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Can social pensions for the elderly mitigate shocks?

Lessons from Mozambique

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Abstract: This study investigates the contribution of Mozambique's flagship social pension programme, the *Programa de Subsídio Social Básico*, to building resilience against shocks. Applying a fuzzy regression discontinuity approach to bespoke survey data, we separate direct effects of programme transfers from anticipation effects related to becoming programme-eligible. Our results show that while eligibility is associated with adopting more positive coping strategies, the impact of transfers is mixed. Specifically, we find that transfers made close in time to major climate shocks offer some short-term benefits. However, we demonstrate that increasingly acute operational challenges, including extended delays in receiving payments, have materially weakened household resilience. Complementary analysis of nationally representative household budget data confirms that programme transfers support significant consumption gains, but these fade within around six months. These findings highlight the critical importance of timely payments and reliable 'last mile' administration to fulfil the programme's potential for effectively supporting vulnerable populations exposed to shocks.

Key words: social protection, elderly, pensions, fuzzy regression discontinuity, Mozambique

JEL classification: H53, I38, Q54

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1 Introduction

> [C]ash transfer programs overall have important implications for household resilience. By providing a steady and predictable source of income, cash transfer programs can build human capital, improve food security, and potentially strengthen households' ability to respond to and cope with exogenous shocks, allowing them ... to prevent future fluctuations in consumption. (Asfaw and Davis 2018: p. 69)

Over the past decade, two complementary hypotheses have emerged regarding the contribution of existing social protection systems to shock responses in developing countries (O'Brien et al. 2018). The first 'piggy-back' hypothesis contends that long-term social protection systems can provide an operational basis for rapid shock response, providing a degree of short-run counter-cyclical insurance (Alderman and Haque 2006; Beazley et al. 2021; Devereux 2021). Suggested by the quotation above, the second 'resilience' hypothesis states that programme beneficiaries can build up their innate or anticipatory capacity to respond to shocks, reducing the need to resort to survivalist coping strategies that undermine future well-being.[1]

In principle, these positive impacts can be achieved by a wide range of social protection systems, including government-run social pensions, defined here as non-contributory and unconditional transfers to a target group of eligible persons, typically the elderly. In practice, social pensions raise particularly complex challenges that may undermine their broader effectiveness. As elaborated further below (Section 2), such programmes can generate both anticipation effects and unintended (negative) consequences, as well as face ongoing implementation problems, which can be particularly acute in low-income contexts where needs are great but operational capacity is limited. However, these challenges have only rarely been considered by previous academic quantitative studies of cash transfer programmes, which have tended to implicitly assume that programme delivery is unproblematic.[2]

Our point of departure is that close empirical examination of specific social protection programmes as they function at scale is warranted. Responding to this, the present study contributes new quantitative evidence regarding the contribution of Mozambique's social pension scheme to household resilience, proxied by asset ownership, taking into account the consequences of both anticipation effects and payment delays in particular. We focus on the elderly component of the country's *Programa de Subsídio Social Básico*, offered to labour-constrained, vulnerable elderly persons aged 60 and over. Established in the 1990s, the PSSB-Elderly has grown to encompass over 400,000 individuals, entitling them to a cash transfer of around US$8 per month. At the national level, around 40% of all heads of households aged 60 and above are enrolled in the programme; and based on data collected for this study (Section 4), we find around two-thirds of persons aged 60+ (the threshold for eligibility) are covered in selected locations. Even so, operational challenges dog the programme, worsening over recent years (for an earlier assessment, see MGCAS 2022). In particular, transfers are not indexed to living costs, registration and enrolment are cumbersome, and payments have become increasingly irregular—at the time of our survey none have been made for at least eight months. As such, the *de facto* contribution of the programme to household resilience and other outcomes is ambiguous.

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[1] Following DFID (2011), resilience is broadly defined here as the ability of an individual or household to maintain their living standards in the face of external shocks or stressors.

[2] This assumption is indeed captured by the quotation at the start of the paper. In part, ignoring delivery issues reflects researchers' preference for smaller-scale (randomized) experimental interventions, where implementation is closely controlled. As discussed in different contexts (Bold et al. 2018; Davis et al. 2017), results from such studies plausibly only give upper-bounds on the magnitudes of programme effects that may be expected at scale.

We assess the contribution of the PSSB-Elderly to shock response and resilience using two complementary data sources. The first is a bespoke survey of PSSB beneficiaries and comparison households residing in shock-prone locations. Leveraging an age-based eligibility threshold for causal identification, as per a fuzzy regression discontinuity design (e.g. Alloush et al. 2024), we separate the effects of the programme into two distinct components: a direct transfer effect, associated with formal programme enrolment; and an indirect anticipation effect, associated with becoming programme-eligible. As discussed in Section 3, the latter effect is especially germane since individuals are either formally or informally classified as programme-eligible prior to enrolment, possibly remaining in this intermediate status for long periods.

Distinguishing between these anticipation and transfer effects appears important. We find that households eligible to receive the PSSB tend to engage in more positive coping strategies in response to shocks. However, with respect to resilience and shock responses, the pure transfer effects associated with the programme appear to be negative, particularly among recent enrollees and those that have not received payments for a long period. Indeed, since beneficiaries in our sample had not received any PSSB payments for at least eight months, we find that payment delays appear to have contributed to a worsening of households' capacity to withstand shocks. Nonetheless, examining experiences associated with a specific major weather shock—namely, Cyclone Gombe in 2022—we find some evidence of positive effects of PSSB transfers on coping strategies when payments occur close in time to such events.

Our second data source is two recent nationally representative household surveys, which we combine with PSSB digital registry data to examine the link between payment timing, shocks, and outcomes. Using a similar fuzzy regression discontinuity strategy, we find large positive effects of the transfers on consumption but also clear evidence that such benefits dissipate over time. Confirming findings from our bespoke survey, our estimates suggest that the consumption gains associated with the PSSB fully dissipate within at least one year. We also find weak evidence of additional consumption benefits when transfers occur close in time to weather shocks and that PSSB (recent) beneficiaries are also less likely to self-report experiencing a negative weather shock.

Taken together, and in keeping with a large body of prior evidence, the findings presented here confirm the PSSB-Elderly programme does have some *potential* to enhance welfare and resilience to shocks among poor elderly households. However, material and ongoing operational challenges, resulting in long payment delays and unpredictable registration (expansion) processes, mean the programme's potential is not being fulfilled. At worst, we find that ongoing failures to undertake payments in a timely fashion negatively impact household resilience and responses to shocks. Furthermore, at least part of the benefit of the programme appears to stem from anticipation effects, which we associate with crowding in of (new) household members as well as social network effects. This sober assessment underlines the critical importance of reliable 'back office' functions, professional staffing, and consistent funding to support social protection schemes, particularly for recurrent programmes such as old-age pensions. Minimally, getting the basics of programme delivery right would seem to be a fundamental prerequisite to achieve broader desired goals.

2 Relevant literature

This study connects to four themes within the extensive literature on social protection. We take as given that giving cash to the poor and vulnerable almost always yields net positive short-run effects on consumption (for reviews, see Beegle et al. 2018; Harman et al. 2016). Here, the pertinent question is whether these same programmes generate sustained longer-term benefits, including enhanced resilience against shocks, as well as the necessary conditions for such effects to emerge. On this issue, however, evidence is both limited and mixed, especially as it pertains to social pensions. Indeed, among 27 studies identified by Harman et al. (2016) as containing high-quality evidence on the savings, investment, and production effects of cash transfer programmes, none refer to elderly pensions and the majority are comparatively small-scale pilot (experimental) interventions.

With respect to the effects of larger programmes on resilience, Abay et al. (2022) evaluate the effect of Ethiopia's Productive Safety Nets Program on household resilience using longitudinal data. They find the programme boosts resilience, but only when transfer amounts are material and beneficiaries are enrolled continuously in the programme for an extended period. A similar conclusion is drawn by Otchere and Handa (2022) in the context of Malawi, finding that regular and predictable cash grants significantly boost household resilience, operationalized as an index capturing access to services, household adaptive capacity, and asset ownership. And, looking across a broad range of programmes, Andrews et al. (2018) find systematic increases in livestock ownership from participation, but mixed (inconclusive) effects on some other proxies for longer-run household resilience on average, such as use of child labour and outward transfer capacities.

As hinted, various reasons have been suggested for these more nuanced (less than stellar) effects of cash transfers on longer-run outcomes, including resilience. The first is (unintended) offsetting effects, particularly on labour supply. While various studies find that cash transfers directed towards working-age able-bodied adults generally do not lower engagement in productive activities (employment), findings are less clear for transfers to the chronically ill or elderly. As discussed by Abel (2019), existing evidence for South Africa's old-age pension has been contradictory on this matter. Using an instrumental variables strategy, he finds increases in the number of eligible household members are associated with reductions in overall household labour supply, especially in formal work.[3] These findings echo other studies that indicate cash transfers may crowd-out private transfers (e.g. Jensen 2004; Nikolov and Bonci 2020), but also may crowd-in new members to the household (see below; also see Özler et al. 2021), resulting in complex final welfare effects that vary across programmes and contexts.

A second explanation for nuanced effects lies in the domain of programme design and implementation. Putting aside the issue of transfer amounts, it is reasonable to assume that (more) positive long-run effects are (more) likely to emerge when transfers are predictable, non-inflationary, and free from social or political interference—for example, as regards who receives transfers or how transfers are used. Rigorous quantitative estimates of the effects of deviations from this ideal are rare. Nonetheless, there is widespread anecdotal evidence that such deviations are commonplace, with numerous reports of payment delays, technical glitches, and registration errors, which in turn are seen to undermine programme efficacy (e.g. Das et al. 2023; Gronbach et al. 2022; Palermo and Zuilkowski 2020; Roelen et al. 2015). For example, as set out by Basu et al. (2024), India's public works scheme NREGA requires payments must be made within 15 days of project completion, but in 2016–17 less than one-third of recipients received payments within 30 days and nearly one-quarter waited for at least 90 days. Consistent with a theoretical model linking payment delays to lower welfare (permanent income), the authors find a vicious cycle whereby greater delays induce higher programme participation.

Finally, in contrast to spatially or temporally narrow programmes, social pensions are often both sufficiently large in scale and well-known that individuals form beliefs over their future eligibility and possibly alter their behaviour in expectation of receiving transfers. Anticipation effects of this sort have been found with respect to retirement and pensions in various high-income contexts (e.g. Bottazzi et al. 2006; Engels et al. 2017; Merz 2018). And while there is no particular reason why they might not apply to social protection schemes in developing countries (see Attanasio and Mesnard 2006), relevant

[3] For similar evidence in other contexts, see Kaushal (2014), Mena and Hernani-Limarino (2015), and Zimmermann (2024).

evidence has remained scant.[4] Nonetheless, Edmonds et al. (2005) and Edmonds (2006) find significant changes in household composition, hours worked, and schooling decisions when black households become eligible for the South African old-age pension, which is attributed to their anticipated increase in income. Similarly, Salinas-Rodríguez et al. (2014) point to changes in mental health and empowerment (participation in decision-making) in anticipation of receiving the '70 y más' social pension in Mexico.[5]

In sum, our reading of the literature suggests that while social pensions certainly have the potential to improve various welfare metrics, including households' resilience to shocks, such benefits are not guaranteed. Unintended consequences, implementation challenges, and anticipation effects can modify or weaken such outcomes, reinforcing the need for careful examination of individual programmes as they operate at scale. In this spirit, we now turn to an examination of Mozambique's social pension.

3 Programme context

The PSSB-Elderly traces its origins to the *Programa de Subsídio de Alimentos* (food subsidy programme), introduced in the 1990s (Soares et al. 2010). Its current form stems from the first National Strategy for Basic Social Security (ENSSB, 2010–14), which established the focus on labour-constrained vulnerable households. Thus, unlike schemes in some other countries, the PSSB-Elderly has never been a universal benefit. According to the current procedures manual (INAS 2022), which sets out the formal beneficiary selection process, four main stages should be followed to enter the programme. The first is geographic prioritization, aimed to identify high-poverty communities. The second is a form of community-based selection, whereby a public list of 'potential beneficiaries' is drawn up, comprising persons deemed to meet all relevant eligibility criteria. For the PSSB-Elderly, these include being 60+ years old, as well as being poor and having no outside source of (financial) support, such as access to a contributory pension.

Listing of potential beneficiaries is run by volunteer local intermediaries, known as *permanentes*, typically prominent senior community members such as teachers or village elders. These *permanentes* play a crucial role in administering all aspects of different social protection programmes on the ground, serving as the bridge between (potential) beneficiaries and the relevant authority (*Instituto Nacional de Acção Social*, INAS), receiving a small honorarium for services rendered.[6] Third, individuals on these lists are subject to registration and verification visits by INAS officials, which also serve to collect socioeconomic data on potential candidates and assess their vulnerability status. In principle, such assessments can be used to select which of the genuinely eligible candidates should be prioritized in future programme expansion rounds.

Indeed, as the manual makes clear, being formally verified as eligible is necessary but not sufficient to actually receive PSSB transfers. The last step—formal enrolment (*inscrição*)—crucially depends on

[4] Perhaps this is because anticipation effects are often viewed by researchers as a nuisance to causal identification and therefore (where possible) are eliminated in *ex ante* experimental research designs. In addition, due to credit and liquidity constraints, anticipation effects are often not expected to be material for monetary outcomes, such as consumption.

[5] Galiani et al. (2016), however, find no anticipation effects on earnings or savings of a different non-contributory old-age pension in Mexico.

[6] Referring to the pivotal role played by these *permanentes*, Jones et al. (2016) comment that: 'The lack of an independent liaison agent and effective checks and balances on power therefore hampers transparency and opportunities to exercise social accountability' (p. 1219). It also merits note that many *permanentes* appear to be beneficiaries of one or other INAS programme in their own right.

central budget allocations, which in recent years have proven highly irregular across time and space.[7] As a result, even in theory, poor elderly individuals can be expected to be found at different stages of the selection process, the stages being: ineligible → potential candidate → validated candidate → enrolled beneficiary.

Furthermore, both our interactions with INAS and qualitative insights from fieldwork revealed that various components of the above textbook selection process are not consistently followed in practice. In particular, digitized socioeconomic data on potential candidates is missing, a proxy means test to filter and prioritize the same candidates has not been finalized, and wait lists (*listas de espera*) appear to operate differently in different locations, if at all. In some places, INAS staff hold written lists of individuals who have been formally validated to enter the programme (as per stage 3); in other locations, the *permanente* has an informal list of potential candidates, reflecting who would be put forward for validation in the future (in line with stage 2); and in others, no attempt seems to have been made to construct such lists, apparently so as not to create inflated expectations as well as due to the absence of budget space. Additionally, required annual INAS verification visits to check on existing beneficiaries (e.g. proof of life) do not happen systematically due to resource limitations. In sum, capacity to follow formal procedures is limited and there is considerable local variation in day-to-day operational practices around the PSSB, which appears intimately related to the reliance on volunteer *permanentes* to identify and interact with (potential) beneficiaries.

4 Vulnerable Lives Survey

4.1 Data

Dubbed the Vulnerable Lives Survey (VLS), in 2024 we designed and implemented a survey of PSSB-Elderly beneficiaries and comparison households in Mozambique. The aim of the survey was to provide rigorous evidence about the functioning and impacts of the programme in shock-prone contexts. Six communities were chosen from each of the three major regions (North, Center, South), selected on the basis of exposure to different kinds of past shocks. Four of the communities had experienced severe disruptions to livelihoods during the civil conflict of the 1980s and 1990s, associated with paralysis of agricultural employment on commercial sugar plantations, in some places lasting to the present day. The other two communities had been exposed to recent climate shocks, namely the 2022 Cyclone Gombe in Nampula Province. Full details regarding the sample locations, survey design, and instruments are contained in Berkel et al. (2024).

The key survey design challenge is how to construct a plausible group against which current PSSB beneficiaries can be compared. Ideally, a probability sample could be taken from the universe of older adults in each community, stratifying on PSSB status and distinguishing between those on a wait list (potentially eligible) and current beneficiaries (enrolled). Unfortunately, this is not feasible. Complete residential lists are unavailable and would be too costly to manually collect at the geographic level of interest—here, entire administrative posts (*postos administrativos*). Furthermore, a conventional two-

[7] The manual states that: 'As Delegações do INAS-IP determinam o número de potenciais beneficiários a inscrever com base na informação sobre os limites orçamentais para todas as componentes do programa e obtêm as listas de potenciais beneficiários a inscrever e em espera. A linha de corte é específica por cada Comunidade e para cada componente do PSSB e é calculada como a fracção entre o orçamento disponível (numerador) e a estimativa do montante anual da transferência (denominador)' [The INAS-IP delegations determine the number of potential beneficiaries to be enrolled based on information about budget limits for all components of the programme and obtain lists of potential beneficiaries to be enrolled and placed on the waiting list. The cut-off line is specific to each community and each component of the PSSB and is calculated as the ratio between the available budget (numerator) and the estimated annual transfer amount (denominator)] (p. 21). This implies that coverage will vary across different communities, depending on budget allocations rather than just needs.

stage approach involving random sampling of geographic clusters followed by enumeration of all households in each cluster would not deliver a sufficient sample of our target population at an acceptable cost. Indeed, older adults in Mozambique are a somewhat hard-to-survey population—for example, at the national level just 15% of individuals are aged over 60; and in poor rural areas, covered by our survey, such individuals are less prevalent and often physically more isolated. In turn, PSSB beneficiaries are a subset of a rare group.

In light of these constraints, our point of departure for the design is the digital list of enrolled PSSB-Elderly beneficiaries. In collaboration with the Ministry of Economy and Finance and the National Institute for Social Assistance (INAS, who run the PSSB programme), we were given access to the digital registry of enrolled beneficiaries, from which we took a random sample in each of the chosen locations. While the registry data is limited and does not provide addresses or specific locations for each beneficiary, we were also able to work with the local INAS *permanentes* to direct us to each participant chosen from the list.

Echoing the challenges of two-stage cluster sampling, geographic spot randomization—that is, listing households in a radius of the homesteads of selected beneficiaries—proved unworkable due to large distances between households and a scarcity of household members aged over 40, the age floor required for participation in the survey (among the comparison group). Consequently, we defined the comparison group as being members of the social network of current beneficiaries aged 40+. In the original spirit of Goodman (1961), we constructed a one-stage five-name snowball sample of non-PSSB beneficiaries using referrals from beneficiaries selected from the digital registry. Concretely, in each interview from the registry we asked for names of five individuals aged over 40 from nearby households who did not receive the subsidy; from this list at least one was then randomly chosen to interview.

As discussed in Goodman (2011), since the zero (seed) stage of survey participants is based on a probability rather than a convenience sample, the subsequent referral wave can be treated as a probability sample of connections into the social network of the seed population.[8] This approach effectively yields a probability sample of clusters, where each cluster is a unique group of seeds and their referrals (beneficiaries and named comparisons). In turn, standard inferential procedures in the presence of multiple clusters can be adopted (see Kennedy-Shaffer et al. 2021). Further details on our econometric implementation are provided below.

The foregoing does not explicitly address how to sample individuals on a wait list—that is, the potentially eligible but not enrolled. As already noted, there is a potentially lengthy intermediate period between being either formally or informally classified as PSSB-eligible and finally enrolling in the programme. Moreover, we cannot rule out the hypothesis that recognition of eligibility may have an independent effect on a range of relevant outcomes. For instance, since PSSB transfer values are calculated in relation to household size, there may be incentives to adjust household membership prior to formal validation and registration in the scheme, with a variety of knock-on effects such as access to labour supply. Similarly, anticipation of benefits may induce changes in economic activity and it is also feasible that other actors in the community (such as NGOs) use the same (informal) lists of vulnerable households to distribute other kinds of support.

In short, to the extent we wish to disentangle any eligibility effects from those of enrolment, it would be unwise to ignore wait-list individuals from our survey. Nonetheless, a robust probability sample of wait-list individuals similar to that of enrolled beneficiaries is not available—presently individuals only enter the digital registry once enrolled, not before; and curated wait lists are not available in all

[8] As the author puts it: 'This [type of] snowball sampling in survey research is amenable to the same scientific procedures as ordinary random sampling from a given population. Where the population in ordinary samples is a population of individuals, here it is two populations: one of individuals and one of relations among individuals' (pp. 348–49).

locations. As a result, we relied on referrals from local *permanentes* who provided our survey team with identities of wait-listed persons residing in the same communities as existing beneficiaries, where available.[9] For inferential purposes, we randomly assign each interviewed wait-list person to one of five geographically nearest beneficiary-comparison clusters; and we allocate sample weights in accordance with the approximate ratio of wait list to beneficiaries in each community.[10] We recognize that this component of our design is not optimal; nonetheless, the process by which wait-list individuals enter our sample roughly mimics the process by which individuals are selected to enter the programme, relying on *permanentes* for referral. Furthermore, as elaborated below, we consistently treat membership of the wait list as endogenous (mis-measured) and we validate robustness of our results to alternative definitions of the wait list.

4.2 Empirical methods

To summarize the previous section, our sample contains three distinct groups: (1) current PSSB beneficiaries, randomly selected from the INAS digital register; (2) a random sample from the social network of the same beneficiaries, presumed to share similar characteristics; and (3) a non-probability sample of eligible (wait-list) households, identified via referrals from local INAS volunteers. The ensuing methodological challenge, discussed here, is how to identify programme impacts.

Our point of departure is Alloush et al. (2024), who analyse the effects of a broadly similar social pension in South Africa during the COVID-19 pandemic. As in their case, participation in the PSSB-Elderly is not universal but is targeted at the needy, and the criteria used to determine need are not completely observed. While participation therefore is endogenous, we can nonetheless use age-eligibility as an exogenous threshold to identify the impacts of becoming a beneficiary.[11] Specifically, on the assumption that (1) crossing the age-eligibility threshold (here, being 60+) is associated with a significant conditional increase in the probability of participation; and (2) there is no independent effect of crossing the threshold on outcomes (other than that running through participation), then the age threshold can be used as a relevant and valid external instrument for scheme participation.

In the context of the PSSB-Elderly, the main complication of this strategy concerns anticipation effects. Echoing previous discussion, and as set out in the directed acyclic graph (DAG) of Figure 1, moving across the PSSB age-eligibility threshold is not uniquely associated with becoming a beneficiary, but also with becoming a potential candidate—for example, entering a wait list or anticipating a future transfer. Where eligibility independently affects outcomes (Y), as captured by parameter $\gamma \neq 0$, then contrary to assumption (2) above, the age threshold cannot be a valid instrument for receipt of the PSSB transfer alone (for discussion in a separate context, see Mellon 2024). Simply put, a reduced-form regression of the age-eligibility instrument on a given outcome will capture a combined effect composed of the impact of becoming eligible (γ) and the more specific enrolment or transfer effect ($\alpha \cdot \beta$).

To address this, we proceed in two steps. First, we recognize that the reduced-form effect remains of analytical interest since it captures the total effect of the programme across the eligible and the enrolled. And for this effect, the proposed IV approach in which age-eligibility is used as an external instrument should be valid conditional on various controls, including age. Second, as described in Bellemare et al. (2024), we can use a front-door criterion to further isolate the pure transfer effect (also Thoemmes

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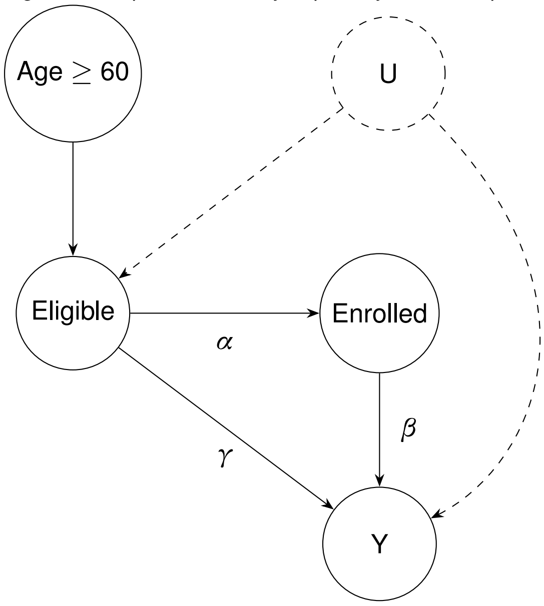
[9] In practice, the number of wait-listed individuals varied across survey locations, with more complete lists being available in the two peri-urban locations (Vila de Marromeu and Municipio de Manhiça).

[10] For instance, if there is one wait-listed individual for every ten beneficiaries, we assign the former a weight equal to 1/10 of the latter. These weights can be seen as crude forms of pseudo-inclusion probabilities, used elsewhere to combine probability and non-probability samples (e.g. Elliott and Valliant 2017).

[11] The South African pension has been extensively studied, typically invoking age-eligibility as an exogenous source of identification (e.g. Abel 2019; Edmonds 2006; Edmonds et al. 2005).

and Kim 2023). Under the assumption—discussed below—that unobserved confounders of programme participation only effect eligibility, but not later enrolment (or transfer receipt), it follows that the relationship between enrolment and outcomes should not be confounded conditional on being eligible. This is illustrated in the DAG by the back-door path connecting eligibility and Y through U; conditioning on eligibility in a regression of enrolment on Y will thereby block this back-door path.

Figure 1: Graphical summary of primary relationships



Note: the figure sets out hypothesized relationships between the main variables as per a DAG; U is an unobserved (latent) variable; Y is the outcome of interest.

Source: authors' own elaboration.

Under these assumptions, all quantities of interest can be identified from two main sets of equations. First, we have an IV regression analysis:

$$\text{Eligible}_i = \mu_{1k} + \theta[\text{Age} \geq 60\ ]_i + X_i'\delta_1 + \varepsilon_{1i} \tag{1a}$$

$$Y_i = \mu_{2k} + \varphi\text{Eligible}_i + X_i'\delta_2 + \varepsilon_{2i} \tag{1b}$$

where i indexes individuals, k indexes locations, X are observed controls including age and its square (see below), and ε are residual noise terms. Allowing $\mathrm{E}(\varepsilon_{1i}\varepsilon_{2i} \neq 0)$, Equations (1a) and (1b) provide a basis for fuzzy regression discontinuity estimates of the relationship between eligibility and Y, where age is the running variable. Referring back to Figure 1, coefficient estimates on eligibility in the second stage (Equation 1b) will capture the total effect of eligibility (anticipation) and enrolment (transfers).

Second, we have the front-door analysis:

$$\text{Enrolled}_i = \mu_{3k} + \alpha\text{Eligible}_i + X_i'\delta_3 + \varepsilon_{3i} \tag{2a}$$

$$Y_i = \mu_{4k} + \beta\text{Enrolled}_i + \kappa\text{Eligible}_i + X_i'\delta_4 + \varepsilon_{4i} \tag{2b}$$

where β is the pure transfer effect of interest.

These two sets of estimates can be combined to back-out an estimate for the anticipation effect alone:

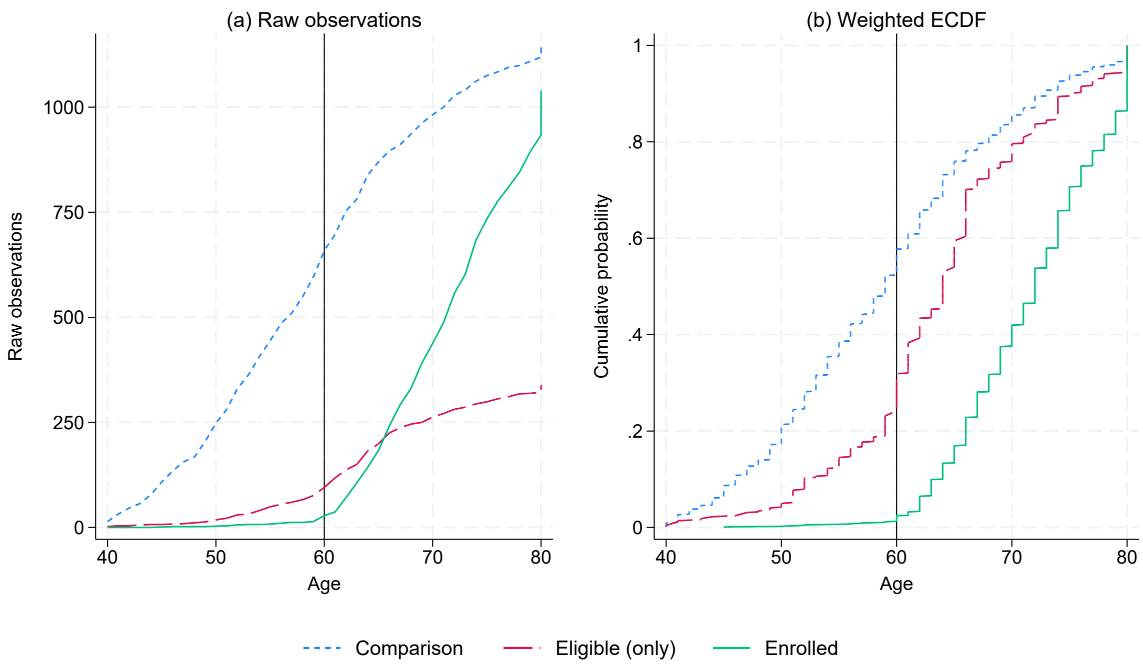
$$\underbrace{\gamma}_{\text{Anticipation}} = \underbrace{\varphi}_{\text{Total}} - \alpha \cdot \underbrace{\beta}_{\text{Transfers}} \tag{3}$$

Two further points merit note. Following the logic of our DAG, estimates of κ in Equation (2b) are expected to be confounded by unobserved factors simultaneously affecting eligibility and outcomes, including the selection of our wait-list sample. This proposition can also be empirically tested from the difference between estimates for γ and those of κ—that is, in the absence of confounding, the two estimates should be indistinguishable. At the same time, the front-door identification assumption, which holds that estimates for β in Equation (2b) should be consistent, cannot be tested directly. Even so, we contend this assumption is plausible. As discussed, institutionalized means testing does not take place and systematic, consistent socioeconomic data on candidates is also lacking. Rather, movement from the wait list to formal enrolment appears to depend primarily on fiscal conditions (i.e., budget allocations), not latent individual characteristics. Thus, conditioning on a rich set of location fixed effects, as well as observed individual characteristics such as age (see below), should mitigate any possible remaining bias.

4.3 Descriptive statistics

Before turning to the regression results, we briefly present some descriptive statistics. First, Figure 2 plots the distribution of individuals by age in our dataset. Plot (a) gives the number of observations per sub-sample and plot (b) gives the weighted empirical cumulative distribution function. As expected, there are almost no individuals enrolled in the PSSB who report to be younger than 60 years, and while some 20% of individuals classified as eligible (wait-listed) are below 60, the majority of these are above 55 years old. In contrast, the (pure) comparison group covers a wide range of ages above and below 60. Thus, based on a visual inspection, the PSSB age threshold appears to be a highly relevant yet imperfect predictor of programme participation.

Figure 2: Age composition of sample, by person type



Note: figures (a) and (b) plot the distribution of ages in the sample, split between the pure comparison group, wait-listed individuals, and those enrolled in the PSSB programme; plot (a) is the cumulative number of observations and (b) is the weighted ECDF.

Source: authors' own estimates from VLS data.

Second, Table 1 summarizes the main variables of interest, also differentiating across the sub-samples. The top part of the table reports subgroup means and standard deviations for our four key outcomes. The first of these is an index of resilience, which, in line with Otchere and Handa (2022), is defined as the first principal component of a range of variables covering asset ownership and access to savings instruments.[12] The logic is that individuals who have access to (multiple) assets are not only likely to be more resilient to shocks, but (in theory) continuous access to social protection can build resilience of this sort.

Table 1: Descriptive statistics (VLS survey)

			Comparison		Eligible		Enrolled		
	Min.	Max.	Mean	SD	Mean	SD	Mean	SD	Prob.
Resilience index	–3.09	4.24	–0.00	(1.29)	0.05	(1.21)	–0.39	(1.29)	0.00
Shock experience	–10.00	5.00	–2.94	(1.83)	–2.98	(2.09)	–2.93	(1.89)	0.99
Shock response	–9.00	7.00	0.26	(1.50)	0.14	(1.89)	0.29	(1.58)	0.22
Shock recovery	–8.00	5.00	–1.85	(1.85)	–2.16	(1.80)	–1.92	(1.93)	0.56
Age	40.00	96.00	59.21	(10.16)	64.20	(8.86)	72.05	(6.80)	0.00
Female	0.00	1.00	0.64	(0.48)	0.66	(0.47)	0.62	(0.49)	0.36
Years of edu.	0.00	15.00	2.68	(3.07)	2.76	(2.87)	1.78	(2.43)	0.00
Catholic	0.00	1.00	0.32	(0.47)	0.23	(0.42)	0.34	(0.47)	0.09
Protestant	0.00	1.00	0.35	(0.48)	0.38	(0.49)	0.32	(0.46)	0.01
Muslim	0.00	1.00	0.11	(0.31)	0.08	(0.27)	0.13	(0.34)	0.00
Parents apart (infancy)	0.00	1.00	0.29	(0.45)	0.24	(0.43)	0.25	(0.43)	0.22
Poverty index (infancy)	0.00	1.00	0.88	(0.20)	0.87	(0.20)	0.93	(0.14)	0.00
Subj. poor (infancy)	0.00	1.00	0.57	(0.50)	0.58	(0.49)	0.64	(0.48)	0.01
Asset deprived (infancy)	0.00	1.00	0.82	(0.38)	0.86	(0.35)	0.90	(0.31)	0.00
War veteran	0.00	1.00	0.02	(0.13)	0.03	(0.17)	0.02	(0.15)	0.59
N	.	.	1147.00	(.)	338.00	(.)	1040.00	(.)	.

Note: the table shows minima, maxima, means, and standard deviations for the main variables. The top half of the table shows key outcomes, the bottom half shows the selected controls. *N* is number of observations. Comparison, eligible, and enrolled are separate sub-samples, where the latter are PSSB beneficiaries; the final column reports the probability from a test of the null hypothesis that sub-sample means are equal, based on a robust regression.

Source: authors' own estimates based on the VLS data.

The remaining variables refer to experiences and responses to shocks over the recent past. The VLS questionnaire included a roster of five different types of shocks (destructive winds, drought, flood, violence, lost or destroyed assets). For each of these we asked whether and when the individual had last experienced such a shock within the last three years, its qualitative impact (ranging from very negative to very positive), the coping strategies they adopted in response, and whether they had now recovered from the event. We combine the experiences of all shocks into an aggregate score, which for each individual shock takes a value of –2 to +2, depending on the direction of impact. As such, the theoretical minimum score is –10, attained if the participant had a very negative experience of all five shocks.

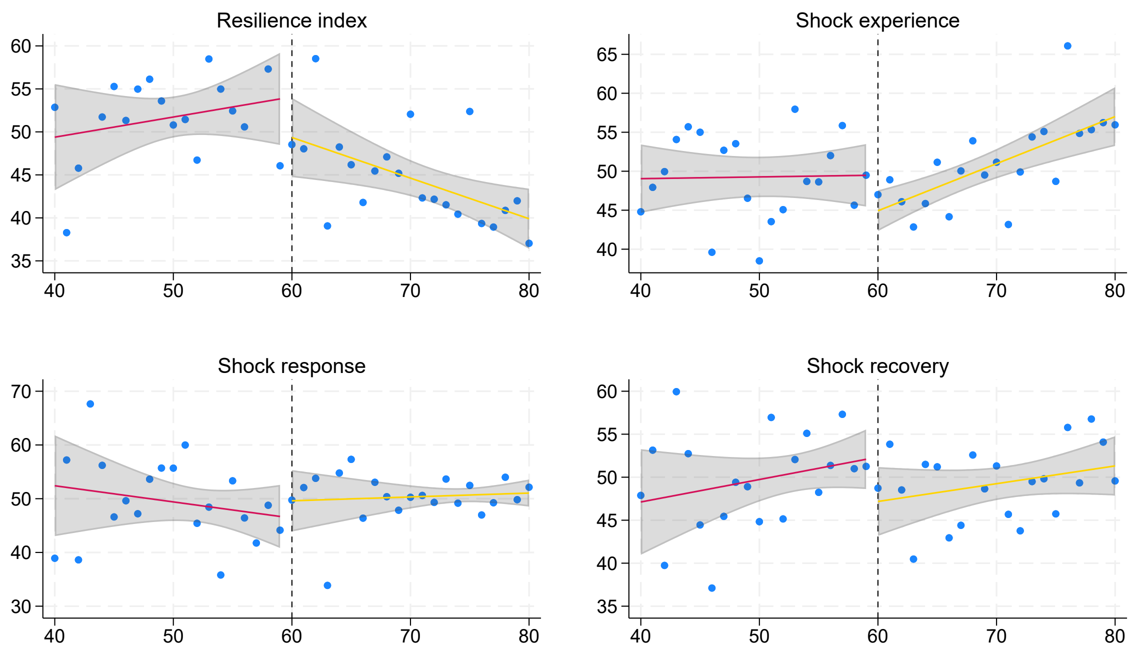
With respect to responses, in keeping with previous literature, we classify coping strategies into so-called positive, neutral, and negative varieties (e.g. Béné et al. 2020; Corbett 1988; Otchere and Handa 2022). We define positive strategies as those that draw on existing (liquid) reserves, such as using savings or finding additional work, thereby helping to maintain current levels of consumption; negative strategies involve either restricting consumption or depleting critical resources that may harm future well-being, such as putting children to work or selling core assets. The sum of these positive minus negative strategies taken across all shocks experienced by the household gives a crude measure of the overall magnitude and direction of household responses. Finally, our metric of recovery is also a summation, taken across all shocks, of households' qualitative assessments of whether they have now recovered or not (scored from –2 to +2, per shock).

[12] Specifically, we split information on ownership of 22 kinds of assets into three classes: livestock, other economic assets, and general assets (e.g. housing). We then calculate the sum of assets in each class and, alongside access to savings, these are entered into the PCA to derive a latent score. We recognize that there are other ways to operationalize resilience, but do not explore these options here.

It is important to highlight that these metrics of shocks—experiences, responses, and recovery—represent subjective as opposed to objective assessments. As such they must be considered endogenous in the sense that they plausibly reflect the inherent capacity of households to withstand shocks in the first place. As discussed by Nguyen and Nguyen (2020), households are only likely to report having experienced a weather shock if they are affected by it (also see Fluhrer and Kraehnert 2022). Thus, more frequent and more negative self-reported experiences of shocks would generally be consistent with lower levels of *ex ante* resilience, a view supported by a moderate positive association between the two measures.

Looking at the outcome means in Table 1, there do not appear to be large systematic differences across the subgroups. This is supported in the final column (prob.), which reports the probability from a joint test that the means are equal across all groups.[13] The exception is the resilience index, which is somewhat higher among the eligible group and lower in the enrolled group relative to the comparison sample. These insights are nuanced by visual inspection of the relationship between each outcome and participant age, shown in Figure 3, which allows for distinct linear trends (akin to structural breaks) above and below the age-eligibility threshold and where all outcomes are now standardized, taking a mean of zero and standard deviation of 1 in the comparison group. With the exception of shock responses, the remaining three variables indicate a break in the expected level of the outcome at the threshold. This is suggestive of material effects associated with the PSSB, as per a reduced-form relationship, which we explore below.

Figure 3: Means of primary outcomes, by age



Note: each figure is a scatter plot of (weighted) means of each outcome by participant age; solid lines are separate linear trends above and below 60 years old, with 95% confidence intervals; all outcomes are transformed to standard centre ridits, identified from the comparison distribution, ranging from 0 to 100.

Source: authors' own estimates based on VLS data.

[13] Based on a regression of each outcome against sample indicators, absorbing location fixed effects and clustering on the referral clusters (see below).

4.4 Results

Table 2 summarizes our headline regression results, showing the IV estimates of each outcome against the binary variable of being PSSB-eligible (takes a value of 1 if on the wait list or enrolled) and where the coefficient on this variable yields our estimate for φ. Columns labelled FD report the front-door results, in which our main focus is the estimate for being enrolled (β). All regressions include a full set of controls, including a quadratic function in age and location fixed effects (by *bairro*), and standard errors are clustered by the referral groups discussed in Section 4.1. In addition, to focus on the age-eligibility threshold, we multiply the sample weights with an age-based quadratic (Epanechnikov) kernel that takes a maximum value at the threshold and declines to zero at the highest age in the sample. Furthermore, to facilitate interpretation and take account of the synthetic nature of all four outcomes, we transform them using the ridit procedure using the distribution of outcomes in the non-eligible (comparison) subgroup as the reference.[14] Broadly, this means regression coefficients reflect (differences in) percentiles of the comparison group distribution.

Table 2: Instrumental variable and front-door regression estimates for main outcomes

	Resilience		Shock experience		Shock response		Shock recovery	
	IV	FD	IV	FD	IV	FD	IV	FD
Eligible	–13.53	4.84**	–13.47	–4.39**	15.24*	1.40	–2.21	0.66
	(9.57)	(2.18)	(8.34)	(1.97)	(8.56)	(1.88)	(8.85)	(1.93)
Enrolled (β)		–8.33***		0.58		–1.68		–1.12
		(2.21)		(1.93)		(1.86)		(1.93)
Age	–0.01	–0.28***	0.38*	0.13**	–0.30	0.11*	0.06	0.01
	(0.26)	(0.08)	(0.22)	(0.07)	(0.23)	(0.06)	(0.23)	(0.06)
Female	–8.58***	–8.66***	–2.35*	–2.36**	3.34***	3.36***	–1.83	–1.85
	(1.43)	(1.42)	(1.20)	(1.19)	(1.19)	(1.16)	(1.20)	(1.20)
N	2,525	2,525	2,525	2,525	2,525	2,525	2,525	2,525
RMSE	26.39	25.87	23.18	22.85	23.69	22.86	23.30	23.29
Alt. estimate (φ^*)	–14.55		–11.14		16.67*		–1.87	
Enrolled ($\gamma+\beta$)		–15.37		–13.34		14.87		–2.46
Eligible (γ)		–7.03		–13.92		16.55		–1.34

Note: the table summarizes regression results for primary outcomes; columns labelled IV are the instrumental variable (fuzzy regression discontinuity) estimates, where programme eligibility is instrumented from the age-eligibility threshold. Columns labelled FD are the front-door estimates, in which programme enrolment is the key estimate of interest. All regressions include a standard set of controls, plus location and enumerator fixed effects. Standard errors are clustered at the level of beneficiary-referral groups. The final rows report derived estimates, including the anticipation effect (γ), the total effect for beneficiaries ($\gamma+\beta$), and an alternative estimate for the same effect based on a sub-sample IV regression (φ^*).

Source: authors' own estimates based on VLS data.

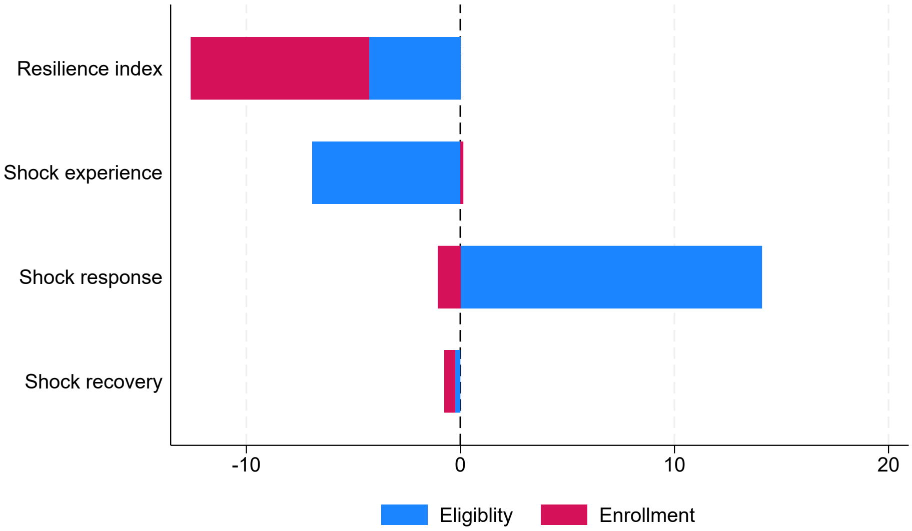
The first stage of the IV estimates (not shown) are material, strongly passing tests for weak and under-identification, thereby confirming the relevance of the age-eligibility instrument. The second-stage estimates, denoted by columns labelled 'IV' in the table, indicate no strong relationships between outcomes and eligibility, in part due to relatively large standard errors. However, the direction of effects is negative for resilience and shock experiences, but positive and borderline significant for shock responses. Pure enrolment effects from the front-door estimates, however, are negative and significant for both shock resilience and shock responses. These suggest that relative to other households, those enrolled in the programme are less resilient (by about 6 ridits) and adopt fewer positive coping strategies in response to shocks (although this effect is not different from zero). Thus, actual monetary transfers provided under the PSSB do not appear to support households' response to shocks.

Before digging further into these results, the last three rows of the footer of Table 2 provide complementary estimates. The first is an alternative estimate for φ, derived from running the same IV regressions

[14] See Brockett and Levine (1977) for discussion; implemented in Stata using the `wridit` command.

but now excluding all wait-listed individuals, equivalent to fixing $\alpha = 1$. These estimates are very close to our derived aggregate estimates for being enrolled, as reported in the penultimate line of the table, which are also plotted in Figure 4.[15] The final line in the footer is our estimate for the anticipation effect, where we approximate the standard error using a similar strategy—namely, we rerun the IV regressions but exclude all enrolled individuals (fixing $\alpha = 0$). These results confirm that becoming eligible for the PSSB is significantly associated with more positive shock responses (effect size of 15 ridits), an effect that operates entirely via the anticipation channel.

Figure 4: Overall effect of PSSB-Elderly on main outcomes



Note: the figure summarizes the findings from Table 2, showing derived estimates for γ (eligibility effect) and β (enrolment effect); point estimates are shrunk by the probability they are non-zero.

Source: authors' own estimates based on VLS data.

Two key questions emerge from the preceding analysis. First, what might account for what appear to be large (albeit imprecise) anticipation effects? To understand these, we replicate the previous analysis for a range of secondary or proximate outcomes. As shown in Table 3, anticipation effects appear wide-ranging. In particular, becoming PSSB-eligible is associated with an increase in household size, a higher propensity to have access to non-agricultural cash income, and a higher measure of social capital, defined as an index of multiple underlying variables (for further details and analysis, see Almeida et al. 2024a,b). In keeping with a small literature elsewhere (e.g. Edmonds et al. 2005), this suggests that becoming eligible for the PSSB induces changes to household living arrangements, as well as livelihood strategies and social status, where the latter two domains reflect both changes in household structure and that future transfers may provide a form of collateral. Thus, even if the PSSB is not entirely reliable (see below), in situations of extreme and generalized poverty, as found in many locations in Mozambique including our survey sites, the mere anticipation of a future new cash income stream can have far-reaching effects.

A possible critique of these findings is that they may be influenced by the survey design, particularly our reliance on what is essentially a convenience sample of individuals on the wait list. While we recognize this is a limitation, we nonetheless highlight that our key estimates (namely, the IV estimates in Tables 2 and 3) rely on the exogenous age threshold for identification. Also, to validate robustness, we rerun the same analysis, replacing the observed (possibly biased) indicator of being on a wait list with simulated proxies. Namely, we interact being over 60 years old with a range of indicators of prior circumstances

[15] To assist visualization, Figure 4 plots shrunken coefficient estimates, given by the product of the point estimates and an estimate of the probability it is different from zero—for example, $\tilde{\beta} = \hat{\beta} \cdot (1 - \Pr(\beta = 0))$.

that are plausibly correlated with current vulnerability, such as having no education and growing up in poverty. Using the row-wise average of these proxies as our new indicator of eligibility, which also takes a value of 1 for all PSSB participants, Appendix Tables A1 and A2 and Figure A1 report the results. As can be observed, all our main findings are sustained, implying anticipation effects are indeed relevant across multiple outcomes.

Table 3: Instrumental variable and front-door regression estimates for secondary outcomes

	Household size		Non-ag. income		Transfer income		Social capital	
	IV	FD	IV	FD	IV	FD	IV	FD
Eligible	1.66*	0.21	0.38**	0.11***	0.11	0.08**	14.80	4.13**
	(1.01)	(0.24)	(0.17)	(0.04)	(0.15)	(0.03)	(9.44)	(1.93)
Enrolled (β)		–0.03		–0.13***		–0.01		1.66
		(0.26)		(0.04)		(0.03)		(1.93)
Alt. estimate (φ^{*})	1.39		0.30		0.14		11.29	
Enrolled ($\gamma+\beta$)		1.66		0.35		0.11		15.17
Eligible (γ)		1.68		0.48**		0.12		13.51

Note: the table summarizes the regression results for secondary outcomes. Columns labelled IV are the instrumental variable (fuzzy regression discontinuity) estimates, where programme eligibility is instrumented from the age-eligibility threshold; columns labelled FD are the front-door estimates, in which programme enrolment is the key estimate of interest. All regressions include a standard set of controls, plus location and enumerator fixed effects. Standard errors are clustered at the level of beneficiary-referral groups. The final rows report derived estimates, including the anticipation effect (γ), the total effect for beneficiaries ($\gamma+\beta$), and an alternative estimate for the same effect based on a sub-sample IV regression (φ^{*}).

Source: authors' own estimates based on VLS data.

A second question is what might explain our finding that enrolled PSSB beneficiaries fare *worse* than non-enrolled counterparts, including the eligible? One explanation is that since PSSB beneficiaries are on average older than members of the other subgroups, this finding may reflect omitted age-related interactions. To examine this, we extend our estimates by adding the interaction between enrolment and age. As shown in columns (a) of Table 4, which focuses on extensions of the front-door estimates only, this explanation does not seem material—none of the interaction terms is significant and the core results are unaffected.

Table 4: Extended front-door regression estimates for main outcomes

	Resilience		Shock experience		Shock response		Shock recovery	
	Ia	Ib	IIa	IIb	IIIa	IIIb	IVa	IVb
Eligible	4.79*	4.96*	–5.84**	–6.05***	2.29	2.42	1.19	1.12
	(2.60)	(2.61)	(2.31)	(2.33)	(2.18)	(2.21)	(2.35)	(2.37)
Enrolled (β)	–7.89***	–4.67	2.10	–1.20	–3.13	–0.62	–1.44	–2.45
	(2.68)	(4.56)	(2.26)	(3.66)	(2.30)	(3.48)	(2.36)	(3.74)
Enrolled $\times$ age	0.14	0.12	0.17	0.14	–0.29	–0.33	0.02	0.01
	(0.24)	(0.24)	(0.20)	(0.20)	(0.23)	(0.23)	(0.20)	(0.20)
Enrolled $\times$ gap		–3.98		1.34		–4.03		0.54
		(3.44)		(2.71)		(2.48)		(2.90)
Enrolled $\times$ years		0.19		0.41**		0.35*		0.10
		(0.21)		(0.19)		(0.18)		(0.18)
N	2,525	2,525	2,525	2,525	2,525	2,525	2,525	2,525
RMSE	25.88	25.88	22.85	22.84	22.86	22.85	23.30	23.31

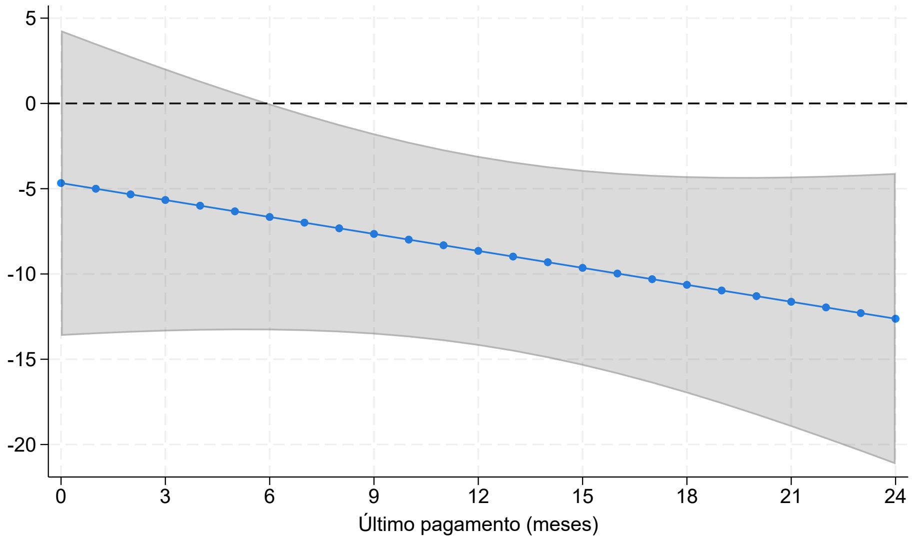
Note: the table summarizes the front-door regression results for the main outcomes, adding interaction terms between programme enrolment, namely: participant age, gap since last payment, and years enrolled in PSSB. All regressions include a standard set of controls, plus location and enumerator fixed effects. Standard errors are clustered at the level of beneficiary-referral groups.

Source: authors' own estimates based on VLS data.

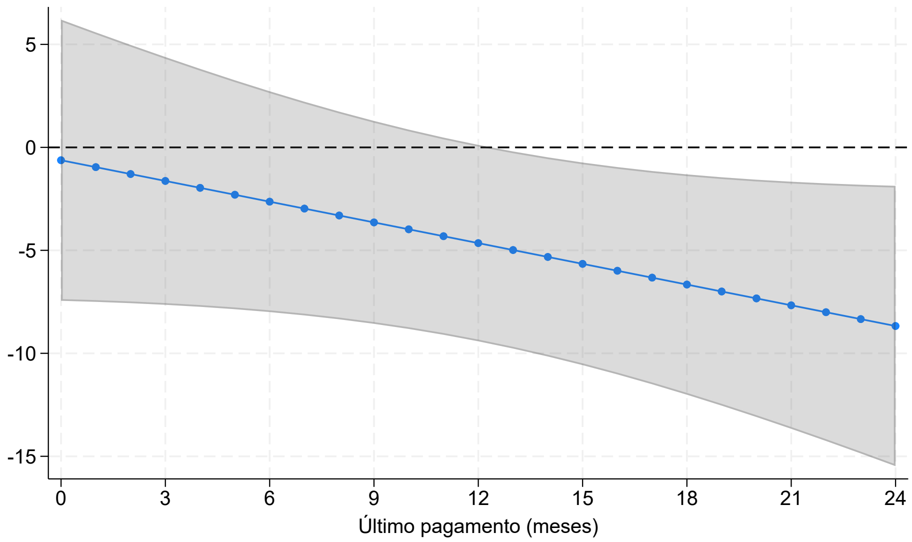
Another explanation is that our survey was undertaken at a time when PSSB payments had been severely delayed. According to data from the INAS digital registry (see below), which we matched to our survey participants, the most recent transfer payment across all our sample areas occurred in September 2023, approximately eight months prior to the start of the survey, and nearly one-third of beneficiaries had received their most recent payment only in 2022. Since these dates do not fully align with recall-based information collected from the participants, we define the payment gap as the time in years since the last payment, using the median of the two sources. Interacting this variable with PSSB enrolment, the coefficients in columns (b) of Table 4 capture how outcomes vary as the time since the last payment increases. The key insight, albeit imprecise, is that delays are highly important. Illustrated in Figure 5(a), which plots the coefficient results directly, 12-month payment delay is associated with an 8.64 decline in resilience [95% CI: –14.20, –3.08]; when there is no delay, now captured by the coefficient on enrolled alone, there is no significant difference associated with being a beneficiary. A similar finding pertains to shock responses, which decline as the horizon since the last payment declines, becoming significantly different from zero at around one year [95% CI: –9.41, 0.13]; see also Figure 5(b).

Figure 5: Estimated effect of PSSB payments (β) on outcomes at different horizons, based on front-door regression estimates

(a) Resilience



(b) Shock response



Note: the figure summarizes the findings from columns Ib and IIIb of Table 4, showing the expected effect of receiving a PSSB transfer (enrolment effect) at different periods of time as defined by the gap since last payment. The shaded grey area is the 95% confidence interval.

Source: authors' own estimates based on VLS data.

Another relevant source of heterogeneity is the length of time the individual has been a beneficiary (also in years). The results in columns (b) of Table 4 include this additional interaction term, pointing to small yet significant positive effects of long-run participation on both shock experiences and responses (controlling for age and relevant interactions). For instance, the expected effect of the programme on shock experiences among individuals enrolled for at least five years is no longer statistically different from zero (as opposed to negative); and participation over the much longer run pushes the estimated effect into positive territory. Taken together, these findings indicate the PSSB scheme has some potential to support the elderly response to shocks, especially among long-run beneficiaries. Yet, realization of this potential currently is undermined by operational challenges, including major payment delays.

This conclusion is supported by a specific analysis of the experience of Cyclone Gombe, a Category 3-equivalent tropical cyclone that hit Nampula Province in March 2022, just six weeks after another major storm (Tropical Storm Ana).[16] As previously noted, the two VLS communities in Nampula were selected to participate in the survey due to their exposure to the cyclone in 2022; but they were also chosen because PSSB payments by chance were made within a few weeks of the same event—one community received payments just before the cyclone hit, and the other community a few weeks after. Restricting our sample to these Nampula communities, Table 5 reports front-door regression results for the three shock measures now split across different years, based on recall. We find no relationship between PSSB enrolment and outcomes in either 2021 or 2023; but in 2022 the sign of responses is positive and significantly different from zero among PSSB beneficiaries (by 11 ridits). This provides support to the view that timely social protection payments can indeed support adoption of more positive (or less negative) coping strategies. However, in this data, there seems to be no material difference in outcomes between those who received the PSSB payment before versus after the shock event, as per the additional included interaction term.

Table 5: Front-door regression estimates for Nampula, by year

	Score			Response			Recovery		
	2021	2022	2023	2021	2022	2023	2021	2022	2023
Eligible	–1.06	1.86	–1.83	–1.75	–12.22**	–0.26	–0.05	4.18	–3.74
	(3.36)	(3.96)	(3.73)	(3.42)	(5.23)	(4.81)	(3.43)	(6.65)	(3.93)
Enrolled (β)	2.35	–2.80	–1.18	0.56	10.72**	–0.61	0.77	–0.35	2.32
	(3.38)	(3.98)	(3.86)	(3.51)	(5.22)	(4.49)	(3.61)	(6.85)	(4.24)
Enrolled × paid before	1.51	–2.63	–0.43	0.31	–0.48	–0.19	1.67	–6.18*	2.36
	(1.94)	(2.52)	(2.79)	(1.92)	(3.23)	(2.16)	(2.20)	(3.45)	(2.77)
N	841	841	841	841	841	841	841	841	841
RMSE	14.53	19.28	19.91	14.61	24.61	16.17	16.25	25.33	19.69

Note: the table summarizes front-door regression results for three main outcomes, focusing on the Nampula sub-sample and distinguishing between shocks occurring in different years. Interaction between programme enrolment and receiving a payment prior to Cyclone Gombe is also added. All regressions include a standard set of controls, plus location and enumerator fixed effects. Standard errors are clustered at the level of beneficiary-referral groups.

Source: authors' own estimates based on VLS data.

5 Household budget surveys

The previous section found no clear evidence that Mozambique's social pension scheme effectively supports responses to shocks. However, we did find that sustained and reliable payments matter. Thus, recent delays in transfers appear to have contributed to weakened resilience among elderly beneficiaries. In this section we delve further into the issue of payment timing and reliability, now drawing on nation-

[16] For further details, see https://en.wikipedia.org/wiki/Cyclone_Gombe.

ally representative household survey data collected in 2020 and 2022, when payments were undertaken on a more regular basis.

5.1 Data and methods

Our first source of data for this analysis is INAS's digital registry of beneficiaries and transfers. Started in 2019, the registry records basic details of each beneficiary (e.g. name, age, gender, household size), as well as dates and values of each transfer made. Brief examination of this data indicates that the vast majority of all beneficiaries in any given location (e.g. *posto administrativo*) are paid on or around the same dates. This reflects the current cash-based payment modality, whereby payments are made manually by each INAS delegation during visits to each community coordinated via the *permanentes*. However, these visits are not especially predictable. In 2022, the median beneficiary received four separate payments, with an average of 86.5 days and standard deviation of 27 days between each payment.

Aggregated to the level of administrative posts, we match this payment data to the two most recent rounds of the country's household budget survey, undertaken in 2019/2020 and 2022.[17] These contain detailed information on multiple facets of each household, including consumption, multi-dimensional deprivation, experiences and responses to shocks, and receipt of social protection transfers. Similar to the previous analysis, we are able to identify (self-reported) PSSB beneficiaries in the data and, for each of them, we estimate the gap between the date of the survey interview and the most recent payment they are likely to have received, as per the eINAS registry. As such, our primary interest is a counterpart to Equation (1b), given by the following relationship:

$$Y_i = \mu_k + \rho \text{PSSB}_i + \tau(\text{PSSB}_i \times \text{Gap}_i) + X_i'\delta + \varepsilon_i \tag{4}$$

As before, receipt of the subsidy (PSSB) is expected to be endogenous. Thus, echoing the previous RD estimation procedure, we use the PSSB age threshold (60+) and its interaction with payment timing as external instruments for the two endogenous terms. Following previous logic, estimates for ρ are thus expected to capture the combined effect of being eligible and receiving transfers (e.g. $\gamma + \alpha\beta$); however, estimates for τ should *only* reflect variation in the pure transfer effect. Throughout, we assume variation in payment timing within each location is random, with differences primarily reflecting operational constraints. Furthermore, differences in macro-fiscal conditions that may be correlated with payment timing and outcomes are absorbed by a complete set of location and period fixed effects, namely district-by-year and survey quarter effects.

5.2 Descriptive statistics

Table 6 summarizes descriptive statistics from the matched eINAS and household survey database. Here we focus on the sample of households, which is the level at which most variables are observed. Individual variables correspond to the household head. The top part of the table summarizes the main outcomes of interest, which cover information on per capita consumption, as well as self-reported dimensions of shocks. The former are of particular interest since they represent metrics of overall household well-being, where higher values naturally reflect greater resilience and adaptive capacity. Total and food consumption are reported here as nominal daily values per capita (in MZN). Both variables underline the precarious situation faced by most households in Mozambique—more than half of all households are classified as consumption-poor and the average value of food consumed is less than US\$0.5 per person per day (during this period the US\$:MZN exchange rate was about 1:63).

[17] For further details on the surveys, see Arndt et al. (2016) and Salvucci and Tarp (2024).

Table 6: Descriptive statistics (household survey)

	Min.	Max.	Comparison Mean	Comparison SD	PSSB Mean	PSSB SD	Prob.
Nominal cons. (total)	0.77	948.99	66.11	(98.03)	49.76	(75.80)	0.22
Nominal food cons.	0.04	185.00	24.32	(21.18)	22.68	(18.18)	0.98
Any shock	0.00	1.00	0.36	(0.48)	0.54	(0.50)	0.02
Number of shocks	0.00	3.00	0.57	(0.83)	0.75	(0.78)	0.05
Shock response	–5.00	3.00	–0.14	(0.57)	–0.32	(0.63)	0.01
Shock recovery	–1.00	1.00	–0.23	(0.61)	–0.29	(0.68)	0.95
Age	12.00	100.00	42.61	(14.18)	68.58	(11.27)	0.00
Female-headed hhld.	0.00	1.00	0.24	(0.43)	0.43	(0.50)	0.00
Highest edu. (years)	0.00	19.00	7.06	(4.23)	5.57	(3.89)	0.00
Dependency share	0.00	1.00	0.51	(0.20)	0.64	(0.25)	0.00
Cyclone path	0.00	1.00	0.53	(0.50)	0.53	(0.50)	0.54
Last cyclone (years)	0.01	3.96	1.02	(0.76)	1.15	(0.75)	0.47
PASP beneficiary	0.00	1.00	0.00	(0.05)	0.01	(0.09)	0.30
N	.	.	24,719.00	(.)	580.00	(.)	.

Note: the table shows minima, maxima, means, and standard deviations for the main variables based on 2019/20 and 2022 household budget surveys (pooled). The top half of the table shows key outcomes, the bottom half shows selected controls. *N* is number of observations. Comparison and PSSB are separate sub-samples, where the latter are self-identified PSSB beneficiaries. The final column reports the probability from a test of the null hypothesis that sub-sample means are equal, based on a robust regression. PASP is a separate social protection programme run by INAS, known as the *Programa de Acção Social Produtiva* (Productive Social Action Program).

Source: authors' own estimates based on IOF data.

Similar to the VLS, shock-related outcomes are based on a dedicated survey module asking the household about different kinds of shocks and their responses. We include a binary indicator to capture whether the household had ever suffered a shock (*calamidade natural*), as well as the number of shocks in the past 12 months that had created losses for the household, based on a list of 11 different shock types (excluding COVID-19). We code shock responses into positive and negative types, calculating an aggregate score. We allocate a score of –1 if the household reports to be in a worse situation now compared to a 'normal' year and +1 if the household self-reports to be in a better situation (and 0 otherwise). It merits note that shocks are far from uncommon—over one-third of households report to have suffered a natural disaster and about 40% report experiencing at least one specific shock in the past year. Average responses to shocks are negative and households have not completely recovered from them.

A selection of control variables are reported in the second part of the table. These include information on the household head's age and gender, the highest level of education in the household, and the share of members classified as dependents (those under 15 or over 65 years old). In addition, we complement the self-reported metrics of shocks with objective measures of recent experiences of cyclones. Specifically, we take geospatial information on the dates and paths of major tropical storms that made landfall in Mozambique over the period 2019–22. From these, we identify households in the survey that had been exposed to these storms, and the period of time (in months) since the last such exposure. Reiterating the shock-prone nature of the country, we observe that over half of all Mozambican households experienced a major storm over the period covering the two surveys.

5.3 Results

Tables 7–9 report our main findings. For each outcome we report four separate estimates based on Equation (4). Ordered by columns these are: (a) a simple OLS regression; (b) an IV regression, dealing with the endogeneity of being a PSSB recipient; (c) the same IV regression applying age-based kernel weights in addition to sample weights; and (d) an extended version of (c) where we add interactions with objective measures of recent cyclone experiences. A fixed set of control variables is used throughout, alongside an extensive set of location and period fixed effects, together absorbing 450 degrees of freedom (not reported).[18] Standard errors are clustered by enumeration area, which number 952 in our dataset. In the IV regressions (columns b–d) we include the (log.) number of PSSB beneficiaries in each location as an additional external instrument, capturing geographic prioritization and thereby differences in the propensity to become a beneficiary. In turn, this allows us to test for instrument validity via the Hansen-J statistics (shown in the tables).[19]

Table 7: Household budget survey regression results for consumption outcomes

	Total consumption (real, log.)				Food consumption (real, log.)			
	Ia	Ib	Ic	Id	IIa	IIb	IIc	IId
PSSB	0.15^{**}	3.66^{***}	3.83^{***}	3.93^{***}	0.18^{**}	3.94^{***}	4.30^{***}	4.20^{***}
	(0.06)	(0.80)	(0.76)	(0.85)	(0.06)	(0.85)	(0.89)	(0.98)
PSSB × gap (years)	–0.04	-5.84^{**}	-4.46^{*}	-4.33^{*}	0.02	-5.94^{*}	–4.62	–4.44
	(0.36)	(2.93)	(2.49)	(2.53)	(0.41)	(3.13)	(2.81)	(2.85)
PSSB × cyclone				0.59				1.78
				(0.93)				(1.19)
PSSB × last cyclone				–0.61				-1.15^{**}
				(0.45)				(0.58)
Age	0.01	-0.05^{***}	-0.05^{***}	-0.05^{***}	0.00	-0.06^{***}	-0.07^{***}	-0.07^{***}
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Female-headed hhld.	0.02	0.03	–0.00	–0.00	-0.04^{*}	–0.03	-0.07^{**}	-0.07^{***}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Cyclone path	–0.16	–0.18	–0.14	–0.16	–0.08	–0.09	–0.10	–0.14
	(0.15)	(0.15)	(0.14)	(0.14)	(0.18)	(0.18)	(0.17)	(0.17)
Cyclones (number)	0.09	0.07	0.06	0.05	–0.01	–0.03	–0.02	–0.02
	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.06)	(0.07)
Last cyclone (years)	0.05	0.03	0.01	0.02	0.04	0.02	–0.01	0.03
	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)	(0.07)
N	25,299	25,283	24,686	24,686	25,299	25,283	24,686	24,686
Hansen-J (prob.)		0.52	0.96	0.99		0.70	0.23	0.19

Note: the table summarizes regression results for consumption outcomes. Columns (a) are OLS regressions; all remaining columns apply IV (fuzzy regression discontinuity) estimates, where programme eligibility is instrumented using the PSSB age-eligibility threshold. Columns (a) and (b) use standard sample weights; columns (c) and (d) apply kernel weights, focusing on observations close to the age-eligibility threshold (60 years). All regressions include a standard set of controls, including location and survey period fixed effects. Standard errors are clustered at the level of (geographic) primary sampling units.

Source: authors' own estimates based on IOF data.

Table 7 focuses on estimates of total and food consumption, which are converted into real values by dividing through by the poverty line and are then transformed into natural logarithms. Supporting our earlier analysis based on the VLS, we find large and significant immediate effects of receiving the PSSB, but these benefits dissipate over time. Based on the kernel-weighted IV estimates in column 1c, per capita total consumption jumps by a multiple of around four on receipt of the PSSB [95% CI: 2.69, 5.55] but is expected to fall back to its baseline (non-beneficiary) level within less than a year if there are no further payments. Similar results obtain for food consumption, suggesting that past consumption (welfare) benefits enjoyed by beneficiaries on account of the subsidy will now have fully dissipated given the major delays in payments since 2023. And while these effect sizes appear large, we reiterate that PSSB benefits are typically paid in instalments of many months (e.g. three or four months), and consumption is measured as a daily value. Thus, it is reasonable to suppose that comparatively large lump sum transfers induce large short-run increments in consumption, especially where these expenses often include bulk food items.

[18] Controls used include age, gender, religion (dummies), marital status, education, and family structure (see Table 6). Note, as before, household size is not employed as a control since it may be affected by transfer receipt.

[19] Exclusion of this additional instrument does not affect our results.

The additional interaction terms in columns (d) investigate whether the effects of receiving the PSSB are further modified by (objective) weather events. The first of these (PSSB × cyclone) is the binary PSSB treatment indicator multiplied by a dummy variable for having been exposed to a cyclone, which in the IV estimates is instrumented by the interaction between the age threshold and the same exposure measure. The second (PSSB × last cyclone) interacts the treatment indicator with the length of time (in years) since the most recent cyclonic event, if any. Thus, when the former exposure variable takes a value of 1 and the latter variable is close to 0, the coefficient on the first interaction term gives the additional impact of receiving the PSSB around the time of a cyclone; and the coefficient on the second interaction indicates the extent to which any such impacts vary over time. While these results are not conclusive, again due to large standard errors, they nonetheless point to a moderate and statistically significant extra positive effect of the PSSB on (food) consumption when transfers occur near in time to a cyclonic event. However, as with the gaps between PSSB payments, this positive effect also dissipates over time.

The remaining tables consider the self-assessed shock outcomes, all of which except the first dummy variable are transformed using the same ridit procedure used previously. While we find no effects associated with the PSSB on responses or recovery from shocks, Table 8 indicates that beneficiaries who have recently received a PSSB transfer are significantly less likely to report having experienced a shock. In the case of the aggregate experience score (based on the number of shocks experienced), recent PSSB recipients are located 29.23 ridits lower than comparable non-beneficiaries [95% CI: –63.3, 4.87]; and, while imprecise, this effect also dissipates with the time since the last transfer.

Table 8: Household survey regression results for self-reported shock experiences

	Natural calamity (dummy)				Shock experience score			
	Ia	Ib	Ic	Id	IIa	IIb	IIc	IId
PSSB	0.08*	–0.98**	–0.92**	–0.93**	1.88	–34.33	–34.36*	–36.65*
	(0.05)	(0.45)	(0.43)	(0.46)	(2.33)	(21.11)	(19.56)	(20.81)
PSSB × gap (years)	–0.43	2.01	1.86*	1.80*	–10.10	70.98	65.28	61.46
	(0.30)	(1.25)	(1.05)	(1.05)	(17.16)	(65.10)	(55.17)	(56.02)
PSSB × cyclone				–0.31				–19.27
				(0.36)				(19.68)
PSSB × last cyclone				0.27				18.52*
				(0.19)				(10.18)
Age	0.01***	0.03***	0.03***	0.03***	0.78***	1.30***	1.35***	1.39***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.16)	(0.36)	(0.38)	(0.39)
Female-headed hhld.	0.01	0.01	0.01	0.01	0.47	0.46	0.95*	0.99*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.61)	(0.61)	(0.52)	(0.52)
Cyclone path	0.04	0.04	0.07	0.08	2.25	2.47	–0.03	0.41
	(0.09)	(0.09)	(0.09)	(0.09)	(4.95)	(4.95)	(4.78)	(4.83)
Cyclones (number)	–0.05	–0.04	–0.06	–0.05	3.23	3.29	3.61	3.69
	(0.04)	(0.05)	(0.04)	(0.04)	(2.40)	(2.39)	(2.27)	(2.26)
Last cyclone (years)	0.02	0.02	0.01	0.00	–0.31	–0.17	–0.23	–0.80
	(0.03)	(0.03)	(0.03)	(0.03)	(1.97)	(1.96)	(1.85)	(1.91)
N	25,299	25,283	24,686	24,686	25,299	25,283	24,686	24,686
Hansen-J (prob.)		0.02	0.04	0.03		0.72	0.78	0.75

Note: the table replicates the results in Table 7 for self-reported experiences of shocks.

Source: authors' own estimates based on IOF data.

Table 9: Household survey regression results for shock response and recovery

	Shock response				Shock recovery			
	Ia	Ib	Ic	Id	IIa	IIb	IIc	IId
PSSB	–1.46	–16.33	–9.09	–9.96	–3.18	–30.56	–21.17	–16.13
	(2.04)	(19.86)	(21.49)	(22.19)	(2.06)	(21.19)	(21.06)	(22.65)
PSSB $\times$ gap (years)	–8.47	33.20	13.28	15.11	42.51**	112.07*	67.29	67.84
	(11.79)	(53.21)	(45.57)	(45.55)	(15.08)	(64.05)	(58.99)	(58.83)
PSSB $\times$ cyclone				–12.65				–18.85
				(19.62)				(21.47)
PSSB $\times$ last cyclone				6.62				4.57
				(9.89)				(10.41)
Age	–0.13	0.06	–0.04	0.04	–0.41**	–0.04	–0.12	–0.06
	(0.13)	(0.34)	(0.42)	(0.45)	(0.16)	(0.35)	(0.38)	(0.39)
Female-headed hhld.	0.31	0.27	0.16	0.18	–1.20*	–1.25*	–0.88	–0.86
	(0.61)	(0.62)	(0.48)	(0.49)	(0.67)	(0.67)	(0.54)	(0.54)
Cyclone path	1.23	1.07	0.55	0.87	–0.78	–1.14	0.41	0.83
	(3.16)	(3.16)	(2.96)	(3.01)	(4.32)	(4.32)	(4.32)	(4.37)
Cyclones (number)	1.42	1.61	1.83	1.85	–3.77*	–3.39	–3.69*	–3.67*
	(1.82)	(1.84)	(1.65)	(1.67)	(2.15)	(2.17)	(2.16)	(2.18)
Last cyclone (years)	–1.32	–1.23	–1.17	–1.34	1.02	1.24	0.83	0.74
	(1.06)	(1.06)	(1.02)	(1.04)	(1.22)	(1.23)	(1.20)	(1.24)
N	25,299	25,283	24,686	24,686	25,299	25,283	24,686	24,686
Hansen-J (prob.)		0.00	0.00	0.00		0.37	0.43	0.44

Note: the table replicates the results in Table 7 for self-reported responses to and recovery from shocks.

Source: authors' own estimates based on IOF data.

6 Conclusion

This paper examined the contribution of Mozambique's social pension, the elderly component of the PSSB, to enhancing household resilience against shocks. As a point of departure we noted that the programme, which encompasses over 450,000 registered beneficiaries, has been facing long-running operational challenges, including severe payment delays since at least 2023.

Our analysis used data from a bespoke survey of beneficiaries and comparison households. A fuzzy regression discontinuity design, exploiting the programme's age-based eligibility threshold, allowed for causal identification of both transfer and anticipation effects, where the latter is relevant due to the prevalence of programme waiting lists. Payment timing and event-specific shocks, such as Cyclone Gombe, were investigated to examine the temporal dynamics of programme impacts. In addition, we combined administrative data on payment timings with nationally representative household data. Together, these data sources offer a comprehensive understanding of the PSSB's effectiveness at scale and provide one of the first analyses of how enrolment and payment delays impact beneficiary outcomes.

Our main findings speak to the unfulfilled potential of the PSSB. We found that while programme eligibility positively influences coping strategies through anticipation effects, which seem to be driven by changes in household composition and social networks, material benefits associated with PSSB transfers appear limited. Indeed, from our bespoke survey undertaken in 2024, we found that the resilience of beneficiaries, proxied through asset ownership, is now lower than among comparison groups, reflecting the absence of payments for at least eight months. Using household survey data, however, we found that consumption benefits from PSSB transfers are large and significant, but relatively short-lived, dissipating within six months. We also found evidence that timely payments—made during periods of acute need, such as immediately before or after a cyclone—can improve household responses to shocks.

These results underscore the importance of strengthening the operational framework of social protection programmes such as the PSSB to realize their full potential. Reliable and predictable disbursements, streamlined administrative processes, and enhanced fiscal and institutional capacity are necessary to ensure these programmes can act as effective instruments for resilience-building. Future research should explore how reliable and efficient social protection schemes can be deployed at scale in low-income countries, thereby contributing to broader efforts to design programmes that effectively shield vulnerable populations from shocks.

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Appendix A: Additional material

Table A1: Instrumental variable and front-door regression estimates for main outcomes using proxy for wait-list status

	Resilience		Shock experience		Shock response		Shock recovery	
	IV	FD	IV	FD	IV	FD	IV	FD
Eligible	–6.23	0.07	–6.12	–0.54	7.13*	1.26	–1.13	–4.05
	(4.44)	(3.10)	(3.90)	(2.74)	(3.97)	(2.73)	(4.17)	(2.76)
Enrolled (β)		–4.36**		–2.76		–1.21		1.67
		(2.17)		(1.83)		(1.88)		(1.88)
Age	–0.16	–0.26***	0.24*	0.12	–0.14	0.09	0.04	0.09
	(0.16)	(0.10)	(0.14)	(0.08)	(0.14)	(0.08)	(0.15)	(0.08)
Female	–8.71***	–8.63***	–2.47**	–2.40**	3.48***	3.39***	–1.86	–1.89
	(1.43)	(1.42)	(1.19)	(1.19)	(1.16)	(1.15)	(1.20)	(1.20)
N	2,525	2,525	2,525	2,525	2,525	2,525	2,525	2,525
RMSE	25.92	25.89	22.89	22.87	22.95	22.86	23.28	23.28
Alt. estimate (φ^{*})	–14.55		–11.14		16.67*		–1.87	
Enrolled ($\gamma+\beta$)		–7.19		–6.73		6.86		–0.76
Eligible (γ)		–2.84		–3.98		8.07		–2.43

Note: the table replicates Table 2, using a proxy for programme eligibility based on the combination of age (being 60 years or above) and prior correlates of vulnerability (e.g. having no education).

Source: authors' own estimates based on VLS data.

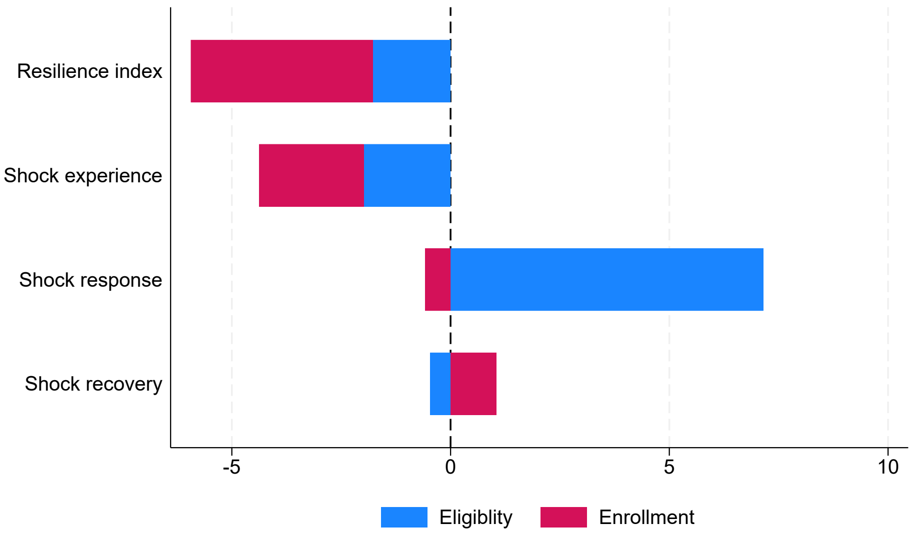
Table A2: Instrumental variable and front-door regression estimates for secondary outcomes using proxy for wait-list status

	Household size		Non-ag. income		Transfer income		Social capital	
	IV	FD	IV	FD	IV	FD	IV	FD
Eligible	0.78*	0.59*	0.18**	0.11**	0.05	-0.03	6.77	0.24
	(0.47)	(0.35)	(0.08)	(0.05)	(0.07)	(0.05)	(4.37)	(2.94)
Enrolled (β)		-0.18		-0.10***		0.08**		4.96**
		(0.26)		(0.04)		(0.03)		(2.07)
Alt. estimate (φ^{*})	1.39		0.30		0.14		11.29	
Enrolled ($\gamma+\beta$)		0.74		0.16		0.07		7.86
Eligible (γ)		0.92		0.26***		-0.01		2.90*

Note: the table replicates Table 4 using a proxy for programme eligibility based on the combination of age (being 60 years or above) and prior correlates of vulnerability (e.g. having no education).

Source: authors' own estimates based on VLS data.

Figure A1: Overall effect of PSSB-Elderly on main outcomes using proxy for wait-list status



Notes: the figure replicates Figure 3 using the results from Table A1.

Source: authors’ own estimates based on VLS data.