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For Children's Sake: Intergenerational Altruism and Parental Migration Intentions

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Abstract

While economic research has extensively studied the consequences of migration on children, we advance that having (or intention to have) offspring may constitute a reason to move per se. Exploiting individual-level data, this paper investigates to what extent the perceived lack of child well-being fosters parental migration intentions. Taking advantage of a large survey, the Gallup World Poll, which covers 23 Latin American and Caribbean countries between the years 2009 and 2015, we show that the perception of poor opportunities for children in one's own country is an important push factor in the parental intention to migrate internationally, besides the other individual determinants of the decision to move abroad. The magnitude and robustness of the estimate are examined by running a battery of tests. Furthermore, with the goal of tackling the potential endogeneity issues, for a subset of countries, we exploit the region of residence and the date of the interview of respondents and we instrument individual perceptions of child well-being with the timing and location of Catholic clergy scandals concerning the sexual abuse of minors.

Keywords: International Migration, Intergenerational Altruism.

JEL: F22, J13, 015, D64.

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"Look at how they treat their children. [...]

If it takes a village to raise a child, it takes a village to abuse one."

Mitchell Garabedian in Spotlight, 2015.

1 Introduction

During the 2013 High-Level Dialogue on International Migration and Development, the Secretary-General of the United Nations, Ban Ki-moon, stated that "*Migration is an expression of the human aspiration for dignity, safety and a better future*". He also added that "*Migration is part of the social fabric, part of our very make-up as a human family*". Indeed, when looking for better opportunities abroad, family ties might be considered a further source of motivation, or, in some cases, a constraint. This is the first paper, of which we are aware, to study the role of child well-being as a factor in the desire of (actual or prospective) parents to move out of the country of origin. Collecting new data on paedophilia scandals, we construct an original instrumental variable for our main variable of interest, and we show empirically that besides from traditional push factors, individual migration intention is strongly influenced by the personal perception of the well-being of children in the home country.

The question is particularly relevant if we consider that, despite tremendous progress in reducing child deaths, as well as progress in getting children into school and lifting them out of poverty, the world is still confronted with millions of children's lives being blighted for no other reasons than the circumstances into which they are born. In 2017, 17.5 percent of children in the world (or 356 million) younger than 18 years lived on less than $1.90 PPP per day, as opposed to 7.9 percent of adults aged 18+ (Silwal et al., 2020). If extreme child poverty is concentrated in low income countries (Sub-Saharan Africa accounts for 65.8 of extremely poor children), richer countries are not exempted from child poverty. On average across the OECD countries, almost one child in seven lives in relative income poverty. According to the OECD Income Distribution Database, the highest shares of child relative income poverty belong to China, South Africa, Brazil, Costa Rica and Colombia (OECD, 2018). These vast inequities and dangers imperil the future of children and perpetuate inter-generational cycles of disadvantage.

While public governments and international organizations can intervene through suitable policies to break this vicious cycle, migration is often one of the few strategies that parents and their children have to counter poverty and to benefit from important opportunities abroad. Indeed, the Human Development Report 2009 (Klugman, 2009) has elucidated how most migrants reap gains in the form of improved prospects for their children, including higher incomes, better access to education, and better healthcare. Esipova et al. (2011) also point out how those people who intend to migrate are more likely to believe that children are better off in the destination country when compared to possible migration scenarios within the country of origin. Following through with this idea, the condition of children at home can be considered to be an important push factor in the migration decisions of young adults with a family or with the aspiration of having children in the future.

Economic literature extensively inquires both economic and non-economic push and pull factors at the core of migration decisions.[1] Several recent contributions have exploited the rich set of variables of the Gallup World Poll (GWP henceforth) to further explore the determinants of migration decisions. Dustmann and Okatenko (2014) empirically analyse the link between individual wealth and migration intentions, covering information relevant to sub-Saharan Africa, Asia, and Latin America in the years 2005 and 2006. Manchin and Orazbayev (2018) and Bertoli and Ruyssen (2018) consider the importance of social networks on migration behaviour. The former use information on 150 countries from 2010 to 2013 to detail the role of close social networks abroad as well as domestically, and broad social networks abroad. The latter rely on data from 147 countries from 2007 to 2011 to study the link between migration intentions and personal connections in each destination. Docquier et al. (2020) examine the relevance of cultural traits and religiosity levels. Narrowing the focus down to 17 MENA countries for the 2007-2017 decade, they are able to investigate whether emigrants self-select on cultural traits such as religiosity and gender-egalitarian attitudes. Friebel et al. (2018) examine migration intentions from Africa and the Middle-East between the years 2010 and 2012 in order to assess the role of the human smuggling industry towards Europe. Ruyssen and Salomone (2018) track women's intention and preparation to migrate from 26 countries between the years 2005-2016 to explore to what extent worldwide female migration can be explained by perceived gender discrimination. Using the same data source, Migali and Scipioni (2019) compile a comprehensive analysis of worldwide migration intentions. Without inferring any causality, they find that having children is related to a higher probability of aspiring to migrate for individuals living in middle- and low-income countries, and to a lower probability for the ones in high income countries. Interestingly, when they perform their analysis by geographical areas, they find positive and statistically significant associations in the cases of Africa, Latin America, and the Caribbean (no associations are found for Asia, Europe, and North America and Australia). Combining GWP data on bilateral migration desires from over 140 origin countries as well as combining data on policies in 38 destination countries over the period of 2007-2014, Beine et al. (2020) investigate the role of migration policies in affecting potential migrants' destination choices. They find that immigrants tend to favour countries with more generous regulations in terms of labour market participation for non-natives, permanent residence, as well as easier access to the nationality of the host country. Additionally, they document how educational opportunities affect the migration desires of individuals aged from 15 to 24 years, who can personally benefit from educational opportunities at the destination, but they do not find any statistically significant effect for the respondents in other age groups.

Yet, we are not aware of any other study that systematically addresses the importance of child living

[1] See Berger and Blomquist (1992), Borjas (1999), Gibson and McKenzie (2011), Kennan and Walker (2011), Stark and Wang (2000), among others.

conditions as a potential push factor for emigration choices. In this paper, we show that besides from traditional covariates of interests, the aspiration of migration for both actual and prospective parents is strongly influenced by the perception about the well-being of children in the country of origin.

Original individual-level data compiled by the GWP allows us to construct an indicator for the individual perceptions towards the conditions of children for 23 Latin American countries between 2009 and 2015. We focus on Latin American and Caribbean countries for several reasons. First, a more precise variable is available to identify parents. Second, Latin America has sizeable migration flows to North America (the US and Canada) and Europe (mainly Spain and Italy) as well as intra-continental migration.[2] Third, unlike in sub-Saharan Africa and Asia, wealth constraints are generally not binding and local amenities are found to be very important determinants of the emigration choice (Dustmann and Okatenko, 2014).

In our empirical analysis, we link views on child well-being to (parental) permanent migration intentions, while controlling for confounding economic and demographic characteristics. Permanent migration can be assumed to be a predictor of whole family eventually migrating abroad (or as an intention to build a family abroad).We acknowledge that bringing children abroad is one peculiar strategy to improve their future, whereas other altruistic parents might opt for migrating without the family and sending remittances.[3] However, this latter scenario is not covered in this paper and thus our main conclusions might underestimate the actual magnitude of migration-related decisions made to protect children.

Our empirical analysis is exposed to the usual threats against identification. First, reverse causality can be an issue if individual migration intentions exert an influence on the perception of child well-being, via the comparison of the situation in their home region to that of the planned destination. Second, both the perceived child well-being and the attitudes toward migrating can be influenced by omitted and/or unobservable variables. Third, perceptions could be measured with errors. In order to reduce omitted variable problems, we control for several individual covariates, country and time-fixed effects, thereby partially capturing unobservable individual heterogeneity. In support of our analysis, we implement the test designed by Oster (2019). Finally, to deal with all the sources of endogeneity, we apply an instrumental variable (IV) approach.

Exploiting the region of residence and the date of the interview of respondents in the GWP dataset, we build an original instrument for our indicator of child well-being. Individual perceptions on child well-being are instrumented by region-specific time-varying paedophilia scandals. To this end, we collected original data on paedophilia scandals, containing the region of each parish involved in a Catholic clergy

[2]In our estimation sample, almost 40 percent of individuals declare that they want to emigrate permanently to the United States. The other two main destinations are Spain- for the 14 percent of the sample- and Canada - for the 6 percent of the sample. On average, conditions of child well-being in these preferred destinations are better than the ones at origin: the mean of the indicator of child well-being, computed on individuals aged 25-64, ranges from 0.12 for the US to 0.47 for Canada, above the mean of the indicator in our estimation sample.

[3]A growing literature tries to appraise the net impact of parental migration on child outcomes: Yang (2008), McKenzie and Rapoport (2011), Antman (2011), Antman (2013), Antman (2015), and Cortes (2015).

sexual abuse case and the exact date on which each accusation became public. For this identification strategy, data limitations entail a restriction of the sample to two countries: Argentina and Chile. We investigate the validity of the exclusion restriction by implementing several tests. In all the specifications, our results show that aside from traditional individual covariates of interest, intention to emigrate is strongly influenced by the perception of child well-being at home.

The remainder of the paper is structured as follows. Section 2 describes the data used in the empirical analysis as obtained from the GWP. Then, in Section 3, we provide the empirical framework and, in Section 4, the corresponding estimations of the link between child well-being and parental migration behaviour. Next, Section 5 deals with potential threats to endogeneity and delivers instrumented estimation results. Section 6 concludes the paper.

2 The Data

The individual-level data employed in this study are obtained from the GWP, a world-wide survey that has documented personal and household characteristics, as well as opinions on a variety of topics since 2005. A typical Gallup annual wave interviews about 1,000 randomly selected individuals within each country.[4] Data are collected through telephone surveys in countries where the telephone coverage represents at least 80% of the population. In developing regions, including much of Latin America, an area frame design is used for face-to-face interviewing. The sampling frame represents the whole civilian, non-institutionalised population aged 15 and over, covering the entire country and hence including rural areas.[5]

Our sample of interest contains almost 70 thousand respondents interviewed over the period 2009-2015 in 23 countries in Latin America and the Caribbeans.[6] When we apply an instrumental variable technique, the sample is restricted to Chile and Argentina, and we are left with an output of 6,673 respondents for the period of 2010-2015. Table 1 reports the summary statistics of the population of interest (Panel A), which is then restricted to the two presented countries (Panel B). In what follows, we explain in detail how the variables and the sample of interest have been constructed to quantify the importance of parental concerns about the well-being of children in the migratory choice.

Identifying the sample of interest. We focus on individuals older than 25 and younger than 64 years in order to identify working-age individuals who might consider it feasible to move, excluding students. The age span entails that the sample includes those individuals who have children of their own and those who do not, but could be parents in the future. This is an approximation, as the GWP does

[4]In some large countries such as China, India, and Russia, in major cities or areas of special interest, over-samples are collected, resulting in a larger total numbers of respondents (Gallup, 2012).

[5]That is with the exception of areas where the safety of the interviewing staff is threatened, scarcely populated islands, and areas that interviewers can reach only by foot, animal, or small boat.

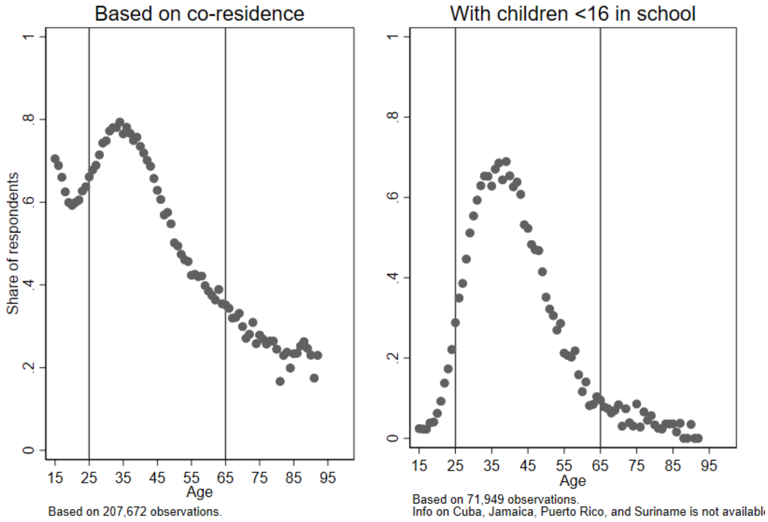
[6]They comprise of one low income country (i.e. Haiti), 8 lower-medium income countries (i.e. Belize, Bolivia, Ecuador, El Salvador, Guatemala, Honduras, Nicaragua, and Paraguay), 13 upper-medium income countries (i.e. Argentina, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Jamaica, Mexico, Panama, Peru, Suriname, Uruguay, and Venezuela), and one high income countries (i.e. Puerto Rico) according to the World Bank income categories for 2008.

Table 1: Descriptive statistics

Panel A	Full sample				
	N	mean	sd	min	max
Intending to move permanently abroad	68522	0.24	0.43	0.00	1.00
Perception of ch. well-being	68522	-0.01	1.00	-1.56	1.18
Ch. opportunities	68522	0.61	0.49	0.00	1.00
Ch. status	68522	0.43	0.50	0.00	1.00
Satisfied with local educ. system	68522	0.68	0.47	0.00	1.00
Married	68522	0.66	0.48	0.00	1.00
Female	68522	0.53	0.50	0.00	1.00
Age	68522	40.80	10.97	25.00	64.00
Urban	68522	0.51	0.50	0.00	1.00
Network abroad	68522	0.36	0.48	0.00	1.00
Education level (dummy for high)	68522	0.14	0.34	0.00	1.00
In employment	68522	0.63	0.48	0.00	1.00
Ln (1+ pc household income)	68522	7.39	1.49	0.00	12.95
Ln (1+ pc household income squared)	68522	14.77	2.99	0.00	25.90
Wealth index	68522	-0.10	0.90	-1.21	2.22
Satis. with standard of living	68522	0.68	0.47	0.00	1.00
Panel B	**Chile and Argentina**				
	N	mean	sd	min	max
Intending to move permanently abroad	6673	0.18	0.38	0.00	1.00
Perception of ch. well-being	6673	0.00	1.01	-1.86	1.67
Married	6673	0.61	0.49	0.00	1.00
Female	6673	0.55	0.50	0.00	1.00
Age	6673	42.33	11.04	25.00	64.00
Urban	6673	0.68	0.47	0.00	1.00
Network abroad	6673	0.29	0.45	0.00	1.00
Education level (dummy for high)	6673	0.10	0.31	0.00	1.00
In employment	6673	0.67	0.47	0.00	1.00
Ln (1 + pc household income)	6673	8.12	1.28	0.00	12.68
Ln (1+ pc household income squared)	6673	16.24	2.55	0.00	25.35
Wealth index	6673	-0.52	0.77	-1.21	2.22
Satis. with standard of living	6673	0.71	0.45	0.00	1.00

Average sample weights are applied to compute descriptive statistics.

Figure 1: Age distribution of parents



not contain precise information on parenthood.[7]

As regards the left truncation of the age distribution, we are guided by two different variables to predict the age of parents and to overcome the lack of exact family composition in the original survey data. The first question reads: "How many children under 15 years of age are now living in your household?". This question is based on co-residence, but it is subject to substantial measurement error (e.g. it does not control for extended families, it includes sons and daughters, it does not include the age of offspring). Secondly, we look at "Do you have children under 16 years of age who are in school?", which pertains the direct offspring of the respondent, but considers only school-aged children.[8] This variable is available only for the Latin American sample, but in less than 26% of the observations, so it can be used only descriptively as an indication towards the age truncation necessary to identify parents.[9]

Based on these two variables and considering the full uncensored sample of respondents, we look at the percentage of alleged parents by age (Figure 1) with the goal of qualitatively confirming the appropriateness of the mentioned age truncation to address our research question. As expected, relying on co-residence would entail a very noisy identification of parents, especially for the left tail of the age distribution. Furthermore, since intentions to move abroad to study are outside the scope of this

[7]In principle, in order to single out prospective parents we might focus on fertility ideals, as surveyed in "What is the ideal number of children for a family to have?", maintaining that respondents with strictly positive fertility ideals are already parents or will likely become parents in the future. Unfortunately exploiting this piece of information is not viable for three different reasons. First, the information is available only for 2009. Second, it might capture general ideals, rather than personal aspirations. Third, it records strictly positive replies for 99.9% of the respondents.

[8]We consider negligible the instance in which the respondent does have children but they are not in school as nowadays most countries in Latin America and the Caribbean have achieved universal primary schooling and are witnessing a rapid expansion of higher education.

[9]In our sample of interest, while the number of co-resident children was recorded for 68,066 respondents, the variable on the number of children at school does not appear in all waves, with only 17,736 non-missing observations.

paper, individuals aged 15 to 24 are excluded from the sample of interest.[10] [11] The suggested age truncation seems to be a reasonable choice when also looking at fertility rates among young adolescents (i.e. childbearing between the ages of 10-14 years). They are considerably low in Latin America and the Caribbean: the highest rate was recorded for Venezuela, with five births per 1,000 girls aged 10-14 years, followed by two per 1,000 for the Dominican Republic.[12]

Finally, as our research question is relevant also to potential parents, it seems reasonable to include also individuals who are not parents but are of the relevant age.

In conclusion, in order to minimise measurement errors in these regards, we have restricted the (actual or potential) parental population of interest to respondents between the ages of 25-64 years old. Finally, the sample is selected on the basis of the availability of the variables of interest in the data.

Measuring Migration Intentions. For measuring our outcome of interest, we consider the following GWP question: "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?".[13] Whereas GWP encompasses other migration-related questions, we examine the willingness to migrate permanently abroad through the mentioned one, as permanent migrants are more likely to move with their family or to reunite with their left-behind children at some point in the destination country (Dustmann and Görlach, 2016). A growing body of literature considers intention data as a proxy for actual migration. Indeed, the translation of intended into actual migration might be prevented by numerous personal circumstances such as health, finances, or family obligations (Esipova et al., 2011), as well as institutional hurdles related to migration regulations restricting the free movement of people (Docquier et al., 2014; Dustmann and Okatenko, 2014). While migration intentions do not translate systematically into actual migration flows, the two measurements appear to be strongly correlated, which suggests that the factors driving emigration intentions are also the same factors which induce people to move (Bertoli and Ruyssen, 2018).

Nonetheless, it is important to check whether migration intentions mirror observed migration dynamics in our data. Exploiting data on the annual flow of immigrants reaching OECD countries from the International Migration Database of the Organization for Economic Co-operation and Development (OECD) relative to the total population (as made available by the World Bank), we assess whether the GWP measure of intentions to migrate permanently does reflect the actual outflows from the South American countries under analysis. Although the reference population in such sources of data does not

[10] Beine et al. (2020) investigate the role of migrant rights in the destination country and document how educational opportunities for migrants affect the migration desires of individuals aged 15 to 24 years, but not those of other age groups.

[11] Concerning mandatory education and its contribution to reduce adolescent fertility and to delay family formation, lower secondary schooling according to UNESCO's International Standard Classification of Education is compulsory in all Latin American countries except in Nicaragua, while upper secondary schooling is compulsory in 12 of the 19 Latin American countries (López et al., 2017)

[12] UN Population Facts: Fertility among very young adolescents, April 2019 No. 2019/1.

[13] The questionnaire also has two follow-up questions: "Are you planning to move permanently to another country in the next 12 months, or not?", which is asked only to those who replied yes to the baseline question; "Have you done any preparation for this move (for example, applied for residency or visa, purchased a ticket, etc.)?", which is asked only to those who replied yes to the question on planning. We will not utilise this information in this context since it is beyond the scope of our paper and would remarkably reduce the number of observations.

correspond to our 25 - 64 age bracket, we focus on the overall population and on the share aged 15 - 64, and we find that the correlation between intentions to migrate permanently and the actual outflows is positive and strongly correlated (0.69 and 0.73, respectively).[14] Regarding the discrepancy between migration intentions and actual migration, it is important to stress that while the former might also encompass irregular migration plans, the latter records only legal migration (Docquier et al. (2014), Friebel et al. (2018), Mbaye (2014)).

Measuring Perceptions of Child Well-being. GWP provides a *Youth Development Index* which includes general measurements for the "development of youth" and for "respect for youth" along with a measurement of satisfaction with the educational system. More specifically, it combines three YES/NO questions using equal weights. The first question mostly refers to child status, and interviewees are asked: "Do you believe that children in this country are treated with respect and dignity, or not?". The second one refers to educational opportunities for children: "Do most children in this country have the opportunity to learn and grow every day, or not?". The last one reads: "Are you satisfied with the educational system or the schools in the area where you live".[15]In order to measure individual perceptions on child well-being, we improve upon the *Youth Development Index* and we construct a similar composite indicator using multiple correspondence analysis (MCA).

As Tuccio and Wahba (2015) and Turati (2020) argue, three main approaches can be adopted to aggregate binary variables into a composite indicator: equal weights, principal component analysis, and MCA. The first of these has been extensively used for its simplicity and apparent objectivity (Frias, 2008). However, since the imposition of numeric equality is completely arbitrary, the use of statistical procedures to determine weights should be favoured (Filmer and Pritchett, 2001). In this respect, MCA studies the relative frequency of each component and should be preferred to analyse qualitative, categorical, and binary variables (Ferrant, 2014).[16]

Appendix A contains descriptive evidence on the variables of interest. In particular, Figure A.1 illustrates cross-country variation in average intentions to move and perceptions of child well-being, suggesting a negative correlation between the two variables.

[14]We aggregate our GWP data to country level using sample weights. The correlations are computed over the period 2009-2015, and for a set of 22 out of 23 countries of our sample, as data for Venezuela are missing.

[15]There are other variables in the GWP related to child well-being for which either the geographical or/and the temporal samples are restricted: "Are you satisfied or dissatisfied with the educational opportunities available to you?"'; "Are you satisfied or dissatisfied with the schools in this country?"; "Are many children in this country required to work long hours to assist in providing for their family?".

[16]MCA is a dimensionality reduction technique developed as an extension of principal component analysis for categorical data. Testing the alternative methods, we point out that the indicator obtained using polychoric principal component analysis correlates well with our independent variable of interest (0.98). Moreover, running the probit regression on the alternative indicator, we observe that the estimated coefficients remain in the same ballpark. Lastly, for comparison, we checked whether the aggregate indicator *Youth Development Index* proposed by GWP delivers different estimates. While still significantly different from zero, the magnitude of the coefficient of interest (in absolute terms) is smaller. Regression tables are available upon request.

3 The Empirical Specification

This section describes the empirical framework used to analyse the impact of the perceptions of child well-being on (parental) migration aspirations, alongside traditional controls.

More specifically, we estimate the following empirical model:

$$Prob(Intention_{ijt} = 1) = \alpha + \beta\ Perception_{ijt} + \gamma\ X_{ijt} + \delta_j + \varphi_t + \epsilon_{ijt} \tag{1}$$

where the dependent variable $Intention_{ijt}$ is a dummy variable which takes the value of 1 if individual i living in country j at time t intends to permanently move abroad. $Perception_{ijt}$ is our main outcome of interest and accounts for the respondent i's perception on the condition of children in the country of origin j at time t. The country fixed effects, δ_j, allow one to account for unobserved fixed characteristics common to all inhabitants in the country; time fixed effects φ_t account for common time shocks.[17] As is standard in the literature (e.g. Ruyssen and Salomone, 2018), we assume that migration intentions are rational and are hence determined by the same variables typically found to explain actual migration decisions. The vector X_{ij} denotes the set of personal and household characteristics traditionally used in the literature to explain the individual decision to migrate: gender, age, marital status (married or not), education level (obtained a college degree or not), urbanisation (if the individual lives in an urban area), income (log of per capita household income in PPP international dollars +1), income squared, a dummy if the respondent is employed, and the presence of network abroad (i.e. having a household member, a friend, or a relative abroad to rely on). The model also includes in the set of regressors a wealth index summarising data on the household ownership of durable consumer goods and on housing quality, as is the case in Dustmann and Okatenko (2014), who show that migration intentions respond to individual wealth via the budget constraint.[18] Finally, to be reassured that our main variable of interest is not just capturing general factors of well-being, we control for an indicator of satisfaction with the overall personal standard of living.[19]

We estimate Equation 1 using a linear probability model (LPM).[20] Standard errors are robust to heteroscedasticity, and are clustered at the country level. We also weight regressions using survey sample weights. We will address endogeneity issues in section 5.

[17] By construction, wave and year fixed effects are almost identical. Results featuring wave fixed effects are available upon request.

[18] The variables factored in are possession of a television, access to the internet, lack of money to buy food and afford shelter in the past 12 months. This measure is obtained using principal component analysis in line with Dustmann and Okatenko (2014).

[19] For the moment, we defer the discussion on the parental status of the respondent to Table 4.

[20] Probit, logit and LPM estimations deliver similar statistically significant results. Estimates are available in Table B.1 of Appendix B.

4 Estimation Results

4.1 Baseline Findings

Table 2 reports OLS estimated coefficients of increasingly demanding specifications. Our main variable of interest, the "Perceptions of child well-being" indicator, is always negative and statistically significant across all the specifications.[21] Considering col. 3, we find that one standard deviation increase in the composite indicator on child well-being on average leads to a decrease of 3.3 percentage points in the probability of being an aspiring migrant. To facilitate the understanding of the magnitude of this effect, it might be more informative to compare the estimated coefficient of our variable of interest with the dummy variable for having a network abroad, which is recognised in the relevant literature as a strong predictor of intentions to move: having a network abroad increases the probability of being an aspiring migrant by 13 percentage points.

We find that migration intentions decrease with age, and increase for individuals living in an urban area. Women are found to be less likely to migrate than men, in line with previous empirical findings (see for example Bang and Mitra (2011), Baudassé and Bazillier (2014), and Antman (2018)). The presence of a network abroad is statistically positively associated with migration intentions. Concerning the role of per capita household earnings, there is a U-shaped relationship with a minimum being at a very low level (around 1.7 of international dollars, i.e. about 7 times lower than value of the sample mean). The results seem to indicate that a very low level of income might deter emigration intentions, probably because of financial and information constraints. After the turning point, higher levels of income are positively associated to emigration intentions.[22] The association between education and migration intention is positive, but not precisely estimated. The same holds for being in employment. Lastly, in col. 3, our findings are robust to the inclusion of variables capturing the satisfaction with the overall personal standard of living and wealth. As displayed in the table, in our sample the estimated coefficient for personal standard of living is negative and statistically significant, while the one for the wealth indicator is positive and statistically significant.[23]

[21] We cluster the standard errors at the country level. One may worry about the small number of clusters (we have 23 clusters). As a robustness, we used the Wild bootstrap approach for correcting the standard errors with a limited number of clusters (Cameron and Miller, 2015). Wild–bootstrapped t-statistics/p-values are calculated with the boottest Stata command, using the standard number of bootstrapping repetitions (999 repetitions) and the default Rademacher weights. P-values are 0.000 for col. 1, col. 2, and col. 3 of Table 2, respectively.

[22] This result is not necessarily in contrast to the inverted-U relationship found in the literature, the so-called "mobility transition". From one side, given the short time period of our analysis, we might capture only a small slice of this transition, in particular its rising part, and might thus be unable to describe the shape of the transition. From the other side, Gallup data suggest that "for countries at every level of GDP, it is generally those individuals who are richer rather than poorer (by the standards of their own countries) who are more likely to say they wish to migrate." as in Clemens (2014).

[23] Dustmann and Okatenko (2014) study the role of wealth in predicting migration intention out of sub-Saharan Africa, Asia, and Latin America. Interestingly, in the latter sub-continent the likelihood of intending to move decreases slightly with the wealth index, with similar patterns both along the index itself and along the percentiles of the index distributions. In other words, while they document the presence of budget constraints restricting migration of the relatively poor regions, such restrictions seem to be less important in Latin America, where migration intentions tend to slightly decrease with wealth.

4.2 Heterogeneous Effects

In this sub-section, we look at heterogeneous effects in selected sub-samples.

Firstly, we investigate whether older people are prone to migrating as much as younger adults. Indeed, it is less plausible that people older than 40 are going to emigrate permanently abroad. As expected, in Table 3 col. 1 and 2, the magnitude of the estimated coefficient is greater (in absolute terms) for younger people, though the correlation remains negative and statistically significant for the older, who presumably face milder incentives to move. The difference between the two estimated coefficients is statistically significant.[24]

One concern is whether immigrants may affect our results. Former immigrants might plan to return home. They might also be more likely to move again as they probably display a higher propensity to move than natives do, given their weaker ties with the local community. Col. 3 of Table 3 shows that our main results are preserved when excluding immigrants from the sample, hence keeping only natives among the respondents. We then focus on heterogeneity by gender. If women value child welfare more than men (e.g. Doepke and Tertilt, 2019), one might therefore conclude that women are more concerned than men about child well-being when considering moving abroad. In col. 4 and 5 of Table 3, we find instead no statistical difference of child well-being on migration intentions between genders.

The answer to the GWP question regarding child well-being (and its relative importance when an individual considers to move abroad) might be influenced by the socio-economic environment of the individuals. We then focus on heterogeneity by education and by the location of residence (urban vs. rural areas). In col. 6 and 7 of Table 3, the estimated coefficients of child well-being are still negative and statistically significant for both highly skilled and medium-to-low skilled individuals, and their difference is not statistically significant. Finally, in col. 7 and 8, we focus on individuals residing in an urban area vs. those living in a rural one. Still, our main results are preserved and no statistically difference between the two groups is observed.[25]

We next analyse the possibility that the effects of child well-being on migration intention may be heterogeneous along the dimension of parenthood. From one side, we would expect stronger effects for parents or prospective parents, who are plausibly more concerned about child conditions. On the other hand, more generally, adults can still feel uncomfortable living in an unsafe environment for children. In Table 4 we introduce an interaction term between child well-being and a binary indicator for whether the respondent has children younger than 16 years in school (i.e. parents).[26] As mentioned, unfortunately

[24]In order to assess the statistical significance of the difference in regression coefficients between the two samples we use the suest command in stata.

[25]In this regard, we also verify that the destination indicated by intended migrants in their reply to "Which country would you move to?" is not within their own country of origin, hence ruling out internal migration. Internal migration appears not to be relevant to our data, with some inconsistency in the replies appearing in only 10 observations.

[26]Gallup uses random-route procedures to select sampled households. In face-to-face and telephone methodologies, random respondent selection is achieved by using either the latest birthday method or Kish grid method.Gallup implements quality control procedures to validate the selection of samples and that the interviewer selects the correct person in each household. Therefore we can safely assume that interviewed mothers and fathers do not belong to the same family and are

this question was not included in the questionnaire of all waves, resulting in a drop of the sample size. Col. 1 shows that our main results are preserved also in this reduced sample. As expected, col. 2 reveals that the perception of child well-being as a migration push factor is negative and statistically significant for both parents and non-parents, but the effect is stronger for parents. To further investigate this result, we split the sample into two age groups: 25-47 and 48-64. The results in the table suggest interesting differences between the two groups. For the oldest age bracket, which includes individuals not of fertile age, the effect of child well-being is not statistically significant for individuals without children younger than 16 years in school (and less plausibly potential parents or parents with very young children). For the youngest age bracket, i.e. individuals of fertile age or who are more likely to have young children not of school age, there is no statistical difference between parents and non-parents. These results suggest that parents and potential parents (or parents with young kids not of school age) are concerned about child conditions in their decisions to emigrate. They also reassure that our indicator really captures child well-being rather than general living conditions which would affect parents/potential parents and non-parents in a similar way.

4.3 Robustness Checks

In this sub-section, we provide further robustness checks with respect to our baseline estimates of Table 2.

Alternative indicators. We break down the composite indicator of perception on child well-being into its three components: perception of opportunities for children, perception of the status enjoyed by children, and satisfaction with the local educational system. Table 5 shows that our results are not driven by any particular indicator. This suggests that our summary index is adequate to capture the overall stance of individuals on child well-being.

Including additional controls. Our estimation results are exposed to the usual concern of omitted variables, i.e. unobserved factors that influence both perceptions of child well-being and migration intention. These variables might be either unobserved individual characteristics or factors at the aggregate level. In the baseline analysis, in order to partially deal with omitted variable bias we controlled for several individual characteristics such as gender, income of the household, and satisfaction with the personal standard of living, all of which left our main findings unaltered. In addition, geographic fixed effects control for omitted country-level variables that are constant or highly persistent over time, such as the quality of institutions and the level of trust in the country.

To further mitigate concerns of omitted variable bias, we re-estimate our baseline model, substituting country fixed effects with regional fixed effects. By including regional fixed effects we are able to control for all the time-invariant geographical, institutional, and cultural characteristics which are common to all

representative of the overall population.

individuals living in the same region. While regional fixed effects control for finer spatial characteristics, and largely increase the number of clusters, unfortunately the region identifier is missing for a large number of observations.[27] Table 6 shows that regional fixed effects do not change the coefficient estimates meaningfully.[28]

It may be argued that - together with one's schooling - other socio-economic factors experienced while growing up are able to influence perceptions of young adults. This is especially the case for parenting styles and decisions related to the investment in the human capital of children, which are shaped by a combination of both observed and expected income inequality.[29] While the current income disparities in the country are already controlled for, we introduce other covariates accounting for the individual perceptions of fairness and ambition for youngsters. To put this in more detail, we believe that by controlling for the intergenerational change in educational achievements witnessed by respondents, we capture part of the (otherwise unobserved) projections and aspirations towards the welfare of the next generations, since parents might rely on their own intergenerational experience to form a dynamic optimistic or negative outlook. In Table B.2 of Appendix B, we thus account for intergenerational educational mobility by introducing country- and cohort-specific indicators summarising the extent to which the individual's generation has experienced social mobility compared to the parental one, as is computed by Neidhöfer et al. (2018).[30] Specifically, in col. 1, we include the average of years of schooling of the people who became parents in the year of birth of the respondent. In constructing this indicator, the highest level of educational attainment between the two parents has been taken into account. In col. 2, we control for the predicted probability of upper class persistence. In other words, it captures the probability of receiving higher education conditional on one's parents being highly educated. In col. 3, we introduce the "educational persistence" index which is the most widely used mobility index in intergenerational mobility literature in economics. [31] None of these intergenerational factors seem to significantly affect the probability of being an intended migrant, and the estimated coefficient of child well-being remains stable.

Oster's test. Despite our attempts to control for observable factors, our estimates might still be biased by unobservable factors. We investigate this possibility by applying the method recently developed by Oster (2019). The test establishes bounds to the true value of the coefficient of interest under two polar cases. In the first case, there are no unobservables and our regression with controls is correctly

[27] The number of clusters increases to 355.

[28] Regions are defined according to maps from Global Administrative Areas (henceforth GADM). See `https://gadm.org/`.

[29] The literature linking economic inequality (and college premiums) experienced by parents with parenting styles is summarised in Doepke and Zilibotti (2019).

[30] Neidhöfer et al. (2018) calculates time series for several indexes of relative and absolute intergenerational education mobility for 18 Latin American countries over 50 years. They estimated the indexes using rich sets of harmonised survey data as the Latinobarometro and other national household surveys of the single countries.

[31] It is the slope coefficient from a linear regression of years of schooling of children on the years of education of the parent with the highest degree. This conditional correlation between years of education of children and parents has also been weighted by the ratio of standard deviations of years of schooling of children and parents. This index captures absolute changes - e.g. because of educational expansions -, as well as relative movements of families within the distribution. The higher the index, the lower social intergenerational mobility is.

specified. We denote as $\widehat{R^2}$ the estimated R^2 and $\widehat{\beta}$ the estimated coefficient. In the second case, there are unobservables, but observables and unobservables are equally related to the treatment ($\delta = 1$ in Oster's notation). When unobservables are included, we assume, as suggested by Oster, that the R^2 is equal to $R_{max} = min(1.3\widehat{R^2}, 1)$. We denote β^* the consistent estimated parameter, obtained when we allow for the presence of unobservables. If zero can be excluded from the bounding set $[\widehat{\beta}, \beta^*]$, then accounting for unobservables would not change the direction of our estimates. Table 7 shows that the bounding set does not include zero, suggesting that our estimates are robust after correcting for selection on unobservables.

Table 2: Baseline OLS estimates: intention to migrate on perception of child well-being

	(1)	(2)	(3)
Perception of ch. well-being	-0.0434***	-0.0383***	-0.0330***
	(0.0038)	(0.0034)	(0.0032)
Married		-0.0335***	-0.0305***
		(0.0043)	(0.0041)
Female		-0.0224***	-0.0232***
		(0.0067)	(0.0066)
Age		-0.0041***	-0.0043***
		(0.0004)	(0.0004)
Urban		0.0339***	0.0369***
		(0.0060)	(0.0059)
Network abroad		0.1244***	0.1300***
		(0.0085)	(0.0083)
Education level (dummy for high)		0.0030	0.0135
		(0.0140)	(0.0139)
In employment		0.0060	0.0098*
		(0.0056)	(0.0053)
Ln (1+ pc household income)		-0.4448**	-0.5406**
		(0.1983)	(0.2241)
Ln (1+ pc household income squared)		0.2188**	0.2687**
		(0.0994)	(0.1122)
Wealth index			0.0194***
			(0.0032)
Satis. with standard of living			-0.0638***
			(0.0052)
Observations	68522	68522	68522
Year FE	yes	yes	yes
Country FE	yes	yes	yes

Robust standard errors are clustered by country and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Table 3: OLS estimates: intention to migrate on perception of child well-being by sub-samples

	(1) Aged 25-40	(2) Aged 41-64	(3) Natives	(4) Females	(5) Males	(6) High Sk.	(7) Med.-Low Sk.	(8) Urban	(9) Rural
Perc. of ch. well-being	-0.0368***	-0.0280***	-0.0333***	-0.0332***	-0.0327***	-0.0428***	-0.0317***	-0.0342***	-0.0317***
	(0.0037)	(0.0033)	(0.0032)	(0.0038)	(0.0042)	(0.0070)	(0.0032)	(0.0035)	(0.0044)
Observations	34384	34138	67337	39948	28574	8495	60027	35130	33392
Demographics	yes	yes	yes	yes	yes	yes	yes	yes	yes
Network abroad	yes	yes	yes	yes	yes	yes	yes	yes	yes
Satis. with std of living	yes	yes	yes	yes	yes	yes	yes	yes	yes
Wealth index	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes	yes	yes	yes	yes

Robust standard errors are clustered by country and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Table 4: OLS estimates: intention to migrate on perception of child well-being. Heterogeneity for parents

	(1)	(2)	(3)	(4)
	Aged 25-64	Aged 25-64	Aged 48-64	Aged 25-47
Perception of ch. well-being	-0.0202***	-0.0126**	-0.0048	-0.0191**
	(0.01)	(0.01)	(0.01)	(0.01)
Perception*Parent (ch. in school)		-0.0164**	-0.0398**	-0.0058
		(0.01)	(0.02)	(0.01)
Parent (ch. <16 in school)		0.0147	0.0078	0.0182*
		(0.01)	(0.02)	(0.01)
Demographic controls	yes	yes	yes	yes
Network abroad	yes	yes	yes	yes
Satis. with standard of living	yes	yes	yes	yes
Wealth index	yes	yes	yes	yes
Country FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	17736	17736	5816	11920

Robust standard errors are clustered by country and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Table 5: OLS estimates: intention to migrate on the components of perception indicator

	(1)	(2)	(3)	(4)	(5)	(6)
Ch. opportunities	-0.0586***	-0.0413***				
	(0.0069)	(0.0062)				
Ch. status			-0.0689***	-0.0526***		
			(0.0068)	(0.0054)		
Sat. w/ local educ. system					-0.0738***	-0.0568***
					(0.0067)	(0.0058)
Demographic controls	no	yes	no	yes	no	yes
Network abroad	no	yes	no	yes	no	yes
Satisf. with standard of living	no	yes	no	yes	no	yes
Wealth index	no	yes	no	yes	no	yes
Country FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Observations	68522	68522	68522	68522	68522	68522

Robust standard errors are clustered by country and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Table 6: OLS estimates: intention to migrate on perception of child well-being. Alternative geographic fixed effects

	(1)
Perception of ch. well-being	-0.0355***
	(0.0025)
Demographic controls	yes
Network abroad	yes
Satis. with standard of living	yes
Wealth index	yes
Region FE	yes
Year FE	yes
Observations	53125

Robust standard errors are clustered by region and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Table 7: Selection on unobservables

	Baseline LPM	(1) $R_{max} = 1.3 * \widehat{R^2}$ Identif. set
Perception on ch. well-being	-0.0330*** (0.0032)	$[-0.033; -0.026]$
$\widehat{R^2}$	0.134	

With the goal of evaluating the size of the bias due to selection on unobservable characteristics, this table presents the results of the statistical test suggested by Oster (2019). We assume that no controls are unrelated to the set of proportionally related unobservables. Col. 1 displays the identifying set bounded below by $\widehat{\beta}$ and above by β^*. It is calculated by assuming $R_{max} = 1.3 * \widehat{R^2}$ and $\sigma = 1$. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

5 Endogeneity Issues and IV Approach

In this section, we further develop the discussion on the sources of endogeneity that may bias the estimates presented so far. As previously pointed out, our regressions might suffer from omitted variables bias. In order to partially deal with omitted variables we controlled for several individual characteristics and geographic fixed effects. In addition, we applied the Oster's test, which suggests that accounting for unobservables would not change the direction of our estimates.

Moreover, reverse causality might pose a threat to identification if individual migration intentions exert an influence on the perception of childhood-related issues once an intended migrant starts comparing the situation in his home region with that of the planned destination. Lastly, our main variable of interest can be measured with errors, thus inducing an attenuation bias.

In order to address all these sources of endogeneity, we apply an IV approach on a sub-sample of countries (Chile and Argentina) for which additional data are available. In particular, utilising records published by Bishop Accountability, a non-governmental organisation that compiles a public list of sexual abuse allegations against representatives of the Catholic church, we collect data on paedophilia scandals, containing the region where the child abuse case was brought up and the exact date when each accusation became public. This way, exploiting the region of residence and the time of the interview of individuals in the GWP dataset, we introduce an original exogenous source of variation for our endogenous indicator: individual perceptions on child well-being are instrumented with region-specific time-varying paedophilia scandals. Other studies exploited data on cases of child sexual abuse in the Catholic Church in the United States. Scholars collected data on this phenomenon from the early 2000s onwards[32] in order to focus on a variety of outcomes in the field of economics of education, political economy, and religiosity.[33] To the extent of our knowledge, we are the first to digitise and rely on these data in the context of South America and in the migration literature.

Before moving to the description of the instrumented specification, let us stress the possible implications of narrowing down the sample to Chile and Argentina. We should notice that these two countries stand out from the neighbouring countries in several socio-economic dimensions. As a matter of fact,

[32] In North America, the incidents surfaced in the 1960s, peaked around 1980, and a general 40% decline of allegations followed from 1992 to 2000. This could be due to publicity about the scandals leading to the institution of guidelines to eliminate opportunity for potential offenders to be alone with children, and to increased vigilance by parents. In any case, the topic regained momentum in January 2002, when the Boston Globe's investigative journalism unit uncovered cases of widespread and systemic child sex abuse in the Boston area by numerous Roman Catholic priests.

[33] Carattini et al. (2012) recommended measurements of Catholic sex abuse scandals to mitigate concerns on the endogeneity of private school enrolment.Dills and Hernández-Julián (2012) complement data compiled by `bishopaccountability.org` with Lexis-Nexis searches of major world publications and news wire services with each diocese's name and the words "sex" and "abuse". They find that negative publicity explains 5% of the decline in Catholic schools in the United States. In a follow-up study, Dills and Hernandez-Julian (2014) relate the Catholic sex abuse scandals to voting outcomes and preference for government provision of social services. Bottan and Perez-Truglia (2015) estimate the causal effects of the scandals on religious participation, religious beliefs, and pro-social behaviour. They conduct an event-study analysis that exploits the fine distribution of the scandals over space and time. Among other findings, they state that a scandal causes a significant and long-lasting decline in religious participation in the zip code where it occurs. Lastly, Hungerman (2013)'s contribution explores the possibility of religious populations shifting to other doctrines, and sheds novel empirical light on substitution between charitable activities. He considers the impact of the shock of the 2002 Catholic sex abuse scandal on religious participation and in particular how the clergy misconduct impacted non-Catholic religious affiliation.

together with Uruguay, they belong to the so-called "Southern Cone": the most prosperous geographic macro-region in Latin America. In terms of social, economic, and political progress, the Southern Cone has been considered the "emerging" area of South America. From a liberal point of view, the region is praised for its significant participation in the global markets.

Along the same lines, another peculiar feature of the Southern Cone is its relatively high standard of living. Argentina and Chile's ratings on the Human Development Index - 0.827 and 0.847 respectively - is the highest in the sub-continent and are similar to those of the richest economies in Eastern Europe, such as Slovenia, Poland, and Hungary. Despite somewhat troubling levels of income inequality, these countries display high life expectancy, access to health care and education, and relatively low fertility rates. In light of these characteristics, we advance that the estimated impact of concerns about child well-being for this set of countries represents a lower bound to what would be measured if we had data on the entire sub-continent. Moreover, in emerging markets the magnitude of the effect of interest could be attenuated by forward-looking considerations: non-myopic adults might be willing to tolerate the risk of an unhealthy environment for children if, later in life, their children will be able to thrive professionally, benefiting from favourable prospects.

5.1 Data Source on Child Abuse Scandals

In Argentina allegations of sexual abuse by clergymen started to surface in the 1990s and were published as major scandals around 2000 and in the years after. In early 2017, some Argentinian media outlets published a list of 62 well-defined cases reported since 2002. A different source announced that at least 16 priests and two nuns are under investigation in the penal, civil, or ecclesiastical jurisdictions for paedophilia complaints in ten provinces and for events that occurred between 1970 and 2016, according to the registry carried by the Survivors Network of Ecclesiastical Abuse.[34] Many Catholic sexual abuse cases are widely known in Chile as well, to the point that, in May 2018, Pope Francis intervened by summoning all the members of the Chilean Episcopal conference to Rome to discuss the sexual abuse scandal rocking the Chilean church.

As a matter of fact, these two countries are among the handful of states all around the world that have been subject to the systematic scrutiny of `bishopaccountability.org`, a website dedicated to consolidating and preserving all records of Catholic clergy misconduct unearthed by law enforcement and the legal system through depositions taken by lawyers and media reports.[35]

[34]Source: https://www.pagina12.com.ar/13078-no-abusaras-el-mandamiento-que-falta, January 8th 2017.

[35]This collection of allegations has been compiled referring to several journalistic sources (e.g. criminal investigation records, accounts from survivors and their families, etc.), as well as diocesan and religious order documents. The latter include the official list of clerics found guilty under canon or civil law that was posted by Chilean bishops. The collaborators of the Bishopaccountability website stored all updates of the list of guilty clerics even when the bishops later removed them from the clergy's own website. These documents have been released by the Press Office of the Chilean Episcopal conference and are titled *Executed condemnatory sentences in civil field for offences against children committed by people who were clergy at the moment of the crime* and *Canonical executed sentences for offences against minors who restrict the ministry to persons which were clergy at the time of the crime.* They are dated November 11th 2011, October 1st 2012, July 27th

Since we are interested only in accusations that had public repercussions and since media documents are considered "sufficient" information to be included in the Bishopaccountability records, we are confident that our data give a fairly complete account of the Catholic abuse scandals. However, we should stress that the compiled data are not related to the prevalence of paedophilia in general, but only the phenomenon within the Catholic church.

Starting from this source of information, we registered the dates of publication of online newspaper articles on alleged paedophiles in the Church. In order to build an instrument for the perception of child well-being, we rely on the number of scandals, i.e. *novel* cases of abuse of minors as reported in the lists published by `bishopaccountability.org`.

The date of each scandal is the precise date of the first newspaper article mentioning the *novel* case. We do not quantify or distinguish the various documents, nor the number of the articles for each case, nor the role (e.g. bishop, fray, etc.) or the gender of the molester. Hence, the list refers to every church official being publicly accused of sexual abuse for the first time, regardless of the outcome of the investigations.

As regards the location(s) mentioned in the relevant piece of news, it may be the case that the alleged abuser is foreign, or has moved abroad, or has died, or has retired to a secluded house. Similarly, the victim may have moved elsewhere. Given the level of detail of the source of information, we digitise the location of both the victim and of the accused at the time of the scandal, as we believe that the media resonance would affect the perceptions of the parishioners and neighbours in both regions. Hence, for robustness, we identify two main categories of scandals depending on the location of the accused priest and the victim at the time of reporting. More in detail, we link the scandals to the country-specific first level subdivisions categorized by GADM (Global Administrative Areas) and accordingly by the GWP survey.[36]

As exemplified in Table C.1 in Appendix C, we follow the criteria adopted in Bottan and Perez-Truglia (2015) to categorise scandals. Importantly, relying on a manually-compiled list rather than on web-scraped data, we are confident that episodes prosecuted abroad are accounted for only if the cases involved a local victim or a local abuser and were covered by the national press. Analogously, news related to similar episodes in other countries might be covered by the local press but are not included in our data-set, unless the priest belonged to a domestic diocese and is a missionary abroad. Finally, it should be noted that in the case that the two regions do not coincide, the same abuse case produces a duplicate observation at the date of the reporting (see bottom row of Table C.1), although this is not very common as shown in Table 8.

Please note that multiple charges against the same Church official would either pertain to different regions (as clergymen typically relocate and the abuse events occurred under very different circumstances)

2013, July 25th 2014, November 11th 2014, January 4th 2016, and June 29th 2018. In the most recent list 5 priests remain unnamed and were declared to have passed away.

[36]The list of regions is available at `https://gadm.org/maps/ARG_1.html` and `https://gadm.org/maps/CHL_1.html`.

or - if within the same region - only the earliest one would matter for the purposes of our analysis. However, in the construction of our instrument we do not discriminate between the two types of scandals.

Concerning the nature of child abuse scandals, another caveat is recommended. One might say that the instrument is characterised by strong serial correlation, as one victim pressing charges might induce a wave of other episodes being reported. We claim that the presence and the timing of the scandals in the press are sources of plausibly exogenous variation, since victims may not report the incident immediately (or at all) and/or sometimes the authorities decide to withhold facts from the press.

Summary statistics are included in Table 8, where the period under analysis and the importance of the phenomenon in each country are documented. In less than 20 years, 139 abuse events emerged in Chile and 110 in Argentina.[37] The large majority of novel cases are counted once (item "Distinct cases"), whereas in a few instances another region has been impacted by the scandal given that the complainant was living elsewhere at the time of the reporting (item "Type B"). Geographically, both central and remote areas witnessed some occurrences, with only a few regions apparently unharmed.

In light of the importance of the geographical reference of scandals, and as reported in Table 8, the earliest waves of GWP cannot be exploited in the instrumented regressions: observations with information on the region of residence have only been available since 2010 and 2011, for Chile and Argentina respectively. The emergence of scandals and the country-specific GWP waves are plotted in Figure 2.

5.2 Construction of the Instrument

We exploit the fact that, for each wave of the survey, the timing of interviews vary between individuals.[38]Hence, we build the instrument considering the individual time-varying cumulative effect of paedophilia cases up to the day of the interview. Considering that the perception of interest is quite general and surveyed in a given instant, we maintain that it might be influenced by the consolidated information available to the respondent. This implies that an increasing number of scandals do reduce the perceived well-being of children.

More specifically, we count the number of scandals over the period shortly before the respondent-specific date of the interview, assuming that a number of episodes of child abuse would need to occur in order for the perceptions of child well-being to be impacted. We also suppose that individuals are influenced by events taking place both in their region of residence and in other regions. More precisely,

[37]It should be noted that the list of abuse cases is in constant update by journalists, particularly for 2019. The data utilised in this paper refer to cases of public knowledge in September 2019. For the soundness of the empirical analysis this does not pose an empirical threat, since the last survey wave we examine was recorded in 2015; nonetheless, we acknowledge that measurement error might contaminate the falsification test of Table 13. This would be the case if, in 2019, a large number of cases emerged without being detected by the press and/or by the editors of the website.

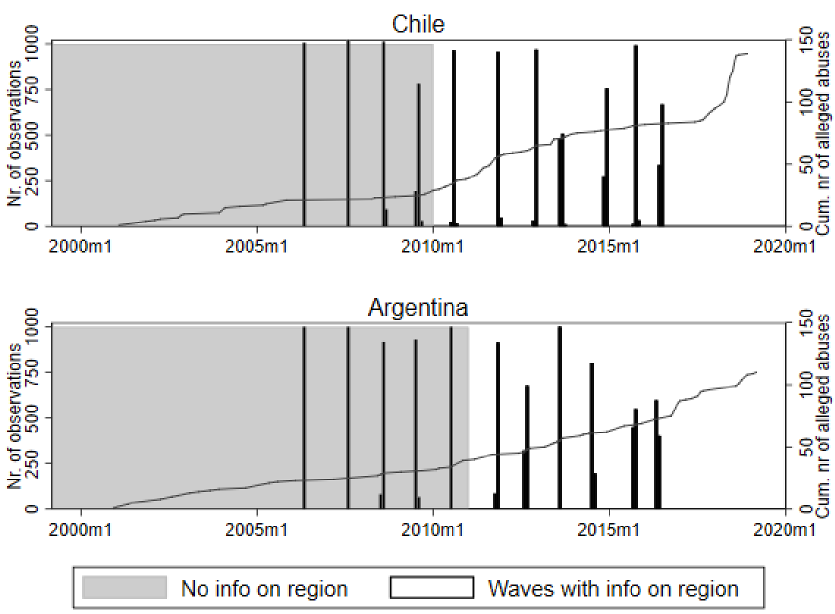
[38]For example the Gallup wave 9.1 was held entirely in 2014 but over different months: it took 2 months to be completed - during July and August in Argentina while in Chile it was during December and November. In that specific wave, the number of interviews per day ranges from one person to 63.

Table 8: Summary statistics on scandals data

	Chile	Argentina
Panel A		
Interviewees with info on region		
Sample size aged 25-64	3,754	2,919
First observation	Jul 2010	Oct 2011
Last observation	Nov 2015	Oct 2015
Panel B		
Scandals		
Number of scandals	139	110
Distinct cases	126	97
Type A	127	94
Type B	12	16
Over time		
First scandal	01 Feb 2001	05 Dec 2000
Last scandal	12 Jul 2018	18 Mar 2019
Number scandals before GWP	34	44
Number scandals during GWP	48	28
Number scandals after GWP	57	38
Across regions		
Number of regions in GWP	15	22
Number of regions with at least one scandal	15	19

Panel A: Descriptive statistics refer to the estimation sample. Panel B: Data on scandals refer to cases of public knowledge in September 2019. Source: data digitised by the authors and based on the website `bishopaccountability.org`.

Figure 2: Overlap between child abuse scandals and Gallup waves



Source: data digitised by the authors and based on the website `bishopaccountability.org`. They refer to cases of public knowledge in September 2019.

the instrument is defined as follows:

$$exposure_{t,r,n} = \sum_{k} \left\{ \frac{1}{1 + Dist(r,k)} \sum_{\tau=t-n}^{t} scandal_{\tau,k} \right\}$$

where $scandal_{\tau,k}$ is a dummy indicating whether at the date τ and in the region k there has been a novel paedophilia case, i.e. about a priest that had not been accused before. Note that $\frac{1}{1+Dist(r,k)}$ is the inverse geodetic distance between the capital of region r in which the respondent is based and the capital of the region k in which the scandal took place. Cases happening in the region of the respondent accrue with the maximum weight, that is unity. By the same logic, we assume that scandals occurring in closer regions have stronger effects on perceptions. Then, we sum up all the novel paedophilia cases in the past n months before the Gallup interview in date t. Following Bottan and Perez-Truglia (2015), our preferred duration over which we sum the novel scandals is 3 years (i.e. $n = 36$). Section C.2 in the Appendix proposes an alternative definition of the instrument in which the importance of the scandal is depreciated with time: the further back in the past the piece of news appeared in the media, the smaller its weight in the summation.

The exclusion restriction requires that, conditional on geographic and time fixed effects and on the set of individual controls, paedophilia cases are uncorrelated with characteristics and shocks influencing migration intentions other than individual perceptions of child well-being. We maintain that this requirement is likely to hold in our context. In particular, we believe that the timing of the scandals is an exogenous shock. The patterns of reporting sexual abuse can in fact also depend on the exogenous Vatican and judicial response to the reporting of child abuse, with a contagion effect in all Catholic countries.

Still, one might suggest that the probability of public accusations against priests depends on the quality and trust of both national and local institutions, which may also affect migration intentions. To be exhaustive, in our regressions we include country fixed effects to control for the quality of national institutions. In addition, in Section 5.3, we show that our IV results are robust when we control for additional covariates such as the *national institutions index*, the *law and order index*, or *confidence in the local police*.

Lastly, given that the scandals might have an effect on other individual perceptions (such as generalised trust), which in turn might affect migration intentions, we stress that our baseline estimations include a variable capturing the level of satisfaction with the general standard of living.[39] Unfortunately, GWP does not have a variable capturing the individual level of generalised trust. Please refer to Table 12 for robustness tests related to this matter. However, we point out that Bottan and Perez-Truglia (2015) find that scandals do not have significant effects on individual generalised trust, so we expect similar results

[39] Economic literature has generally pointed out a positive correlation between social capital, trust, and overall satisfaction. See for instance Bjørnskov (2003)

to apply in our own context.

Before moving to the related findings, some additional considerations on the appropriateness of the instrument are in order. We acknowledge the implicit assumption of news spreading easily throughout the country. It is safe to assume that news is indeed circulated in some format, especially news being of a wide interest the general public both at the local and at the national level, such as paedophilia scandals, and are likely to be equally covered by different media channels. Hence, the underlying primary source for our instrument - i.e. online newspaper articles - is a good proxy for the overall coverage. Moreover, we claim there is no technological divide: respondents are sampled from middle-income countries where technology has reached most remote villages. This is also due to the advent of smartphones, which has greatly reduced communication costs, thereby allowing individuals to send and to obtain information quickly across urban-rural and rich-poor areas. As a matter of fact, in the database at hand the percentage of people that are completely disconnected is negligible (i.e. 1,2% of individuals in both countries).[40]

5.3 IV Estimates

We estimate our equation using a (weighted) two stage least square (2SLS) regression.[41] Table 9 shows our baseline estimates, without accounting for covariates. The first stage results are displayed in col. 1. They show that the $exposure_{t,r,n}$ has a negative and statistically significant impact on our indicator of perceptions of child well-being, suggesting that scandals do reduce the perceived well-being of children. Since the associated F-statistic is above the critical value of 10, we reject the hypothesis that our instrument is weak.

The reduced form results are presented in col. 3: the estimated coefficient of $exposure_{t,r,n}$ is positive and statistically significant, thus indicating that more scandals increase the probability of intending to emigrate permanently abroad. Finally, col. 2 shows the second stage results. They confirm our previous analysis and show that an increase in the perceived well-being of children decreases the probability to move abroad.

[40] While we do not interact the article count with a variable capturing media access at the individual level, as this would violate the exclusion restriction, we do include among the covariates variables related to personal ability, wealth, and availability of modern technologies in the household, which may be strongly associated to the migration decision. More precisely, we control for educational level and the wealth composite index, which in turn includes television ownership and internet access, among other such things. Even if with contradictory results, the economic literature has extensively emphasised the role of media on internal and international migration. For instance, the exogenous staggered expansion of TV broadcasting in Indonesia allowed Farré and Fasani (2013) to identify the causal effect of media exposure on individual migration decisions. The fact that, in increasing the quality and quantity of accessible information in Indonesia, we observe a reduction in internal movements suggests that - in the absence of information - Indonesian citizens were over-estimating the net gains from internal migration. Aker et al. (2011) find that mobile phone technology in Niger has a positive effect on seasonal migration by increasing information about the labour market at the target destination. Finally, abstracting from the decision to move elsewhere, findings from Keefer and Khemani (2014)'s natural experiment in village-level radio access show that households with greater exposure to media make larger private investments in the education of their children in northern Benin. As regards the most suitable exogenous instrument, a vast literature has predicted media access using broadband deployment, or radio/television station parameters and topographic characteristics to determine which areas receive a signal and at what strength. See DellaVigna and Kaplan (2007), Durante et al. (2019), and Armand et al. (2020) to name a few featuring exogenous variation in access to the news. In our case, data limitations on the exact location of respondents prevent us from replicating this strategy.

[41] Please note that analogous IV estimations using a (weighted) IV probit (twostep) have been tested and lead to similar findings.

Table 9: Instrumented estimates - 2SLS

	Perceptions of ch. well-being	Intending to migrate	
Exposure	-0.0354***		0.0066***
	(0.01)		(0.00)
Perceptions of ch. well-being		-0.1853***	
		(0.05)	
Country FE	yes	yes	yes
Year FE	yes	yes	yes
Observations	6673	6673	6673
F-stat		18.71	

Notes: this table presents the coefficients obtained with the IV approach implemented on the Argentinian and Chilean sample. Robust standard errors are clustered by region and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Table 10 adds traditional covariates to the above specification. The F-stat is always greater than 10, suggesting that we do not have a weak instrumentation problem. With respect to the second stage, we find that (instrumented) perceptions of child well-being have a statistically significant negative effect on the intention to move abroad permanently.[42]

The magnitude of the instrumented coefficient is interpreted in relation to the (linear probability model) baseline non-instrumented results for the same estimation sample, as shown in Table C.3 in the Appendix. In absolute terms, the estimated coefficient is greater than the OLS estimated coefficient. The endogeneity was leading to an underestimation of the effects of child well-being on migration intention. While it is difficult to understand which the main cause of the bias is, this result might suggest that reverse causality could indeed be an issue in our estimation; that is, once individuals start thinking about migrating, their reference point on child well-being could become less optimistic. Attenuation bias due to measurement errors of the main independent variable could also play a role.

Considering col. 3, we find that one standard deviation increase in the composite indicator on child well-being on average leads to a decrease of 5.2 percentage points in the probability of being an aspiring migrant. Interestingly, having a network abrod increases this probability by 11.3 percentage points.

Instrument validity and robustness checks. Additional checks are appropriate to validate the robustness of our findings. First, in order to test the sensitivity of our first stage's relation to the specific definition of the $exposure_{t,r,n}$ indicator, we modify the length of the time window over which novel scandals are summed up. In fact, the recollection of such scandals might vary depending on the

[42]Throughout our IV analysis, we cluster the standard errors at the regional level. While allowing for intra-region serial correlation seems the most suitable approach to get correct inference, one may worry about the small number of clusters (we have 37 clusters). As a robustness, we used the Wild bootstrap approach for correcting the standard errors with a limited number of clusters (Cameron and Miller, 2015). Wild–bootstrapped t-statistics/p-values are calculated with the boottest Stata command, using the standard number of bootstrapping repetitions (999 repetitions) and the default Rademacher weights. P-values are 0.0260; 0.0881; and 0.0871 for col. 1, col. 2, and col. 3 of Table 10, respectively. These results suggest that in our setting the relatively low number of clusters does not seem to affect our inference much.

Table 10: Baseline instrumented estimates

	(1)	(2)	(3)
Perceptions of ch. well-being	-0.1853***	-0.1370**	-0.1364**
	(0.05)	(0.05)	(0.05)
Married		-0.0398***	-0.0364***
		(0.01)	(0.01)
Female		-0.0513***	-0.0506***
		(0.02)	(0.02)
Age		-0.0050***	-0.0051***
		(0.00)	(0.00)
Urban		0.0233	0.0231
		(0.02)	(0.02)
Network abroad		0.1122***	0.1131***
		(0.01)	(0.01)
Education level (dummy for high)		0.0455***	0.0491***
		(0.02)	(0.02)
In employment		-0.0075	-0.0047
		(0.01)	(0.01)
Ln (1+ pc household income)		-5.7747	-9.3541
		(17.72)	(15.21)
Ln (1+ pc household income squared)		2.8828	4.6731
		(8.86)	(7.60)
Wealth index			0.0037
			(0.01)
Satis. with standard of living			-0.0402**
			(0.02)
Country FE	yes	yes	yes
Year FE	yes	yes	yes
Observations	6673	6673	6673
F-stat	18.71	12.85	12.41

Notes: this table presents the coefficients obtained with the IV approach implemented on the Argentinian and Chilean sample. Robust standard errors are clustered by region and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

timing and the frequency of the news. On the one hand, recent facts are more relevant and influential in building one's opinion; on the other hand, persisting news might consolidate the feeling of lack of safety for children. Table 11 shows that our instrument becomes weak if we consider very long time-windows.[43]

Table 11: Instrumented estimates by different time span of the exposure

	(1) 3 years	(2) 4 years	(3) 5 years	(4) 6 years
Second stage				
Perception of ch. wellbeing	-0.1364**	-0.1457**	-0.1640**	-0.1836**
	(0.05)	(0.06)	(0.07)	(0.08)
First stage on perceptions of ch. well-being				
Exposure	-0.0295***	-0.0210***	-0.0155***	-0.0121***
	(0.01)	(0.01)	(0.01)	(0.00)
Demographic controls	yes	yes	yes	yes
Network abroad	yes	yes	yes	yes
Satis. with standard of living	yes	yes	yes	yes
Wealth index	yes	yes	yes	yes
Country FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	6673	6673	6673	6673
F-stat	12.41	9.99	8.35	6.97

First- and second-stage regressions for alternative definitions of the instrument. Scandals are summed up over increasingly long time intervals.
Robust standard errors are clustered by region and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Secondly, the exclusion restriction requires that the instrument affects migration intentions only by influencing child well-being, conditional on controls. With the goal to further validate the adopted identification strategy, it is worth reflecting on alternative mechanisms through which the scandals might affect one's decision to migrate elsewhere. Cases of paedophilia might be correlated with omitted factors, other than perceptions of child well-being, which may affect migration intention. It is possible that widespread child abuse cases shake the social foundations of the society in general, causing lack of trust in the community, in the authorities and/or in the local parish. This might hold for childless individuals

[43] When we consider abuse cases hitting the news in the 12 months preceding the survey, the instrumental variable is able to shock the perception of respondents, as it appears in the coefficient of the first stage, but the effect of the scandals is not statistically significant in the second stage (and in the reduced form). When the summation covers 2 years, coefficients and strength of the relationship are very similar to our preferred specification. Estimates are available upon request. In other words, we observe a "delayed influence": it seems that an accumulated influence of the scandals is self-reinforcing over time, to the point of affecting intentions to migrate. The analysis of Moghtaderi (2018) is helpful in interpreting this weaker short-time impact. In his paper, he also appraises the effect of negative publicity arising from public notices of child abuse allegations on the enrolment to Catholic schools. What he finds is that abuse prior to 2002 Boston Globe's scandal had no effect on enrolment, yet reports after the major scandal had a long-lasting effect that explains about two-thirds of the decline in Catholic schooling. He then borrows the psychological findings of MacLeod and Campbell (1992) to conclude that the extensive coverage of child abuse triggered a new awareness that eased later recallings and imaginings of these events, and consequently changed the perceived risk of future events for parents. The possible role of this expectation calls for additional testing, performed below in Section 5.3. Moreover, if we had data before 2002, it would be interesting to test whether the major Boston Globe's scandal managed to psychologically prime parents also outside of the United States.

or parents with older offspring, as the events might cast some doubts on their surroundings to the point of making individuals feel discomfited in that place. Alternatively, it is reasonable to think that scandals are observed in larger number where individuals trust more institutions and the judicial system.

Thus, in Table 12, we include to the baseline set of controls additional regional variables capturing the level of trust in several institutions.[44] We specifically control for the share of individuals in the region who state trust in the local police (col. 1) and the judicial system and courts of the country (col. 3), and this does not alter our main coefficient of interest to a large extent. Similarly, three composite indexes made available by the GWP are also added to the IV regressions to account for the regional views on institutions and social capital. Firstly, in col. 2, we include the *civic engagement index*, which is a proxy for social capital as it combines information on whether respondents have done any of the following in the past month: donated money to a charity, volunteered time to an organisation, or helped a stranger or someone they did not know who needed help. Then, in col. 4, the *national institutions index* equally weighs the respondents' confidence in the military, in the judicial system and courts, in the national government, and in the honesty of elections. Finally, the *law and order index* summarises people's sense of personal security and their experiences with crime and law enforcement (col. 5). Despite the inclusion of these additional controls, the estimates remain negative, statistically significant, and rather unaltered in magnitude.

Another concern is that news on child sex abuse perpetrated by Church officials can be especially salient to Catholics. If scandals decrease individuals' confidence in the Church, religious individuals might want to emigrate abroad in religion-friendly destinations, where they can trust more religious organisations. In col. 6 and 7, we also control for the religiosity of the respondent and their confidence in religious organisations, respectively.[45] Again, the inclusion of these variables yields similar results. Lastly, we include all of these additional controls simultaneously. The wave-specific average trust in various institutions does not play a particularly large role and the coefficient of interest remains stable.

[44]We aggregate individual variables at the regional level, using sample weights.

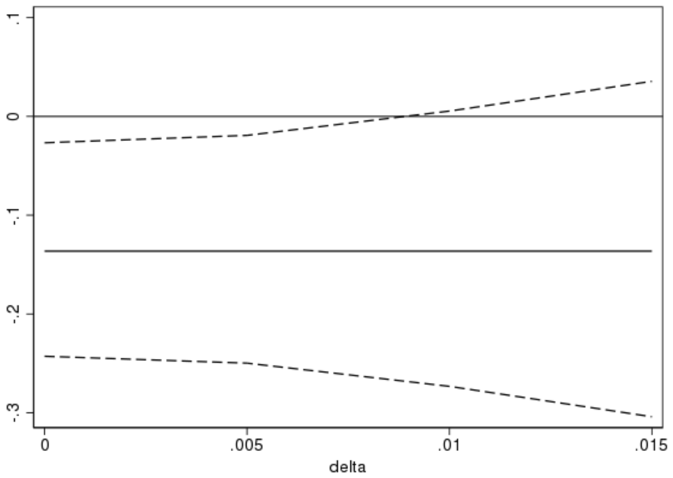
[45]We use the following two questions: "Is religion an important part of your daily life?"; "In this country, do you have confidence in each of the following, or not? How about religious organizations (churches, mosques, temples, etc.)?"

Table 12: Instrumented estimates - Additional controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Perception of ch. wellbeing	-0.1349**	-0.1605**	-0.1874**	-0.1815**	-0.1228**	-0.1441***	-0.1304*	-0.1799**
	(0.0660)	(0.0624)	(0.0774)	(0.0817)	(0.0564)	(0.0545)	(0.0684)	(0.0872)
Confidence in local police	0.0045							0.0347
	(0.0653)							(0.0952)
Civic engagement index		0.0014*						0.0000
		(0.0008)						(0.0011)
Confidence in judicial system			-0.1706*					-0.1066
			(0.1027)					(0.1387)
National institutions index				0.0021				0.0016
				(0.0015)				(0.0020)
Law and order index					-0.0008			-0.0010
					(0.0008)			(0.0016)
Religion important in daily life						0.0048		0.0225
						(0.0124)		(0.0140)
Confidence in religious organizations							-0.0132	-0.0151
							(0.0311)	(0.0358)
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes
Network abroad	yes	yes	yes	yes	yes	yes	yes	yes
Satis. with standard of living	yes	yes	yes	yes	yes	yes	yes	yes
Wealth index	yes	yes	yes	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Observations	6673	6673	6673	6673	6673	6599	4173	4132
F-stat	7.7287	11.4807	8.4458	8.5858	9.4739	12.1199	13.6629	11.8386

Robust standard errors are clustered by region and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Figure 3: Conley test of plausible exogeneity



Source: Authors' calculations on Gallup World poll data. Following the method developed by Conley et al. (2012), the figure displays the 95% confidence intervals in the presence of small violations of the exclusion restriction. The direct effect of the instrument on the intention to migrate is assumed to be normally distributed: $\gamma_2 \sim \mathcal{N}(0, \delta\beta)$

As a third robustness test, along the lines of Bottan and Perez-Truglia (2015), we check that our findings are robust to the construction of the *exposure* instrument relying on type-A scandals only. The inclusion or exclusion of type-B scandals does not alter our results, as displayed in Appendix C, Table C.5.

While the robustness tests discussed above are encouraging, we further check for the sensitivity of our results, allowing small deviations from the hypothesis that our instrument is excludable, applying the idea of plausible exogeneity introduced by Conley et al. (2012).

In our setting, that would mean $Inten_{ijtr} = \gamma_0 Perception_{ijtr} + \gamma_1 X_{ijtr} + \gamma_2 exposure_{trn} + \delta_j + \varphi_T + \epsilon_{ijtr}$ with $\gamma_2 \neq 0$. The so-called local-to-zero approach makes assumptions on the support of γ_2 and then runs 2SLS on different possible values of γ_2. Accordingly, we verify that our baseline estimates are robust to small deviations from the excludability condition of the instrument. We assume that the direct effect of *exposure* on the outcome of interest is normally distributed with a mean of zero and standard deviation equal to $\delta\beta$, where β is the IV effect of *perception* estimated in Table 10 and δ varies between 0 and 0.015.[46] We show in Figure 3 the 95% confidence intervals associated with all possible values of δ. These confidence intervals do not include zero for δ smaller than 0.009, supporting the view that our results are robust to small deviations from excludability.

Falsification Test. It might be put forward that our instrument is in fact spuriously picking up a generalised upward trend of widespread misconduct and violence against children rather than specific abusers being exposed. This would undermine the appropriateness of the chosen definition and should be checked. Similarly, there might be a mounting mistrust of the local (religious) community motivated

[46] Note that we consider the absolute value of β.

by the scandals that have not yet reached the press. Given that individuals may be non-myopic in this respect, it is important to verify whether the parental perceptions mirror pessimistic expectations of future scandals. To that end, a falsification test is proposed. We instrument the perceptions of child well-being with an indicator of exposure to future news about child abuse. In the first two columns of Table 13, the exposure instrument is replaced with its analogue lead in the 4 years following the date of the sampled interview. In the specification displayed in col. 3 and 4, the "placebo" instrument is computed slightly differently: the summation of future scandals does not add those within the first year since the interview, the idea being to not count possible paedophilia-related shocks foreseeable by the respondent. As indicated by the F-statistics, the absence of any predicting behaviour of the future exposure to scandals on present replies about infant well-being is reassuring about the true link between the instrument and the endogenous variable of interest. Confirming this fact, we also examine the reduced-form relationship and find no statistical significance.

Table 13: Falsification test with leads of the exposure to the child abuse scandals

	(1)	(2)	(3)	(4)
	Next 1st, 2nd, 3rd, and 4th year		Next 2nd, 3rd, and 4th year	
	2SLS	Reduced form	2SLS	Reduced form
Percep. of ch. well-being	-0.2467		-0.2657	
	(0.15)		(0.19)	
Exposure in the future		0.0021		0.0020
		(0.00)		(0.00)
Demographic controls	yes	yes	yes	yes
Network abroad	yes	yes	yes	yes
Satis. with std of living	yes	yes	yes	yes
Wealth index	yes	yes	yes	yes
Country FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	6673	6673	6673	6673
F-stat	6.01		4.63	

Robust standard errors are clustered by region and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

6 Conclusions

Relying on the notion of intergenerational altruism or "warm glow" -that is, of parents being motivated by the direct utility they receive from the act of providing for their own children-,this study empirically looks at child well-being as a critical determinant of parental migration intentions.

We quantify to what extent perceived child well-being fosters individual migration intentions, taking advantage of a large individual-level cross-section survey, the Gallup World Poll, covering Latin American and Caribbean countries between 2009 and 2015. Thanks to this unique dataset providing perceptions towards the conditions of children, the latter are identified as important migration push factor aside from traditional individual covariates. The novelty of our contribution stems from empirically looking at child well-being as a critical determinant of parental intentions to move permanently out of the country of origin.

Additionally, extensive robustness checks are performed, and - focusing on Chile and Argentina - we apply an instrumental variable approach, thus mitigating concerns about potential threats to identification posed by unobservables, reverse causality and measurement issues. Exploiting the repeated cross-section nature of the survey and collecting original data on reported cases of violence against children, we instrument perceptions of child welfare with the timing of local scandals concerning sexual abuse of minors.

In conclusion, our findings shed light on the relationship between child conditions and the size of potential migration. From the standpoint of development policies, this empirical analysis also underlines the role of public spending priorities that target the protection of children, caring for their health care, nutrition, and education. Even in countries where the basic needs are not endangered, young adults might perceive a lack of safe conditions for their offspring and move abroad looking for better opportunities to raise it.

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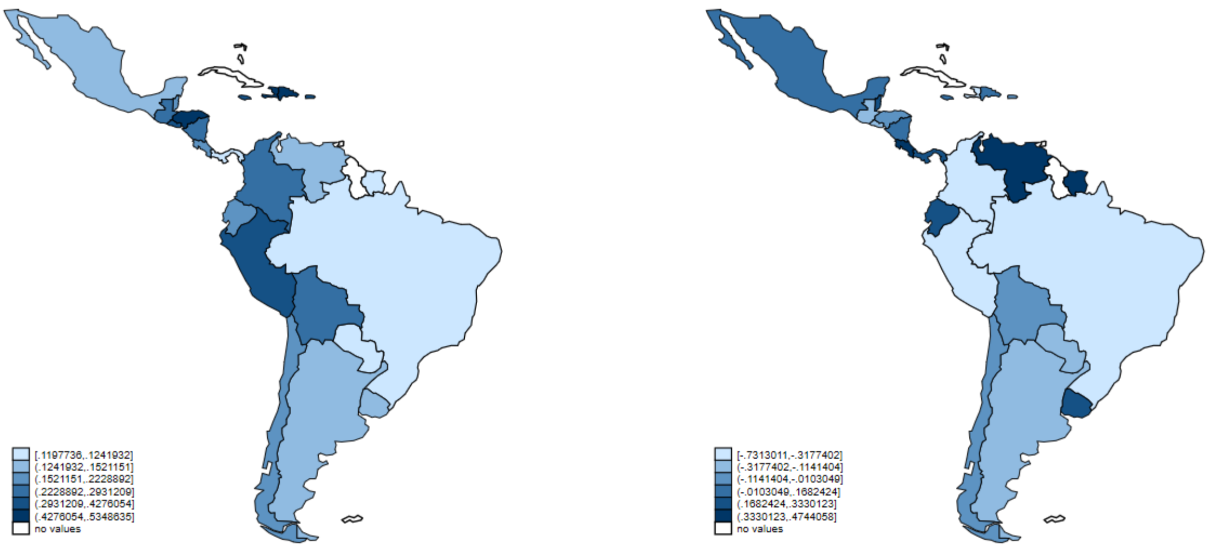
Appendix A Data Description

This appendix contains descriptive evidence on the variables of interest for this study. Figure A.1 illustrates the average values taken by the two main variables of interest. The figures are computed on the baseline sample, pooling all the waves, and taking the weighted mean at the country level. In Figure A.1a, we observe that the countries in which a large share of the sample is willing to migrate permanently (up to 53% of the population within the age-limit) are Peru, Honduras, Haiti, Puerto Rico, Jamaica, and the Dominican Republic. In Figure A.1b, the variation in the average composite indicator on the perceptions of adults on the well-being of children is displayed. The darker the shade, the more positive perceptions are. The countries in which the population states negative opinions are Haiti, Peru, Brazil, Colombia, Paraguay, Guatemala, El Salvador, and Argentina.

Figure A.1: Cross-country variation in average intentions to move and perceptions of child well-being

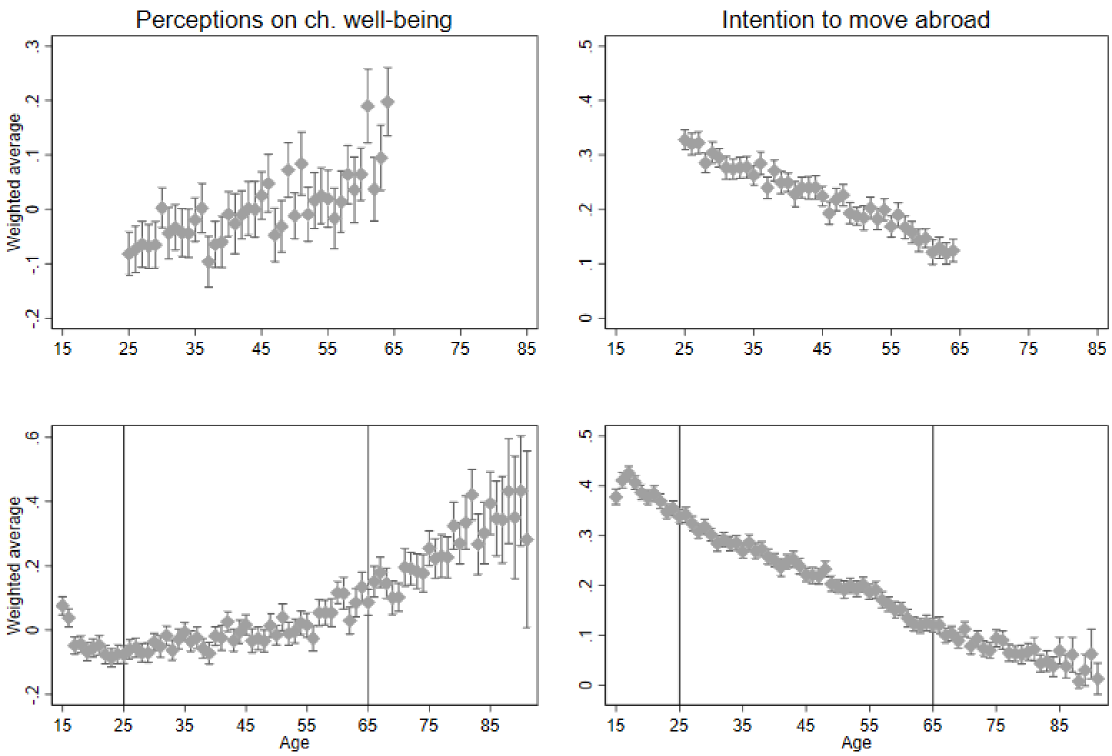


(a) Intention to migrate permanently

(b) Perception of child well-being

Next, aggregating survey data, we inspect the distribution of the dependent and independent variable of interest by age to show how they vary in the different cohorts of respondents. The scatter plots of Figure A.2 exhibit the weighted age-specific average of those variables with their 95% confidence intervals. The upper pair of graphs represents the baseline sample of the empirical analysis, selected on the basis of age and the availability of data in all the variables. The bottom pair of graphs regards the universe of respondents to the GWP for Latin America and the Caribbean countries. At a glance, it seems that the relationship between age and the variables of interest is smooth and, most importantly, the sample selection does not introduce a distortion with respect to the universe of respondents.

Figure A.2: Distribution of dependent and independent variable by age



95% confidence intervals for the means. In top graphs: sample of the empirical analysis. In the lower pair: universe of respondents.

Appendix B Supplementary Tables

As is standard in the literature, we contrast the magnitude of the probit coefficients with the two alternative models for binary dependent variables. As a robustness test of the baseline estimation, we confirm that the correlation between intention to migrate and the composite indicator of child well-being in the country is not altered by the implemented econometric method (Table B.1).

As indicated in the main text, in Table B.2 we test the robustness of our estimates against the inclusion of indicators of intergenerational mobility. They correspond to a set of cohort-specific indicators summarising the extent to which the respondents' generation has witnessed social mobility compared to the parental one, as was computed by Neidhöfer et al. (2018). They calculated time series for several indexes of relative and absolute intergenerational education mobility for 18 Latin American countries over 50 years. Indexes have been estimated using a rich set of harmonised household data like the Latinobarometro, as well as other national household surveys of the single countries. None of these intergenerational factors seem to significantly affect the probability of being an intended migrant, as the coefficient of interest remains stable.

Table B.1: Baseline estimates - Alternative estimation models

	Lpm	Probit	Logit
Perception of ch. well-being	-0.0330***	-0.0353***	-0.0343***
	(0.0032)	(0.0038)	(0.0039)
Demographic controls	yes	yes	yes
Network abroad	yes	yes	yes
Satis. with standard of living	yes	yes	yes
Wealth index	yes	yes	yes
Country FE	yes	yes	yes
Year FE	yes	yes	yes
R2	0.1340		
Adjusted R2	0.1335		
McFadden's R2		0.124	0.123
McFadden's adjusted R2		0.123	0.122
Observations	68522	68522	68522

Average marginal effects. Robust standard errors are clustered by country and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Table B.2: OLS estimates: intention to migrate on perception of child well-being. The role of intergenerational mobility

	(1)	(2)	(3)
Perception of ch. well-being	-0.0327***	-0.0328***	-0.0327***
	(0.0033)	(0.0034)	(0.0033)
Parental years of schooling	-0.0036		
	(0.0053)		
Resilience high class		-0.0110	
		(0.0209)	
Intergenerational Mobility			0.0284
			(0.0174)
Demographic controls	yes	yes	yes
Network abroad	yes	yes	yes
Satis. with standard of living	yes	yes	yes
Wealth index	yes	yes	yes
Country FE	yes	yes	yes
Year FE	yes	yes	yes
Observations	65465	64979	65465

This table includes additional variables of intergenerational mobility, as provided in Neidhöfer et al. (2018). In col. 1, we account for the average number of years of schooling for the people who became parents in the year of birth of the respondent. In col. 2, we control for the predicted probability of upper class persistence. In col. 3, intergenerational mobility is measured as the slope coefficient from a linear regression measuring the number of years of schooling for children based on the years of education of their parent with the highest degree and captures absolute changes - e.g. because of national educational expansions - as well as relative movements of families within the distribution. The higher the index, the lower social intergenerational mobility is. Robust standard errors are clustered by country and reported in parentheses. Sample weights are applied in the estimation. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Appendix C Supplementary IV Materials

Before devoting additional graphs and tables to the instrumented analysis, Figure C.1 illustrates the age distribution of respondents to the two questions on parenthood in Chile and Argentina. Even in this context, by narrowing down the sample to working age population we ensure that we are most likely focusing on parents.

Table C.1: Types of scandals depending on locations at the time of reporting.

Type	**Where it is relevant**	**Location victim**		**Location accused**	
		Arg or Chl	Abroad	Arg or Chl	Abroad ($*$)
A	Reg. X	Reg. X		Reg. X	
A	Reg. Y		Reg. Z	Reg. Y	
B	Reg. X	Reg. X			Reg. Z
B A	Reg. X Reg. Y	Reg. X		Reg. Y	

C.1 Data Validation of Scandals

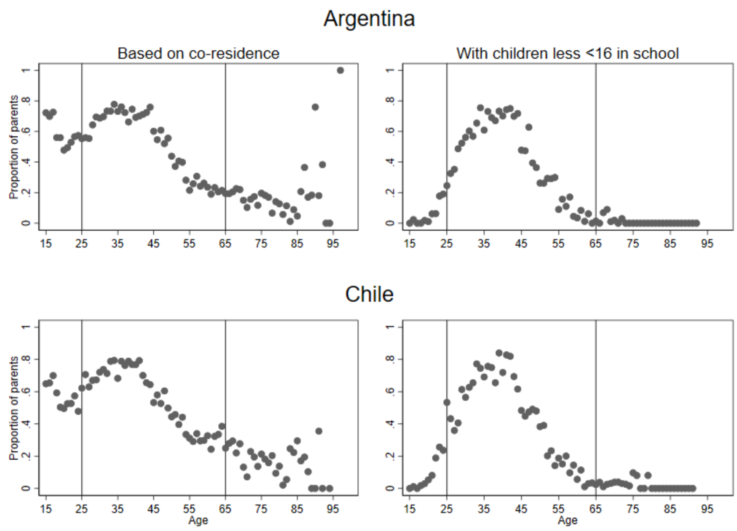
Compared to other data collection techniques, we cannot claim that the measured occurrences are exhaustive, as we are relying on a manually-compiled list. On the other hand, contrasting the method with web scraping algorithms, we are confident that false negatives are minimised. False negatives may occur when, for example, articles abroad are not referring to scandals in the countries of interest.The opposite case - in which a foreign newspaper is running a domestic case - is correctly accounted for since the international resonance is indeed proportional to the local relevance of the scandal.

In order to validate the source of data, we compare it with a similar figure, collected at the national level rather than at the regional one.[47] We make use of a web search performed with Europe Media Monitor, a tool developed by the Joint Research Center of the European Commission that scans news reported by the world's online media.[48] In particular, web scraping has been used to count daily all the online newspaper articles containing given keywords spanning from 2005 - 2019. Given that accused priests have often been transferred from their original parish, the reach of the scandal and the judicial follow-up might have scaled up. Therefore this figure is geo-referenced only at the country-level, including both national and local media outlets. In other words, it tracks the domestic intensive margin of the

[47] We believe that in such vast countries this national proxy is too generic to shape individual perceptions. Hence, national aggregates are only used qualitatively, excluding them from the empirical analysis.

[48] Monitoring thousands of news sources in over 70 languages, the system uses advanced information extraction techniques to determine automatically what is being reported in the news. It detects and classifies articles as they appear in the media without any human intervention, as software algorithms generate it automatically. This service ensures a very thorough search, as the full text of the article is checked, not just the headline and abstract. It covers media outlets at both the national and the local (and regional) levels by targeting almost 700 different sources for 18 Central and South American countries in their national language. For Argentina, it reviews 69 newspaper, 33 for Bolivia, 121 for Brazil, 39 for Chile, 36 for Colombia, 13 for Costa Rica, 27 for Dominican Republic, 25 for Ecuador, 23 for El Salvador, 25 for Guatemala, 10 for Honduras, 76 for Mexico, 24 for Nicaragua, 24 for Panama, 29 for Paraguay, 35 for Peru, 23 for Uruguay, and 56 for Venezuela.

Figure C.1: Age distribution of parents - Chile and Argentina



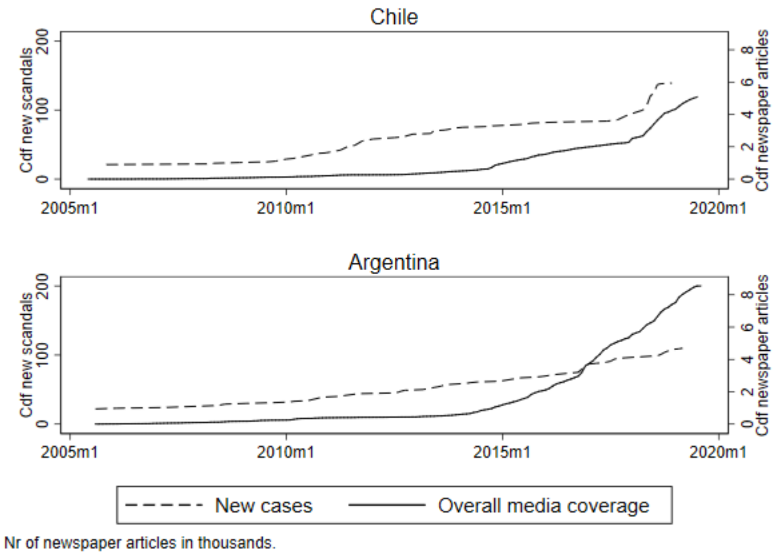
child abuse crisis. Table C.2 displays the keywords and logic operators used in the query.[49] As it appears in Figure C.2 from the time series of the total new scandals aggregated at the national level and the time series of the overall media coverage of the phenomenon, at first the increase of the online newspaper articles mentioning relevant facts is proportional to the number of new cases appearing. This might be due to a limited diffusion of the internet, or to scandals being considered news-worthy only by local media outlets. After a few years, as the issue has become of public domain worldwide and has reached the ecclesiastical hierarchy, the web press devotes more and more attention to it.

Table C.2: Search criteria for newspaper articles

Language	**Countries**	**Keywords**
Spanish	Argentina, Bolivia, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay, Venezuela	[acusa* | presunto* | denuncia* | víctim* | arrest* | condena* | delito* | imputa* | proces*] & [men|* | niñ* | chic* | adolescent* | infant* | monaguillo*] & [clero | Obispo | cura* | párroco* | sacerdote* | dióces* | clérig* | capellán* | parroquia*] & [sexual* | abus* | pedofil* | pederast* | viola*]
Portuguese	Brazil	[acusa* | suposto* | denuncia* | vítima* | prende* | condena* | delito* | imputa* | proces*] & [menor* | menin* | garot* | criança* | adolescent* | infant* | coroinha*] & [clero | bispo | pastor* | sacerdote* | dioces* | clérig* | capelão* | paróquia*] & [sexual* | abus* | pedófil* | pederast* | viola*]

[49] In performing the search of online newspaper articles, the keywords have been selected with the goal of minimising false positives. Please see Table C.2 for the the language-specific search criteria adopted for the newspaper-scraping task.

Figure C.2: Trend of cumulated new alledged abusers and national media coverage



C.2 Non-instrumented estimates on the reference IV sample

Table C.3 provides the results of interest estimated with simple Linear Probability, Logit, and Probit models on the estimation sample of Table 10. These coefficients are used for reference when inferring the magnitude and direction of the bias in the IV analysis.

C.3 Alternative definition of the exposure instrument

In this sub-section we study the sensitivity of the instrumented results to the formula adopted to compute the exposure indicator. It might be the case that - in one's mind - child abuse cases learnt about from the news are not all of equal relevance or importance to individual respondents. On the contrary, it is reasonable to assume that memories further in the past weigh less than recent ones. Table C.4 provides the results of an estimation with an alternative version of the instrument in which distant memories are depreciated according to the following formula:

$$exposure_{t,r,n} = \sum_k \left\{ \frac{1}{1 + Dist(r,k)} \sum_{\tau=t-n}^{t} (\gamma - \frac{\gamma}{n}(t-\tau)) * scandal_{\tau,k} \right\}$$

In this definition the memory of the scandal depreciates over time linearly. The slope of the decline is set so that the very last period considered in the rolling sum has zero weight. For simplicity, γ is equal to 1. As displayed in Table C.4, the results obtained with this instrument mimic those of Table 11, even if the F-statistic is slightly smaller.

Table C.3: Baseline non-instrumented estimates on the instrumented sample

	Lpm	Probit	Logit
Perception of ch. well-being	-0.0293**	-0.0303***	-0.0279***
	(0.0015)	(0.0080)	(0.0086)
Demographic controls	yes	yes	yes
Network abroad	yes	yes	yes
Satis. with standard of living	yes	yes	yes
Wealth index	yes	yes	yes
Country FE	yes	yes	yes
Year FE	yes	yes	yes
R2	0.092		
Adjusted R2	0.089		
McFadden's R2		0.100	0.100
McFadden's adjusted R2		0.094	0.094
Observations	6673	6673	6673

Notes: this table presents the marginal effect coefficients obtained with the baseline specifications on the Argentinian and Chilean sample. Robust standard errors in parentheses are clustered at the region level. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value.

Table C.4: Instrumented estimation with alternative definition of the instrument

	3 years	4 years	5 years	6 years
Second stage				
Perception of ch. well-being	-0.1155**	-0.1285**	-0.1368**	-0.1451**
	(0.06)	(0.06)	(0.06)	(0.06)
First-stage on perceptions of ch. well-being				
Exposure (with depreciation)	-0.0528***	-0.0421***	-0.0344***	-0.0284***
	(0.02)	(0.01)	(0.01)	(0.01)
Demographic controls	yes	yes	yes	yes
Network abroad	yes	yes	yes	yes
Satis. with standard of living	yes	yes	yes	yes
Wealth index	yes	yes	yes	yes
Country FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Observations	6673	6673	6673	6673
F-stat	11.85	11.66	10.79	9.46

Notes: this table presents the coefficients obtained with the IV approach implemented on the Argentinian and Chilean sample. Robust standard errors in parentheses are clustered at the region level. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value

Table C.5: Instrumented estimation without accounting for Type B scandals

	(1)	(2)	(3)
Perception of ch. wellbeing	-0.2026***	-0.1569**	-0.1576**
	(0.05)	(0.06)	(0.06)
Demographic controls	no	yes	yes
Network abroad	no	yes	yes
Satis. with standard of living	no	no	yes
Wealth index	no	no	yes
Country FE	yes	yes	yes
Year FE	yes	yes	yes
Observations	6601	6601	6601
F-stat	15.10	10.20	10.09

Notes: this table presents the coefficients obtained with the IV approach implemented on the Argentinian and Chilean sample. Robust standard errors in parentheses are clustered at the region level. Significance level: *0.10>p-value ** 0.05>p-value*** 0.01>p-value

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