Who is Left Behind? The Take-up of Digital Programs: Evidence from Brazil

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Abstract

We study whether low-cost digital informational interventions can increase the take-up of fully online public programs. We embed a nationwide randomized controlled trial into the rollout of *Desenrola Brasil*, a digital debt relief initiative targeting indebted households in Brazil. Over 2.3 million eligible individuals were randomly assigned to receive email messages with information about how to access the program online, how to seek in-person assistance at the post office, or both. On average, the intervention had no detectable effects on platform access, debt renegotiation, or repayment. This null result is driven by extremely low exposure: only 3% of recipients opened the messages. Email opening was selective, with more vulnerable and less digitally connected individuals substantially less likely to engage. Conditional on exposure, however, providing enrollment guidance significantly increased platform access, renegotiation, and repayment. Heterogeneity analysis shows that effects are largest among socioeconomically vulnerable individuals who are not fully digitally excluded. The findings highlight both the potential and limitations of digital nudges: although they can relax frictions for reachable users, they fail to engage populations facing deeper digital barriers, constraining take-up at scale.

1 Introduction

The rapid digital transformation worldwide presents significant opportunities for enhancing public policy provision through increased efficiency. Digital platforms promise substantial gains and have become central to how governments deliver services and interact with citizens (OECD, 2024). However, the adoption and effective use of digital platforms remain uneven, due to persistent digital divides that go beyond geographic disparities and infrastructure availability (Forman et al., 2021). Even in areas with adequate connectivity, substantial differences in digital engagement and literacy persist, particularly along socioeconomic dimensions such as income and education (Pereira et al., 2024). As a result, digital government initiatives, while designed to broaden access to services, risk excluding the very populations that stand to benefit most, thereby exacerbating long-standing challenges in the take-up of social programs (Currie, 2004).

Our study combines a nationwide randomized controlled trial with rich administrative data from Brazil, an emerging economy that has recently pursued an ambitious digital transformation agenda

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to modernize public service delivery and expand inclusion.[1] Particularly, we study the Brazilian government's debt relief program, *Desenrola Brasil* (henceforth *Desenrola*), which exemplifies both the opportunities and the challenges of digital public service delivery. Launched as a fully online debt relief initiative, *Desenrola* offered substantial discounts to indebted individuals—especially low-income households, as a means to support financial recovery after the COVID-19 pandemic. Given the scale and generosity of the program, the government expected strong take-up. However, early usage data revealed extremely low engagement: several months into the program, only about 16% of eligible individuals had accessed the platform. This puzzlingly low figure raised important questions about digital access and user engagement in large-scale public programs.

We leverage the setting of Brazil's *Desenrola* program to evaluate whether low-cost, scalable informational interventions can effectively increase take-up in fully digital public service initiatives. We conduct a nationwide randomized controlled trial involving over 2.3 million eligible individuals, testing whether simple email messages can reduce barriers to digital engagement. Participants are randomly assigned to receive email messages with information about how to access the program online, how to seek in-person assistance at the post office, or both.

Our large-scale setting offers a valuable test of whether informational interventions can remain effective when broadly deployed, especially in the context of digital delivery. A growing body of research has documented that the effects of behavioral interventions can attenuate substantially when implemented at scale due to institutional features, low baseline engagement, and delivery challenges (Banerjee and Duflo, 2009; DellaVigna and Linos, 2022; Mitchell et al., 2023). Related studies also find that even when early pilot trials are promising, scaling up can lead to lower average impacts because of site selection bias and differences in implementation environments (Allcott, 2015). Recent theoretical work has further formalized how strategic site and sample selection can bias program evaluations and undermine external validity (Di Tillio et al., 2017, 2021). These challenges may be even more pronounced in settings with widespread digital exclusion.

Our findings speak directly to and complement this literature. First, on average, the intervention had no effect on program take-up, a result driven by extremely low engagement with the emails: only about 3% of recipients opened the messages. Second, engagement was selective. Individuals who opened the emails differed systematically from those who did not: they were more educated, less likely to be conditional cash transfer recipients, and more likely to have previously interacted with the *Desenrola* platform. By contrast, those who did not open the emails constitute a more socioeconomically vulnerable segment of the eligible population and were effectively left outside the scope of the intervention.

Finally, among those who did open the emails, receiving guidance on program enrollment significantly increased the likelihood of using the platform and program take-up. The intervention likely benefited those with some level of digital readiness who were struggling with the step-by-step enrollment process. Consistent with this, heterogeneity analysis shows that treatment effects are largest among individuals who are socioeconomically vulnerable but not fully digitally excluded. Conditional cash transfer beneficiaries and Black or Brown users exhibit the largest gains in platform access, suggesting that informational frictions disproportionately constrain disadvantaged groups. However, effects are much smaller for the elderly population, consistent with the idea that digital literacy is a prerequisite for acting on informational nudges. These findings highlight both the potential and the limits of light-touch digital nudges: while they can support participation among reachable users, they fall short of addressing deeper digital exclusion.

[1] Brazil ranks 50 out of 193 countries in the UN E-Government Development Index, with a score of 0.84—well above the world average of 0.64.

This paper contributes to a growing literature examining program take-up and the barriers that prevent it (see Currie, 2004, for a comprehensive review). Low participation in social programs is typically attributed to three core factors: lack of information about eligibility, transaction costs (including complex enrollment procedures), and stigma associated with benefit receipt. A growing body of research in behavioral economics shows that beyond awareness, reducing administrative and psychological frictions is critical to increasing take-up. For example, Bhargava and Manoli (2015) find that low participation in the U.S. Earned Income Tax Credit program is driven by confusion and complexity rather than financial or eligibility constraints. In France, Castell et al. (2025) show that providing eligibility information via an online simulator has no impact on take-up, while offering in-person assistance increases participation by up to 31%.

While these findings highlight the promise of light-touch interventions, the literature also exposes important limitations. Finkelstein and Notowidigdo (2019) show that information and assistance significantly increase enrollment in the Supplemental Nutrition Assistance Program (SNAP) among eligible U.S. adults, but also reduce program targeting: individuals who respond to the interventions tend to be less vulnerable than those who do not, suggesting that optimization frictions are especially binding for the most disadvantaged. This evidence reinforces the idea that even well-designed behavioral interventions may fail to reach or benefit the most vulnerable segments of the population, who often require more intensive forms of support.

We build on this literature by studying a fully digital public service in a middle-income country, where we target transaction costs through explicit guidance on how to enroll. Prior evidence suggests that simply enabling access to digital tools may be less effective than lower-tech alternatives, particularly for low-income populations, underscoring the importance of delivery mode in shaping take-up outcomes (e.g. Reyes et al., 2021). Our setting allows us to assess not only the overall effectiveness of light-touch digital interventions but also how digital readiness moderates individuals' responsiveness to them.

Consistent with Finkelstein and Notowidigdo (2019), we find that enrollment guidance can increase take-up, but only among individuals who are already somewhat digitally connected and thus reachable. Average treatment effects, however, remain null, primarily due to extremely low engagement. This highlights a key limitation of digital nudges: their effectiveness is constrained by persistent inequalities in digital access and digital literacy. In particular, digitally excluded individuals fail to engage with email-based messages, who are also those most in need of support. Our findings underscore the importance of complementing digital outreach with alternative delivery mechanisms, including in-person assistance, to ensure equitable access to technology-enabled public services.

By embedding a light-touch intervention within a nationwide digital public service, this paper also contributes to the literature on the attenuation of intervention effects at scale, which has thus far relied primarily on meta-analyses and cross-study comparisons (Dehejia et al., 2021; DellaVigna and Linos, 2022). Unlike many pilot studies, which typically recruit participants before randomizing the intervention, our design begins at scale, randomizing treatment across millions of eligible individuals drawn directly from the universe of program beneficiaries. This approach avoids the selection bias inherent in recruitment-based designs, which often exclude individuals who are not reachable or do not engage with the program (Allcott, 2015).

A key advantage of our setting is that we observe actual engagement with the intervention (e.g., email openings), allowing us to recover the subset of individuals who would likely be included in a more traditional randomized control trial. Among this engaged sample, we find positive treatment effects, consistent with prior smaller-scale studies using light-touch nudges (Bettinger et al., 2012; Bertrand et al., 2010; Karlan et al., 2016). However, analyses restricted to this group would bias intervention impacts, as they fail to capture the differential responsiveness, and lower reachability, of the broader target population. Our findings thus highlight the importance of large-scale experimental de-

signs for assessing not only average treatment effects, but also the external validity of behavioral interventions in real-world delivery environments (Di Tillio et al., 2021; Athey et al., 2025; Olsen et al., 2013).

2 Context and Background

Brazil has embarked on an ambitious journey towards digital transformation, seeking to modernize its public services and enhance citizen engagement through e-government initiatives. This push is reflected in national digital strategies such as the Brazilian Digital Transformation Strategy ("E-Digital", 2022–2026) and the National Digital Government Strategy (2024–2027), which aim to expand digital infrastructure, promote interoperability and digital identity, and strengthen digital literacy among citizens and public servants (Brazilian Ministry of Science and Innovation, 2022). Investments in national artificial intelligence capabilities, such as a proposed 23 billion-reais plan from 2024–2028, as well as longstanding public institutions in information and communication technologies (ICT), such as the Federal Data Processing Service (Serpro), reinforce Brazil's commitment (Gaier, 2024).

The government recognizes the potential of digital platforms, such as the *Gov.br* portal, a centralized digital ID platform that allows citizens to interact with a wide range of federal services, and foundational digital public infrastructure (DPI) to improve efficiency, transparency, and accessibility in service delivery, but faces challenges in ensuring inclusive access and preventing the reinforcement of existing socioeconomic inequalities (World Economic Forum, 2025). Addressing interoperability, public communication, and regulatory modernization remains vital for success.

Within this broader transformation framework, the government launched *Desenrola Brasil* ("Emergency Debt Renegotiation Program for Indebted Individuals") as a flagship initiative to tackle household debt and promote financial inclusion (Ayres, 2023). *Desenrola* was designed as a fully digital program, operated entirely through online platforms without physical service channels, underscoring the government's commitment to leveraging digital infrastructure to reach scale and reduce administrative costs. It is a two-phase, digitally mediated debt renegotiation program: Phase 1 (July 2023) targeted debts up to R$5,000 for citizens earning up to two minimum wages or registered in the government's unified registry for low-income individuals; Phase 2 (October 2023) expanded to include non-bank debts and higher incomes (Ayres, 2023; Ribeiro and Cunto, 2023). It offers discounts via auctioned creditor proposals, backed by government guarantees for the lower-income band and regulatory incentives for banks (Ayres, 2023).

Given its generous discounts, broad eligibility criteria, and the urgency of household indebtedness, *Desenrola* was expected to generate substantial take-up. The program offered attractive renegotiation terms, including government-backed guarantees and creditor incentives, particularly for low-income individuals with limited access to traditional credit markets. These features created strong incentives for participation among eligible beneficiaries. Nonetheless, the program take-up fell significantly short of expectations. Although *Desenrola* was intended to renegotiate debts for roughly 30 million individuals, actual participation remained well below this target by the end of 2023, prompting extensions of the deadline and raising concerns about bureaucratic hurdles and low user engagement (Le Monde, 2024).

A potential explanation for the low take-up is the program's fully digital design, which, when combined with the vulnerable profile of its target population, may have introduced several barriers to participation, particularly for individuals with limited digital literacy or restricted access to secure identification tools. Aggregate administrative data reveal that low take-up was not confined to final enrollment outcomes, but reflected attrition at multiple stages of engagement with the program. As shown in Table 1, although nearly 30 million individuals were eligible for *Desenrola*, only 16.3% accessed the platform prior to our intervention. Fewer than 8% renegotiated their debts, and

less than 7% made at least the first payment on a renegotiated debt.. This sharp attrition suggests that barriers arise well before the final decision to renegotiate or repay, pointing to frictions in access, navigation, or understanding of the program itself.

Table 1. Losing people along program enrollment

	% of Eligible	Number of Eligible
Eligible for Desenrola	1.000	29,434,970
Any access before treatment	0.163	4,791,234
Renegotiated before treatment	0.077	2,265,104
Paid renegotiation before treatment	0.068	1,999,762

Note: This table reports the proportion and number of individuals who progressed through key steps of the *Desenrola Brasil* program prior to our intervention. "Access" refers to logging into the digital platform; "Renegotiated" indicates completion of the debt renegotiation process; and "Paid renegotiation" indicates repayment of at least the first installment of the renegotiated debt. All statistics are computed over the universe of individuals eligible for *Desenrola* at the time of implementation.

A key feature of *Desenrola* is the complexity of its digital enrollment process. As illustrated in Figure A.1 in Appendix A, accessing the program required completing a sequence of autonomous steps beyond basic eligibility verification. In addition to authentication through the *Gov.br* system, individuals had to identify eligible debts, compare creditor offers, interpret financial terms, and actively select a repayment option. Each step demanded a combination of digital navigation, financial understanding, and confidence in interacting with online platforms.

This multi-step workflow likely posed additional challenges for individuals with low financial literacy or limited experience using digital services. The absence of real-time support or guided assistance may have further discouraged users who lacked the skills to manage the process independently. As a result, the program's effective reach was shaped not only by eligibility criteria, but also by a sequence of digital, informational, and procedural hurdles that disproportionately excluded less digitally prepared individuals.

2.1 Understanding barriers: Survey evidence and enrollment predictors

To better understand the mechanisms underlying low engagement, we conducted a survey among eligible individuals, delivered via email concurrently with the informational intervention described in the next section. The survey aimed to identify perceived barriers to participation, including lack of information, difficulties accessing the platform, digital constraints, and trust in government services.

Among the roughly 2 million eligible individuals invited to participate in the survey, only 3% opened the email, highlighting a significant engagement barrier. Nonetheless, the responses collected offer meaningful insights into the challenges individuals faced when interacting with the *Desenrola* program. Respondents frequently cited difficulties related to platform usability, lack of clear information about eligibility and procedures, and limited confidence in using digital tools. Even among those aware of the program, many reported being unsure of how to proceed, suggesting a gap between awareness and actionable understanding. To better explore the nature and prevalence of these constraints, we next examine descriptive evidence from the survey, with particular attention to reported access barriers, indicators of digital preparedness, and perceptions of the enrollment process.

Figure 1 highlights key barriers to *Desenrola* adoption identified in the survey. A striking 62% of households reported not knowing how to access the platform, underscoring the critical need for improved communication strategies and user-friendly access points. Eligibility concerns were also also mentioned, with about 29% of households believing they were ineligible, suggesting a potential misinterpretation of eligibility criteria. Finally, only 10% of households indicated a lack of interest,

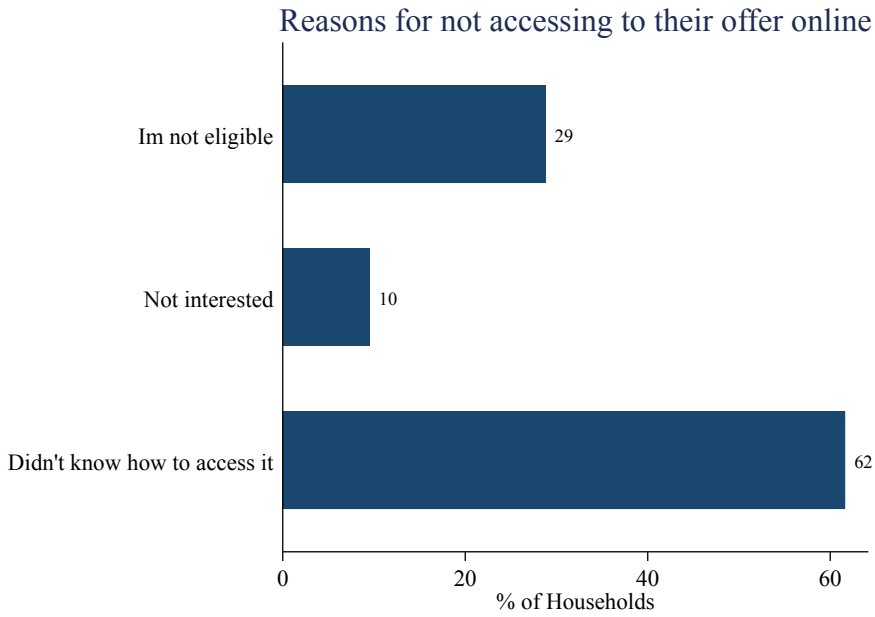

Figure 1. Survey Responses: Knowledge of Platform Access

Notes. Figure reports survey responses collected via email from a sample of individuals eligible for *Desenrola*. Participation was conditional on opening the survey email, which was sent to approximately two million eligible individuals. Percentages are computed over survey respondents.

which shows that skepticism, stigma, or a preference for alternative debt resolution approaches was not a major barrier when compared to informational barriers.

Figure 2 delves into the specific challenges encountered by users attempting to navigate the *Desenrola* platform. The most significant obstacles were difficulties locating the correct website link (53%) and difficulties with the *Gov.br* verification step (44%), pointing to potential issues with the platform's accessibility. Complications with potential platform loading (40%) and unclear instructions (36%) further exacerbated the user experience. The need for in-person assistance (36%) highlights the importance of addressing the digital divide. Although lack of internet access (28%) was an important constraint, it was much lower than the other problems mentioned by the respondents.

Figure 3 presents a compelling correlation between households' financial stability and their knowledge of and ability to navigate the *Desenrola* platform. Among those struggling most to cover monthly expenses ("Very Hard"), a significant 62% lack the knowledge to navigate the platform, despite 59% being aware that *Desenrola* is a debt renegotiation program. This indicates that awareness alone is insufficient to ensure access and utilization. Even among households with greater financial ease, a small proportion still struggle to navigate the platform. The trend reveals that as the ease of covering expenses increases, so too does the proportion of households who know about *Desenrola* and can navigate its platform, highlighting the potential for socioeconomic factors to influence program accessibility.

3 Research Design

To investigate the impact of low-cost informational interventions on the take-up of the *Desenrola* debt relief platform, we conducted a large-scale randomized controlled trial (RCT) with five exper-

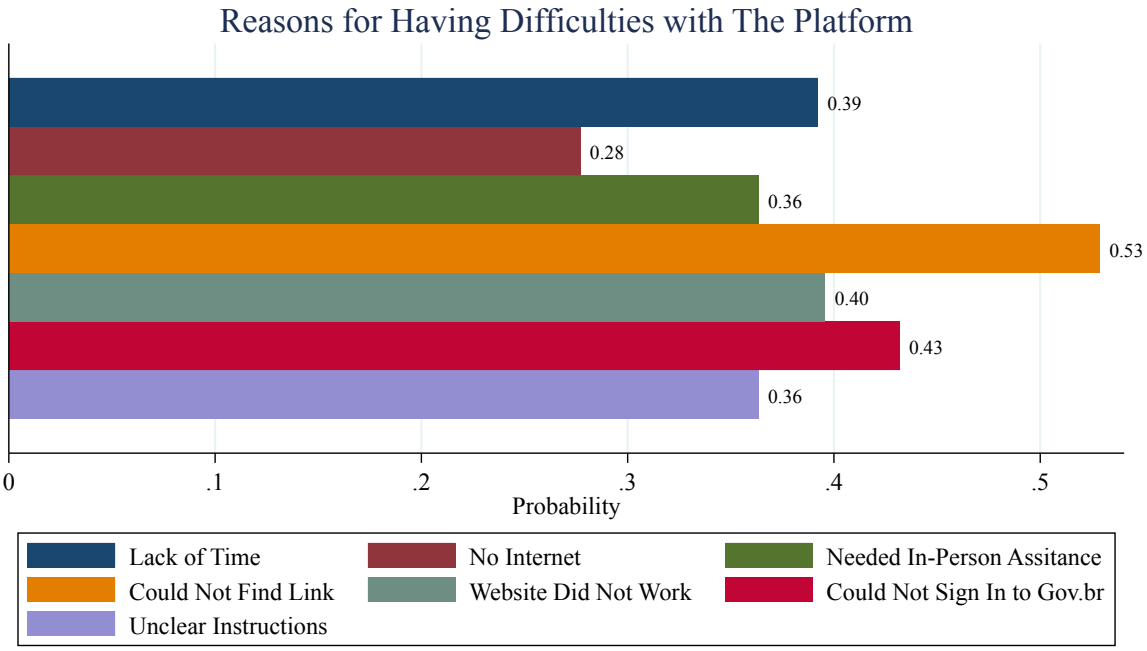

Figure 2. Survey Responses: Problems Accessing the Website

Notes. Figure reports survey responses collected via email from a sample of individuals eligible for *Desenrola*. Participation was conditional on opening the survey email, which was sent to approximately two million eligible individuals. Percentages are computed over survey respondents.

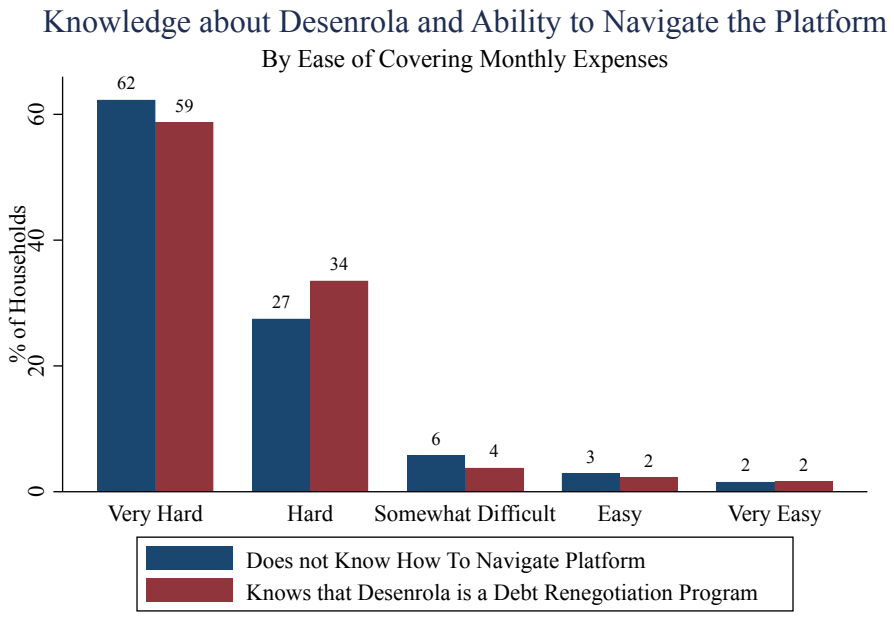

Figure 3. Survey Responses: Difficulty Navigating the Platform

Notes. Figure reports survey responses collected via email from a sample of individuals eligible for *Desenrola*. Participation was conditional on opening the survey email, which was sent to approximately two million eligible individuals. Percentages are computed over survey respondents.

imental groups. The intervention was implemented through email communications, providing varying levels of guidance on accessing the platform.

Participants were randomly assigned to one of the following groups:

- **Control:** Participants received an email inviting them to complete a placebo survey without any mention to the *Desenrola* program whatsoever. Individuals were screened out after accepting the consent form.
- **Desenrola survey email only (*Generic Desenrola Email*):** Participants received an email inviting them to complete a survey regarding their awareness and usage of the *Desenrola* program.
- **Survey email + digital platform guideline (*Online Guide*):** In addition to the *Desenrola* survey invitation, participants received guidance on how to access and navigate the digital platform. This guidance included step-by-step instructions and links to relevant resources.
- **Survey email + Post Office assistance info (*Post Office*):** Along with the *Desenrola* survey invitation, participants received information about in-person assistance available at local post offices, including addresses, hours of operation, and a description of the services offered.
- **Survey email + guideline + Post Office (*Online Guide + Post Office*):** Participants received the *Desenrola* survey invitation, digital platform guidance, and post office assistance information, providing a comprehensive intervention.

The content of these communications was carefully designed to minimize cost while providing actionable information to potential users. The survey invitations and informational emails included standardized layouts, concise text, and visual aids to facilitate engagement. All materials used in the survey and intervention are presented in Appendix B. A detailed description of the survey instrument—including question wording, routing logic, and response scales—is provided in Appendix C.

3.1 Data and Study Design

Our analysis relies on multiple sources of data. We use administrative records from the *Desenrola* platform, which include information on program enrollment, debt renegotiation, and repayment outcomes. We also draw on administrative data from the *Gov.br* system, from which we extract individuals' digital credential levels—bronze, silver, or gold. In addition, we use email engagement metrics that track whether recipients opened and interacted with the email communications, as well as sociodemographic information from Cadastro Unico (CadUnico), the federal registry of low-income households used to determine eligibility for social programs. The CadUnico data include information on gender, age, race, and educational attainment. Together, these data allow us to assess both behavioral responses to the intervention and heterogeneity in treatment effects across population groups.

The intervention was initially delivered to approximately 3 million randomly selected individuals drawn from a population of 7 million individuals who were both eligible for *Desenrola* and belonging to CadUnico with complete profile information. After the emails were sent, we applied some sample restrictions to focus the analysis on individuals for whom the intervention was economically meaningful. Specifically, we excluded individuals who had already fully repaid at least one renegotiated debt prior to the intervention, as these individuals had successfully completed the program on their own and were therefore unlikely to be affected by additional informational guidance. We also excluded individuals with very small outstanding debts (below R$100), for whom enrollment in *Desenrola* was automatic and did not require navigating the digital platform. After imposing

these restrictions, the final analysis sample consists of 2,299,486 individuals. We employed a stratified randomization design to ensure balance across key baseline characteristics. Participants were stratified by socio-economic status, geography, and prior engagement with digital services, with random assignment to treatment groups then conducted within each stratum to ensure that differences in outcomes are attributable to the interventions.

The email messages were sent in two phases:

Phase 1 (Survey Implementation): From March 22–29, 2024, email invitations were sent. Over 460 thousand individuals received a placebo survey invitation, while about 1..8 million individuals received the *Desenrola* survey invitation.

Phase 2 (Intervention): Four hours after the survey invitations were sent, intervention emails were deployed to the respective treatment groups (Generic *Desenrola* Email, Online Guide, Post Office, Online Guide + Post Office).

3.2 Implementation

A defining feature of the *Desenrola* program is that participation required individuals to successfully navigate a sequence of procedural and digital steps. As discussed section 2, substantial attrition occurred at each stage of program enrollment, with only a small fraction of eligible individuals ultimately renegotiating or repaying their debts. A similar pattern characterizes the implementation of our informational intervention. Although the intervention was deployed at large scale, effective exposure was severely constrained by low engagement.

Table 2 documents the loss of individuals along the intervention pipeline. Emails were sent to approximately 2.3 million eligible individuals. While delivery rates were high, over 90 percent of emails were successfully received, engagement was extremely limited: only about 3 percent of recipients loaded (opened) the survey email. Because all intervention content was embedded in the body of the email, loading the email is a necessary condition for exposure to the treatment.

Table 2. Losing people along intervention

	% of Eligible	Number of Eligible
Survey email sent	1.000	2,299,486
Received survey email	0.91	2,089,884
Loaded/opened survey email	0.03	58,178

Notes. The table reports the fraction and count of eligible individuals retained at each step of the intervention pipeline. "Received survey email" refers to emails successfully delivered to inboxes, as recorded by the email provider. "Loaded/opened survey email" indicates that the email's content was successfully loaded on the recipient's device, which we use as a proxy for exposure to the informational treatment. Percentages are expressed relative to the 2,299,486 survey emails sent.

This sharp drop in engagement highlights an important distinction between deployment and reach. Although the intervention was delivered at population scale, only a small subset of eligible individuals was actually exposed to the informational content. As a result, any potential behavioral response to the intervention is necessarily concentrated among this exposed subgroup.

Importantly, these implementation losses are not random: individuals who drop out along the intervention pipeline tend to be systematically more vulnerable. Table 3 reports summary statistics for the full analysis sample and for the subset of individuals who loaded the survey email, which

Table 3. Summary statistics

	Mean Full Sample	SD Full Sample	Mean Loaded Email	SD Loaded Email	P-Value
CCT receiver	0.50	0.50	0.47	0.50	0.00
Elderly	0.11	0.32	0.12	0.32	0.03
Female	0.63	0.48	0.67	0.47	0.00
Black or Brown	0.67	0.47	0.64	0.48	0.00
Completed High School	0.40	0.49	0.48	0.50	0.00
Debt Relief Discount	0.79	0.18	0.79	0.17	0.00
Original Debt Value (R$)	4,909.72	6,758.57	5,120.89	6,936.94	0.00
Golden GovBr	0.34	0.47	0.37	0.48	0.00
Silver GovBr	0.12	0.32	0.14	0.35	0.00
Platform Access (pre-treatment)	0.13	0.34	0.21	0.41	0.00
Survey email delivered	0.93	0.25	1.00	0.00	0.00
Loaded survey email	0.03	0.16	1.00	0.00	
Observations	2,299,486		58,178		

Note. Loading the email indicates that its content was displayed and serves as a proxy for exposure. Differences refer to pre-intervention characteristics. P-values test equality of means across groups.

we interpret as those effectively exposed to the intervention. Although differences are often modest in absolute terms, the two groups differ systematically along several dimensions that are central to digital engagement. Individuals who loaded the email are less likely to be conditional cash transfer recipients, more educated, and more likely to hold silver or gold *Gov.br* credentials. They also exhibit substantially higher pre-treatment engagement with the program (platform access). These patterns indicate that exposure to the intervention is not random at scale: even low-cost, population-wide digital outreach disproportionately reaches individuals who are relatively more digitally prepared and already closer to program participation. This selection into exposure underscores a key challenge of large-scale digital interventions, while they can be deployed broadly at low cost, their effective reach remains constrained by pre-existing inequalities in digital access, literacy, and familiarity with government platforms. Together, these patterns underscore a central challenge of large-scale digital interventions: reach is endogenous and systematically correlated with individual characteristics.

3.2.1 Predictors of program participation and email engagement

To further characterize this selection process, Table 4 examines predictors of digital readiness, program engagement, and exposure to the intervention across the full population of eligible individuals. Three patterns are particularly salient. First, conditional cash transfer (CCT) recipients exhibit disadvantages in digital preparedness and intervention exposure: they are substantially less likely to hold higher-level *Gov.br* credentials and less likely to load the email, although they are slightly more likely to have the email successfully delivered. At the same time, CCT recipients are more likely to have accessed the platform and renegotiated debt prior to the intervention. One interpretation is that financial distress and the salience of debt relief created stronger incentives for program participation among this group, even in the presence of digital barriers. Second, elderly individuals face steep barriers throughout the digital funnel: they are markedly less likely to possess higher-level credentials, to access the platform, to renegotiate their debts, and to have the email delivered. Conditional on delivery, however, they are somewhat more likely to load the email. Finally, individuals who completed high school display the opposite pattern: they are more likely to hold higher-level digital credentials, to access the platform, to renegotiate their debts, and to load the email, consistent with higher digital literacy and greater ability to navigate bureaucratic procedures.

Table 4. Predictors of enrollment and email loading

	Golden/Silver Govbr	Platform Access		Renegotiated		Received Email		Loaded Email	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CCT receiver	-0.075***	0.009**	0.015***	0.002***	0.003***	0.003***	0.003***	-0.003***	-0.003***
	(0.006)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Elderly	-0.108***	-0.075***	-0.067***	-0.009***	-0.007***	-0.014***	-0.013***	0.002***	0.003***
	(0.009)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.054***	0.023***	0.027***	0.003***	0.003***	-0.007***	-0.007***	0.004***	0.005***
	(0.008)	(0.005)	(0.005)	(0.001)	(0.001)	(0.002)	(0.002)	(0.000)	(0.000)
Black or Brown	-0.009	-0.008	-0.007	-0.000	-0.000	0.007***	0.007***	-0.003***	-0.003***
	(0.007)	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Completed High School	0.118***	0.068***	0.060***	0.006***	0.004***	-0.012***	-0.012***	0.008***	0.008***
	(0.007)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Golden/Silver Govbr			0.072***		0.013***		0.004***		0.004***
			(0.002)		(0.001)		(0.001)		(0.000)
MDV	0.461	0.134	0.134	0.013	0.013	0.934	0.934	0.025	0.025
Observations	2299486	2299486	2299486	2299486	2299486	2299486	2299486	2299486	2299486
R^2	0.028	0.019	0.030	0.002	0.005	0.001	0.001	0.001	0.001

Notes. This table reports coefficients from regressions of digital readiness and program participation outcomes on baseline characteristics for the full analysis sample. Columns (1)–(9) correspond to the outcomes listed in the header. "Golden/Silver Gov.br" indicates possession of higher-level digital credentials required for *Desenrola* enrollment. "Platform access" and "Renegotiated" refer to pre-intervention behavior. "Received email" captures whether the survey email was successfully delivered; "Loaded email" measures loading of the email content, which we use as a proxy for opening the email. Standard errors are clustered at the strata level.

3.2.2 Randomization and balance

While there is selection into exposure since only a subset of individuals load the email, this does not threaten the internal validity of the experimental design. Balance checks confirm that treatment assignment is well balanced across the full eligible sample (see Appendix Table A.1), as expected given randomization. Importantly, balance is also preserved within the subsample of individuals who opened the email (see Appendix Table A.1).

The few statistically significant differences observed in the conditional balance table, most notably in completed high school and pre-intervention platform access, are very small in absolute magnitude and do not indicate meaningful compositional differences across treatment arms. Moreover, all baseline covariates reported in the balance tables are included as controls in our empirical specifications, further alleviating concerns about residual imbalance.

Taken together, this evidence indicates that selection affects reach rather than randomization: although exposure is uneven, treatment assignment remains as-good-as-random among exposed individuals. This supports a causal interpretation of treatment effects estimated within the loaded-email sample and underscores that attenuation at scale in our setting arises from limited engagement, not from compromised experimental validity.

3.3 Empirical Strategy

To estimate the effects of the informational intervention on program engagement, we rely on linear fixed-effects regressions at the individual level. We examine three main outcomes: (i) platform access, (ii) debt renegotiation, and (iii) repayment of renegotiated debt. For each outcome, we estimate both intent-to-treat (ITT) effects in the full eligible sample and treatment-on-the-treated (TOT) effects restricting the sample to individuals who opened the intervention email.

Our empirical strategy follows an analysis-of-covariance (ANCOVA) specification, in which post-intervention outcomes are regressed on treatment assignment controlling for the corresponding pre-intervention values. Our baseline specification takes the following form:

$$Y_i = \alpha + \beta_1 \, GenericDesenrolaEmail_i + \beta_2 \, EnrollmentInfo_i + \gamma \, Y_{i0} + X_i'\delta + \mu_m + \varepsilon_i. \tag{1}$$

where Y_{i0} denotes the pre-intervention value of the outcome and μ_m are municipality fixed effects. The vector X_i includes baseline individual characteristics measured prior to the intervention: indicators for participation in the Brazilian conditional cash transfer program, elderly status, gender, race, educational attainment, and *Gov.br* credential level, as well as the debt relief discount associated with the individual's eligible debt. Standard errors are clustered at the randomization stratum level.

Our empirical strategy compares the effects of informational emails about *Desenrola* to a control group that received a generic email unrelated to the program. This comparison allows us to isolate the causal impact of program-related communication from general email contact.

A key component of the design is the inclusion of a *generic Desenrola email*, which mentions the program but does not provide enrollment guidance. This treatment serves to test a pure salience mechanism: if simply reminding individuals about the program is sufficient to increase enrollment, then exposure to the generic Desenrola email should generate measurable effects relative to the generic non-Desenrola email.

The remaining treatment arms augment this baseline salience channel by providing actionable enrollment information, such as step-by-step online guidance, information on in-person assistance, or both. Comparing these informational treatments to the generic Desenrola email allows us to assess whether reducing informational and procedural frictions generates additional effects beyond salience alone.

Table 5. Program enrollment: Overall (ITT)

	Platform Access		Renegotiated		Paid Renegotiation	
	(1)	(2)	(3)	(4)	(5)	(6)
Generic Desenrola Email	-0.000	-0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Enrollment Info Email	-0.001		-0.000		-0.000	
	(0.000)		(0.000)		(0.000)	
Online Guide		-0.000		0.000		-0.000
		(0.001)		(0.000)		(0.000)
Post office		-0.001		-0.000		-0.000
		(0.001)		(0.000)		(0.000)
Online Guide + Post office		-0.001		-0.000		-0.000
		(0.001)		(0.000)		(0.000)
Controls	✓	✓	✓	✓	✓	✓
Baseline mean	0.134	0.134	0.013	0.013	0.000	0.000
Observations	2299474	2299474	2299474	2299474	2299474	2299474
R^2	0.555	0.555	0.311	0.311	0.008	0.008

Notes. ANCOVA estimates on full sample. Post-intervention outcomes are regressed on treatment assignment controlling for baseline outcomes, baseline covariates (CCT status, elderly status, gender, race, education, *Gov.br* credential, and debt relief discount), and municipality fixed effects. Standard errors clustered at the randomization stratum level. Baseline mean refers to the pre-intervention mean in the control group.

All specifications are estimated both on the full sample of eligible individuals—capturing intent-to-treat effects—and on the subsample of individuals who opened the email, which isolates effects among those effectively exposed to the intervention.

4 Results

We begin by presenting intent-to-treat (ITT) estimates for the full population of individuals who were assigned to receive an email, regardless of whether they engaged with the message. We then turn to results conditional on email opening, which capture treatment effects among individuals who were actually exposed to the informational content.

4.1 Overall Effects on Program Enrollment

Table 5 reports the overall (ITT) estimates of the effect of the email interventions on three outcomes: platform access, debt renegotiation, and repayment. Across all specifications, we find no statistically or economically meaningful effects of any treatment arm in the full sample. Neither the generic Desenrola email nor the informational emails, providing online guidance, information on in-person assistance, or both, increase platform access, renegotiation, or repayment relative to the control group that received a generic non-Desenrola email.

These null results are uniform across across outcomes and specifications and consistent with the low levels of engagement observed with our intervention at scale. As shown in Section 3.2, only about 3% of recipients opened the email, implying that the vast majority of individuals assigned to treatment were never exposed to its content. As a result, average treatment effects in the full sample are mechanically attenuated toward zero, even if the intervention is effective among those who are reached.

Table 6. Program enrollment: Loaded email (exposed)

	Platform Access		Renegotiated		Paid Renegotiation	
	(1)	(2)	(3)	(4)	(5)	(6)
Generic Desenrola Email	0.002	0.002	0.002	0.002	0.001	0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Enrollment Info Email	0.008***		0.005***		0.004**	
	(0.003)		(0.002)		(0.002)	
Online Guide		0.012***		0.006**		0.005**
		(0.003)		(0.003)		(0.002)
Post office		0.008*		0.006**		0.006**
		(0.005)		(0.002)		(0.003)
Online Guide + Post office		0.003		0.004		0.003
		(0.004)		(0.003)		(0.003)
Controls						
Baseline mean	0.214	0.214	0.023	0.023	0.000	0.000
Observations	57086	57086	57086	57086	57086	57086
R^2	0.610	0.610	0.354	0.354	0.048	0.048

Notes. ANCOVA estimates on sample of individuals who loaded the email. Post-intervention outcomes are regressed on treatment assignment controlling for baseline outcomes, baseline covariates (CCT status, elderly status, gender, race, education, *Gov.br* credential, and debt relief discount), and municipality fixed effects. Standard errors clustered at the randomization stratum level. Baseline mean refers to the pre-intervention mean in the control group.

4.2 Effects Conditional on Email Opening

Table 6 restricts the sample to individuals who opened the email, thereby focusing on those who were actually exposed to the intervention content. Within this exposed sample, the results contrast sharply with the overall effects.

First, simply mentioning the program does not appear sufficient to change behavior: the generic Desenrola email has small and statistically insignificant effects on all outcomes. In contrast, providing actionable enrollment information substantially increases engagement with the program. Receiving the enrollment information email raises platform access by 0.8 percentage points, corresponding to a 3.7% increase relative to the baseline access rate of 21.4%. The same treatment increases the probability of renegotiating debt by 0.5 percentage points, a 21.7% increase relative to the baseline renegotiation rate of 2.3%. Effects on repayment are also positive and similar to the effects on renegotiation.

Conditional on platform access, we find small and imprecise effects on renegotiation and no robust effects on repayment (see Appendix Table A.3). While point estimates for renegotiation are positive for some informational treatments, these effects are not statistically distinguishable from zero once we account for endogenous selection into access using Lee (2009) bounds (see Appendix Table A.4). This pattern is consistent with the intended scope of the intervention. The informational emails were designed to reduce informational and procedural frictions leading up to platform access and the initial steps of enrollment. They were not designed to affect downstream decisions, such as whether to complete renegotiation terms or finalize repayment, once individuals had already reached and navigated the platform.

The results also reveal important limits to informational intensity. While the online guide and post office treatments increase platform access and renegotiation when offered separately, combining the two channels yields no additional gains. The absence of effects for the combined treatment suggests that increasing informational complexity may offset the benefits of additional guidance, consistent with evidence that excessive information can reduce take-up in complex administrative environments (Bhargava and Manoli, 2015).

4.3 Dynamic effects on platform access

To further assess how the intervention affected engagement with the *Desenrola* platform, we complement the static estimates with a dynamic event-study analysis of platform access. Leveraging administrative data on the exact date of first access, we examine whether the informational treatments altered the timing and trajectory of access around the intervention.

Specifically, we construct a balanced panel at the individual–week level for a window spanning five weeks before and four weeks after the intervention email was sent. For each individual, we define a weekly indicator for platform access and estimate event-study specifications that interact treatment status with relative time to the intervention. All specifications include week fixed effects and control for baseline socio-demographic characteristics, with standard errors clustered at the individual level.

The results for platform access are shown in Figure 4. Two patterns stand out. First, as expected, there is no evidence of differential pre-trends across treatment arms: estimated coefficients in the weeks prior to the intervention are close to zero and statistically indistinguishable from one another. This supports the identifying assumption that, absent the intervention, access trends would have evolved similarly across groups.

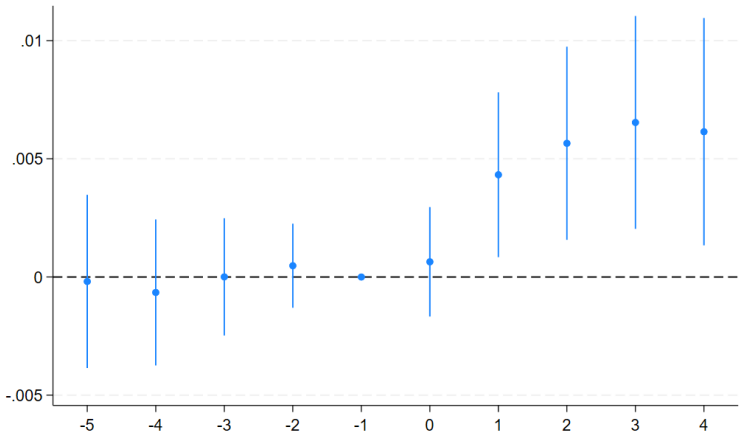

Figure 4. Dynamic effects of informational interventions on platform access

Notes. This figure reports event-study estimates of the effect of the informational interventions on weekly platform access, relative to the week before the intervention. The specification includes week fixed effects, baseline outcome (ANCOVA) and baseline controls; standard errors are clustered at the individual level.

Second, access increases sharply following the intervention for the informational treatments. Beginning in the first week after exposure, individuals receiving the information emails exhibit a discrete and persistent increase in platform access relative to receiving a generic email. The effects grow over the subsequent weeks and remain positive throughout the post-intervention window.

4.4 Heterogeneous effects on platform access

We next study heterogeneity in the effects of the informational intervention on platform access. While the average effects reported above show that actionable information increases engagement among exposed individuals, these averages may conceal substantial variation in responsiveness across individuals. This is particularly relevant in our setting, where digital readiness and socioeconomic vulnerability vary widely, and where informational frictions are unlikely to bind uniformly across the population.

To explore this heterogeneity in a data-driven way, we estimate Group Average Treatment Effects (GATES) following Chernozhukov et al. (2018). Rather than defining subgroups ex-ante, the GATES framework uses machine-learning predictions of individual-level treatment effects to rank individuals by their predicted responsiveness and then estimates average treatment effects within groups. This approach allows heterogeneity to emerge flexibly along multiple dimensions, while avoiding the multiple-testing and specification concerns associated with extensive subgroup analyses.

Our estimand is the effect of receiving any informational *Desenrola* email (online guide, post-office instructions, or their combination) relative to receiving a generic email. We restrict the analysis to individuals who opened the email, as heterogeneity in treatment effects is only meaningful among those exposed to the intervention content. The outcome of interest is platform access.

We implement the GATES procedure using an augmented inverse probability weighting (AIPW) estimator with cross-fitting. Potential heterogeneity is allowed to vary along a rich set of individual characteristics capturing socioeconomic vulnerability and digital preparedness, including receipt of conditional cash transfers, age, race, educational attainment, possession of a bronze *Gov.br* credential, and baseline platform access. All specifications flexibly control for gender, debt relief discount eligibility, receipt of the generic *Desenrola* email, and municipality fixed effects.

Figure 5 displays the estimated Group Average Treatment Effects (GATES) for platform access across quartiles of predicted responsiveness. Quartile 1 contains individuals with the highest predicted treatment effects and Quartile 4 those with the lowest. The estimates exhibit a clear ranking pattern: the informational intervention generates sizable and statistically significant increases in platform access for the top quartile (approximately 2 pp), smaller but still positive effects for Quartile 2, and near-zero effects for Quartiles 3 and 4. Formal heterogeneity tests reject the null of homogeneous treatment effects across groups, indicating that informational nudges differentially benefit subpopulations rather than shifting take-up uniformly. These results motivate a closer examination of the characteristics that distinguish highly responsive individuals from those who remain unaffected by the intervention.

To understand who drives the strong effects observed in Quartile 1 and the null effects in Quartile 4, Table 7 compares baseline characteristics across the most and least responsive groups. Two key patterns emerge. First, the informational intervention is most effective among individuals who are socioeconomically vulnerable and individuals that did not engage with the platform before the intervention. Relative to the least responsive quartile, individuals in the first quartile are substantially more likely to be conditional cash transfer recipients and to identify as Black or Brown. Importantly, they exhibit almost no pre-intervention platform access, indicating that the scope for improvement is largest precisely among those who had not previously engaged with the *Desenrola* platform.

Second, however, responsiveness to information appears to require a minimum level of digital readiness. Despite being more vulnerable on several socioeconomic dimensions, the most responsive group is markedly less likely to be elderly and is more likely to have completed high school. This pattern suggests that while informational barriers are particularly binding for disadvantaged individuals, the effectiveness of purely digital outreach still depends on basic digital and cognitive skills. Individuals facing more severe digital constraints—such as older adults remain less responsive even when exposed to the intervention.

Taken together, these results highlight a non-monotonic relationship between vulnerability and responsiveness to digital information. Informational interventions are most effective for individuals who are disadvantaged but not completely digitally excluded: those who lack access and knowledge ex ante, yet possess sufficient literacy to act once procedural and informational frictions are reduced. This helps explain why average effects mask substantial heterogeneity and reinforces the view that digital nudges alone may not reach the most digitally marginalized populations.

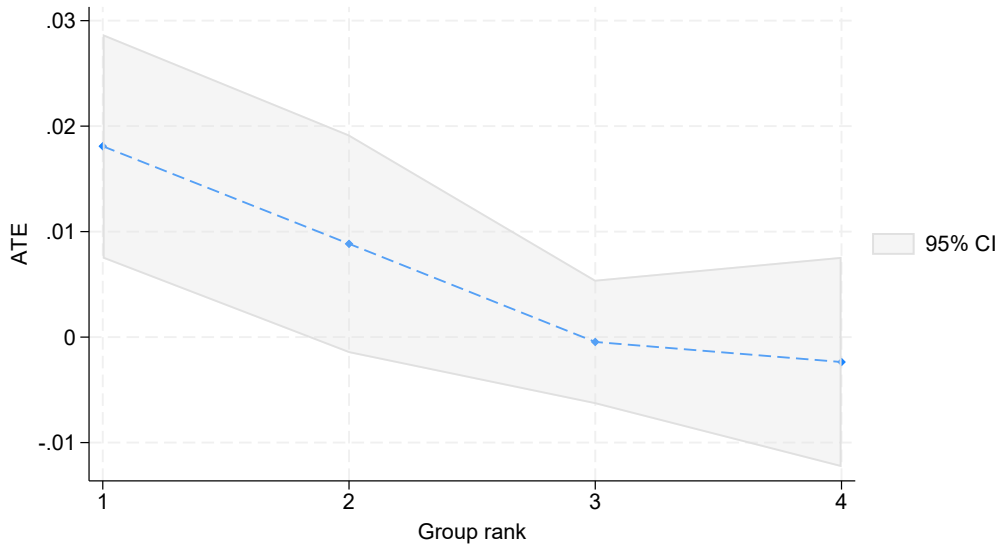

Figure 5. Heterogenous Treatment Effects for platform access

Notes. Figure displays Group Average Treatment Effects (GATES) for platform access. Individuals are partitioned into quartiles of predicted treatment responsiveness, with Quartile 1 representing the highest predicted effects. Estimates are obtained using the augmented inverse probability weighting (AIPW) CATE estimator with cross-fitting. Heterogeneity is estimated over baseline characteristics, including conditional cash transfer status, age, race, educational attainment, *Gov.br* credential level, and baseline access, controlling for gender, debt relief discount, treatment assignment, and municipality fixed effects. Standard errors correspond to group-level estimates. Formal heterogeneity and group comparison tests reject the null of homogeneous treatment effects across quartiles.

5 Conclusion

This paper examines the scalability and reach of light-touch digital informational interventions in the context of a nationwide program aimed at alleviating household debt in Brazil. By embedding a randomized intervention into the rollout of *Desenrola Brasil*, a fully digital debt renegotiation program, we evaluate not only whether simple guidance can increase take-up, but also whether such interventions remain effective when deployed at scale through digital-only channels.

Our findings offer three main insights for research and policy. First, in the full eligible population, the intervention has no detectable effects on platform access, renegotiation, or repayment. This null effect is driven by extremely low exposure: only about 3% of recipients opened the emails, sharply limiting the scope for any subsequent behavioral response. Importantly, engagement was selective. Individuals who did not open the emails were systematically more vulnerable—more likely to be conditional cash transfer beneficiaries and less likely to have completed high school—suggesting that digital channels struggle to reach precisely those whom the program aims to serve.

Second, conditional on exposure, informational guidance meaningfully increased participation. Among individuals who opened the emails, enrollment information increased platform access by 3.7% relative to baseline and raised downstream renegotiation and repayment. These patterns show that procedural and informational frictions were binding for a subset of users and that relatively low-cost guidance can relax them.

Third, effects among the exposed population are highly heterogeneous. Using the GATES framework (Chernozhukov et al., 2018), we find that the largest treatment effects occur among socioeconomically vulnerable individuals who had not interacted with the program before the intervention, but who were not fully digitally excluded. The most responsive group is disproportionately composed of conditional cash transfer recipients and Black or Brown users, yet is

Table 7. Characteristics of individuals by predicted treatment responsiveness

	Predicted treatment effect group		
	Highest quartile (G1)	Lowest quartile (G4)	Difference (G1–G4)
Conditional cash transfer recipient	0.817	0.276	0.541***
Elderly	0.062	0.179	-0.117***
Black or Brown	0.737	0.571	0.167***
Completed high school	0.575	0.472	0.104***
Bronze *Gov.br* credential	0.640	0.469	0.171***
Baseline platform access	0.001	0.169	-0.168***
Observations	15,743	13,445	

Notes. This table reports mean characteristics for individuals in the highest (G1) and lowest (G4) quartiles of predicted treatment effects on platform access, based on the GATES procedure. Differences are computed as G1 minus G4. Baseline platform access refers to access prior to the intervention. *** denotes significance at the 1% level.

markedly less likely to be elderly and more likely to have completed high school. This pattern highlights a non-monotonic relationship between vulnerability and responsiveness: informational nudges benefit the "reachable vulnerable," but not those facing deeper digital barriers such as limited literacy or difficulty navigating online authentication.

A broader implication of our findings concerns the limits of digital-only delivery models in public programs. As governments expand the use of digital platforms to reduce administrative costs and scale service provision, exposure to program information becomes endogenously determined by the distribution of digital readiness. In such environments, informational interventions may exhibit non-uniform treatment effects: average effects can be null not because the underlying mechanism fails, but because exposure is both selective and correlated with socioeconomic disadvantage. Small-scale studies that condition on exposure—whether explicitly through sampling or implicitly through self-selection—may therefore overstate the external validity of informational nudges when deployed at population scale via digital channels.

Moreover, our results highlight that digitization does not eliminate the role of procedural and informational frictions; rather, it shifts them onto new margins related to authentication, platform navigation, and digital trust. Individuals who are digitally marginal but not fully excluded appear responsive to light-touch interventions, whereas individuals facing more severe digital constraints remain unresponsive even when incentives are strong. This pattern suggests that the distributional consequences of digital program design depend not only on eligibility rules and financial incentives but also on the joint distribution of digital access and literacy. Without addressing variation along these margins, digital transformation may inadvertently reproduce or amplify existing inequalities in program take-up.

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Appendix A Additional Figures and Tables

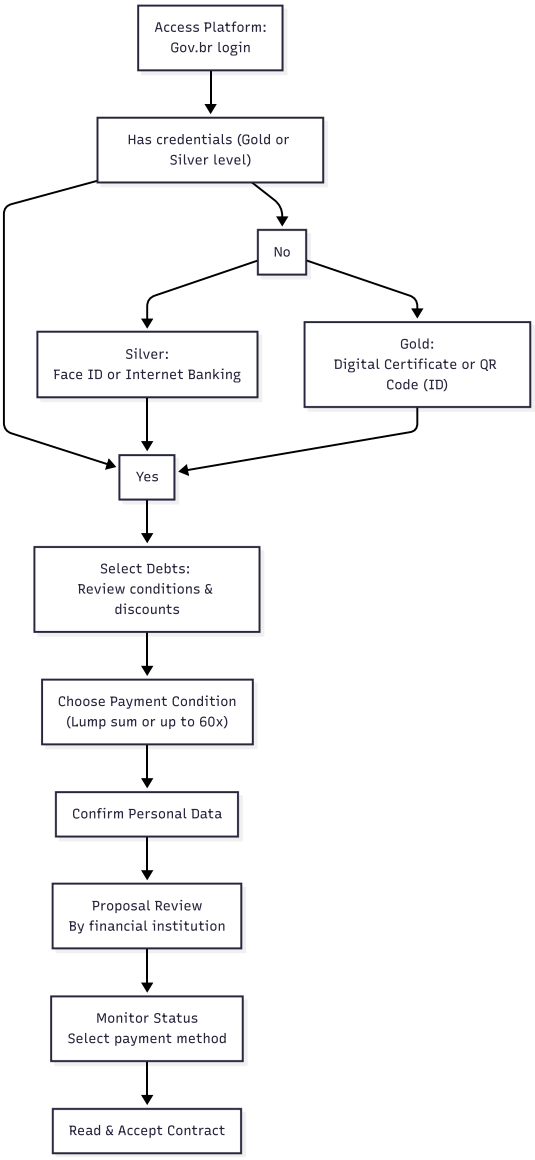

Figure A.1. Desenrola take-up process

Notes. This figure provides a schematic representation of the steps required for an eligible individual to enroll in *Desenrola Brasil*. The process involves authentication through the *Gov.br* digital identity system, selection of eligible debts, comparison of creditor proposals, and choice of payment options.

Table A.1. Balance checks: Unconditional

	Online Guide	Post office	Online Guide + Post office	Generic Desenrola Email	Generic Email	Joint test (p-value)
CCT receiver	0.50	0.50	0.50	0.50	0.50	0.76
Elderly	0.12	0.12	0.11	0.11	0.11	0.70
Female	0.63	0.63	0.64	0.63	0.64	0.94
Black or Brown	0.67	0.67	0.67	0.67	0.67	0.13
Completed High School	0.40	0.40	0.40	0.40	0.40	0.97
Debt Relief Discount	-0.00	-0.00	0.00	-0.00	0.00	0.18
Original Debt Value (R$)	4,912.31	4,910.88	4,905.70	4,909.63	4,910.09	1.00
Bronze GovBr	0.54	0.54	0.54	0.54	0.54	0.57
Platform Access (pre-treatment)	0.13	0.13	0.13	0.13	0.13	0.25
Renegotiated(pre-treatment)	0.01	0.01	0.01	0.01	0.01	0.32
Survey email delivered	0.93	0.93	0.93	0.93	0.93	0.23
Loaded survey email	0.02	0.02	0.02	0.03	0.04	0.00
Observations	459,669	460,298	460,466	459,687	459,366	

Notes. This table reports unconditional means of baseline characteristics across the five experimental arms prior to the intervention. Columns correspond to treatment assignments. The final column reports the p-value from a joint test of equality of means across groups. Baseline characteristics include demographic variables, socioeconomic indicators, pre-intervention program outcomes, and email delivery/engagement metrics.

Table A.2. Balance checks: Conditional on Loading email

	Online Guide	Post office	Online Guide + Post office	Generic Desenrola Email	Generic Email	Joint test (p-value)
CCT receiver	0.47	0.47	0.47	0.47	0.47	0.95
Elderly	0.12	0.12	0.12	0.12	0.12	0.98
Female	0.67	0.68	0.67	0.68	0.67	0.32
Black or Brown	0.65	0.65	0.64	0.64	0.65	0.63
Completed High School	0.48	0.46	0.48	0.47	0.49	0.00
Debt Relief Discount	0.01	0.01	0.02	0.00	0.03	0.17
Original Debt Value (R$)	5,029.81	5,128.42	5,205.68	5,060.49	5,164.29	0.28
Bronze GovBr	0.49	0.50	0.49	0.49	0.49	0.68
Platform Access (pre-treatment)	0.21	0.22	0.22	0.21	0.21	0.09
Renegotiated(pre-treatment)	0.02	0.02	0.02	0.02	0.02	0.94
Observations	10,193	10,023	10,019	11,652	16,291	

Notes. This table reports means of baseline characteristics across treatment arms for the subsample of individuals who loaded the survey email. Columns correspond to treatment assignments. The final column reports the p-value from a joint test of equality of means across groups. Baseline characteristics include demographic variables, socioeconomic indicators, pre-intervention program outcomes, and email delivery/engagement metrics.

Table A.3. Renegotiated/Paid conditional on access

	Renegotiated		Paid renegotiation	
	(1)	(2)	(3)	(4)
Generic Desenrola Email	0.004	0.004	-0.002	-0.002
	(0.009)	(0.009)	(0.008)	(0.008)
Enrollment Info Email	0.011*		0.007	
	(0.006)		(0.006)	
Online Guide		0.014*		0.008
		(0.008)		(0.007)
Post office		0.013*		0.011
		(0.008)		(0.008)
Online Guide + Post office		0.006		0.001
		(0.009)		(0.009)
Controls				
MDV (Baseline)	0.023	0.023	0.000	0.000
Observations	17473	17473	17473	17473
R^2	0.322	0.322	0.077	0.077

Notes. This table reports estimated treatment effects on debt renegotiation and repayment conditional on having loaded the intervention email and accessed the Desenrola platform. All regressions use an ANCOVA specification that controls for the baseline value of the outcome and include the same set of covariates as in the main analysis: indicators for conditional cash transfer participation, elderly status, gender, race, educational attainment, *Gov.br* credential level, and the debt relief discount, as well as municipality fixed effects. Standard errors are clustered at the randomization stratum level.

Table A.4. Renegotiated/Paid conditional on access – Lee (2009) bounds

Lee (2009) bounds		
	Renegotiated	Paid renegotiation
Desenrola Intervention		
Lower bound	-0.018	-0.023
	(0.021)	(0.022)
Upper bound	0.018	0.012
	(0.011)	(0.009)

Notes. This table reports Lee (2009) bounds for the effect of the informational Desenrola intervention on renegotiation and repayment, conditional on accessing the platform. The trimming procedure uses access as the selection variable and is tightened using baseline covariates to reduce residual selection. Standard errors for the bounds are reported in parentheses.

Appendix B Survey Invitations and Intervention Material

Figure B.1. Survey Invitation

Subject: “Convite: pesquisa sobre o Desenrola Brasil”

Email text:

MINISTÉRIO DO PLANEJAMENTO E ORÇAMENTO

GOVERNO FEDERAL BRASIL UNIÃO E RECONSTRUÇÃO

Olá, estamos entrando em contato como parte de um estudo conduzido pelo **Ministério do Planejamento e Orçamento**, para compreender a participação no **programa "Desenrola Brasil".**

Convidamos você a responder a esta **pesquisa** sobre o assunto. Suas respostas são confidenciais e serão usadas apenas para avaliar **os efeitos e a necessidade de políticas públicas, como o programa "Desenrola Brasil".** O tempo estimado para responder a esta pesquisa é **inferior a 5 minutos**. Você pode respondê-la clicando neste **link** [add hyperlink with individual link here].

Agradecemos sua participação!

Caso não queira receber mais nossos e-mails, clique **aqui** para se descadastrar. [add the unsubscribe link]

Figure B.2. Placebo Survey Invitation

Subject: "Convite – pesquisa"

Email text:

MINISTÉRIO DO **PLANEJAMENTO E ORÇAMENTO**

GOVERNO FEDERAL BRASIL UNIÃO E RECONSTRUÇÃO

Olá, estamos entrando em contato como parte de um estudo conduzido pelo **Ministério do Planejamento e Orçamento**.

Convidamos você a responder uma **pesquisa**. Suas respostas são confidenciais e serão usadas apenas para avaliar **os efeitos e a necessidade de políticas públicas.** O tempo estimado para responder a esta pesquisa é **inferior a 5 minutos**. Você pode respondê-la clicando nest **link** [add the **placebo** individual link here – screen out after consent form].

Agradecemos sua participação!

Caso não queira receber mais nossos e-mails, clique **aqui** para se descadastrar. [add the unsubscribe link]

Figure B.3. Desenrola Survey Email + Online Guide Intervention Email

Subject: "Participe do Desenrola Brasil – Guia passo-a-passo"

Email text:

MINISTÉRIO DO PLANEJAMENTO E ORÇAMENTO

GOVERNO FEDERAL BRASIL UNIÃO E RECONSTRUÇÃO

Olá novamente! Estamos enviando este e-mail como parte de um **estudo** conduzido pelo **Ministério do Planejamento e Orçamento**.

Sabemos que às vezes pode ser um pouco **complicado fazer coisas digitalmente**, então estamos aqui para ajudar você com um **guia passo-a-passo sobre como participar do programa "Desenrola Brasil"**:

["Infographic_onlineguide.pdf" goes here]

Caso não queira receber mais nossos e-mails, clique **aqui** para se descadastrar. [add the unsubscribe link]

Figure B.4. In-person Help Intervention Email

Subject: “Participe do Desenrola Brasil com a ajuda dos Correios”

Email text:

MINISTÉRIO DO PLANEJAMENTO E ORÇAMENTO

GOVERNO FEDERAL BRASIL UNIÃO E RECONSTRUÇÃO

Olá novamente! Estamos enviando este e-mail como parte de um **estudo** conduzido pelo **Ministério do Planejamento e Orçamento**.

Sabemos que às vezes pode ser um pouco **complicado fazer coisas digitalmente** e gostaríamos de **compartilhar uma novidade**: agora você pode ir até uma **Agência dos Correios** para obter **ajuda pessoalmente** para se inscrever no **programa “Desenrola Brasil”**. Veja abaixo um guia passo-a-passo sobre como proceder para ter a **ajuda dos Correios**:

[“Infographic_inpersonguide.pdf” goes here]

Caso não queira receber mais nossos e-mails, clique **aqui** para se descadastrar. [add the unsubscribe link]

Figure B.5. Desenrola Survey Email + Online Guide + In-person Help Intervention Email

Subject: "Participe do Desenrola Brasil – online ou com a ajuda dos Correios"

Email text:

MINISTÉRIO DO PLANEJAMENTO E ORÇAMENTO

GOVERNO FEDERAL BRASIL UNIÃO E RECONSTRUÇÃO

Olá novamente! Estamos enviando este e-mail como parte de um **estudo** conduzido pelo **Ministério do Planejamento e Orçamento**.

Sabemos que às vezes pode ser um pouco **complicado fazer coisas digitalmente** e gostaríamos de **compartilhar uma novidade**: agora você pode ir até uma **Agência dos Correios** para obter **ajuda pessoalmente** para se inscrever no **programa "Desenrola Brasil"**. Veja abaixo um guia passo-a-passo sobre como proceder para ter a **ajuda dos Correios**.

Se você preferir fazer tudo digitalmente, pode seguir o nosso **guia passo-a-passo de como se inscrever no programa online**, que também disponibilizamos abaixo**.**

["Infographic_inpersonguide.pdf" goes here]

["Infographic_onlineguide.pdf" goes here]

Figure B.6. Desenrola Survey Email + Online Guide Info-graphics

Renegocie suas dívidas com o Desenrola Brasil

Um guia passo a passo

INICIATIVA EM PARCERIA COM

MINISTÉRIO DA **FAZENDA**

MINISTÉRIO DO **PLANEJAMENTO E ORÇAMENTO**

GOVERNO FEDERAL

BRASIL

UNIÃO E RECONSTRUÇÃO

Ainda dá tempo de negociar suas dívidas no Programa Desenrola Brasil! Aproveite juros mais baixos, descontos de até 96% e parcelamentos em até 60 meses, com primeira parcela em até 60 dias, ou seja, sem entrada.

Veja como se inscrever abaixo.

PASSO 1

O primeiro passo para renegociar suas dívidas, é ter uma conta **gov.br.** Se você já tem essa conta, passe para o passo 2. Caso ainda não tenha, siga as instruções para configurar uma conta aqui: **acesso.gov.br/faq/_perguntasdafaq/contaacesso.html**

Sabia que você pode se cadastrar no **Gov.br** em menos de 5 minutos?

Sabia que agora você pode acessar o programa mesmo com uma conta bronze do **Gov.br**?

PASSO 2

Depois de ter uma conta Gov.br, acesse a plataforma Desenrola e use seu login do Gov.br para acessar a plataforma em: **desenrola.gov.br**

PASSO 3

No menu "Minhas dívidas", você pode visualizar todas as dívidas que foram cadastradas no programa e estão elegíveis para renegociação.

*numeros meramente ilustrativos

Figure B.7. In-Person Help Info-graphics

Appendix C Survey Material

Section A0 – General Questions about the "Desenrola" Program

A0.1) Do you know about the existence of a new government program called "Desenrola"?

(a) No *(Send to Section B)*

(b) Yes, it is a new cash transfer program. *(Send to Section B)*

(c) Yes, it is a program to facilitate debt renegotiation. *(Continue)*

(d) Yes, it is a tax reform. *(Send to Section B)*

(e) Yes, it is a pension reform. *(Send to Section B)*

A0.2) Did you access the debt renegotiation offers available on the "Desenrola" Program platform (website)?

(a) Yes *(Send to Section A3)*

(b) No *(Continue)*

A0.3) Why didn't you access the debt renegotiation offers through the platform?

(a) Because I know I am not eligible. *(Send to Section B)*

(b) Because I am not interested. *(Send to Section A1)*

(c) Because I did not know how to access it. *(Send to Section A2)*

Section A1 – Lack of Interest in the Program

On a scale from 1 (completely disagree) to 5 (completely agree), indicate your agreement with each statement:

A1.1) Reasons for lack of interest:

(a) I do not have past debts eligible for renegotiation.

(b) I do not have any available income to pay debts.

(c) I do not see any benefits in paying past debts.

(d) Even after paying past debts, banks will still not offer me new loans.

(e) I think my past debts are unfair and I have no intention of paying them.

(f) I have other debts not included in the program.

(g) I have been able to get loans/installment plans despite a negative credit history.

(Send to Section B after this block.)

Section A2 – Difficulties Accessing the Platform

On a scale from 1 (completely disagree) to 5 (completely agree), indicate your agreement with each statement:

A2.1) Reasons for difficulty:

(a) I did not find the time.
(b) I do not have reliable internet access.
(c) I needed in-person assistance but couldn't find it.
(d) I do not know the website address.
(e) I tried but the website does not work properly.
(f) I did not know how to log into my Govbr account.
(g) The instructions were unclear or hard to follow.

(Send to Section B after this block.)

Section A3 – For Those Who Saw the Offers

A3.1) Did you renegotiate any of your debts?

(a) Yes *(Send to Section B)*
(b) No *(Continue)*

A3.2) On a scale from 1 (completely disagree) to 5 (completely agree), indicate agreement with reasons you did not renegotiate:

(a) The discounts were not large enough.
(b) I was not given a credit line to pay debts.
(c) The installments were still unaffordable.
(d) Other debts made it pointless to pay those included.
(e) I'm not interested in paying debts.
(f) I had problems accessing the platform and gave up.

(Send to Section B after this block.)

Section B – Consumption and Standard of Living

B.1) In the past year, the monthly household income allowed you to live:

(a) Very hardly
(b) Hardly
(c) With some difficulties
(d) Easily
(e) Very easily

B.2) In the past year, did your household delay any bills due to financial difficulties?

(a) Yes
If yes, which bills?
(i) Rent or mortgage
(ii) Water
(iii) Electricity
(iv) Gas
(v) Installment payments
(vi) Other

(b) No

B.3) During the most recent application for credit/loan, what was the outcome?

(a) Rejected
(b) Approved in full
(c) Approved for a smaller amount
(d) Withdrawn due to high interest
(e) Could not provide necessary documents
(f) Blacklisted

Section B – Mental Health (MHI-5)

During the past month:

B.4) How much of the time were you a happy person?

B.5) How much of the time have you felt calm and peaceful?

B.6) How much of the time have you been a very nervous person?

B.7) How much of the time have you felt downhearted and blue?

For each, options:

(a) All of the time
(b) Most of the time
(c) Some of the time
(d) A little of the time
(e) None of the time

Based on: Stillman et al. (2009)