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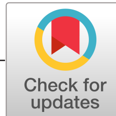

ARTICLE

Migration and household finances: How a different framing can improve thinking about migration

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Abstract

Motivation: It is time to reframe fundamentally the research agenda on migration, remittances, payments and development. Many policy-makers in the developing world, and researchers, tend to view migrant remittances as windfall income, rather than as returns on investment, which is how families with migrants tend see remittances. Migration is thus, among other things, a strategy for financial management in poor households: location is an asset, migration an investment.

Purpose: We propose that this shift of perspective on remittances—from windfall to return on investment—leads to more fruitful research questions.

Methods: Reviewing the literature, we summarize the research questions that have dominated the literature on migration and remittances and why, owing largely to unhelpful perspectives, they lead us to less informative answers.

Findings: We suggest 12 new and alternative research questions that have received relatively little attention in the literature on migration and remittances, to spur new, more productive research.

Policy Implications: Migrant remittances are now several times larger than foreign aid. The effects of migration regulation and financial regulation on remittances have directly and significantly affect poverty and human security worldwide. But some of the most basic questions about remittances and their effects remain inadequately answered, in part because of a blinded research agenda. Asking better questions is a step toward better policies, programmes and regulations and, above all, to enable people on low incomes to improve their lives.

KEYWORDS

development, finance, investment, migration, mobility, remittances

> "When we run over libraries, persuaded of these principles, what havoc must we make?" (Hume, 1772/2007, p. 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

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develop complex, costly strategies to manage income—strategies that researchers are only beginning to understand (Collins, Morduch, Rutherford, & Ruthven, 2009; Morduch & Schneider, 2017; Noggle, Gallagher, & Stuart, 2018; Rutherford & Chatterjee, 2019).

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 few are available.[1] They can start a business, but that revenue can also be volatile and most microenterprises earn small or negative profits (de Mel et al., 2009; Rutherford & Chatterjee, 2019; Breza & Kinnan, 2018; Hardy & Kagy, 2018). They can invest in other assets, like livestock or bank accounts (Barrett, Bellemare, & Osterloh, 2006), but these can be costly, difficult and risky (Narayan, Patel, Schafft, Rademacher, & Koch-Schulte, 2000; Dupas, Karlan, Robinson, & Ubfal, 2018).

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 over time, including income and risk diversification. In this sense, migration is an investment and a tool for household financial management. Families across the developing world use it for that purpose (Banerjee & Duflo, 2011; Clemens, 2018; DeParle, 2019; Lee, Morduch, Ravindran, Shonchoy, & Zamany, 2017).[2] 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 other strategies. Major threads of development economics are occupied with understanding poor households' low take-up of objectively profitable investments (Duflo, Kremer, & Robinson, 2008, 2011) and beneficial insurance products (Cole et al., 2013). This literature centres on testing whether poor households are making "mistakes" or are extrinsically constrained from making good decisions. There is a third option—that they are optimizing (or at least attempting to optimize) across factors 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 pay-out from that asset? In this article we argue that the mainstream research literature tends to treat remittances like a form of windfall income. This treatment entails a constrained set of research questions. A literature that instead modelled migration as a household financial strategy would generate a different set of research questions, questions about which we know much less. We explain why several of the standard research questions about migration and 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 synonymous with skill or knowledge. But knowledge/skill is only one form of human capital. Location, too, is a form of human capital. A costly change of location—migration—is also a form of human capital investment. Economists, including Sjaastad (1962) and Schultz (1972), have recognized this for some time.

What is the human capital of a Russian professional ballerina in Krasnoyarsk, Siberia? Her human capital is much more than the classes she has taken and skills she has developed. It is true that her earning potential is lower if she has not studied formal techniques such as the Vaganova Training Syllabus (Kostrovitskaya, 1995). But her earning potential is also lower if she has rarely performed publicly, if

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

[2]DeParle's *A Good Provider is One Who Leaves*, the story of one Filipino family's experience with migration across multiple generations, is a particularly useful exploration of the role of migration from a household perspective.

she knows no ballet directors, or if her muscles atrophy. That is, her income is directly affected not only by her knowledge and skill, but by her experience, connections and physical condition—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 labour next year. Any traits like this are human capital.

The ballerina's location, just like these other factors, is a changeable trait that affects her earning potential. One of the best investments she could make 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. Thus, no accounting of human capital is complete without adding location to the calculation.

Similarly, investors and entrepreneurs bear costs and risks up front for gains tomorrow. The vast majority choose to pay this cost, without the direction of any state, in expectation of future benefits. The very same description applies to a migrant. In every sense, a costly change of location for future economic benefit is an investment (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 (Bryan, Chowdhury, & Mobarak, 2014). Workers who move from a poor country to a rich country can experience immediate and lasting (very probably) increases in earnings of hundreds of percent (Clemens, Montenegro, & Pritchett, 2019; Gibson & 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 & McKenzie, 2010; Clemens & Tiongson, 2017; Lee et al., 2017) via remittances. The ability to receive remittances from a migrant (whether to a city within the country or across a border) also makes households more resilient to shocks and provides other benefits (Lee et al., 2017; Jack & Suri, 2014; Blumenstock, Eagle, & Fafchamps, 2016).

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, Lin, Tanaka, & Plewa, 2009). Similarly, policy to use aid to low-income countries as a deterrence to migration shows the opposite—that rising incomes in low-income countries or areas increases migration, probably because more people are able to afford the initial cost of the migration investment (Clemens & Postel, 2018). Their presence in a rich country is an asset that most value more highly than the few thousand dollars typically paid by such programmes. The non-income returns in terms of avoiding violence would add substantially to any complete accounting of the return on investment of migration (Clemens, 2017).

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% 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 6.84% of remittances on average; remittances to sub-Saharan Africa cost approximately 9% (World Bank, 2019a, 2019b). For a typical USD 200 payment, this means a charge of USD 14 on average and USD 18 for payments to sub-Saharan Africa.

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—USD 529 billion in 2018 (World Bank, 2019b). Noting as above that intermediaries now capture ~7% of global remittances, this means that, if remittance costs fell by half, over USD 23 billion in new finance would flow to developing countries. That is much more than the net disbursements of the entire World Bank in 2018 (USD 17.3 billion). And remittances, unlike World Bank or bilateral aid disbursements, typically go directly into the pockets of poor families. Recent literature suggests cash transfers have important effects on household-level development outcomes (Baird, McIntosh, & Özler, 2011; Banerjee et al., 2015; Haushofer & Shapiro, 2016; Bedoya, Coville, Haushofer, Isaqzadeh, & Shapiro, 2019).

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 behaviour (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 encourage entrepreneurship (Lindh & Ohlsson, 1996), they have extensively studied whether or not remittances cause new business formation and investment (Durand & Massey, 1992; Basok, 2000; Chami, Fullenkamp, & Jahjah, 2005; VanWey, 2005; Brown, 2006; Osili, 2007; Giuliano & Ruiz-Arranz, 2009; Bjuggren, Dzansi, & Shukur, 2010; UNCTAD, 2012). This includes some of our own work.

First, this question is much less interesting if we conceive of migration itself as 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 the superior option. 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 fully exploited.

It should not surprise anyone if migrant households do not tend to invest their remittances. Overseas work is often attractive precisely to those households that *lack* profitable investment opportunities at home (Basok, 2000; Clemens & Tiongson, 2017). 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."

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 (Roberts, Hanson, Cornwell, & Borger, 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 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 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. A notable 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 labour market.[3] Far fewer studies find the opposite.[4]

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 (Thaler, 1985; Hastings & Shapiro, 2013). 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, probably 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 labour supply (Imbens, Rubin, & Sacerdote, 2001; Henley, 2004; Kimball & Shapiro, 2008). Analogously, researchers have devoted great energy to determining whether remittances affect recipients' labour supply (Kozel & Alderman, 1990; Görlich, Omar Mahmoud, & Trebesch, 2007; Shonkwiler, Grigorian, Melkonyan, & de Vrijer, 2008; Lokshin & Glinskaya, 2009; Cox-Edwards & Rodríguez-Oreggia, 2009; Jadotte, 2009; Binzel &

[3]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 to a greater extent than other income. Cardona Sosa and Medina (2006) find that households in Colombia have a higher propensity to spend remittance income on education than non-remittance income, but otherwise treat remittance and non-remittance income similarly. Davies, Easaw, and Ghoshray (2009) find that Malawian households' 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 healthcare. 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.

[4]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 (2012) 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.

Assaad, 2011; Antman, 2013; Powers & 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 labour force by remittance-receivers should therefore 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, "The problem is that in many cases migration also entails potential losses of income associated with the migrants' absence from their families and communities. In other words, remittances are not exogenous transfers but rather they substitute for the home earnings that migrants would have had if they had not decided to leave their countries to work abroad."

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 practically insurmountable obstacles that include credit constraints and visa barriers (Clemens, 2011). Reducing those barriers would *strictly expand* poor households' choices for financial management. If you are capable of choosing the best financial management tool for yourself, then "costs" cannot be imposed 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 be simply declared to impose "costs" or "losses" or "negative effects" on households *relative to not having the choice*. Most poor households around the world do not have the choice. (To be sure, there must be some point at which having too many options does not enhance welfare; it is safe to say that millions of poor households across the world do not suffer from an excess of financial management options.)

Many poor households see migration in this light. Wouterse and Taylor (2008) find that in Burkina Faso, "[h]ouseholds with intercontinental migrants abandon or choose not to engage in activities that compete for household time while producing returns inferior to those from intercontinental 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 labour 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, McKenzie, and Stillman (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 labour 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 (2017) 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 labour force participation by other household members.

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

3.3 | Do remittances cause financial development?

Economists have studied the effect of windfall income on savings and consumption decisions (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 (Levine, 1997, 2005) that make life better and more secure for individual households (Karlan & Morduch, 2009). This has led economists to study remittances as a way of spurring financial development and financial inclusion (Martínez Pería, Mascaró, & Moizeszowicz, 2008; Gupta, Pattillo, & Wagh, 2009; Anzoategui, Demirgüç-Kunt, & Martínez Pería, 2013).

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. (2013) 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. 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. Recent work from East Africa on the effect of access to mobile money provides a stark illustration of the wrong-headedness of the common logic. Dupas and Robinson (2012) and Wieser, Bruhn, Kinzinger, Ruckteschler, and Heitmann (2019) find that women's access to transfers reduces the amount of risky transactional sex during income shocks. No one should decry the "loss" of access to income from sex work due to increased access to income from other sources.

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.

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. It is plausible that in low-income households in developing countries some household members are able to force costs onto other household members, yielding an overall net decrease in utility.

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 & Conaco, 1998; Nguyen, Yeoh, & Toyota, 2006; Antman,

2011, 2012, 2013; McKenzie & Rapoport, 2011; Cortes, 2015; De Paoli & Mendola, 2012) or, occasionally, elderly relatives (Antman, 2010a).

Labour migration is an investment in creating new job opportunities for children's parents, so this literature fits within a large, old body of literature exploring whether parents' labour-supply decisions—particularly women's decisions—harm children. The social science literature of the 1940s and 1950s is filled with hand-wringing about the effects of female labour force participation on "latchkey" 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" (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 & Ramanan, 1991; Goldberg, Prause, Lucas-Thompson, & Himsel, 2008; Dunifon, Hansen, Nicholson, & Nielsen, 2013).[5]

Similarly, the true effects of migration and remittances on children are notoriously difficult to identify (Rapoport & Docquier, 2006; McKenzie & Yang, 2012). If a household has sufficiently poor employment prospects in their current location to motivate the great sacrifices usually required to migrate, such conditions are likely to go hand-in-hand 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, 2010a, 2010b) is not obviously helpful; children's marriage decisions are closely related to their labour market opportunities, which in turn reflect local economic conditions that could substantially influence parental health. 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 (Gibson & McKenzie, 2010; Gibson et al., 2011; Clemens & Tiongson, 2017).

More comprehensively Clemens (2018) tests for evidence of repugnance— a repugnant transaction is one that offends some people, such as slavery, transactional sex, gambling or bribery—in migration transactions with South Asian migrant workers in the Gulf, an area with notoriously poor worker conditions and protections. Still, there is little evidence that migrants are engaging in transactions that cause them or others harm.

This issue is far from settled. But it is not a fruitful area for further research at this time. The reason we make that strong claim is best expressed by analogy to the "latchkey" 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 labour 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

[5]A recent study does find effects of a lack of child supervision on children's criminal behaviour (Anderson, Butcher, & Levine, 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.

are tightly restricted—a better first step would be to understand why they are prevented by force from entering the labour 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.

Similarly, 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: (a) 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 (b) the large majority of people around the world who wish to make the migration investment (often for the sake of their children's well-being) are unable to do so.

3.5 | Are remittances insurance?

Development researchers in particular have studied the ways that low-income families smooth income and consumption shocks (Morduch 1995; Banerjee & Duflo, 2007; Collins et al., 2009; Morduch & Schneider, 2017). 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, Chiang, & Leung, 2003; Giesbert, 2007; Sakho-Jimbira & 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 (Agarwal & Horowitz, 2002; Yang & Choi, 2007; Mohapatra, Joseph, & Ratha, 2012; Blumenstock et al., 2016; Jack & Suri, 2014; Lee et al., 2017). The migration asset is serving the purpose of most valuable assets: 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 a factor of between three and 10 (Clemens et al., 2019). When one member of a close family has a high-paying job, regardless of its location, this makes the rest of the family less vulnerable to economic vagaries. It is likely that readers of this article will have their own experience of high-earners helping to 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 & Docquier, 2006) are broadly similar to the motives for any other intrahousehold transfers (Laferrère & 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? 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 | 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 as part of the return on investment.

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 illustrates 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.

4.1 | 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 (see sub-section 3.1). This suggests that social scientists have little to gain by asking why households do not specifically invest more of their remittances; 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 labour 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.

The principal way to encourage investment of remittances would be to encourage investment of all kinds. Catrinescu Leon-Ledesma, Piracha, and Quillin (2009) find that remittances promote economic

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?

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.

4.2 | Who, within and across households, migrates?

Migration is a potentially lucrative investment, but clearly many people who would benefit financially from increased income by crossing a border do not do so. Some of that is due to legal and financial constraints, but clearly there are other factors also at play. As with the risky investment of starting a business, some people are more likely to make the investment in migration based on some combination of intrinsic qualities, motivation, existing stock of human capital, intrahousehold factors, context, etc. We know very little about the factors that make a difference, and even less about whether those factors affect outcomes.

Hicks, Kleemans, Li, and Miguel (2017) examine the much discussed productivity gap between urban and rural areas in Kenya and Indonesia, and find that those who migrate to cities tend to be more productive ex-ante, accounting for up to 80% of the productivity gap. Imbert and Papp (2018) find there are large non-monetary costs to migration from village to city in India, leading to differences in who seasonally migrates. Connor (2019) studies Irish historical mass migration to the US from 1850 to 1913, finding that illiterate and unskilled men were much more likely to migrate than those with more human capital, and that social networks were an important determinant of who migrated. That, however, was influenced by location: the parts of Ireland that had fewest and least attractive employment, and hence had the highest rates of emigration, were also those where illiteracy was highest.

Intrahousehold bargaining probably has a large effect on who migrates, but once migration happens it is likely to have a large effect on intrahousehold bargaining power, which in turn is liable to affect decisions about how much to remit and how income will be spent or invested in the future. While it is an area of interest to many researchers, there are still many questions about intrahousehold bargaining and spending, saving and investment decisions. For instance, Schaner (2015) explores how differences in intrahousehold preferences lead to inefficient savings decisions. Jakiela and Ozier (2016) find that women are willing to pay to hide investment returns from relatives. Meanwhile, Bernhardt, Field, Pande, and Rigol (2019) re-examine data on differences in returns to microenterprise investment between men and women and find the lower returns for women documented by de Mel, McKenzie, and Woodruff (2008) and Fafchamps, McKenzie, Quinn, and Woodruff (2014) are entirely explained by the presence of a male-owned enterprise in the household (which presumably receives the bulk of the investment).

Better understanding how the decision to migrate is made, who migrates, how that affects the return on investment to migration, and how migration affects decisions about how to consume, save and invest the returns on that investment are vital for migration and remittance policy decisions. Expanding existing work on intrahousehold bargaining to address migration and cross-border households is a ripe area for research.

4.3 | 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 (Clemens et al., 2019) and credit

constraints (Hatton & Williamson, 1998; Bazzi, 2013). 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, Beam, & Yang, 2016). There is only limited research on the costs of rural-to-urban migration within country and ways of addressing those costs (which would probably also be relevant to international migration) (Imbert & Papp, 2019; Bryan et al., 2014).

One fact is strangely absent from most research and policy discussions on remittances: 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 researchers much less policy-makers.

4.4 | How much do migrants remit, anyway?

Whenever 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 study.

Kapur and Akee (2012) compare two independent sources of data on precisely the same remittance 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 reverse pattern is seen when comparing central bank data to self-reported remittances in nationally representative survey data: total remittances as reported by the Reserve Bank of India in 2006–2007 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 Christmas 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?

Clemens and McKenzie (2018) find that for some major remittance-receiving countries, including Mexico, large recent increases in remittances differ vastly when measured by national accounts data compared to when measured by household survey data. This raises the possibility that measured remittances, particularly as they change over time, may contain large components of measurement error. The simple measurement of remittance flows remains understudied and a high-priority area for new research.

4.5 | 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 being 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 (from rural Ghana) "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 socioeconomic condition of the poor, then certainly migration "improves" those families' "socioeconomic condition" in the short and long run, as has been shown in research assessing how remittances reduce vulnerability to negative shocks, (Blumenstock et al., 2016; Jack & Suri, 2014)

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, Pozo, and Rossi (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 study nevertheless makes the strong conclusion that "the family left behind cannot be compensated for the increase in unhappiness … with remittances from abroad."[6] 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 the exact same subjects that would have been measured by propensity score matching (McKenzie, Stillman, & Gibson, 2010; Clemens & Tiongson, 2017). 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 about what lessons we draw from them.

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.

4.6 | 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 or credit 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 programmes for overseas migration that have been attempted in Sri Lanka and the Philippines. A fruitful agenda lies in studying how to overcome barriers of information asymmetry and regulation that could prevent migration banks from taking off.

[6]It is worth noting that if this statement was true it would invalidate tort law.

More broadly, what are the policies that facilitate households' ability to use migration for financial management (Bryan et al., 2014; Jack & Suri, 2014; Lee et al., 2017)? What remittance products might help households save more (Ashraf, Aycinena, Martínez A., & Yang, 2015; Somville & Vandewalle, 2018) or invest more (Ambler, Aycinena, & Yang, 2015)? What is the potential for government policy—at the destination and origin—to make remittances cheaper and easier for households (Gibson, Boe-Gibson, Rohorua, & McKenzie, 1997)? How do migrants' remittances respond when remittances get cheaper (Gibson, McKenzie, & Rohorua, 2006; Aycinena, Martinez A., & Yang, 2010; Ambler, Aycinena, & Yang, 2014; Jack & Suri, 2014)? How do different macroeconomic policies at migrant origins and at destinations affect how families use migration for financial management? The literature is only a start on these subjects, and there is an opportunity to contribute.

4.7 | 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 are not 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 that households have for managing their financial lives (financial inclusion is generally thought of as the extensive margin, while financial deepening is the intensive margin of access to finance). 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 non-migratory 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 pension programmes and social cash transfers. But this work could be extended to include households' whole financial portfolio. A useful research step would be modelling 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 clearly harder for members of poor households to migrate across borders than to open formal savings, credit or insurance accounts (and if there are places where it is easier to migrate than to open a formal account, those places are worth of study). Therefore, we need a model that helps us understand how much worse these tools must do the job at hand than migration does.

4.8 | What might induce remitters to send more?

Once we think of remittances as the return on a costly investment, development researchers' priorities naturally shift toward 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 behaviour is shaped by different remittance products and prices—particularly products designed to overcome information asymmetries between migrants and recipients (Poirine, 1997; Batista & Narciso, 2018). Ashraf et al. (2015) 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. Lee et al. (2017) examine how the ease and cost of urban-to-rural remittances in Bangladesh affect flows by

encouraging the uptake of mobile money. They find that simple training and encouragement materially increases (within country) remittance flows.

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, Nahm, and Tani (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, McKenzie, Morten, & Rapoport, 2011).

4.9 | What is the right subsidy or tax on remittances?

Developing countries routinely subsidize investment. Few subsidize migration. The research literature has focused on 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 that such taxes arbitrarily assign partial ownership of a person's endowments and investment in developing their human capital to others. The logic of such taxes implies that the talents and intelligence of, for instance, a Ugandan belong to other Ugandans rather than to her. If that is true then taxes for moving from one town to another or even from one job to another, taxes that most would reject as grossly unfair impositions on a person's liberty and autonomy, make just as much sense. A second objection is that such taxes ignore how massive quantitative restrictions on skilled workers' migration already constitute the equivalent of large migration taxes, just as tight trade quotas affect traders equivalently to high tariffs.

A tax on remittances is, in global terms, equivalent to a tax on low-skill wages—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 potentially 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 liquidity and 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.

4.10 | 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, 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 (Woodford, 2011; Chahrour, Schmitt-Grohé, & Uribe, 2012) and for other economic changes (Moretti, 2010). But very little related 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 & Habib, 1989; Adelman & Taylor, 1990; Nishat & 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 gross domestic product (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.

4.11 | 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 (Baird et al., 2011; Haushofer & Shapiro, 2016) with many more on the way as interest in Universal Basic Income expands. 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 modelled as 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 programmes 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?

4.12 | 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 facilitate 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?

5 | CONCLUSION

Policy-makers 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 toward unfruitful questions.

Researchers, policy-makers 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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