

## Conditional Cash Transfers and Migration: Reconciling Feminist Theoretical Approaches With the New Economics of Labor Migration

Christina Hughes

**ABSTRACT** In 2019, I published a study titled “Reexamining the Influence of Conditional Cash Transfers on Migration from a Gendered Lens,” to which Oded Stark has since issued a formal comment. This response has been written to address the major themes of Stark’s comment. While the first three sections focus on specific items related to framing, selection bias, and endogeneity, the fourth and final section tackles a more substantive theoretical debate between Stark and me over how to conceptualize the New Economics of Labor Migration framework in relation to gender. In my original paper, I argued that conditional cash transfers (CCTs) are gendered in their program conditions in ways that promote a normative gendered division of labor and that constrain beneficiary women from migrating. I note here that Stark’s primary issue with this point appears to be his contention that CCTs are not necessarily gendered but rather that women have a comparative advantage in completing housework and care work. My response first compares Stark’s argument to that made by Gary Becker in *A Treatise on the Family* and engages with the literature that has emerged to critique Becker’s own arguments regarding gendered comparative advantage. I then conclude my final section by offering some suggestions that might open a common theoretical path forward—one that insists on grounding microeconomic analyses of family behavior on assumptions that take gender and other aspects of culture and institutions seriously and one that also moves toward a bargaining model of microeconomic behavior rather than one that assumes consensus among all relevant actors.

**KEYWORDS** Gender • Family • Migration • Conditional cash transfers • New Economics of Labor Migration

In my recently published article, “Reexamining the Influence of Conditional Cash Transfers on Migration From a Gendered Lens” (Hughes 2019), I presented the case for integrating theories on gender, migration, and development studies to more deeply understand how conditional cash transfer programs (CCTs) influence behavioral outcomes among their recipients. At the household level, the study found that beneficiary households tended to see an *increase* in the likelihood of only men migrating as well as, crucially, a *decrease* in the likelihood that only women migrated (compared with nonbeneficiary households). At the individual level, I found that beneficiary women were *less* likely to migrate compared with nonbeneficiary women, whereas beneficiary men did not statistically differ from nonbeneficiary men in terms of their predicted probabilities of migration.

Here I issue a response to Stark's comment on the paper. From my understanding, Stark's comment can be divided into the following four overarching themes, which I will address individually in the sections that follow: (1) how migration and CCTs, as ends in and of themselves, are being framed, (2) how selectivity on intention to migrate influences CCT enrollment at all, (3) how the aforementioned selection on intention to migrate may result in an endogeneity issue, and (4) how the paper could benefit from an optimization framework that more thoroughly engages with the New Economics of Labor Migration (NELM) perspective.

## Framing Migration and Conditional Cash Transfers

At its inception, Stark's comment assumes that I am framing migration as an inherently "good" activity. He first suggests that I imply that "CCT impose a curb on an engagement that in the absence of the CCT would have been rewarding for women." It goes on to say that I view CCTs "as a form of compensation to women for sacrificing an attractive migration option" and that he "take[s] issue with the analysis that lies behind this claim."

My understanding from these remarks is that Stark takes issue with the presumptive claim I make regarding the gendered consequences of CCTs on curbing migration's inherently good payoffs. In essence, Stark believes that I argue that migrations are more or less good and that CCTs are more or less bad. This criticism of the moral dichotomy I supposedly apply to migration and CCTs is later used by Stark to make a larger point about how migration is just one part of a household's calculus to optimize the well-being of all its members. I address only the former part of his claim in this section and will elaborate on the latter part in the final section of this response.

I believe that the conclusion section of my paper in question contradicts the characterization that I believe migrations are "good" and that CCTs are "bad." The following excerpt features a discussion on the ambiguity of both migration and CCTs (Hughes 2019:1602):

Despite evidence that the maternalist structure of CCTs reduces the likelihood that beneficiary women migrate, *migration is a complicated process that runs the gamut from agency to coercion* [emphasis added]. In some circumstances, lowering the likelihood of migration may benefit women in their everyday lives given that at least some migrations are disruptive events that serve as last resorts in light of no other options. In other cases, incentivizing recipients with cash transfers and citing maternal duty in their service may remove women's options to move or work outside the home, which in their own right have been shown to increase women's autonomy. It is also entirely and simultaneously possible that CCTs grant women greater autonomy and power in other ways (Franzoni and Voorend 2012). Having more money and perhaps greater discretion over how CCT funds are spent can serve as a form of power for beneficiary women. One could argue that CCT disbursements at least partially compensate women for work that they would be expected to accomplish without pay anyway. As a result, *no value claims are being made in regard to whether migration or CCTs themselves are good or bad* [emphasis added], although maternalism itself should be interrogated more critically.

To that end, I would like to reiterate that I do not see migration as unequivocally good and rewarding for women, or that not migrating is a kind of sacrifice. I took no stance on how to morally calculate the consequences of CCT programs, although I noted the depth and contributions of the literature that has emerged to critique them. Instead, I treated both migration and CCTs as social phenomena structured by gender and explored how those interconnections can help us to more deeply understand how gender—as, in part, an ideology that reinscribes a gendered division of labor—configures bodies in space, within the home, and across national boundaries.

### Selectivity on Intention to Migrate

Stark next highlights a selectivity issue that may have influenced the study's results—namely, the possibility that “it is not that the receipt of CCT severs a migration option; it is that not contemplating migration favors accepting CCT.” In other words, Stark suggests that the results from the study are misinterpreted because of preexisting differences between the types of women who never intend to migrate and those who do in terms of their overall selection into CCT program enrollment. I agree that this is a possible alternative (or, at least, additional) explanation. There is always the chance that people make certain decisions because the alternative—in this case, the decision to migrate—was never something they entertained as a possibility or desire.

I find it unlikely, however, that there is total separation among the nationally representative sample of women in the data in terms of their predisposition to migrate that *overdetermines* their decision to enroll in a CCT program. Unfortunately, the data used in the analysis (i.e., the Mexican Family Life Survey) contain CCT participation information only during the first wave of data collection, thus creating time ordering issues in accounting for any selection into CCT program participation. However, the analyses did control for a number of theoretically important covariates also collected during Wave 1 in order to better isolate the effect of CCT participation on migration. Specifically, the individual-level analyses included a covariate that measured respondents' intentions to migrate in the future. The imputed data set shows that approximately 12% of women in the sample reported intending to migrate at Wave 1 despite reporting that they were also enrolled in Mexico's CCT program. Although this is just a cross-sectional tabulation, this divergence at the very least casts some doubt on the notion that women who never intend to migrate select into CCT program participation but women who plan to migrate at some point do not enroll. Ultimately, whether selection on intention to migrate is substantively and statistically significant enough to challenge the conclusions drawn from my study is an empirical question that the data cannot directly answer. I wholeheartedly support any future scholarship that seeks to address this or any of the study's limitations.

### Endogeneity of Intention to Migrate

By way of extension, the comment also levels that “if a household perspective is brought to bear, then a household's free choices weaken the appeal of migration by women . . . This reduction does not arise from an exogenously imposed curb

but rather from endogenously determined preferences.” Thus, Stark contends that migration holds no appeal for women enrolled in CCT programs because of their endogenous preferences to never migrate anyway. If that is the case, Stark says, this ultimately creates an endogeneity issue in the analysis. As I acknowledged in the previous section, I agree that prior intention to migrate may select some women into CCTs, which would then introduce potential endogeneity issues when this aspect has not been or cannot be included in the analytical strategy.

However, to make this particular point, Stark contends that “If Hughes’ variable “thought about moving” were to be considered as a proxy for migration preferences, then what we just noted could explain why the statistical significance of variables related to the CCT participation is considerably lower in the models that include this variable than in the models that do not.” I would like to respond to this point by first explaining that the analytical strategy of my study can be divided into two general parts: (1) a set of analyses conducted at the household level with household- and community-level covariates, and (2) a set of analyses conducted at the individual level with individual-, household-, and community-level covariates. Each set of analyses was conducted to speak to different aspects of the multipart hypothesis. Whereas the first set of analyses aimed to test whether men and women within the same beneficiary households were statistically dissimilar from one another in their migration likelihoods, the second set of analyses sought to test whether beneficiary women differed from nonbeneficiary women in their migration likelihoods.

As an individual-level covariate, “thought about moving” was included in only the latter set of analyses conducted at the individual level of observation. Moreover, the “thought about moving” covariate was included as part of the baseline specification for all the models included in this part of the analysis. Therefore, the estimated effects of CCT program participation on migration are all estimates derived from models that consistently took individuals’ intentions to migrate into account. This is why I take particular issue with Stark’s claims that “the statistical significance of variables related to the CCT participation is considerably lower in the models that include this variable than in the models that do not.” From this, I gather that Stark is comparing the overall statistical significance of the CCT variables from the *individual-level analyses* and claiming that the smaller significance levels seen there compared with the significance levels estimated for CCTs in the first set of analyses conducted at the *household level* provide evidence for his argument that CCT participation does not actually constrain women’s migration. Rather, it is simply that I did not control for the collective intentions of family members to migrate. This specific claim, then, assumes that one can simply compare the magnitude of effect sizes and significance levels across models that are specified and modeled differently and with mismatched units of analysis. This is not a statistically appropriate way to compare estimated coefficients or statistical significance. Therefore, although I agree with the possibility of selectivity and endogeneity, I argue that this point forwarded by Stark does not actually support the case he is trying to make.

## Engaging the New Economics of Labor Migration Perspective

Lastly, Stark suggests that the theoretical framework of this study should engage more deeply with the NELM perspective and with the household as a collective deci-

sion-making unit more broadly. Specifically, the comment states that “When a household that seeks to participate in migration is offered CCT, the household assesses the implications of receiving CCT for its options, opportunities, and well-being . . . . When CCT are made available, the calculus of who should migrate (if anyone), and who will not is revised: there is an income effect, and an assignment effect . . . . The assignment effect implies that if the wife is better than the husband in making productive use of the CCT, then the possibility of her migrating will be less attractive. If the household still favors migration (the income effect weakens but does not negate the desire to partake in migration), then the balance of selecting the household’s migrant-designate shifts in favor of the husband.” Hence, Stark suggests an alternative framework for understanding the results—one that views families as collaborative units within which family members coordinate whether anyone will migrate and who will migrate based on which option optimizes the family’s well-being. From this perspective, Stark maintains that a beneficiary woman is likely “better than the husband in making productive use of the CCT” and implies that this comparative advantage shifts the migration calculus to more readily assign men as migrants within beneficiary families.

I would like to respond first to the last point by asking Stark to clarify what he believes likely makes beneficiary women better than their male counterparts at making productive use of CCTs. CCTs generally require recipient households to regularly meet requirements aimed at improving the overall health and educational attainment of recipient families, with specific conditions and benefits aimed at children and, in some cases, seniors. These requirements can include a service component, such as attending and volunteering for public health workshops as well as the everyday management of household activities like sending children to school. From this, it could be gathered that CCTs are structured by a gender rationale that frames program requirements as feminized tasks. However, from Stark’s perspective, CCTs are neutral and do not impose any exogenous gendered restrictions on their recipients. It is, rather, that women are simply more advantaged in being able to meet the requirements built into the design of CCT programs. By using an optimization framework, Stark reasons that women’s comparative advantage with CCTs (and by implication, household management and care work) is what causes beneficiary men to be designated as migrants more often.

I argue that this point echoes an analogous logic famously forwarded by Gary Becker (1981) in *A Treatise on the Family*, in which Becker begins his theorization under the assumption that with even very small biological sex differences, the comparative advantage in cross-sex couples manifests itself so that it is almost always better for women to specialize in housework and for men to specialize in paid work in order to maximize household production. Becker himself wrote of his own skepticism toward any constructive role government programs can play in changing the labor dynamics among cross-sex couples, which I believe parallels Stark’s sentiment in the case of CCTs. Stark, too, appears to believe that CCTs play no constructive role in shaping household decisions or the assignment of tasks and instead argues that differences in husbands’ and wives’ comparative advantages in performing certain tasks are what account for differences in household specialization.

However, in contrast to Becker’s predictions, it would appear that married and cohabiting couples have become less specialized over time. In observing recent trends

in housework and labor force participation, scholars have found limited empirical support for Becker's theory and have consistently documented less specialization in paid work and housework among cross-sex couples, with greater participation in paid work among women and greater participation in housework among men (although total parity has not been observed) (e.g., Bianchi et al. 2012; Hook 2006; Raley et al. 2012; Ruppanner 2008). The implication of following this trajectory of *A Treatise on the Family* and its reception is that Becker's theory of gendered comparative advantage in paid work and housework has been shown to be insufficient, failing to account for why gender continues to matter even as specialization has become less and less of a feature in cross-sex couples' households. I suggest that it may follow that relying on a comparative advantage assumption that essentializes women's predisposition toward the housework and care work required by CCTs will present similar issues for Stark's comment, which is tempered by evidence from other studies that document shifts in women's increased labor force participation and migration for work in and from Mexico (Cerrutti and Massey 2001; Curran and Rivero-Fuentes 2003; Donato 1993; Fernández-Kelly 1983; Flippen and Parado 2015; Kossoudji and Ranney 1984; Parado and Flippen 2005).

Nonetheless, it appears that the major suggestion Stark forwards in his comment entails a recommendation that I engage further with the NELM perspective or substitute it entirely for the one offered in my own paper. As such, I would like to use the final portion of my response to contemplate this alternative framework. Initially formulated by Oded Stark and David E. Bloom (1985), the NELM perspective understands migration decisions as cost-benefit, optimization, and diversification calculations collectively made by members in any given household. The significant contribution of the NELM to demographic inquiries into migration lies in its prioritization of the household as a decision-making unit, which stood in contrast to dominant neoclassical perspectives at the time that focused on *individual* prospective migrants. I believe that this recognition of the embeddedness of individual behaviors within larger social formations like the family has been a crucial contribution of the NELM model to the literature. Many scholars continue to find explanatory power in its application to a number of migration contexts, which serves as a testament to its enduring utility (e.g., Chen et al. 2003; Lindstrom and Lauster 2001; Massey and Espinosa 1997). However, feminist and gender scholars have long critiqued the NELM perspective's failure to recognize how gendered norms and expectations create unequal power relations among family members that can make it difficult for women to migrate (Boyd and Grieco 2003; Chant and Radcliffe 1992; Curran et al. 2006; Kanaiaupuni 2000; Oishi 2005; Pedraza 1991). The NELM framework, although useful for understanding the microdynamics of decision-making within families, too simplistically frames migration as resulting from straightforward, optimized, microeconomic calculations rather than as decisions frequently negotiated within the context of and delimited by cultural, political, and institutional factors.

Interestingly, feminist and gender scholars in economics, demography, sociology, and other related disciplines have attempted to critique and somewhat recuperate the legacy and contributions of both Becker's work in *A Treatise on the Family* as well as the NELM approach (e.g., Dolfma and Hoppe 2003; Donath 2000; Ferber and Nelson 2009). To the extent that this paper can engage with the optimization and NELM approach as Stark suggests, looking to the writings of these scholars may open

a theoretical path forward. Frances Woolley (1996) published a comment to Barbara Bergmann's (1995) "Becker's Theory of the Family: Preposterous Conclusions," calling on feminists to reclaim the economics of the family and to revisit neoclassical, rational choice maximization models of human behavior. Robert Pollack (2003) went even further to suggest that the conclusions derived from Becker's analysis did not necessarily result from the application of neoclassical models on family behavior per se but from the oversimplified assumptions that Becker initially forwarded to ground his analysis. Instead, Pollack cited one of his own earlier articles that made the case for *more specificity* in formulating assumptions to ground rational choice models—assumptions that take culture and institutions seriously in how they can reconfigure and redefine preferences, opportunities, and constraints (Pollack and Watkins 1993). CCTs, from this view, can be understood as operating as part of a larger institutional and cultural apparatus that reconfigures the choice sets and meanings associated with various behaviors. Rather than arguing that CCTs are neutral and play no role in family migration decision-making because families are simply maximizing their well-being according to preexisting preferences, I contend that CCTs can be understood as gendered social policies that potentially reconfigure how beneficiaries create meaning around motherhood, work, and the family in ways that shift the underlying foundation that determines their preferences, opportunities, and constraints. Some qualitative research has indicated that beneficiaries do create new meanings around CCT receipt and understand their opportunities differently because of CCT program availability (Bradshaw and Viquez 2008; Holmes and Jones 2013; Molyneux and Thomson 2011; Piovani and Aydiner-Avsar 2015; Tabbush 2010). Recognizing this role played by CCTs can constitute a crucial, and most importantly, specific assumption (as Pollack recommended) made about the case to which one can then apply the optimization models of human behavior that Stark calls for.

Additionally, Pollack (2003) noted that this revision of microeconomic theories on the family has led to the development of bargaining models of collective behavior instead of consensus models. Although the NELM assumes that family members reach consensus on migration through collaboration and collective commitment to maximizing the family's well-being, this assumption of consensus can shift to a bargaining perspective in ways that open up other theoretical possibilities. Paul (2015), in research on women migrating from the Philippines, revised the NELM in her development of a "negotiated migration model" that is akin to this bargaining perspective. In response to the NELM's inability to account for how family members do not always agree on what they should do and who should do it, Paul instead demonstrated that women perform gender normatively as "dutiful daughters" to negotiate their gender responsibilities with other family members and to obtain support for their upcoming migrations. The women who sought to migrate, in other words, leveraged normative gender expectations to convince their families that allowing them to migrate would maximize the family's economic well-being. Paul's study demonstrates how theories of gender interaction and performance are not necessarily incompatible with a revised formulation of the NELM approach.

Overall, these modifications that call for *specific, contextualized assumptions* underlying optimization, behavioral choice models and for a bargaining (as opposed to consensus) approach to group decisions may bridge the gap between what I argue in my own paper and what Stark forwards in his comment. I would like to conclude by

saying that perhaps Stark's assessment reflects a true theoretical divergence between his work and mine. I, however, would like to think that I am writing in response to a long history of migration scholarship of which NELM *and* feminist theories of institutions and behavior are fundamentally a part. ■

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Christina Hughes  
[cmhughes@uw.edu](mailto:cmhughes@uw.edu)

Department of Sociology, University of Washington, Seattle, WA, USA