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Diether Beuermann
Bridget Hoffmann
Marco Stampini
David Vargas
Diego Vera-Cossio

Inter-American Development Bank
Department of Research and Chief Economist

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Diether Beuermann*
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Diego Vera-Cossio*

* Inter-American Development Bank
** University of California San Diego

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Abstract

A key challenge for policymakers is how to design methods to select beneficiaries of social programs when income is volatile and the target population is dynamic. We evaluate a traditional static proxy-means test (PMT) and three policy-relevant alternatives. We use a unique panel dataset of a random sample of households in Colombia's social registry that contains information before, during, and after the 2020 economic crisis. Updating the PMT data does not improve social welfare relative to the static PMT. Relaxing the eligibility threshold reduces the exclusion error, increases the inclusion error, and increases social welfare. A dynamic method that uses data on shocks to estimate a variable component of income reduces exclusion errors and limits the expansion in coverage, increasing social welfare during the economic crisis. We consider these targeting metrics together with the curvature of governments' social welfare function and budgetary and political constraints.[1]

JEL classifications: I38, D31, D63

Keywords: Social protection, Targeting, Household income, Poverty

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1 Introduction

More than 120 low- and middle-income countries invest in cash transfer programs to support poor households (Banerjee et al., 2022). The social returns to these investments are crucially dependent on the ability of governments to accurately identify beneficiary households. The traditional approach to targeting social programs selects beneficiaries using Proxy Means Tests, or PMT (Fiszbein et al., 2009), which are statistical models that predict the structural or permanent component of income. However, households' income fluctuates over time, and even if the data underlying PMTs is updated, these income fluctuations may not be reflected in PMT scores.

Economic shocks are ubiquitous among low- and middle-income households and lead to substantial income volatility because they are typically under-insured (Gertler and Gruber, 2002). Because other social protection programs are largely absent, cash transfer programs often function as a form of insurance against shocks. For instance, because unemployment insurance covers only formal workers who tend to have higher-incomes, cash transfer programs can play an important role in achieving the goals of both anti-poverty programs and unemployment insurance programs (Bottan, Hoffmann and Vera-Cossio, 2021), particularly among informal workers (Cañedo, Fabregas and Gupta, 2023). Moreover, even when low-income households rely on informal risk-sharing networks to mitigate idiosyncratic uncorrelated shocks, they may still remain uninsured against aggregate correlated shocks (Kinnan et al., 2024). Timely expansions of the coverage of cash-transfer programs may help attenuate the impacts of large shocks such as natural disasters (Pople et al., 2021; Premand and Stoeffler, 2022) or recessions (Brooks et al., 2022).

Considering the 1.3 billion households globally that are vulnerable to sliding into poverty, targeting based on current income as opposed to permanent income could provide important social benefits.[2] The key policy challenge is how to design methods for selecting beneficiaries of social programs when income is volatile, the target population for social protection is dynamic, and there are important fiscal budget limitations.

We leverage a unique panel dataset following a random sample of households registered in Colombia's social registry to provide insights for addressing this policy challenge. We exploit the timing of the COVID-19 pandemic and data spanning the pre-crisis, crisis, and post-crisis

[2] The statistics on vulnerable households were obtained from the World Bank Poverty and Inequality Platform (version 20230328-2017-01-02-PROD) at `https:pip.worldbank.org` on June 1, 2023.

periods when substantial fluctuations in income were ubiquitous. First, we assess the performance of a traditional static PMT before and during an episode of severe economic decline, when the permanent component of income is likely to be a worse approximation for families' economic well-being. Second, we evaluate policy-relevant alternative targeting methods, including a dynamic method that uses shocks to predict income fluctuations, on clear targeting and social welfare metrics. Third, we consider how budget and political constraints that governments face in tandem with their preferences for redistribution affect the choice of targeting method. Together, these three key components allow us to shed light on this policy challenge.

Households in the social registry have volatile income due to frequent economic shocks and there is substantial entry and exit from the target population. For example, during the economic downturn of 2020, 35% of households reported suffering a non-labor shock, and 55% experienced a job disruption. Because these shocks are not well-insured, they lead to substantial reductions in income, changes in consumption, and transitions into poverty. Using a difference-in-differences design, we find that labor market shocks reduced per capita income by 47%, increased the probability of reporting hunger by 3.3 percentage points, and increased the probability of falling below the national extreme poverty line by 13 percentage points.

We evaluate four methods of selecting beneficiaries for a hypothetical program that aims to deliver cash transfers to households with per-capita incomes below the extreme poverty line. For each method, we estimate an econometric model of per capita household income using 50% of the sample (i.e., the training sample). We then use the estimated model to select beneficiaries for the hypothetical program and evaluate the performance before, during, and after the economic downturn triggered by the COVID-19 pandemic using the remaining 50% of the sample (i.e., the testing sample). We focus on three key metrics: the exclusion error (the probability that an eligible household is misclassified as noneligible), the inclusion error (the probability that an ineligible household is misclassified as eligible), and a social welfare function based on a CRRA utility function with curvature parameter $\rho = 3$, which places a higher weight on the poorest households and is a function of household's total per-capita income as in Hanna and Olken (2018). We replicate this process over 1,000 sample splits to reduce statistical uncertainty stemming from the sample splits.

We first evaluate the targeting performance of a benchmark static proxy-means-test (PMT) approach to selecting beneficiaries—the most common approach globally. For this, we use detailed data on pre-crisis income, asset ownership, dwelling quality, and demographic characteristics to estimate a predictive model of the permanent component of income, mimicking the approach employed by the Colombian government to determine eligibility for social programs. Following the standard practice among governments, we keep the model parameters fixed across the 3-year span and assume that the information used to estimate this model is not updated during this time period.

We find that the exclusion error increases from 30% in 2019 to 35% in 2020, under the benchmark static PMT approach. As the benchmark PMT approach uses information to predict the permanent component of income, this approach misses the substantial changes in income and fails to include those newly poor households in the safety net. In contrast, inclusion errors remain constant. The results suggest that, as households suffer severe labor market shocks, the accuracy of the traditional PMT approach quickly declines.

Next, we compare the performance of the PMT approach to a set of policy-relevant, budget-neutral counterfactuals. Motivated by the scale-up of transfers around the world amid the 2020 recession,[3] our second approach expands the safety net by shifting the threshold of eligibility from the extreme poverty line to 1.3 times the extreme poverty line using the static benchmark PMT to predict incomes.[4] Relative to the benchmark scenario, the expansion of the safety net reduces the exclusion error by almost 50%. As the coverage of the program is expanded without improvements in the targeting tool, the reduction in exclusion errors comes at the cost of larger inclusion errors. Given the fixed budget, the inclusion of additional households implies a decline in the average per-household transfer size. For welfare functions with high curvature—those that place a higher weight on the poorest households, the reduction in the exclusion error makes up for the decline in transfer size achieving levels of social welfare higher than those under the benchmark scenario (21% in the case of a curvature parameter $\rho = 3$). These gains in welfare disappear for low-curvature social welfare functions ($\rho = 1.5$).

Our third approach is based on the current policy in Colombia that allows households to request to be resurveyed to update their data in the social registry. In particular, we hold the statistical model constant while incorporating negative changes in asset ownership — the most common inputs used in PMT econometric models. We do not find substantial improvements in targeting errors

[3] Over 1 in 6 households around the globe received a government transfer during 2020 (Gentilini, 2022).

[4] According to Gentilini (2022), 22% of programs implemented during the crisis relied on existing social registries to identify beneficiaries.

and welfare compared to the benchmark PMT. As such, relying on on-demand asset ownership updates during economic downturns may fail to substantially improve targeting. Although asset ownership predicts the permanent component of income, relinquishing assets to cope with shocks may be relatively difficult during crises, and thus negative updates to assets may not predict income fluctuations well.

Finally, we evaluate a dynamic targeting approach that predicts changes in income based on labor market changes and other shocks to complement the benchmark PMT. While updating the information that predicts permanent income does little to improve targeting, dynamic information on shocks, particularly labor market shocks, complements the data underlying traditional PMTs by predicting income fluctuations. The dynamic approach reduces exclusion errors and increases welfare (by 12%) relative to the benchmark scenario. These gains are observed for different curvature parameters of the utility function. We also show that the welfare gains from a dynamic approach can be larger when more flexible econometric models are used, and that allowing accounting for over-reporting of job losses and under-reporting of job gains following moral hazard estimates rooted in the literature does not jeopardize the welfare gains of the dynamic approach.

Our results suggest that targeting methods that account for income fluctuations can improve welfare during systemic crises. When beneficiaries are selected using targeting methods that combine proxies for structural poverty with information on labor market shocks, the program resembles a combination of an anti-poverty program and an unemployment insurance program for low-income households. Particularly in contexts with high labor market informality among low-income households, allowing households to enter and exit the program based on labor market shocks increases social welfare. With little or no savings to tap into, the insurance aspect of the program contributes to social welfare by providing households with proxies of permanent income above the extreme poverty line with a minimum level of consumption when severe shocks hit.

Our initial assessment suggests that a dynamic targeting approach and an expansion of the safety net using the benchmark targeting method can generate higher levels of welfare than the benchmark scenario. However, it is less clear *when* a policymaker should consider each approach. We show that the decision will depend on the underlying preferences of the policymaker (e.g., the relative weight placed on the poorest households relative to richer ones) and on the political and budgetary constraints that they face.

In our context, if policymakers face a fixed budget and are allowed to modify the transfer size, an expansion of the safety net is welfare-maximizing as long as policymakers have pro-poor preferences. In contrast, under this same scenario, a dynamic approach is welfare-maximizing if policymakers have more neutral preferences. When policymakers do not have the political capital to cut the transfer amounts and are mainly concerned with minimizing the budget, the dynamic targeting approach appears to be more appealing. Relative to the benchmark scenario, it increases welfare by 13% while increasing the budget by 8% (a welfare-budget elasticity of 1.6). In contrast, the expansion of the safety net achieves a 32% increase in welfare but increases the budget by 36% (a welfare-budget elasticity of 0.88). Finally, when both the coverage rate and transfer amount (and hence the budget) are fixed, *who* is included in the safety net matters substantially for welfare. We find that, on average, the exclusion and inclusion errors do not vary substantially between the dynamic targeting approach and the static benchmark PMT approach for different fixed coverage rates. However, the dynamic targeting approach increases welfare, relative to the static benchmark PMT approach, for low coverage levels. These gains in welfare dissipate as the coverage rate increases. Thus, the social returns to investing in dynamic targeting tools are larger when policymakers face tighter budget constraints.

This paper contributes to two strands of the literature on targeting beneficiaries of social programs. First, it contributes to the recent literature studying the targeting performance of methods of selecting beneficiaries based on new sources of data. A large literature has studied different approaches to acquire information about potential beneficiaries, such as community-based targeting (Alatas et al., 2012; Premand and Schnitzer, 2021; Schnitzer and Stoeffler, 2021), local agents (Bandiera et al., 2023; Vera-Cossio, 2021; Maitra et al., 2020), geographic targeting (Smythe and Blumenstock, 2022), and self-selection mechanisms or ordeals (Alatas et al., 2016). Much less emphasis has been placed on understanding the importance of the *type* of information for improving targeting. Our results highlight the role of the type of information in targeting accuracy, particularly when income fluctuates. Targeting methods that proxy for the permanent component of income can lead to substantial targeting errors in the presence of economic shocks, and these errors can be attenuated by incorporating a proxy of *changes* in income into the targeting method. Thus, our results also provide empirical support for policies that seek to improve targeting by incorporating alternative sources of data, such as administrative records (e.g., health, debt, contributions to social security) in middle-income countries,[5] or mobile phone data and satellite imagery in lower-income countries (e.g., Aiken et al. (2022); Smythe and Blumenstock (2022)) that may be informative for changes in income.

Second, our paper complements the recent literature analyzing the targeting performance of proxy means tests in selecting beneficiaries for anti-poverty social programs in low- and middle-income countries (Brown, Ravallion and van de Walle, 2018; Aiken et al., 2023). Both studies emphasize that the targeting accuracy of PMTs decreases with the time since data collection due to a depreciation of either the social registry data or the model used to predict poverty. Specifically, Aiken et al. (2023) finds that most of the increase in targeting errors is due to data decay, as opposed to model decay. This paper complements these studies by providing a framework to evaluate how policy-relevant alternatives based on a proxy means test perform over time when income fluctuates. The distinct alternatives that we evaluate are based on alternative solutions, such as updating social registry data annually or incorporating additional high-frequency data on economic shocks, to the challenge that income changes at a higher frequency than social registry data. The framework presented in this study makes significant contributions to policy design by going beyond traditional targeting metrics and incorporating social welfare considerations, preferences for redistribution, and political and fiscal constraints.

2 Context and Data

2.1 Context

We study the case of Colombia before, during, and after the onset of the COVID-19 pandemic. Three features make the Colombian context uniquely suited to studying the targeting performance of proxy means tests and policy-relevant alternatives across these three time periods.

First, Colombia has a state-of-the-art social registry that is the basis of eligibility for a number of social programs, mostly targeted to the poorest households using a PMT. The social registry,

[5] Twenty seven percent of the new programs implemented during the COVID-19 pandemic used administrative records to select beneficiaries including social security contributions or tax collection (Gentilini, 2022). In the case of Colombia, administrative records are often used to verify the incomes registered in the social registry. Similarly, the data from Brazil's social registry are combined with vehicle ownership records for verification purposes (Bartholo, Mostafa and Osorio, 2018).

called System for the Identification of Potential Beneficiaries (SISBEN by its acronym in Spanish), includes detailed household-level information and covers close to 50% of the population. Families enter SISBEN in one of two ways. First, the municipal governments identify geographic locations with high concentrations of low-income families and survey all families in these areas. Second, families who live outside the areas identified for universal surveying can submit a request to their municipality to be surveyed. Due to this process, higher-income households are under-represented in SISBEN relative to the overall population. This implies that the social registry includes a greater share of the population of households that are likely to qualify for social programs.

Second, Colombia is an upper-middle income country with a large share of vulnerable, non-poor households. Specifically, Stampini et al. (2021) estimate that 35% of Colombian households are not poor but nonetheless vulnerable to sliding into poverty. In this setting, economic shocks can generate transitions into and out of poverty at a large enough scale to enable us to detect changes in exclusion and inclusion errors.

Third, the Colombian context provides policy-relevant alternative targeting methods to the static PMT. Similar to many other countries around the world, Colombia implemented changes to its safety net in 2020 to attenuate the effects of the crisis. Specifically, the country expanded the coverage of the safety net for more than two years after the onset of the COVID-19 pandemic. This policy choice motivates one of our counterfactual targeting methods, relaxing the eligibility threshold. In addition, individual households can request to be resurveyed to update their data in SISBEN if their circumstances have changed. This motivates our counterfactual targeting method of updating the data at higher frequency. Finally, Colombia's social registry collects information regarding labor market status, which motivates our counterfactual targeting method that uses changes in labor market outcomes to improve targeting.

2.2 Data

We utilize data from the Colombian government's social registry, which is based on detailed household surveys that capture numerous dimensions of family well-being, including family and dwelling characteristics and asset ownership. The SISBEN registry includes close to 10 million families, accounting for over 25 million people or close to 50% of the Colombian population.[6] For details see

[6] The average family size is 2.5 members.

Departamento Nacional de Planeación (2016).

SISBEN IV, the most recent version of the social registry, is based on survey data collected in 2017. We use the 2017 SISBEN data as the sampling frame for follow-up surveys. Specifically, we drew a representative sample of households in SISBEN IV and collected data for 4,049 families through phone surveys. The survey collected updated information on the asset ownership, dwelling quality, and labor market variables in SISBEN and collected information on demographics, education, employment, income, expenditure, food security, and exposure to economic shocks such as job loss, natural disasters, illnesses, deaths, crime, and fire. Although we conducted only one survey round, we were able to recover information pertaining to 2019, 2020, and 2021, which enables us to construct a panel dataset at the household level.

Appendix Table A1 assesses the validity of our sample. Column 1 reports means for the households included in the survey sample, and Column 2 reports means of baseline characteristics corresponding to the universe of households in SISBEN IV (our sampling frame). While the head of household of the surveyed sample appears to be older than the average household head in the registry, we do not find systematic differences in asset ownership (across different categories) or in the key variables used to proxy for the time-invariant component of income.

3 Exposure to Shocks and Income Dynamics

In this section, we document two key facts that guide our empirical analysis of the performance of different targeting methods as aggregate economic shocks unfold and dissipate.

Fact 1: A large share of households is exposed to a variety of economic shocks.

We collected detailed information about whether and when each household was exposed to different types of shock. Panel A in Table 1 shows the incidence of economic shocks by year in our survey data. In 2019, the most common shocks are involuntary job loss for the highest earner in the household (30.8%), followed by accident and illness (16%), and exposure to natural disasters (4.7%). The different nature of the shocks underscores the multiple sources of vulnerability of Colombian households. Overall, 48% of households were exposed to at least one type of shock during 2019.

Figure 1a depicts the incidence of shocks during 2019 by distance to the baseline extreme poverty line (2019). While the poorest households had the highest exposure to shocks, roughly 25% of households with income above the extreme poverty line were also exposed to labor market and non-labor market shocks.

Households' exposure to shocks, in particular those related to job disruptions, increased when the economy entered an aggregate economic downturn. Column 2 in Panel A of Table 1 shows that, relative to 2019, the share of households in which the primary earner experienced a job loss increased from 30.8% to 54.6% as the COVID-19 crisis unfolded in 2020. The economic downturn also affected the reception of remittances and the closure of small businesses or bankruptcy, though at lower magnitudes. These economic shocks affected households along the entire income distribution. Figure 1b displays two key features. First, across 2019 income categories, households were exposed to job disruptions at a greater rate than non-labor market shocks in 2020. Second, exposure to labor market and non-labor market shocks in 2020 was not substantially lower for households with 2019 incomes above the extreme poverty line than those below. For example, approximately 50% of households with incomes well above the extreme poverty line experienced job losses.

Fact 2: Exposure to economic shocks generates important changes in income and transitions into extreme poverty.

Figure 2 plots income and extreme poverty dynamics. The top panel distinguishes between households that suffered a labor-market disruption in 2020 and those that did not. In 2019, income levels were relatively similar across these groups, but diverged in 2020. Average per-capita household income (in logs) declined marginally for households that were not exposed to labor market shocks. In contrast, average per-capita household income declined substantially during 2020 for households in which the primary earner lost their job. Interestingly, as the economy started to recover towards the end of 2021, income levels increased among households that experienced a labor market shock, but they do not recover to their 2019 levels. This suggests that the labor market shocks appear to have at least mid-term effects. Figure 2 also shows that the probability of falling into extreme poverty increased substantially in 2020 for households that experienced a labor market shock. In 2021, the extreme poverty rate among these households remained above its 2019 value, and above that of households whose main earner did not lose their job.

The bottom panel of Figure 2 shows that non-labor market shocks also explain income dynamics, though they imply smaller declines in income and increases in the extreme poverty rate. Further, the impact of these shocks appears to fully dissipate by 2021, as opposed to the income dynamics associated with the labor-market shocks.

To obtain estimates of the magnitude of the impact of these economic shocks, Table 2 displays the average effect of being exposed to the shocks in 2020 estimated using a difference-in-difference design. We compare changes in income before and after 2020 among households that were exposed to these shocks to changes in income before and after 2020 among households that were not exposed to these shocks. Specifically, we estimate:

$$Y_{i,t} = \alpha_i + \delta_t + \beta(\text{Shock}_i \times \text{Post}_t) + \upsilon_{i,t} \tag{1}$$

where $Y_{i,t}$ denotes the outcome of interest of household i observed in period t, $Shock_i$ takes the value of 1 if a household experienced a shock during 2020, $Post_t$ takes the value of 1 for the years 2020 and 2021, and α_i and δ_t are household and time fixed effects. The coefficient of interest is β which captures the average effect of the shock on outcome Y. Standard errors are clustered at the household level to account for serial correlation.

Columns 1 to 3 in Table 2 report average impacts of labor market shocks across two post-shock periods. On average, a job disruption during 2020 reduced average per-capita income by 46% and increased extreme poverty by 13 percentage points. Importantly, these shocks were consequential in terms of consumption and household welfare; they increased the probability of going hungry by 3 percentage points. Columns 4 to 6 show that, across the two-year time span, being exposed to non-labor market shocks reduced average per-capita income by 6% and increased extreme poverty by 2 percentage points, although the second effect is not statistically significant. Column 6 shows that non-labor market shocks increased the probability of going hungry by almost 5 percentage points, demonstrating that economic shocks can have implications for food security even if they do not push households into extreme poverty.

Together, the facts that labor-market shocks are ubiquitous and that they predict declines in income in the short and medium run implies that there are likely to be substantial transitions into and out of the target population over time. If that is the case, traditional PMT approaches that select beneficiaries for social programs based on a proxy for the permanent component of income may miss important changes in income induced by these shocks. These statistics suggest that many households that were initially correctly excluded from Colombia's safety net experienced shocks that reduced their income below eligibility thresholds. While some of these households quickly recovered their income, many other households' income did not fully recover, implying that the transition may have been longer-term. Moreover, the shocks have important welfare implications as they also appear to reduce food consumption. Considering the high labor informality among this population (about 71%), insurance mechanisms against these economic shocks are largely absent (e.g., unemployment insurance). Therefore, the impacts of such shocks may have implications for inclusion and exclusion errors and the associated level of social welfare that can be achieved by different targeting methods. We study these implications in the following sections.

4 Evaluating Targeting Performance

We evaluate the performance of four targeting methods to select beneficiaries for a hypothetical social program that seeks to provide monthly transfers to all households with per capita income below the extreme poverty line. For simplicity, we analyze a program in which households receive the same transfer amount regardless of their demographic characteristics.

We calibrate the per-household transfer amount by setting the total program budget to be USD 1,318,184,821 PPP, which is based on Colombia's planned budget to implement cash transfer programs in 2020. Similar to Glewwe and Kanaan (1989), Ravallion and Chao (1989), and Grosh and Baker (1995), the program budget remains fixed throughout our analysis. We use this fixed budget to calculate the per-household monthly transfer amount by simply dividing the total annual budget by the number of beneficiary households and dividing the resulting amount by 12. Fixing the program budget allows us to directly compare welfare across different targeting methods.

Following Hanna and Olken (2018), we evaluate the targeting performance of each method of selecting beneficiaries along three core dimensions: the inclusion error (i.e., the share of households that are not extremely poor that are selected as beneficiaries), the exclusion error (i.e., the share of extremely poor households that are not selected as beneficiaries), and the associated social welfare based on a Constant Relative Risk Aversion (CRRA) function ($U = \frac{\sum_i^N (\bar{c}+y_i+b_i)^{(1-\rho)}}{1-\rho}$).

Here, y_i denotes income net of transfers, and b_i denotes the transfer amount received by household i. The degree of concavity of the CRRA utility function is governed by the parameter ρ. Thus, the marginal utility of an extra dollar is decreasing with household income. Holding the program's budget constant, our social welfare function captures the tradeoff between covering a larger number of households and delivering more resources to those with higher marginal utility. We set ρ equal to three in our main specification and also discuss how this tradeoff varies with changes in the value of the parameter ρ. We also impose a subsistence consumption level $\bar{c}$, calibrated to match the monthly COP PPP equivalent of the international extreme poverty line (Li, Shim and Wen, 2017).[7] We do so as the utility function is not well defined for 0 incomes, which is not an uncommon income value during crises.

We determine eligibility for this hypothetical program using each of the four different targeting methods based on PMTs (which we describe in the next section). For each method, we begin by estimating a model that predicts the per-capita income of each household using baseline (2019) data for 50% of the households of our sample (i.e., the training sample). We then use this model, trained on baseline (2019) data, to select beneficiaries of the hypothetical program at each point in time (2019-2021) using the remaining 50% of households in our sample (i.e., the testing sample). Next, we compute key aggregate statistics of interest: coverage rate, transfer size, exclusion and inclusion errors, social welfare, and the share of households living under extreme poverty.

This process generates two sources of uncertainty: one related to the initial split between training and testing samples, and estimation uncertainty conditional on the training sample. To account for these sources of uncertainty, we randomly split the sample into a training and testing subsample 1,000 times. For point estimation of the key metrics, we calculate each metric using each testing subsample, and then report the average across the 1,000 testing subsamples. For inference, we report 95% confidence intervals based on percentiles of the observed distribution of the metric across the subsamples.

[7] We calibrate the subsistence consumption level to 1.9 USD per capita per day, which is the international extreme poverty line in 2011 PPP terms. See **URL:** `https://www.who.int/data/gho/indicator-metadata-registry/imr-details/4744#:~:text=The%20current%20extreme%20poverty%20line,ranked%20by%20per%20capita%20consumption.`.

5 Performance of Targeting Methods

5.1 Benchmark: Static Proxy Means Test

To begin, we evaluate the performance of traditional proxy-means testing (PMT) as an approach to select beneficiaries. We exploit detailed data on asset ownership and dwelling characteristics to estimate a PMT score that mimics the one used for determining eligibility for social programs in Colombia. For privacy reasons, we do not observe the statistical model used to generate the scores, but we do observe the set of variables that are included in the model. Using data from 2019, before the pandemic, we estimate a statistical model to predict per-capita income using a vector of household demographic characteristics, a vector of asset ownership, a vector of dwelling characteristics, and data on the employment status of the head of the household.[8] We refer to this model as the "benchmark PMT."

Appendix Figure A1 assesses the predictive performance of the benchmark PMT model. It displays a ROC curve that illustrates the tradeoff between exclusion and inclusion errors associated with our baseline PMT score estimated with 2019 data.[9] The ROC curve shows that, for each level of inclusion error (horizontal axis), the ROC curve is above the 45-degree reference line. This suggests that the PMT method is more accurate at classifying households as eligible for the program than a random assignment of beneficiaries. The curve also illustrates that in order to reduce the exclusion error, the baseline PMT approach needs to tolerate higher inclusion errors. The slope of the curve changes at different levels of inclusion error (horizontal axis). The slope is steep near the origin, indicating that the inclusion error will not rise very quickly with reductions in the exclusion error when the exclusion error is very high. In contrast, the slope is flatter far from the origin where the exclusion error is relatively low, indicating that further improvements to the exclusion error are very costly in terms of increases in the inclusion error.

Our benchmark scenario is one of a static PMT. As such, households' predicted per-capita income and program eligibility status do not change over time. However, as shown in Section 3, households' true economic situations change over time, suggesting that the targeting performance

[8] For reference, Appendix Table A2 shows the coefficients for all the variables in the model, estimated using all available observations in 2019.

[9] "Receiver operating characteristic" (ROC) curves plot a binary classifier (in this case, whether the predicted per-capita income is below the cut-off for eligibility for the program) to illustrate the tradeoff between true positives and false positives as the eligibility cutoff is varied (Hanna and Olken, 2018).

of the static PMT is likely to change over time. Therefore, we evaluate the performance of the baseline PMT in selecting beneficiaries in two situations. First, we focus on the short-term, when the data used to estimate the PMT are current. Second, we analyze how the performance of the PMT changes over time as households' economic situation changes.

5.1.1 Results

Table 3 reports key outcomes achieved by the benchmark PMT targeting method. In 2019, selecting beneficiaries using the benchmark PMT implies that the program covers 47% of the households in the social registry and delivers a monthly transfer amount of USD 13.5 PPP per household (COP 20,458), which is equivalent to 37% of the average per-capita household income in 2019 among households living in extreme poverty. In 2019, the implied inclusion error for the hypothetical program is 29.3%, which reduces the size of the transfer that each household receives. This inclusion error is paired with an exclusion error of 29.9%.

Next, we analyze whether the accuracy of the benchmark PMT changes over time as households are exposed to severe shocks. Because PMTs are designed to capture the permanent component of income, this snapshot may remain relatively accurate over time. However, traditional PMTs are not frequently updated and typically do not include data capturing a household's exposure to shocks. Thus, a static approach to selecting beneficiaries may fail to identify those who entered and those who exited extreme poverty.

Figures 3a to 3d report inclusion and exclusion errors, transfer size, and social welfare over time for our benchmark PMT model, and the three other policy-relevant alternative targeting methods which we discuss in detail in Section 5.2. Because the benchmark PMT is a static score, there are no changes in the coverage of the program (i.e., the number of beneficiaries) or the transfer size over time. The inclusion error in the baseline model remains relatively constant over time. However, the exclusion error varies substantially. It increased from 29.3% in 2019 to 34.7% in 2020 (a 16% increase). One explanation is that, as the crisis unfolded, the target population expanded. Figure 4 shows trends in the probability of having income below the extreme poverty line (based on income net of transfers), by predicted poverty status using the benchmark PMT approach. It shows that among the households that were ex ante correctly classified as ineligible for the program, those that experienced labor-market shocks also experienced a decline in income in 2020. This decline in income pushed them below the extreme poverty line, making them part of the program's target population. In other words, the program's target population moved.

As many households fell into extreme poverty without becoming beneficiaries of the program, social welfare declined in 2020 relative to 2019 (Figure 3d). Specifically, columns 2 and 3 in Table 3 imply a decline in social welfare of 183% for a curvature parameter (ρ) of 3. This pattern is qualitatively similar for a CRRA utility function that places a relatively equal weight on households across the income levels (i.e., $\rho = 1.5$) as well as in the case of a utility function that gives a higher weight to households with lower net of transfers income (i.e., $\rho = 4.5$).

In 2021, exclusion errors appear to return to baseline levels, mimicking the decline in extreme poverty rates among households that were classified as ineligible based on the static PMT (see Figure 4). Likewise, social welfare partially recovers but does not return to the level in 2019, which is consistent with the partial recovery of incomes (see Figure 2). The results suggest that when there is entry into and exit from a program's target population over time, but program eligibility status is fixed, the social value of social programs can substantially and quickly depreciate. This result is consistent with evidence from other settings (Aiken, Ohlenburg and Blumenstock, 2023; Brown, Ravallion and van de Walle, 2018) and motivates our analysis of alternative targeting tools. In the next section, we discuss three alternatives to the static PMT and evaluate whether they are better able to maintain the social value of social programs as households enter and exit the target population over time.

5.2 Alternatives Based on the Proxy Means Test

The results in the previous section emphasize the importance of studying different approaches that governments could consider to target social programs in the presence of economic shocks and their implications for the tradeoff between inclusion and exclusion errors. In this section, we introduce three policy-relevant alternative targeting methods based on proxy-means tests and assess the extent to which they can increase social welfare relative to the benchmark PMT by balancing inclusion and exclusion errors while holding a fixed budget. As recent evidence shows that the performance of PMTs decays over time primarily due to data decay, as opposed to model decay (Aiken, Ohlenburg and Blumenstock, 2023), we focus on policy options that are add-ons to the benchmark PMT approach to select beneficiaries.[10]

Adjusting the cut-off. We analyze a policy counterfactual that expands the coverage of the safety net to households that ex ante were not living in extreme poverty while holding the program budget constant. Many countries scaled up transfers during the pandemic by expanding the set of eligible households while holding the targeting tool constant. Indeed, in Latin America, the coverage of non-contributory cash transfer programs increased by 9 percentage points, on average, between 2019 and 2020—roughly a 30% increase in one year (Stampini et al., 2021).[11] This rapid increase in coverage was likely facilitated by the availability of well-established social registries and PMTs.

The rationale for including more households by adjusting the eligibility threshold while holding the targeting tool constant is twofold. First, ex ante extreme poor households that were misclassified as non-extreme-poor may be more likely to have predicted per-capita incomes (based on the benchmark PMT) just above the program's original cut-off than far above this cutoff. Second, many ex ante non-extreme-poor households that were correctly excluded from the program but were on the margin of (extreme) poverty have a higher risk of becoming (extremely) poor due to shocks. Compared to the status quo, this approach would reduce the exclusion error by including ex ante excluded households that were either originally incorrectly on the margin of eligibility or slid below the original eligibility threshold due to a shock. However, to the extent that not all households with predicted incomes between the original and new eligibility thresholds transition into poverty, it may increase the inclusion error.

To evaluate an expansion in coverage, we use our benchmark PMT score to classify all households with predicted ex ante per-capita income below 1.3 times the extreme poverty line as eligible for

[10] We do not evaluate other targeting methods, such as self-targeting (i.e., ordeals) and community-based targeting, that are common in the literature. Self-targeting relies on transaction costs to induce self-selection in beneficiaries. In the context of Indonesia, ordeals reduced the inclusion error (Alatas et al., 2016), but these approaches also increase the administrative burden for poor households, which can increase exclusion errors (Finkelstein and Notowidigdo, 2019). Although we do not evaluate a self-targeting approach, compared to the static PMT, self-targeting will likely reduce the inclusion error, which, as shown in Section 3, did not change dramatically during the crisis.

Community-based targeting relies on local information available to community members to identify beneficiary households (Alatas et al., 2012). We do not evaluate the community-based targeting approach because, similar to other countries in Latin America, most of the Colombian population is concentrated in large urban centers.

[11] For example, in Colombia, the government expanded the provision of cash transfers to individuals that despite being poor were not covered by pre-existing social programs and to individuals whose incomes placed them above the poverty line. Before the pandemic, only households classified as poor were eligible to receive transfers. Essentially, the government moved the eligibility threshold without changing the underlying PMT score or the data used in the statistical model that generated the PMT score.

the program, consistent with the 30% increase in coverage observed in Latin America (Stampini et al., 2021). We then use data on observed income over time to compute exclusion, inclusion errors, transfer size, and social welfare. Note that this policy counterfactual neither modifies the underlying statistical model to select beneficiaries nor updates the data fed into the model. Instead, it simply expands the eligibility threshold. We therefore refer to this approach as the "Expanded coverage" approach.

Updating poverty assessments on demand. Next, we analyze a policy counterfactual in which household data in the social registry is updated each year, based on households' requests. This policy counterfactual reflects the current policy in Colombia in which households can request to be re-surveyed to update their data in the social registry. Specifically, this targeting method feeds updated assets data into the existing model to compute an updated PMT score. As shocks occur, the most affected households may choose to request an update of their poverty assessment, which should decrease the exclusion error, relative to the benchmark static PMT approach.

We operationalize this counterfactual by exploiting data on sales and purchases of assets. Following Brown, Ravallion and van de Walle (2018), we focus on updates to assets as opposed to also including data on living conditions as assets are the most common input in econometric targeting tools such as PMTs. Specifically, for each year (2019-2021), we asked whether a household member bought or sold a series of assets, including vehicles, computers, and household appliances. For those who reported a transaction, we also asked for the date of the transaction. We combined these survey data with the administrative records in the social registry to update the initial asset information in the social registry. We then use the updated data on assets to generate an updated score using the same statistical model used for our benchmark PMT approach. For each point in time (2020 and 2021), we use our updated PMT to select beneficiaries for the program. To mimic the on-demand process, we only update negative changes in asset ownership, as those are the households that have an incentive to update their information.[12] Because we use a traditional PMT based on assets and dwelling characteristics, but we update the asset data annually, we refer to this approach as the "Updated PMT - Assets" approach.

Dynamic targeting. One common feature of the approaches discussed above is that, by relying

[12] This approach implicitly assumes that households are not deterred from updating their data by the administrative burden of doing so. Additionally, note that this choice will estimate lower bounds for the inclusion error associated with this targeting tool as only those that truthfully experienced assets losses will have their data updated.

on a PMT that aims to approximate the permanent component of income, none of them incorporate dynamic shocks, such as the ubiquitous labor market adjustments. Moreover, these types of shocks are the key factors driving changes in incomes and transitions into extreme poverty (see Section 3). As countries integrate additional administrative data sources such social security contributions, health records, or credit bureau records into their social registries,[13] one alternative approach is to rely on labor market data and other shocks to predict changes in income and *dynamically* select beneficiaries.

Below, we analyze an alternative approach that complements the benchmark PMT approach by including labor-market shocks as predictors of the time-varying component of income. We note that household income in a given period $Y_{i,t}$ can be written as a function of the permanent component of income ($\bar{Y}_{i,t}$), the fluctuations around the permanent component ($c_{i,t}$), and a random error ($\epsilon_{i,t}$). Thus, it is possible to create a dynamic score that combines relatively time-invariant information to predict the permanent component of income (through a traditional PMT) and data on exposure to shocks, such as changes in labor market outcomes, to estimate income fluctuations. Relative to the case of the static benchmark PMT, this dynamic approach updates the PMT score by including information on households that suffered a shock, which in turn may enable these households to become eligible for our hypothetical program. Likewise, it will automatically graduate households from the program when they experience positive shocks.

Specifically, we approximate the permanent component of income ($\bar{Y}_{i,t}$) by using our benchmark static PMT score. The fluctuations around the permanent component of income ($c_{i,t}$) are approximated using a predictive model of changes in income based on data capturing job losses and job gains for a household's primary earner. We estimate a model that allows income changes to respond differently to employment gains and losses using the following specification: $\Delta Y_{i,t} = \beta_1 \text{Job disruption}_{i,t} + \beta_2 \text{Employment gain}_{i,t} + \upsilon_{i,t}$, where $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-1}$ denotes changes of income, $\text{Job disruption}_{i,t}$ and $\text{Employment gain}_{i,t}$ are indicators or whether the primary

[13]In the case of Colombia, administrative records are often used to verify the incomes registered in the social registry. The combination of survey data and administrative records is relatively common in other middle-income countries such as Brazil, Chile, and Turkey (Barca, 2017). In Brazil, the Ministry of Social Development runs periodic cross-checks of their social registry (*Cadasro Unico*) with other data sources to ensure accuracy of data including death certificates, income from formal workers and contributions to social security. In Chile, the social registry (*Registro Social de Hogares*) is fed by multiple administrative data sources as well as surveys. In Turkey, the primary approach for data collection and updating is through virtual integration (interoperability) of existing administrative databases from 22 institutions. This approach took on more relevance during the 2020 pandemic: 27% of the new programs implemented during the COVID-19 pandemic used administrative records to select beneficiaries including social security contributions or tax collection (Gentilini, 2022).

earner of household i lost their job or transitioned from unemployment to employment, respectively. $\upsilon_{i,t}$ is an error term.[14] Thus, the Dynamic PMT score at time t is computed by: $PMT_i + \Delta\hat{Y}_{i,t_{i,t}}$. We estimate this model using a training sample and compute targeting errors and welfare using the testing sample. As in our previous analysis, we replicate this approach across 1,000 sample splits.[15]

We focus on labor market shocks as they were the most frequent type of shock during the analysis period and are the type of shocks for which we will be able to detect changes in aggregate targeting outcomes. However, we also discuss the implications of including additional idiosyncratic shocks such as illnesses and loss of remittances, among others. We refer to this approach as the "Dynamic" approach.

5.2.1 Results

Adjusting the cut-off. Expanding the coverage of the safety net generates important differences relative to the benchmark PMT. First, it implies an increase in the coverage of the program from 47% to 64% (a 36% increase) of the households in the social registry (see Table 3). Second, Figure 3a shows that this increase in coverage implies an exclusion error in 2020 that is 52% lower than that achieved by the benchmark PMT approach in 2020. Third, Figure 3b shows that, in 2020, this expansion of the safety net would have increased the inclusion error to 45% relative to 29% in the case of the benchmark PMT approach. The increase in the inclusion error is consistent with the fact that not all ex ante non-poor households with baseline PMT scores between the original and expanded cutoffs slid into extreme poverty during the crisis, even though they would be considered vulnerable to extreme poverty based on the baseline PMT score.

Figure 3c illustrates the tradeoff between the coverage of the program and the transfer size when the program's budget is fixed. The expansion of the coverage of the safety net implies a substantially smaller transfer size. In 2020, the average transfer amount declined by 27% relative to that delivered by the benchmark PMT approach. Thus, the gains in social welfare relative to

[14] For reference, we report coefficients of this model estimated over the entire sample in Column 2 of Appendix Table A3.

[15] One would like to estimate a model of changes in income using several pre-crisis years, and then use the model coefficients to predict income out of sample based on the observed employment trajectories under the assumption that such coefficients are time-invariant. However, we only observe household income for one pre-shock year. Instead, we the use 2 first years of data in our dataset (2019-2020) to estimate a predictive model for income changes. This alternative approach relies on the same assumption as the approach based on pre-crisis data: that the underlying model coefficients are time-invariant.

the benchmark PMT will depend on two opposing forces: an increase in welfare due to the increase in income for the marginal households entering the safety net, and a decrease in welfare due to a reduction in income among infra-marginal households. Figure 3d shows that in 2020 the first force seems to dominate. For a CRRA curvature parameter (ρ) of 3, the expanded-coverage approach attenuates the welfare loss experienced by the benchmark PMT. Indeed, social welfare under the expansion is 24% higher relative to that under the benchmark PMT. In 2021, when income partially recovers, the difference in welfare between the benchmark PMT and the expanded coverage model is reduced by about half.

Table 3 reports social welfare levels for different curvature parameters of the CRRA utility function. When the social welfare function places a higher weight on the poorest households (e.g., ρ=4.5), the gains in welfare achieved by the expansion of the safety net in 2020 relative to the benchmark PMT are even larger. In contrast, there appears to be no gain in social welfare for utility functions that weight households across income levels more uniformly (e.g., $\rho = 1.5$). Thus, for a set budget, an expansion of the safety net is welfare maximizing when policymakers place a larger weight on delivering transfers to the poorest households, but the traditional approach might be attractive for more neutral policymakers.

Updating poverty assessments on demand. Updating the asset data does not lead to a substantial expansion in the coverage of the program relative to the benchmark model. Consequently, we do not observe substantial changes in either the inclusion or exclusion errors relative to the benchmark PMT. Similarly, we do not observe substantial differences in social welfare. These results are a consequence of a remarkably low amount of asset transactions. On average, only 2.7% of the households in our sample sold assets during 2020. This lack of transactions may not be due to asset misreporting as individuals do not know the weight of the assets in their score, which is consistent with experimental evidence showing that adding additional assets to a PMT does not distort reporting of such assets (Banerjee et al., 2020). Instead, the lack of transactions may reflect the fact that finding buyers for these assets might be challenging during a period of severe economic downturn, as the households that are likely to demand such assets may also be affected by the aggregate shock.

Dynamic targeting. In 2020, the dynamic approach modestly expands the coverage of the program from 47% in the case of the baseline PMT approach to 50.6% of the households in the social registry. Figure 3a shows that the dynamic approach is able to prevent the exclusion error from rising abruptly during the crisis because it allows ex ante excluded households to enter the program based on their predicted income losses. The exclusion error was roughly 30% throughout the analysis period. Figure 3b shows that, relative to the benchmark PMT approach, the dynamic approach achieves a smooth trajectory of the exclusion error by allowing a larger inclusion error.

Given a fixed budget size, Figure 3c shows that the dynamic approach induces an 8.3% decline in the per-household transfer, relative to the benchmark PMT method. This decline is small, as the dynamic approach generates a modest increase in coverage. The dynamic approach also attenuates the decline in social welfare associated with the benchmark PMT in 2020. Specifically, welfare under the dynamic targeting approach is 12% higher than under the benchmark PMT (for $\rho = 3$). As the economy begins to recover in 2021, the differences dissipate. Table 3 reports social welfare levels for different curvature parameters of the CRRA utility function. Note that, in 2020, the dynamic approach also achieves welfare gains relative to the benchmark PMT for relatively more income-neutral welfare functions (e.g., $\rho = 1.5$) and for welfare functions with more curvature (e.g., $\rho = 4.5$).

Because our empirical approach uses one simple econometric model as an add-on to the benchmark PMT, one concern may be that the results are model specific. Our main specification allows job losses and job gains to predict different changes in income. This is more flexible than a specification that imposes a common coefficient capturing the sensitivity of income to changes in employment but is more restrictive than a specification that also allows for different co-movements based on whether the job losses or gains are associated with formal or informal workers. Appendix Figures A2a-d compare the performance of these alternative dynamic targeting specifications to the benchmark PMT approach and our main dynamic approach. All models behave qualitatively similarly, achieving lower exclusion errors and larger levels of welfare in 2020 than the benchmark PMT approach. Interestingly, as the model becomes more flexible (allowing coefficients to differ between positive and negative shocks, and by type of employment), targeting becomes more accurate in terms of the exclusion error, and social welfare increases. In Appendix Figure A3a-c, we also report results using a predictive model for changes in income that includes exposure to other negative shocks (loss of remittances, illness, exposure to natural disasters, etc.) as predictors. The results are similar to those obtained using the main dynamic targeting model.

Finally, we also report the results adjusting for potential moral hazard: people may have incentives to under-report job gains and over-report job losses. To account for this, we assume that X% of individuals report having lost a job when in fact they remained employed, and that X% of individuals report not being employed even though they regained employment. We calibrate X using causal estimates of moral-hazard behavior in the literature. Specifically, we set X=8.7%, which is consistent with research from Uruguay showing that being eligible for a cash transfer program reduced formal employment by 8.7 percentage points among single mothers (Bergolo and Cruces, 2021).[16] Figure A4 shows that allowing for misreporting does not substantially change the results.

6 Choosing between Alternative Targeting Approaches

The analysis in the previous section suggests that, during aggregate economic downturns that lead to rapid changes in incomes, a dynamic targeting approach and an expansion of the coverage of the safety net can increase welfare relative to the static benchmark PMT. However, it is less clear *which* approach a policymaker should select to attenuate the negative effects of systemic shocks. Policymakers may face underlying political and fiscal constraints that limit the margins of adjustment for social protection programs. Further, within these constraints, the choice may depend on the curvature of the policymaker's social welfare function. We discuss the implications of these factors below.

Fixed budget allowing for changes in the transfer size and the coverage of social protection. Our previous discussion considered a scenario with a fixed program budget and allowed for changes in the coverage rate and the transfer size. In this context, an expansion of the coverage of the safety net entails a reduction in the transfer size and an increase in the inclusion error, which have negative consequences on social welfare. However, there is also a reduction in the exclusion error. Together, the increase in the inclusion error and the decrease in the exclusion error imply that the program covers a larger share of the population, including additional households with high marginal utility, such as those with zero income net of the transfer. Table 3 shows that as incomes declined in 2020, the share of households with zero income after transfers is 0.8% under the expansion of the safety net compared to 1.3% under the dynamic approach.

[16] This is a relatively large effect on moral hazard behavior as Bergolo and Cruces (2021) find a 6-percentage point decline in formal employment using all the sample, and Bosch and Schady (2019) find even smaller effects in Ecuador, but only among women.

As a result, with a fixed budget, expanding the safety net dominates the dynamic targeting approach in terms of welfare for higher curvature parameters of the welfare function (e.g., when the weight that policymakers place on the poorest households relative to richer households is greater). Table 3 suggests that, during the economic downturn in 2020, the expansion of the safety net led to welfare levels that were 9% and 24% higher than those achieved by the dynamic targeting approach with curvature parameters of $\rho = 3$ and $\rho = 4.5$, respectively. In contrast, a policymaker who places a relatively uniform weight on households of different income levels will be less forgiving of lower transfer sizes. Table 3 shows that, for a curvature parameter of $\rho = 1.5$, the dynamic targeting approach achieves a social welfare gain equivalent to 4% of that achieved by the expansion of the safety net.

Fixing transfer sizes and allowing for changes in the budget and coverage of social protection. Our previous analysis assumed that governments have a fixed budget for the social program. In this case, as the coverage changes, the margin of adjustment is the transfer size. This allowed clear comparisons of social welfare across different targeting methods, but in practice, reducing transfer sizes may be politically infeasible. It is hard to conceive a government reducing the amount of the transfer during a crisis. In this setting, governments may explore ways to increase the coverage of a program while minimizing spending. Thus, the policy-relevant exercise may entail comparing the total government spending needed to implement each alternative. Figures 5a-b report changes in welfare and in program budget for different targeting methods, while fixing the transfer size to be equal to that of our benchmark PMT method in 2019 (USD 13.5 PPP per-month).

As expected, because the expansion of the safety net increases the coverage rate, the aggregate level of welfare increases substantially (a 32% increase) relative to that of the benchmark scenario in 2020, but so does the budget required to implement the policy. In 2021, the social welfare is still larger than that of the benchmark PMT scenario, but the gap between them is narrower even though the budget increase is similar to that of 2020. Expanding the coverage of the safety net increases social welfare but at a high cost. Relative to the status-quo, increasing the eligibility threshold requires a 37% increase in the fiscal budget.

The dynamic targeting approach also increases the coverage rate relative to the benchmark PMT but less so than the expansion of the safety net. Relative to an expansion of the safety net, the dynamic approach yields somewhat lower welfare levels for a utility function with curvature $\rho = 3$ (a 13% increase relative to the benchmark PMT approach in 2020), but at a fraction of the cost. The increase in the fiscal budget needed to finance this approach is only 8% of that needed at baseline, which is only one-fourth of the additional budget required by the expansion of the safety net. Thus, the welfare elasticity (% change in welfare by % change in budget) is larger for the dynamic targeting approach (1.6), than for the expansion (0.86). One explanation is that the dynamic approach includes new beneficiaries who are more likely to suffer severe income losses than the average new beneficiary of a broad expansion of the safety net.

Fixing transfer sizes and coverage rates. In several settings, governments may simply be unable to increase the program's budget and may not have the political capital to reduce the transfer size. In other settings, governments may have a set budget to expand the coverage of the safety net, subject to maintaining the transfer size. In both scenarios, the key adjustment margin is *who* enters and exits the program. Appendix Figure A5 compares exclusion and inclusion errors between the benchmark PMT approach and the dynamic targeting approach for different coverage levels. To select which households are included in the program, we rank households according to their predicted pre-capita income according to each approach. Figures A5a and A5b show minimal differences in overall levels of exclusion and inclusion errors. This suggests that any differences in welfare across the two methods would be related to differences in the marginal utility of extra income across included and excluded households. Figure 6 reveals an interesting pattern. The dynamic targeting approach appears to achieve higher levels of social welfare as the crisis evolves, but only for lower coverage levels (10% and 30%). Thus, when budgets are only enough to guarantee small coverage rates, investing in a dynamic approach to targeting is beneficial. In contrast, when social protection has a broader coverage, investments in targeting tools may yield very modest returns in terms of welfare.

7 Discussion and Conclusions

We evaluate a traditional static PMT and three policy-relevant alternatives in terms of errors of inclusion, errors of exclusion, and social welfare. We use data from a household survey of a random sample of households in the Colombian social registry that collected data on household assets, income, and economic shocks over time. While no method is a panacea, the dynamic method results in higher social welfare than the traditional static PMT. These welfare gains are due to a reduction in the exclusion error (i.e., providing transfers to households with relatively high marginal utility of income), and the fact that there is less expansion in coverage (which increases the amount of the transfer per beneficiary household).

Because the dynamic approach selects beneficiaries based on a combination of the permanent component of income (targeted via traditional PMT methods) and income fluctuations (predicted by shocks suffered by the household), the approach enhances the anti-poverty program by also insuring low-income households against uncovered shocks such as job losses. A social protection program that combines these two features can improve social welfare over a strictly anti-poverty program targeted to households with permanent income below the extreme poverty line. This becomes particularly relevant in contexts in which many low-income households are excluded from unemployment insurance schemes due to high labor market informality.[17] With little or no savings to tap into, the insurance aspect of the program contributes to social welfare by providing households with proxies of permanent income above the extreme poverty line with a minimum level of consumption when severe shocks hit.[18]

Although the welfare improvement achieved by this type of dynamic targeting method may be larger in contexts with greater labor market informality, high levels of labor market informality also create two potential challenges for this approach. First, pervasive labor market informality among the population of potential beneficiaries implies that governments cannot rely solely on existing administrative employment data, which includes only formal workers. While other administrative datasets such as credit bureau data may provide useful information for targeting, governments will likely need to invest in primary data collections to update labor market variables at a high frequency. Second, this approach may lead to moral hazard. It may be easier for households to hide their informal employment status than to manipulate a proxy means test based on an unknown statistical model with hundreds of potential variables. We ameliorate this concern showing that our results are robust to incorporating estimates of moral hazard in reporting employment

[17]Within our context, about 70% of employed individuals registered in SISBEN were informal in 2019 (i.e., in our baseline pre COVID period). When considering individuals living in households with income percapita below 1.3 times the poverty line, informal employment accounts for 80% of workers.

[18]Indeed, the extent to which public programs add value by insuring aspects not directly conceived within the original design of them has been the subject of recent debate. For example, Deshpande and Lockwood (2022) shows that the value of disability insurance in the United States goes beyond insuring health risks alone as recipients, especially those with less-severe health conditions, are much more likely to have experienced a wide variety of non-health shocks (e.g., job loss, foreclosure, eviction) than non-recipients.

from the literature. Nonetheless, the establishment of de facto unemployment insurance for low-income households regardless of formality status could incentivize informal work. Although there is evidence that social protection programs can create perverse incentives that encourage informality (Bosch and Campos-Vázquez, 2014), this is less worrisome within this case, as the program benefits are not predicated on informality and the vast majority of workers in the target population have limited opportunities for formal employment.

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Figures and Tables

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Table 1: Incidence of Shocks

% Of sample experiencing:	2019	2020	2021
Involuntary job loss - Main earner	30.8	54.6	37.1
Accident or illness	16.0	19.3	19.1
Death of household member	2.5	2.5	2.4
Separation of spouses	4.2	2.8	3.0
Bankruptcy or closure business	3.2	8.2	3.2
Theft or destruction of property	2.5	2.3	3.1
Victim of armed conflict	1.7	1.2	1.4
Loss or cut-off of remittances	3.5	6.8	4.2
Fire	0.5	0.1	0.2
Natural disaster	4.7	5.3	6.4
Any shock	48.4	67.3	56.4

Note: The table reports the proportion of households that reported experiencing each situation at least once during a given year, using survey data.

(a) 2019

(b) 2020

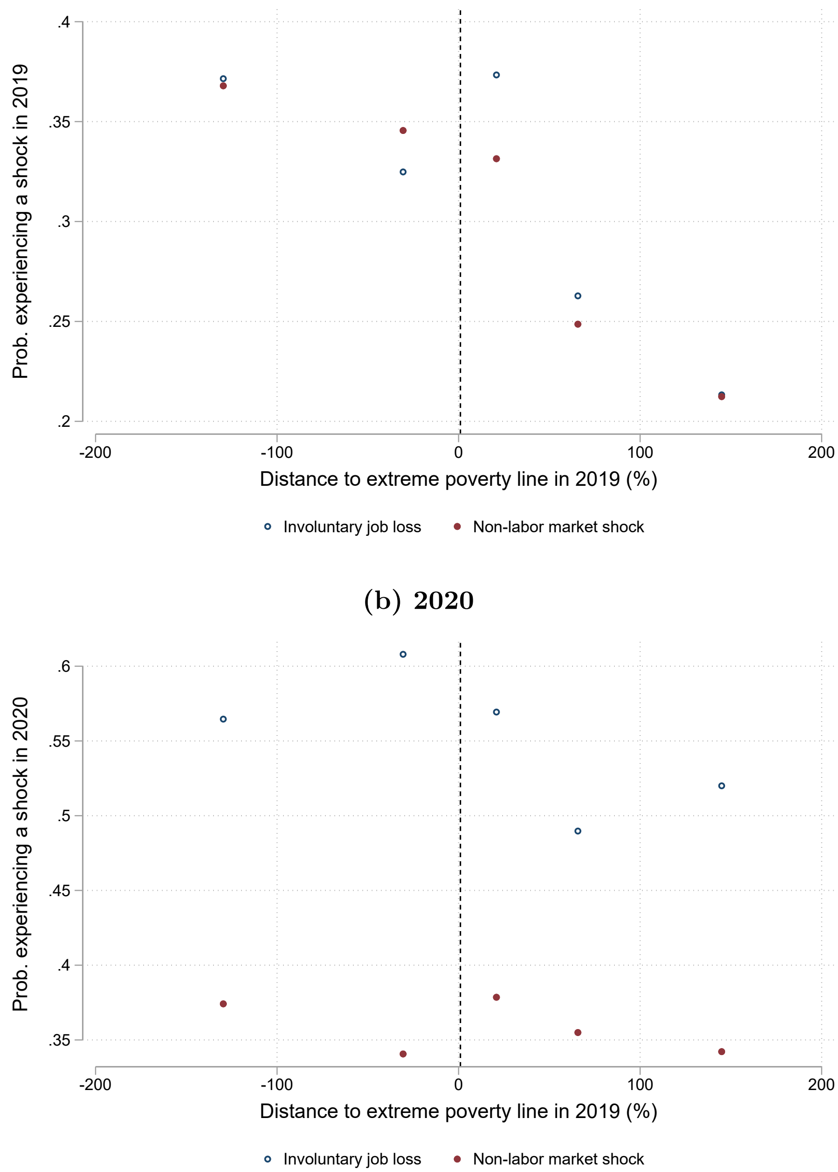


Figure 1: Exposure to Economic Shocks During 2019 and 2020 by Distance to Extreme Poverty Line in 2019

Notes: The figure depicts the probability that a household has experienced the involuntarily job loss of the main earner and any non-labor market shock in 2019 (Panel A) and 2020 (Panel B) by distance between per-capita income in 2019 and the extreme poverty line in 2019. Bins are constructed based on quintiles of distance to the 2019 extreme poverty line as a share of the extreme poverty line in 2019.

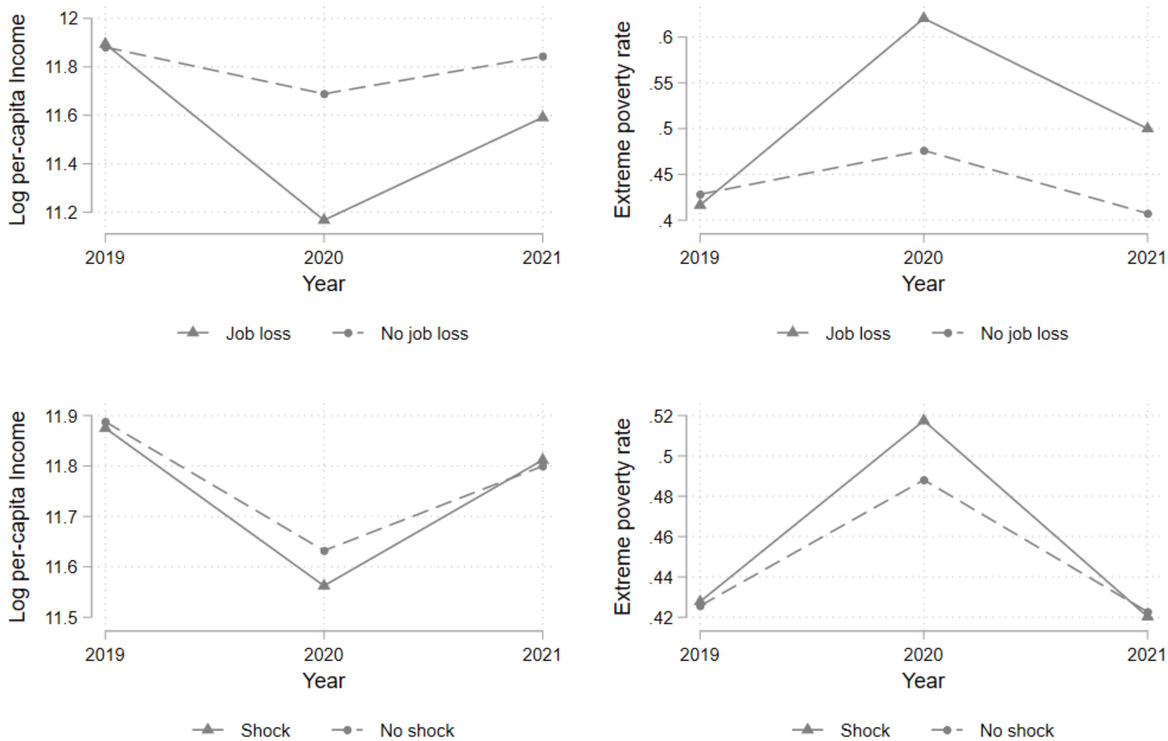


Figure 2: Income and Poverty Dynamics by Exposure to Shocks During 2020

Notes: The figure depicts means over time by exposure to shocks in 2020. The figures on the top distinguish between households whose main earner worked in 2019 but did not in 2020. The figures in the bottom distinguish between households that suffered at least one non-labor market shock in 2020 or not (see Table 1). The figures in the right-hand-side depict means of log per-capita income over time. The figures in the left-hand-side depict the proportion of households with incomes below the 2019 average extreme poverty line in Colombia.

Table 2: **Effects of Shocks on Income, Extreme Poverty, and Hunger**

VARIABLES	(1) Log Per-capita Income	(2) Extreme Poverty	(3) Went Hungry	(4) Log Per-capita Income	(5) Extreme Poverty	(6) Went Hungry
Post X Job Disruption	-0.468***	0.132***	0.0336*			
	(0.0527)	(0.0188)	(0.0172)			
Post X Shock (non labor)				-0.0658**	0.0217	0.0466***
				(0.0327)	(0.0133)	(0.0117)
Observations	11,913	11,913	12,147	11,913	11,913	12,147
R-squared	0.772	0.765	0.704	0.769	0.763	0.704

*** p<0.01, ** p<0.05, * p<0.1

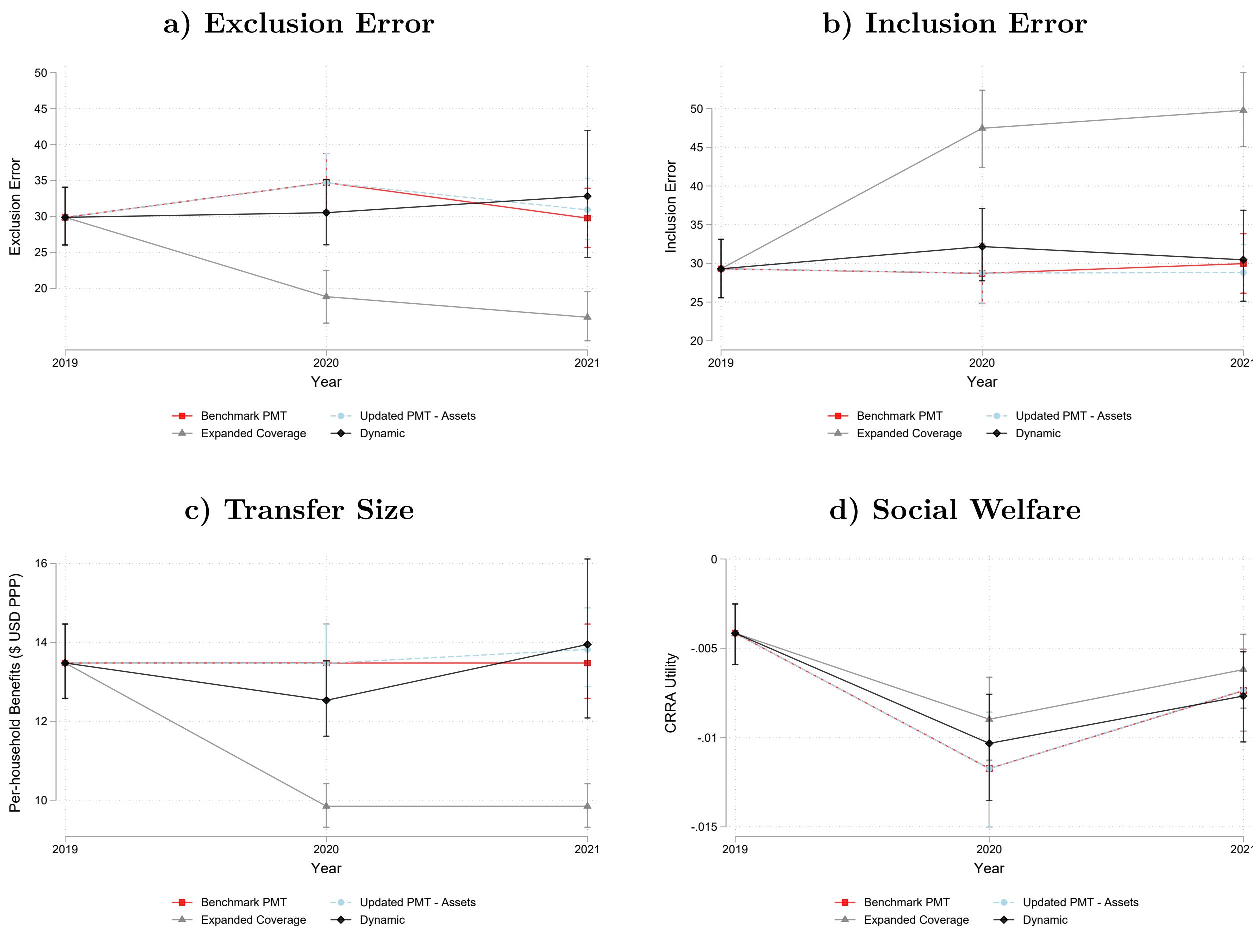
Notes: The table report coefficients corresponding to the specification described in equation (1). Columns 1 to 3 report impacts of labor-market shock, while columns 4 to 6 report impacts of non-labor shocks. All models are estimated using survey data for 2019-2021. Standard errors are robust to heteroskedasticity.

Table 3: Targeting Performance by Targeting Method

	Baseline	Benchmark PMT		Expanded Coverage		Updated PMT - Assets		Dynamic	
	2019	2020	2021	2020	2021	2020	2021	2020	2021
Coverage (%)	47.0	47.0	47.0	64.3	64.3	47.0	45.8	50.6	45.7
	[43.7,50.3]	[43.7,50.3]	[43.7,50.3]	[60.7,67.9]	[60.7,67.9]	[43.7,50.3]	[42.5,49.1]	[46.7,54.4]	[39.2,52.3]
Per-household monthly transfer USD PPP	13.5	13.5	13.5	9.8	9.8	13.5	13.8	12.5	13.9
	[12.6,14.5]	[12.6,14.5]	[12.6,14.5]	[9.3,10.4]	[9.3,10.4]	[12.6,14.5]	[12.9,14.9]	[11.6,13.5]	[12.1,16.1]
Inclusion error (%)	29.3	28.7	30.0	47.5	49.8	28.7	28.8	32.2	30.5
	[25.6,33.1]	[24.8,32.6]	[26.2,33.8]	[42.4,52.4]	[45.1,54.7]	[24.9,32.6]	[25.2,32.4]	[27.7,37.1]	[25.1,36.9]
Exclusion error (%)	29.9	34.7	29.8	18.8	16.0	34.7	30.9	30.5	32.8
	[26.0,34.1]	[30.8,38.7]	[25.7,33.9]	[15.2,22.5]	[12.7,19.5]	[30.7,38.7]	[26.8,35.3]	[26.1,35.2]	[24.3,41.9]
% of households with 0 income after transfers	0.5	1.5	0.9	0.8	0.6	1.5	0.9	1.3	1.0
	[0.2,0.8]	[1.0,2.1]	[0.5,1.3]	[0.4,1.2]	[0.2,0.9]	[1.0,2.1]	[0.5,1.3]	[0.8,1.8]	[0.6,1.5]
% of households in extreme poverty after transfers	40.9	48.1	40.8	47.2	41.0	48.1	40.8	46.9	40.2
	[39.0,42.9]	[46.0,50.1]	[38.9,42.6]	[45.1,49.1]	[39.1,42.9]	[46.0,50.1]	[38.8,42.6]	[44.8,49.1]	[38.2,42.3]
Social Welfare									
ρ=1.5	-54401	-64275	-57666	-63898	-57700	-64276	-57633	-61759	-56754
	[-56706, -51963]	[-67670, -61109]	[-60316, -54897]	[-67048, -60820]	[-60375, -55050]	[-67673, -61114]	[-60280, -54873]	[-66084, -57322]	[-59864, -53736]
ρ=3	-.004155	-.01174	-.007369	-.008974	-.006193	-.01174	-.007349	-.01033	-.007673
	[-.005915, -.002518]	[-.01502, -.008583]	[-.00963, -.005065]	[-.01126, -.00662]	[-.008353, -.004217]	[-.01502, -.008585]	[-.009616, -.005032]	[-.01352, -.007575]	[-.01024, -.005196]
ρ=4.5	-1.08e-08	-3.41e-08	-2.06e-08	-2.08e-08	-1.45e-08	-3.41e-08	-2.06e-08	-2.90e-08	-2.22e-08
	[-1.74e-08,-4.77e-09]	[-4.61e-08,-2.28e-08]	[-2.92e-08,-1.22e-08]	[-2.95e-08,-1.26e-08]	[-2.27e-08,-7.13e-09]	[-4.61e-08,-2.28e-08]	[-2.92e-08,-1.22e-08]	[-4.05e-08,-1.92e-08]	[-3.20e-08,-1.31e-08]

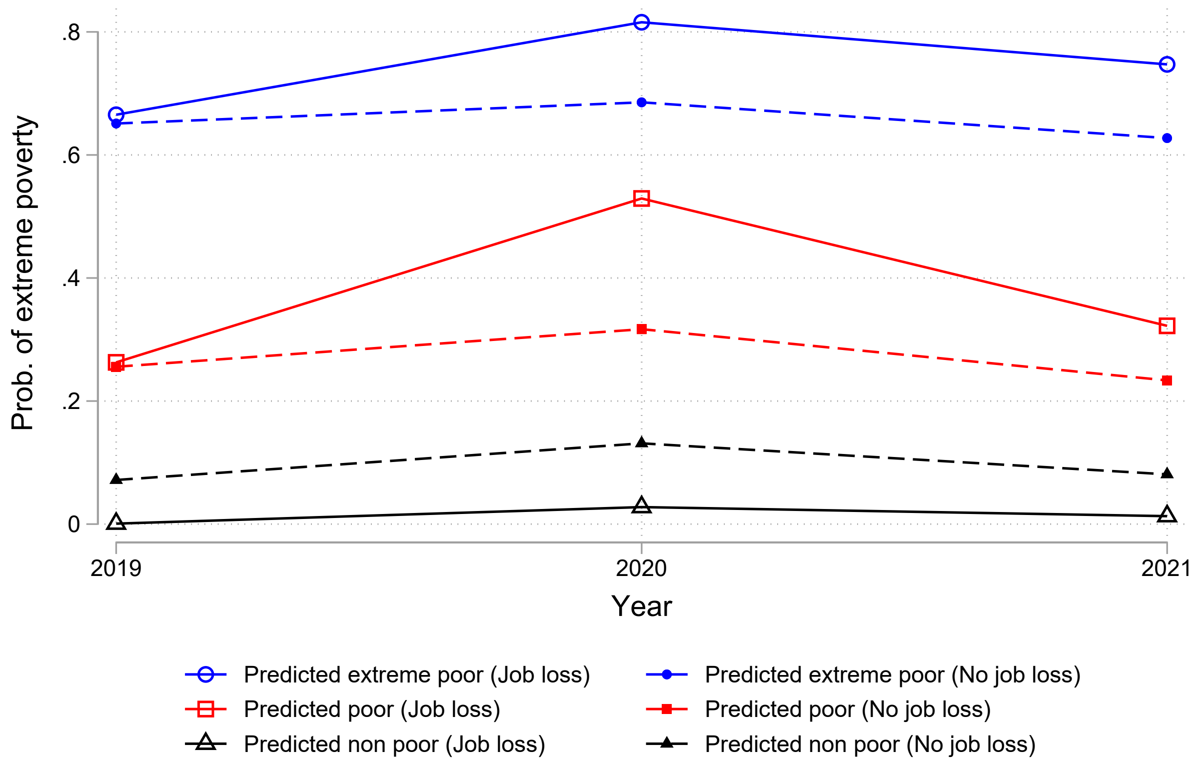
Note: The table reports different targeting metrics under alternative policy regimes. Column 1 reports 2019 baseline estimates, which are common across regimes. In all cases, the underlying targeting model was estimated using a 50% training sample, and the targeting metrics were computed using a 50% testing sample. ρ denotes the curvature parameter of the CRRA Utility function. All estimates are computed by taking averages across 1,000 sample splits. 95% confidence intervals are reported in brackets.

Figure 3: Targeting Errors, Transfer Size, and Welfare over Time under Alternative Regimes



Notes: The figure reports aggregate targeting errors, transfer size and social welfare under alternative regimes. The exclusion error is calculated as the share of households who would be classified as eligible under each targeting tool, but that, in practice, have incomes higher than the program eligibility threshold. The inclusion error is calculated as the share of households who would be classified as ineligible for the program under each targeting tool, but that, in practice, have incomes that fall below the program eligibility threshold. The per-household monthly transfer size is computed by dividing the total program budget (fixed across targeting approaches) by the corresponding number of covered households, based on each targeting tool. Social welfare is computed by adding individual values of a CRRA utility function with curvature parameter $\rho = 3$ across all households in the sample. The 95% confidence intervals are based on 1,000 iterations.

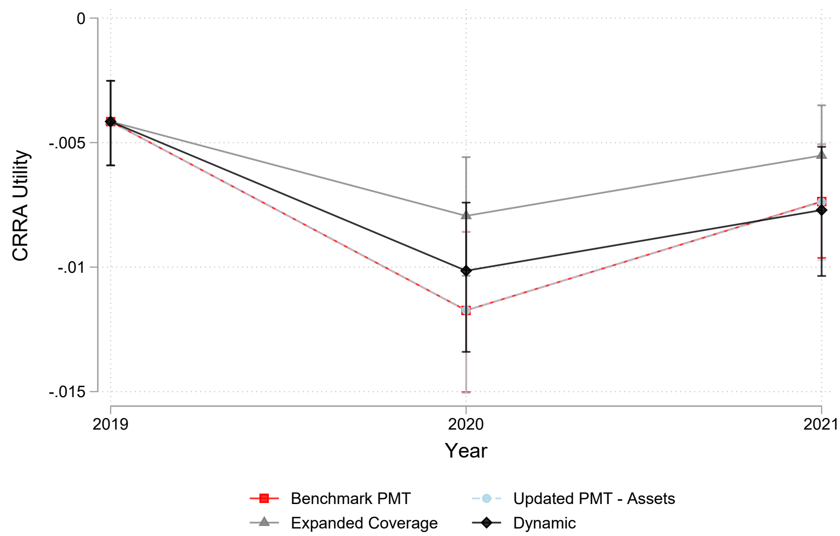
Figure 4: Extreme Poverty Rates by Predicted Pre-Crisis Poverty Status (Benchmark Model)



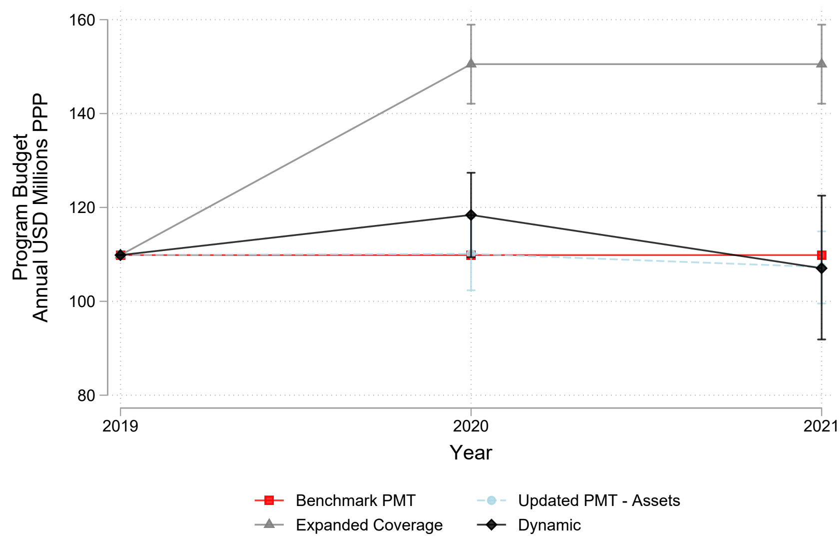
Notes: The figure plots the incidence of extreme poverty in 2019,2020, and 2021 as a function of the predicted poverty status in 2019, based on the benchmark PMT model.

Figure 5: Social Welfare and Fiscal Budget under Alternative Regimes

(a) Social Welfare

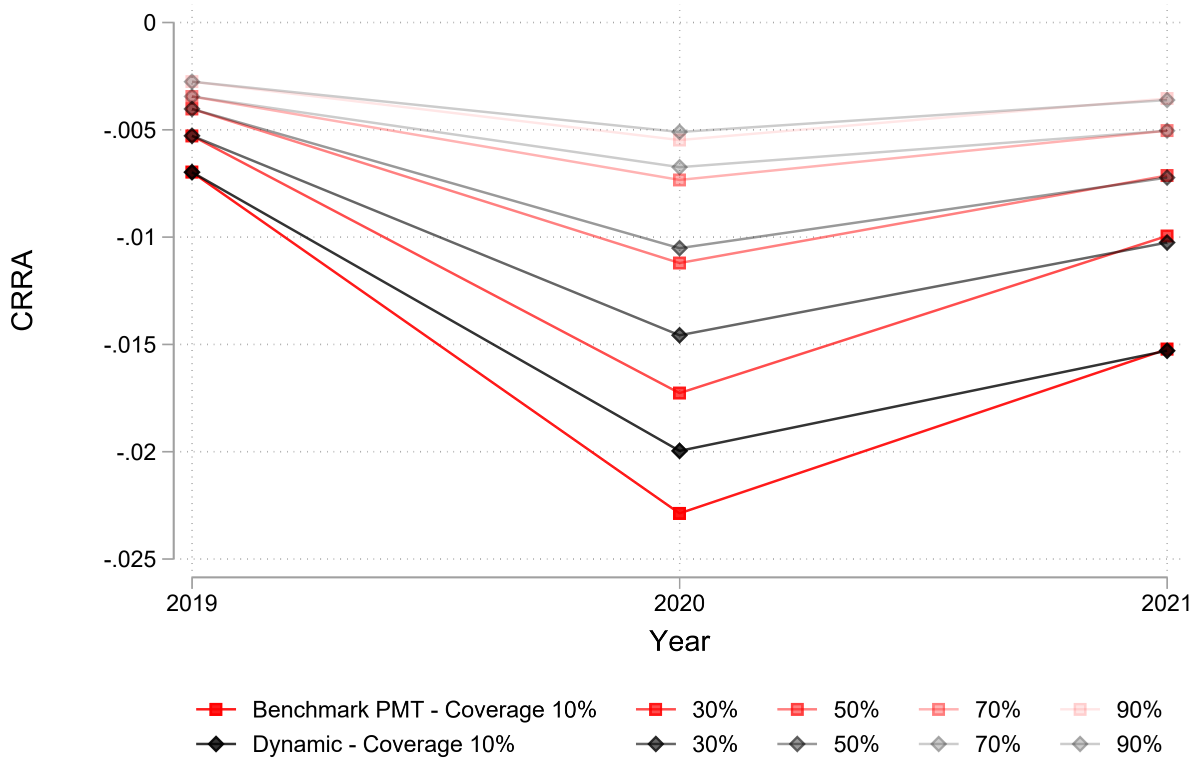


(b) Budget (Millions of USD PPP)



Notes: Social welfare is computed by adding individual values of a CRRA utility function with curvature parameter $\rho = 3$ across all households in the sample. The aggregate budget for under each regime is calculated by multiplying the fixed monthly transfer size by the number of program beneficiaries and multiplying the resulting product by 12 to obtain annual equivalents. The 95% confidence intervals are based on 1,000 iterations.

Figure 6: Social Welfare Changes for Different Coverage Rates by Targeting Tool



Notes: Social welfare is computed by adding individual values of a CRRA utility function with curvature parameter $\rho = 3$ across all households in the sample, by different levels of coverage rate, holding transfer size fixed.

APPENDIX (For Online Publication)

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Table A1: Summary Statistics: Administrative Records

	Surveyed sample	All
Age (household head)	45.40	37.80
	(15.35)	(21.82)
Educational attainment: none	0.07	0.09
	(0.25)	(0.29)
Educational attainment: Elementary	0.51	0.50
	(0.50)	(0.50)
Educational attainment: Secondary	0.32	0.26
	(0.47)	(0.44)
Educational attainment: Tertiary	0.11	0.11
	(0.31)	(0.31)
Works	0.45	0.33
	(0.50)	(0.47)
Formal work	0.13	0.11
	(0.34)	(0.32)
# of household members	2.56	2.68
	(1.44)	(1.53)
Urban	0.72	0.72
	(0.45)	(0.45)
Per-capita Income (1000s of $ CPO)	363.62	408.02
	(408.12)	(500.56)
Owns a fridge	0.51	0.50
	(0.50)	(0.50)
Owns a washing machine	0.24	0.26
	(0.43)	(0.44)
Owns a computer	0.08	0.09
	(0.27)	(0.29)
Owns a motorcycle	0.09	0.09
	(0.28)	(0.29)
Owns a tractor	0.00	0.00
	(0.05)	(0.05)
Owns a car	0.04	0.04
	(0.19)	(0.19)
Observations	4,049	9,956,688

Notes: The table reports sample means, and standard deviations (in parentheses) based on administrative data from the social registry. Column one reports means based on observations from the surveyed sample. Column two reports means based on the universe of observations in the social registry.

Table A2: Baseline PMT Regression

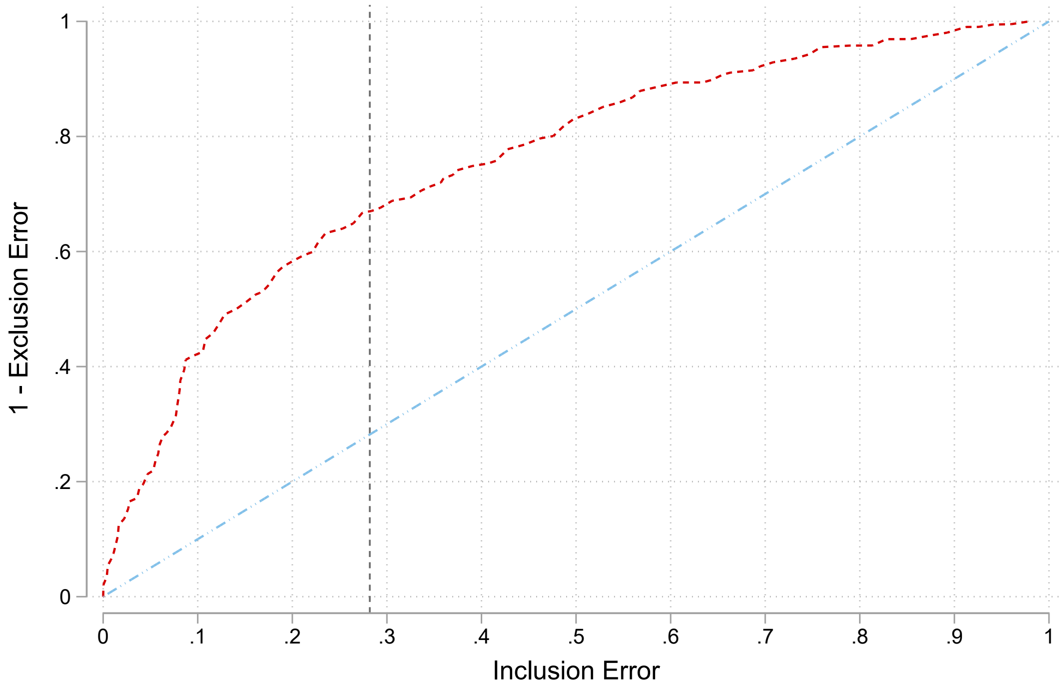
	Log per-capita income
Urban	0.046
	(0.82)
Age - Main earner	-0.008
	(3.89)**
Proportion of kids (under 18)	-0.041
	(0.24)
Number of HH members	-0.116
	(8.77)**
Education: Elementary	0.278
	(2.92)**
Education: High school	0.414
	(3.93)**
Education: Tertiary	0.458
	(3.61)**
Has children	-0.235
	(2.59)**
Household head cohabits with partner	0.061
	(1.32)
Owns Washing machine	0.195
	(3.50)**
Owns Tractor	1.710
	(14.44)**
Owns Motorbike or scooter	0.161
	(2.72)**
Owns Car	0.183
	(1.32)
Owns Computer	0.324
	(3.37)**
Owns Refrigerator or fridge	0.242
	(5.39)**
Finished walls	0.056
	(0.95)
Finished floors	0.301
	(5.43)**
Cooking power gas or electric	0.182
	(2.82)**
WC: with sewer connection	0.075
	(1.06)
Dwelling has a kitchen	-0.076
	(1.07)

Utilities: Electric	0.005
	(0.04)
Water source: aqueduct	-0.169
	(2.77)**
Waste: picked up by the sanitation services	0.124
	(1.60)
Paid formal work	0.595
	(8.49)**
Paid informal work	0.323
	(4.83)**
Constant	11.658
	(59.69)**
R^2	0.27
N	3,860

* $p < 0.05$; ** $p < 0.01$

Notes: The table reports coefficients of a linear regression model estimated using the surveyed sample based on data for 2019. The dependent variable is the log per-capita monthly income in 2019 $ CPO. Standard errors, reported in parenthesis, are robust to heteroskedasticity.

Figure A1: Exclusion and Inclusion Errors: Baseline PMT Approach



Notes: The Figure illustrates the tradeoff between inclusion and exclusion errors corresponding to the benchmark PMT approach, using 2019 data. The curve is computed by iteratively increasing the threshold under which households would become eligible for the program based on their PMT score, following (Hanna and Olken, 2018). The dashed vertical line marks the inclusion error associated to using the benchmark PMT method to determine eligible households for a hypothetical program targeting households with per-capita incomes below the extreme poverty line.

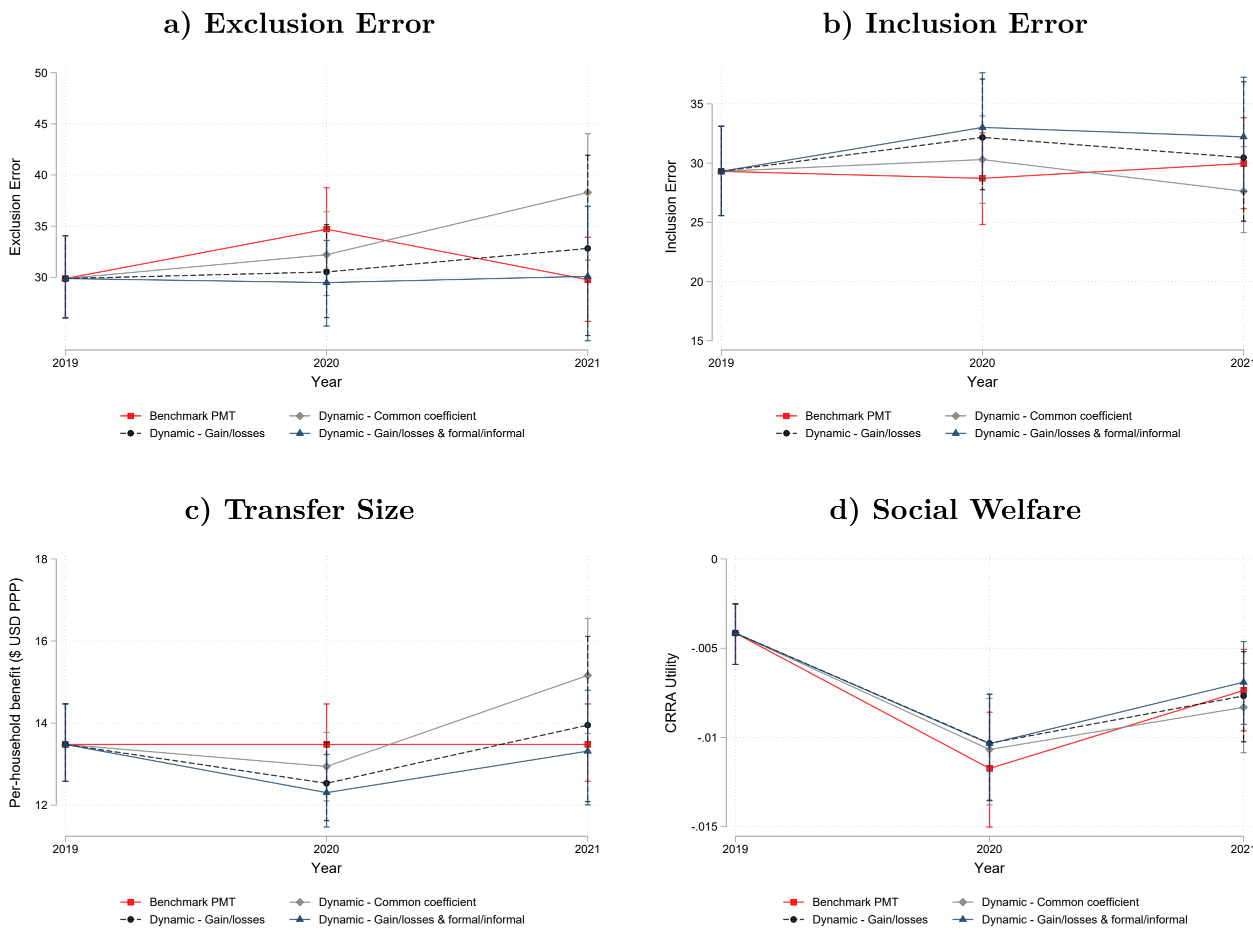
Table A3: Predictive Models for Changes in Income: Income - Employment Co-movements

	(1)	(2)	(3)
Change: Work status	52,174.661		
	(12,767.751)***		
Recovery: Paid work		48,506.212	
		(16,792.713)***	
Loss: Paid work		-55,812.393	
		(19,225.035)***	
Recovery: Paid formal work			67,439.043
			(39,574.115)*
Loss: Paid formal work			-75,295.724
			(18,476.028)***
Recovery: Paid informal work			38,218.195
			(9,985.608)***
Loss: Paid informal work			-54,604.127
			(21,522.763)**
R^2	0.00	0.00	0.00
N	7,777	7,777	7,777

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

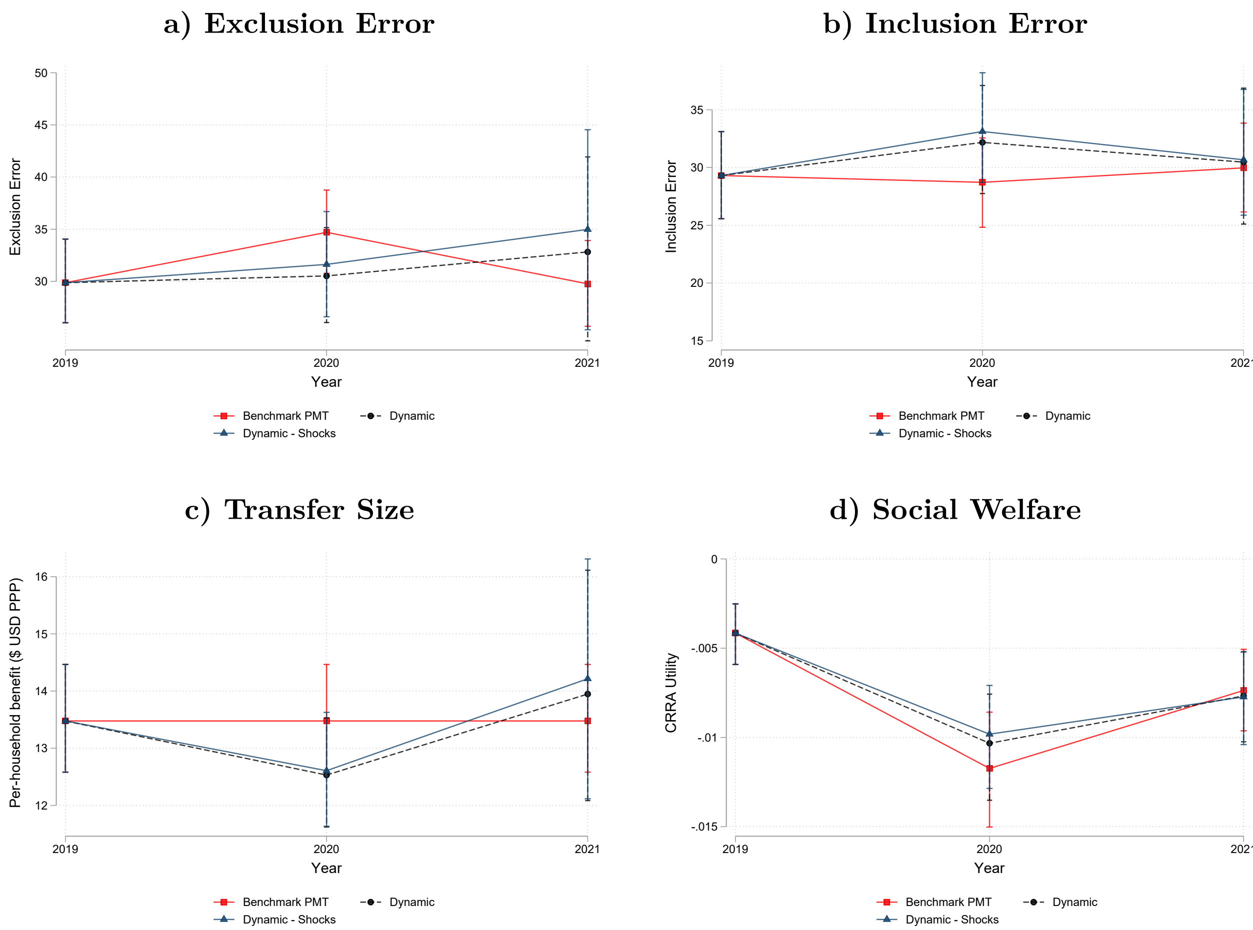
Notes: The table reports correlations between annual changes in income and changes in employment status under several specifications, estimated through OLS. All models are estimated using survey data for 2019-2021. Standard errors are robust to heteroskedasticity.

Figure A2: Robustness: Targeting Errors, Transfer Size, and Welfare over Time under Alternative Dynamic Targeting Models



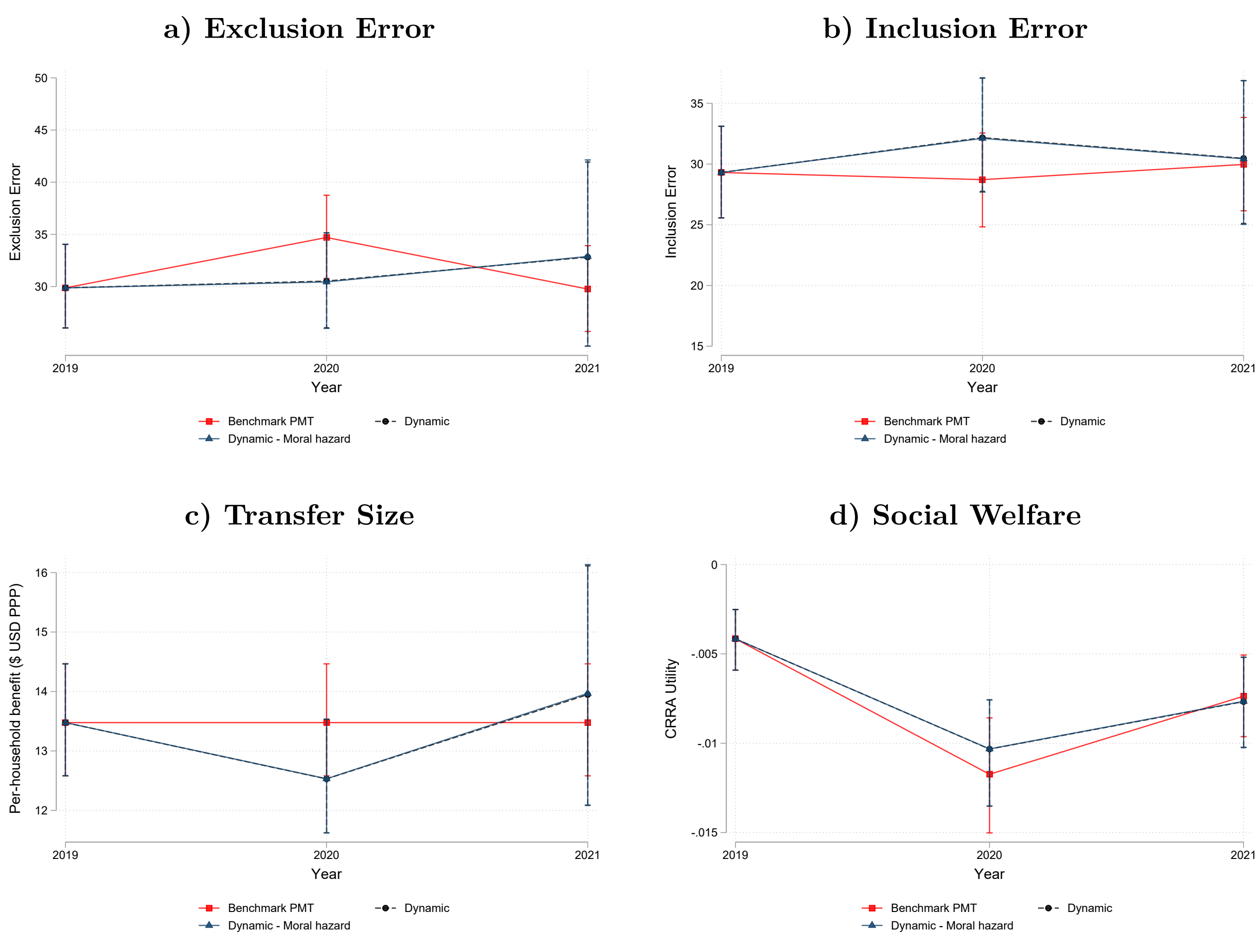
Notes: The figure reports aggregate targeting errors, transfer size and social welfare under alternative regimes holding the program's budget fixed. Social welfare is calculated by adding individual values of a CRRA utility function with curvature parameter $\rho = 3$ to all households in the sample. The 95% confidence intervals are based on 1,000 iterations.

Figure A3: Robustness: Targeting Errors, Transfer Size, and Welfare over Time under Dynamic Targeting Including Negative Non-labor Shocks



Notes: The figure reports aggregate targeting errors, transfer size and social welfare under alternative regimes holding the program's budget fixed. Social welfare is computed by adding individual values of a CRRA utility function with curvature parameter $\rho = 3$ across all households in the sample. The 95% confidence intervals are based on 1,000 iterations.

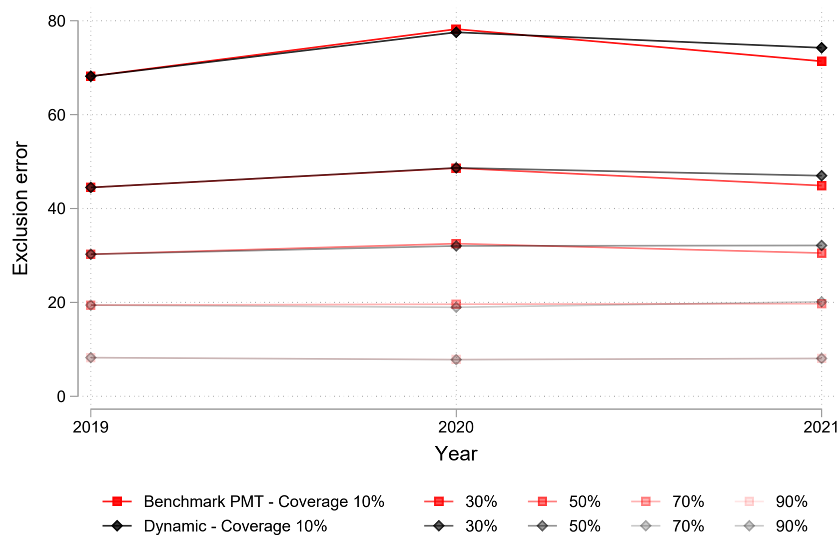
Figure A4: Robustness: Targeting Errors, Transfer Size, and Welfare over Time Correcting for Moral Hazard in Reported Employment



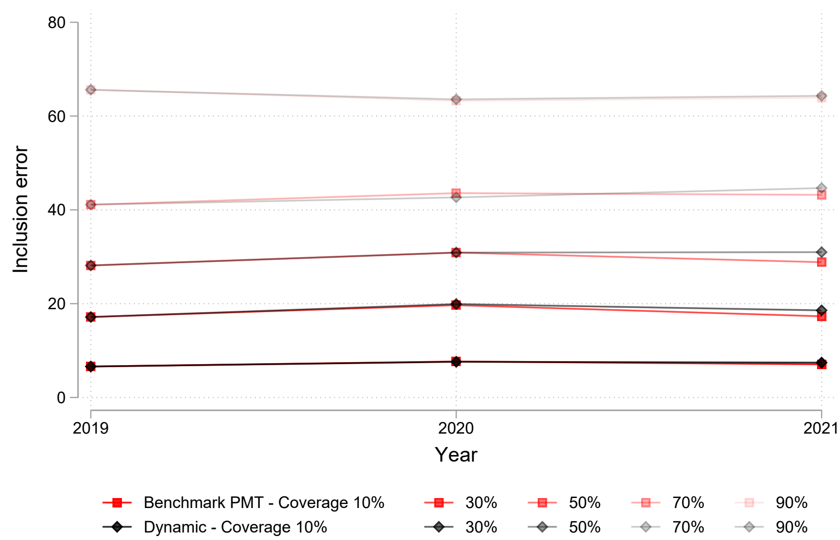
Notes: The figure reports aggregate targeting errors, transfer size and social welfare under alternative regimes holding the program's budget fixed. Social welfare is computed by adding individual values of a CRRA utility function with curvature parameter $\rho = 3$ across all households in the sample. The adjustment for moral hazard assumes that 8.7% of already employed households report not working. The 95% confidence intervals are based on 1,000 iterations.

Figure A5: Exclusion and Inclusion Errors by Different Coverage Rates and Targeting Tools

(a) Exclusion Error



(b) Inclusion Error



Notes: The figure reports exclusion of inclusion errors, by different levels of coverage rate and targeting method.