Publicly provided healthcare and migration

Clotilde Mahé*

December 2, 2018

Abstract

This paper investigates whether social health policies affect migration. I exploit the spatial and temporal variation in the expansion of a publicly provided healthcare programme in Mexico, as well as the panel dimension and the timing of the Mexican Family Life Survey. Difference-in-differences estimations reveal that non-contributory healthcare increases internal migration by freeing up care constraints and strengthening household economic resilience. International migration, costlier by nature, remains unaffected. Results point to the relevance of including resident, non-resident and household members who have migrated in assessing the impacts of social health policies. They suggest that, in the setting studied, publicly provided healthcare complements, rather than substitutes for, livelihood strategies, by enabling labour force detachment of working-age members in affiliated households.

JEL classifications: I13, I15, I18, I38, J21, O15
Keywords: Health insurance, migration, occupational choice, Mexico

Updated version of: Mahé, C. (2017). Does publicly provided healthcare affect migration? Evidence from Mexico. UNU-MERIT Working Paper 2017-049, Maastricht: United Nations University Maastricht Economic and Social Research Institute on Innovation and Technology

*clotilde.mahe@maastrichtuniversity.nl, Maastricht University, P.O. Box 616, 6200 MD, Maastricht, the Netherlands.

1 Introduction

Publicly provided healthcare has received growing attention in the academic and policy debate. Extending public healthcare is claimed to be one of the most effective ways of reaching universal health coverage (Jamison et al., 2013). While positive impacts on health and poverty are acknowledged (Finkelstein and McKnight, 2008; Finkelstein et al., 2012), concerns about adverse labour market effects, such as disincentives to work or switching from formal to informal work, have been raised (Gruber and Simon, 2008; Levy and Schady, 2013). Overall, empirical evidence on labour market outcomes remains mixed.[1]

By providing (near-)poor households with the means to deal with risk, non-contributory health insurance might affect labour market behaviours, and simultanesouly alter household livelihood strategies. Social health protection[2] could impact in particular one of these, the need to migrate (Hagen-Zanker and Leon-Himmelstine, 2013). Since Stark and Bloom's (1985) New Economics of Labour Migration (NELM), migration has been seen as a coping strategy, that is a means for households to respond to shocks.

Assuming that household members share their resources, accessing alternative sources of livelihood through social health policies could influence the decision to migrate. Publicly provided healthcare would relax financial constraints, and enable (healthier) working-age household members to migrate. It could indirectly influence migration through local labour markets by inducing them to reallocate time from care giving to working outside their households, which might involve migrating. That beneficiaries migrate could explain the mixed evidence on the links between non-contributory healthcare and labour market behaviors often found in the literature.

In addition to potential bias due to self-selection into programme affiliation and migration, a major empirical challenge in studying the relationship between publicly provided healthcare and migration is the existence of endogenous migration. Variations in health insurance coverage across space could incite low-income families to migrate, if they were not provided with similar benefits in their current places of residence. Indeed, existing findings point to an increase in mobility from low- to high-benefit areas (Moffitt, 1992). If individuals migrated to municipalities where non-contributory health insurance was introduced to access healthcare, any migration would be the result of individuals pulled to migrate to benefit from the programme, rather than healthcare affiliation enabling them to migrate.

[1] For a review, see Ravallion (2003); for evidence from the United States (US), Baicker et al. (2014), Dave et al. (2015) or Garthwaite et al. (2014); for evidence from Mexico, Azuara and Marinescu (2013), Bosch and Campos-Vázquez (2014) or del Valle (2016).

[2] Social health protection refers to policy initiatives that aim at protecting individuals from health risks through good quality of care and financial protection from health and care shocks. In this regard, non-contributory health insurance is a means to ensure financial protection in the healthcare sector.

This paper exploits the expansion of a publicly provided healthcare programme initiated in Mexico in 2003, *Sistema de Protección Social en Salud* (hereafter, *Seguro Popular*), to identify the causal effect of social health policies on the likelihood to migrate. This initiative aimed at ensuring access to healthcare to improve financial strength through health services by guaranteeing subsidized, publicly provided basic universal healthcare services, to those not covered by any social security institution. Offered to some 50 million Mexicans without social security, it institutionalised a pilot programme, *Seguro Popular de Salud*, running from October 2002 to December 2003, that gradually expanded across Mexico.

To overcome potential endogeneity biases, I combine administrative and household survey data, and take advantage of the variation in coverage change across municipalities and over time in the middle of the roll-out of this programme, as well as the timing of the Mexican Family Life Survey (MxFLS), a three-wave household panel conducted from 2002, before the start of *Seguro Popular* pilot phase, to 2005 and 2009, during its expansion phase. The panel structure of the MxFLS allows controlling for reverse causality by assigning changes in coverage to individuals based on their municipality of residence at the beginning of each time period.

I use this information in a difference-in-differences setting, in which treatment and control groups are defined by the distribution of healthcare coverage change, similar to the approach used in recent articles studying the effects of subsidised child care on children's long-run outcomes (Havnes and Mogstad, 2011) and fertility (Bauernschuster et al., 2016), or of non-contributory health insurance on educational outcomes (Alcaraz et al., 2016). Individuals living in municipalities that experienced an above 10% change in coverage in the middle of *Seguro Popular* roll-out constitute the treatment group. Those living in municipalities that experienced a below 10% change in coverage before and during its expansion phase constitute the control group. Several robustness checks confirm the validity of the identification strategy against threats of time-trending unobservables, and suggest that changes in migration propensity prior to the programme were negatively correlated with its expansion. This indicates the relevance of a difference-in-differences specification to estimate the effect of publicly provided healthcare on migration.

Exposure to a major change in healthcare coverage is found to raise internal migration, but to have a statistically insignificant effect on international migration. Estimates suggest that access to publicly provided healthcare might play a role in reducing credit and care giving constraints, enabling working-age members to migrate in families vulnerable to adverse shocks. The relative increase in disposable income might not be substantial enough to fund international migration, costlier by nature, contrary to the migration effect of conditional cash transfer programmes, as evidenced in Angelucci (2015). In contrast to contributory insurance that tends to crowd out migration by tying affiliates to formal employment and a specific location (Sana and Hu, 2007; Hagen-Zanker et al., 2009), non-contributory healthcare seems to complement, rather than substitute for, alternative livelihood strategies. Findings contribute to existing evidence on social policies that find an effect on labour force detachment in affiliated households in developing economies, specifically non-contributory pension schemes (Inder and Maitra, 2004; Posel et al., 2006; Sienaert, 2008; Ardington et al., 2009) and conditional cash transfers (e.g. Angelucci, 2015).[3] While the value of these works is evident, they are very different from the Mexican reform. This article is the first to assess whether publicly provided healthcare, granted to individuals not already covered by employment-based insurance, triggers migration. Analysing dynamics between access to healthcare and migration is particularly relevant given the importance publicly provided healthcare has been receiving as a means to reduce poverty, while potentially distorting labour markets, in low- and middle-income countries.

Results show that publicly provided healthcare initiative, initially intended to ensure access to healthcare and improve financial strength through health services, could have unexpected effects. By alleviating financial and time constraints, affiliated households can now afford to send working-age members away, which might not have been the case otherwise. Considering the possibility to migrate as a result of affiliation, that is including non-resident household members and household members who have migrated, in assessing the impact of such an initiative could explain the heterogeneity in the existing empirical evidence on the relationship between non contributory healthcare and labour market outcomes, such as Bosch and Campos-Vázquez's (2014) and Azuara and Marinescu's (2013) contrasting findings. Household affiliation to healthcare might enable working-age beneficiaries to migrate, who would then 'disappear' from estimation samples. Not accounting for the potential effects of social health protection programmes on the likelihood to migrate might question the reliability of results obtained when evaluating their impacts.

Last, in studying whether publicly provided healthcare helps surmount financial and care giving constraints, this paper clarifies the migration decision-making process as well as livelihood strategies. Results suggest that social health policies could have multiplier effects on economic development and welfare through migration. Given the documented effects of migration on development,[4] adding evidence on whether migration complements or substitutes for social health policies could give insights into what prevents migration. This is necessary to improve the design and target of policies seeking to remove impediments to

[3] See Hagen-Zanker and Leon-Himmelstine (2013) for an extensive review of the existing evidence on the relationship between social protection and migration.

[4] On the positive development impacts of migration, see for instance Adams and Page (2005); on its negative effects, see Portes (2006) or Vullnerati and King (2008).

mobility for those who could benefit from welfare gains, and to leverage the contribution of migration to the development of origin communities (Cazzuffi and Modrego, 2017).

The rest of this paper is structured as follows. Section 2 describes the mechanisms linking publicly provided healthcare to migration, and section 3, Mexico's health insurance system. Section 4 presents the empirical strategy; section 5, the data; section 6, the estimation results. Section 7 concludes.

2 Conceptual framework

Departing from neo-classical models,[5] later theoretical approaches, such as Stark and Bloom's (1985) New Economics of Labour Migration (NELM), consider migration as a decision made within a household to maximise wealth, diversify income sources and minimise risks between household members and across space. Whether and which household members migrate is decided jointly by those likely to migrate and those likely to stay behind, to support migrants or benefit from their migration, through the receipt of remittances for instance. The decision to migrate depends on opportunities inside and outside households' places of residency, costs induced by moving and being absent, number and share of household dependants, and credit constraints.

It follows that migration can be viewed as an informal livelihood coping strategy, that is a means for households to respond to shocks, since deciding to migrate is expected to be determined by financial and time (care giving) constraints. And, assuming that household members share their resources, accessing alternative sources of livelihood through social health policies could influence the decision to migrate. Publicly provided healthcare would relax financial constraints, and enable (healthier) working-age household members to migrate. It could indirectly influence migration through local labour markets by inducing them to reallocate time from care giving to working outside their households, which might involve migrating. Figure A1 depicts these dynamics.

First, by minimising health-related shocks and expenditures, publicly provided healthcare might reduce the need to diversify income sources, and to reallocate the labour force outside one's household. Labour, migration and safety nets would be substitutes in this case: accessing healthcare would crowd out work and migration, because households and individuals would not have to rely on labour markets to minimise risks any longer. In addition, the opportunity costs of migrating would increase, if it implies losing health insurance coverage.

On the other hand, non-contributory healthcare could support affiliated households and their

[5] See, for instance, Harris and Todaro's (1970) model of rural-urban wage differences.

working-age members in looking for work, locally or outside their communities of origin. This could be effected by improving individual and dependants' health, relaxing care giving constraints, thus enabling the reallocation of their labour force, and increasing financial resources, previously used to tackle household health shocks, towards remunerative activities outside their households. Healthcare, labour and migration would be complements, in this case. This might be particularly relevant in a developing country context, where labour markets tend to be imperfect, and it is difficult and costly to hire non-household members to care for dependants.[6]

Second, affiliation to healthcare is expected to improve mental health (Haushofer et al., 2017).[7] This is likely to enhance the productivity of working-age affiliates, which could strengthen their ability to migrate, consistent with a 'healthy emigrant effect'.[8] Although publicly porvided healthcare has led to significant improvements in healthcare use (Kondo and Shigeoka, 2013; Limwattananon et al., 2015; Bernal et al., 2017), effects on physical health appear mixed, in particular in Mexico.[9] A possible explanation is that health insurance might encourage affiliates to look for care when sick, that is to report they were ill, which they did not, or could not afford previously, without access to healthcare (Wagstaff and Lindelow, 2008).

Third, if financial constraints hinder the capacity of households to send migrants away, access to healthcare, i.e. a punctual but exogenous source of income, could relieve such constraints and alter the degree of labour attachment of working-age members in affiliated households (Hagen-Zanker and Leon-Himmelstine, 2013). By limiting daily and catastrophic health expenditures, and thus increasing disposable income in relative terms, non-contributory healthcare could alter the use of the household budget. It could not only boost consumption, but also directly finance migration (Bryan et al., 2014).[10] This would be in line with evidence

[6] Since access to public healthcare is conditional on not working in the formal sector, it could encourage formal workers with employer-based health insurance to become informal with publicly provided, cheaper if not free, health insurance, and dissuade informal workers from becoming formal, by increasing the non-monetary benefits of informal work (Bosch and Campos-Vázquez, 2014; Azuara and Marinescu, 2013) and the relative price of formal (health) insurance (Gruber and Madrian, 1994).

[7] Akin to a 'peace of mind' effect, Haushofer et al. (2017) found that the simple fact of having coverage improved sleep, in particular among more vulnerable people.

[8] Migrating often involves many obstacles. The literature suggests that only the fittest, and hence healthiest, would successfully emigrate. The harder such obstacles, the stronger the positive health selection (Jasso et al., 2004).

[9] While Barros (2008), Knox (2008), King et al. (2009) or Duval-Hernandez and Smith-Ramirez (2011) did not find significant effect on health status improvement, Bleich et al. (2007), Ruvalcaba and Vargas (2010) and Pfutze (2014) showed a decrease in cholesterol and high blood pressure, hypertension and infant mortality, respectively.

[10] Bryan et al. (2014) show that, when households were randomly assigned a financial incentive in rural Bangladesh, 22% of recipient families sent a member away during the pre-harvest lean season. By limiting risks induced by migration, this cash incentive contributed to diversify income sources through migration.

on other non-contributory cash (pension) programmes found to induce migration within multi-generational households by alleviating budget constraints, whereby enabling working-age members to search for (and eventually find) work in urban areas (Inder and Maitra, 2004; Posel et al., 2006; Sienaert, 2008; Ardington et al., 2009).

However, non-contributory health insurance could simultanesouly further health expenses, if affiliates decide to pay themselves for health services that are in short supply (Bernal et al., 2017), or if state-run facilities are seen of lower quality, rendering them as 'inferior goods' compared to better-quality private services, when accreditated services are inexistent (Sosa-Rubi et al., 2009).[11] As a result, affiliates might (keep) pay(ing) health expenses out of pocket.

Non-contributory healthcare could also be used to finance migration indirectly. Programme entitlement has been shown to relax borrowing constraints. For instance, Angelucci (2015) shows that poor households' entitlement to an exogenous source of income through a conditional cash transfer, *Oportunidades*, increased emigration to the United States (US). Although cash transfers were mainly consumed, families who could not previously afford to migrate, used entitlement to *Oportunidades* as collateral to ask for loans and finance migration.

3 Mexico's health insurance system

Mexico's health system is divided into two sectors. Health services are provided upon contributions to social security institutions run by the government for workers in the formal public and private economy; they are accessed through formally registered employers. Those out of these sectors, either out of the labour force or working in the informal sector – about half of the population – access a small number of underfunded services through the Department of Health (*Secretaria de Salud y Asistencia*, SSA), or pay for health services out of pocket at private entities. They are left with (i) renouncing to healthcare; (ii) looking for care offered by state-run or informal institutions; or (iii) spending a significant part of their budget on private services (Sosa-Rubi et al., 2009). As a result, there have been major gaps in resource allocation and inequalities between beneficiaries of these two healthcare systems (Frenk et al., 2009).

For this reason, *Seguro Popular* was introduced in 2003 to ensure access to qualified public healthcare, and improve financial strength through health services. It guarantees subsidised,

[11] To refer to Sosa-Rubi et al.'s (2009) example, this would be the case if *Seguro Popular* paid for complications of a parent's disease, a pregnant daughter could afford a private obstetrician.

publicly provided basic universal healthcare services.[12] This initiative has eliminated fees for services and drugs, removed access to health services and extended the supply of health services through medical infrastructure.[13]

Seguro Popular provides healthcare almost for free, only to those not covered by any social security institution, some 50 million individuals. This is a voluntary insurance plan through which affiliates make subsidised contributions, based on their budget constraints. To be affiliated, individuals must reside in Mexico and may not benefit from any other social security institutions. They can apply for (nuclear) family affiliation on a voluntary basis,[14] after providing the necessary information for a socio-economic evaluation of their families.[15] If an affiliate happens to be outside her place of residence, she is covered for medical emergencies, or if she is a patient in transit, as long as her policy is in force and she can refer to her entity of origin.[16]

It institutionalised a pilot programme, *Seguro Popular de Salud*, introduced in October 2002 in five states, and then expanded in stages. 14 additional states adopted it until December 2003. As shown by Figures 1 and 2, expansion was relatively fast, with almost full coverage reached in 2011 (Pfutze, 2015).

While the roll-out of the programme should have given priority to the poorest areas with sufficient healthcare infrastructure, political and logistical considerations might have played a role in its introduction (Barros, 2008; Díaz-Cayeros et al., 2006). Governors decided when to participate, and had some degree of autonomy in choosing when the programme would be implemented in eligible municipalities. However, other works have evidenced exogeneity with regard to 2000 municipality and state covariates, such as income, number of uninsured, industrial structure, informality and labour market outcomes – factors likely to affect the decision to migrate – during its expansion (Aterido et al., 2010; Azuara and Marinescu, 2013;

[12] Such as essential primary and secondary services and high-complexity healthcare interventions, medication, laboratory and cabinet analyses. Affiliates have access to medical-surgical, pharmaceutical and hospital services to meet their health needs. It currently offers coverage to 275 medical interventions, as well as services that support people who do not have access to formal social security, and who suffer from high-cost diseases that can put their lives and family assets at risk.

[13] For a detailed review of this initiative, see Bosch et al. (2012).

[14] That is (i) spouses or cohabiting partners, (ii) parents who are not married, (iii) children younger than 18 years old, (iv) minors who are part of the household and are related to spouses by blood, partners or parents, (v) single children up to 25 years old who can prove to be students, (vi) dependant disabled children, (vii) straight-line direct ancestors over 64 years of age, who are economic dependants and live in the same household, and (viii) persons not related to spouses, partners or parents, but who live in the same house and depend economically on it and are under 18 or disabled dependant of any age.

[15] Such as proof of address, a unique code of population registry, birth certificate, official identification with photograph of the person who acts as family head, receipt of payment for the corresponding family fee, except in the case of households that enter the non-contributory regime due to their socio-economic status.

[16] To receive the services offered by *Seguro Popular* in a hospital or health centre in another state, an affiliate must submit a voting card, a Mexican passport or a military service primer.

Bosch and Campos-Vázquez, 2014; del Valle, 2016). In this sense, the roll-out of *Seguro Popular* provides a good setting to investigate the links between publicly provided healthcare and migration.

4 Empirical strategy

A major challenge in estimating the effect of healthcare provision on migration is the existence of endogeneity due to omitted variables – self-selection – and reverse causality. A naïve estimation might be positively biased if individuals living in marginalised communities, more likely to access *Seguro Popular*, are more likely to seek job opportunities elsewhere. Reversely, the wealthiest might be less likely to enrol in this programme, and less likely to look for alternatives elsewhere, if their places of residence offer adequate labour options. Or, there might be a negative bias if the poorest individuals, more likely to obtain publicly-provided healtcare, are also less likely to afford migration; and if the wealthiest, less likely to enroll in such a programme, are more likely to bear the costs of migration, if their places of residence do not offer adequate (labour market) options.

In addition, variations in cross-municipality health insurance coverage could incite low-income families to migrate, if they were not provided with similar benefits in their current places of residence. Existing findings point to an increase in mobility from low- to high-benefit areas (Moffitt, 1992). If individuals migrated to municipalities where non-contributory health insurance was introduced to access healthcare, any migration would be the result of individuals pulled to migrate to benefit from the programme, rather than healthcare affiliation enabling them to migrate.

The causal effect of publicly provided healthcare on migration is recovered by combining household survey data with individual administrative records of *Seguro Popular* affiliation by municipality by quarter from the Department of Health. I exploit variation in coverage change across municipalities and over time, observing that *Seguro Popular*'s expansion throughout Mexico was plausibly random from the last quarter of 2004 onwards, that is in the middle of its roll-out.[17] I also take advantage of the timing of the Mexican Family Life Survey (MxFLS), conducted from 2002, before the start of its pilot phase, to 2005 and 2009, during its expansion phase.

Treated and control municipalities are classified by quartiles of healthcare coverage change, that is by the above 10% increase change in healthcare coverage, similar to the approach used in articles studying the effects of subsidised child care on children's long-run outcomes (Havnes and Mogstad, 2011) and on fertility (Bauernschuster et al., 2016), or of non-

[17] This is along the lines of the identification strategy followed by del Valle (2016).

contributory health insurance on educational outcomes (Alcaraz et al., 2016). Individuals living in municipalities that experienced an above 10% change in coverage in the middle of *Seguro Popular* roll-out constitute the treatment group. Those living in municipalities that experienced a below 10% change in coverage before and during its expansion phase constitute the control group.

Municipalities that experienced a significant change in coverage from 2002 to 2004, i.e. in its pilot phase, are excluded.[18] In estimating the specifications, municipalities where the change in coverage was at least 10%, but strictly less than 20%, are also excluded. This is done to ensure estimates are not biased by treatment contagion (Alcaraz et al., 2016). This would be the case if control municipalities were similar to treated municipalities that experienced a relatively small increase in coverage that was big enough to be defined as treated.

By defining the treatment by an above 10% change in coverage, excluding municipalities that experienced at least 10% increase in coverage in the pre-treatment period and municipalities that experienced a change of at least 10% and strictly less than 20%, this treatment-control classification approximately leads to treatment-control groups defined by the above-median increase in coverage change. Figure 3 represents municipalities by treatment status.

The benchmark difference-in-differences regression estimates the impact of a significant change in healthcare coverage in municipality m at time t on the propensity to migrate in the subsequent wave of the survey. A linear probability model with municipality fixed effects is run as shown below:

$$Y_{imt} = \alpha + \beta_1 Post_t + \beta_2 (Treated * Post)_{mt} + \delta' X_{imt} + \gamma Municipality_m + u_{imt} \quad (1)$$

where Y represents the outcome variable of interest, between wave migration, of a 21-65 year-old individual successfully interviewed in at least two consecutive waves, living in municipality m at the beginning of time period t. As illustrated by Figure 1, the dependent variable of interest is constructed as the propensity to migrate between two subsequent waves of the survey, that is between 2002 and 2005, and between 2005 and 2009. Out of three waves of data, specifications are thus run on two time periods. While this helps control for some endogeneity threats, the analysis is limited in testing the common trend hypothesis of the difference-in-differences framework I adopt. Descriptive statistics and several tests confirm estimate robustness against threats of time-trending unobservables. They suggest that changes in migration propensity prior to the programme were negatively correlated

[18] Figure A2 shows Mexican municipalities by changes in *Seguro Popular* coverage rate up to the last quarter of 2004 (pre-treatment period) on the left, and from the last quarter of 2004 to the last quarter of 2008 (treatment period) on the right, according to this treatment-control classification. Note that, on this part of Figure A2, municipalities that experienced a significant change in coverage rate by the last quarter of 2004 are coloured in dark red regardless of their change in coverage rate in the second time period.

with its expansion. This indicates the relevance of a difference-in-differences specification to estimate the effect of publicly provided healthcare on migration.

Treated is a binary variable taking value 1 if changes in *Seguro Popular* coverage in a municipality m in which respondent i lived at the beginning of each time period were strictly smaller than 10% between the last quarter of 2002 and the last quarter of 2004, and of at least 10% between the last quarter of 2004 and the last quarter of 2008 (treated municipalities); 0, if changes in coverage in a municipality were strictly less than 10% in both periods (control municipalities). *Post* and *Municipality* are respectively time and municipality fixed effects. β_2 is the difference-in-differences estimator. It captures the impact of a significant healthcare coverage change on migration. In this specification, municipality fixed effects are assumed to take into account unobserved characteristics at the treatment (municipality) level. Any unobserved variable that might be related to migration is assumed to be uncorrelated with *Seguro Popular* expansion, conditional on observed covariates. Estimate robustness to this assumption is assessed later, and individual fixed effects alternatively used.[19]

The analysis takes advantage of the timing and the panel dimension of the MxFLS to control for potential endogenous migration. Changes in *Seguro Popular* coverage are assigned to respondents based on their municipality of residence at the beginning of each time period – their 2002 municipality of residence when looking at the relation between changes in coverage and migration between MxFLS wave 1 (2002) and 2 (2005), and their 2005 municipality of residence when looking at the relation between changes in coverage and migration between MxFLS wave 2 (2005) and 3 (2009).

X is a vector of individual, household and municipality characteristics. It includes gender, age, years of schooling of respondents; household heads' gender, age, years of schooling and indigenous origins; household dependency ratios for 0-7, 8-14, 15-20 and 66 years old and more; whether a household has experienced any economic shock in the preceding five years; household wealth (asset) index, excluding farm-related assets; and lagged indices of marginalization at the municipality level,[20] which should control for the presence of other welfare programmes like *Oportunidades* as their introductions are based on such marginalization indices. u_{imt} is the error term. Standard errors are robust to

[19] The specification I estimate thus becomes:

$$Y_{imt} = \zeta + \eta_1 Treated_m + \eta_2 Post_t + \eta_3 (Treated * Post)_{mt} + \theta' X_{imt} + \iota_i + v_{imt} \quad (2)$$

[20] Compiled by *Consejo Nacional de Población* (CONAPO), marginalization indices are based on several indicators of education, dwelling and income, collected every five years, to inform about the degree of poverty, inequality and exclusion at some administrative level. The higher marginalization indices are, the poorer localities are.

heteroscedasticity.[21]

5 Data

5.1 Data source

The MxFLS specifically identifies all migrants, internal or international, even those who permanently moved to the US. This avoids potential biases of other data sets used in the literature (Kaestner and Malamud, 2014). In addition, the MxFLS provides individual and household level details on demographic and socio-economic characteristics.

Information on individual administrative records of *Seguro Popular* by municipality by quarter, as used by Bosch and Campos-Vázquez (2014), comes from the Department of Health. Of the number of affiliates to *Seguro Popular* in each quarter from 2002 to 2009, municipalities that experienced a significant change in coverage, at least 10%, from the last quarter of 2002 to the last quarter of 2004 are excluded to simultaneously take advantage of the timing of the MxFLS and the plausibly random variation in the middle of *Seguro Popular* implementation, from the last quarter of 2004 onwards.

As in Kaestner and Malamud (2014), the estimation sample is limited to 21-65 year-old men and women, successfully interviewed in at least two waves of the survey. After dropping observations with missing variables, a sample of 5,872 unique respondents interviewed across two time periods is obtained, forming an unbalanced panel of 9,431 observations.

5.2 Descriptive statistics

Descriptive statistics provided in Tables 1-3 reveal differences across treatment and control groups. As Table 1 indicates, individuals living in treated municipalities are relatively older, and have a lower level of education than individuals living in control areas. Their household heads are also more likely to be men, with lower education, and more likely to be from an ethnic minority background. They come from households with a greater share of dependants; poorer households that are more likely to have reported an economic shock in the previous 12 months, but less likely to have reported a health-related shock. Lagged marginalization indices of treated municipalities are higher than in control municipalities. Table A1 suggests that these differences are observed in both 2002-2005 and 2005-2009 periods, except regarding household health-related shocks. The analysis takes into account such differences and

[21] Estimates are also robust to clustering standard errors at the municipality (treatment unit) level, a common practice in the difference-in-differences literature (Bertrand et al., 2004).

unobserved heterogeneity by including time-invariant and -varying characteristics, time fixed effects, and alternatively municipality or individual fixed effects.

While Table 1 suggests that individuals in treated municipalities have, on average, a lower probability to migrate between waves than individuals in control municipalities, Table 2 indicates that individuals living in treated municipalities display a significantly lower propensity to migrate in the pre-treatment period, but not any significant difference in the post-treatment period. Migration propensity before the introduction of the programme was also lower in treated areas than in control areas, and migration propensity in control areas slightly decreased between the two periods. Looking at respondents' migration propensity since 12 years old before the introduction of *Seguro Popular*, it appears that those living in control areas are significantly more likely to have permanently migrated – for 12 months or more – since the age of 12 (row (1)). It is worth noting that individuals residing in treated and control municipalities in 2002 do not show any statistically significant difference in temporarily migrating – for at least one month but less than 12 months – before its introduction (row (2)). From Table 2, it can be inferred that there is a (downward) common trend in migration between individuals living in both treated and control municipalities. This justifies the use of a difference-in-differences framework.

This downward shift in migration is consistent with migration trends in Mexico. While the proportion of the Mexican population who lived in a state different from their state of birth increased from 10.6% in 1940 (2,081,000 people) to 19.2% in 2000 (18,752,000), it remained almost constant in relative terms in 2010 (19.3%, 13,976,000 people) (Pimienta-Lastra et al., 2012). These downward trends are confirmed for internal migration between functional territories (Cazzuffi and Pereira-López, 2016).[22] International migration has also been decreasing, likely because of a labour-foreign direct investment (FDI) effect in receiving Mexican states (Aroca and Maloney, 2005), combined with increasing costs involved in international migration (Orrenius, 2001). Descriptive statistics of the estimation sample thus suggest that the expansion of healthcare coverage might have offset, cancel out an *ex-ante* downward trend in migration in Mexico at the time of the survey.[23]

Table 3 presents descriptive statistics by migration status. On average, migrants are younger, more likely to be men and more educated than non-migrants. They are more likely to have spent time caring for their dependants, and to have borrowed money in the last 12

[22] Functional territories are based on commuting flows between municipalities, using cluster analysis. These units help to avoid problems common with administrative units, e.g. commuting, as people could travel back and forth without migrating (Cazzuffi and Pereira-López, 2016).

[23] Of the information gathered from the MxFLS, migration seems to be largely intra-state and undertaken for job purposes. Unfortunately, data on migration variables could not give consistent information. They are available on request.

months. The heads of their households tend to be younger, more educated and less likely to be of indigenous origins. They come from households with a greater share of 0-7-year-old dependants, but with a lower share of 8-14-year-old, 15-20-year-old or 66+ dependants. Their households are more likely to have experienced economic shocks in the last 12 months, to have spent on health expenditures, and to reside in less wealthy areas.

Disaggregating statistics by treatment status, Table 3 further shows that, on average, migrants in treated areas are less educated than migrants in control areas. They come from poorer, more vulnerable families with a slightly higher share of below 15-year-old dependants, and are located in more marginalized areas, compared to households with migrants living in control municipalities. These statistics seem to point to affiliation to *Seguro Popular* increasing the probability of those less educated, who might have less job opportunities, with greater time constraints and coming from poorer, more vulnerable households, to migrate. This is as if publicly provided healthcare enabled families vulnerable to adverse shocks, and who could not afford otherwise – families, individuals on the edge of poverty for who a small amount can make a difference – to send members away by relaxing their financial and time constraints.

Following Angelucci (2015), Figure 4 depicts pre-programme distributions of years of schooling (left), dependency ratios (right) and household wealth (bottom) for non-migrants in control municipalities, migrants in control municipalities and migrants in treated municipalities. The migrant skill distribution in control municipalities has more density in its middle (dashed grey line) than non-migrants (solid grey line), but it is shifted to the right compared to that of migrants in treated municipalities. As in the case of *Oportunidades* and international migration in Angelucci (2015) or in Greenwood and McDowell (2011), the figure on the left indicates that, while migrants in control municipalities are positively selected into migration, migrants in treated municipalities are negatively selected into migration with regard to education, which is consistent with Table 4. By alleviating financial constraints on those who are the most likely to be affected by health shocks, significant changes in health insurance coverage might worsen migrants' skill profiles, since, if skill set is a proxy for labour market opportunities, unskilled migrants are those facing the greatest difficulties in funding migration. Accessing health insurance might thus enable those with a rather limited skill set to expand their work opportunities across space. While the figure at the bottom suggests that migrants in treated municipalities are poorer than migrants in control municipalities, the statistical relationship between either dependency ratios or wealth is not confirmed by Mann-Whitney tests.[24]

[24] Estimates are available on request.

6 Results

6.1 Benchmark results

Estimates of equation (1) are presented in Table 4. Column (1) indicates that the probability of migrating between waves increased by 3.69 percentage points for respondents in municipalities that experienced a significant change in *Seguro Popular* coverage compared to individuals who lived in municipalities that did not. This estimate is robust to the inclusion of individual instead of municipality fixed effects, with a slight decrease in estimate magnitude and significance, which could be explained by the reduction in sample size (column (2)). With municipality fixed effects and controlling for individual and household time-invariant and time-varying variables, from columns (3) to (4), and municipality time-varying variables, in column (5), the estimates similarly give an effect between 3.40 to 3.78 percentage points.[25] Table 4 clearly points to the expansion of *Seguro Popular* increasing the likelihood to migrate. Looking across baseline specifications, the introduction of non-contributory healthcare appears to increase migration by an average of about 3 percentage points. Compared to an average propensity to migrate of about 7.967 percentage points in control municipalities, point estimates of column (5) suggest, for instance, an increase in migration of about 42.660 per cent of the level in control municipalities.

Benchmark results suggest that the expansion of *Seguro Popular* is different from contributory schemes that might be tying affiliates to formal employment, and hence to a specific location. By linking social health protection to formal employment, contributory health insurance systems are likely to reduce the need to migrate, since formal employment increases income stability, decreasing the necessity to diversify income sources. In contrast, *Seguro Popular* might act as an unconditional cash transfer programme, such as the South Africa Old Age Grant, by which the reduced occurrence and duration of health shocks, and the alleviation of budget constraints, might free caregivers' time. This could enable them to relocate and diversify income sources, while ensuring coverage of household dependants.

6.2 Robustness checks

Several tests are conducted to assess the validity of the identifying assumptions. An important threat to identification would be a significant relationship between municipality-specific timing of *Seguro Popular* roll-out and migration trends – differential time trends between treated and control municipalities correlated with its expansion. This would bias the estimated average treatment effects. While municipality-specific trends cannot be controlled

[25] Estimates are also robust to including state fixed effects.

for as there are only two time periods, placebo tests are performed.

First, I estimate equation (1) on first-period data with treatment status as variable of interest. Column (1) of Table 5 indicates that the propensity to migrate was significantly but negatively related with a major expansion of *Seguro Popular* in the pre-treatment period.

Second, I restrict the estimation sample to control municipalities, and randomly assign a 'fake' treatment to half of them. Equation (1) is then estimated, and this procedure is repeated (bootstrapped) 1,000 times. As shown in column (2) of Table 5, the difference-in-differences estimator is statistically significant and negative.

Columns (1)-(2) support the identification strategy. They indicate that (i) individuals living in treated municipalities display a significantly lower propensity to migrate in the pre-treatment period, i.e. there is negative self-selection into *Seguro Popular*; and that (ii) baseline estimates represent the lower bound of the true effect of the expansion of *Seguro Popular* on migration.[26] Consistent with declining internal and international migration trends in Mexico at the time of the survey, the migration shift in control areas suggests that the expansion of healthcare coverage might have cancelled out an *ex-ante* downward trend in migration in treated areas, and that baseline estimates do not reflect the existence of any positive selection into the treatment or pre-programme positive trend in treated municipalities.

Another threat to identification would be that the timing of *Seguro Popular* expansion is associated with significant changes in the probability to migrate before its introduction. For instance, if *Seguro Popular* was expanded to react to (pre-programme) downward trends in migration, estimates could mirror what was intended, i.e. changes to average migration rates. Individuals and households could also have anticipated that they would benefit from a greater coverage and lowered their propensity to migrate before its expansion. In this case, estimated effects would reflect returns to 'normal' migration rates.

Since there are only two time periods, the robustness of the estimates to a potential pre-treatment 'trend' specific to treated observations – an Ashenfelter dip effect (Ashenfelter, 1978) – cannot be assessed. However, it is reasonable to assume that increasing migration rates have not driven the expansion of *Seguro Popular*, as policies tend to fight rather than encourage migration, in particular internal migration, fearing unwieldy, unsustainable urbanisation. Migration has been decreasing both internally and globally in Mexico, and respondents in treated areas display a significantly lower propensity to migrate than those residing in control areas before the introduction of the programme, as indicated by Table 3. Moreover, focusing on municipalities that experienced changes in coverage in its expansion

[26] In a context of non-decreasing or increasing migration trends, publicly providing healthcare could then be expected to have a stronger effect on migration.

phase, not in its pilot phase, is assumed to rule out the existence of households' or individuals' anticipatory migration behaviours.

A fourth threat to identification is the attrition of households and individuals from the MxFLS. Around 51% of estimation sample observations were not successfully interviewed in all three waves. Estimates would be biased if there were selection into attrition (retention) due to the expansion of *Seguro Popular*.

The probability of benchmark estimation sample respondents not to be interviewed in all three waves of the survey is estimated in column (3) of Table 5. The effect of a change in coverage is negative but statistically insignificant. This is potential evidence of negative (positive) selection into attrition (or retention) – those living in municipalities that experienced a significant change in coverage might be more likely to be successfully interviewed in all three waves. Equation (1) is then run on a balanced panel. The difference-in-differences estimate in column (4) of Table 5 is of a similar magnitude, but slightly loses in statistical significance compared to difference-in-differences estimates with the unbalanced panel. This suggests that, despite potential selection, panel attrition might not substantially affect the estimated effect of *Seguro Popular* on respondents' propensity to migrate.

Table 6 distinguishes between low, medium and high treated municipalities to investigate non-linearity in the effect of *Seguro Popular*. Following Ferreira et al. (2018), I include three binary variables for individuals living in low, medium and high treated municipalities at the beginning of each time period.[27] Although the increase in migration is greater for respondents in low treated municipalities compared to those in medium and high treated municipalities, there is not any significant statistical difference between coefficient estimates.[28] Estimates hold to including respondents living in municipalities that experienced a change in coverage from 10% to (and excluding) 20% (column (3)).

Last, Table 7 inspects the robustness of baseline estimates to alternative definitions of treatment and control groups. Equation (1) is run with treatment variables defined from a change in *Seguro Popular* coverage of at least 10% to 90% between the two time periods. Table 8 shows that accessing non-contributory healthcare significantly increases the propensity to migrate overall (column (11)) as well as up to (and including) a 50% change in coverage (columns (1)-(6)). Beyond, this effect is negative and statistically insignificant, likely to be affected by a reduced sample size of treated.

[27] The low treated group corresponds to municipalities that experienced a change in healthcare coverage from 10% to (and excluding) quartile 2, 20.46%; the medium treated, from (and including) quartile 2 to (and excluding) quartile 3, 28.40%; and the high treated, from (and including) quartile 3 to (and including) quartile 4, 100%.

[28] The F-statistic of a test of coefficient estimate equality between low and medium treated is 0.00 (with p-value 0.988); low and high is 0.18 (0.668); and medium and high is 0.14 (0.711).

6.3 Investigating mechanisms

This subsection sheds light on mechanisms that might be at stake by running equation (1) on a different set of outcomes. Column (1) of Table 8 suggests that a change in healthcare coverage has an almost null, insignificant effect on subjective health reported at the end of each time period. Column (2) indicates that changes in coverage are associated with a greater propensity to report health-related economic shocks in a household. This is consistent with the fact that, to benefit from non-contributory healthcare, health-related shocks have to be reported, and so that health insurance encourages affiliates to look for care when sick (Wagstaff and Lindelow, 2008). Coverage changes are also found to decrease the propensity (3) and the number of hours of care giving (4), indicating an alleviation of working-age members' time constraints. Finally, columns (4)-(5) show a positive and significant link between coverage changes and a household's likelihood to borrow, and columns (6)-(7), with health expenditures. This suggests that accessing non-contributory healthcare relaxes financial (and borrowing) constraints, enabling affiliated households to spend on non-covered health items, often seen of better quality.[29]

6.4 Heterogeneous effects

If there is any effect, effects are likely to differ by household composition. Because women tend to bear the greatest care giving burden in Mexican households, *Seguro Popular* might have differential impacts across gender. Since women are more likely to take care of dependants than men, accessing health coverage might push women to enter the (informal) local labour market, now freed from caring for their dependants (del Valle, 2016). The associated alteration of budget use and decrease in time constraints might not be significant enough to prompt women to migrate. In contrast, men, less likely 'expected' to take care of dependants compared to women, might show greater labour attachment flexibility. Upon affiliation to healthcare coverage, they might be more likely to leave source households in order to further diversify household income sources. Women would simultaneously work (part-time) outside of their households, and take care of household dependants.

In Table 9, the estimation sample is decomposed by gender to account for the existence of gender-differentiated time and task allocation. Columns (1) and (2) confirm this hypothesis: men are slightly – and significantly – more likely to migrate than women following a change in coverage (respectively 3.87 and 2.90 percentage points). This is consistent with evidence from South Africa and India showing that, when women are those affiliated

[29] This would be in line with Sosa-Rubi et al. (2009) who found that, through an income effect, affiliation to *Seguro Popular* reduced the demand for non *Seguro Popular* sponsored, state-run care, and increased the demand for private care.

to social protection schemes, other family members, in particular men, tend to migrate (e.g. Sienaert, 2008). Treated households might simultaneously follow different livelihood strategies. Some household members, women, would stay home to benefit from local labour market opportunities and affiliation to *Seguro Popular*, while taking care of dependants when they are not working outside their households. Men, now financially 'enabled' to leave, with less time tied to dependants and not socially expected to care for them, would migrate.

Moreover, since international migration tends to be more costly than internal migration, and as the entitlement to *Seguro Popular* does not represent an exogenous stream of income as such, but rather a relative increase in disposable income due to a reduction in health expenditures, this insurance might be more likely to affect internal than international migration. Existing empirical findings suggest that safety nets have different, often opposite effects on domestic and global migration.[30] Internal and international migration bear different costs: internal migration is less expensive and less risky, since conditions to migrate internally are easier to meet (Stecklov et al., 2005). International migration might only be affected when it is less costly than internal migration, which is unlikely to be the case.

The probability to migrate internally and internationally are separately regressed on the full estimation sample. As columns (3) and (4) of Table 9 show, difference-in-differences estimates only hold for internal migration, with a statistically significant increase of 3.42 percentage points. The effect on international migration is almost null but statistically insignificant. The fact that the insignificance of the expansion of *Seguro Popular* on international migration is explained by the very low average international migration in the estimation sample (0.68%) cannot be ruled out. However, it can be the case that access to healthcare has a significant effect on internal migration but insignificant on international migration, in particular if difficult access to financial capital and budget constraints have been limiting migration. This might be because internal migration tends to be less expensive, less risky than international migration, and because affiliation to *Seguro Popular* does not directly provide cash, but increases disposable income by limiting health expenditures.

Last, the full estimation sample is decomposed by location. Urban areas are more likely to offer labour opportunities as well as to benefit from a developed network of private health services, compared to rural areas. Affiliates living in urban areas might thus have less interest in looking for opportunities outside their communities of residence than those living in rural areas. Columns (5) and (6) support this idea: they reveal that baseline estimates are driven by affiliates living in rural areas. Those living in urban areas are less likely to migrate as a

[30] While Chau et al. (2012), Inder and Maitra (2004), Posel et al. (2006), Sienaert (2008) and Ardington et al. (2009) have confirmed a positive effect of safety nets on internal migration via its funding channel, Angelucci (2015) does not find a consistent effect of *Oportunidades* on internal migration.

result of *Seguro Popular* expansion.

7 Concluding remarks

This paper assesses the effect of social health policy on migration. I exploit the expansion of a publicly provided healthcare programme initiated in 2002 in Mexico, *Seguro Popular*, and take advantage of the timing and the panel structure of the MxFLS to obtain causal estimates on this relationship. Difference-in-differences estimations point to a non-negligible migration response in municipalities that experienced a major change in coverage rate in the middle of its roll-out. Robustness checks confirm the validity of the identification strategy against threats of time-trending unobservables that might vary significantly between treated and control municipalities. They reveal that changes in migration propensity prior to the programme were negatively correlated with its expansion, consistent with migration trends within and from Mexico at the time of the survey.

Results suggest that individuals were more likely to migrate following an increase in healthcare coverage, compared to respondents living in municipalities that did not experience a similar change. Potential explanations for this relationship include the alleviation of time (care giving) and financial constraints. Examining effect heterogeneity suggests that associated increases in disposable income were not substantial enough to fund international migration, in contrast to other non-contributory health insurance schemes, such as conditional cash transfer programmes. This migration response is greater for men, supporting the idea that, in a context of gender-differentiated task distribution and income source diversification as in Mexico, men are those more likely to migrate compared to women. It is only significant in rural areas, less likely to offer labour opportunities or developed networks of health services, than in urban locations.

In showing that non-contributory safety nets can increase the propensity to migrate, these results shed light on some unattended effects of publicly provided healthcare. They suggest that migration might be a channel through which labour market behaviours and livelihood strategies are affected. Building financial strength and freeing up caregivers' time by limiting the incidence of health shocks can encourage labour force detachment of working-age members in households vulnerable to adverse shocks. By enabling the relocation of labour available within a household, accessing healthcare coverage could further the diversification of household income sources, which is likely to help families break out of poverty.

Given the importance publicly provided healthcare has been receiving as a means to reduce poverty, while potentially distorting labour markets, analysing dynamics between access to healthcare and migration is likely to be at the centre of the social policy debate. This paper contributes to this discussion by suggesting that it is necessary to include recipients, household members living with them *and* who have migrated in assessing such initiatives. Not accounting for the links between social health protection programmes and migration might question the reliability of results obtained in evaluating their impacts.

Figure 1: Seguro Popular beneficiaries and implementation phases

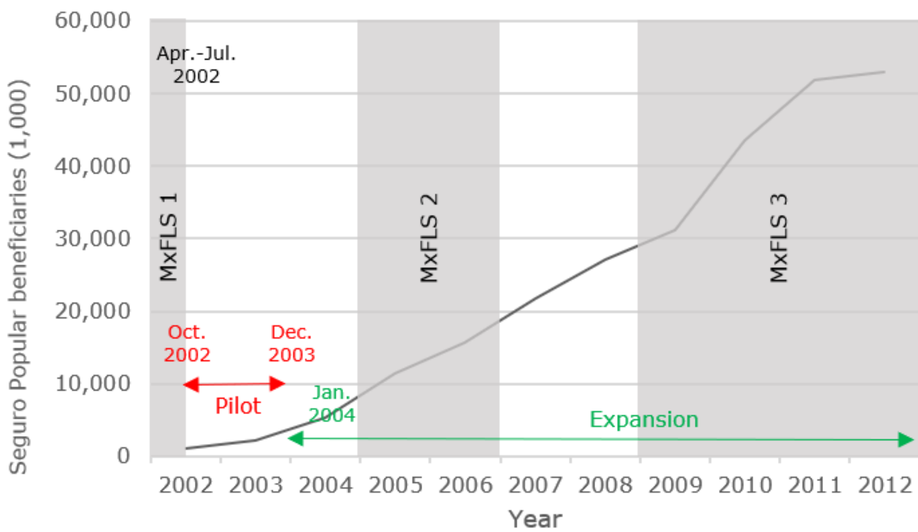

Figure 2: Municipalities by change in coverage rate in the last quarter of 2004 (l) and 2008 (r) (continuous variable)

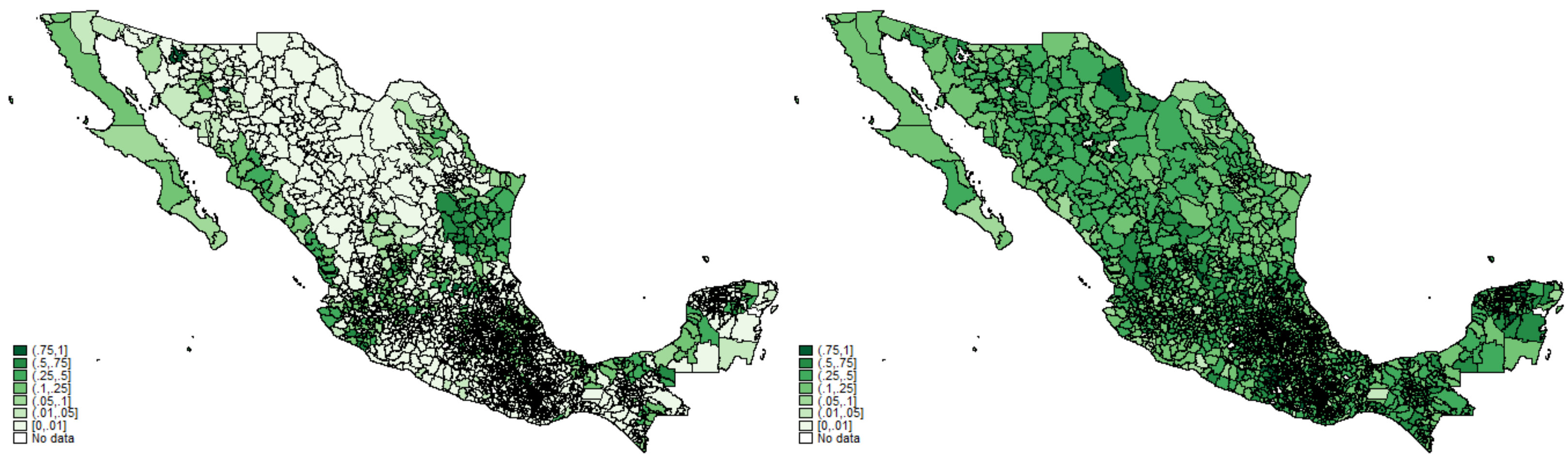

Figure 3: Municipalities by treatment status

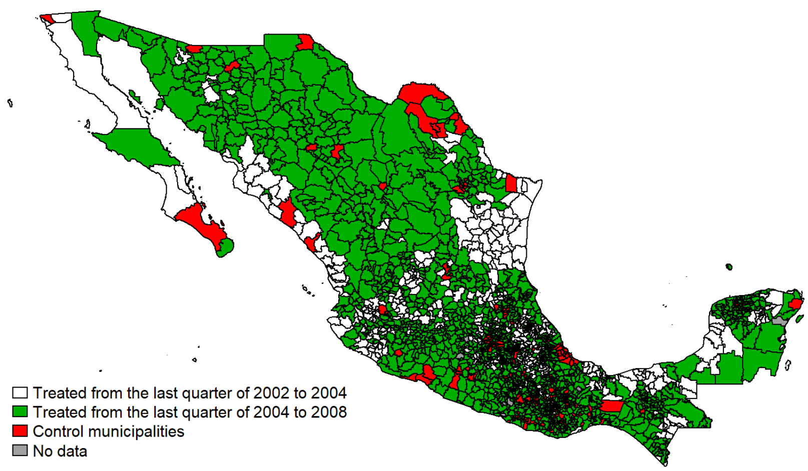

Figure 4: Years of schooling (l), dependency ratio (r) and wealth index (b) by treatment status

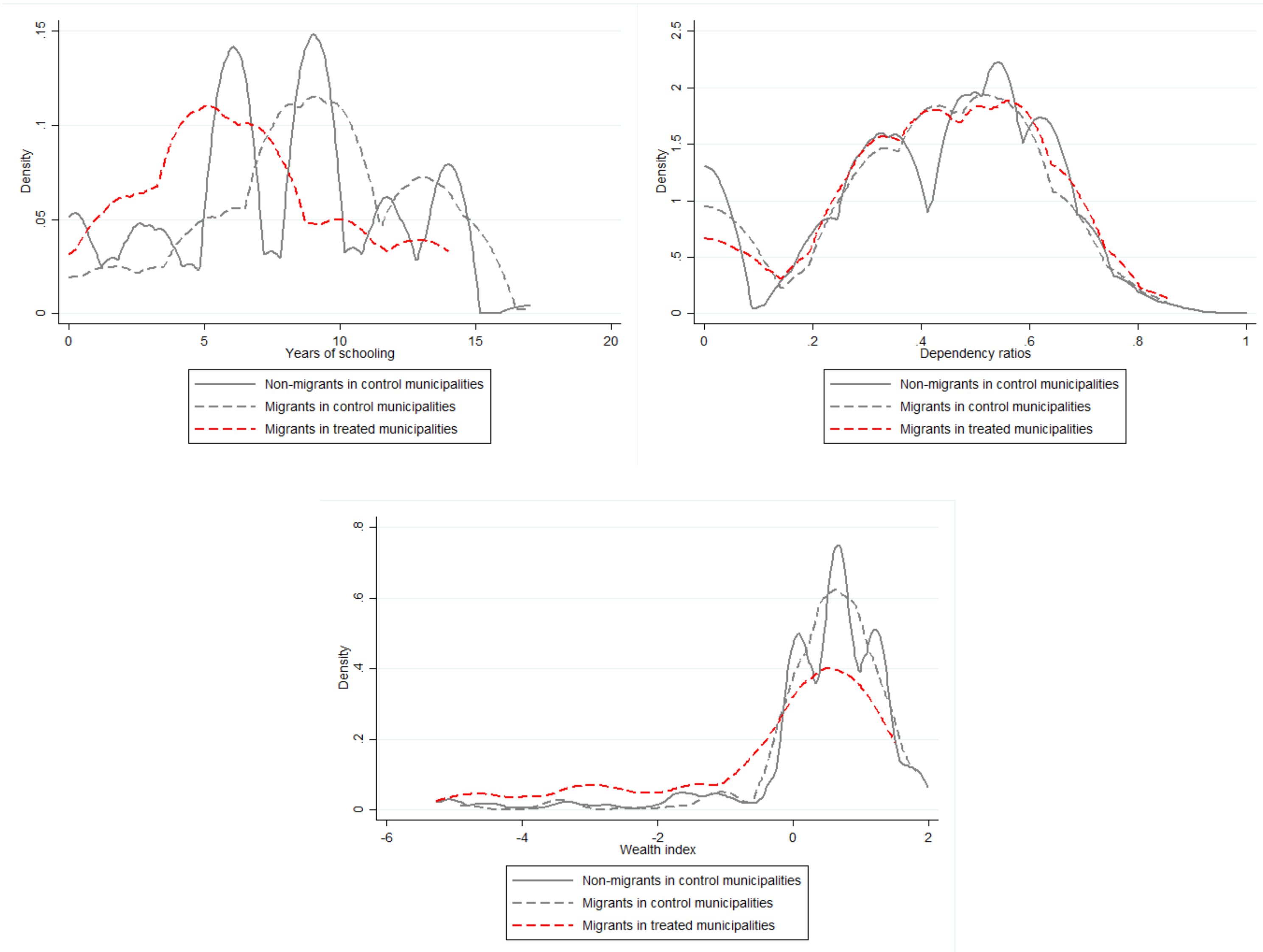

Table 1: Descriptive statistics of estimation sample

	Full sample Mean	SD	Treated Mean	Control Mean	t-test
Outcome variables (period end)					
Migrated between waves	.063	.244	.053	.080	-5.18***
Internal	.058	.233	.046	.076	-5.96***
International	.008	.086	.009	.006	1.57
Control variables (period start)					
Age	39.991	11.5149	40.229	39.613	2.53**
Male	.468	.499	.473	.459	1.31
Years of schooling	6.894	4.073	6.356	7.750	-16.41***
Household head					
Age	47.739	13.105	47.794	47.652	0.51
Male	.832	.374	.848	.806	5.36***
Years of schooling	6.175	4.199	5.716	6.907	-13.53***
Indigenous	.1465	.354	.187	.083	13.98***
Household dependency ratio					
0-7	.131	.164	.130	.131	-0.35
8-14	.142	.166	.149	.131	4.87***
15-20	.109	.148	.113	.104	2.79***
66 and more	.036	.105	.038	.033	1.97**
Gave care	.223	.416	.225	.220	0.56
Hours giving care	5.833	15.304	5.708	6.033	-1.00
Health status[a]	.947	.224	.945	.950	-0.85
Household economic shock	.290	.454	.295	.283	1.26
Household health shock	.1225	.328	.114	.136	-3.18***
Household wealth index	.140	1.425	-.016	.388	-13.55***
Borrowed (individual)	.100	.300	.095	.108	-2.03**
Borrowed (household)	.249	.432	.228	.281	-5.79***
Had health expenditures[b]	.991	.095	.993	.987	2.89***
Amount of health expenditures[c]	349.011	1701.176	338.94	365.33	-0.72
Lagged marginalization index	-.983	.889	-.687	-1.454	44.95***
Observations	9,431		5,791	3,640	

Notes: Means and standard deviations (SD) of variables of interest of the estimation sample, 5,872 unique individuals aged 21-65 years old, forming an unbalanced panel of 9,431 respondents interviewed across 2002-2009. *** p<0.01, ** p<0.05, * p<0.1. [a]Means are based on 8,860 observations. [b]Means are based on 9,183 observations. [c]Means are based on 9,099 observations.

Table 2: Migration by wave

	Treated	Control	t-test
(1) pre-2002 migration since 12 years old	.2787	.3540	-5.66***
(2) pre-2002 migration in the last two years	.0367	.0375	-0.18
(1) + (2)	.3033	.3731	-5.15***
(3) 2002-2005 migration	.0531	.0947	-5.76***
(4) 2005-2009 migration	.0530	.0612	-1.14
Observations	5,791	3,640	

Notes: Please, refer to Table 1.

Table 3: Descriptive statistics of estimation sample by migration status

	Migrants	Stayers		Migrants			Stayers		
	Mean	Mean	t-test	Treated Mean	Control Mean	t-test	Treated Mean	Control Mean	t-test
Age	35.089	40.322	-10.81***	35.752	34.386	1.57	40.479	40.065	1.64
Male	.541	.463	3.71***	.567	.514	1.30	.468	.455	1.21
Years of schooling	8.054	6.816	7.21***	7.329	8.821	-4.67***	6.302	7.658	-15.40***
Household head									
Age	44.253	47.975	-6.73***	44.303	44.2	0.09	47.989	47.951	0.14
Male	.839	.832	0.48	.857	.821	1.20	.848	.805	5.26***
Years of schooling	6.898	6.127	4.35***	6.407	7.417	-2.95***	5.677	6.862	-13.01***
Indigenous	.099	.150	-3.41***	.147	.048	4.07***	.189	.086	13.26***
Household dep. ratio									
0-7	.170	.128	6.10***	.162	.179	-1.08	.128	.127	0.30
8-14	.123	.143	-2.90***	.136	.109	2.03**	.149	.133	4.35***
15-20	.078	.111	-5.39***	.083	.072	1.10	.114	.107	2.33**
66 and more	.027	.036	-2.18**	.027	.027	0.02	.038	.034	1.90*
Gave care	.286	.219	3.84***	.296	.276	0.55	.221	.215	0.64
Hours giving care	7.811	5.700	3.26***	8.121	7.483	0.45	5.573	5.908	-1.01
Health status	.954	.946	0.84	.948	.962	0.421	.945	.948	0.532
Household shock	.343	.287	2.96***	.355	.331	0.62	.291	.279	1.30
Health shock	.124	.122	0.11	.104	.145	-1.50	.115	.135	-2.88***
Household wealth	.175	.138	0.61	-.126	.493	-5.47***	-.010	.379	-12.56***
Borrowed (individual)	.161	.096	5.09***	.169	.152	0.59	.091	.105	-2.06**
Borrowed (household)	.335	.243	5.04***	.277	.397	-3.12***	.226	.271	-4.85***
Had health expenditures	.988	.991	-0.79	.997	.979	1.94*	.993	.988	2.38**
Amount of health expenditures	521.95	337.52	2.50**	478.22	567.73	-0.51	331.37	347.79	-0.44
Lagged marginalization	-1.113	-.974	-3.69***	-.735	-1.513	13.64***	-.684	-1.449	42.85***
Observations	597	8,834		307	290		5,484	3,350	

Notes: Please, refer to Table 1.

Table 4: Coefficient estimates of benchmark specifications

	Has migrated				
Variables	(1)	(2)	(3)	(4)	(5)
Treated X 2005	0.0369***	0.0267**	0.0361***	0.0378***	0.0340***
	(0.0114)	(0.0122)	(0.0114)	(0.0114)	(0.0122)
2005	-0.0327***	-0.0220**	-0.0239***	-0.0238***	-0.0191*
	(0.0088)	(0.0098)	(0.0088)	(0.0088)	(0.0099)
Treated	-	-	-	-	-
Age			-0.0018***	-0.0016***	-0.0016***
			(0.0003)	(0.0004)	(0.0004)
Male			0.0148**	0.0130**	0.0130**
			(0.0059)	(0.0060)	(0.0060)
Years of schooling			0.0014	0.0017	0.0017
			(0.0010)	(0.0013)	(0.0013)
Age of head				-0.0002	-0.0002
				(0.0004)	(0.0004)
Head is male				-0.0030	-0.0029
				(0.0083)	(0.0083)
Years of schooling of head				-0.0007	-0.0007
				(0.0012)	(0.0012)
Head is indigenous				-0.0078	-0.0082
				(0.0118)	(0.0118)
0-7 dependency ratio				0.0055	0.0051
				(0.0244)	(0.0244)
8-14 dependency ratio				-0.0803***	-0.0802***
				(0.0196)	(0.0196)
15-20 dependency ratio				-0.0708***	-0.0705***
				(0.0201)	(0.0201)
66 and more dependency ratio				-0.0551*	-0.0548*
				(0.0329)	(0.0329)
Household economic shock				0.0257***	0.0257***
				(0.0069)	(0.0069)
Household wealth index				-0.0016	-0.0018
				(0.0025)	(0.0025)
Lagged marginalization index					0.0184
					(0.0148)
Municipality FE	Yes	No	Yes	Yes	Yes
Individual FE	No	Yes	No	No	No
Mean of dependent variable	.0658	.0595	.0658	.0658	.0658
	(.2480)	(.2366)	(.2480)	(.2480)	(.2480)
Observations	7,231	5,528	7,231	7,231	7,231
R-squared	0.0245	0.5631	0.0341	0.0404	0.0405

Notes: Estimates are for 21-65-year-old individuals, interviewed in at least two consecutives waves. Respondents exposed to a change in coverage of at least 10% and strictly lower than 20% are excluded. The dependent variable is a binary variable taking unity if an individual migrated between wave 1 (2002) and wave 2 (2005) and/or between wave 2 and wave 3 (2009). Columns (1)-(5) present coefficient estimates of linear probability models. Standard errors robust to heteroscedasticity are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Robustness checks

Variables	Has migrated (1)	Has migrated (2)	Attrition (3)	Has migrated (4)
Treated X 2005			-0.0144	0.0311**
			(0.0198)	(0.0143)
2005		.0336***	-0.1073***	-0.0118
		(.0000)	(0.0154)	(0.0133)
Treated	-0.0333***			
	(0.0094)			
Fake treatment X 2005		-.0143***		
		(.0009)		
Municipality FE	No	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Mean of dependent variable	.0732	.0815	.3876	.0540
	(.2606)	(.0002)	(.4872)	(.2260)
Observations	3,919	3,508	7,231	7,074
R-squared	0.0285	.0359	0.3190	0.0489

Notes: Estimates are for 21-65-year-old individuals, interviewed in at least two consecutive waves in columns (1)-(3); in three consecutives waves in column (4) (balanced panel). Respondents exposed to a change in coverage of at least 10% and strictly lower than 20% are excluded. In column (1), the estimation sample is limited to the first time period. In column (2), observations are limited to never treated that were assigned a fake treatment. In columns (1), (2) and (4), the dependent variable is a binary variable taking unity if an individual migrated between wave 1 (2002) and wave 2 (2005) and/or between wave 2 and wave 3 (2009). In column (3), the dependent variable is a binary variable that takes value 1 if a respondent with non-missing information was not successfully interviewed in three consecutive waves. Columns (1)-(4) present coefficient estimates of linear probability models. Column (1) presents estimates of a placebo test; column (2), of a falsification test; columns (3)-(4) investigates panel attrition. Standard errors robust to heteroscedasticity are in parentheses. In column (2), random assignment of fake treatment and regressions were bootstrapped (1,000 repetitions) (standard errors reported in parentheses in column (2) are bootstrapped). *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Multiple treatment status

Variables	(1)	(2)	(3)
10% coverage change X 2005	0.0340***		
	(0.0122)		
Low treated X 2005		.0505**	.0296**
		(.0209)	(.0130)
Medium treated X 2005		.0313**	.0298**
		(.0147)	(.0146)
High treated X 2005		.0363**	.0354**
		(.0141)	(.0140)
2005	-0.0191*	-.0193*	-.0169*
	(0.0099)	(.0099)	(.0098)
Municipality FE	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
Mean of dependent variable	.0658	.0658	.0633
	(.2480)	(.2480)	(.2435)
Observations	7,231	7,231	9,431
R-squared	0.0405	0.0406	0.0408
		F-statistic	F-statistic
β_{Low} X 2005 = β_{Medium} X 2005		.78	0.00
		(.3783)	(.9884)
β_{Low} X 2005 = β_{High} X 2005		.46	.18
		(.4971)	(.6683)
β_{Medium} X 2005 = β_{High} X 2005		.11	.14
		(.7407)	(.7109)

Notes: Estimates are for 21-65-year-old individuals, interviewed in at least two consecutive waves. In columns (1)-(2), respondents exposed to a change in coverage of at least 10% and strictly lower than 20% are excluded. Columns (1)-(3) present coefficient estimates of linear probability models. In column (1), the treatment variable is a binary variable taking value one if respondents were exposed to a change in coverage of at least 10%. In columns (2)-(3), low treated is a binary variable taking value one if respondents were exposed to a change in coverage of at least 10% and strictly less than quartile 2; medium treated, of at least quartile 2 to strictly less than quartile 3; and high treated, of at least quartile 3 to 100% coverage change. Standard errors robust to heteroscedasticity are in parentheses. In the lower part of the table, p-values are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Alternative treatment-control classifications

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
10% coverage change X 2005	0.0340***	0.0311***									
	(0.0122)	(0.0111)									
20% coverage change X 2005			0.0214**								
			(0.0103)								
30% coverage change X 2005				0.0166							
				(0.0116)							
40% coverage change X 2005					0.0174						
					(0.0125)						
50% coverage change X 2005						0.0362*					
						(0.0194)					
60% coverage change X 2005							-0.0092				
							(0.0105)				
70% coverage change X 2005								-0.0092			
								(0.0105)			
80% coverage change X 2005									0.0000		
									(0.0000)		
90% coverage change X 2005										0.0000	
										(0.0000)	
Coverage change											0.0736**
											(0.0296)
2005	-0.0191*	-0.0164*	-0.0049	0.0011	0.0018	0.0030	0.0051	0.0051	0.0048	0.0048	-0.0103
	(0.0099)	(0.0098)	(0.0074)	(0.0063)	(0.0062)	(0.0056)	(0.0056)	(0.0056)	(0.0053)	(0.0053)	(0.0089)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.0658	.0633	.0633	.0633	.0633	.0633	.0633	.0633	.0633	.0633	.0633
	(.2480)	(.2435)	(.2435)	(.2435)	(.2435)	(.2435)	(.2435)	(.2435)	(.2435)	(.2435)	(.2435)
Observations	7,231	9,431	9,431	9,431	9,431	9,431	9,431	9,431	9,431	9,431	9,431
R-squared	0.0405	0.0408	0.0403	0.0401	0.0401	0.0401	0.0399	0.0399	0.0399	0.0399	0.0404

Notes: Estimates are for 21-65-year-old individuals, interviewed in at least two consecutive waves. In column (1), respondents exposed to a change in coverage of at least 10% and strictly lower than 20% are excluded. Columns (1)-(11) present coefficient estimates of linear probability models. In columns (1)-(2), the treatment variable is a binary variable taking value one if respondents were exposed to a change in coverage of at least 10%; in column (3), of at least 20%; in column (4), of at least 30%; in column (5), of at least 40%; in column (6), of at least 50%; in column (7), of at least 60%; in column (8), of at least 70%; in column (9), of at least 80%; in column (10), of at least 90%; in column (11), it is a continuous variable measuring the change in coverage exposure between time periods. Standard errors robust to heteroscedasticity are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Potential mechanisms

Variables	Health status (1)	Gave care (2)	Hours giving care (3)	Health shock (4)	Borrowed (individual) (5)	Borrowed (household) (6)	Had health expenditures (7)	Amount of health expenditures (8)
Treated X 2005	-0.0046	-0.0379**	-0.6992	0.0556***	0.0216	.0432**	0.0155***	0.2427*
	(0.0117)	(0.0193)	(0.7318)	(0.0168)	(0.0154)	(.0213)	(0.0051)	(0.1449)
2005	-0.0005	0.0450***	1.0890*	0.0024	0.0531***	-.0133	-0.0106**	-0.2292**
	(0.0087)	(0.0152)	(0.5865)	(0.0135)	(0.0125)	(.0172)	(0.0043)	(0.1163)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.9422	.2228	5.9508	.1230	.1026	.2514	.9908	1.8468
	(.2335)	(.4161)	(15.5124)	(.3319)	(.3035)	(.4339)	(.0954)	(2.9177)
Observations	6,811	7,231	7,231	7,231	7,231	7,231	7,070	7,005
R-squared	0.0411	0.1565	0.1341	0.0422	0.0492	0.0639	0.0214	0.0758

Notes: Estimates are for 21-65-year-old individuals, interviewed in at least two consecutive waves. In column (1), the dependent variable is a binary variable taking unity if an individual reported to have regular, good or very good health at the end of the time period; 0, if s/he reported bad or very bad health. In column (2), it is a binary variable taking unity if an individual dedicated time to caring for dependants over the previous week at the end of the time period. In column (3), it is a continuous variable measuring the number of hours an individual dedicated to caring for dependants over the previous week at the end of the time period. In column (4), it is a binary variable taking value 1 if an individual belongs to a household that experienced at least one health-related economic shock in the five years preceding the end of the time period. In column (5), it is a binary variable that takes value 1 if an individual has borrowed in the 12 months preceding the end of the time period, and in column (6), if it is any member of their households. In column (7), it is a binary variable that takes value 1 if the household of respondents has had health expenditures in the 3 months preceding the end of the time period. In column (8), it is a continuous variable of the natural logarithm of how much respondents' households have spent on health expenditures in the 3 months preceding the end of the time period plus 1. Columns (1)-(8) present coefficient estimates of linear probability models. Standard errors robust to heteroscedasticity are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9: Heterogeneous effects

	Has migrated					
	Female	Male	Internal	International	Rural	Urban
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Treated X 2005	0.0290*	0.0387**	.0342***	-.0003	.0396*	-.0498*
	(0.0154)	(0.0195)	(.0116)	(.0037)	(.0231)	(.0290)
2005	-0.0225*	-0.0129	-.0176*	-.0059**	-.0244	.0218
	(0.0124)	(0.0160)	(.0095)	(.0024)	(.0216)	(.0187)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.0582	.0746	.0577	.0068	.0487	.0835
	(.2342)	(.2628)	(.2333)	(.0820)	(.2153)	(.2767)
Observations	3,865	3,364	7,169	6,801	3,675	3,556
R-squared	0.0520	0.0399	0.0431	0.0422	0.0405	0.0421

Notes: Estimates are for 21-65-year-old individuals, interviewed in at least two consecutive waves. In columns (1)-(6), the dependent variable is a binary variable taking unity if an individual migrated between wave 1 (2002) and wave 2 (2005) and/or between wave 2 and wave 3 (2009). In column (1), the estimation sample is limited to women; in column (2), to men. In column (3), the dependent variable takes value 1 if migration was internal; 0, otherwise. In column (4), it takes value 1 if migration was international; 0, otherwise. In column (5), the estimation sample is limited to respondents living in rural areas; in column (6), in urban areas. Columns (1)-(6) present coefficient estimates of linear probability models. Standard errors robust to heteroscedasticity are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Acknowledgements

I would like to thank Raymundo Miguel Campos-Vázquez for sharing data on *Seguro Popular* beneficiaries. I am furthermore grateful to participants of the 32nd Conference of the European Society for Population Economics, Banco de México Seminar Series, the 2018 Association for Public Policy Analysis and Management International Conference, and Mannheim University Seminar Series; as well as Carlo Alcaraz, Wim Naudé, Eleonora Nillesen, Sergio Parra-Cely, Isabel Ruiz, Melissa Siegel and Judit Vall Castelló, who provided comments, suggestions and encouragements. The usual disclaimer applies.

Funding

I would like to acknowledge the support of the European Union under the Marie Curie Initial Training Network (ITN), Transnational Migration, Citizenship and the Circulation of Rights and Responsibilities (TRANSMIC) (Grant Agreement No. 608417).

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Appendices

Figure A1: Linkages between publicly provided healthcare and migration

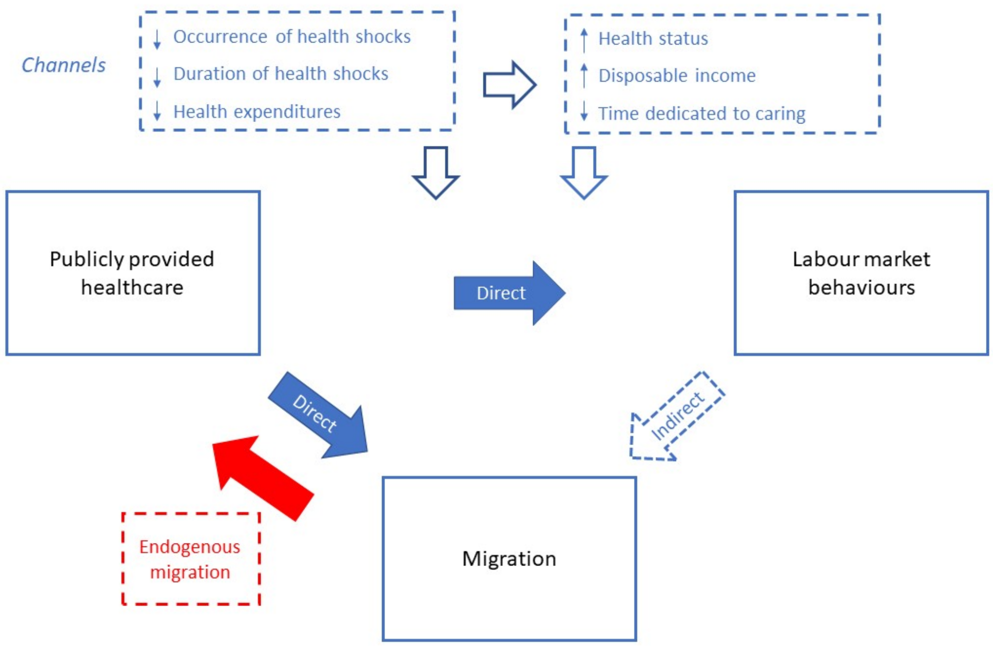

Figure A2: Municipalities by significant (+/- 10%) change in coverage rate in the last quarter of 2004 (l) and 2008 (r) (binary variable)

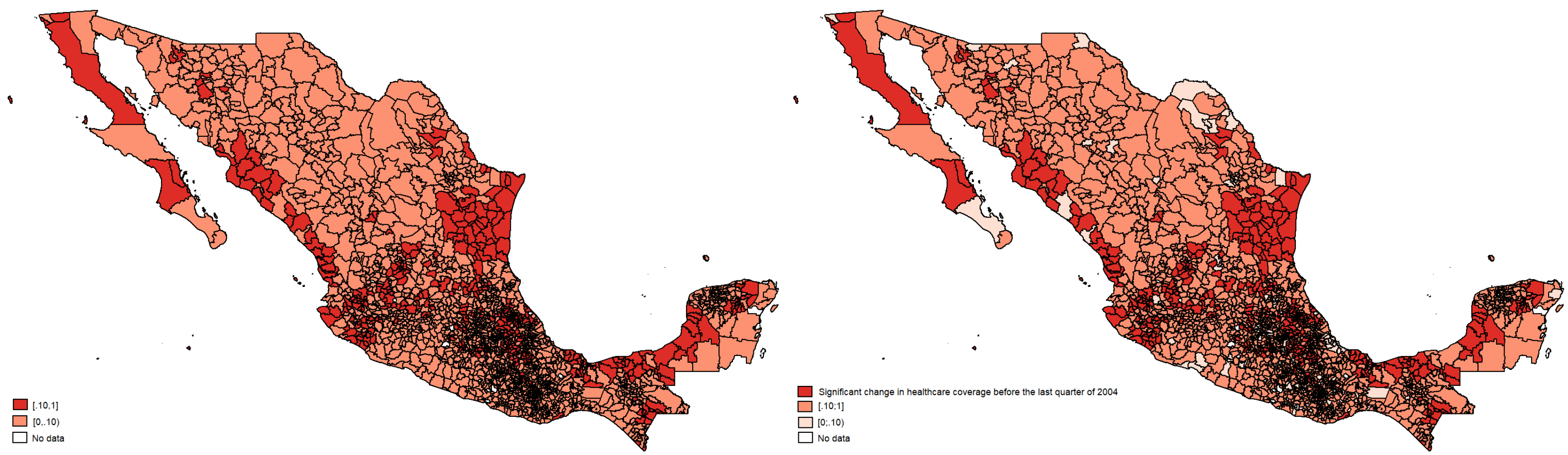

Table A1: Covariates of estimation sample by wave (period start)

	2002			2005		
	Treated	Control	t-test	Treated	Control	t-test
Age	38.257	37.535	2.24**	42.607	42.167	1.24
Male	.479	.466	0.85	.467	.451	1.02
Years of schooling	6.496	7.840	-11.78***	6.188	7.641	-11.42***
Household head						
Age	46.552	46.36	0.51	49.291	49.239	0.13
Male	.853	.810	4.15***	.842	.802	3.41***
Years of schooling	5.814	6.874	-8.95***	5.598	6.946	-10.27***
Indigenous	.186	.073	11.54***	.187	.096	8.12***
Household dependency ratio						
0-7	.138	.138	0.01	.120	.123	-0.52
8-14	.154	.135	3.88***	.142	.127	2.98***
15-20	.110	.100	2.21**	.116	.108	1.70*
66 and more	.035	.032	1.03	.041	.035	1.76*
Gave care	.217	.211	0.46	.235	.231	0.31
Hours giving care	5.552	5.934	-0.89	5.896	6.154	-0.52
Health status	.951	.953	-0.39	.939	.945	-0.80
Household economic shock	.344	.281	4.76***	.235	.285	-3.63***
Health shock	.090	.134	-4.99***	.143	.139	0.38
Household wealth index	-.024	.339	-9.02***	-.007	.450	-10.24***
Borrowed (individual)	.070	.087	-2.23**	.126	.135	-0.82
Borrowed (household)	.237	.294	-4.60***	.219	.266	-3.52***
Had health expenditures	.990	.989	0.18	.997	.985	4.53***
Amount of health expenditures	309.37	360.79	-1.25	374.5	370.93	0.06
Lagged marginalization index	-.666	-1.342	28.87***	-.712	-1.591	35.73***
Observations	3,166	2,007		2,625	1,633	

Notes: Please, refer to Table 1.