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Research Article

Crossing the Finish Line: Administrative Attention Overload, Reminders, and SNAP Application Completion

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Abstract: Incomplete online applications are a substantial challenge for the Supplemental Nutrition Assistance Program (SNAP), the largest anti-hunger program in the United States. As applications become easier to start, they may also become easier to abandon. We conceptualize administrative attention overload as a form of cognitive load that arises when the sustained attentional demands of an administrative process exceed an individual's available cognitive capacity. In the large U.S. state we studied, more than 40 percent of applicants did not complete their applications. We test whether simple reminder messages can help applicants return and finish their applications. In a field experiment (N = 5,978), receiving a reminder increased application completion among applicants who had abandoned their applications by 26 percentage points within four days (95% CI: 24.4 to 27.7). By contrast, varying reminder timing by four days did not affect long-run completion rates. The difference was only 0.5 percentage points (p = 0.68). A simple reminder can substantially raise completion among people who have already shown enough interest to start an application. As states increasingly digitize safety net enrollment, reducing administrative burdens requires not only making applications easier to start but also providing support to help applicants finish them.

Keywords: Social Policy, Food Insecurity, Administrative Burden, Digital Accessibility, Administrative Attention Overload
Supplements: Open data, Open materials

The digitalization of public services has reduced some administrative burdens of interacting with the state and increased others. Compared to processes manned by street level bureaucrats, digital systems can be easier by reducing the need to physically travel and complete forms. "Automated encounters," however, come with their own limitations that can increase digital administrative burdens (Peeters, 2023), including burdens caused by poor design and digital exclusion experienced by those with lower technological literacy. Other types of barriers in these encounters are less obvious. Digital opportunities also create new cognitive demands on users. Online applications make it easy to start a process, but also make it easier to walk away unfinished since a digital form carries little of the friction of, say, abandoning an in-person appointment. For policymakers, the digital footprint offers an opportunity to convert demonstrations of interest in a program into completion of an administrative process. Relatively low-level nudges may be able to push those who started an administrative process to reach the finish line.

The seemingly mundane challenge of application completion has large, real world policy implications. For example, the Supplemental Nutrition Assistance Program (SNAP) is the largest anti-hunger program in the

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United States. In May 2025, SNAP helped 41.7 million people in 22.4 million households across the country (DeSilver, 2025). Yet the program continues to fall short of reaching its full potential. In fiscal year 2022, SNAP participation rate rose to a historically high 88 percent, up from 81 percent in fiscal year 2020. Still, about 12 percent of eligible individuals, roughly 5 million people, were left out, a number larger than the entire population of Louisiana or Oregon (Vigil & Rahimi, 2024).

Administrative burden (Herd & Moynihan, 2018; Heinrich, 2016) is a key reason for this implementation challenge, encompassing three types of costs: learning costs, such as confusion about eligibility or required documentation; compliance costs, such as the time and effort required to complete applications and recertify benefits; and psychological costs, such as stress, stigma, and fear. A growing body of research has examined how these burdens operate across different policy domains, mechanisms, and outcomes. Studies have examined administrative easing in Medicaid enrollment (Fox et al., 2020) and psychological frictions in Earned Income Tax Credit take-up (Bhargava & Manoli, 2015). Other work examines the distributive effects of administrative burdens on decision-making (Martin et al., 2023), tolerance of administrative burden in relation to representative bureaucracy and group identity (Johnson & Kroll, 2021), messenger effects in social program uptake (Bhanot, 2025), and stigma in social safety net delivery (Lasky-Fink & Linos, 2024). Even when low-income individuals are aware of SNAP and motivated to participate, they must still possess the knowledge and ability to navigate and complete the administrative process. Prior research on SNAP and administrative burden shows that information barriers, category confusion, interview requirements, and procedural complexity can reduce program participation (Finkelstein & Notowidigdo, 2019; Moynihan et al., 2022; Giannella et al., 2024; Kim et al., 2025).

SNAP is federally funded, but state administered, and, in some states, administered by counties. In response to persistent access challenges, many states have modernized the application process to make it easier for individuals to apply at a time and place convenient to them. One such development is the creation of benefit enrollment websites where people can apply for SNAP online. This trend began in January 2002 (see Appendix A). As of 2024, two decades later, residents in 48 states can apply for SNAP online, with Idaho and Wyoming as the only exceptions (Code for America, 2025).

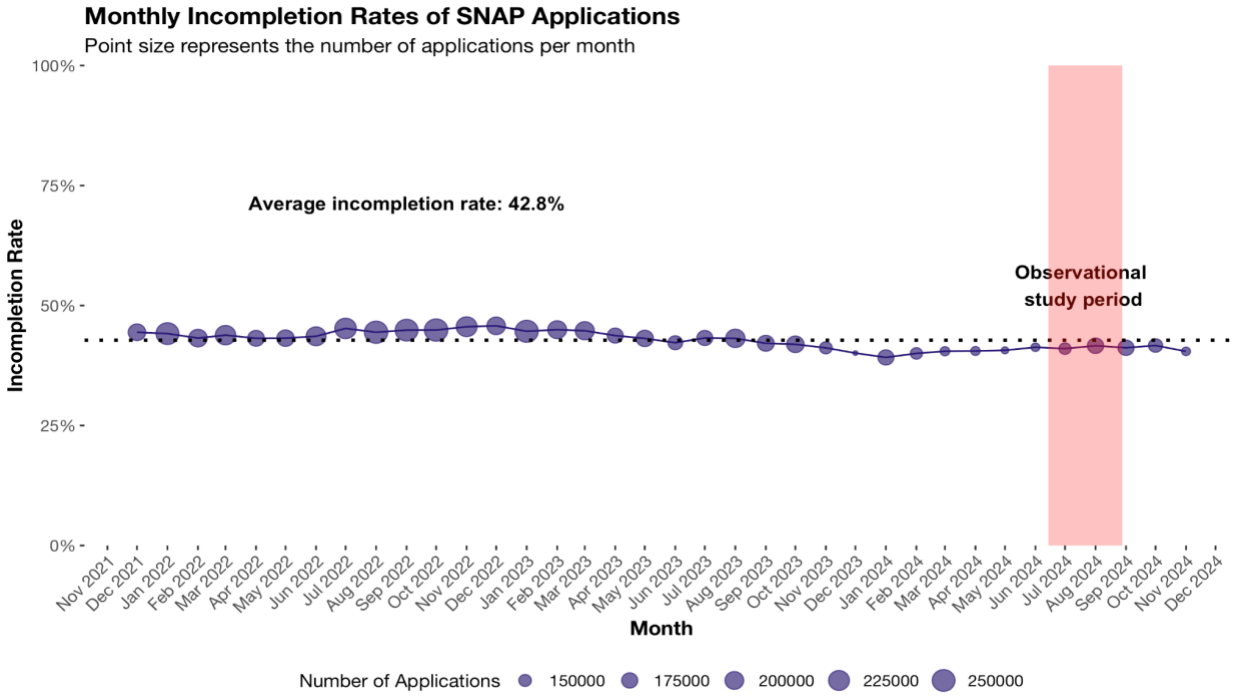
Lowering the transaction costs of starting an application may also lower the cost of abandoning it. There is evidence that sunk costs of this kind shape whether people choose to proceed (Haita-Falah, 2017). Simon (1997, p. 90) argued that the heavier a task, the more single-minded people tend to be, and that lighter tasks leave more room for attention to wander. When the task environment becomes more interruption-prone and choice-rich, attention is more likely to fragment unless an intervention counteracts these structural features (Simon, 1990, p. 7).

The online environment plausibly sits at the lighter end of this continuum, since switching from a benefit application to an email or a social media feed costs the user almost nothing. But whether digital environments impose larger attentional costs per se is contested. While heavy media multitaskers have been shown to have greater susceptibility to distraction (Ophir et al., 2009), replications and a meta-analysis have recovered that pattern inconsistently (Wiradhany & Nieuwenstein, 2017). We therefore do not claim that online forms are inherently distracting. Instead, we claim that online settings offer low-cost exits from partially completed tasks, and that a reminder can counteract them.

Designing safety net applications to be mobile-friendly is a sound policy choice, particularly for serving low-income Americans who rely on smartphones for internet access (Pew Research Center, 2025). Yet the same mobile environments that expand access also intensify exposure to distraction. As more SNAP applications have moved online, so have daily activities and potential distractions. Most administrative systems provide no clear mechanism to prompt reengagement once attention is diverted. The digital structure of an online application process may lead applicants to assume they can return and complete the form later. Without structured reminders, they remain vulnerable to competing demands on their attention (Ophir et al., 2009).

This is not a small problem, as the volume of abandoned cases is substantial. In the large U.S. state where we conducted our study, data from the SNAP online application platform show that about four in ten applicants abandoned their applications. Because the state has offered SNAP applications online since 2007, we cannot estimate changes associated with their adoption. However, the incompletion rate remained stable during the period for which we have data, from December 2021 through November 2024 (see Figure 1).

Figure 1. Monthly Incompletion Rates of SNAP Applications



Notes: This figure shows the share of incomplete SNAP applications each month between December 2021 and November 2024 in the large and diverse U.S. state where the study was conducted. Point size reflects the number of applications submitted in each month, illustrating variation in application volume over time. The dotted horizontal line marks the average incompletion rate across the period (42.8 percent). The red shaded area marks the observational study period, when reminder sends were logged (June 14 to August 28, 2024); reminders began in May 2024 and were sent only to applicants who had consented to receive text or email messages. Excluding the monthly observations from this period yields an average incompletion rate of 42.9 percent, a difference of just 0.085 percentage points.

This trend, however, can be reversed by drawing applicants' attention back to the task. If the task environment contributes to the problem, modifying that environment may shape applicants' behavior.

As a proof of concept, we tested whether reminders could prompt applicants to complete their unfinished applications. A simple before-and-after comparison showed a 12.7-percentage-point increase in the 14-day completion rate among applicants who were sent a reminder, suggesting that reminders can effectively bring applicants back to finish what they started.

Because this evidence was vulnerable to selection bias, we designed a two-arm experiment, preregistered on AsPredicted (March 4, 2025)[1], to test the effects of the reminder and its timing, using a sample of SNAP applicants from one of the most populous states in the United States (N = 5,978). Among applicants who had abandoned their applications, a simple reminder increased online SNAP application completion by 26 percentage points within four days (95% CI: 24.4 to 27.7 percentage points). By contrast, varying the timing of the reminder by four days did not affect long-run completion rates. The difference was only 0.5 percentage points ($p = 0.68$).

We interpret this finding through the lens of administrative attention overload, which we conceptualize as a form of cognitive load that arises when the sustained attentional demands of an administrative process exceed an individual's available cognitive capacity. In consumer choice contexts, the adverse effects of policies that do not account for consumers' or investors' limited attention capacity have been examined (Abaluck & Gruber, 2011; Chetty et al., 2009; Chin & Beckett, 2021; DellaVigna & Pollet, 2009; Persson, 2018). Yet this attention

challenge, and its relations to task environment, has been less studied in digital administrative procedures, despite their increasing relevance and substantial stakes. Introducing the reminder did not change the underlying task or the applicant's capacity to complete it. In Simon's (1990) terms, it left the computational-capability blade untouched. Instead, it altered the task environment by inserting a cue that redirected applicants' attention back to the unfinished application.

In what follows, we discuss how attention is shaped by the task environment and why it matters for completing unfinished tasks, in this case online benefit applications. We then describe the data, experimental design, results, and discussion.

Task Environment, Administrative Attention Overload, and Reminders

In Herbert Simon's theory of bounded rationality, human rational behavior is bounded by "a scissors whose two blades are the structure of task environments and the computational capabilities of the actor" (Simon, 1990, p. 7). A reminder does not affect the second blade, computational capability. It neither raises an applicant's cognitive capacity nor reduces the information the form demands. Instead, it affects the first blade, the task environment. A standard online application provides no cue to return once an applicant's attention has moved on, whereas the reminder intervention provides such a cue. Simon's metaphor suggests that reshaping the environment can change outcomes even when the actor's capacity does not change.

Why do applicants fail to complete their applications even as the process becomes easier? Within the administrative burden framework (Herd & Moynihan, 2018), an applicant may pause to learn about a requirement, they may be unable to provide needed documentation, or may feel stigmatized by the respective bureaucratic process. Indeed, any of these burdens can occur whether or not the applicant is paying attention or being distracted. Arguably, navigating these burdens is easier when one is able to pay attention. Our focus here is one specific channel, namely the ease with which attention is drawn away from an unfinished task as digital processes become easier to navigate. While we do not claim it is the only channel through which burdens are altered, it is a plausible and underexamined one in digital administrative settings. We argue that this occurs, paradoxically, because the task environment becomes easier. This dynamic may operate through a sunk cost logic: although the initial difficulty of a process should not affect completion decisions under full rationality, individuals often incorporate prior effort into their choices, such that lower initial costs can increase the likelihood of abandonment (Haita-Falah, 2017).[2]

SNAP is a means-tested program that requires applicants to answer income- and asset-related questions on the online form. Completing this process, which often takes about an hour, requires sustained attention (Kim et al., 2025). This becomes more difficult in environments with frequent distractions, where switching from one task to another is easy. While staying focused online is difficult for everyone, this attention challenge is especially acute for low-income households, who already face cognitive pressure from scarcity (Mullainathan & Shafir, 2013), and for people with health challenges, such as pain, attention disorders, and anxiety (Bell et al., 2023).

As Norman (1999) argues, building on Gibson's (1979) concept of affordances, what matters in design is not what actions are *objectively possible* but what users *perceive to be possible*. When a form moves from paper to an online platform, the perceived affordances of the task environment change. This shift may also alter how applicants expect to move through the process, including whether they expect to finish in one sitting.

To connect these behavioral theories to administrative procedures, we draw on administrative burden theory (Herd & Moynihan, 2018). In this framework, both learning and compliance costs are shaped by task environments. When a task environment is difficult to navigate, learning costs increase. When requirements are difficult to meet, compliance costs increase. Attention is a precondition for these cognitive and physical efforts, and it is a scarce resource, requiring executive functioning (Christensen et al., 2020). This places attention alongside the different cost categories in the administrative burden framework. An applicant may leave an application unfinished because their attention is drawn elsewhere. Attention may also shift during the interval between sessions, making the applicant less likely to return. When the task environment fragments attention in these ways, completing the application becomes more difficult.

We define administrative attention overload as a form of cognitive load that arises when the sustained attentional demands of an administrative process exceed an individual's available cognitive capacity. Reminders can counter this overload by redirecting attention to unfinished tasks and prompting applicants to return to

their online SNAP applications. Prior research shows that reminder-based interventions can increase interview attendance among SNAP applicants, which is critical because failure to complete an interview within 30 days of application results in a procedural denial. For example, Kim et al. (2025) combined flexible interview scheduling with multiple text reminders sent throughout the application process.

A potential alternative explanation is that reminders work through memory rather than just attention. Memory can enter in two distinct ways. The most straightforward one is through forgetting. An applicant may intend to finish their application and forget about it. The reminder works by reactivating their memory about it. A second, more specific, mechanism is the Zeigarnik effect, in which an unfinished task stays active in memory, evokes a sense of incompleteness and thereby pushes individuals toward resolution (Masicampo & Baumeister, 2011; Zeigarnik, 1938). Under this account, a reminder about a partially completed task is more effective than one about a task never attempted, because it draws on the memory of the initial effort and the tension of leaving it undone. These two potential channels are related but different and can operate in isolation from each other. We distinguish them here because they carry different implications for how reminder timing should matter (as discussed below).

This theoretical distinction leads to two testable predictions, though we note that we are not able to sharply separate the underlying mechanisms here. Under an attention account, a reminder reactivates attention to the unfinished task whenever it arrives. This means its effect should turn on upon receipt and a modest difference in timing when it arrives should matter little. A memory account is less clear-cut. If the binding constraint is forgetting or decay, an earlier reminder should help more, because the application is more accessible in memory when the reminder arrives earlier. But the opposite is also plausible under the Zeigarnik account: if the tension associated with an unfinished task strengthens over time, a later reminder may be more effective and may, on net, increase cumulative completion. The two memory mechanisms therefore yield opposing directional predictions. We treat the timing contrast as suggestive evidence for distinguishing the attention account from these memory accounts, but not as a decisive test of which mechanism predominates.

Research Design

Our research proceeded in two steps. We first conducted an observational pilot and then used the findings to design a randomized experiment. The pilot ran in 2024: reminders began in May, and sends were logged from June 14 to August 28. Each day at 11 a.m. local time, a text or email reminder was sent to online SNAP applicants who had started an application the previous day, had consented to receive messages, and had not yet completed their applications. Because data for March through May were unavailable, we compared applicants who met these criteria and started their applications between January 1 and February 23, 2024, with those who started their applications between June 13 and August 15 and were sent a reminder. The latter window ensured that every reminder was logged and every application could be followed for 14 days, within which most applicants completed their applications. Applicants whose applications had stalled for a day had much lower completion rates than applicants overall (approximately 60 percent; see Figure 1). Their completion rate was 17.3 percent (1,253/7,261) without a reminder and 30.0 percent (1,866/6,222) with a reminder, a difference of 12.7 percentage points (95% CI: 11.3–14.2).[3]

Applicants who applied for SNAP online before the reminder intervention were not directly comparable to those who applied afterward. Nonetheless, the observational findings were useful in informing our experimental design. Power calculations based on the pilot indicated that a total sample of 4,500 participants, with 2,250 per arm, would provide 80 percent power to detect a 4-percentage-point increase in submission rates at the 1 percent significance level and a 2-percentage-point increase in approval rates at the 5 percent significance level. [4]

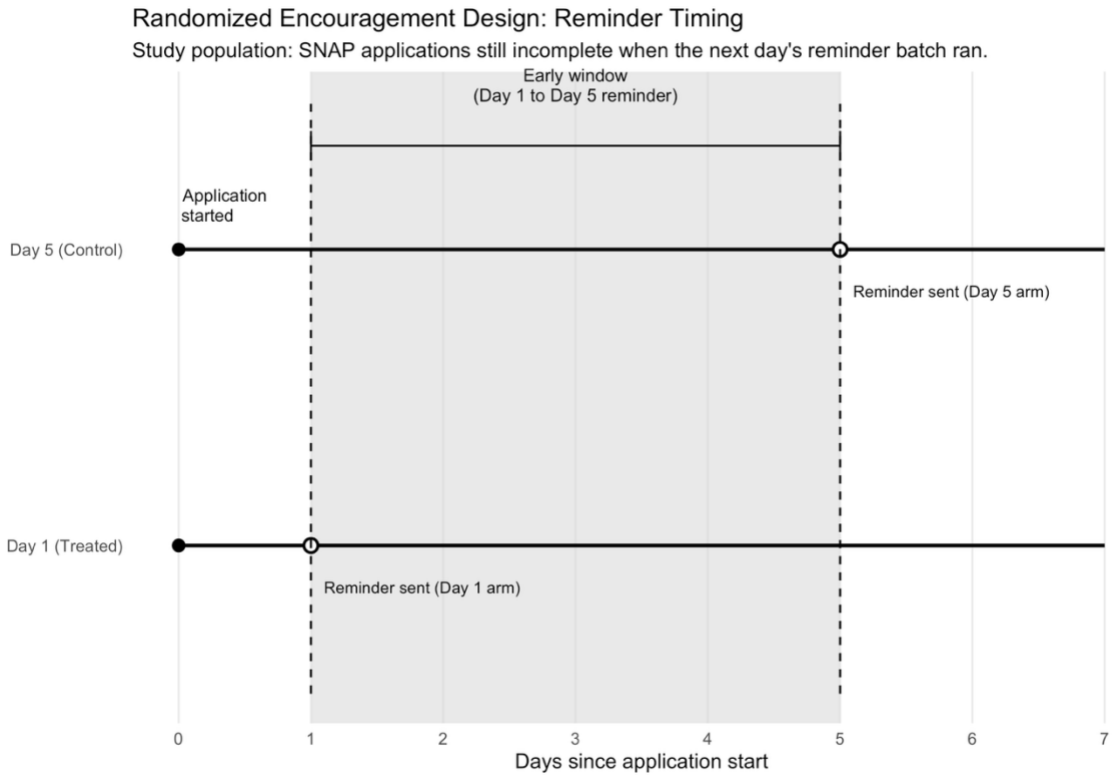
To obtain causal estimates, we implemented a preregistered randomized encouragement design (see Note 1) in which all applicants received reminders but the timing varied. This design allowed us to estimate both the effect of receiving a reminder and the effect of its timing. The online SNAP application platform automatically conducted the randomization. It labeled a case "abandoned" when an individual began a SNAP application but did not complete it by the next day. These abandoned cases constituted the study population and were randomly assigned to one of two reminder-timing arms:

- **Day 1**: reminder scheduled 1 day after abandonment.
- **Day 5**: reminder scheduled 5 days after abandonment.

Our data do not record when an application was abandoned, so we count days from the date the application was started. Reminders were sent in a daily batch: every logged Day 1 reminder was sent one day after the start date, and every logged Day 5 reminder five days after it.

Table B.2 in Appendix B shows that the treatment and control groups were well balanced across the observed applicant characteristics, including language of submitted application, household composition, employment status, and reported income.

Figure 2. Research Design Diagram



Notes: Abandoned SNAP applications are randomly assigned to receive a reminder either 1 day (treated) or 5 days (control) after abandonment. The early window (Days 1–4) is the identification period for the ITT effect, during which only the treated group has received a reminder.

In this study, the "early window" is the period from the Day 1 reminder batch (11 a.m. on the day after the application was started) up to (but not including) the Day 5 reminder batch (11 a.m. on the fifth day after it).[5] In this window, the Day 1 arm has received its reminder, and the Day 5 arm has not. Because the assignment is random, the difference in completion rates between the two arms in this window estimates the effect of receiving the reminder. Accordingly, we label Day 1 as the treated day and Day 5 as the control day. Figure 2 illustrates this research design.

- **Treated**: SNAP applicants with abandoned applications assigned to Day 1 who receive a reminder within the early window.
- **Control**: SNAP applicants with abandoned applications assigned to Day 5 who have not yet received a reminder within the early window.

Based on this design, we ask:

- **H1 (Reminder Effect).** Receiving a reminder prompts abandoned SNAP applicants to complete their applications during the early window.
- **H2 (Timing Effect).** Earlier and delayed reminders produce different cumulative completion rates over the full observation period. The forgetting account predicts higher completion following an earlier reminder, whereas the Zeigarnik account allows for the possibility of higher completion following a later reminder if the tension associated with the unfinished task strengthens over time.

We estimate intention-to-treat (ITT) effects on application completion outcomes: (1) the effect of receiving a reminder in the early window and (2) the effect of reminder timing, comparing Day 1 to Day 5 over the full period.[6] Because we do not observe whether applicants actually read or engage with the reminder, treatment is defined as the assignment to receive a reminder rather than actual exposure. This estimand captures the policy-relevant effect of implementing an earlier reminder strategy in practice, accounting for potential non-compliance or inattention. We estimate this estimand using an ordinary least squares (OLS) linear probability model with heteroskedasticity-consistent standard errors (HC1) (MacKinnon & White, 1985).[7]

Specifically, we estimate the following regression:

$$Y_i = \alpha + \beta_1 D_i + X'_i \beta_2 + \delta_{c(i)} + \varepsilon_i,$$

Y_i is binary and defined in two ways: (1) full-period completion, equal to 1 if the application *i* was ever completed, and (2) early-window completion, equal to 1 if the application *i* was completed during the early window (Days 1–4). D_i is the assignment indicator, equal to 1 for the Day 1 reminder and 0 for the Day 5 reminder. In the early-window analysis, this compares treated (Day 1) to not-yet-treated (Day 5). In the full-period analysis, it compares an earlier reminder to a later reminder. The coefficient β_1 is the ITT effect. X_i is a vector of covariates: Spanish-language application submitted, household size, number of household members over age 59, number under age 18, employment status, self-employment status, net income reported, and gross income reported;[8] [9] β_2 represents their estimated effects. $\delta_{c(i)}$ represents county fixed effects. We present both unadjusted and covariate-adjusted models.

Data

The initial dataset includes 9,744 SNAP applicants who applied between December 19, 2024, and April 6, 2025. As preregistered, we excluded repeat applicants (n = 376; about 4 percent), yielding 9,368 unique applicants. Exact reminder timestamps are missing for 44 percent of applications. Missing reminder timestamps stem from two distinct issues: a scheduler limitation and a truncation of exported reminder logs after March 23, 2025. For these applications, we cannot verify whether and when a reminder was delivered.[10] Table B.1 in Appendix B shows that timestamp availability is largely unrelated to treatment assignment and observed covariates. The exceptions are a small difference in employment status (3 percentage points, $p < 0.01$) and a marginally significant difference in treatment assignment (2 percentage points, $p = 0.07$).

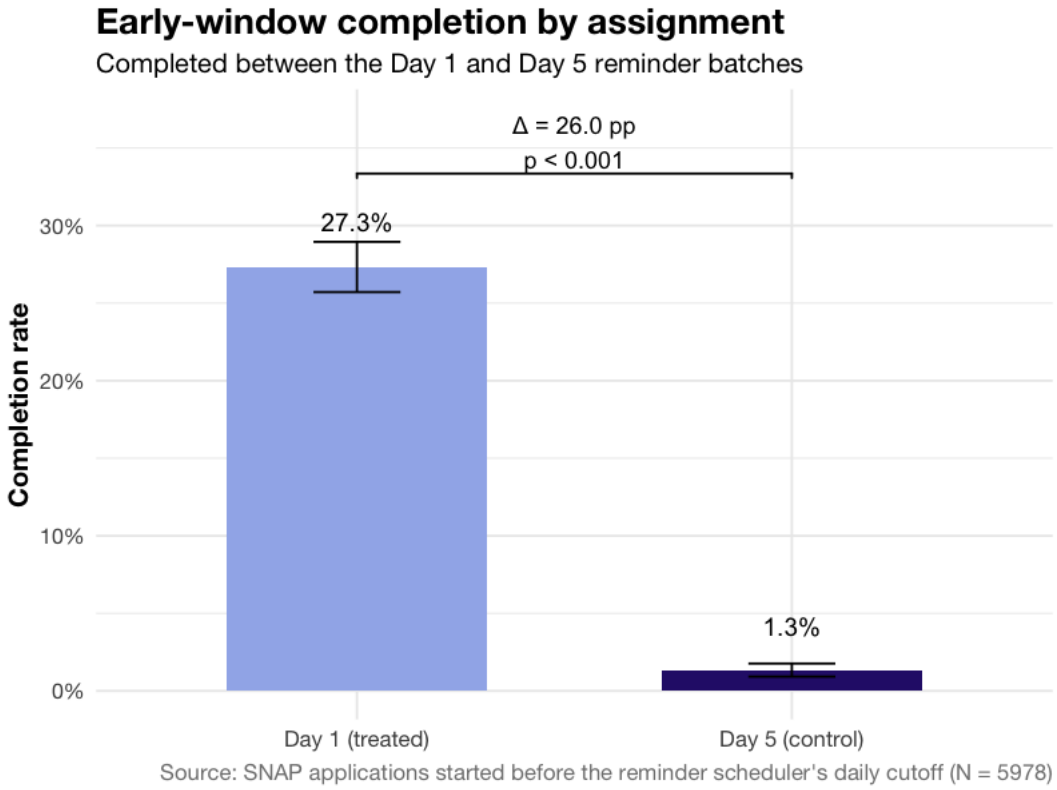
Because the reminder scheduler did not pick up applications started after its daily cutoff, we restrict our analyses to applications started before it. This reduces the sample to 5,978 cases (Day 1: 2,966; Day 5: 3,012). Start time is determined before assignment, so this restriction does not affect the comparison between arms. We use this restricted dataset to estimate the ITT effect.

Tracing cumulative completion over the study period (Figure 4) does not require a reminder record: it counts days from the date each application was started, which is recorded for every applicant, so it uses the full sample of 9,368 applicants.

Results

We first examine whether reminders helped SNAP applicants complete their applications. Figure 3 compares completion rates between applicants assigned to receive a reminder one day after abandonment (Day 1; treatment group) and those assigned to receive a reminder five days after abandonment (Day 5; control group). Bars indicate completion rates within the early window, with error bars representing 95 percent confidence intervals. During the early window, 27.3 percent of applicants in the Day 1 group completed their applications, compared to 1.3 percent in the Day 5 group. This corresponds to a 26 percentage point treatment effect (95% CI: 24.4 to 27.7 percentage points). This result supports H1, which posits that receiving a reminder increases application completion among abandoned cases during the early window. Note that we did not create a pure control group in which no one received a reminder, for ethical reasons. Instead, we leveraged this narrow time window to estimate the causal effect of receiving a reminder.

Figure 3. Early-Window Completion Rates by Reminder Assignment

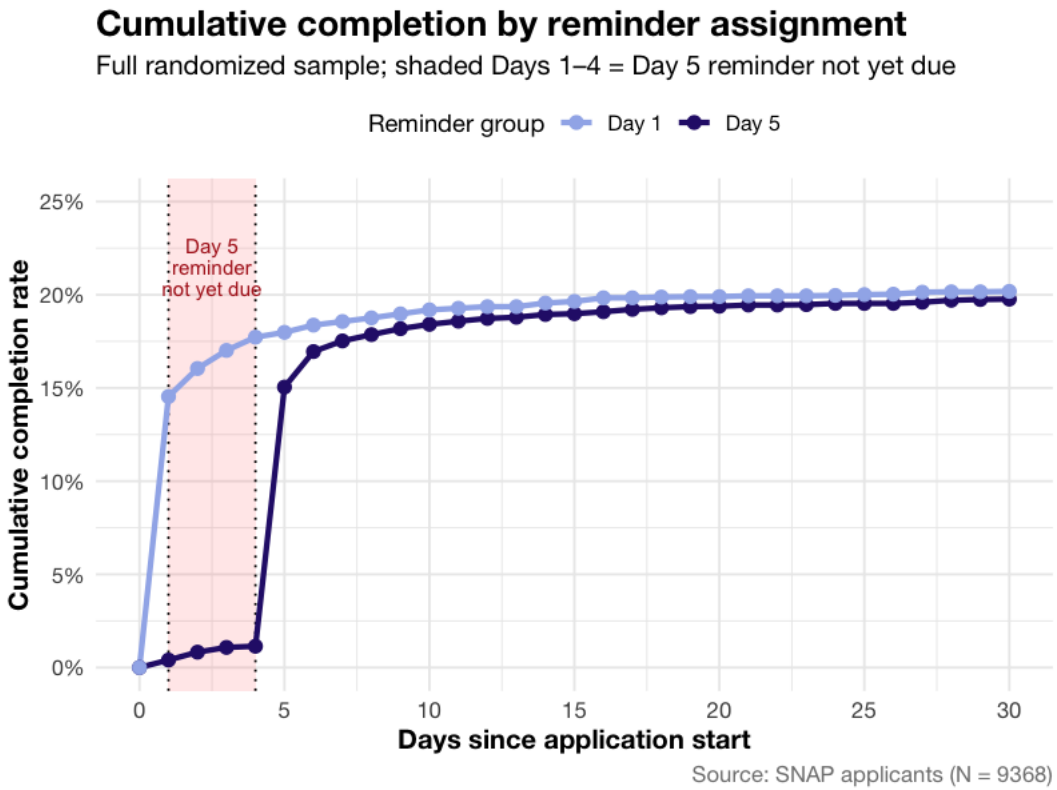


Notes: This bar chart focuses on the early window; It includes the 5,978 applications started before the reminder scheduler's daily cutoff.

Figure 3 shows that reminders increase application completion. We next test whether timing matters, which provides suggestive evidence about whether the reminder operates through attention, forgetting, or the continued activation of an unfinished task. Figure 4 plots cumulative application completion rates by reminder timing. The x-axis shows days since the application was started, and the y-axis shows the cumulative proportion of applicants who completed their applications. The shaded red area marks Days 1–4 after application start, when the Day 1 group's reminder had been sent (treated) and the Day 5 group's reminder was not yet due (control). Lines represent cumulative completion rates for each group.

In Figure 4, the Day 1 line rises quickly over Days 1–4 after application start, reaching 17.7 percent by Day 4. The Day 5 line rises only slightly over the same period, to 1.1 percent, reflecting completion without a reminder among applicants who abandoned their applications. Across all 9,368 randomized applicants, the difference over this shaded window, before the Day 5 group is reminded, is 16.6 percentage points (95% CI: 15.4 to 17.7). Once the Day 5 group's reminder is sent on Day 5, its completion rate increases steeply and catches up with the Day 1 group. From then on, both groups show nearly identical growth rates. The effect of reminder is concentrated in the days before the Day 5 group is reminded. Over the longer horizon, the two groups' completion rates converge. The pattern is consistent with an account in which the reminder works mainly by drawing attention back to the abandoned application. We interpret this evidence as suggestive rather than conclusive because the design does not isolate any single mechanism. The absence of a timing difference is more consistent with the attention account than with either timing-sensitive memory prediction.

Figure 4. Cumulative Application Completion by Reminder Timing



Notes: Figure 4 offers a descriptive view of completion across the randomized population. The figure counts days from the date each application was started (x-axis), traces cumulative completion (y-axis) by reminder arm (lines), and covers all 9,368 randomized applicants. Of these, 4,114 had no reminder record, most of whom likely never received a reminder (5.7 percent completed, compared with 31.6 percent of those with a record). Completion in Figure 4 is therefore lower than early-window completion in Figure 3, which is restricted to applications started before the reminder scheduler's daily cutoff.

Table 1 formalizes the patterns in Figures 3 and 4 by estimating OLS models of the effect of receiving a reminder on application completion. Consistent with H1 (Reminder Effect), applicants reminded one day after abandonment show a large and statistically significant increase of about 26 percentage points in the early window ($p < 0.001$). This early window is critical for identification: before Day 5, only the Day 1 group has received a reminder, while the Day 5 group has not. The difference at this point therefore represents the causal effect of receiving a reminder. A placebo test supports this interpretation: applications started after the

scheduler's daily cutoff never received a reminder, and for these 3,390 applicants the difference over the same window is −0.2 percentage points (95% CI: −0.8 to 0.5). After Day 5, when both groups had received a reminder, they no longer differed in reminder exposure. The full-period outcome shows no significant difference between the two groups, indicating that reminder timing did not matter within the four-day gap tested. Estimating the timing contrast on the full randomized sample of 9,368 applicants, including those the reminder system never reached, yields a similar null result (0.2 percentage points, SE = 0.8, p = 0.79). This result does not support H2 or either directional prediction associated with the two memory mechanisms. Effect sizes and precision are nearly identical between the unadjusted and adjusted specifications for both the reminder effect in models 1 and 2 and the timing effect in models 3 and 4 of Table 1. Table 1 includes county fixed effects; full results are reported in Table D.1 in Appendix D. Application completion rates vary substantially across counties. Because counties administer SNAP applications in this state, differences in local procedures may contribute to this variation. Completion rates range from 9 to 25 percent across counties with at least 100 applications (Figure D.1 in Appendix D). Estimates from the same models without county fixed effects are nearly identical (Table C.1 in Appendix C).

To assess whether treatment effects differed across applicant subgroups, we estimated interactions between treatment assignment and the prespecified baseline covariates, including submitted application language, household composition, employment status, and income measures. Three interactions are statistically significant after a Holm correction for multiple tests (see Table E.1 in Appendix E): the reminder effect was larger for applications submitted in Spanish (10.6 percentage points) and smaller for applicants who reported gross income (−6.1) or net income (−4.9). Because the Day 5 group rarely completed during the early window, these differences mainly reflect which applicants complete after a reminder, and we treat them as exploratory. Appendix F reports completion by the day of the week on which the application was started; the reminder effect was smaller for applications started on Saturday.

Table 1. Reminder and Timing Effects on Application Completion (Linear Probability Model Estimates)

	Early window completion: reminder effect		**Full-period completion: timing effect**	
	(1)	**(2)**	**(3)**	**(4)**
	Unadjusted	**Adjusted**	**Unadjusted**	**Adjusted**
Day 1 reminder	0.259***	0.263***	0.002	0.005
	(0.008)	(0.009)	(0.012)	(0.012)
Baseline covariates	No	Yes	No	Yes
County fixed effects	Yes	Yes	Yes	Yes
N	5,978	5,889	5,978	5,889

Notes: Entries are coefficients from linear probability models and are interpretable as percentage-point changes in the probability of application completion. "Early-window" completion is defined as completion during the early window (Days 1–4). "Full-period" completion is defined as completion at any point after abandonment during the study period. Models 2 and 4 include the prespecified baseline covariates. All models include county fixed effects and HC1 robust standard errors. Standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Discussion and Conclusion

Moving applications online is not unique to SNAP. As of 2024, Medicaid applications are available online in all 50 states plus the District of Columbia and Puerto Rico, Temporary Assistance for Needy Families (TANF) in 46 states, the Child Care Assistance Program (CCAP) in 34 states, and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) in 20 states. Moreover, 35 states now offer three or more programs within a single application to facilitate cross-enrollment (Code for America, 2025).

Online benefit applications offer many advantages over traditional paper-based processes for both clients and caseworkers. From the client's perspective, they save time by eliminating the need to mail applications or visit an office, both of which require time, money, and schedule adjustments. From the caseworker's perspective, online systems can reduce the time required to enter application data into eligibility systems (Urdapilleta et al., 2021).

One potential issue with online benefit applications is that while they are easy to start, they may be harder to finish. The online setting offers frequent opportunities to switch away mid-process. We frame this as a plausible explanation rather than a demonstrated one, however, as our experiment shows that a reminder raises completion, which is consistent with an attention-reactivation account. But it does not isolate the online environment as a source of the original lapse.

Using both observational and experimental data, we show that simple reminders speed up and increase online SNAP application completion rates in the days after abandonment, consistent with the idea that they bring applicants' attention back to their unfinished tasks. In short, they can help realize the promise of a more accessible digital application process. Sending automated reminders by text or email is extremely low cost: texts typically cost less than five cents each (Code for America, 2022), and emails are virtually free. In practice, this can be achieved by embedding an automated workflow, a programmed sequence that detects inactivity and triggers reminders, within the case management system. As such, this intervention is highly cost-effective and can be integrated into routine SNAP application processes or other online benefit systems facing similar challenges.

Nudges are not always highly effective (Damgaard & Gravert, 2018; DellaVigna & Linos, 2022). DellaVigna and Linos (2022, pp. 82–89) analyzed 126 randomized controlled trials conducted by the two largest Nudge Units in the United States. They found an average treatment effect of 1.4 percentage points with a standard error of 0.3. By comparison, the 26-percentage-point difference within four days among applicants who had abandoned their applications (95% CI: 24.4 to 27.7 percentage points) represents a substantial effect.

There are four considerations when interpreting this result. First, the nudges in this study are informational nudges. They notify applicants that they abandoned their cases and may need to return to complete them. In another SNAP study, Kim et al. (2025) found that text reminders about a required eligibility interview, combined with flexible "interview anytime" scheduling options, increased interview attendance by 10 percentage points and reduced procedural denials. Together, these findings suggest that the effectiveness of nudges in improving access to safety net programs depends on the type of information they deliver and how that information alleviates different types of critical administrative burdens (Herd & Moynihan, 2018). For example, a broad and general nudge, such as, "you might be eligible for a program" is likely to produce a smaller effect than a nudge that is embedded in a process already under way. However, our design does not directly compare different types of nudge interventions. Future research could examine whether different forms of nudges produce different effects on reducing administrative burdens and increasing program take-up.

Second, developing strategies to identify which types of information are most useful to safety net applicants requires a careful understanding of the administrative burdens they face. These burdens differ in their salience, especially as the technological context of service delivery continues to evolve. Some burdens, such as compliance costs (for example, completing a required interview), may remain constant whether the application process takes place on paper or online. In contrast, administrative attention overload is becoming increasingly critical as safety net programs are modernized and moved online. Although digitization promises greater convenience, it also introduces new limitations and challenges. We therefore present the mechanistic evidence as suggestive. Our main contribution is the main effect of reminders on completion. The explicitly

cognitive route, whether attention, forgetting, the continued activation of an unfinished task, or another mechanism, is a question our empirical design can inform but not settle.

Third, it is important to note that the approximately 26-percentage-point increase in completed applications does not necessarily translate into a corresponding increase in households receiving SNAP benefits. Kim et al. (2025) report that among online SNAP applicants in Boulder, Colorado, only about half are aware that they must complete an eligibility interview within a limited time window. Without additional support to navigate subsequent steps, such as completing the interview and submitting required verification documents, some applicants who successfully submit their applications may still be denied for procedural reasons. In our observational data, about half of applicants were ultimately approved. The 26-percentage-point gain from a simple reminder is therefore encouraging but reflects an effect at the early stage of the administrative process. Sustained support is needed to help applicants reach the real finish line of obtaining benefits.

Finally, the treatment effect also reflects the conditions under which the intervention was designed. The reminder was developed through in-context analysis of administrative data and in partnership with implementing entities, which allowed the research team to identify a specific moment of uncertainty and target it precisely. This level of background data and analytic capacity is not always present in nudge interventions. The observed effect should therefore be interpreted as the product of both the informational message and the institutional process that generated it.

Notes

1. The study was preregistered on AsPredicted on March 4, 2025. The experiment was implemented on December 10, 2024, but the analysis plan was registered before the authors accessed any data. At the request of the implementing partner, and in accordance with our data-sharing agreement, all information identifying the partner was masked. A copy of the preregistration, with the partner's name and registration identifiers masked, is available at https://osf.io/rxgqm/files/ytxq6
2. Alternatively, paper-based forms may foster greater psychological ownership (De La Rosa et al., 2021), as they are tangible and can be personally held, even in PDF form, whereas online platforms are owned and controlled by external entities. This distinction may reduce engagement over time if digital processes are perceived as diminishing autonomy.
3. The preregistration reports a preliminary estimate from the same pilot, an increase of about eight percentage points rather than the 12.7 reported here. That preliminary analysis behind that estimate had three limitations. It classified applications started June 1–12, 2024, as pre-pilot, although reminders were already being sent but not yet logged. It also counted completions at any point rather than within 14 days of starting, and it selected applications using a 24-hour cutoff rather than the reminder send schedule. The figures reported here correct these issues.
4. The preregistration listed approval and time to approval as outcomes. We do not estimate them because SNAP approval requires income verification, mandatory interviews, and caseworker processing that can take up to 30 days, well beyond the five days after application start that our main analysis examines.
5. According to the application platform's data, among applicants who eventually complete their applications, nearly 90 percent do so within five days of starting. Delaying the second group's reminder until Day 5 therefore leaves it unreminded during the period when most completions occur, which allows us to isolate the effect of receiving a reminder.
6. We estimate an intention-to-treat (ITT) effect because we cannot observe whether applicants read the reminder. As a proxy for exposure, we record whether applicants clicked the link in the reminder. Among applicants with a logged reminder, 49 percent clicked; 63 percent of those who clicked completed their applications, compared with 2 percent of those who did not.
7. Conventional OLS standard errors assume homoscedasticity, meaning equal variance of potential outcomes, which may not hold if the variance of completion differs by treatment status or covariates. HC1 standard errors adjust for this possibility by incorporating residual-based variance estimates, providing consistent inference even under heteroskedasticity.

8. Because roughly 40 percent of applicants have gross income recorded as zero and about 60 percent have net income recorded as zero, there is little variation in the raw amounts. We therefore rely on reported-income indicators, which more clearly capture whether income was reported at all.
9. These income-related variables are correlated but also distinct. Among applicants with a job, 90 percent reported gross income and 61 percent reported net income; among those without a job, 39 percent still reported gross income and 27 percent reported net income. The correlation between reporting gross and net income is 0.68, indicating substantial overlap but not collinearity. This helps explain why gross income reporting is negatively associated with completion, while employment status and net income are not.
10. Missing reminder timestamps have two sources. First, applications submitted after about 4 PM local time (5 PM during daylight saving time), the daily cutoff of the reminder scheduler, were usually not picked up. Figure B.1 shows that the rate of missing reminder records increases sharply for applications submitted after 4 PM. The missing-record rate is 43 percent for the Day 1 group (95% CI: 41.5 to 44.4) and 45 percent for the Day 5 group (95% CI: 43.4 to 46.3). Second, the exported reminder logs were truncated after March 23, 2025. The absence of an exported record after this date does not necessarily mean that a reminder was not sent, but it prevents us from verifying its delivery.

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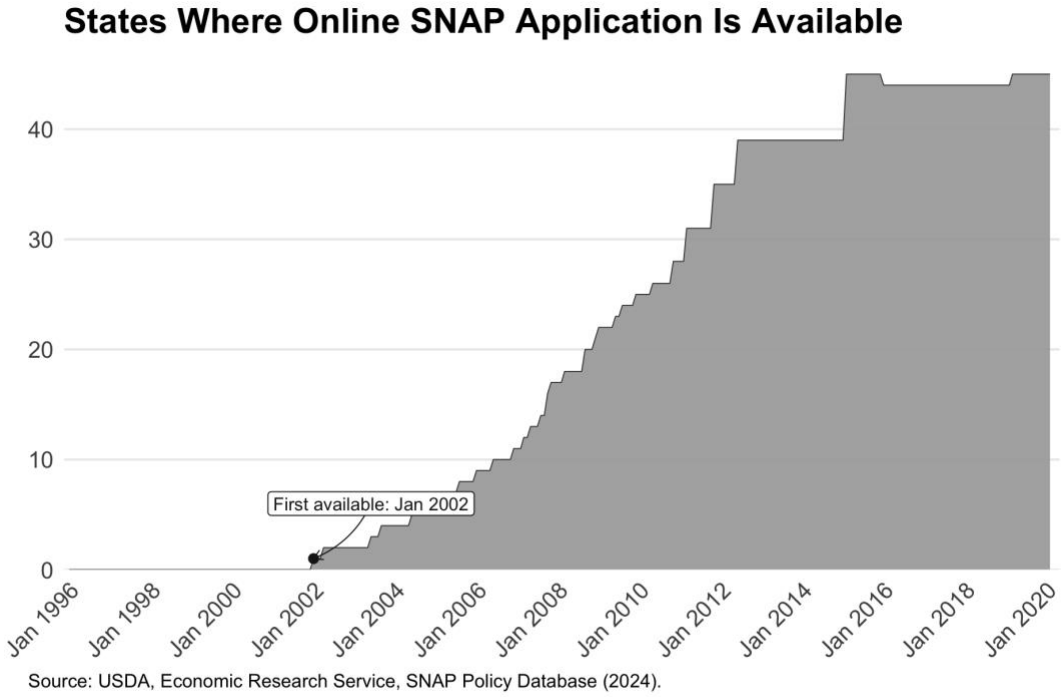
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Appendix

Appendix A. SNAP Online Application Availability Trend

Figure A.1 displays this trend by tracking the number of states that made the SNAP online application available from 1996 through February 2020, based on United States Department of Agriculture data (2024).

Figure A.1. SNAP Online Application Availability Across States (1996–2020)



Appendix B. Covariate Balance by Reminder Timestamp Availability

Table B.1. Covariate Balance: Observed vs Missing Reminder Timestamps

	Missing (N=4114)		Observed (N=5254)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p-value
Assignment status	0.48	0.50	0.50	0.50	0.02	0.07
Submitted application language (Spanish)	0.09	0.28	0.09	0.28	0.00	0.98
Household size	1.67	1.16	1.64	1.13	-0.03	0.24
Over 59 N	0.17	0.45	0.19	0.45	0.01	0.11
Under 18 N	0.33	0.77	0.34	0.78	0.02	0.26
Has job	0.45	0.50	0.43	0.49	-0.03	<0.01
Self employed	0.11	0.32	0.10	0.31	-0.01	0.11
Gross income	1145.87	1653.79	1170.17	1566.60	24.30	0.47
Net income	536.94	1038.83	561.84	1108.30	24.90	0.27

Notes: "Observed" indicates records with a reminder timestamp present; "Missing" indicates no timestamp. The table reports group means, the difference (Observed − Missing), and p-values from Welch two-sample t-tests. Binary variables are coded 0/1, so differences are in percentage points.

Figure B.1. Missing Reminder Log Rate by Hour of Application Submission

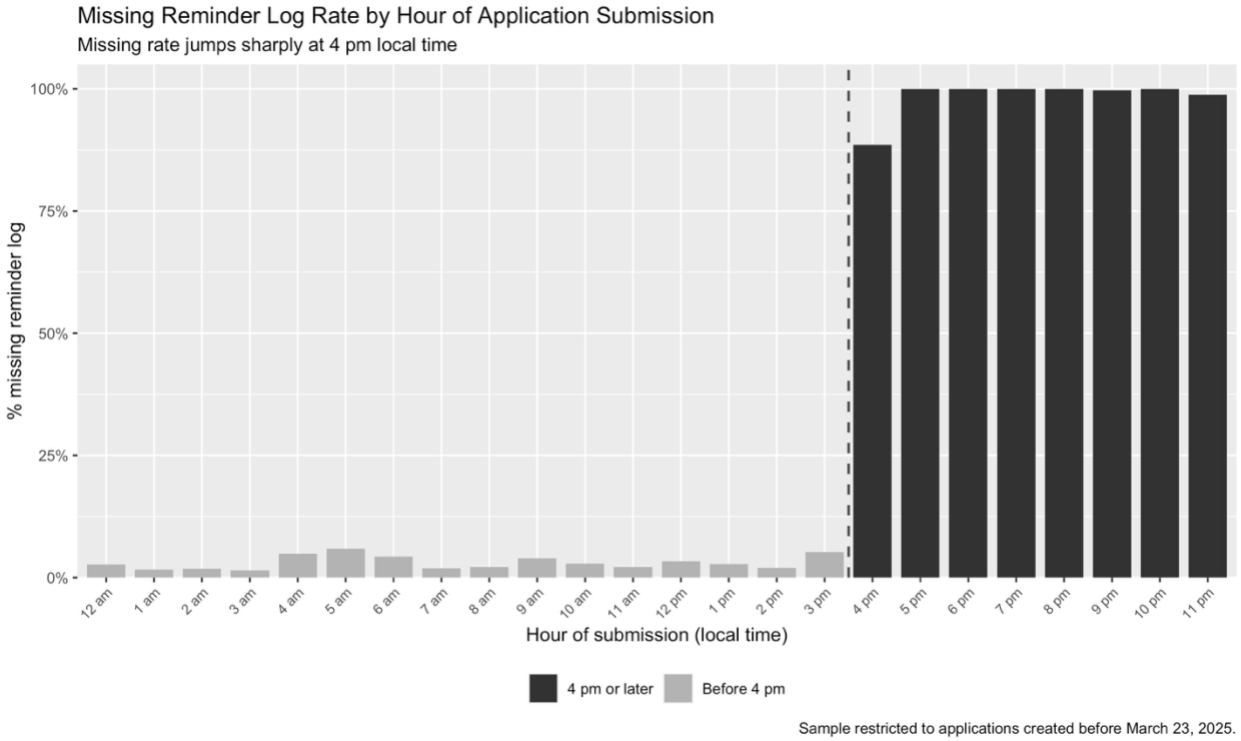


Table B.2. Covariate Balance Across Treatment Arms

	Day 5 (control) (N=3,012)		Day 1 (treated) (N=2,966)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Submitted application language (Spanish)	0.093	0.290	0.094	0.292	0.001	0.884
Household size	1.634	1.127	1.656	1.134	0.023	0.439
Under 18 N	0.340	0.778	0.348	0.791	0.008	0.695
Over 59 N	0.187	0.457	0.192	0.459	0.005	0.699
Has job	0.416	0.493	0.432	0.495	0.016	0.225
Self employed	0.105	0.307	0.105	0.307	0.000	0.975
Gross income	1150.038	1599.727	1184.524	1496.396	34.486	0.392
Net income	541.581	1130.207	572.259	1052.398	30.677	0.281

Notes: The table reports group means, the difference in means (Day5− Day1), and p-values from Welch two-sample t-tests.

Appendix C. Main Regression Results

Table C.1. Reminder and Timing Effects on Application Completion (without County Fixed Effects)

	Unadjusted (early window)	Unadjusted (full-period)	Adjusted (early window)	Adjusted (full-period)
	Reminder	Timing	Reminder	Timing
Day 1 reminder	0.260 [0.244, 0.277]	0.003 [-0.020, 0.026]	0.264 [0.247, 0.281]	0.006 [-0.018, 0.029]
	p = <0.001	p = 0.798	p = <0.001	p = 0.645
Submitted application language (Spanish)			0.045 [0.013, 0.077]	0.097 [0.054, 0.141]
			p = 0.006	p = <0.001
Household size			-0.022 [-0.035, -0.009]	-0.041 [-0.060, -0.022]
			p = <0.001	p = <0.001
Over 59 N			0.006 [-0.013, 0.025]	0.012 [-0.015, 0.040]
			p = 0.542	p = 0.374
Under 18 N			0.046 [0.027, 0.066]	0.073 [0.046, 0.101]
			p = <0.001	p = <0.001
Has job			0.002 [-0.021, 0.025]	-0.022 [-0.054, 0.010]
			p = 0.848	p = 0.188
Self employed			0.020 [-0.010, 0.051]	0.042 [-0.001, 0.085]
			p = 0.191	p = 0.055
Net income reported			-0.009 [-0.032, 0.014]	0.003 [-0.030, 0.035]
			p = 0.436	p = 0.877
Gross income reported			-0.021 [-0.048, -0.006]	-0.041 [-0.078, -0.004]
			p = 0.120	p = 0.029
N	5,978	5,978	5,889	5,889
Standard errors	HC1 robust	HC1 robust	HC1 robust	HC1 robust

Notes: "Full-period" models use a binary outcome equal to 1 if an application was completed at any time after abandonment. "Early window" models use a binary outcome equal to 1 if an application was completed during the early window (Days 1–4). Models do not include county fixed effects and report heteroskedasticity-robust (HC1) standard errors. Sample sizes differ because adjusted specifications exclude cases with missing covariate values.

Appendix D. Variation in SNAP Application Completion Rates by County

This figure plots county-level completion rates among SNAP applicants. Each point represents a county, with point size proportional to the number of applications submitted. Vertical bars show 95 percent confidence intervals around the completion rate estimates. The figure illustrates meaningful variation across counties, with completion rates ranging from 9 to 25 percent, even after restricting to counties with sufficient sample sizes. These rates cover the 24 counties with at least 100 applications (9,019 of the 9,744 applications in the study period, before repeat applicants were excluded)

Figure D.1. Variation in Application Completion Rates by County

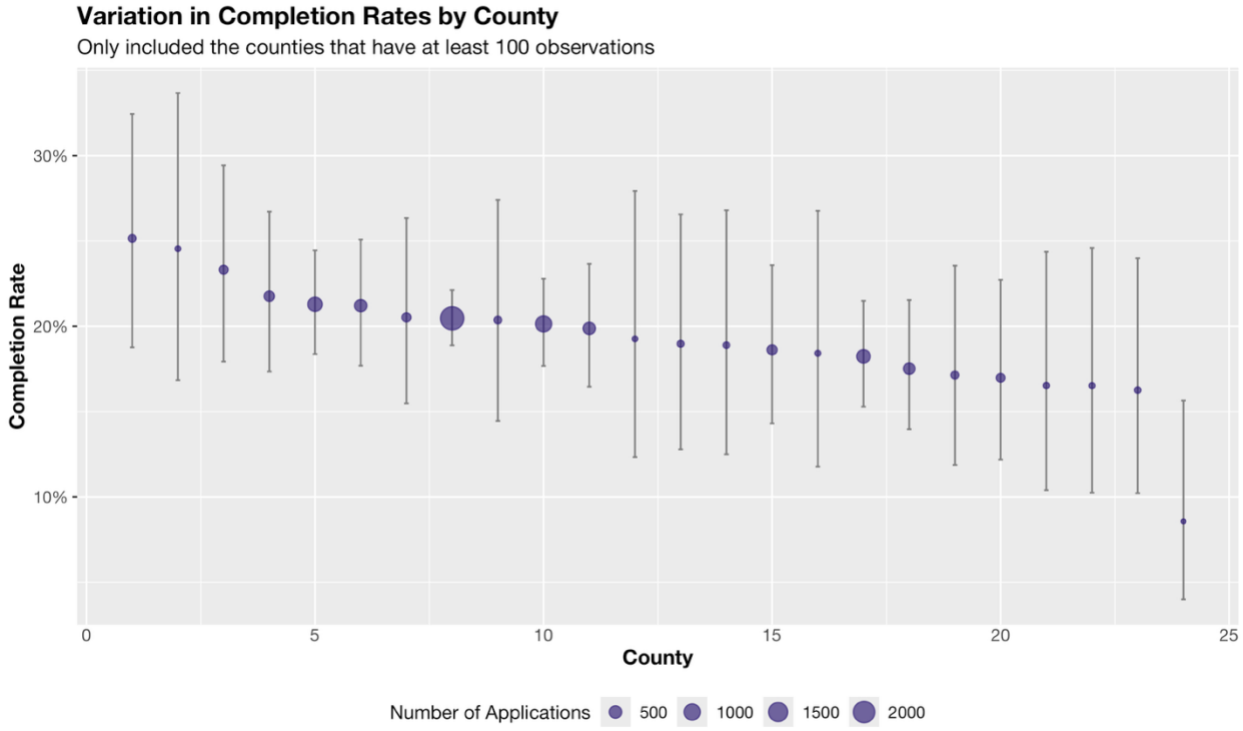


Table D.1. Reminder and Timing Effects on Application Completion (Linear Probability Model Estimates)

	Unadjusted (early window) + County FE	Unadjusted (full-period) + County FE	Adjusted (early window) + County FE	Adjusted (full-period) + County FE
Day 1 reminder	0.259 [0.242, 0.276]	0.002 [-0.021, 0.026]	0.263 [0.246, 0.280]	0.005 [-0.019, 0.029]
	p = <0.001	p = 0.848	p = <0.001	p = 0.682
Submitted application language (Spanish)			0.044 [0.012, 0.076]	0.101 [0.056, 0.146]
			p = 0.007	p = <0.001
Household size			-0.022 [-0.035, -0.008]	-0.042 [-0.061, -0.022]
			p = 0.001	p = <0.001
Over 59 N			0.005 [-0.014, 0.024]	0.012 [-0.016, 0.039]
			p = 0.608	p = 0.407
Under 18 N			0.046 [0.026, 0.065]	0.075 [0.047, 0.103]
			p = <0.001	p = <0.001
Has job			0.002 [-0.022, 0.025]	-0.023 [-0.055, 0.009]
			p = 0.897	p = 0.160
Self employed			0.022 [-0.009, 0.053]	0.043 [0.000, 0.087]
			p = 0.163	p = 0.050
Net income reported			-0.009 [-0.032, 0.014]	0.002 [-0.030, 0.035]
			p = 0.454	p = 0.884
Gross income reported			-0.022 [-0.049, 0.005]	-0.042 [-0.079, -0.005]
			p = 0.105	p = 0.028
N	5978	5978	5889	5889
Standard errors	HC1 robust	HC1 robust	HC1 robust	HC1 robust

Notes: "Full-period" models use a binary outcome equal to 1 if an application was completed at any time after abandonment. "Early window" models use a binary outcome equal to 1 if an application was completed during the early window (Days 1–4). All models include county fixed effects and report heteroskedasticity-robust (HC1) standard errors. Sample sizes differ because adjusted specifications exclude cases with missing covariate values.

Appendix E. Interaction Tests between Treatments and Covariates

The study was designed based on pilot data to detect a 4 percentage point increase in completion at $\alpha = 0.01$ (two-sided) and 80% power with N = 4,500 (2,250 per arm). For an interaction test with a binary moderator split evenly between groups, the treatment effect is estimated separately within each moderator stratum, and the interaction is the difference between those estimates. Achieving the same power for the same effect size therefore requires four times as many participants. Approximately 18,000 participants would be needed to detect a 4 percentage point interaction at 80% power. Our available sample of N = 5,978 implies a minimum detectable interaction of approximately 6 percentage points at 80% power ($\alpha = 0.01$), well below our estimated main effect of 26 percentage points (95% CI: 24.4 to 27.7 percentage points). The remaining null interaction results therefore indicate that any heterogeneity present is smaller than this study can reliably detect, rather than absent.

Table E.1. Interaction Tests between Treatments and Covariates

Outcome	Covariate	N	Interaction (pp)	95% CI low	95% CI high	p-value (interaction)
Early window	Submitted application language (Spanish)	5978	10.60	4.50	16.69	0.00
Early window	Has children	5978	5.49	1.22	9.76	0.01
Early window	Has job	5907	-2.82	-6.19	0.54	0.10
Early window	Self employed	5907	4.34	-1.22	9.90	0.13
Early window	Gross income	5903	-6.14	-9.61	-2.66	0.00
Early window	Net income	5898	-4.90	-8.26	-1.55	0.00
Early window	Household size	5978	0.00	-1.52	1.52	1.00
Early window	Over 59	5978	-1.61	-5.09	1.86	0.36
Full period	Submitted application language (Spanish)	5978	1.82	-6.70	10.34	0.68
Full period	Has children	5978	5.33	-0.54	11.20	0.07
Full period	Has job	5907	2.14	-2.60	6.88	0.38
Full period	Self employed	5907	2.17	-5.60	9.95	0.58
Full period	Gross income	5903	0.63	-4.24	5.51	0.80

Full period	Net income	5898	-1.54	-6.31	3.23	0.53
Full period	Household size	5978	0.09	-2.00	2.19	0.93
Full period	Over 59	5978	0.91	-4.08	5.90	0.72

Appendix F. Outcome Variations by Day of Week

Table F.1 reports completion rates by application start day. Applications were distributed across all seven days, with somewhat fewer on weekends. Saturday applicants had the lowest full-period completion rate (24.6%), while Sunday applicants completed at rates comparable to weekdays. A bivariate OLS model shows that Saturday applicants completed at rates 6.7 percentage points lower overall (95% CI: [−10.4, −3.1]) relative to non-Saturday applicants; the early-window difference is not statistically significant (−1.2; 95% CI: [−4.0, 1.7]).

Table F.2 tests whether the Day 1 treatment effect varies by Saturday application start. The early-window interaction is negative and statistically significant (−6.2 percentage points; 95% CI: [−11.8, −0.6]), while the full-period interaction is not. Because this is a single exploratory test, we interpret it cautiously.

Table F.1. Completion Outcomes by Application Start Day of Week

Day of week	Applications started (N)	Early window completion (%)	Full-period completion (%)
Mon	964	16.6	33.0
Tue	1,060	14.6	33.4
Wed	950	13.5	31.2
Thu	964	13.9	29.8
Fri	883	12.9	29.7
Sat	609	13.1	24.6
Sun	548	14.2	30.3

Notes: Day of week = day the application was started (local time). Early completion = completion on Days 1–4 after the application was started, pooled across both arms. Full-period completion = completion at any point during the study period.

Table F.2. Treatment Effect Heterogeneity by Saturday Application Start

	Early window	Full-period
Day 1 (treated)	0.266 [0.249, 0.284]	-0.002 [-0.026, 0.023]
Saturday application	0.024 [0.004, 0.044]	-0.086 [-0.135, -0.037]
Day 1 x Saturday	-0.062 [-0.118, -0.006]	0.040 [-0.033, 0.113]
N	5,978	5,978

Notes: OLS with HC1 robust SEs. 95% CIs in brackets. Saturday = applications started on Saturday (local time).