Bayesian MCMC as in LeSage Pace 2009 but they add fixed-effects in a panel data setting

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ORIGINAL ARTICLE

Spillover effects in adoption of cash transfer programs by Latin American countries

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Abstract

Some of the most effective public programs used in Latin America to reduce poverty and inequality have been non-contributory cash transfers. We examine country-specific characteristics that lead countries to adopt these programs over time using a state-transition spatial probit panel data model that takes into account dependence between countries' decision to adopt these programs. Intuitively, past adoption of cash transfer programs by other countries might have an impact on the probability that a country implements this type of program. We explore alternative connectivity structures to model dependence, spatial proximity as well as connections based on population migration flows, finding out-migration as most consistent with our sample data and spatial regression specification. For our panel of 17 Latin American countries over the period 2000–2017, we find evidence of dependence between countries in the probability of adoption of conditional cash transfer programs, but no such evidence in the case of unconditional cash transfer programs.

Keywords Conditional cash transfer programs · Government programs · Latin America · Cross-sectional dependence

JEL Classification I38 · N96 · C51 · R11

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1 Introduction

Latin American countries have been using non-contributive cash transfer programs as redistributive schemes to alleviate poverty and promote economic activity since the late 1990s. These programs mimic the *Prospera* initiative in Mexico and *Bolsa Familia* in Brazil. Some of these programs are conditioned on participation in human capital development efforts, like schooling and health care checkups, which we label conditional cash transfer programs (CCT), while others have no strings attached, simply transferring resources between segments of the population, which we label unconditional cash transfer programs (UCT). By 2013, CCT programs reached 135 million people in 17 Latin American and Caribbean countries, while UCT schemes benefited 17 million individuals in these countries. Beneficiaries accounted for approximately 90% of the poor in the case of CCT transfers—although these programs reached only half of the extremely poor.[1]

Figure 1 shows the percent of GDP devoted to cash transfer programs in our sample of 17 Latin American countries during the 2000–2017 time period. While only Brazil, Ecuador and Mexico had CCT programs in place in 2000, devoting an average of 0.29% of GDP in each country (which represented 0.05% of total regional GDP), the popularity of this type of programs led thirteen other countries to offer CCT transfers by 2017—devoting approximately $20.4 billion US dollars or almost 0.4% of regional GDP. Only four countries provided UCT transfers in 2000, accounting for almost 0.4% of regional GDP, but the growth of these programs has increased funding to $50.6 billion US dollars (almost 0.95% of regional GDP), when fourteen of the countries in our sample had these programs in place. Overall public transfers (UCT plus CCT) accounted for approximately 1.3% of regional GDP in 2017.

While there are no long-term studies of the influence these cash transfers can have on poverty and productivity (Robles et al. (2015) use nationally representative surveys for Latin American countries to show that coverage is not as optimal as thought, and that programs in place present significant leakages to the non-poor), evaluation of specific programs in some countries has provided substantial evidence of a beneficial impact, leading policy-makers and scholars to promote these programs because they seem to effectively reduce poverty and promote equality. Figure 2 shows poverty in the 17 Latin American countries of our sample based on various measures of poverty, including four international dollars per day, 2.50 international dollars per day and 1.90 international dollars per day. According to the 4 international dollars per day measure, poverty has declined from approximately 41% in 2000 to 20% in 2017 (lifting approximately 112 million people out of poverty). These programs aim to raise labor market skills and health of beneficiaries, leading to enhanced productivity, better paying and more secure jobs that move families out of poverty. The early success of such programs has led to the implementation of similar programs in more than sixty countries around the world.

[1] The distinction between poor and extremely poor is that those earning less than 2.5 international dollars a day are considered extremely poor, while those earning less than 4 international dollars a day are poor (experiencing moderate poverty).

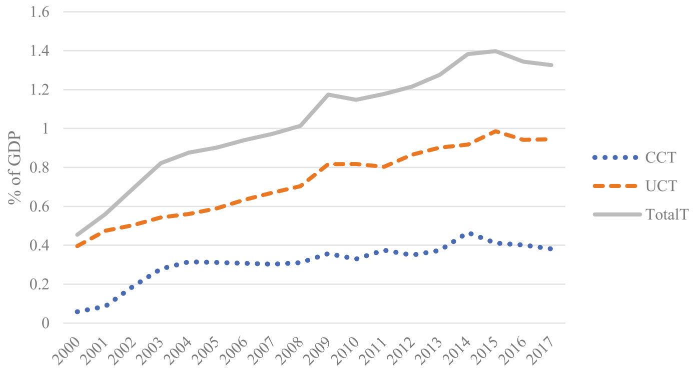


Fig. 1 Cash transfer programs as a percent of Gross Domestic Product (GDP) in Latin America. *CCT* conditional cash transfer programs, *UCT* unconditional cash transfer programs, *TotalT* total transfer

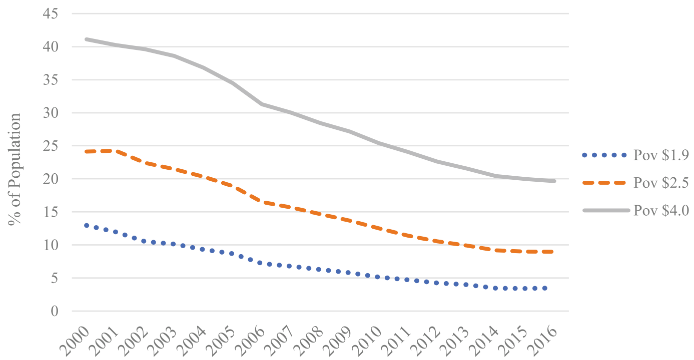


Fig. 2 Poverty rates in Latin America according to the 1.9, 2.5, and 4.0 international dollars per day measure

The literature provides evidence of program success—especially for the CCT programs which have been evaluated—in term of improvements in educational attainment, nutrition, consumption and labor market participation. Parker and Todd (2017) for example note that the *Oportunidades* program (renamed *Prospera* until it was discontinued in 2019) was able to reduce poverty while improving school attendance, grade progression, health care access, savings and even income. Schultz (2004) and Behrman et al. (2005) for their part find that the Mexican program has improved school enrollment and facilitated grade progression. Furthermore, Todd and Wolpin (2006) show that conditional cash transfers are significantly more efficient in improving schooling, relative to unconditional transfers. But cash transfer programs do not only affect education and health care access, they can also impact economic engagement of participating households. For example, Bianchi and Bobba (2013) examine the behavior of entrepreneurs in Mexico and find that recipients of these transfers increase risk taking because they provide a stable source of income.

Recipients of cash transfers showed a greater willingness to start self-employed ventures, increasing micro-entrepreneurship. In addition, Gertler et al. (2012) show that recipients of these monetary transfers were able to increase their long-term income and consequently raise their consumption levels (presumably by investing part of these transfers in productive initiatives). Indeed, Behrman et al. (2011) and Parker and Vogl (2018) find that *Oportunidades* raised education and labor force participation of females in the longer term.

Aizer et al. (2016) in an empirical study show that unconditional cash transfers in the USA between 1911 and 1935 produced long-term improvements in longevity, education, health and income. Other studies have used theoretical models to overcome data limitations and corroborate long-run beneficial impacts. For example, Cespedes (2014) found an increase in human capital and years of education using a simulation model for the Mexican program, reducing poverty and income inequality in the long run and fueling an economic expansion of approximately 6.5%. Peruffo and Cavalcanti Ferreira (2017) for their part calibrate their model to the Brazilian *Bolsa Familia* program and find that conditional cash transfers have a significant effect on increasing primary school educational attainment and reducing child labor in the long run, although it temporarily forces children to work more to become eligible to participate in the program. Human capital slowly builds over time leading to future increases in output. Vacaflores (2019) provides a model that allows increases in cash transfers to lower poverty rates and create economic growth if the program raises productivity by a large enough margin. His results are based on a model calibrated to the same 17 Latin American countries used in this study.

While these studies provide insight into the effectiveness of these cash transfers on schooling, nutrition, health care and productivity, very little is known about factors that lead to implementation of such programs. We hypothesize that countries would be more inclined to implement redistributive cash transfer programs when they experience high levels of poverty, or inequality, and when they have enough resources to support these redistribute schemes. Another hypothesis is that in the presence of cross-sectional dependence, effectiveness of such programs in one country should exert spillover impacts on the probability of implementation of such programs in other countries.

We contribute to the literature on cash transfers by exploring country-level characteristics associated with the probability of adoption of UCT and CCT programs. Our approach relies on a state-transition panel probit model that allows for cross-sectional dependence in decisions to adopt UCT and CCT programs by individual countries. This involves estimation of the model using state-transition behavior of 17 Latin American countries some of which have implemented these programs during the 2000 to 2017 period. By state-transition, we mean that countries adopting the program transition from a 0-state to a 1-state at time t when they adopt the program.[2] The sequence of 0,1 states for our panel of 17 countries over time represents the dependent variable that we model using a panel probit specification that allows

[2] Elhorst et al. (2017) propose this methodology and apply it to adoption of inflation targeting regimes by a sample of countries.

for cross-country dependence. The specification allows the 0,1 state of one country to depend on program implementation decisions made by other countries.

We explore alternative exogenous specifications for the connectivity structure that describes the dependence relationships. One specification defines the dependence of country i's decision to be based on decisions made by the set $j \in \mathcal{S}$ of geographically neighboring countries (those with common borders). Another defines decision dependence of country i as based on the set $j \in \mathcal{I}$ of countries that provide the largest number of population in-migrants, and a final specification defines country i's decision as dependent on the set $j \in \mathcal{O}$ of countries to which country i sends a large number of population out-migrants.[3]

We use data on conditional cash transfers and disbursements to non-contributory pension schemes gathered by the Economic Commission for Latin America and the Caribbean (ECLAC) to construct a cross-sectional panel of countries that covers the 2000–2017 time period, and we note these transfers do not include social security payments. We explore how country-specific characteristics impact the probability of CCT and UCT program adoption and test for the presence of significant spillover effects arising from dependence of program adoption decisions made by each country on a set of j other countries. Intuitively, factors such as the size and role of the government sector in the economy, the extent of population living in poverty, population size and levels of economic development would have an impact on the decision to adopt cash transfer programs. By spillover effects, we mean that changes in country i characteristics will have an impact on the probability that country i adopts the program as well as an impact on the probability that countries in the set j also adopt the program. Our model quantifies the magnitude of these spillover impacts resulting from dependence in program adoption decisions.

We find evidence of positive dependence of program adoption decisions for our sample of Latin American countries in the case of CCT programs, but no significant dependence in adoption decisions regarding the UCT programs. Positive dependence in the probability of CCT program adoption suggests that the presence of CCT programs in the set j of peer countries increases the probability of program adoption in the typical country i.[4] The lack of significant dependence of (the typical) country i decisions regarding adoption of UCT programs suggests that the presence or absence of UCT programs in the set j of other countries has no influence on the probability of adopting a UCT program.

Section 2 develops the panel probit state-transition model specification that allows for decision dependence regarding program adoption between countries. We provide a theoretical motivation for this model, as well as a motivation for three

[3] The set $\mathcal{S}$ contains N elements, one for each country indicating spatially neighboring countries. For example, $\mathcal{S}(1) = (2, 4, 6)$ would indicate that countries 2,4,6 are those that have common borders with country 1, $\mathcal{S}(2) = (4, 6)$ that countries 4 and 6 have common borders with country 2 and so on. The same applies to the sets $\mathcal{I}$, $\mathcal{O}$, which would contain N lists of countries that are the basis for in- and out-migration countries on which program adoption decisions are dependent for each of the N countries in our sample.

[4] Like all regression models, inferences from our model should be interpreted as reflecting an average across the sample of countries.

alternative dependence structures that exogenously specify the dependence sets $j \in \mathcal{S}, j \in \mathcal{I}$ and $j \in \mathcal{O}$. We also discuss log-probability and quadratic probability scores (LPS, QPS) that are used to determine which of the alternative dependence structures is most consistent with the model specification and sample data. Section 3 contains a description of Markov Chain Monte Carlo (MCMC) estimation of the state-transition panel probit model along with a discussion of how to interpret estimates and draw inferences for this model. Section 4 presents results from application of the model to the panel of 17 Latin American countries. Section 5 concludes.

2 A state-transition panel probit model for program adoption

LeSage and Pace (2009, Chapter 10) set forth a spatial Durbin model (SDM) variant of the conventional cross-sectional probit model that we can adapt to the (static) panel data case here. In (1), $y^\star$ represents an $NT \times 1$ vector reflecting the latent unobservable utility associated with each country i adopting a transfer program at time t, and $\otimes$ represents the Kronecker product. We let the matrix $W(j)$ denote an $N \times N$ matrix with nonzero values in the (i, k)th position for countries in the set j. For example, if $i = 1, j \in \Leftarrow S)$, then $k = 2, 4, 6$ for the example given in footnote #3. The matrices $W(j)$ are row-normalized to have row-sums of unity, and $\varepsilon \sim N(0, I_{NT})$.

$$y^\star = \rho\big(I_T \otimes W(j)\big)y^\star + X\beta + \big(I_T \otimes W(j)\big)X\theta + \big(\iota_T \otimes I_N\big)\mu_N + \big(I_T \otimes \iota_N\big)\mu_T + \varepsilon. \tag{1}$$

We use an $N \times T$ matrix Y with zero, one values to reflect the (observable) presence or absence of the transfer program in country $i = 1, \dots, N$ at time $t = 1, \dots, T$. The observable dependent variable in our model is then $y = \text{vec}(Y)$, an $NT \times 1$ vector containing zero, one values. The $NT \times k$ matrix X contains explanatory variables consisting of time-varying country-specific characteristics for each country at each time period. The scalar parameter ρ measures the strength of dependence, with a value of zero indicating independence. Clearly, a conventional panel probit model reflecting decisions that are independent emerges when $\rho = 0$. It should be noted that we have a static panel data model which means that the same dependence structure exists between countries for all time periods. The block diagonal matrix $(I_T \otimes W(j))$ does not allow for interaction between different time periods, only simultaneous dependence at each time period t. The estimate for the scalar parameter ρ reflects an average level of dependence over all countries and time periods, and the static model implies the same data generating process (DGP) operates for all time periods.

The model relationship also includes characteristics of the set j of countries on which decisions are dependent, denoted by the Kronecker matrix product involving the explanatory variables, $(I_T \otimes W(j))X$. These variables would reflect an average of characteristics for (say) each country's k spatially neighboring countries defined by the dependence structure $\mathcal{S}$. This set of explanatory variables can be thought of a reflecting the context in which policy-makers in each country operate. For example, country i may be a small/large population country with low/high income levels with a dependence set of spatially neighboring countries $\mathcal{S}(i)$ that are on average large/small population countries with high/low income levels. The model relationship allows for these contextual effects to explain variation in the utility of transfer program adoption.

The model also allows for an $N \times 1$ vector of country-specific fixed effects (μ_N) and a set of T time-specific effects μ_T.

2.1 A theoretical motivation for the state-transition panel probit model

The Bayesian approach to modeling binary limited dependent variables treats the binary 0,1 observations in y as indicators of latent, unobserved $y^\star$ (net) utility associated with the choice of adopting the transfer program, with the unobservable utility underlying the observed (0,1) pattern of program adoptions over time. For example, in our case where the binary dependent variable reflects the presence/absence of the transfer program, the decision to adopt the program would be made when the net (perceived) utility from having the transfer program ($y = 1$) versus not having the program ($y = 0$) is positive. LeSage and Pace (2009, Chapter 2) provide a number of theoretical econometric motivations for how situations arise where policy-makers in one country would exhibit utility that depends on that of policy-makers in other countries.

One of those scenarios is relevant here. If program adoption decisions by politicians are posited to be influenced by behavior of politicians located in other countries in the dependence set j countries in previous time periods, then we can formally express this type of utility dependence of $y_t^\star$ at time t on past period utility of politicians in the set of countries j as shown in (2). Note that politicians in country i at time t can observe the presence/absence of the transfer program in the dependence set of countries j during the previous period.

$$\begin{aligned}
y_t^\star &= G(j)y_{t-1}^\star + Z\delta + F\pi_t + \varepsilon_t, \\
G(j) &= \rho W(j), \\
Z &= \begin{pmatrix} X & W(j)X \end{pmatrix}, \\
\delta &= \begin{pmatrix} \beta & \theta \end{pmatrix}', \\
F &= \begin{pmatrix} I_N & \iota_N \end{pmatrix}, \\
\pi_t &= \begin{pmatrix} \mu_N & \mu_t \end{pmatrix}',
\end{aligned} \tag{2}$$

where we have assumed that underlying characteristics of the countries X remain relatively fixed over time or exhibit growth at a constant rate: $X_t = \phi^t X_0$, allowing us to write X without a time subscript. We do allow for country-specific fixed effects μ_N and a time-specific effect μ_t. Since variation in country-level characteristics such as poverty, population size, level of development and the relevance of the public sector in the economy—measures used in the matrices X_t of our model—changes slowly over time, this assumption seems reasonable. Of course, this assumption need only be approximately valid to justify the results that follow.[5] Systematic differences in

[5] In the online Appendix, we present a graph of the relatively stable variance of observations across time, which is consistent with this assumption. We also present estimates of the time-specific effects which are significantly different from zero for some time periods, suggesting sufficient variation to esti-

the levels of country-level characteristics are reflected by μ_N, and time-specific differences by μ_t. The assumption implies that country population or GDP growth (say, GDP per capita) is constant over time and dependent on initial period size.

The dynamic relationship in (2) implies a relationship for time $t-1$ shown in (3), which can be used to replace $y^\star_{t-1}$ in (2), resulting in the expressions in (4) and (5).

$$y^\star_{t-1} = G(j)y^\star_{t-2} + Z\delta + F\pi_{t-1} + \varepsilon_{t-1} \tag{3}$$

$$y^\star_t = Z\delta + F\pi_t + G(j)\big(Z\delta + F\pi_{t-1} + G(j)y^\star_{t-2} + \varepsilon_{t-1}\big) + \varepsilon_t \tag{4}$$

$$y^\star_t = Z\delta + F\pi_t + G(j)(Z\delta + F\pi_{t-1}) + G(j)^2 y^\star_{t-2} + G(j)\varepsilon_{t-1} + \varepsilon_t. \tag{5}$$

Recursive substitution of past values for the vector $y^\star_{t-r}$ on the right-hand size of (5) over q time periods leads to (6).

$$\begin{aligned} y^\star_t &= \big(I_N + G(j) + G(j)^2 + \cdots + G(j)^{q-1}\big)Z\delta + G(j)^q y^\star_{t-q} + u + v \\ u &= \varepsilon_t + G(j)\varepsilon_{t-1} + G(j)^2\varepsilon_{t-2} + \cdots + G(j)^{q-1}\varepsilon_{t-(q-1)} \\ v &= F\pi_t + G(j)F\pi_{t-1} + G(j)^2 F\pi_{t-2} + \cdots + G(j)^{q-1}F\pi_{t-(q-1)}. \end{aligned} \tag{6}$$

Expression (6) can be simplified by noting that $E(\varepsilon_{t-r}) = 0, r = 0, \ldots, q-1$, which implies that $E(u) = 0$. In addition, the magnitude of $G(j)^q y^\star_{t-q}$ becomes small for large q, given the stability restrictions for the dependence parameter $(-1 < \rho < 1)$ and the fact that the matrix $W(j)$ is row-stochastic (has row-sums of unity), since row-stochastic matrices have a principle eigenvalue of one. Also, $E(v) = (I_{NT} - \rho(I_T \otimes W(j)))^{-1}[(\iota_T \otimes I_N)\mu_N + (I_T \otimes \iota_N)\mu_T]$.

The implication of this development is that we can interpret the dependence that arises in the model for time t utility as the outcome or expectation of a long-run equilibrium or steady-state relationship, shown in (7).

$$\lim_{q\to\infty} E(y^\star) = \big(I_{NT} - \rho(I_T \otimes W(j))\big)^{-1}\big(Z\delta + (\iota_T \otimes I_N)\mu_N + (I_T \otimes \iota_N)\mu_T\big). \tag{7}$$

This is the expectation for the data generating process of the static panel data probit model given in (1).

2.2 A motivation for alternative dependence sets *W(j)*

Estimates from the model are conditional on the specific type of dependence set used to define the matrix $W(j)$ that determines the set of countries on which program

Footnote 5 (continued)

mate these fixed effects parameters. The online Appendix also shows a graph of the variance of observations across countries, which shows considerably more variation in this dimension of the data. Of course, our ability to estimate the model parameters $\delta = (\beta, \theta)'$ requires sufficient variation in X within the sample of N countries, not variation in X over time periods.

adoption decisions are dependent. We consider three alternative dependence sets $j \in \mathcal{S}, j \in \mathcal{I}, j \in \mathcal{O}$, each of which defines an alternative group of countries on which program adoption decisions depend.[6] Intuitively, this dependence is related to the way in which information propagates between countries. One hypothesis is that geographical proximity facilitates transfer of information, with information transfer decaying with distance between countries. We define the set of countries $j \in \mathcal{S}$ as those with common borders to each country i, reflecting the notion that information about program success in neighboring countries is more readily available to decision makers in country i. Another definition of the set $j \in \mathcal{I}$ was based on countries that provide a large proportion of in-migrants to each country i who bring information with them about the presence/absence and success/failure of cash transfer programs in their countries of origin. For example, in-migrants from countries with successful cash transfer programs such as Brazil or Mexico may have experienced the benefits of *Bolsa Familia* and *Prospera* and would share information regarding their value with residents of the destination country. The third definition of the set $j \in \mathcal{O}$ was based on countries to which a large proportion of out-migrants from each country i flow. Here, it could be the case that migrants transfer information about the presence/absence of social programs in their destination country back to families in their country of origin—migrants from Bolivia living in Brazil—could be sharing information about the *Bolsa Familia* program in Brazil with their families back in Bolivia.

To identify countries in the sets $j \in \mathcal{I}, j \in \mathcal{O}$, we rely on data from Trends in International Migrant Stock, from the United Nations, and use the average between 1990 and 2000 to address the fact that migration might depend on socioeconomic factors. These figures determine the relative importance of each country according to their migration patterns (migration flows have been relatively stable since the turn of the century for the countries of our sample).[7] The $N \times N$ weight matrices $W(j \in \mathcal{I})$ and $W(j \in \mathcal{O})$ were constructed based on the migrant stock from each other country in the sample with zeros on the main diagonal, reflecting migration from/toward all other countries. We assign zero value to row elements that represent less than 8% of the total in- or out-migration stock to produce a number of nonzero elements in the matrices $W(j \in \mathcal{I})$ and $W(j \in \mathcal{O})$ similar to the matrix $W(j \in \mathcal{S})$ based on spatial proximity (countries with common borders).[8] Specific weight matrix elements were then defined for each country of origin or destination based on the migration stock from these countries, and the weight matrices were re-normalized to have row-sums of unity.

[6] Recall that each country i exhibits dependence on different groups of spatial neighbors, in- or out-migration neighbors because the sets $\mathcal{S}, \mathcal{I}, \mathcal{O}$ contain N elements

[7] Venezuela is going through an intense migration process, but is not included in our sample.

[8] There were 49 nonzero elements in the matrix $W(j \in \mathcal{S})$ and 55 nonzero elements in the matrix $W(j \in \mathcal{I})$ defined using the stock of in-migration flows with the 8% cutoff. Without the cutoff, there were 148 nonzero elements in the matrix $W(j \in \mathcal{I})$. When connectedness is defined using out-migration, we ended up with 45 nonzero elements in the matrix $W(j \in \mathcal{O})$ with the 8% cutoff, down from the original 145 nonzero elements without the threshold.

Table 1 Log-probability and quadratic probability scores (LPS, QPS) results for alternative dependence set definitions

Weights	CCT		UCT	
	LPS	QPS	LPS	QPS
$W(j \in \mathcal{S})$	0.1129	0.0431	0.1479	0.0575
$W(j \in \mathcal{I})$	0.1144	0.0440	0.1305	0.0493
$W(j \in \mathcal{O})$	0.0920	0.0342	0.1298	0.0482

Notes: *CCT* conditional cash transfer, *UCT* unconditional cash transfer programs, *LPS* log-probability and *QPS* quadratic probability scores

Because migration exhibits a "gravity effect," migrants are deterred from moving to countries further away, so in many cases the source country of in-migrants would be immediately neighboring countries. This might also be true of destination countries for out-migrants. If this is the case, the three alternative weight matrices $W(j \in \mathcal{S}), W(j \in \mathcal{I}), W(j \in \mathcal{O})$ would exhibit a high degree of similarity. To measure similarity in the alternative dependence matrices, we use an approach set forth in LeSage and Pace (2014), who suggest using the correlation between the vectors: $(I_T \otimes W(j \in \mathcal{S})y, (I_T \otimes W(j \in \mathcal{I})y, (I_T \otimes W(j \in \mathcal{O})y$, where y is our $NT \times 1$ dependent variable vector containing 0,1 values. These correlations range from a low of 0.70 up to a high of 0.87 for both programs, so we should see relatively similar estimates and inferences from model specifications based on alternative definitions of the dependence sets.

To distinguish between performance of models based on these alternative dependence set definitions, we calculated log-probability and quadratic probability scores (LPS, QPS). These scores represent an analogue to mean absolute error and root mean squared error for situations where the observed outcomes are binary 0,1 and the model predictions are probabilities of the 0,1 outcomes. If we let $\hat{p}_{y_i=1}$ denote probability predictions from our model and y the observed 0,1 values, $\text{QPS} = (\sum_{i=1}^{NT} 2(\hat{p}_{y_i=1} - y_i)^2)/NT$, and $\text{LPS} = -(1/NT) \sum_{i=1}^{NT} [(1 - y_i) \log(1 - \hat{p}_{y_i=1}) + y_i \log(\hat{p}_{y_i=1})]$. The QPS ranges from 0 to 2, with QPS scores of 0 reflecting perfect accuracy, and LPS values closer to 0 reflect better accuracy.

Table 1 shows the LPS and QPS results for models involving both CCT and UCT program adoption decisions based on the three alternative definitions of countries that represent the dependence sets. From the results, we see that the definition of the set $j \in \mathcal{O}$ is most consistent with our model specification and sample data. The relatively small differences between scores reported in the table are consistent with the high correlation noted for spatial lag vectors based on alternative $W(j)$ matrices. Based on these results, we will report results in our empirical application based on the dependence set of countries defined by out-migration ties $W(j \in \mathcal{O})$ between our sample of 17 countries. This approach greatly simplifies presentation of results and allows us to use the matrix W to represent the weight matrix defined on the basis of $j \in \mathcal{O}$ in the sequel to simplify notation.[9]

3 Estimation and interpretation of the model

The Bayesian estimation approach to these models is to replace the unobserved latent utility with parameters that are estimated. For the case of our SDM probit model, given estimates of the $NT \times 1$ vector of missing or unobserved (parameter) values that we denote as $y^\star$, we can proceed to estimate the remaining model parameters β, ρ, θ by sampling from the same conditional distributions that are used in the continuous dependent variable Bayesian SDM models [see Chapters 5 and 10 in LeSage and Pace (2009)].

There is, however, the issue of fixed effects for both countries and time periods that arise in our panel probit model specification. We transform the explanatory variables to deviation from means form to conform with the probit assumption that $\sigma_\varepsilon^2 = 1$, which should eliminate country-specific fixed effects. Time-specific effects (dummies) were included in the model as additional explanatory variables, which we subsume in the matrix $X_0 = \begin{pmatrix} X & (I_T \otimes \iota_N)\mu_T \end{pmatrix}$. The matrix Z containing own-region explanatory variables, time dummies and dependence region explanatory variables consists of $Z = \begin{pmatrix} X_0 & (I_T \otimes W)X \end{pmatrix}$.[10]

More formally, the program adoption choice (at time t) depends on the difference in utility: $(\pi_{1i} - \pi_{0i}), i = 1, \dots, N$ associated with observed 0,1 program absence/presence indicators, where π_{1i} represents utility (of country i) associated with program adoption and π_{0i} that from not having the program. The probit model assumes this difference at each time period t, $y_{it}^* = \pi_{1it} - \pi_{0it}$, follows a normal distribution. We do not observe y_{it}^*, only the program adoption choices made, which are reflected in:

$$
\begin{aligned}
y_{it} &= 1, \quad \text{if} \quad y_{it}^* \geq 0 \\
y_{it} &= 0, \quad \text{if} \quad y_{it}^* < 0.
\end{aligned}
$$

If the vector of latent utilities y^* were known, we would also know y, which led Albert and Chib (1993) to conclude: $p(\beta, \rho, \theta | y^*) = p(\beta, \rho, \theta | y^*, y)$.[11] The insight here is that if we view y^* as an additional set of parameters to be estimated, then the (joint) conditional posterior distribution for the model parameters β, ρ, θ (conditioning on both y^*, y) takes the same form as a Bayesian regression problem involving a continuous dependent variable rather than the problem involving the discrete-valued vector y. This approach was used by LeSage and Pace (2009, Chapter 10) to implement a Bayesian Markov Chain Monte Carlo estimation procedure for the SDM

9 The full results for the three different dependence set definitions are available on the journal webpage.

10 Of course, we do not want to transform the dependent variable vector that consists of 0,1 values.

11 Of course, Albert and Chib (1993) did not deal with the case of spatial dependence, so $\rho = 0$ in their independent probit model.

probit model. We rely on this approach to estimate the parameters of our static panel data probit model.

Interpreting the way in which changes in the explanatory variables in the matrix X impact the probability of a country choosing to adopt the cash transfer program in the SDM probit models requires some care.[12] Lacombe and LeSage (2018) argue that the DGP for this model in expression (7) results in a nonlinear relationship between the probability of program adoption involving the multivariate probability rule involving changes in the explanatory variables (as in conventional non-spatial probit) as well as the term $(I_{NT} - \rho I_T \otimes W)^{-1}$. They advocate the need to explore observation-level variation in the partial derivatives showing how changes in own- and connected-country characteristics impact the probability of transfer program adoption. In our application, we show that there is no statistically significant country-level variation in the partial derivatives, allowing us to extend the approach of LeSage and Pace (2009) to our panel probit model. This involves use of an average of the diagonal elements from the $NT \times NT$ matrix: $\partial \text{Prob}(y = 1)/\partial x'_v$ to produce a scalar summary of the direct effects, which are derived from the own-partial derivatives: $\partial \text{Prob}(y_i = 1)/\partial x_{v,i}$. Similarly, we can use an average of the (cumulated) off-diagonal elements from the $NT \times NT$ matrix: $\partial \text{Prob}(y = 1)/\partial x'_v$ $(i \neq k)$ to produce a scalar summary of the (cumulative) indirect effects associated with the cross-partial derivatives: $\partial \text{Prob}(y_i = 1)/\partial x_{v,k}$. This scalar summary measure cumulates the spillovers falling on counties in the dependence set of country i as well as countries in the dependence sets of those countries in the dependence set of the countries in the dependence set and so on.

When we allow for dependence among observations/countries, changes in the explanatory variables associated with one country, say poverty in county i, will influence the dependent variable value reflecting program adoption in county i as well as other counties. For the case of decision dependence, the (nonzero) cross-partial derivatives represent what are commonly thought of as spillover impacts. Changes in the value of an explanatory variable in a single observation/country i can (potentially) influence all $N - 1$ other observations/countries. This is true for all $i = 1, \ldots, N$ values of the vth explanatory variable leading to an $N \times N$ matrix of own- and cross-partial derivatives. We note that given the block diagonal nature of the matrix $I_T \otimes W$, impacts are not transmitted to countries in other time periods, resulting in an $N \times N$ matrix of own- and cross-partial derivatives that depends on estimates for the parameters ρ, β, θ which reflect averages over all countries and time periods. LeSage and Pace (2009) argue for the use of scalar summary measures of the $N \times N$ matrix of own- and cross-partial derivatives based on an average across the sample of observations, similar in spirit to the way conventional least-squares regression estimates are interpreted. Specifically, an average of the main diagonal elements from the $N \times N$ matrix reflecting own-partials is used as a scalar summary for the direct effects, and an average of the cumulative sums of off-diagonal elements from each row is used as a scalar summary for the indirect or spillover effects. An important point is that

[12] We do not interpret coefficients associated the time dummy variables in the matrix X_0, just those associated with explanatory variables in the matrix X.

the scalar summary measure of spillover effects cumulates the spillovers falling on all other observations, but the magnitude of impact will be greatest for countries in the immediate dependence set and decline in magnitude for higher-order dependence.[13] The sum of the two effects (direct and indirect) represents the (cumulative) total effect associated with a change in an observation for that explanatory variable.

The decision dependence model collapses to an independence model when the scalar dependence parameter ρ takes a value of zero. In this case, the cross-partial derivatives reflecting spillovers are all zero. An implication of this is that conventional probit models assume independence between decisions of observations which restricts spillovers to be zero. As a scalar summary measure of average total effect, LeSage et al. (2011) use an average of the vector of (cumulative) total effects which is the sum of direct plus indirect effects.

To see the point of Lacombe and LeSage (2018), consider an example where the probability of adoption of cash transfer programs depends on the population size characteristic of countries (used as an explanatory variable). Evaluating the partial derivative impacts for a very small population country might produce a very different probability response relative to these impacts for a very large population country. To conform with standard probit assumptions that the disturbances have a constant scalar error variance: $\sigma_\varepsilon^2 = 1$, explanatory variables are transformed to deviations from means. Nonetheless, countries with population far below the mean versus those with population far above the mean could give rise to large (nonlinear) differences in the partial derivatives. However, we calculated effects based on the entire $NT \times 1$ vector $z^v\delta$, for each explanatory variable v, and then averaged across the T time periods to produce country-specific effect estimates. These estimates should capture differences that arise due to differences in characteristics of the sample of countries considered. In presentation of our estimation results, we show that country-specific estimates were not significantly different from simpler scalar summary estimates that average over all countries. This allows us to simplify presentation of the direct, indirect and total effect estimates, since we can ignore the large number of country-specific estimates and focus on scalar summary estimates.

4 Data and results

We use data from 17 Latin American countries (Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, the Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Mexico, Nicaragua, Panama, Paraguay, Peru and Uruguay) from CEPAL (2019), and while the cash transfer data come from actual expenditures, it uses budgeted figures when actual expenditures are missing. We also extrapolate cash

[13] If k represents the set of countries that depend on country i, then second-order dependence would be on countries in the dependence sets of the countries in k, say the sets $k_1, k_2, \ldots k_m$ for the m countries in the set k. Third-order dependence would be on countries in the dependence sets of the countries in $l_1, l_2, \ldots l_m$ that are in the dependence sets $k_1, k_2, \ldots k_m$, which is the dependence set of country i and so on.

Table 2 Coefficient estimates for the spillovers probit model

	Conditional cash transfers		Unconditional cash transfers	
	SLX	SDM	SLX	SDM
Variable	Coefficient	Coefficient	Coefficient	Coefficient
Poverty	− 0.141	0.112	− 0.688***	− 0.740***
log(population)	0.819***	1.536***	0.615***	0.655***
log(GDP per capita)	0.445**	1.000***	0.383**	0.413**
log(Gov as % GDP)	0.142	0.101	0.881***	0.957***
W*Poverty	− 1.269***	− 1.786***	− 1.006***	− 1.053***
W*log(population)	0.465***	− 0.119	1.781***	1.891***
W*log(GDP per capita)	− 1.422***	− 2.721***	− 1.255***	− 1.396***
W*log(Gov as % GDP)	0.189	0.230	0.145	0.105
ρ		0.550***		0.070
Nobs, Nvars	306,25	306,25	306,25	306,25
#0,1 y-values	73,233	73,233	110,196	110,196

*90% significance level, **95% and ***99%

Notes: *SLX* spatial lag of X, *SDM* spatial durbin model

transfer figures when less than two years are missing to accommodate for countries that report these figures biannually, using the simple average of the reported years. Although the data measure expenditures on these programs as a percentage of GDP, we are interested only in a dichotomous measure of the absence/presence of such programs. Specifically, for each year that a country has the cash transfer program in place, we use a 1-state variable, and for years when the program is not in place a 0-state variable.

The data for the explanatory variables come from the World Development Indicators database, in yearly frequency, and are lagged one year to allow the governments considering adoption of the program to respond to previous period conditions.[14] We use the population of each country to account for the size of the economy, the poverty rate (those living with less than 4 international dollars a day) to measure the magnitude of the disadvantaged population in each country, the real GDP per capita in purchasing power parity terms to measure the level of economic development of each country and Government Expenditures as a percentage of GDP to account for the size of the public sector in each country. Our spillover effects panel probit model allows decisions made regarding program adoption to depend not only on own-country characteristics, but also on those of other dependent countries, which are reflected by the explanatory variables matrix WX, and associated parameters θ.

[14] The typical endogeneity concern does not apply here because it takes time for poverty conditions to lead to program implementation, which in turn would take additional time to affect poverty—there is no contemporaneous effect between poverty and cash transfer programs. In addition, because the dependent variable reflects a binary state-transition at a discrete point in time and the explanatory variable is continuous, the conventional reverse causality scenario is not likely to occur.

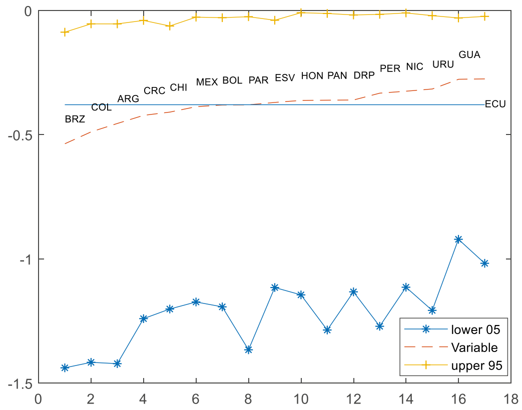
4.1 Estimates of the underlying model parameters

As already noted, we use the spatial Durbin probit model to estimate the impact of our explanatory variables on the probability of countries adopting cash transfer programs. Table 2 presents the results for the spatial lag of *X* (SLX) and SDM probit models for both type of programs when the set *j* of dependent countries are defined based on out-migration flows. The SDM model subsumes the SLX specification as a special case where dependence of the adoption decision of the typical country on other countries adoption decisions is not present. (This is the case when the coefficient estimate for ρ is not significantly different from zero). Estimates for the SLX specification can be shown to be biased and inconsistent in the presence of significant dependence of adoption decisions, or in other words when the scalar coefficient ρ is different from zero. Coefficients on the *X*-variables in the SLX model are interpreted as direct (own-partial derivatives) effects, and those on the *WX* variables as reflecting spillover effects. Spillover effects in the SLX model impact only countries in the dependence set *j* of country *i*, having no higher-order dependence impacts.

The motivation for presentation of SLX probit estimates alongside those for the SDM probit specification is a test for the presence of simultaneous interaction between country adoption decisions. We note that SLX estimates can be theoretically shown to be biased and inconsistent in the face of dependence in country adoption decisions, so a comparison of estimates from these two specifications provides an indication of the relative importance of appropriately modeling interaction in country decisions. We see that in the case of the CCT programs, the estimate of ρ is significantly different from zero, suggesting dependence of countries adoption decisions, where the dependence set of countries are defined as countries where large shares of out-migrants from each country have settled.

The SLX coefficients reported here show that population and GDP per capita exert a statistically significant direct effect on CCT program adoption, and these explanatory variables together with poverty have statistically significant spillover effects on program adoption. This means that most characteristics of countries in the dependence set are significant in framing the context in which policy-makers operate and are important in explaining variation in the 0,1 dependent variable values used in the SLX regression. Of course, these regression estimates are biased due to the presence of dependence between countries adoption decisions. Coefficients associated with the *X* and *WX* variables are presented for the SDM model, but as already discussed these cannot be interpreted in the usual way that regression coefficients are treated, as partial derivatives. We present direct and indirect effect estimates for the SDM probit model in the next section.

For UCT programs, the SLX coefficients show that poverty exerts a negative and statistically significant direct effect on program adoption, while the population size, GDP per capita and the relevance of the public sector exert a positive and statistically significant effect on program adoption. Only government spending as a percentage of GDP fails to exert a statistically significant indirect impact on program adoption. The SDM estimate for the dependence parameter ρ is found to be statistically insignificant for the UCT programs, indicating that there is no dependence between country adoption decisions in this case.



Note: ARG stand for Argentina, BOL for Bolivia, CHI for Chile, COL for Colombia, CRC for Costa Rica, DRP for the Dominican Republic, ECU for Ecuador, ESV for el Salvador, HON for Honduras, GUA for Guatemala, MEX for Mexico, NIC for Nicaragua, PAN for Panama, PAR for Paraguay, PER for Peru and URU for Uruguay.

Fig. 3 Observation-level total effect for poverty

4.2 CCT program adoption results

Based on underlying parameters from the SDM model specification, we calculated country-specific estimates of the direct and spillover effects that mimic conventional probit model marginal effects. Estimates for these were calculated based on the entire $NT \times 1$ vector $z^v\delta$, for each explanatory variable v and then averaged across the T time periods to produce country-specific effect estimates for the weight matrix based on out-migration flows.

An examination of the county-specific effect estimates showed no statistically significant difference between scalar summary effect estimates based on an average across all countries and the effects for individual countries. The effects for individual countries exhibit wide confidence intervals for all variables, meaning that distinguishing between country-specific effects was not meaningful. Figure 3 provides an illustration of this point, showing country-specific estimates of the total effect associated with the poverty variable. The lower 0.05 and upper 0.95 credible intervals were calculated from the set of retained MCMC draws. The scalar summary measure that averages over all countries is shown as the horizontal line in the figure. Given the lower 0.05 and upper 0.95 intervals in the figure, it should be clear that despite the small differences in country-specific estimates, there are no significant differences in these estimates between countries. A similar result was found for all other direct, indirect and total effect estimates, where the wide lower and upper credible intervals pointed to a lack of significant differences between country-specific estimates.

This also means that the scalar summary estimate (shown as the horizon line in the figure) provides a valid basis for inference regarding the total effect of the

Table 3 Summary of effect estimates for the probability of adopting cash transfer programs

	Conditional cash transfers (CCT)	Unconditional cash transfers (UCT)
Direct effects		
Poverty		
Minimum	− 0.6561*	− 0.3883*
Median	− 0.5135*	− 0.2510*
Maximum	− 0.4320*	− 0.1644*
Population		
Minimum	0.1242*	0.2398*
Median	0.1688*	0.3652*
Maximum	0.2319*	0.5664*
GDP per capita		
Minimum	− 0.2766*	− 0.2048*
Median	− 0.1970*	− 0.1309*
Maximum	− 0.1455*	− 0.0823*
Government spending		
Minimum	0.0244	0.0945*
Median	0.0310	0.1471*
Maximum	0.0467	0.2300*
Indirect effects		
Poverty		
Minimum	− 0.2580*	− 0.0251
Median	− 0.1839*	− 0.0158
Maximum	− 0.1328*	− 0.0091
Population		
Minimum	0.1122*	0.0133
Median	0.1546*	0.0231
Maximum	0.2151*	0.0368
GDP per capita		
Minimum	− 0.2587*	− 0.0125
Median	− 0.1855*	− 0.0078
Maximum	− 0.1323*	− 0.0044
Government spending		
Minimum	0.0223	0.0052
Median	0.0297	0.0092
Maximum	0.0435	0.0147
Total effects		
Poverty		
Minimum	− 0.5404*	− 0.4143*
Median	− 0.3866*	− 0.2676*
Maximum	− 0.2829*	− 0.1760*
Population		
Minimum	0.2372*	0.2571*
Median	0.3287*	0.3887*

Table 3 (continued)

	Conditional cash transfers (CCT)	Unconditional cash transfers (UCT)
Maximum	0.4494*	0.6042*
GDP per capita		
Minimum	− 0.5397*	− 0.2196*
Median	− 0.3853*	− 0.1401*
Maximum	− 0.2805*	− 0.0885*
Government spending		
Minimum	0.0482	0.1017*
Median	0.0624	0.1570*
Maximum	0.0926	0.2458*

*90% significance level, **95% and ***99%

poverty variable on the probability of program adoption. Recall that the concern raised by Lacombe and LeSage (2018) was that scalar summary estimates might not provide an adequate basis for inference in situations where a great deal of observation-specific variation in the effect estimates exists. This result allows us to greatly simplify presentation of the estimation results by focusing on scalar summary estimates of the direct, indirect and total effect estimates that reflect an average over the entire sample of countries.

Table 3 presents the country-specific effect estimates using the minimum, median and maximum effect estimate from the set of 17 country-specific estimates produced. Despite the variation in magnitudes shown in the table, as already noted, these differences in magnitude are not statistically significant. The explanatory variables data were transformed to deviation from the means divided by the standard deviation to produce the required standard normal distribution for the disturbances in the probit model specification. Vectors subjected to transformation of this type have been labeled "studentized" in the literature. This makes interpretation of the direct, indirect and total effects magnitudes quite literally an indication of how a one unit change in the studentized vectors reflecting the explanatory variables would impact the probability of program adoption.

Turning first to the direct effects of CCT program adoption, countries with lower poverty rates have a higher probability of CCT program adoption, and this direct effect is significant for all countries based on lower 0.05 and upper 0.95 credible intervals constructed using the MCMC draws. These results suggests that lower poverty rates have a positive impact on the probability of program adoption, which could mean that countries with lower poverty maintain (or expand) CCT programs in an effort to be perceived as the generators of these trends, and to receive the future political support.[15] It is also possible that countries with levels of poverty beyond a certain threshold level fear adverse budgetary impacts that would arise from high participation rates in these programs, whereas countries where poverty is trending downward have reached a poverty threshold that eliminates these concerns.

A reviewer suggested possible endogeneity of the poverty rate variable. One way to test for endogeneity is to regress the vector of N countries future period poverty on the N countries past period program adoption states, e.g, $\text{pov}_{t+1} = \alpha + \beta y_t + \varepsilon_t$. If poverty is not endogenous with regard to program adoption states, then the coefficient estimate for β will be zero. The results from this regression for each year showed no cases where the null-hypothesis is rejected at the 99% level (and only two cases where it is rejected at the 95% level).

In terms of the impact of population on the probability of CCT program adoption, we find that larger countries have a higher probability of adoption, and the direct effects are significant for all countries. Intuitively, the number of people experiencing harsh conditions is more visible in large countries, which should exert pressure on their governments to institute redistributive programs. Our measure of development, GDP per capita, is also found to exert a negative and statistically significant direct effect on CCT program adoption, indicating that countries with lower levels of development have a higher probability of program adoption. This effect can be rationalized as an extension of the welfare state in poorer countries, where there is more dependence on the public sector to solve welfare issues. Our last explanatory variable, the relative size of the government sector in the economy does not have a statistically significant effect on the probability of program adoption, suggesting that the size of the government is irrelevant to adoption of CCT redistributive programs.

Given our reliance on a weight matrix defined by out-migration flows, information that migrants from an origin country residing in a host country send to their relatives and friends back home is presumed to affect policymaking in the country of origin. A question of interest for this model specification is whether the spillover (or indirect) effects are significant or not, because significant spillover effects would indicate that a model specification that ignores other countries' influences on the probability of program adoption would produce estimates that are biased and inconsistent. In other words, a model that ignores country decision interaction would set spillover effects to zero (restricting the parameter ρ to zero), leaving only direct effects, which would be biased. Table 3 shows the indirect effect estimates for the four explanatory variables, where we see that three of the four variables exhibit significant indirect effects in the case of the CCT programs. The signs of these indirect effects are the same as those of the direct effects presented in the table, compounding the effect of a particular variable on the probability of adopting these CCT programs. The magnitudes reported in the table for indirect or spillover effects indicate that these account for a substantial portion of the total effects arising from changes in the explanatory variables on the probability of program adoption. Specifically, we

[15] We also examined World Bank measures for extreme poverty that use a 1.9 international dollars per day and a regional measure of 2.5 international dollars per day, as well as a Gini coefficient measure. All of these produced a negative direct effect estimate.

find magnitudes for the direct effects of the poverty variable to range from − 0.432 to − 0.656, while the indirect effects range from − 0.132 to − 0.258.

The negative indirect effect for poverty rates suggests that countries operating in a contextual setting where the set of dependence countries have lower poverty rates are more likely to adopt CCT programs, whereas countries whose dependence set consists of countries with higher poverty rates are less likely to adopt CCT programs. Since our dependence set of countries is based on countries where a large proportion of out-migrants from country *i* reside, this implies that a typical country *i* having ex-patriots residing in lower poverty rate countries is more likely to adopt CCT programs, and a typical country *i* having ex-patriots residing in higher poverty rate countries less likely to adopt CCT programs. This seems consistent with the notion that countries whose out-migrants are moving to lower poverty rate countries may be engaged in competitive attempts to entice out-migrants to stay in the home country rather than seek economic opportunity in countries with less poverty.

The negative indirect effect found for our GDP per capita measure suggests an inverse relationship between adoption of transfer programs and income of the set of *j* countries on which adoption decisions are dependent. Since our dependence set of countries is based on countries where a large proportion of out-migrants from country *i* reside, this implies that a typical country *i* having ex-patriots residing in lower GDP per capita countries is more likely to adopt CCT programs, and a typical country *i* having ex-patriots residing in higher GDP per capita countries less likely to adopt CCT programs. Here again, the fact that out-migrants are moving to lower GDP per capita countries might make it appear that home countries have a chance at retaining out-migrants through implementation of CCT programs.

The positive indirect effect for population suggests that countries operating in a contextual setting where the set of dependence countries are larger in population size are more likely to adopt CCT programs, whereas countries whose dependence set consists of countries with smaller population are less likely to adopt CCT programs. Since our dependence set of countries is based on countries where a large proportion of out-migrants from country *i* reside, this implies that a typical country *i* having ex-patriots residing in large population size countries is more likely to adopt CCT programs, and a typical country *i* having ex-patriots residing in smaller population sized countries less likely to adopt CCT programs. This result seems consistent with the fact that larger population sized countries such as Brazil and Mexico have successful cash transfer programs in place, and information transfer from these countries to the home country provides political pressure for implementing CCT programs.

Since the sum of the two effects (direct and indirect) represents the (cumulative) total effect associated with a change the explanatory variables, and since both sets of estimates present the same sign, the total effects of our four explanatory variables are the same as the direct and indirect effects discussed above. For example, lower own-country poverty increases the probability of CCT program adoption, and also increases the probability of CCT adoption in the set *j* of dependent countries, those countries where a large number of ex-patriots reside.

In the case of the UCT programs, presented in the right column of Table 3, we find that the direct effect estimates associated with the poverty, population and GDP per capita measures are similar in signs and significance to those found for the CCT program, but government spending also becomes statistically significant for the UCT programs. Lower Poverty and GDP per capita increase the probability of cash transfer program adoption, while lower population reduces the probability of adopting UCT programs in Latin America. In terms of government spending, we find that it exerts a positive and significant direct effect on UCT program adoption over time, suggesting that countries with larger public sectors have a higher probability of adopting UCT programs. Having the resources, or the economic structure with larger public sectors, facilitates the adoption of these types of programs, as might be expected.

Turning to the indirect effect estimates for the UCT programs, we find that the country-level indirect effects are not statistically significant for this type of program. This suggests that implementation of the UCT programs by a typical country i does not exert a significant impact on other countries in the dependence set. This was true for all three types of dependence sets explored here. The estimated ρ for the case of a spatial dependence set was 0.115, for the in-migration dependence set − 0.019, and for the out-migration dependence set 0.070, and none of these were statistically significant based on the lower 0.05 and upper 0.95 credible intervals. These results are reported in the online Appendix available on the journal webpage.

The insignificant indirect effect estimates result in country-level total effect estimates that are similar in magnitude to the direct effects, since these are the sum of direct plus indirect effect estimates and present the same statistical significance as the direct effects. The total effects magnitudes are slightly larger than the CCT estimates found for population and government spending, but are slightly smaller than the CCT estimates found for poverty and GDP per capita. Table 2 is consistent with these findings, since the SLX and SDM estimates are similar both in terms of magnitude and significance, since dependence of the adoption decision for the typical country on other countries adoption decisions is not present.

5 Conclusions

Public cash transfer programs have been implemented to reduce overall income inequality and poverty rates, but have been shown to promote long-term growth as well, because they tend to improve human capital. UCT are less redistributive in nature and have a lower contribution to human capital, but are significantly larger in Latin America (even if they cover fewer people). CCT, on the other hand, are smaller in size but reach more people, and are better targeted to those in need, improving their standard of living through their contribution to educational attainment and health care access, boosting GDP per capita in the long term. These programs have gained prominence throughout Latin America for their perceived impact on reducing poverty and improving the reach of education, and are being introduced in many other countries around the world because of the perceived benefits.

In contrast to the past literature regarding these programs, we examine the factors that lead to the adoption of these programs in 17 Latin American countries and test for the presence of significant spillover effects arising from dependence of program adoption decisions made by each country on a set of *j* other countries. The empirical results are consistent with significant spillover impacts for the CCT programs, and this conclusion is robust to varying definitions of connectivity structures used to model dependence between countries. This implies that decisions to adopt conditional cash transfers programs are significantly influenced by the presence/absence of these programs in other countries.

In terms of own-country characteristics that significantly impact the probability of cash transfers programs, we find that larger countries (measured by population) had a higher probability of adopting these programs over time, in line with the conventional wisdom. Poverty is found to have a significant but negative impact on the probability of program adoption (for a number of different poverty measures). The somewhat counterintuitive negative impact might arise because countries with poverty beyond some threshold level may be reluctant to adopt these programs because of budgetary concerns that would arise in the face of widespread program participation. It might also be the case that in countries where poverty begins trending downward over time, political leaders want to be perceived as responsible for these downward trends as a result of their implementation of popular cash transfer programs.

The nature of indirect effects differs between CCT and UCT programs, with the former displaying positive indirect effects and the later insignificant indirect effects. Positive indirect effects for the CCT programs mean that the presence (absence) of a program in the typical country is—positively—associated with the presence (absence) of the program in the group of countries on which program adoption decisions depend. Presumably, sharing of information regarding positive experiences with outcomes such as educational attainment and health care access between connected countries leads to this type of positive association. The insignificant indirect effects found for UCT programs mean that adoption decisions are made independently of the decisions taken by other countries, so we see no systematic pattern of adoption or lack thereof between a typical country and other countries in our sample of 17 Latin American countries.

Lastly, we find that a specification where dependence is defined based on the relative importance of ex-patriots from a particular country living in a host country is most consistent with our sample data, based on log-probability and quadratic probability scores (LPS, QPS) having lower values. This seems intuitively plausible and suggests that information regarding these programs in the destination countries is transmitted by ex-patriots to their relatives back home. Alternative information transmission channels based on spatially neighboring countries and those from which a large proportion of in-migrants arrived were found to produce slightly higher LPS and QPS values. Nonetheless, we note that dependence defined based on spatial proximity, in- and out-migration patterns were highly correlated and produced similar estimates and inferences.

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