (though time-varying heterogeneity may still introduce bias).</p> <p>Disentangle technology/ service? No.</p>	<p>poverty for 2 measures of poverty (2014). Effects are stronger for female-headed households for the levels of consumption and of extreme poverty. Consumption <i>growth</i> for male-headed households was negative; that of female-headed households was positive and statistically significant. (The result is robust to interactions between changes in agent density and other observable household characteristics.)</p> <p><i>Mechanisms</i> Mobile money access (prior agent density) cannot explain the (level of) the log of assets. The regression of the log of total financial savings (including mobile money accounts) does not control for mobile phone ownership, wealth, marriage, income, education as in other savings studies, but only for gender, age and age squared of the household head. That said, “usage” promotes saving without a gender effect. With greater mobile money access (prior agent density), fewer report their major occupation as farming, for both genders, and more females report their main occupation to be in business, sales, or retail. The results are interpreted as saying mobile money has increased the efficiency of allocation of consumption over time, allowing allocation of labour to be more efficient, reducing poverty.</p>
<p>Murendo and Wollni (2016)</p> <p>Dependent variable:</p>	<p><u>Uganda</u></p> <p>Cross-sectional survey of 482 households in 39 villages in two regions in</p>	<p><u>OLS regressions/ endogenous treatment effect models</u> (for food consumption or the continuous food security index and treatment variable: mobile money usage dummy)</p>	<p>Only one IV result is reported: (i) <i>Food expenditure</i>: OLS estimates are relied on; (ii) <i>Food insecurity (continuous measure)</i>: IV regression used</p>	<p><i>Food expenditures</i> Mobile money use (10% significance level) increases food expenditure per AE by 9 percentage points; frequency of use and volumes transferred (both with 1%</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
<p>2 measures of “food security”:</p> <p>(1) Food consumption: OLS/IV: per capita aggregated food consumption expenditures (monthly per adult equivalents (AE): 7-day recall for regular purchases, 30-day recall for less frequent purchases);</p> <p>(2) Food Insecurity Indexes: OLS/IV: continuous Household Food Insecurity Access Scale (HFIAS), using weights from factor analysis or Probit/Probit (IV): binary Food Insecurity Index (constructed on HFIAS data).</p> <p>Definition of M-money usage: Households that used mobile money services at least once in the previous year.</p>	<p>November and December 2013.</p>	<p><u>OLS/Instrumental Variables regressions</u> (for food consumption or the continuous food security index and treatment variables: continuous volume or frequency of transfer)</p> <p><u>Probit and Probit (IV) models</u> (for the binary food security index and all treatment variables)</p> <p><i>Treatment variables:</i> M-money dummy equal to 1 for households that used mobile money services and 0 otherwise; or continuous variables for frequency of use of services or the volume transferred via mobile money.</p> <p><i>Controlling for:</i> treatment variable; and household characteristics.</p> <p>[<i>Household characteristics:</i> age, education (years) and gender of household head, household size, ratio of dependents (below 15 & above 65 years) to workforce (16-64 years), adult equivalent, land size, log value of farm equipment, dummy for household member(s) engaged in off-farm income activity, dummy for household-accessed credit, total livestock units, dummy for household ownership of a motorcycle and/or car, distance to output market and district dummies; & 3 <i>proxies for access to information:</i> "the number of mobile phones owned"; "extension contact" for whether a household accessed information from an extension service; and "group membership" for community learning about agricultural and market information.]</p> <p>[<i>Instruments:</i> innovative instruments: household-specific mobile phone network connectivity & the size of the information exchange network of the household. These instruments were created through interviews]</p> <p>[Robust standard errors]</p>	<p>for mobile money usage; OLS used for frequency of use and volumes transferred; (iii) <i>Food insecurity (binary measure)</i>: ordinary probit estimates used.</p> <p>It is possible that the instruments are weak: no critical values are reported e.g. for the Cragg-Donald Wald F statistic. For the reported IV result, the level of significance of M-money dummy is low. The first instrument entails ownership of a phone and proxies for wealth, which may affect food security. The second instrument may be correlated with other information controls in the regression, and may signal a household with good connections and high status, affecting food security. Failure to find appropriate instruments would not legitimate the OLS results.</p> <p>Cross-sectional analyses are highly vulnerable to failure to control for household and village level heterogeneity.</p> <p>Disentangle technology/service? Yes. A version of a mobile phone dummy is used.</p>	<p>significance) increase food expenditure per AE by 1.9 percentage points and by 1 percentage point, respectively. Farm equipment and livestock units, mobile phone ownership and household size (negative effect), are important co-variates.</p> <p><i>Continuous measure of food insecurity</i> Mobile money use and the volumes transferred (both with 1% significance) reduce food insecurity by 0.20 index points (1/5th of the standard deviation) and by 0.007 index points, respectively. Land size and ownership of a means of transport and livestock units are significant co-variates.</p> <p><i>Binary scale food insecurity</i> Mobile money use reduces the probability of food insecurity by 10 percentage points (10% significance). A one-unit increase in the volume of money transferred via mobile phone reduces the probability of food insecurity by 1.2 percentage points (5% significance). Land size, ownership of a means of transport, livestock units and group membership are significant co-variates.</p>
Munyegera and Matsumoto	<u>Uganda</u>	<u>Panel Difference-in-Differences (DD)</u>	There are issues with zeroes or	<i>Total and food, non-food and social</i>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
<p>(2016a)</p> <p>Dependent variables: <u>DD</u>: log of monthly real per capita household consumption: - Total consumption for a household at a particular location and time; - Disaggregated food, non-food and social expenditure (expenditure on ROSCAs, mutual funds, insurance and churches).</p> <p>(IN logs, not mentioned in article, but see footnote in text)</p> <p>Definition of M-money usage: Exact definition of “use” unclear.</p>	<p>Balanced panel of 838 households generated from the 3rd & 4th rounds of household and community surveys in Uganda, 2009 & 2012 (RePEAT) project.</p>	<p><u>regressions</u></p> <p>“Random” intervention: the introduction of mobile money services</p> <p>Controlling for: M-money dummy equal to 1 for households that used mobile money services and 0 otherwise; household fixed effects; location-by-time dummies; dummy for household mobile phone possession; and household characteristics.</p> <p>[Household characteristics: household size, log of value of assets and land endowments, age, gender and education level of the household.]</p> <p><u>Instrumental Variables</u></p> <p>Controlling for: <u>as above</u></p> <p>[Instrument for mobile money adoption at the household level: with log of the distance to nearest mobile money agent]</p> <p><u>Propensity score model</u></p> <p>Matched users and non-users with similar characteristics.</p> <p>[Robust but not clustered standard errors]</p>	<p>small numbers in the log specification, see text; this may account for the disaggregated results.</p> <p>Household fixed effects control for all time-invariant unobservables. Inclusion of location-by-time dummies averages over individuals within locations, and eliminates some (not all) unobserved, location-level, time-varying heterogeneity. Thus, time-varying unobservable household heterogeneity may remain.</p> <p>The specification requires agent roll-out to be random, which is questionable.</p> <p>The validity of the instrument relies on lack of systematic correlation between agent density and observable household characteristics that could affect household consumption (they refer to (do not report) only <i>bivariate</i> correlations). There may still be correlation with <i>unobservables</i> or poorly-measured observables (e.g. wealth) that may help households to smooth risk. F tests suggest instruments are not weak; no tests are reported for whether they are exogenous. They do successfully conduct placebo tests.</p> <p>The IV result (where the FE coefficient increases 4-fold) is</p>	<p><i>consumption</i></p> <p>FE model: given the adoption of mobile money services, there is a 9.5% (5% significance level) increase in total household per capita consumption; an insignificant coefficient for food consumption (most food is self-farmed); and greatly higher 20% increase for non-food and 47% increase for social expenditure (both at 5% significance level). IV model: total per capita consumption increases 4-fold upon adoption of mobile money (but with 17% standard error). Propensity score methods for comparable households recover a coefficient of around 7% (5% significance level) for overall consumption, but for food consumption are insignificant.</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
			<p>problematic. Propensity scoring was used, though too little information is given to assess this properly.</p> <p>Disentangle technology/service? Yes. Mobile phone dummy used.</p>	
<p>Sekabira & Qaim (2016)</p> <p>Dependent variables: <u>FE/RE</u>: outcome variables: - Total real household income (all net earnings from on-farm and off-farm sources, including remittances); - Per capita consumption; remittances received; - Proportion of coffee sold as shelled green beans allowing entry to higher-value markets; - Average coffee price received by farmers in the respective year.</p> <p>(inflation-adjusted income, see text footnote) (NOT IN logs)</p> <p>Definition of M-money usage: Households with at least one member who had a mobile money account and used services at least once in the previous year.</p>	<p><u>Uganda</u></p> <p>Unbalanced panel data from survey of smallholder coffee farmers; 2 randomly-selected robusta coffee-growing districts in Central Uganda [Round 1(2012) covered 419 households. Round 2 (2015) addressed a 6% attrition rate and also increased sample to 455 households. Unbalanced panel: 874 observations from 480 households. Mobile money questions only in 2015 Round]</p> <p>[Definitions: per capita value of food and non-food goods & services; food consumption data from 7-day recall; non-food items monthly; all expenditure data converted to daily basis. Off-farm income: salaries, wages & pensions of household, land rents and capital earnings, and net profit from non-</p>	<p><u>Panel fixed effects and random effects regressions</u></p> <p><i>Controlling for:</i> M-money dummy equal to 1 for households that used mobile money services and 0 otherwise; year dummy to control for time fixed effects; dummy for mobile phone use; dummy for participation in certification schemes for sustainability standards; and household/farm characteristics.</p> <p>(no location-by-time dummies)</p> <p>[Farm and household characteristics: education (years of schooling), age, and gender of the household head; land owned; value of other productive asset; distance to the next tarmac road; and a district dummy.]</p> <p>[Ordinary standard errors]</p>	<p>Consumption and income results are badly biased as they use inappropriate linear specifications, see text.</p> <p>Log specifications should have been tested for the remaining two dependent variables, but these regressions are at least interpretable, see RHS.</p> <p>Unbalanced panels may introduce biases.</p> <p>Time fixed effects are included; but location-by-time dummies should also have been included to address potential, unobserved, time-varying heterogeneity at the district level.</p> <p>Disentangle technology/service? Yes. Mobile phone dummy used.</p>	<p>This study aims to explore the role of agricultural marketing and off-farm economic activities to promote welfare.</p> <p><i>Household income and per capita consumption</i> We do not report the seriously biased consumption and income results.</p> <p><i>Valued added production and prices received</i> FE model: for mobile money users, the proportion of coffee sold as shelled beans increases by 19 percentage points (almost doubling), as less cash-constrained farmers are more willing to sell after drying and processing, and can transact with buyers from outside their location; mobile money users receive a 7% increase over the mean prices received by non-adopters through selling more of their coffee as shelled beans and having better access to buyers in higher-value markets.</p> <p>Important covariates in both cases are distance to road and sustainability certification, and additionally for coffee prices, productive assets (e.g. vehicles and transport equipment).</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
	agricultural businesses.]			
<p>Kikulwe et al. (2014)</p> <p>Dependent variables: <u>FE/RE</u>: outcome variables: - Total real household income (the sum of all net earnings from on-farm and off-farm sources, including remittances); - Remittances received (all transfers from relatives and friends not residing in the household); - Transactions in agricultural input and output markets; and farm profits. <i>(inflation-adjusted income, see text footnote)</i> <i>(NOT IN logs)</i></p> <p>Definition of M-money usage: Households that used mobile money services at least once in the previous year.</p>	<p><u>Kenya</u></p> <p>Balanced panel data for end-2009 and end-2010, focusing on 320 households from banana-growing villages in the Central and Eastern Provinces of Kenya.</p>	<p><u>Panel fixed effects and random effects regressions</u></p> <p><i>Controlling for:</i> M-money dummy equal to 1 for households that used mobile money services and 0 otherwise; year dummy to control for time fixed effects; and household/farm characteristics. <i>(no location-by-time dummies)</i></p> <p><i>[Farm and household characteristics:</i> farm size (land owned), household size, the gender, age, and education (years of schooling) of the household head, the distance of the household to markets and roads, a ‘high-potential area’ dummy, which takes a value of one for regions with more fertile soils and higher amounts of rainfall, and zero otherwise, and a variable measuring the percentage of households using mobile phones at the village level to capture neighbourhood effects.]</p> <p>[Ordinary standard errors]</p> <p><u>Instrumental Variables</u></p> <p><i>Controlling for:</i> <u>as above</u></p> <p><i>[Instrument for mobile money use at the household level:</i> with the proportion of households using mobile money and for those owning a mobile phone at the village level]</p> <p><u>Propensity score model</u></p> <p>Matched users and non-users with similar characteristics.</p>	<p>The results are badly biased as they use inappropriate linear specifications, see text.</p> <p>Not including a dummy for mobile money ownership means use of mobile money may be picking up this excluded factor.</p> <p>Location-by-time dummies should have been included to address potential, unobserved, time-varying heterogeneity at the village level.</p> <p>The wealth measure of land size is largely time-invariant over the short period of the study; a broader measure of less illiquid wealth is an essential control which could be time-variant over the sample.</p> <p>The exogeneity of the instruments with respect to income is in doubt, as they may proxy for wealth.</p> <p>Propensity scoring was used, though too little information is given to assess this properly.</p> <p>Disentangle technology/service? No.</p>	<p><i>Income, remittances, profits, inputs, marketed outputs and profits</i></p> <p>FE models: the results are seriously biased because of several model misspecifications.</p> <p>They suggest that mobile money users have greater household income, higher remittances received, to apply more purchased farm inputs, market a larger proportion of their output, and have higher profits than non-users of this technology. The reported average treatment effects are implausibly large, e.g. a 40% income gain relative to the mean income of non-users, and a 35% profits gain over non-users.</p>
<p>Kirui et al. (2013)</p> <p>Dependent variables: outcome & input variables: - Household agricultural input use (value of</p>	<p><u>Kenya</u></p> <p>Cross-sectional data, from a small survey of 379 multi-stage randomly selected farm households</p>	<p><u>Propensity score model</u></p> <p>Match treatment with controls (i.e., users of M-Money with non-users) that are similar in terms of their observable characteristics using 3 matching techniques. The differences in</p>	<p>Biases and heteroscedasticity as in the above two papers, as logs were not used for the unscaled dependent variables, and for the relevant unscaled independent variables. Thus, larger farms or</p>	<p><i>Income, inputs and commercialisation</i></p> <p>The results are biased because of model misspecification.</p> <p>Propensity Score methods: they find that mobile money transfer services significantly increased the level of annual</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
<p>purchased inputs); - Agricultural commercialisation (ratio of the value of sales to the value of total production); - Farm incomes (value of agricultural revenue). (<i>NOT IN logs</i>)</p> <p>Definition of M-money usage: Exact definition of “use” unclear.</p>	<p>in 3 provinces of Kenya in March-April, 2010.</p> <p>[<i>Definitions:</i> inputs included fertilizer, improved seed varieties, pesticides, and hired labour.]</p>	<p>outcome variables between the matches are averaged to obtain the average treatment effect on the treated.</p> <p>[<i>Matching characteristics:</i> gender, age, distance to nearest mobile money agent, distance to nearest bank, household size, asset endowment variables, household non-farm income, current value of assets, land size, education, group membership and regional dummies.]</p>	<p>wealthier households are given undue emphasis when taking arithmetic means. At the least, geometric means should have been checked for robustness.</p> <p>Propensity scoring: reduction of the bias by 20% does not eliminate it. Moreover, it is assumed that observed characteristics will be correlated with unobserved characteristics; this is not necessarily the case, and cannot be proved.</p> <p>The generalisability from such a small sample is also in doubt.</p> <p>Disentangle technology/service? No.</p>	<p>household input use by \$42, household agricultural commercialization by 37% and household annual income by \$224.</p>
Public/donor cash transfers using mobile money				
<p>Aker, Boumnijel, McClelland, and Tierney (2015)</p> <p>Dependent variables: <u>OLS:</u> various outcomes of interest (costs, uses of the cash transfer, food security and assets) of individual or household in village.</p> <p>Definition of M-money usage: Selected participants (see Col.4.) were given mobile money-enabled mobile phones.</p>	<p><u>Niger</u></p> <p>Cross-section or pooled cross-section. Household survey of 1,152 recipients in 96 intervention villages: baseline in May 2010, follow-ups in Dec.2010 and May 2011 (main sample: 1082 households in Rounds 2 & 3); village-level survey; anthropometric data on children, for 691 households in May 2011; weekly price data in 45 markets, May 2010 to Jan.2011.</p> <p>[Most regressions use the Dec.2010 household data, straight after the transfer.</p>	<p>Randomised Controlled Trials (RCT).</p> <p><i>Random intervention:</i> treated participants received cash transfer through mobile payments.</p> <p><u>Simple reduced form regression specification</u> variously comparing differences in outcomes for the 3 channels in Dec.2010 or May 2011, or for pooled data from Dec.2010 and May 2011 rounds.</p> <p><i>Controlling for:</i> indicator variables for participation in the M-money transfer program, and for whether a mobile phone was received; geographic fixed effects at the commune level; vector of household baseline covariates; presence of a seed distribution program at the village level.</p> <p>[<i>Household characteristics that differed at baseline:</i> age, raising livestock as an income source]</p>	<p>The first stage of selection may not be random, and there are other problems of potential heterogeneity (see Deaton’s critique, Box 2). They do, however, control for household characteristics that differed between groups at baseline.</p> <p>Cost-savings rely on a well-established agent infrastructure.</p> <p>The results may not be generalisable.</p> <p>Disentangle technology/service? Yes, 3 channels: manual; electronic plus mobile money-enabled mobile phone; & manual, plus mobile money-enabled mobile phone.</p>	<p><i>Various outcomes</i> Transactions costs reduced, especially travelling and queuing time. Increased intra-household bargaining power for women. Increased diet diversity; better nutrition for children; women more likely to cultivate and market cash crops; fewer depleted durable and non-durable assets. No evidence of “leakage”.</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
	When available, data for Dec.2010 & May 2011 are pooled and a linear time trend added.]	[Clustered standard errors]		
<i>Salary payments using mobile money</i>				
Blumenstock, Callen, Ghani and Koepke (2015b) Dependent variables: <u>FE:</u> Various outcomes of interest (saving, transfer and airtime purchase through M-Paisa, and welfare indicators such as consumption and self-reported happiness) of employees. Definition of M-money usage: Participants received mobile money-enabled mobile phones.	<u>Afghanistan</u> Panel data. Seven provinces, Jul. 2012 to April. 2013. Sample: 341 employees of Central Asia Development Group. Mobile operator Roshan transaction records, interviews, administrative records. Pre-baseline survey, baseline survey (before receipt of phones and training) and endline survey, and monthly phone surveys between the latter two.	Randomised Controlled Trials (RCT). <i>Random intervention:</i> treated participants received salaries through mobile payments. <u>Simple fixed effects regression specification</u> comparing outcomes in the endline and baseline rounds. <i>Controlling for:</i> indicator variables for a treated individual and for whether the observation was made after treatment, and the cross-effect of these two dummies; individual level fixed effects; survey wave fixed effects. <i>(No individual characteristics were included)</i> [Clustered standard errors]	The first stage of selection may not be random, and there are other problems of potential heterogeneity (see Deaton's critique, <u>Box 2</u>). No individual controls were included. But fixed effects and survey wave effects would help control for heterogeneity. The results may not be generalisable from this special group of individuals; the time period of observation is short and sample size is small. Disentangle technology/service? Yes. Mobile phones provided to both treatment and control groups.	<i>Effect of wages payment through M-Paisa on employer' costs, and savings behaviour of employees</i> Significantly reduced net costs for disbursing firm; larger and more frequent airtime purchases and more spent in total by recipients; increased usage of mobile transfers and mobile savings by recipients, but with usage patterns differing by prior banking status and size of salary. Greater liquidity preference and savings withdrawal with increased perceptions of physical insecurity. <i>Consumption/ self-reported happiness)</i> No significant result obtained.
<i>Saving</i>				
Munyegera and Matsumoto (2016b) Dependent variables: <u>Probit:</u> Zero-1 dummy: for reported savings, credit and remittances; <u>Tobit:</u> log of annual savings, credit or remittances; <u>OLS:</u> log of annual savings, credit or remittances.	<u>Uganda</u> Cross-section of 820 households interviewed in 2014 on financial access and usage; household characteristics for same HHs from 4th round of household survey in Uganda, 2012 (RePEAT) project.	<u>Probit regressions</u> <i>Controlling for:</i> M-money dummy equal to 1 if at least one household member "used" mobile money services and 0 otherwise; district dummies; and vector of household characteristics (household size, log of total asset value, age, gender and education (years of schooling) of household head, the log of distance to nearest mobile money agent). <u>Tobit regressions</u> <i>Controlling for:</i> the above, with additional characteristics (distance in logs to the nearest town not nearest mobile money agent; dummies	Two approaches address endogeneity: adding residual from a first stage Probit regression for adoption in regressions; and propensity score matching. Little is significant beside the usage dummy (see RHS). The authors suggest this is because heterogeneity has been successfully removed. However, in cross-section it is very difficult to control for unobserved heterogeneity.	The authors suggest a role for mobile money in encouraging savings and as a channel for loans and remittances. <i>Savings and credit</i> Probit models: yield no significant variables at the 1% significance level, save for the (positive) mobile money usage dummy. <i>The monthly flow of savings</i> Tobit models: yield no significant variables at the 1% significance level, save for the (positive) mobile money

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
<p>Definition of M-money usage: Exact definition of “use” unclear.</p>		<p>for a migrant worker in household and a SACCO in district; and a land wealth variable). <u>Variant regressions:</u> (i) the residual from a first stage Probit regression for mobile money adoption is added to help control for endogeneity of mobile money and the log value of land is added; and (ii) the distance to the nearest mobile money agent is used as an exogenous measure of mobile money access. <u>OLS regressions weighted by the propensity score</u></p> <p><i>Controlling for:</i> <u>as for Probit regressions</u>, plus additional characteristics (log value of land, log of distance to 3 other financial institutions and to district town).</p> <p>[Clustered standard errors]</p>	<p>Whether the significance of mobile money usage is indeed important or whether the coefficient is biased strongly upwards as it proxies for unobservables is unclear.</p> <p>Disentangle technology/service? No.</p>	<p>usage dummy. Partly controlling for the endogeneity of mobile money by adding the residual from a probit adoption regression: this is significant in the savings and credit regressions (the coefficient on mobile money usage remains stable). Assets promote savings and credit (10% significance level) in savings models without the residual; household size reduces savings (5% significance level).</p> <p>Propensity score matching models: nothing significant save for the (positive) mobile money usage dummy (coefficient on mobile money drops), and the value of assets (5% level) for savings.</p>
<p>Mbiti and Weil (2016)</p> <p>Dependent variable: <u>FE IV:</u> a set of outcome variables including saving <i>methods</i>.</p> <p>Definition of M-money usage: the proportion of individuals that use M-Pesa in a sub-location, but exact definition of “use” unclear.</p>	<p><u>Kenya</u></p> <p>Balanced panel of <i>locations</i> (note: not of households), from combining the 2006 and 2009 FinAccess surveys.</p> <p>[Wealth measure constructed with principal component analysis applied to household assets and durable goods; grouping respondents by wealth quintile.]</p>	<p><u>First differenced, fixed effects Instrumental Variables regression</u></p> <p><i>Controlling for:</i> a time fixed effect; a sub-location fixed effect; and vector of individual characteristics (education (level), gender, age, marriage rate and wealth (index and quantile dummies)).</p> <p>[<i>Instruments for M-Pesa usage:</i> 2006 perception responses (before introduction of M-Pesa) about riskier, slower and more costly transfer methods: the proportions of residents who identify the post office or a money transfer company or a friend as relatively more risky <i>than each other</i>]</p> <p>[Clustered standard errors]</p>	<p>Differenced specification removes biases due to time-invariant unobservables.</p> <p>The definition of the instruments is <i>not intuitive</i> (see text, <u>Section 4.6</u>). F tests suggest instruments are not weak; no tests are reported for whether they are exogenous. They do conduct some placebo tests. The instruments might be correlated with unobserved, time-varying characteristics of households that could be associated with the outcomes (e.g. ability, dynamism) and time-varying wealth if self-reported wealth is poorly measured and with (potentially) time-varying omitted variables like banking status.</p> <p>Disentangle technology/</p>	<p><i>Saving methods</i></p> <p>Effect of M-Pesa adoption is to reduce both the use of informal savings groups and having to hide cash in secret places.</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
			service? No.	
<p>Batista and Vicente (2016)</p> <p>Dependent variables: <u>OLS</u>: binary dummy variables: - willingness to save and remit to migrants in Maputo; - willingness to save and remit using M-kesh (mobile money).</p> <p>Definition of M-money usage: Treated individuals receive training about a new mobile money product, MKesh.</p>	<p><u>Mozambique</u></p> <p>Experimental data generated in rural provinces: Maputo-Province, Gaza, and Inhambane, March 2012 (102 rural Enumeration Areas: 51 locations in 3 regions randomly selected as treatment areas; the residual is control group). Administrative mobile money records combined with household survey data (3 years, 2012-14).</p> <p>[<i>Selection criteria</i>: rural treatment locations required mCel coverage & 1 or more commercial banks; targeted individuals required a mobile phone number and a migrant family member in Maputo with mobile phone number.]</p>	<p>Randomised Controlled Trials (RCT).</p> <p><i>Random intervention</i>: treated individuals receive training about a new mobile money product.</p> <p><u>Simple OLS reduced form regression specification</u> comparing differences in outcomes for targeted and control individuals for the years 2012, 2013, 2014 and for these years pooled.</p> <p><i>Controlling for</i>: treatment dummy variable; province dummies; year dummies; and individual controls for age and gender.</p> <p>[Clustered standard errors]</p>	<p>The first stage of selection may not be random, and there are other problems of potential heterogeneity (see Deaton's critique, <u>Box 2</u>). Other selection criteria (see LHS) <i>narrow</i> the type of population tested, which reduces the generalisability of results.</p> <p>There is a problem of interpreting a treatment effect when intervention depends also on the type of training information provided (see Aker and Blumenstock (2015)).</p> <p>The results may not be generalizable. Remittances flow in the unusual rural to urban direction. Sample size is small and quantities saved/remitted are tiny.</p> <p>Disentangle technology/ service? Yes. Only individuals with a phone number are selected.</p>	<p><i>Saving and remitting through M-Kesh</i></p> <p>Willingness to save and to remit through M-kesh increases for targeted individuals. The effect for savings is 23-25 percentage points and for remittances is 26-27 percentage points (both at 1% significance level). Dissemination of M-kesh raised willingness to send money transfers regardless of transfer method, and at the margin M-kesh substituted traditional methods of saving.</p>
<p>Demombynes and Thegeya (2012)</p> <p>Dependent variables: <u>Probit</u>: Zero-1 dummy: for reported general savings; & zero-1 dummy; for reported M-Kesho savings (savings account with interest accessed via phone for mobile money users); <u>OLS, IV</u>: log of average</p>	<p><u>Kenya</u></p> <p>Cross-section, survey conducted by the Financial Sector Deepening Kenya organization covering 6,083 individuals, during Oct.-Nov.2010.</p> <p>[Total savings: M-Pesa, MKESHO/PESA PAP, KCB connect, bank</p>	<p><u>Probit and IV Probit regressions for total savings & for M-Kesho savings</u></p> <p><i>Controlling for</i>: M-money dummy for M-Pesa registration or instrument; and vector of individual characteristics (gender, age, age squared, marriage, education (unclear how measured), location (rural/ urban), log of household income, and 4 wealth index quintiles).</p> <p><u>OLS & IV regressions</u></p> <p><i>Controlling for</i>: <u>as above</u>.</p>	<p>Instrumenting for the endogenous M-Pesa usage dummy with a <i>location</i>-level instrument in both types of regression averages over individuals within locations, and eliminates some but not all unobserved location-level heterogeneity. The results are suggestive only.</p> <p>There are no statistics examining the validity of the</p>	<p><i>Total savings and M-Kesho savings</i></p> <p>Probit models: savings in general more likely if older, male, married, living in rural areas, with higher levels of education, reported income and wealth; with these controls, M-Pesa users are 32% more likely to report savings (1% significance). (Few used M-Kesho, but the same outcome was reached: wealthier, married, more educated, and male.) Instrumenting for M-Pesa usage drops the coefficient to 20% (1% significance).</p>

<i>Study</i>	<i>Data</i>	<i>Method</i>	<i>Endogeneity & other issues</i>	<i>Claimed result</i>
<p>monthly savings.</p> <p>Definition of M-money usage: M-Pesa registrations from the telecommunications firm.</p>	<p>account, SACCO account, ASCA, ROSCA, Microfinance Institution and “other” means.]</p> <p>[Wealth index created using principal components analysis, grouping respondents by wealth quintile.]</p>	<p>[Instrument for M-Pesa registration: the fraction of respondents in the sub-location registered with M-Pesa.]</p> <p>[Ordinary standard errors]</p>	<p>instruments.</p> <p>Disentangle technology/ service? No.</p>	<p>Using OLS: M-Pesa users save 12% more than those un-registered (5% significance). Using IV: the coefficient for M-Pesa users is not statistically significant.</p>
Regulation and mobile money usage				
<p>Gutierrez and Singh (2013)</p> <p>Dependent variable: <u>Logit</u>: Zero-1 dummy: for whether an individual uses mobile money (receive, send or pay bills with mobile money or a combination of these)</p> <p>Definition of M-money usage: Households that used mobile money services at least once in the 12 months surveyed.</p>	<p><u>35 countries</u></p> <p>Cross-section, using the World Bank’s Global Findex survey (2011) usage micro-data; and constructed regulatory indices based on Porteous (2009), either equally-weighted or assigned weights through a Principal Components methodology.</p>	<p><u>Logit regression</u></p> <p><i>Controlling for:</i> country fixed effects; the interaction of regulatory indexes with individual characteristics; and vector of individual/country characteristics.</p> <p>[Note: the regulatory indexes themselves are not included]</p> <p>[Vector of individual characteristics: education (secondary schooling), gender, access to formal banking, age (and age squared) and income quintile). In some regressions, vector of country characteristics: log of GDP per capita, % unbanked population, % urban population, % population owning a mobile phone, concentration of banks, population density and total population.]</p> <p>[Ordinary standard errors]</p>	<p>The index is <i>de jure</i> rather than <i>de facto</i>. The index may be correlated with omitted country characteristics; most possible instruments for the index have the same potential problem. By using location fixed effects to reduce endogeneity, they are unable to include the index itself, but only its interaction with individual characteristics.</p> <p>Disentangle technology/ service? No.</p>	<p><i>Effect of regulation on mobile money usage</i></p> <p>The interaction effects suggest: a regulatory framework that supports interoperability promotes higher usage among the poorest; and stronger consumer protection reduces usage by the poorest (costs) but promotes usage amongst the educated.</p>

Sources: Constructed by the author from sourced papers in column 1.

Notes: 1. **Disentangle technology/ service?** Some RCT studies are able to disentangle the mobile money services delivery from ownership of a mobile phone by providing new phones to both treatment and control groups, or by considering only participants with a mobile phone number. Other studies achieve this by introducing a dummy for ownership of a mobile phone into regressions. 2. **Definition of M-money usage:** For the unwary, there are definitional ambiguities using both telecoms and self-reported data, see [Section 4.1](#). If individuals own multiple, valid SIM cards with different providers, or if there are inactive accounts, this will exaggerate users. If registered customers are inactive (and globally two thirds of registered accounts are inactive with a *generous* 90 day definition), this will exaggerate the participation. On the other hand, there is undercounting of overall usage where unregistered customers intensively use an over-the counter service, as in South Asia.