

Universidad de San Andrés Departamento de Economía

Maestría en Economía

# How to measure the long run effects of social spending: a general proposal<sup>1</sup>

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Buenos Aires 2 de febrero, 2021

 $<sup>^1{\</sup>rm This}$  is the result of research done in the Independent Research course supervised by Prof/James Foster, and multiple discussions with Mariano Tommasi. I am extremely grateful to both of them. I also thank Nora Lustig and the CEQ Institute for providing insights of their methodology. All remaining errors are my own.

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### "Hacia una medición de efectos de largo plazo de políticas sociales: una propuesta preliminar"

#### <u>Resúmen</u>

El efecto a largo plazo de las políticas sociales rara vez es estudiado debido a la falta de datos de panel. Este trabajo presenta un nuevo enfoque para abordar este problema que combina transferencias monetarias con efectos a corto plazo ya estudiados y la metodología de paneles sintéticos. Recurrimos a la metodología del CEQI para identificar a los beneficiarios y asignar fondos.

#### Palabras clave

pobreza intertemporal, transferencias condicionadas, pobreza intergeneracional, paneles sintéticos, política social, largo plazo

## Universidad de

"How to measure the long run effects of social spending: a general proposal"

#### Abstract

The long run effect of social policies is rarely studied due to the lack of panel data. This work presents a new approach to deal with this problem that combines monetary transfers with already-studied short run effects and synthetic panel data. I rely on the CEQ Institute to identify beneficiaries and allocate funds.

#### Keywords

inter-temporal poverty, CCT, inter-generational poverty, synthetic panels, social policy, long term

Códigos JEL: H5, I3

## 1 Introduction

Short term effects of social programs on education and family outcomes have widely been studied for multiple countries and different transfer designs. Still, the study of the long term effects has been limited to a few programs that counted with panel data. One of the most famous study in this topic was the Perry program in the US, with which James Heckman showed the high impact of investing in early childhood and eventually created a whole new brunch in our science<sup>2</sup>. Nevertheless, no methodology also been developed to study the longer term effects of most of the social programs in developing countries, where panel data hardly exists.

In this work I present a methodological alternative to measuring the long term effect of policies in order to fill this void. The methodology consists of comparing the scenario in which beneficiaries receive the transfer (this means, replicate what really happened) against a scenario where the transfer that is being studying doesn't exist. To do so, I propose to combine the CEQ methodology to identify beneficiaries and impute transfers, with short-term non monetary effects found in previous analysis and synthetic panels to measure the evolution of income. Once both income estimations have been done, we can compare the poverty rate under both scenarios.

One could claim that the composition of a government's public spending represents its priorities and interests. Or should we say, what they want them to look like? Over the last few decades, the international community centered its discussion around poverty and inequality reduction, and this was reflected in government budgets all around the world. Even though the total spending (as % of GDP) rose in some countries of Latin America and decreased in others, in almost all cases the public spending on social policies increased. For instance, the social spending as % of GDP increased from 11.3% in 1990 to 14.3% in 1999<sup>3</sup>.

 $<sup>^2 \</sup>mathrm{See}$  Heckman (2006), Heckman et al (2010) or García & Heckman (2017)

 $<sup>^{3}</sup>$ Prado 2013

The process continued in the following decades, although at a slower rate. The social spending in the region as a whole increased from 46,5% of total spending in 2000 to 52,5% in 2018. Looking at the public spending as dollars per capita, the increase during this period was more drastic, of a 102% <sup>4</sup> (CEPAL 2019). Not only do the budgets reflect these intentions, but assistance programs from international organizations and institutions do this too. Within these programs, conditional cash transfers (CCTs), which we will discuss deeply later in this work, were the main enforced policy.

The economic community played a crucial role in this process. The research in the econ academia that studied topics related to poverty, inequality and public spending flourished to provide tools, answers and recommendations to the high demand in the public sector. Firstly, as Alkire and Foster (2007) state, the first decision that needs to be done is the mechanism to identify the poor in a specific society. In this, the definition of poverty itself in a given country is trivial. The concept of poverty was enriched from a one dimension problem, to a multidimensional one. Amartya Sen defines development as gains of different types of freedoms, and therefore poverty as a set of unfreedoms:

"Sometimes the lack of substantive freedoms relates directly to economic poverty, which robs people of the freedom to satisfy hanger, or to achieve sufficient nutrition, or to obtain remedies for treatable illnesses, or the opportunity to be adequately clothed or sheltered, or of enjoying clean water. In other cases, the unfreedom links closely to the lack of public facilities, social and health care, educational facilities, or of effective institutions for the maintenance of local peace and order. In still other cases, the violation of freedom results directly from a denial of political and civil liberties from authoritarian regimes and from imposed restrictions of the freedom to participate in social, political and economic life of the community"

<sup>&</sup>lt;sup>4</sup>starting as US\$464 and ending in US\$938 constant 2010 dollars

#### Development as Freedom, Amrtya Sen

Naturally, together with this wider definition of poverty, emerged the challenge to measure it. This was specially tackled by Amartya Sen and James Foster who were pioneers in developing alternative methodologies and indexes that combined multiple variables, and also by Angus Deaton, whose work also spanned on the properties of household surveys to best measure poverty. A central distinction in this sense, according to Sen (1979, 1982), is that poverty measures differ among each other with respect to the identification of the poor and the aggregation methods used to combine information about the poor into an overall poverty measure. The first characteristic is concerned with distinguishing the poor from the non-poor while the second feature of poverty measures refers to the way in which individual-level information of the poor is combined into a summary statistic.

With this call for consciousness and tools to measure and identify poor families, emerged the need to study up to what extension governments were redistributing resources<sup>5</sup>. One of the main analysis tools that emerged in this framework was the methodology and indexes that the Commitment to Equity Institute developed to measure the distributional impact of public spending. The CEQ Institute calculates the redistributive effect of direct taxes (to people) and direct transfers by comparing the Gini coefficient for disposable income with the Gini coefficient for market income (scenario in which contributory pensions are considered a transfer) or for market income plus pensions (scenario in which contributory pensions are considered a deferred income). For instance, the CEQI has found that in Latin America the average redistributive effect is equal to a decrease in the Gini coefficient of 2.8 percentage points. Not only have they been able to show an aggregate effect, but also shed light on which the key components are.

<sup>&</sup>lt;sup>5</sup>The literature related to poverty measurement is vast and addresses multiple topics. More on this disussion can be found in Ruggieri et al (2003) and Citro & Michael (1995). For specific methodological implications, see Cruces at al (2015) for cross-sectional measurements across countries; for the specific case of the USA Blank Greenburg (2008); for inter temporal and chronic poverty measurement Calvo & Dercon (2009) and Fisher et al (2013); for extreme poverty Ferreira et al (2016); for international measurements Jolliffe et al (2016)

Pensions and direct transfers are the most re-distributive policies en the region and make Argentina, Uruguay, and Brazil the countries with the highest level of public fund redistribution<sup>6</sup>.

Still, social programs are aimed not only to reduce poverty in the short run, but also to help beneficiaries **escape poverty in the long run** and not to depend on the government any longer. This means, help households to achieve an autonomous income level that guarantees them a income/consumption standard above the poverty line. Specially in programs aimed at children or at education, a higher future income depends on how much the social program helps to accumulate a higher human capital.

In an attempt to solve the lack of panel data in the study of the effects of social policies in the long run, we propose a methodology that combines existing impact evaluation, CEQ identification and allocation methodologies and synthetic panels.

Readers might correctly be relating this topic with the research propelled by James Heckman. It would be impossible to deny Prof. James Heckman's contribution not only in the research of Public Finance, but in raising the awareness of the importance of spending in children in all Econ and Policy environments. We pick this up in the next section, when making a brief summary of the main findings in Public Finance and CCT literature. The reason we analyze them is that both are the most related to long run effects and human-capital centered research areas. We conclude that even the insights they have provided are more than valuable to understand determinants and causes of human capital accumulation, there is still uncertainty on which policies are the most appropriate and effective to promote it in developing countries (specifically LAC).

Section three provides clarifications on three key components of the methodology we describe later. First, we discuss the outcome variable used to measure poverty and recommendations given by Angus Deaton depending on the data source the

 $<sup>^6\</sup>mathrm{For}$  more on inequality measurement, see Duclos & Araar (2006); Haughton & Khandker (2009) and Plotnick (1981)

researcher will use. Secondly, we provide the key features of synthetic panels and highlight the assumptions lying beneath it. Lastly, we describe the challenge of identifying beneficiaries of social programs, and introduce the CEQ methodology as a solution to it.

What follows in section 4 is the step by step of the general proposal with remarks for anyone who wishes to put them it in practice. Finally, section 5 displays the main limitations and upcoming improvements, and concludes.

## 2 What do we know so far about the long term effects of social spending?

Due to the lack of panel data, the analysis of long run effects of social spending has been limited. The brunch of Public Economics has been the most successful one in doing so, from a perspective of the efficiency in public spending.

Another brunch of economics that has tried to account for the long run effects of the social spending has been the development literature focused on CCTs. Why did this literature show a special interest in accounting for the long run effects? One might think of two main reasons for this. Firstly, as explained above, CCTs have increased its weight in the total social spending over time and managed to become the main policies in most of the LAC countries. Secondly, CCTs are usually conditioned on schooling, so they are expected to have an effect on human capital, that can only be measured in the medium and long run. In this section, I present a bief summary of the main findings and conclusions in both frameworks so far.

### 2.1 Public economics: investment in childhood

One of the main studies of public economics is the efficiency analysis of public spending. Therefore, it is easy to find studies that attempt to compare the spending per capita of some program/transfer, with the extra amount of taxes the state receives in the future thanks to a long run effect of the programs. From that perspective, James Heckman leads the analysis of investment in early childhood. Taking the Perry program as the center of his analysis, Prof. Heckman gave place to the flourishing of a whole area of research that stressed the importance of investing in early childhood, where programs might boost non-cognitive skills that allow for a higher capital accumulation in the future. Specifically for the Perry program, he found that while the government spent \$16,514 dollars per kid, it would then have a benefit of \$144,345 because of increases in taxes revenue and reduction in costs such as crime-related costs opportunities for high-return investment in children decline rapidly with age (Heckman,2006). This not only implies that the program payed for itself, but that it gave net discounted benefits for the government. Hoynes and Schanzenbach (2015, 2018) study the long run effects of safety net protections and also conclude that reallocation of investments to earlier stages of life can be efficiency-enhancing.

This evolved in the development of a theory of skill formation, easily understood as an individual's skill production function. The gains in productivity come from investment in the early years. Prof. Heckman found that rate of return dropped as later in life the transfer was given. This is best understood when observing the Heckman curve (Fig.1). The logic is that if you invest strongly "in the base" you allow for a higher base which then allows for higher returns in future interventions. For instance, achieving a higher level of non-cognitive skills during childhood boosts the development of cognitive skills during school. As time passes, the programs/transfers have lower effect because it is harder to better the base in a significant way, and therefore it also doesn't increase future capital accumulation.

Following these finding, there would be no need to study if a program has a positive effect on human capital, it always should. We still might want to compare the efficiency/positive effect between programs.

Nevertheless, it has been recently found that this relationship is not as straightforward as it should be (even for the US). Hendren and Sprung-Keyser (2020) studied the



Figure 1: Heckman Curve (The Heckman Equation)

benefit that each policy provides its recipients (measured as their willingness to pay) and the policy's cost to the government, for 133 policy changes in the United States. This ratio (between both estimates) is what the authors name Marginal Value of Public Funds ("MVPF"). The general pattern they find is consistent with Heckmann's theory: 1) direct investments in the health and education of low-income children yield the highest returns 2) MVPFs are high throughout childhood 3) many direct investments in low-income children's health education pay for themselves 4) MVPFs are lower for policies targeting adults.

Still, the authors remark the findings of exceptions to the general rule. For instance, they find some policies that targeted adults had high MVPFs. This is specially true for those programs that had spillovers onto children. Also, their conclusions illustrate that not all policies targeting children have a high MVPF. Youth job training programs and college subsidies have low returns, specially when there was no effect on attainment. Again, the relationship between age and investment is not as linear as it was said to be.

A sensible conclusion that emerges from these exercises is that the completeness (several complementing measures) and design of the programs are key to a high rate of return/MVPF. In fact, looking in detail at the most recognized programs by the former authors, such as the Perry program, we see that it did not just consist of a transfer, but also of a set of ways of accompaniment. For instance, social workers would recurrently visit the mothers and spend time training and guiding them. Therefore, there is still space to fill in the analysis of up to what extent transfers by themselves might contribute to higher human capital in the future (and therefore, higher rate of return).

## $2.2 \quad CCTs$

The lack of panel data, and therefore the incapability of studying long-run effects of programs, is a repeated concearn among the CCT literature. As mentioned above, we are specially interested in CCTs as they consist of transfers that are conditioned (mostly) to education and health conditions. Therefore, the chances they have an effect on human capital accumulation are naturally higher.

In this section we present a brief summary of the main findings of the effects on learning and schooling of CCT programs in Latin America<sup>7</sup>. Even though these results are medium-term results, they do give an idea on whether the transfers are generating conditions for the beneficiaries to be more prone to receive higher income in the future. We also include a special study among this literature, on Ecuador's BDH, that both provides long-term effects and early childhood analysis.

Before diving into the literature review, it is worth remarking that almost all studies are made on beneficiaries that received the CCT continuously during several years or during their whole childhood. Therefore, the results don't reflect in any case the effect of receiving the transfer only for one year. To add to the general features across the results for different countries, it might be useful for the reader to keep in mind that the assistance to primary school was considerably high in the region before the transfers were launched. Therefore, finding almost no effect on primary enrollment should not be surprising; and might not provide valuable information.

Let us start with "Familias en Acción", Colombia's CCT. Baez and Camacho

 $<sup>^{7}</sup>$ We cover programs that were both prominent and have been rigorously analyzed

(2011) study the medium run effects on school attendance and long run effects on test scores of beneficiary children of Familias en Acción (FA). Specifically, they study whether multiple cohorts of children who are covered by FA and who have different degrees of program exposure (ranging from one to nine years) complete more years of education –measured by the probability of completing high school– and perform better in a national standardized test at the end of high school. To do so, they used two identification strategies and techniques. Firstly, matching techniques to compare the school completion rates and test scores of different cohorts of children from treatment and control areas that could have finished high school during the program implementation period of 2003-2009 and were interviewed prior to the initiation of the program. Secondly, they exploit the sharp discontinuity that emerges from the eligibility threshold: household were assigned a poverty index score from a census of poor people, that determined their eligibility into different social programs (including FA) with different thresholds. The authors show that, on average, participant children are 4 to 8 percentage points more likely than nonparticipant children to finish high school, particularly girls and beneficiaries in rural areas. Even though this result would lead us to belief the human capital level was increased for beneficiaries, when we look at the results of the tests, the results are much less encouraging. Authors find no significant difference in the performance beneficiary and non beneficiary students have in college.

Regarding Mexico's Progresa/Oportunidades, the effects don't seam as clear as for Colombia. On one hand, Tirado-Alcatraz (2014) find there was only an effect on poverty reduction in 1999 through the direct effect of the transfers on income. Instead, there seams to be no effect on the medium run, from 1997 to 2007. Behrman et al (2011) show positive results on education attendance (years of schooling increases between 0.7 and 1 year) after receiving the transfer for five and a half years. Still, they find no evidence on the working participation of young adults. It might be worth explaining that they only find a positive effect on women that received the transfer in their late childhood, but the authors claim this can't be attributed to the transfer given they found no impact on the educational level of this group. Contrarily, Parker and Vogl (2018) do find possitive significant results of the CCT on both education and labor outcomes in the long run. Authors rely on census data, something previous papers were not able to use because of the timing. They find the program improves educational attainment, labor market outcomes, and household economic outcomes in early adulthood. "Schooling impacts are similar for men and women, at roughly 1.5 years, while labor market impacts are more pronounced for women, amounting to 30-40% of mean labor force participation and 50% of mean labor income in pre-program cohorts" (Parker & Vogl, 2018)<sup>8</sup>.

The short and medium run effects of the CCT in Nicaragua, *Red de Protección Social* (RPS, Social Protection Network) were studied by Barham et al (2013). They study the effect on children that received the transfer during their highschool (12 years old onward), and find the program only caused a half grade increase in schooling for boys. In addition, and somewhat a more optimistic result, they also claim gains (one-quarter standard deviation increase) in achievement scores of both math and language tests, which could imply long-lasting effects.

We will address the specific case of the CCT in Argentina in the first exercise of the proposed methodology, and discuss the results on education and other variables more in detail. It still might be worth mentioning in this section that Edo and Marchionni (2017) study the effects of receiving the transfer for 4 years on school enrollment and completion both of primary and secundary/highschool education. They only find effects on primary completion for boys between 15 and 17 years old (+2%), and a decrease (-7%) on school desertion for girls between 12 and 17 years old.

Araujo, Bosch and Shady (2017) studied Ecuador's Bono de Desarrollo Humano, and it is the most interesting and relevant study in the CCT literature in Latin

<sup>&</sup>lt;sup>8</sup>Another relevant study is Todd & Wolpin (2006a)

America for two main reasons. Firstly, beneficiaries were randomly assigned into a late treatment and an early treatment group. Secondly, authors had access to test results at the end of high-school from a specific cohort (early recipients of this cohort where in uterus when the program started, and late recipients started receiving it 3 years later). This allowed the authors to test the effect on cognitive skills that the transfer had on early childhood. They found the transfer had no effect on the result of any test; even when controlling by gender, region or parent characteristics.

Thirdly, the transfer was assigned according to a poverty index. This provided a discrete shortcut, that allowed the authors to carry out an RDD study on the effects on school completeness, college enrollment and working condition of young 25 year-old adults (that received the transfer all their lives). They only find a very modest effect on the probability that young adults have completed elementary school, which is not surprising given the very high counterfactual completion rates. More surprisingly, they find no effect on the working condition or school employment. Therefore, both types of studies showed no real effect in the long run: there was no capital accumulation due to investment in early childhood, nor effect on the labor condition.

## **3** Necessary theoretical remarks

## 3.1 What variable should we look at: about the outcome variable

Let us recall two aspects from our introduction. Firstly, that if a social program is effective, it should help its beneficiaries escape poverty in the future. Therefore, we somehow need to account for the poverty condition (or non poverty) of beneficiaries in the future. Secondly, that there are multiple ways of measuring poverty. Multiple dimension indexes seam to be the most appropriate way to capture the reality of the living condition of the population <sup>9</sup>. Still, the lack of this type of

 $<sup>^9 \</sup>mathrm{See}$  Foster 2009 and Alkire & Foster 2011

information in most developing countries forces us to work with uni dimensional data.

Moreover, even when working with the one-dimensional definition of poverty, we can find it has been measured in multiple ways. In this section, we will present a brief summary of the recommendations Angus Deaton (2001) gave on how to use household survey data when measuring poverty.

One of the one-dimensional poverty measures is the \$1 poverty count (today, \$1.9). Even though this measure has been widely used, Deaton discourages its use because of the following concerns with the Purchasing Power Parity assumptions. The relative cost of living of an Indian living in Calcutta is considerably different to the cost of living in Sao Paulo. Moreover, the PPP depends on prices of commodities. The beneficiaries we will be studying are mostly poor, whoes price of food baskets and food staples is only loosely (if at all) connected to world commodity prices. Therefore, we urge the use of local poverty lines when using the methodology we present in this work.

Let us then focus on household and consumption domestic surveys. In countries in which consumption is continuously surveyed and includes household and individual characteristics (education, labor, region, etc), we would be able to use consumption to measure the long run effects. Deaton stresses that measuring poverty through consumption overcomes the income measurement, as it reflects better the living conditions and is not as volatile as income. It's only a few countries that have good quality data on consumption and with a high frequency. Frequency is key in the poverty measurement through expenditure, shorter recall period causes more (food) expenditures to be reported. (Visaria 2000). For instance, doubling the frequency made rural poverty in India decrease from 42.6% in the first six months of 1998 to 23.6%, and urban poverty from 32.9% to 20% Unfortunately, it is not easy to convert income data to a consumption basis. It may be done by scaling down the income data in the surveys by the ratio of national consumption to national income in the national accounts. But there are several problems that arise with this, that discourages this conversion and makes it more desirable to stick to the use of income.

Because the quality and frequency of household surveys is much more reliable than the consumption surveys in most developing countries<sup>10</sup>, we will therefore use as a general output variable the autonomous income of a whole household and individual. Autonomous means that the income was earned, and not received through government pensions or transfers. There are some tentative problems the researcher should check before using the data. Specially, analyze frequency with which the CPI (Consumer Price Index) is updated and if there are or not urban biases. In case the researcher has access to good consumption surveys (following the previous considerations done in the section), we encourage to transform income data to consumption level in the three different stages of the methodology that will be presented.

## 3.2 Synthetic panels

One of the first efforts to study long run dynamics without panel data were pseudo panels, where Deaton was the pioneer<sup>11</sup>. Still, because of their emphasis on cohorts rather than the household or individual, pseudo panel methods have not been widely applied to the analysis of poverty dynamics.

Other attempts to solve the lack of panel data included using group of countries with actual panel data to construct the counterfactual. Still, to do so the group needed to be comparable to the country under investigation and it required too much homogeneity for these countries.

A recent paper by Dang, Lanjouw, Luoto, and McKenzie (2011) (hereafter referred to as DLLM) proposes both parametric and non-parametric approaches to construct synthetic panels at the household level from two rounds of cross sections with rather parsimonious assumptions. These synthetic panels can then be used to predict lower-bound and upper-bound estimates of household poverty

 $<sup>^{10}{\</sup>rm Specially}$  for Latin America, region in which this methodology will firstly be tested

<sup>&</sup>lt;sup>11</sup>See Deaton (1985) and Verbeek (2008)

dynamics. Method to construct synthetic panel data from cross sections to provide point estimates of poverty mobility <sup>12</sup>.

In contrast to traditional pseudo-panel methods that require multiple rounds of cross-sectional data to study poverty at the cohort level, the synthetic panel only needs as few as two survey rounds and allows for household level analysis.

The methodology consists of assuming linear projections of household consumption (or income) on household characteristics for each survey round. Suppose we have two rounds, then the equations for consumption/income would be given by

$$y_{i1} = \beta_1' x_{i1} + \epsilon_{i1} \tag{1}$$

$$y_{i2} = \beta'_2 x_{i2} + \epsilon_{i2} \tag{2}$$

where  $y_{it}$  is the household income or expenditure and  $x_{it}$  is a set of variant and invariant household characteristics.

In the absence of true panel data we do not observe  $y_{it}$  for the same household for more than one period, we only have repeated cross sections. Dang proposes to rely on partial and simple correlation coefficients between samples of different periods to estimate the coefficients, and given the evolution of explanatory variables, predict the evolution of incomes/expenditures.

The first step is to estimate equation (1) using the data in survey round 1. In that way, one might obtain the predicted coefficients  $\beta'_1$ , and the predicted standard error  $\hat{\sigma}_{\epsilon 1}$  for the error term  $\epsilon_{i1}$ . One then needs to do the same for period 2.

It suggests that, given the estimated parameters in equations (1) and (2) are close to each other, the partial correlation coefficient for household consumption can be interpreted as the simple correlation coefficient purged of (the geometric mean of) its multiple correlation with household (time-invariant) characteristics in the two survey rounds, and then re-weighted by (the geometric mean of) the shares of the unexplained predicted errors. The key assumption behind this is

 $<sup>^{12}</sup>$ Also see Dang et al 2014

that the coefficients don't vary over time.

Also, if the household survey includes some households for more that one period, this allows to check if the linear model you constructed is generating accurate predictions. We will call upon this in our proposal.

## 3.3 CEQ Institute identification

Most household surveys don't include questions on specific transfers. Nevertheless, looking at the characteristics and design of the different social programs and transfers, one may infer who the beneficiary households are.

The CEQ Institute studies the distributional effect of public spending. Therefore, they need to identify the households affected by each program/tax and allocate the corresponding amount of funds. If the information is not available in the micro-data, they might impute, simulate, infer or predict the allocation of public resources. Among all possible programs and tax regulations, direct transfers to households are quite straightforward.

We will rely on both the identification and allocation the CEQ Institute has defined for every program we study.

## 4 A new proposal, combining existing existing tools

Hopefully, we've managed to convince the reader about the need to account for the long run effects of social spending if governments want to truly reduce poverty. About the efforts our profession has done to shed some light on them and about the challenge we still face due to the lack of panel data.

In this section, we present a general proposal of how to approximate the long run effects of already-existing social spending programs/transfers on a specific cohort. We will compare the income of beneficiaries in this cohort under two scenarios:

- 1. Where they received the program/transfer for a given amount of time, and then continued to work<sup>13</sup> without receiving it. Let us define that beneficiaries start receiving the transfer in t = 1, receive it for m periods ( in every t = 1, ..., m) and then live without it until T (period t = m, ..., T)
- 2. they don't receive any transfer in period t = 1, ..., m

The final income will be studied with the latest household survey. Therefore, the longer the time that passed since the program was firstly implemented, the older the cohort we may work with or the more years of treatment we can study. This will be clearer when we explain the steps.

The proposal combines the use of former short-run impact evaluations, CEQ identification and allocation methods and synthetic panels. In future efforts, we will not only refine this procedure, but extend it to ex-ante analysis. Before presenting the steps, it might be helpful to further explore the concept of what the Treatment will be in each scenario

4.1 Treatment Universidad de

In an ex-post analysis, the data already shows the effect of the social program/transfer. We first need to identify the beneficiaries, and then construct the counterfactual. To identify the beneficiaries we rely on the CEQ methodology. The construction of the counterfactual and therefore, the definition of the treatment, requires some more explanation. In any case, we can have two types of treatments to be included:

- Direct transfer/ monetary benefit: although the household surveys usually don't include government transfers, households are receiving them acting accordingly. Therefore, for periods t = 1, ..., m
  - scenario with program: we allocate the corresponding \$ to beneficiary households (using CEQ). Households final income should therefore be

<sup>&</sup>lt;sup>13</sup>they might also be unemployed

the autonomous income + the transfer. We refer to this as "augmented income". It is key not to include any other transfers, just the one we are studying.

- scenario without program: don't allocate any transfer. Family income is 100% autonomous income
- Effects on intermediate channels: the existence of the program might affect some of the channels that affect human capital accumulation. For instance, there might be effects on labor participation of adults, education, health and so on. To account for these effects, we rely on trustworthy previous impact evaluations (preferably local). Special attention needs to be given to the timing of these effects. There might be immediate effects, that need to be accounted for at the beginning of the exercise, or ex-post effects on beneficiaries that will have to be accounted for later, once the transfer is over but before the beneficiary continued to work without it. If the data and the. Given this study is ex-post, the data already includes these effects and therefore,
  - scenario with program: we don't need to change anything in the data;
    the data already shows those effects
  - scenario without the program: we need to remove the effect from the existing data. As mentioned above, the timing for this is crucial and the researcher will have to evaluate when it is best to eliminate it in order to respect the dynamics.

## 4.2 Initial Steps

1. Identify possible channels through which the program might affect human capital accumulation. Discussion among colleges in Economics and other areas is highly recommended. The most straightforward/common channels are education, health, parent labor condition, poverty alleviation through extra income(transfer). It is important to focus on the intertemporal dynamics of social mobility.

- Find significant local evidence about short-run effects of those channels. Significant "zero effects" are as important as non-zero ones. The selection of reliable and competent former impact evaluation depends on the researcher.
- 3. If no evidence is found, look for **international evidence** trying to adapt to local situation. We will discuss possible ways of adapting international evidence to local programs in future research.
- Define how to introduce these effects. This is not a trivial exercise. Pay special attention the timing of the impact evaluations and the available data.

## 4.3 Simulation under existence of program/transfer

- 5. Begin with household survey in period t = 1. Use CEQ methodology to identify the households where the beneficiaries of the cohort you are studying lived in period t=1. This is needed as most household surveys don't include a specific question on whether they received a program or not.
- 6. Use CEQ methodology to allocate the transfer to the identified households in t=1. Divide the transfer by the amount of members in the household, and adding it to the per capita (autonomous) income variable. In case the database only included the aggregate family income and not the income per capita, you should first generate the per capita variable by dividing the total income among the number of members. We need the per capita variable for the synthetic panel simulation. To avoid confusion, let

us name the per capita income with the transfer as "augmented per capita income" and the one without the transfer, the "autonomous income"

- 7. Repeat 5 & 6 for every period in which the beneficiaries in the cohort received the transfer, until t=m. Note that as time passes, the beneficiaries get older. Therefore, we need to update the age of the cohort before identifying the beneficiary households
- 8. Construct the linear expenditure/income model using the the logarithm of the *augmented per capita income* and characteristics of the head of household in every t = 1, ..., m
- 9. Use synthetic panels to simulate the evolution of the log of the *augmented per capita income* of the head of household. It's extremely important to use augmented per capita income in every period, and not only in t = 1.
- 10. In case the survey includes some households for more than 1 round, check the accuracy of the predictions
- 11. In t = m, the last period in which the households where the cohort lived received the transfer, remove the transfer from the simulated augmented per capita income. Let us call this the <u>simulated household's autonomous income</u>
- 12. Use the <u>simulated household's autonomous income for t=m</u> to match these households with households in the real household survey (t=m) that have members in of the studied cohort. For the matching, create a **pscore** and match with nearest neighbour
- 13. Simulate the evolution of the cohort's income from t = m to t = T. Keep these results.

## 4.4 Simulation under no existence of program/transfer

14. Begin with household survey in period t = 1. In case some of the effects that were found in previous impact evaluations would affect the eligibility criteria, then the data base has to be adjusted in order to "undo" that effect created by the program. For instance, let's imagine an an analysis of a trash transfer that is conditioned on whether parents work in the informal sector or not. Say it has been found that one of the short run effects of the program is that the rate of parents working in the informal sector increases. That is, there is an effect on a variable that determines the eligibility of the transfer does not exist, then we need to **undo** that effect (and therefore adjust the database) before proceeding, because it affects the eligibility. impact some variable in the eligibility criteria of beneficiaries, this is when those effects should be eliminated from the data.

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- 15. Use CEQ methodology to identify the households where the beneficiaries of the cohort we are studying lived in period t=1. This is needed as most household surveys don't include a specific question on whether they received a program or not. This means, repeat "step 5" but with the data base adjusted as indicated in the previous step.
- 16. Construct the linear expenditure/income model using the the logarithm of the per capita income and characteristics of the head of household in every t = 1, ..., m . Note that we don't include transfers from t = 1 to t = m
- 17. Use synthetic panels to simulate the evolution of the log of the *per cápita income* of the head of household for t = 2, ..., m capita
- 18. In case the survey includes some households for more than 1 round, check the accuracy of the predictions

- 19. Once we obtain the <u>simulated household's per capita income for t=m</u>, we take away (if any) the ex-post effects on intermediate channels found in previous impact evaluations. This might include changing the value of some variables of the households or the individuals of our cohort.
- 20. Use the <u>simulated household's income for t=m</u> and relevant variables (once effects are removed) to match these households with households in the real household survey (t=m) that have members in of the studied cohort. For the matching, create a **pscore** and match with nearest neighbour
- Simulate the evolution of the cohort's income from t = m to t = T.
  Keep these results.

## 4.5 Compare results

22. Calculate the poverty rate of the cohort in t = T under both scenarios. Use corresponding poverty line for that period. Note that there might be different poverty lines for different regions in the country. Be sure to assign the corresponding line per region when calculating the country's poverty rate.

## 5 Conclusion and next steps:

The short run impact of cash transfers on poverty alleviation and on other variables such as school attendance, parents labor condition or fertility has been studied widely. Still, there is relative scarcity of analyses of long term impacts, in particular on the incomes of future generations. This is largely due to the lack of panel data (and in some cases due to the youth of the programs).

In an attempt to fill this gap, I have proposed an approach to estimate long term effects building from the evidence on the short to medium term impacts of programs. The aim is verifying the extent to which the intervention helps the future generation live in conditions better than those of their childhood household. We propose to compare the beneficiaries' income and poverty condition under two scenarios once they grow up: one where they receive the program (real scenario) and one where they didn't (simulated scenario). Given that the transfers might impact not only the disposable income of families, but also other non-monetary conditions that affect the evolution of income and capital accumulation, we rely on the existing literature to account for these other effects and adjust the variables before simulating the evolution of income.

In an attempt to solve the lack of panel data in the study of the effects of social policies in the long run, we propose a methodology that combines the identification and allocation methodologies of the Commitment to Equity Institute with existing short term impact evaluations and Dang's synthetic panels.

This work is the first step in a long agenda ahead of us. The guideline has been developed keeping the AUH (Asignación Universal Por Hijo) in mind. As we expand this exercise to other transfers and programs, we will most surely need to add more specifications. For instance, definitely a clear challenge is to define the temporal extension of the treatment. Should we just consider 1 year of program or a longer period? In any case, how do we account for the effect of poverty if we want to reduce it to 1 year? Another well known limitation is the dependence on ex-ante data.

After testing the methodology with the AUH in Argentina and validating it with real poverty data, the natural thing would be to study first other CCTs in the region and then continue to adapt it to measure other types of programs (and not only CCTs).

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