Migration as a Strategy for Household Finance: A Research Agenda on Remittances, Payments, and Development

Michael Clemens and Timothy Ogden

Abstract

It is time to fundamentally reframe the research agenda on remittances, payments, and development. We describe many of the research questions that now dominate the literature and why they lead us to uninformative answers. We propose reasons why these questions dominate, the most important of which is that researchers tend to view remittances as states do (as windfall income) rather than as families do (as returns on investment). Migration is, among other things, a strategy for financial management in poor households: location is an asset, migration an investment. This shift of perspective leads to much more fruitful research questions that have been relatively neglected. We suggest 12 such questions.

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"*When we run over libraries, persuaded of these principles, what havoc must we make?*" (Hume [1772] 2007, 144)

1 Introduction

In poor homes, income is volatile. Living on an *average* of two dollars a day means that some days there is more than two dollars, some days less, some days nothing at all. Poor households tend to develop complex, costly strategies to manage income—strategies that researchers are only beginning to understand (Collins et al. 2009).

The poor can manage volatile incomes in different ways. They can borrow from family and friends, but those people are often poor themselves. They can seek the steady pay of a formal wage job, but most of the poor cannot get one.[1] They can start a business, but that revenue can also be volatile and most microenterprises earn small or negative profits(e.g. de Mel et al. 2009). They can invest in other assets, like livestock or bank accounts (Barrett et al. 2006), but these can be costly and difficult (e.g. Narayan et al. 2000, p. 48).

There is a different tool that many households use, a tool not commonly seen as a financial strategy: migration. Migration usually means up-front costs with a stream of benefits that can be used to diversify risk. In this sense migration is every bit an investment and a tool for household financial management. Families across the developing world use it for that purpose (Banerjee and Duflo 2011, pp. 142, 231). But migration is not often studied as a substitute for, or complement to, other financial strategies.

This is an opportunity for new research. If we understand better the financial strategies that poor households do use, we might learn something about why they do *not* use

[1] Averaging across 40 developing countries analyzed by the ILO (2012), just 42% of jobs are formal.

other strategies. Major threads of development economics are occupied with understanding poor households' low take-up of objectively profitable investments (e.g. Duflo et al. 2008, 2011) and beneficial insurance products (e.g. Cole et al. 2013). This literature centers around testing whether poor households are making 'mistakes' or are extrinsically constrained from making good decisions. There is a third option—that they are making good decisions we do not understand, managing a portfolio more complex than we see.

What might economists learn from studying location as an asset, migration as a form of human capital, and remittances as a payout from that asset? In this note we argue that the mainstream research literature tends to treat remittances like a form of windfall income. This treatment entails one set of research questions. A literature that instead modeled migration as a household financial strategy would entail a different set of research questions, questions about which we know much less. We explain why several of the standard research questions about remittances are less fruitful, and suggest a list of 12 more fruitful questions. We start out by reconceiving migration as the purchase of an asset.

2 Migration is a form of human capital investment

We are used to thinking of human capital as synoymous with knowledge. But knowledge is only one form of human capital. Location, too, is a form of human capital. In common usage, "human capital" is a synonym for skill. But for economists, a costly change of location—migration—is in every way a form of human capital investment. Economists including Sjaastad (1962) and Schultz (1972, p. 4) have recognized this for some time.

What is the human capital of a Russian professional ballerina? Her human capital is much more than the classes she has taken. It is true that her earning potential is lower if she has not studied formal techniques like the Vaganova Training Syllabus (Kostrovitskaya 1995). But her earning potential is also lower if she has rarely performed publicly, if she knows no ballet directors, if she is obese, or if she lives in Krasnoyarsk, Siberia. That is, her income is directly affected by her knowledge, experience, connections, physical condition, and location—all traits of her person, and all changeable. She can improve all of these traits now, durably, and at a cost, to raise the value of her time and labor next year. Any traits like this are human capital. They include knowledge and formal training, but are not limited to it. One of the best investments she could make in her human capital would be to pay the cost of changing her location—to Novosibirsk or Moscow. In fact, without that investment in changing her location, her investments in other personal attributes might be nearly worthless.

Migrants bear large costs and risks up front for large gains tomorrow. The vast majority choose individually to pay this cost, without the direction of any state, in expectation of future benefits. This describes an entrepreneur and a stockholder just as it describes a migrant. In every sense, a costly change of location for future economic benefit is an investment (e.g. Burda 1995; de Haas 2010).

Migration is also the most profitable investment, by far, available to many of the world's poor. Moving to cities causes very large income gains for rural workers (e.g. Bryan et al. 2012). Workers who move from a poor country to a rich country can experience immediate, lasting, and very likely increases in earnings of hundreds of percent (Clemens et al. 2008; Gibson and McKenzie 2012), even for exactly the same tasks (Ashenfelter 2012). Having a member overseas typically causes large increases in the living standards of the origin household (Yang 2008; Gibson and McKenzie 2010; Clemens and Tiongson 2012).

This means that facilitating movement, remittances, and payments could have very large effects. No investment besides migration available to many of the world's poor can offer anything close to reliable returns in the hundreds of percent. Destination-country governments including Spain, Japan, and the Czech Republic have offered to pay immigrants cash to return home; very small percentages accept the offer McCabe et al. (2009). Their presence in a rich country is an asset that most value more highly than the few thousand dollars typically paid by such programs.

The principal constraints on this migration investment are not migrants' preferences, but external limits to acting on those preferences, limits beyond migrants' control. The largest are poverty and policy barriers. More than 40 percent of adults in the poorest quartile of countries tell pollsters that "in an ideal world" they would like to emigrate but cannot, and rich-country work visas available to low-skill, low-income workers are vastly oversubscribed (Clemens 2011).

Those who are able to migrate face a number of implicit taxes which effectively reduce the return on investment in migration. These include costs associated with finding work and costs of lower pay or poor working conditions imposed on migrants who are unable to access workplace protections. Perhaps the largest tax on migration investment returns is that imposed by intermediaries on remittances. Across the globe, intermediaries take 9% of remittances on average; remittances to Africa cost 12.4% (World Bank 2012; Ayana Aga et al. 2013). For a typical $200 payment, this means a charge of $18 on average and $25 for payments to Africa. Services like Obopay are now capable of sending any amount of money between mobile phones for 25 *cents*. But very few remittance receivers have access to these cheaper, newer payment methods, even though the majority of adults in many African countries now own a mobile phone.

Notwithstanding these limits to return-on-investment, a back of the envelope calculation suggests the colossal returns to facilitating migration and remittances. Remittances are pouring into the developing world—$401 billion in 2012, projected at $515 billion in 2015 (Ayana Aga et al. 2013). Noting as above that intermediaries now capture 9% of global remittances, this means that if remittance costs fell by half, over $23 billion in new finance would flow to developing countries. That's much more than the net disbursements of the entire World Bank in 2012 ($14.8 billion). It also means that if the stock of remittance-sending migrants were to increase just 20% from current levels, over $100 billion in new resources would flow to the developing world—much more than the entire G7 gave in bilateral aid in 2011 ($92.1 billion). And remittances, very much unlike World Bank or bilateral aid disbursements, typically go directly into the pockets of poor families. A recent literature suggests cash transfers have important effects on household-level development outcomes (e.g. Baird et al. 2011).

3 The mainstream research agenda

Migration, then, offers higher returns to many poor households than any alternative, and remittances are part of those returns. What questions are most researchers asking about the migration investment and its returns? Researchers typically focus on the potential for remittances to promote investment and financial development at the origin—questions that would be suitable if remittances were a form of windfall income, like lottery winnings.

Researchers interested in the effects of lottery winnings investigate what winners spend the money on, whether it harms them in unexpected ways, and how it affects their saving behavior (e.g. Lindahl 2005). Analogous questions dominate the research literature on remittances. We argue that once we see remittances as a return on investment, many of the traditional questions about remittances as windfall income start to seem irrelevant and inappropriate. Some of these traditional questions follow.

3.1 Do remittances cause investment?

Just as researchers have investigated whether windfall lottery winnings cause entrepreneurship (e.g. Lindh and Ohlsson 1996), they have extensively studied whether or not remittances cause new business formation and investment (Durand and Massey 1992; Basok 2000; Chami et al. 2005; VanWey 2005; Brown 2006; Osili 2007; Giuliano and Ruiz-Arranz 2009; Bjuggren et al. 2010; UNCTAD 2012). This includes some of our own work (e.g. Clemens and Tiongson 2012).

First, this question is much less interesting if we conceive of migration as itself an investment, and remittances as an important part of the return on that investment. If households are investing in migration, most of them have assessed the benefits and costs of that investment relative to other investments they could make—and determined that migration is superior to most other options. The surprise would be to see migrant households investing massively in assets other than the asset of having family members abroad, since they have already made big sacrifices to invest in migration, revealing that it is one of their best options—at least until the migration investment option is already exploited.

Much of the remittances-and-investment literature proceeds from a vague notion that households in migrant-origin countries are credit-constrained investors in new business, and migrant remittances will unleash a wave of entrepreneurship. But migration investments are extraordinarily costly. International transportation costs, visa costs, passport fees (McKenzie 2007), smuggling costs (e.g. Roberts et al. 2010), and foregone income add up to thousands or tens of thousands of dollars—that is, years of typical incomes in many origin countries. If households are not too credit-constrained to make enormous investments like this, it is unclear why we should expect them to be too credit constrained to start small businesses.

Second, there is extremely little evidence that remittances are less likely to be invested than any other kind of income. If we want to know why most remittances are not invested, we need to ask why most *income* is not invested, that is, why there are few investment opportunities at the origin. This question is more difficult but vastly more useful.

Indeed, remittances appear to be slightly *more* likely to be invested than other forms of income. A small subset of the remittances-and-investment literature directly compares the propensity to invest remittances to the propensity to invest other forms of income. The huge majority of these studies find that, all over the world, remittance income is more likely to be saved and invested in land, housing, and human capital than the same amount of income from participating in the origin-country's local labor market.[2] A

[2] In Pakistan, Alderman (1996) finds that households have a higher propensity to save from international remittances than from other income, while Adams (1998) finds they have a higher propensity to invest new income in productive assets like agricultural land if that income comes from external remittances. In Egypt, Adams (1991) finds that migrant households have a higher marginal propensity to invest new income than non-migrant households. Edwards and Ureta (2003) find that remittances cause more investment in schooling than other income in El Salvador, while Adams and Cuecuecha (2010) find that Guatemalan households spend remittance income on schooling and housing with a higher propensity than other income. Cardona Sosa and Medina (2006) find that households in Colombia have a higher propensity to spent remittance income on education than non-remittance income, but otherwise treat remittance and non-remittance income similarly. Davies et al. (2009) find that Malawian households'

relatively tiny literature finds the opposite.[3]

This evidence does suggest that migrants do not treat remittances as perfectly fungible with other income. In general, economists have found that households everywhere engage in 'mental accounting' and spend different types of income differently (e.g. Thaler 1985; Hastings and Shapiro 2012). Remittances are no exception. But given that the marginal propensity to consume from remittance income is systematically *lower* than for other income, across so many studies, we need to shift the emphasis of research. The question is not why remittances in particular do not get invested; they do get invested, more than other income. The more fruitful question is why *income* does not get invested. We will return to this later.

3.2 Do remittances cause 'dependency'?

Economists have studied the effect of windfall income on labor supply (e.g. Imbens et al. 2001; Henley 2004; Kimball and Shapiro 2008). Analogously, researchers have devoted great energy to determining whether remittances affect recipients' labor supply (e.g. Kozel and Alderman 1990; Görlich et al. 2007; Shonkwiler et al. 2008; Lokshin and Glinskaya 2009; Cox-Edwards and Rodríguez-Oreggia 2009; Jadotte 2009; Binzel and Assaad 2011; Antman 2012b; Powers and Wang 2012). Sometimes the literature describes this pejoratively as "remittance dependence".

The concern appears to be that households will become 'dependent' on remittance income, and that withdrawal from the labor force by remittance-receivers should there-

marginal propensity to spend remittances is higher for education than for any other category of expenditure. Taylor and Mora (2006) find that remittances to Mexico are associated with higher budget shares for investment (but lower shares for education). Airola (2007) finds that Mexican households' use of remittance income "favors goods that could be viewed as investments rather than consumption" such as housing and better health care. Castaldo and Reilly (2007) find that remittances to Albania "tend to increase a household's propensity to consume investment-type goods" including home improvements and durable goods. Zarate-Hoyos (2004) finds that remittance-receiving households devote a higher proportion of current expenditure to investment than other households.

[3]We are aware of only two studies finding that a marginal dollar in remittances is less likely to be invested than a marginal dollar of non-remittance income. In China and Tanzania, Christiaensen and Pan (2011) find that remittances are slightly more likely to be spent on consumption than investment in schooling. Clément (2011) finds that remittances to Tajikistan are associated with an increased expenditure share to consumption, at the expense of the investment share in expenditure.

fore be counted as a dangerous 'cost' of remittances. Some researchers even count the home-country job the migrant *would* have taken if migration were impossible as a 'cost' of migration. Fajnzylber and López (2007) report, "Some negative effects [of migration] include the potential losses of income associated with migrants' absence from their families and communities, since remittances are not exogenous transfers but a substitute for the home earnings they would have had if they had not left."

There is a subtle conceptual problem in that sentence. Most of the world's poor who wish to migrate to a rich country cannot do so; they face binding constraints that include credit constraints and visa barriers (Clemens 2011). Reducing those barriers would *strictly expand* poor households' choice set for financial management. If you are capable of choosing the best financial management tool for yourself, then there is no way we can impose "costs" on you by offering you an additional tool to choose from, say three rather than two. If people have the opportunity to choose migration as one more financial management tool, then that choice may have opportunity costs, but the opportunity to migrate—having the choice—cannot impose "costs" or "losses" or "negative effects" on households *relative to not having the choice*. Most poor households on earth do not have the choice.

Many poor households see migration in this light. Wouterse and Taylor (2008) find that in Burkina Faso, "[h]ouseholds with inter-continental migrants abandon or choose not to engage in activities that compete for household time while producing returns inferior to those from inter-continental migration." People fortunate enough to have access to a superior investment are, unsurprisingly, happy to become 'dependent' on its superior returns. In households everywhere, when one member gets a job opportunity or business opportunity that brings abundant income to the household, other household members do not need to make as many sacrifices to bring income to the household, and some choose not to work. But this is a sign of the success of economic development—allowing people freer choices about labor market participation and many other things—not a sign of failure. The pejorative use of 'dependency' in this context seems to arise from a mercantilist allergy to economic benefits that arise from interactions outside a country's borders, rather than from an objective assessment of what makes poor households better off.

Beyond this, there is little evidence of an important 'dependency' effect in the best-identified research on the effects of migration and remittances on origin-country households. Gibson et al. (2011) show how many studies may spuriously find such an effect: They show that after properly controlling for observed and unobserved differences between migrant and non-migrant households, differences in labor force participation by other family members tend to disappear. They use a naturally randomized visa lottery in the South Pacific to carefully identify these effects. Yang (2008, Table 6) uses exchange-rate shocks to carefully isolate the causal relationship between remittance receipts and hours worked by recipient household members: there is no change for wage work, and there is an *increase* for self-employment. Clemens and Tiongson (2012) use a sharp natural policy discontinuity to conduct similar tests for a set of migrant households in the Philippines; careful identification shows there is no discernible effect of migration and remittances on labor force participation by other household members.

This recent work raises the possibility that lower labor force participation in migrant households may reflect a simple correlation with the factors that motivate labor migration in the first place—a lack of attractive local job opportunities—rather than any systematic effect of migration on labor supply at the origin.

3.3 Do remittances cause financial development?

Economists have studied the effect of windfall income on savings and consumption decisions (e.g. Bodkin 1959; Imbens et al. 2001). If remittances are like windfall income and stimulate saving, this might spur demand for various financial products. And such financial development can have macroeconomic benefits (e.g. Levine 1997, 2005) that make life better and more secure for individual households (e.g. Karlan and Morduch 2009). This has led economists to study remittances as a way of spurring financial development and financial inclusion (e.g Pería et al. 2008; Gupta et al. 2009; Anzoategui et al. 2011).

What is missing from this research agenda is a broader view of the financial lives of poor households and the role that migration and remittances can play. Take, for example, the useful work of Anzoategui et al. (2011) in El Salvador. They find that remittances have only a mixed impact on "financial inclusion": remittances stimulate demand for savings accounts, but predictably they depress demand for formal credit. The motivation of this research is that "financial inclusion can have significant beneficial effects on households".

But migration to high-paying work *is* financial inclusion, in every sense. Migration is an investment in a high-yield asset. The returns to that asset, remittances, are a source of capital that competes directly with other sources of capital such as microcredit. If access to the migration asset depresses demand for other sources of capital such as formal credit within the country of origin, this means that most migrant households have determined that migration is a superior source of the capital they need for the purpose they envision.

In other words, remittances are a symptom of financial access. Rather than judging remittances on whether they promote the use of formal financial services, we should judge formal financial services by their inability to capture remittances. Formal financial services are tools to help households improve their financial management strategy, and migration is crucial to the financial management strategy of very large numbers of households.

3.4 Do households sacrifice too much for remittances?

Of course, patches of the migration literature recognize that remittances are not strictly windfall income and that the migration investment, like all investments, requires households to incur costs. This is where typically unspoken, implicit assumptions shape the questions that are asked. One researcher could assume that households are generally the best arbiter of what is good for them; this researcher would require extensive evidence of self-inflicted harm before believing that households are willing to incur costs that exceed benefits. Another researcher could assume that researchers are better able to determine what is good for households as a whole, perhaps because some household members are able to force costs onto other household members; this researcher would require evidence that households do not engage in self-inflicted harm before relaxing the suspicion that households may systematically harm themselves by investing in migration.

Many researchers have set out to test the degree to which migration by some household members harms other household members. Typically the subject of concern is children, with special attention to their school performance (Battistella and Conaco 1998; Nguyen et al. 2006; Antman 2011; McKenzie and Rapoport 2011; Cortés 2012; Antman 2012a,b; De Paoli and Mendola 2012), or occasionally, elderly relatives (Antman 2010a).

Labor migration is an investment in creating new job opportunities for children's parents, so this literature fits within a large, old literature exploring whether parents' labor supply decisions—particularly women's decisions—harm children. The social science literature of the 1940s and 50s is filled with hand-wringing about the effects of female labor force participation on "latch key" children, portentously predicting "a war-bred generation of problem adolescents-to-be in the 1950's and of maladjusted parents-to-be in the 1960's" (e.g. Zucker 1944). Since that time, a number of large-scale, long-term studies have found little or no lasting negative effects of women's work *per se* on their children, and that correlations between women's work and poor child development can be accounted for by properly controlling for confounding factors such as socioeconomic conditions of the household (Vandell and Ramanan 1991; Goldberg et al. 2008; Dunifon et al. 2013).[4]

Likewise, the true effects of migration and remittances on children are notoriously difficult to identify (e.g. Rapoport and Docquier 2006; McKenzie and Yang 2010). If a household has sufficiently poor employment prospects at the origin to motivate the great sacrifices usually required to migrate, such conditions are likely to go hand-in-hand

[4]A recent literature does find effects of a lack of child supervision on children's criminal behavior (Anderson et al. 2003) or obesity (Aizer 2004). But notably, these findings are no longer framed as identifying the effects of women's work *per se*. Income from women's work can purchase, among other things, child supervision by others. The findings from these studies would never be used in a policy forum to question whether or not it is good for women to have the opportunity to work.

with other conditions that lead to poor outcomes for children, including poor school performance. Isolating true causal relationships presents a major challenge, and some of the common techniques in the literature fail to convincingly address this problem. For example, when Antman (2010a) reports that Mexican parents with migrant children have poor health, how can we know whether confounding factors affect both migration decisions and paternal health? Instrumenting for migration with the fraction of those parents' children who are married (Antman 2010b) is not obviously helpful; children's marriage decisions are closely related to their labor market opportunities, which in turn reflect local economic conditions that could substantially influence parental health. The use of instrumental variables, no matter how creative, does not by itself transparently identify causal relationships. In the studies with the most transparent strategies for isolating the purely causal relationship between migration and family welfare find little sign of systematic negative effects (e.g. Gibson and McKenzie 2010; Gibson et al. 2011; Clemens and Tiongson 2012).

This issue is far from settled. But we put forward that it is not a fruitful area for much further research at this time. The reason we make that strong claim is best expressed by analogy to the "latch key" children discussed above. When a woman with children chooses to work, her earnings are her remittance to the household. What if our research agenda focused on whether or not that remittance compensates her children for the harm done to them? What if this research occurred in a context in which the vast majority of women who wished to work were prevented—by force—from legally entering the labor force? In such a strange context, our priors are that we should require extraordinarily definitive evidence before asserting superior knowledge to mothers' own knowledge regarding whether or not their work decisions are, on balance, good for their children. And before questioning women's decisions—when those decisions are tightly restricted—a better first step would be to understand why they are prevented by force from entering the labor force, whether or not that is a good idea, and how that might change. Only when women have the choice at all is it meaningful to study the consequences of their choice.

Likewise, and for the same reasons, the lore of systematic harm to children occasioned by migration should not be a priority area of new research. We say this because 1) there is no sound evidence that migrant households take decisions that systematically harm their own children to such a degree as to offset the benefits that remittances bring to those children, and 2) the large majority of people on earth who wish to make the migration investment (often for the sake of their children's well-being) are unable to do so, legally or at all. The binding constraint on the migration decision for most households is a lack of legal permission or a lack of money, both constraints to freedom. In this context, the effects of additional migration are not separable from the effects of increasing the freedom to choose whether or not to migrate. Restricting parents' freedoms in the name of their own children's welfare is only done, by civilized governments, in the presence of overwhelming evidence that their decisions cause great harm to their children.

3.5 Are remittances insurance?

Development researchers in particular have studied the ways that low-income families smooth income and consumption shocks (reviewed by Morduch 1995; Banerjee and Duflo 2007). They often do this by diversifying sources of income. Migration is one of many strategies that households use to diversify income streams, isolate income from correlated shocks, and cope with shocks (Chen et al. 2003; Giesbert 2007; Sakho-Jimbira and Bignebat 2007; Marchetta 2008; Shonchoy 2011).

It comes as little surprise, then, that remittances serve to insure households against negative income shocks at the origin (e.g. Agarwal and Horowitz 2002; Yang and Choi 2007; Mohapatra et al. 2012). The migration asset is serving the purpose that most valuable assets do: in addition to providing lucrative returns, it provides a cushion that can be drawn upon in lean times.

But why was this ever in doubt? Migrants from poor countries to rich countries can multiply their real earning potential by between 3 and 10 times (Clemens et al. 2008). When one member of a close family has an extremely high-paying job, regardless of its location, this obviously makes the rest of the family less vulnerable to economic vagaries. We are confident that anyone reading this paper can think of examples from their own experience of high-earners helping pay expenses for close family members who lost a job or had a health crisis. Remittances are just such an intrahousehold transfer. Economists have shown that the motives for sending remittances (Rapoport and Docquier 2006) are broadly similar to the motives for any other intrahousehold transfers (Laferrère and Wolff 2006).

We are not aware of a research literature that compares the insurance function of the migration asset to the insurance function served by other, equally valuable assets. Would we demand research to show that rural families with large land holdings are less vulnerable to shocks? This is quite obvious. Why, then, demand research showing that allowing families to invest in the migration asset makes them less vulnerable to shocks? Most whole-household migrants experience large income gains, and most partial-household migrants send home substantial remittances. Both of these result in more income. All income helps households self-insure, especially (in the case of partial-household migration) income that is not correlated with local shocks.

4 Why do less fruitful questions dominate the agenda?

We claim that the mainstream research agenda is shaped by how most people think about migration and remittances. Few consider migration a form of investment, or remittances a return on investment, though these are the roles migration and remittances play in the economic life of many migrants.

Why don't more researchers study migration as an investment? Possible reasons include:

- *Origin-country politics.* From the standpoint of a migrant-origin state, remittances are windfall income to the country, not a return on investment. A state benefits most directly from such a windfall by taxing it. But researchers interested in what makes *people* better off should give relatively less consideration to the incentives of states and more to the incentives of people.

- *Destination-country politics.* The principal limit on international migration is policy barriers erected by destination countries. Policymakers in those countries would face political problems in considering migration as the most profitable investment available to many of the world's poor. This would require them to contemplate and discuss the fact that their agents actively block many of the world's poor from the most profitable investment available to them, while the supremely valuable asset of permission to work in a rich country is allocated primarily by accident of birth. This would lead to many politically inconvenient conversations.
- *Paternalism in development policy.* It is common to think of remittances as a kind of aid, another source of development finance emanating from rich countries (critiqued in e.g. Wimaladharma et al. 2004; Chami et al. 2005; Kpodar and Le Goff 2011). Almost no foreign aid is given as direct cash transfers to the poor. Aid agencies generally believe that they can do more for development and poverty reduction than the poor themselves could if they controlled the money (Easterly 2002). This is closely analogous to the development policy debate between conditional and unconditional cash transfers (see e.g. Baird et al. 2011); aid agencies tend to mistrust the ability of poor households to spend money 'well'.[5]
- *Externalities.* Many development policymakers want to promote investment in poor countries because they believe it will have positive externalities—investing in a highway can induce investment by people who use the highway, investing in one child's education makes it more valuable for parents to invest in other children's education. Migration is a private decision that seems irrelevant, except to the extent that remittances can feed state efforts to invest with positive externalities. But the migration investment also brings positive externalities—including pecuniary externalities through remittance multiplier effects, bringing in new ideas about democracy (Spilimbergo 2009) and fertility (Beine et al. 2013), and transferring technologies (Kerr 2008). And even if it did not, the lack of positive externalities would be a reason not to *encourage* migration, but would not be

[5] For example: "[I]n-kind and voucher programs usually monitor the recipient's behavior to ensure that the intended consumption takes place and that the recipient does not trade the assistance to get other goods or services. The donors' dilemma is particularly evident in cases where the assistance is aiming for improved nutrition of the recipient population: If the recipients' taste for staples will lead them to buy less nutritious food if they get cash, or tobacco or alcohol, then the donor will get larger improvements in nutritional status with the same budget by giving them more nutritious food in-kind" (Villanger 2008).

a reason to actively *prevent* migration. Active prevention is the main constraint on international migration, not lack of encouragement.

- *Poor evidence.* The effects of state investments are visible: If the state had not built a dam, there would be no electricity. The effects of migration investment are much less visible: If a family had not been able to migrate, who can say what their economic lives would have been like? Measuring this effect is made more difficult by the self-selection of migrants (McKenzie and Yang 2010; McKenzie 2012). If a migrant household is worse off than a non-migrant household, is this because migration harmed the migrant household, or because that household's originally poor circumstances caused it to migrate? Researchers can rarely compare the correlates of migration with the effect of migration, when some unusual circumstance allows the effect of migration to be reliably measured. The two are often very different (McKenzie et al. 2010; Clemens and Tiongson 2012).

It is time to overcome these reasons, which have not served us well to guide the research agenda on remittances, payments, and development. Political discourses, especially, are not and should not be the touchstone for a fruitful research agenda in social science. Paternalism is best left outside centers of research. Externalities do deserve further exploration, but without massive and definitive evidence of harm, externalities should not be used to maintain forcible restrictions on human movement—as found today at many national borders. And as our evidence gets better—particularly at transparently distinguishing correlation from causation—we might likewise see our research priorities shift away from the old questions.

5 A more fruitful agenda

Many researchers are not asking the right questions about migration and remittances, questions that conceive of migration as a household financial strategy with vast potential. To make progress we need to be asking the right questions, questions that consider migration an investment, and remittances part of the return on investment.

Table 1: Different assumptions emphasize different questions

	If remittances are a…	
Topic:	*…windfall*	*…return on investment*
Investment	Do migrant families invest remittances?	How can more families invest in migration?
	How can policy get migrants to invest remittances?	How can policy reduce barriers to all kinds of investment?
	Do migrant families become dependent on remittances?	Can families now earn decent income without remittances?
	What do families sacrifice to engage in migration?	What do families sacrifice when they cannot migrate?
	What can be done so families need remittances less?	What limits the amount migrants remit to their families?
Payments	Do remittances cause financial development?	How can financial development facilitate remittances?
Taxation	What is the right tax on remittances?	What is the right subsidy/tax on the migration investment?
Insurance	Do remittances help insure against shocks?	How can more families use migration to recover from shocks?

A more useful research agenda would broadly focus on migration as the most lucrative investment available to many of the poor, it would proceed from greater trust in migrants' ability to decide what is best for them, it would explore payment systems that allow migrant households a higher return on their investment, it would take greater care in isolating causal relationships from correlations, and it would shift the burden of proof on externalities. That is, it would shift the burden from requiring proof that there are no negative externalities before today's tight coercive policy constraints on migration can be relaxed, to requiring proof that there are large and likely negative externalities before considering the maintenance of today's tremendous and rigid barriers to human movement.

Table 1 summarizes what this mental shift might mean for the research agenda on remittances, payments, and development. We discuss several questions that can serve as a starting point for this new research agenda below.

5.1 What causes *investment*?

There is no reliable evidence that poor-country households tend to invest remittance income less than they tend to invest *any* income (subsection 3.1). This suggests that social scientists have little to gain by asking why households do not invest more of their remittances in particular; they should be asking why households do not invest more in general—regardless of how that investment is financed.

Iskander (2005) describes the "dramatic failure" of the *Mi Comunidad* remittance-funded community investment scheme in Guanajuato, Mexico: it was designed to finance local low-end manufacturing, but there was no lucrative market for its products, no reliable supply of appropriately-skilled labor, and poor communications infrastructure. In other words, remittance-financed manufacturing investment in these *guanajuatense* communities was unprofitable—for exactly the same reasons that *any* manufacturing investment in those communities was unprofitable.

It should not surprise anyone that migrant households do not tend to invest most of their remittances. Overseas work is often attractive precisely to those households that *lack* profitable investment opportunities at home (Basok 2000; Clemens and Tiongson 2012). As Zarate-Hoyos (2004) puts it, "the lack of productive investment found in some field studies may be due to the particular circumstances of the regions or towns under study rather than to the characteristics of migrant households in general."

In other words, the principal way to encourage investment of remittances is to encourage investment of all kinds. Catrinescu et al. (2009) find that remittances promote economic growth to a greater degree in a "sound institutional environment", since "in the presence of good institutions, remittances could be channeled more efficiently, ultimately leading to higher output." Good institutions promote investment and growth.

This is true for most investment of most money, and there was never much reason to believe that remittances would be different. Social scientists interested in how to induce remittance-receiving households to invest in their home countries would do well to step back to the more fundamental and more fruitful question of why more investment of any kind is not going on in their home countries.

5.2 How can more people move?

Concurrently, that agenda would benefit from conceiving of migration as investment, and exploring the factors that limit poor people's ability to engage in that form of investment as well. There is little doubt that a major part of these barriers arises from policy (e.g. Clemens et al. 2008) and credit constraints (e.g. Hatton and Williamson 1998; Bazzi 2012). But few researchers on migration, remittances, and development focus on policy instruments that could reduce destination-country policy barriers to movement, or help households finance overseas work opportunities (McKenzie et al. 2013).

One fact is strangely absent from most research and policy discussions we have participated in over the past several years: that the most effective way to increase remittances is to increase migration. Making remittances cheaper cannot increase remittances by 100%; roughly speaking, only doubling migration can do that. As Brown (1997) observes:

> *"The design of policies to increase remittances either by increasing employment opportunities for migrants, or by encouraging them to remit more should be on policy-makers agendas in both the migrant-sending and OECD host and donor countries."*

The first and most powerful of these two options remains largely off the agenda of policymakers. It should rise near the top of researchers' agenda.

5.3 How much do migrants remit, anyway?

Anytime researchers begin to discuss remittances, an unwelcome elephant stands in the room: We have extremely poor information about exactly how much money people are sending. This uncomfortable issue was laid bare in a first-of-its-kind paper.

Kapur and Akee (2012) compare two independent sources of data on the exact same remittances flows: self-reported versus actual deposits to Non-Resident Indian bank accounts. They find that actual deposits per year are almost double self-reported deposits on average. They note that the opposite pattern is seen in comparing central bank data to self-reported remittances in nationally-representative survey data: total remittances as reported by the Reserve Bank of India in 2006–7 were *greater*—by a factor of eight—than aggregated self-reported remittances from the National Sample Survey.

Shonkwiler et al. (2008) discuss research approaches to the problem of large reporting errors in remittance flows. In our own work we have seen how sensitive self-reported remittances can be to fine points of survey design: If a migrant visits at Christmastime and buys the family a motorcycle, would they describe this to a survey enumerator as an "in-kind remittance", or neglect to mention it? How much of the capital brought home by migrants is "remittances" and how much "repatriated savings"? How much of this remains in foreign currency, accepted in many developing-country transactions such as real estate purchases, and remains invisible to the national accounts? The simple measurement of remittance flows remains understudied and a high-priority area for new research.

5.4 What are the effects (not just correlates) of migration (not just remittances)?

We stress again the need for remittances and migration research to more carefully distinguish between correlation and causation, transparently establishing the counterfactual scenario of outcomes without migration (*all else equal*) and comparing it to outcomes with migration. This is important in both theoretical and empirical senses.

In the theoretical sense, what are we to make of the finding of Marchetta (2008) that migration "is a coping strategy . . . unlikely to improve their socioeconomic condition in the long run"? The relevant counterfactual is not some other intervention that would permanently lift the poor of rural Ghana from poverty; no one has identified any such intervention. It is not that useful to assess whether or not migration "improves" outcomes relative to an unspecified and perhaps impossible counterfactual. Rather, the relevant counterfactual is the lives that the same households would have, all else equal, without migration. If migration is an important coping strategy for weathering economic shocks, and vulnerability to shocks is an important part of the the socioeconomic condition of the poor, then certainly migration "improves" those families' "socioeconomic condition" in the short and long run.

In the empirical sense, it is still common to read studies that explore correlates of remittance receipts and migration with various outcomes, and describe those correlations with inappropriately causal language. Borraz et al. (2008) claim to "examine the impact of migration on the happiness of the family left behind" by using propensity score matching to compare migrant and non-migrant families. Although propensity score matching cannot control for unobservable differences between migrant and non-migrant families, the paper nevertheless makes the strong conclusion that "the family left behind cannot be compensated for the increase in unhappiness . . . with remittances from abroad." Correlations are useful to report, and techniques like propensity score matching are helpful to a certain degree. But we now know that rigorously-identified experimental effects of migration can differ substantially from effects on on the exact same subjects that would have been measured by propensity score matching (McKenzie et al. 2010; Clemens and Tiongson 2012). Observational methods, by their nature, can only control for observable differences, and there are many theoretical and empirical reasons to believe that migrant households differ in myriad intangible ways from non-migrant households in ways unobserved by almost all datasets. This is not a reason to eschew observational studies; it is a reason to be careful what lessons we draw from them.

To single out another (nevertheless useful) study: Naufal and Vargas-Silva (2009) explore the correlates of remittances and fertility across countries, with a useful motivation: migration can affect origin-country fertility both by transferring norms and via the economic effects of remittances. But it is very difficult to tell whether the *correlates* of remittances are indeed "determinants" of fertility, as the study claims. Using instrumental variables like vaccination rates, as this study does, does not clearly identify causal mechanisms; many events or country characteristics could simultaneously affect fertility, remittances, and vaccination rates (such as a war).

Koser (2012) stresses the scarcity of careful impact evaluation in migration policy. This is especially needed in studies of migration: People tend to leave places where there is relatively little economic opportunity. That means that migration is very often associated in time and space with all of the things caused by lack of economic opportunity: unhappiness, unemployment, stress, debt, poor school outcomes, bad health, and so on. Separating the true effects of migration from that consistent association is a major problem for research in this area that must be taken more seriously.

5.5 What policies would make migration a more effective tool for families to manage their financial lives?

Many families are prevented from investing in migration, no matter how profitable, by liquidity constraints. Travel costs, recruiter fees, and other expenses can amount to multiples of annual income for the very poor. Martin (2009) presents a business plan for a proposed bank in Bangladesh to finance overseas migration, and describes limited pre-departure loan programs for overseas migration that have been attempted in Sri Lanka and the Philippines. No such bank has yet been created in Bangladesh. A fruitful agenda lies in studying how to overcome barriers of information asymmetry and regulation that could prevent migration banks from taking off.

More broadly, what are the policies that facilitate households' ability to use migration for financial management (Bryan et al. 2012)? What remittance products help households save more (Ashraf et al. 2011)? What is the potential for government policy—at the destination and origin—to make remittances cheaper and easier for households

(Gibson et al. 2007)? How do migrants' remittances respond when remittances get cheaper (Gibson et al. 2006; Aycinena et al. 2010)? How do different macroeconomic policies at migrant origins and at destinations affect how families use migration for financial management? The literature is relatively empty on these subjects, and there is an opportunity to contribute.

5.6 How does financial deepening shape migration?

If indeed migration is an important strategy for financial management in poor households, we would expect to see households treating migration as one asset in a larger portfolio. When other assets aren't available or do not give the returns or stability people want, we might expect them to invest in migration. Financial deepening increases the range of alternative tools households have for managing financial life. But the literature on the relationship between financial deepening and migration is dominated by studies of the other direction of causation: studies asking how migration affects people's use of other financial tools like bank accounts.

A more fruitful agenda lies in exploring how financial deepening gives households nonmigratory ways to manage their financial lives, affecting rates and types of migration. There is some work on how social protection programs affect migration: Hagen-Zanker and Himmelstine (2013) review research, much of it qualitative research from outside economics, suggesting that the availability of different tools for migrants to manage their financial lives affect households' propensity to migrate. These include including pension programs and social cash transfers. But this work could be extended to include households' whole financial portfolio. A useful research step would be modeling and then refining the use of various household financial strategies for the express goal of consumption smoothing and income smoothing. What factors make certain tools more or less useful for these goals? It is hard to imagine that it is harder for poor households to acquire formal savings, credit and insurance than it is to migrate. Therefore we need a model that helps us understand how much worse these tools must do the job at hand than migration does.

5.7 What might induce remitters to send more?

Once we think of remittances as the return on a costly investment, development researchers' priorities naturally shift towards ways to maximize those returns. Frontier research explores ways for poor-country households to get more remittances from the same migration investment.

A budding literature examines how the design of remittance products affects remittance flows. Yang (2011) encourages research on how remittance behavior is shaped by different remittance products and prices—particularly products designed to overcome information asymmetries between migrants and recipients (e.g. Poirine 1997). Ashraf et al. (2011) instantiate this agenda: They show that special remittances products, designed to give migrants greater control over how remittances are spent, can have large effects on migrants' willingness to remit.

Destination-country policy can also shape remittance flows by affecting the degree to which migrants are temporary or permanent, authorized or unauthorized, high-skill or low-skill. First, Dustmann and Mestres (2010) offer evidence that temporary migrants remit much more than permanent migrants. Second, Vaira-Lucero et al. (2012) find that unauthorized immigrants remit more than authorized immigrants. Note that both of these findings are consistent with an investment view of migration: shareholders in a firm would press for larger and more frequent dividends if they knew that they could only hold stock for a short time, or that it could be taken from them at any moment. Third, the latest research suggests that doubts about the relationship between migrant skill and remittances (Faini 2007) have been largely resolved: high-skill migrants, all else equal, generally appear to remit more (Bollard et al. 2011).

5.8 What financial payments mechanisms can make remittances inexpensive and safe?

There is no technological reason for 21st-century remitters to Africa to lose, on average, 12% of their money to intermediaries (World Bank 2012; Ayana Aga et al. 2013).

Financial institutions have created nearly costless remittance channels in numerous settings. An early example was the Banque Centrale Populaire de Maroc, which conveyed remittances almost costlessly from France to Casablanca starting in 1968 (Iskander 2010). A recent example is the partnership between ICICI Bank in India and Lloyds TSB Bank in the United Kingdom (Wimaladharma et al. 2004). This "India Banking Service" allows migrants to hold accounts in both countries and transfer money between them at almost no cost—currently a conversion charge of Rs25 or US$0.46 per remittance—which recipients can withdraw from a nationwide network of several thousand automatic tellers.

Mobile technology shows particular promise to create low-cost remittance channels. Digicel (2011) allows Pacific migrants in New Zealand to send remittances over their mobile phones for a low flat fee of NZ$3, that is, just 1–2% of a typical remittance. It has recently launched what it says is the first biometric system to secure international money transfers by mobile devices (PR Newswire 2012). There is no technological reason why such instruments should not be available in every corner of the earth, including the poorest parts of Africa. An impressive 45% of Africans now have a mobile phone subscription (Yonazi et al. 2012, p. 150).

The technology is here. But most remitters do not access anything like the best technology. Workers who want to send money home face a hydra of threats and obstacles: hidden charges and fees, slow fulfillment, language barriers, limited infrastructure for paypoints and transfers (both between and within countries), legal and regulatory barriers, coordination failures among institutional actors, lack of competitive market conditions, individual risk such as loss of funds in transit, and systemic risk such as money laundering (Orozco 2002; Cirasino and Hollanders 2007; Advisors 2012).

What forces, then, prevent new technologies from slicing through that bewildering and expensive thicket? This is less and less the domain of Silicon Valley and increasingly the domain of researchers in political economy. It is a frontier area of research. Jack and Suri (2011) study the economics of the leading mobile payment system in Africa, M-PESA, whose penetration is so high that Kendall et al. (2011) speak of a Kenyan "mobile money ecosystem". Vaughn et al. (2013) examine the inside story of M-PESA's quick rise to ubiquity in Kenya: financial regulators got out of the way, and the mobile carrier made key strategic and management decisions that allowed M-PESA to grow. More generally, the Better than Cash Alliance is exploring the nuts and bolts of the transition to all-digital payments systems, particularly in developing countries (Bankable Frontier Associates 2012).

The returns to better understanding are extremely high. Bettin et al. (2011) find that financial development in migrant-origin countries—such as increasing penetration of retail banking services—causes remitters to send larger amounts. Better infrastructure and more competition in the banking sector can mean lower costs to remit. And each one percentage-point decrease in the cost of remittances means increased financial flows to the developing world that rival those of the World Bank's IDA arm.[6] But we cannot be sure that lower remittance costs are the channel of this effect. Financial deepening might also raise the return to some kinds of investment at home, inducing migrants to send more money back.

5.9 What is the right subsidy or tax on remittances?

Developing countries routinely subsidize investment. Few subsidize migration. The research literature has focused the degree to which migration should be taxed, either by taxing movement itself (Wilson 2008, 2011) or remittances (Brown 2006).

This research has a poor grounding in public economics. Clemens (2011) discusses core arguments against taxing high-skill migration. One objection is Coasian: such taxes arbitrarily assign partial ownership of migrants' positive externalities to non-migrants (why not, then, do this for negative externalities as well—punishing non-migrants for any crimes that migrants commit?). A second objection is that such taxes ignore how massive quantitative restrictions on skilled workers' migration already con-

[6]Ayana Aga et al. (2013) project that annual global remittances to the developing world will pass US$500 billion by 2015. In fiscal year 2012, IDA's net disbursements were US$7.04 billion. That is, a change of just 1.4 percentage-points in the cost of remittances (as a fraction of the amount sent) would result in new financial flows to developing countries the size of all IDA disbursements. Beyond this, remittances typically go directly into the pockets of poor families, while IDA-financed expenditures do not.

stitute the equivalent of large migration taxes, just as tightly binding trade quotas affect traders equivalently to high tariffs.

A tax on remittances is, in global terms, equivalent to a tax on low-skill migration—just as taxing a worker for arriving at work is equivalent to taxing her earnings. Aycinena et al. (2010) find large effects of remittance pricing on remittance flows, some of it acting through substitution between different remittance channels. This suggests that remittance taxes could be a first-order determinant of remittance volumes. Iskander (2005) explores the experience of *Tres por Uno* (Three for One), a remittance subsidy scheme in Mexico.

It is fair to say that researchers are at the beginning of working out theoretically-grounded and empirically well-understood public finance policies toward remittances. The technical and pecuniary externalities of migration and remittances are so poorly understood—even theoretically—that it is difficult to reliably place a sign on them, to determine whether taxes or subsidies are most appropriate. Without better research, the only objective of remittance taxation is that of tapping a cash cow for state revenue, with potential deleterious effects. The interaction between formal (taxable) and informal (untaxable) remittance markets is likewise very poorly understood, though this interaction will shape market responses to all policy in this area.

The clearest *prima facie* case for migration subsidies is the aforementioned mounting evidence that credit constraints are a major barrier to migration by many families. Households can be impoverished by borrowing constraints or liquidity constraints that block them from the most profitable investment available. Yet somehow, few sending-country governments and essentially no destination-country aid agencies offer meaningful subsidies to the migration investment. Notable exceptions include the government of the Philippines, whose Philippine Overseas Employment Agency offers meaningful assistance to workers seeking employment overseas.

5.10 How do remittances affect economies?

The economic effect of a dollar remitted and locally spent—on domestically-produced goods and services—is not one dollar. If the provider of that good or service spends the money on something else, and so forth, the net economic effect can be substantially more than a dollar. Alternatively, if the remittance is saved (particularly offshore) or spent on imported goods, the net effect could in theory be less than a dollar. Remarkably, there is very little research on this multiplier.

A small industry within economics works to estimate Keynesian multipliers for fiscal policy (e.g. Woodford 2011; Chahrour et al. 2012) and for other economic changes (Moretti 2010). But very little such research has focused on remittances. A handful of dated studies have used assumption-laden models or input-output tables to guess at the economywide Keynesian multiplier associated with remittance receipts (Stahl and Habib 1989; Adelman and Taylor 1990; Nishat and Bilgrami 1991; Glytsos 1993), or suggestive cross-country regressions without transparent identification of the purely causal relationship between remittances and broader economic activity (Chami et al. 2005).

A frontier for new research is to creatively seek natural or contrived experiments that identify the broader economic effects of remittance receipts. Today, the most common shorthand indicator of the importance of remittances to developing countries is a comparison of the pure volume of flows to the size of GDP. This is often presented as a statement such as "remittances are 20% of Haiti's GDP", a problematic assertion in two senses. First, remittances can certainly affect GDP when they are locally spent, but remittances themselves are not part of GDP. Second, the effect of remittances on GDP is poorly understood and could well be much larger than such percentages suggest, via unknown multiplier effects. Much better research designs are needed here.

5.11 How do remittances compare with other cash?

A recent and growing research literature investigates the impact of cash transfers—from governments or other agencies—on poor households. Remittances are the original cash transfer, and it would be helpful to know if and how the incidence, uses, and effects differ. The big difference is that in almost all the cash transfer research we have, cash is strictly a windfall. Cash earned from migration, we have argued, is better modeled is a return on investment. We need to understand better how households spend the return on this investment, and how they spend it differently from the return on other investments.

For example, are remittances better or worse in targeting poor households than cash transfer programs or public subsidies to non-migration investments? Is the information asymmetry of the migrant larger or smaller than that of the government or aid agency? Are household effects changed by the presumably different social obligations to the remitter versus to government? Do governments and aid agencies sustain cash transfers or investment subsidies for longer or shorter periods than migrants do?

5.12 What changes when workers cross borders?

Both domestic and international migration can play a role in household financial management. But these roles differ, and how they differ is poorly understood.

Clearly there are differences: investing in international migration often has higher returns than investing in domestic migration, but comes with greater costs and risks. International remittances tend to dwarf domestic remittances, and the costs and ease of sending can differ as well. What are the relative magnitudes of these costs, risks, and returns? How do households use these two 'assets' in different ways, at different times? Where would investment to faciliate migration or lower the cost of remittances have the greatest impact? What are the interactions between these investments? For example, under what conditions are domestic and international migration complements or substitutes?

6 Conclusion

Policymakers are often associated with governments, and governments tend to see remittances as analogous to windfall income like foreign aid. Governments, after all, did not usually make the investment (migration) on which remittances are a return. Many researchers have followed this lead and studied remittances as windfall income. We have argued that this has led much of the literature on remittances—much of it written by brilliant people using modern tools—toward unfruitful questions.

Researchers, policymakers and practitioners would move toward more fruitful questions and potentially more useful innovations by first reconceiving migration as a household financial tool for achieving goals such as consumption and income smoothing (as well as economic advancement, of course); and second, viewing remittances as a return on the costly investment required to deploy migration as a financial tool. In this light, most of the interesting questions revolve around how to facilitate this investment and raise its returns. Ratha (2006) explicitly bucks the trend of viewing remittances as governments see them, and urges us to see them as migrant households see them:

> *"Remittances ... are personal flows from migrants to their friends and families. They should not be taxed or directed to specific development uses. Instead, the development community should make remittance services cheaper and more convenient ..."*

As the international migrant population continues to grow and remittances continue to rise, there will be greater demand for enlightening research on the important role of remittances and payment systems in economic development. The first and biggest step toward the research we need is to ask the right questions.

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