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Interventions and Cognitive Spillovers

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Abstract

This paper investigates how incentives and behavioral policy interventions affect individuals’ allocation of scarce cognitive resources. Based on experimental evidence, we demonstrate that incentives systematically influence individuals’ allocation of cognitive resources, and their propensity to actively engage with a decision or to stay passive. Policies that steer individuals’ attention to a specific decision lead to more active decision-making and better choices in the targeted choice domain, but induce negative cognitive spillovers on the quality of choices in other domains. In our setting, these two countervailing effects offset each other, such that the overall payoff consequences of the interventions are essentially zero. We further document that cognitive spillovers are especially pronounced for complex choices and for subgroups of the population with a smaller stock of cognitive resources. We discuss implications for the design and evaluation of behavioral policy interventions.

JEL classification: D91, D01, D04, C91

Keywords: Cognitive resources, attention, spillover effects, policy interventions, nudges, default options, passivity

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1 Introduction

Cognitive resources are fundamental for any economic decision. Making choices requires decision makers to pay attention, process information, and evaluate trade-offs between the available alternatives. A growing body of literature documents that individuals’ resources for carrying out these tasks are inherently limited (see Mullainathan and Shafir 2013, Gabaix 2019, and Mackowiak et al. 2021 for a comprehensive overview of the literature). At the same time, people commonly have to allocate scarce cognitive resources across various tasks or decisions, which they simultaneously face. There is an increasing tendency among academics, firms, and policy makers to intervene in this cognitive resource allocation process. We do not only provide pecuniary incentives to focus individuals’ attention on particular decisions, but also implement a variety of other interventions and ‘nudges’ that may alter people’s allocation of cognitive resources. To promote more deliberate and more active decision-making, and ultimately better choices, we remind people of decisions that are to be taken (Altmann and Traxler 2014, Calzolari and Nardotto 2016, Karlan et al. 2016, Damgaard and Gravert 2018), provide them with information (Kling et al. 2012, Fellner et al. 2013, Bhargava and Manoli 2015, Kaufmann et al. 2018), impose deadlines (Heffetz et al. 2021, Altmann et al. 2021), or urge them to make active decisions (Carroll et al. 2009, Stutzer et al. 2011).

In this paper, we study how incentives and choice-promoting interventions influence individuals’ allocation of scarce cognitive resources and their choices. We focus on three main questions. First, how do relative incentives shape individuals’ allocation of cognitive resources across tasks and their propensity to actively engage with a task? Second, do interventions that steer individuals’ attention to a specific task lead to negative cognitive spillovers on other decisions, with detrimental consequences for individuals’ choices in these other domains? And, third, how do such interventions affect the overall quality of people’s decisions, when accounting for their impact on the targeted as well as non-targeted choice domains?

We study these questions in a controlled laboratory setting. A number of features make our experiment ideally suited towards this end. First, to identify the causal impact of relative incentives on the allocation of cognitive resources, we can vary the incentives for different decisions, while holding all other aspects of the decision environment constant across treatments. Moreover, we can gather detailed information on the amount of cognitive resources underlying individuals’ decisions. Finally, we can assess the effect of choice-promoting interventions not only in the domain that these interventions target, but also in terms of potential cognitive spillovers on other choice domains and their overall payoff consequences.
The design of our experiment is guided by a simple theoretical framework, which builds upon the notion that subjects allocate a fixed stock of cognitive resources efficiently across different tasks. In line with this idea, participants in our experiment work on two cognitively demanding tasks—a “background task” and a “decision task”. As background task, subjects have to memorize and recall 7-digit numbers, following a well-established paradigm to induce cognitive resource scarcity (see, e.g., Carpenter et al. 2013, Deck and Jahedi 2015). As decision task, subjects have to find the correct solution to simple math problems with three available options. If individuals do not actively choose an option in the decision task, a randomly selected default governs their choices. The presence of a default provides a natural opportunity for subjects to abstain from devoting any cognitive resources to the decision task, and focus all their available resources on solving the background task. A key feature of our experiment is that we can readily measure this allocation of cognitive resources. Specifically, in our Baseline environment, participants enter the decision task by pressing a button on the keyboard. If they do not hold the button, they face a blank screen. This feature allows us to track whether subjects attend to the decision task at all, and how much time they dedicate to the task—the amount of visual attention they allocate to the decision task.

In the first treatment dimension of our experiment, we vary the relative incentives for the two tasks. We then examine how the induced differences in incentives affect participants’ allocation of cognitive resources, their propensity to stay passive in the decision task, and the quality of their choices. Our data demonstrate that individuals’ allocation of cognitive resources reacts strongly to changes in relative incentives. An increase in the incentives for the decision task increases the amount of attention that subjects devote to the task, and their propensity to actively engage with the task at all. We further document that the observed shift in cognitive resource allocation is associated with a decrease in subjects’ propensity to stay passive—as measured by a lower default adherence rate—in the decision task and an increase in the quality of subjects’ choices in this task.

In a second treatment dimension, we study the effects of interventions that aim at promoting active decision-making by steering individuals’ attention to a particular decision. Specifically, in the Intervention environment, we direct participants’ attention to the decision task by permanently displaying the task on their screen. We study two variants of this intervention that differ in whether the decision task does or does not feature a default option. Our data show that even these rather subtle interventions achieve their primary goal: they lead subjects to make more active decisions and substantially improve the quality of individuals’ decisions in the targeted domain.

When cognitive resources are scarce, however, steering attention to some choice domain might
also lead subjects to withdraw cognitive resources from other domains, with potentially negative consequences for decisions in the latter. Our experiment has the unique feature that we can directly measure whether such negative cognitive spillovers occur. The results show that the positive effects of the interventions in the targeted domain indeed come at the cost of lower choice quality in the non-targeted domain. Individuals’ likelihood to correctly solve the background task in the Intervention environment lies significantly below the levels observed in the Baseline environment.

We further study the relevance and determinants of this cognitive spillover, using two complementary empirical approaches. First, we conduct an additional online experiment to study whether cognitive spillovers also arise with a (slightly) modified task structure, intervention, and participant population. The data from this experiment again show a strong and systematic spillover. Second, we examine the empirical validity of additional comparative static predictions on how the strength of cognitive spillovers should depend on characteristics of decision makers and the choice environment. In line with these predictions, we find that spillovers are more pronounced for individuals with a smaller “stock” of cognitive resources (as measured by their performance on an IQ test), and in situations in which the decision task is more difficult. We also find that cognitive spillovers tend to be smaller in environments with strong incentives to engage with the decision task. The latter association is, however, measured rather imprecisely. Overall, negative cognitive spillovers are thus especially pronounced when cognitive resource constraints are more likely to be binding, i.e., for complex choices and for subgroups of the population with smaller stocks of cognitive resources.

The presence of cognitive spillovers demonstrates that evaluating interventions solely based on their effects in the targeted choice domain may not suffice to adequately measure the policies’ overall benefits and costs. In our experiment, we can readily assess the net effects of the interventions by comparing participants’ overall payoffs in the Baseline and the Intervention environment. We find that the positive effects of the interventions in the targeted domain and the negative effects due to cognitive spillovers in the non-targeted domain offset each other, such that the interventions’ overall payoff consequences are essentially zero. This finding raises the question to what extent individuals’ allocation of attention in our Baseline setup is already efficient. While we cannot directly calculate the efficient allocation of cognitive resources on an individual level, we can use our theoretical framework in combination with the exogenous variation in our data to derive and test two necessary conditions for the allocation to be efficient. The results from this exercise point to some, albeit limited, deviations from the efficient allocation of cognitive resources in our setting.

Our paper contributes to the literature that studies the economic consequences of limited cog-
nitive resources and attention. The theoretical interest in this topic has surged in recent years, with studies analyzing the determinants of (in)attention (e.g., Kőszegi and Szeidl 2012, Bordalo et al. 2012, 2013, Alonso et al. 2014, Gabaix 2014), optimal attentional choice (e.g., Sims 1998, 2003, Caplin and Dean 2015, Caplin 2016), and the consequences of limited attention and cognitive resource scarcity in a variety of applications (see Gabaix 2019 and Mackowiak et al. 2021 for reviews of the literature). Sharing our interest in cognitive spillovers, a recent paper by Nafziger (2020) also provides a formalization of spillover effects arising from attentional interventions. Compared to the rapid theoretical advancement, direct empirical evidence on how people utilize their attentional and cognitive resources is still relatively scarce. A number of papers have used choice data from laboratory (Caplin and Dean 2013, Dertwinkel-Kalt et al. 2021, Dean and Neligh 2019, Martin 2017, Nielsen et al. 2018) and field experiments (Bartoš et al. 2016, Bronchetti et al. 2020) to test models featuring limited cognitive resources. We complement these approaches with a paradigm in which subjects work on two cognitively demanding tasks. This feature is crucial to establish the existence of cognitive spillovers and to study the determinants of how individuals’ allocate cognitive resources across tasks. Moreover, we enrich our behavioral data with direct measures of how subjects allocate their attention. From a methodological perspective, our paper thus stands in the tradition of studies that use related process-tracing methods, such as Mouselab (e.g., Johnson et al. 1989, Gabaix et al. 2006), eye-tracking (e.g., Wang et al. 2010), or data on search processes (Caplin et al. 2011).

Our results also inform the design and evaluation of behavioral policy interventions. Many of these policies interfere with individuals’ cognitive resource allocation, e.g., by reminding or informing people about specific decisions (Altmann and Traxler 2014, Calzolari and Nardotto 2016, Kaufmann et al. 2018, Tiefenbeck et al. 2018). In doing so, the policies potentially give rise to negative cognitive spillovers. A small number of recent studies document that attention-increasing policy interventions can alter individuals’ behavior in domains that are not directly targeted by the intervention: Medina (2020) finds that SMS reminders to avoid late payment fees may increase overdraft fees, Castro et al. (2020) document that messages to increase tax payers’ compliance with rental income taxes can reduce tax payments for other income types, and Luk-Zilberman (2020) shows that messages encouraging one healthy behavior may lead people to engage less in other health-promoting activities. Our paper complements these studies—which all point to an attention-based rationale for the observed policy spillovers—in three important ways. First, our tightly controlled setup allows us to rule out alternative mechanisms that may lead to policy spillovers (e.g., the social desirability to engage in targeted vs. non-targeted activities). Second, as our setting comprises
exactly two tasks, we can assess the net effect of the interventions on the overall quality of people’s choices in the targeted and non-targeted domain(s). Third, we provide new insights on the determinants of cognitive spillovers and the resulting heterogeneity in the spillover’s strength. In fact, we observe cognitive spillovers to be strongest for individuals whose cognitive resources are limited and for environments that are rather complex—that is, the negative consequences of spillovers are most pronounced precisely in those environments and in those subgroups of the population for which the policies are commonly designed. This insight also has direct implications for the evaluation of behavioral policy interventions: a narrow focus of evaluations on the targeted domain will especially be prone to yield biased results for subgroups that are at the core interest of policy designers.

On a more general level, our findings also contribute to the literature that examines the welfare consequences of nudges and behavioral policy interventions (e.g., Carroll et al. 2009, Allcott and Kessler 2019, Bernheim et al. 2015). A number of earlier studies have warned about possible unintended consequences of behavioral policy interventions, by showing that nudges may impair individual (Caplin and Martin 2016, de Haan and Linde 2018) or social learning about the decision environment (Carlin et al. 2013), or that firms’ strategic responses may limit the effectiveness of policy interventions (Duarte and Hastings 2012). Our finding that a nudge in one decision domain can have negative consequences for the quality of choices in others provides another important rationale for why rigorous evaluations of behavioral policy interventions need to go beyond simple impact assessments on the behavior that is targeted by the intervention.

The observation that cognitive resource scarcity leads people to abstain from taking active decisions also speaks to the literature that studies the widespread phenomenon of passive behavior and its underlying sources (e.g., Madrian and Shea 2001, Handel 2013, Bhargava and Manoli 2015, Heiss et al. 2021). Our setup abstracts from a number of factors that have been discussed as potentially important drivers of passivity and the associated tendency to stick to defaults. These include inertia and procrastination (e.g., Carroll et al. 2009), or pecuniary and non-pecuniary costs of opting out of defaults (e.g., Bernheim et al. 2015). As a consequence, our results cannot speak to the question which force is the strongest driver of default effects in any specific environment. Instead, our approach allows us to study in a tightly controlled setting how cognitive resource allocation responds to incentives and how this, in turn, can affect passivity. In doing so, the evidence from our paper informs the literature that investigates the role of cognitive resource scarcity for passive behavior in the field, and tries to disentangle it from other potential mechanisms (e.g., Chetty et al. 2014, Bhargava and Manoli 2015, Heiss et al. 2021).
The behavioral consequences of cognitive resource scarcity might be aggravated by scarcity of other resources. There is, for instance, an ongoing discussion whether concerns about financial resources, hunger, and other aspects of poverty induce a “tax” on individuals’ cognitive resources or bandwidth (Mullainathan and Shafir 2013, Mani et al. 2013, Carvalho et al. 2016, Sharafi 2018, Shah et al. 2018). When seen through this lens, our results suggest that defaults can yield a double dividend for (financially or cognitively) deprived parts of the population. If institutions are capable of choosing high quality defaults, they do not only mechanically improve outcomes for passive individuals, but they may also “free up” cognitive resources that are sorely needed for other tasks. Conversely, however, aggravated cognitive resource scarcity is also likely to make individuals more susceptible to being exploited by “bad” defaults imposed by parties with misaligned interests, e.g., firms attempting to sell particular pre-configured products.

The remainder of the paper is organized as follows. In the next section, we present the design of our experiment. Section 3 discusses our theoretical framework and derives behavioral predictions. Section 4 presents our empirical results and Section 5 concludes.

2 Design of the Experiment

The goal of our experiment is to investigate how incentives and behavioral policy interventions affect individuals’ allocation of scarce cognitive resources and how this, in turn, affects their choices.\(^1\) For this purpose, we set up a stylized decision environment that captures two common features of situations in which decision makers face cognitive resource scarcity. First, individuals often have to juggle various tasks or decisions simultaneously. For instance, they prepare an important meeting with clients at work, take care of their kids, visit their doctor for a check-up appointment, choose a restaurant for a family dinner, and decide on their next mobile phone contract or health care plan. Each of the tasks requires cognitive resources. Second, many decision environments feature defaults that specify what happens if people do not pay any attention to a task, or do not take an active decision for some other reason (see, e.g., Sunstein 2013).

To capture these features in a laboratory setting, we implemented a simple decision environment in which participants face two tasks—a “background task” and a “decision task”. The background task functions as an abstract representation of the bundle of tasks and choices that a decision maker has to handle, except for the decision task. For the background task, we build on a well-established

\(^1\)In what follows, we describe the design of our main laboratory experiment. To investigate the robustness of our empirical results, we also conducted an additional online experiment. The design and main results of the online experiment will be summarized in Section 4.2; a more detailed discussion can be found in the Online Appendix.
paradigm from cognitive psychology, for which it is straightforward to induce cognitive resource scarcity by choosing an appropriate task difficulty. Specifically, subjects in our experiment had to memorize and recall 7-digit numbers as background task. As we will illustrate below, this digit length makes the task generally solvable for the typical participant in our experiment. At the same time, the task is sufficiently demanding to induce cognitive resource scarcity (see also Carpenter et al. 2013, Deck and Jahedi 2015, Huh et al. 2014, Sprenger et al. 2011). At the beginning of each round of the experiment, a new 7-digit number was displayed for 10 seconds on subjects’ screens. Subsequently, the number disappeared and subjects had to keep it in mind. After another 30 seconds, subjects had to type the memorized number in a field on their screen. If their answer was correct, they earned a positive payoff of €0.40. We made sure that subjects had no opportunity to write down the numbers of the background task. In particular, they had no access to scratch paper and had to hand over their mobile phones for the duration of the experiment.

\[
\begin{align*}
\text{four} & + \text{two} + \text{eight} + \text{four} + \text{one} + \text{six} \\
\text{one} & + \text{three} + \text{two} + \text{eight} + \text{eight} + \text{seven} \\
\text{one} & + \text{two} + \text{four} + \text{three} + \text{five} + \text{eight}
\end{align*}
\]

Figure 1: Example of a decision task.

During the 30 seconds in which they had to keep the number from the background task in mind, subjects additionally faced the decision task. Specifically, subjects were presented with three summations, each of which consisted of six addends. Their task was to choose the option that yielded the highest sum (see Figure 1 for an example). Each option resulted in a sum between 20 and 34. If they chose the correct option, subjects earned a positive payoff, which varied across treatments. The decision task featured a default option that was implemented if subjects did not make an active decision. In particular, in each round, one randomly selected option of the decision task was displayed as the default choice (cp. the middle option in Figure 1). Subjects were informed about the existence of a default and that it was randomly determined which option was the default in a given round of the experiment.

The decision task is a slightly modified version of a task used in earlier experiments by Caplin et al. (2011) and Caplin and Martin (2017). It resembles multi-attribute choices that feature a payoff-maximizing option, the identification of which requires cognitive resources (e.g., finding the cheapest health-care plan for a known expected demand profile; cp. Caplin et al. 2011, Kaufmann et al. 2018). A number of features make the task ideally suited for our purposes: it is simple to
understand, it requires cognitive resources to be solved, and a default can be implemented in a straightforward and natural manner.

### 2.1 Treatments

We study a 4x3 between-subjects design in which we vary relative incentives and the characteristics of the decision environment. The first treatment dimension varies the relative incentives—i.e., relative payoffs and/or costs—for solving the decision task vs. the background task. In all treatment conditions, subjects earned €0.40 for solving the background task correctly in a given round of the experiment. In contrast, the payoffs for correctly solving the decision task varied across treatments between €0.10, €0.20, and €0.40, resulting in a relative-payoff ratio of 1:4, 1:2, and 1:1, respectively. As an additional treatment condition, we implemented a boundary case in which we induced maximal (relative) incentives to allocate cognitive resources to the decision task. For this purpose, we fixed the monetary payoffs for the background task and decision task at €0.40 and €0.10, respectively, but considerably reduced the costs of solving the background task: individuals in the Ample condition had to memorize numbers with only 2 digits. Solving the background task in this condition, thus, requires only trivial amounts of cognitive resources and subjects have ample resources available for the decision task. This makes the Ample condition behaviorally equivalent to a condition with scarce resources and maximal incentives to solve the decision task.²

In the second treatment dimension, we exogenously varied whether the decision task was permanently displayed during the 30 seconds in which subjects kept the number of the background task in mind. In the Baseline environment, subjects faced a blank screen after the number to memorize for the background task had disappeared. To access the decision task, they had to press and hold a key on their keyboard. If they released the key, the task disappeared and subjects returned to the blank screen.³ Subjects were informed about this procedure in advance. This feature of the Baseline environment allows for the possibility that subjects devote no cognitive resources to the decision task, in line with the idea that passivity might be triggered by individuals paying zero attention to certain decisions (see Sunstein 2014, Heiss et al. 2021). The Baseline environment, thus, resembles situations in which there is a default—or, alternatively, some status quo choice that prevails unless a decision maker chooses otherwise—but no policy that encourages the decision maker to actively consider the corresponding decision (e.g., Johnson and Goldstein 2003, DellaV-

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²In the next section, we formally discuss the conditions under which the Ample treatment is behaviorally equivalent to an environment with scarce resources but large relative incentives for the decision task.

³A translated version of all screens can be found in the Online Appendix.
igna and Malmendier 2006). Importantly, the press-and-hold feature allows us to gather detailed data on the precise length of the time spans during which subjects attended to the decision task in Baseline. These attention spans provide us with a measure of the amount of cognitive resources that subjects in Baseline allocate to the task.4

In contrast to the Baseline environment, we displayed the decision task permanently to subjects assigned to treatments with Choice-promoting Interventions (henceforth also denoted as Intervention environment). Specifically, throughout the 30 seconds in which they kept the number from the background task in mind, subjects in the Intervention environment were shown the screen with the decision task. We studied two variants of the intervention, which only differed in whether the decision task did or did not feature a default. For treatments in the Directed Attention (or Directed) environment, the decision task featured a (randomly) preselected default option, while in the Forced Choice (or Forced) environment none of the options was preselected. Hence, subjects in Forced had to actively choose one of the options in the decision task.5

The different variants of the Intervention environment mirror some key features of commonly observed policies, which try to promote active decision-making by directing people’s attention to a particular decision or task (e.g., Altmann and Traxler 2014, Calzolari and Nardotto 2016, Karlan et al. 2016), or by directly forcing them to take an active decision (e.g., Carroll et al. 2009, Stutzer et al. 2011).

To study the interplay between the choice environment and the relative incentives to devote cognitive resources to different tasks, we study the environments with and without choice-promoting interventions across all incentive conditions. This leaves us with a total of 12 different treatment cells (see Table 1 for an overview).

In addition to the exogenous variation of treatment assignment, our experiment also exploits two sources of naturally occurring variation within treatments. In particular, as we will explain in more detail in the next section, treatment differences in our experiment should depend on (i) individuals’ baseline stock of cognitive resources as well as (ii) the difficulty of the decision task.

4Dean et al. (2017) define attention as the “ability to focus on particular pieces of information by engaging in a selection process that allows for further processing of incoming stimuli[..]”. Attention is, thus, one part of cognitive functioning. Solving the decision task in our experiment, however, also involves memory and higher-order cognitive functions. Attention spans therefore only provide a proxy of the total amount of resources allocated to the problem. Yet, as attention is a necessary prerequisite to solve the decision task, it moderates other cognitive resources in the decision process. In particular, devoting zero visual attention to the decision task is analogous to not devoting cognitive resources to the task.

5In 4.7% of cases, subjects in the Forced Choice environment nevertheless did not choose any of the three options in the decision task. As they had not picked the correct solution, subjects’ earnings for the decision task were €0 in these cases.
Table 1: Treatment Overview of the Laboratory Experiment

<table>
<thead>
<tr>
<th>Digits</th>
<th>Payoffs</th>
<th>Baseline</th>
<th>Choice-Promoting Intervention</th>
<th>Directed Attention</th>
<th>Forced Choice</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1)</td>
<td>7</td>
<td>10:40</td>
<td>Baseline-10</td>
<td>Directed-10</td>
<td>Forced-10</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>n = 143</td>
<td>n = 138</td>
</tr>
<tr>
<td>(2)</td>
<td>7</td>
<td>20:40</td>
<td>Baseline-20</td>
<td>Directed-20</td>
<td>Forced-20</td>
</tr>
<tr>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>n = 43</td>
<td>n = 40</td>
</tr>
<tr>
<td>(3)</td>
<td>7</td>
<td>40:40</td>
<td>Baseline-40</td>
<td>Directed-40</td>
<td>Forced-40</td>
</tr>
<tr>
<td></td>
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<td></td>
<td></td>
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<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>n = 18</td>
<td>n = 20</td>
</tr>
<tr>
<td>(4)</td>
<td>2</td>
<td>10:40</td>
<td>Baseline-Ample</td>
<td>Directed-Ample</td>
<td>Forced-Ample</td>
</tr>
<tr>
<td></td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>n = 94</td>
<td>n = 96</td>
</tr>
</tbody>
</table>

Note: The table provides an overview of the different treatments of the laboratory experiment and the number of participants in each treatment. A total of \( n = 892 \) subjects participated in the experiment (resulting in a total of \( N = 20 \times 892 = 17,840 \) subject-round observations in the main part of the experiment). “Digits” denotes the digit length of the numbers that subjects had to memorize as background task. “Payoffs” denotes the monetary payoffs (in Euro cents) that subjects received for correctly solving the decision task and background task, respectively.

To test the corresponding theoretical predictions, we elicited an independent measure of individual-level differences in cognitive ability in the post-experimental questionnaire. Specifically, subjects participated in a 10-item version of Raven’s Progressive Matrices. To study the influence of task difficulty, we rely on differences in difficulty that occur naturally within each subject, given our sampling of options for the task. In particular, correctly solving the decision task becomes harder if the sum of the best option—which needs to be chosen to receive a payoff—is “relatively close” to the results of the other options. To operationalize this idea, we use the difference between the highest sum (i.e., the best option) and the second highest sum as a measure of task difficulty.

2.2 Procedures

Each session of the experiment consisted of four parts. At the beginning of each part of the experiment, subjects received written on-screen instructions explaining the rules and details of the corresponding part.\(^6\) In the first and second part of the experiment, we familiarized subjects with the background task and the decision task, respectively. These parts also provide us with individual-level measures of subjects’ baseline ability to solve the background and decision task, when facing

\(^6\)A translation of the instructions of the experiment can be found in the Online Appendix.
only one of the tasks.

The first part of the experiment consisted of ten rounds in which subjects had to memorize numbers of varying difficulty (but faced no decision task). As in the main experiment, numbers were displayed for 10 seconds and subjects had to keep them in mind for 30 seconds. In the second part of the experiment, subjects worked on the decision task (but no background task) for ten rounds, with each round lasting 30 seconds. The third and main part of the experiment consisted of 20 rounds in which subjects simultaneously faced the background task and the decision task, as described above. In all rounds of the experiment, subjects could never leave a screen by themselves, but were automatically forwarded to the next screen when the time for a given screen had elapsed. Only after the third part of the experiment was concluded, subjects received feedback on their performance in the different parts of the experiment.

The experiment ended with a short post-experimental questionnaire. The questionnaire comprised questions about basic sociodemographic characteristics (including age, gender, field of study, High School GPA and math grades) as well as individuals’ subjective assessments of their decision-making in the experiment and their perceived multitasking ability. In addition, subjects participated in a 10-item version of Raven’s Progressive Matrices as discussed in Section 2.1.

All sessions of the experiment were conducted in the BonnEconLab at the University of Bonn, implemented with Otree (Chen et al. 2016), and the online recruitment system by Bock et al. (2014). On average, sessions lasted 75 minutes. Subjects’ mean earnings in the experiment were €15.76, including a show-up fee of €4. The experiment was carried out in two separate waves. The first wave elicited observations for Baseline-10, Baseline-Ample, Intervention-10, and Intervention-Ample (i.e., the first and last row of Table 1). The second wave comprised the treatments depicted in Rows (1)–(3) of Table 1, i.e., all treatments with a relative-payoff ratio of 1:4, 1:2, 1:1, both for the Baseline and Intervention environment. A total of 892 subjects participated in the experiment, 563 in the first wave and 329 in the second wave. An overview of the number of participants in each treatment cell can be found in Table 1. The reason for the overall lower number of observations for treatments that were conducted only in the second wave and the resulting treatment differences in the number of observations was the closure of laboratory facilities in response to the COVID-19 pandemic.7

All key aspects of the experimental paradigm and procedures were held constant across waves.

7The data elicitation for the second wave started in January 2020. The BonnEconLab, at which all our experiments were conducted, was closed in early March 2020 and effectively remained closed until the date of writing (except for a few small-scale experiments with individual subjects).
In particular, the order and content of tasks in the main part of the experiment (in terms of numbers to be memorized, options in the decision tasks, defaults, etc.) was identical across all subjects, treatments, and waves of the experiment. Moreover, when recruiting subjects for the first and second wave, we imposed identical constraints on the characteristics of the participant pool in terms of the number and type of experiments in which subjects had previously participated. Table O.1 in the Online Appendix summarizes balancing checks showing that these procedures resulted in a well-balanced sample of subjects across all treatment cells depicted in Table 1. Two details, however, differed between the first and second wave of the experiment. First, the exact data on attention spans and data on subjects’ performance in the IQ test was elicited for all subjects participating in the second wave, but only for a 50% subsample of participants in the first wave. Second, the precise tasks and remuneration for measuring background and decision task ability in the first and second part of the experiment differed slightly across waves.

To account for the small wave-specific differences in our empirical analysis, we always present, both, the raw treatment differences as well as specifications that account for subject- and wave-specific characteristics. Note that we can control for potential differences across waves by relying on within-wave variation in treatment assignment for both treatment dimensions. In specifications with additional covariates, we include controls for wave fixed effects and subjects’ ability in the decision and background task (allowing for wave-specific effects of the elicited baseline abilities to account for the differences in the measurement of abilities across waves). Moreover we control for subjects’ age, gender, field of study (STEM, Law, Economics, Others), last math grade in high school (in quartiles), high school GPA (in quartiles; including an additional category for subjects without a high school degree), and perceived multitasking ability, measured in absolute terms (on a Likert Scale) and relative to the overall population (on a scale from 1-100).

3 Behavioral Predictions

Our behavioral predictions are informed by an illustrative theoretical framework, which builds upon a simplified version of the model proposed by Alonso et al. (2014) to analyze resource allocation in...
We base our analysis on the premise that individuals have a limited stock of cognitive resources and optimally allocate these resources across tasks. While both of these assumptions naturally provide a simplistic perspective on individuals’ decision processes in the experiment, they help to structure ideas on how relative incentives and choice-promoting interventions affect the allocation of cognitive resources. When deriving our behavioral predictions, we first focus on the case in which cognitive resources are scarce. At the end of the section, we illustrate how the case of ample resources (i.e., the AMPLE condition in the experiment) can be interpreted as a boundary case of the analysis. We relegate formal derivations and proofs of propositions to Appendix A.

Suppose there are \( n \) individuals. Each individual \( j \in \{1, \ldots, n\} \) is endowed with a fixed stock of cognitive resources \( X^j > 0 \). Individuals face a background task \( B \) and a decision task \( D \) to which they can allocate resources \( x_B, x_D \geq 0 \), such that \( x_B + x_D \leq X^j \). Allocating an amount of resources \( x_B \) to the background task results in a likelihood of \( \pi_B(x_B) \) to correctly solve the task and obtain a payoff of \( u_B \). We assume that \( \pi_B(x_B) \) is strictly increasing, strictly concave, and continuously differentiable. Allocating resources \( x_D \) to the decision task results in a likelihood \( \pi_D(x_D, d) \) to solve the task and obtain a payoff \( u_D \), where \( d \in \{c, inc, no\} \) denotes whether the stipulated default option is correct, incorrect, or nonexistent (as in the FORCED CHOICE environment). With a slight abuse of notation, we denote an individuals’ expected probability to solve the task correctly if the default is specified at random by \( \pi_D(x_D) \equiv \frac{1}{3}\pi_D(x_D, c) + \frac{2}{3}\pi_D(x_D, inc) \). We assume \( \pi_D(x_D) \) to be strictly increasing, differentiable, and strictly concave in \( x_D \). If \( x_D = 0 \), the individual ignores the decision task and automatically follows the default (if there is one). For each individual \( j \) the decision problem is then given by:

\[
\max_{x_B, x_D} u(x_B, x_D) = \pi_B(x_B)u_B + \left(\frac{1}{3}\pi_D(x_D, c) + \frac{2}{3}\pi_D(x_D, inc)\right)u_D
\]

s.t. \( x_B + x_D \leq X^j \)

The optimal allocation of cognitive resources in the BASELINE environment, which we denote by \((x^*_B, Base, x^*_D, Base)\), depends on the shape of \( \pi_D \) and \( \pi_B \), the relative payoffs for the tasks \( (u_D/u_B) \), and the stock of cognitive resources \( X^j \). To derive clear-cut predictions, we impose the following condition:

\(^{10}\)We deviate from Alonso et al. (2014) in two respects. First, we abstract from asymmetric information across different regions of the brain. Second, we impose slightly more structure on payoffs, in line with our experimental setup. The main idea of Alonso et al.’s model is that different cognitive tasks are executed by different systems of neurons. These systems simultaneously demand resources, which are allocated by a central executive system. For more details on the underlying research in neuroscience, we refer the reader to the literature review in Alonso et al. (2014) and Brocas (2012).
Condition 1. For all \( x_D \), the probabilities \( \pi_D \) and \( \pi_B \) satisfy the following conditions:

(i) \( \pi_D(x_D, c) > \pi_D(x_D, inc) \)

(ii) \( \frac{\partial \pi_D(x_D, c)}{\partial x_D} < \frac{\partial \pi_D(x_D, inc)}{\partial x_D} \)

(iii) \( \pi_D(x_D, no) = \frac{1}{3} \pi_D(x_D, c) + \frac{2}{3} \pi_D(x_D, inc) \)

(iv) \( \pi_D'(0) u_D < \pi_B'(0) u_B \)

Condition 1 imposes four restrictions. First, correct defaults catalyze correct choices. Hence, holding \( x_D \) constant, the probability to make a correct choice is higher if the default option is correct than if it is incorrect. Second, there is more to be gained from allocating cognitive resources to the decision task if the default option is incorrect, i.e., if the probability to make a mistake is relatively high. Third, for a given amount of cognitive resources allocated to the decision task, subjects are equally likely to solve the task correctly, irrespective of whether there is a random default or no default. This condition implies that there is no arbitrage opportunity in terms of making better choices if a random default instead of no default is established.\(^{11}\) Fourth, the payoffs for the background task are large enough such that individuals with a minimal stock of cognitive resources rather try to solve the background task than the decision task. This assumption is made to simplify exposition; all of our results qualitatively also hold if the condition is not fulfilled (cf. Appendix A.2). In our experiment, the assumption should be fulfilled in most conditions, as subjects receive relatively high rewards for solving the background task (cp. Section 2).

Individuals in the Baseline environment face a central trade-off when allocating cognitive resources across tasks: dedicating more resources to the background task increases the probability of solving this task, but comes at the cost of allocating fewer resources to the decision task, with resulting negative consequences for the probability of solving the latter. The optimal solution to this trade-off will naturally depend on the overall stock of cognitive resources available to an individual and on the relative incentives to solve the two tasks. Subjects with a relatively small stock of cognitive resources will find it profitable to ignore the decision task and instead allocate all their available resources to the background task. Formally, there exists a threshold \( \bar{X} > 0 \) such that all subjects with \( X^j \leq \bar{X} \) allocate all their resources to the background task. For subjects with \( X^j > \bar{X} \), in turn, it will be optimal to devote a strictly positive amount of resources to both tasks.

\(^{11}\)While this assumption is plausible in our setting, it is not completely innocuous when considering default effects more generally. In particular, the assumption rules out the possibility that random defaults decrease choice quality compared to a corresponding no-default scenario. The latter could, however, be relevant in settings in which defaults are sticky because decision makers suffer from an omission/commission bias or overestimate the informativeness of (random) defaults (see, e.g., Altmann et al. 2020; for a comprehensive overview of potential mechanisms behind default effects, see also Sunstein 2013).
With increasing relative incentives to solve the decision task (i.e., an increase in \( u_D/u_B \)), individuals will increase the amount of resources dedicated to this task. Therefore, fewer subjects will ignore the task entirely and \( \bar{X} \) decreases. Overall, the cumulative distribution of cognitive resources allocated to the decision task in a treatment with relatively larger incentives for the decision task should thus first-order stochastically dominate the corresponding distribution in treatments with relatively lower incentives for the decision task.

The increase in cognitive resources devoted to the decision task in response to higher relative incentives for the task directly implies that the quality of choices in the decision task increases. Furthermore, the shift in cognitive resources also reduces individuals’ passivity regarding the task. This is the case for two reasons. First, fewer individuals automatically stick to the default as a consequence of paying no attention to the decision task at all. Second, individuals who increase cognitive resources are less likely to make a wrong decision and (mistakenly) follow an incorrectly specified default. The latter reasoning holds as long as individuals are more likely to follow an ill-specified default than another wrong option. Formally, the probability \( \rho \) to follow the default conditional on the default being incorrect and the subject choosing either the default or the other incorrect option needs to be higher than \( \frac{1}{2} \).

The following proposition summarizes the consequences of changes in the relative incentives for the two tasks.

**Proposition 1.** Suppose that Condition 1 holds. Then for all \( X^j \) it holds that (i) \( \frac{dx^*_D,Base}{d(u_D/u_B)} \geq 0 \), (ii) \( \frac{dx^*_D,Base}{d(u_D/u_B)} \geq 0 \), (iii) the likelihood to follow the default weakly decreases in \( u_D/u_B \) if \( \rho \geq \frac{1}{2} \).

Our framework also provides a natural setting to examine how the Intervention environment affects the allocation of cognitive resources and individuals’ resulting decisions. We assume that, as a result of the interventions, individuals have to devote at least some positive amount of resources, \( x_I \), to the decision task with \( 0 < x_I < X^j \forall j \), where the amount \( x_I \) may depend on the variant of the intervention. This assumption is crucial for our subsequent results. If, instead, \( x_I = 0 \), we would expect no behavioral differences between the Baseline and Intervention environment. Let \( (x^*_{D,Inter}, x^*_{B,Inter}) \) denote the optimal solution to the maximization problem with the additional constraint \( x_D \geq x_I \). The additional constraint imposed by the interventions will be binding for subjects who would otherwise allocate no or only few cognitive resources to the decision task, i.e., subjects with a relatively small stock of cognitive resources. Relative to Baseline, these subjects will increase the amount of cognitive resources devoted to the decision task \( (x^*_D,Inter > x^*_D,Base) \).

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12 For simplicity we assume that \( \rho \) is constant in \( x_D \). Similar results can be derived in a framework where this probability depends on the amount of cognitive resources allocated to the decision task.
yielding a higher probability to solve the task correctly. Following the same arguments as above, we should, therefore, also observe lower rates of passivity in the Intervention conditions compared to Baseline. Nevertheless, the defaults in Directed will attract choices at a higher-than-random frequency. As a result, we expect subjects in Directed to follow the default more often than they choose the corresponding choice alternative in Forced.

**Proposition 2.** Suppose that Condition 1 holds and let \( \rho \geq \frac{1}{2} \). Then for all \( X^j \), \( \pi_D(x_{D,\text{Inter}}^*) \geq \pi_D(x_{D,\text{Base}}^*) \), and the likelihood to follow the default is weakly lower in the Intervention environment than in Baseline.

Our framework also lends itself to examining the consequences of choice-promoting interventions for individuals’ decisions in the background task. Since it is optimal for subjects to exhaust their available cognitive resources, the exogenous reallocation of resources to the decision task induced by the interventions forces individuals to withdraw scarce cognitive resources from the background task. As a consequence of this cognitive spillover, we expect decision quality in the background task to be lower in the Intervention environment than in Baseline.

Importantly, the size of the negative cognitive spillover is not homogeneous across the population, and also depends on the economic characteristics of the choice environment. Recall that the constraint introduced by the interventions (\( x_D \geq x_I \)) will only be binding for individuals, who—in the absence of an intervention—would allocate rather small amounts of resources to the decision task. Hence, cognitive spillovers should be more pronounced among individuals with a relatively small stock of cognitive resources, and in situations in which the relative incentives to allocate resources to the decision task in Baseline are small. The latter is the case if the (relative) payoff for the decision task is small, or if the difficulty of the decision task is relatively high. To conceptualize task difficulty in our framework, we represent it by parameter \( \phi \in \mathbb{R} \) and assume that the probability of correctly solving the decision task is given by \( \pi_D(x_D, \phi) \) with \( \frac{\partial^2 \pi_D(x_D, \phi)}{\partial \phi^2} < 0 \) \( \forall x_D, \phi \).

Hence, the task difficulty affects the slope of \( \pi_D \) with respect to \( x_D \). If task difficulty \( \phi \) increases, this slope decreases, which means that a marginal increase in cognitive resources devoted to the decision task is less effective in solving this task. Overall, the size of the cognitive spillover, which is given by \( \Delta \pi_B = \pi_B(x_{B,\text{Inter}}^*) - \pi_B(x_{B,\text{Base}}^*) \), exhibits the following patterns.

**Proposition 3.** Suppose that Condition 1 holds. Then, for all \( X^j \), there is a negative cognitive spillover \( \Delta \pi_B \leq 0 \) with the following properties: (i) \( \frac{d \Delta \pi_B}{d X^j} \geq 0 \), (ii) \( \frac{d \Delta \pi_B}{d \phi} \leq 0 \), (iii) \( \frac{d \Delta \pi_B}{d (u_D/u_B)} \geq 0 \).

The positive effects of the interventions on choice quality and payoffs in the decision task (Proposition 2) and the negative cognitive spillovers (Proposition 3) have countervailing effects on indi-
individuals’ overall payoffs. As the decision problem of subjects in the Intervention environment is a constrained version of the one in Baseline, the latter effect should dominate and average total payoffs should be weakly higher in Baseline than in the Intervention environment. This reasoning holds as long as the allocation of cognitive resources in Baseline is efficient. The comparison of overall payoffs across conditions, therefore, also allows for an indirect test of the efficiency of cognitive resource allocation in Baseline. Building on this intuition, we can derive and test a necessary condition for the efficiency of subjects’ allocation of cognitive resources (see Section 4.3 and Appendix A.5).

So far, our analysis in this section focused on the case in which cognitive resources are scarce. However, as asserted above, the Ample condition in our experiment can be considered as a boundary case of this analysis. As discussed in Section 2, the design of the Ample condition rests on the idea that memorizing two-digit numbers requires only trivial amounts of cognitive resources. This idea can be conceptualized by setting $\pi_B(x_B) = 1$ for all $x_B \geq 0$. Individuals can thus solve the background task even with minimal cognitive resources and, consequently, they should dedicate all available resources to the decision task. The condition with ample cognitive resources is thus behaviorally equivalent to a situation in which the relative incentives to allocate cognitive resources to the decision task are large. We can thus use the Baseline-Ample condition as a limit case when testing Proposition 1. As subjects in this condition should dedicate all their resources to the decision task, however, the constraint introduced by the interventions will not be binding in the Ample condition. Therefore, there should be no effect of choice-promoting interventions, and behavior in Baseline-Ample, Directed-Ample, and Forced-Ample should be equivalent (cp. Appendix A.4).

To summarize, our framework provides four main predictions for subjects’ behavior in the experiment. (1) An increase in the relative incentives for the decision task should lead to a monotone increase in the amount of cognitive resources allocated to this task, a monotone increase in choice quality, and a monotone decrease in default adherence in the decision task. With respect to our second treatment dimension, we predict (2) that choice-promoting interventions reduce passivity and improve performance in the decision task. (3) The interventions lead to negative cognitive spillovers

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13 The data from our experiment allow for a straightforward test of whether subjects indeed do so. Specifically, we can compare the frequency at which subjects in Baseline-Ample correctly solve the decision task in the main part of the experiment (83.6%) to the corresponding number in the second phase of the experiment (86%), in which they work on the decision task, but face no background task. The difference between the two frequencies is small and not statistically significant (Wilcoxon signed-rank test, $p = 0.308$), lending support to the notion that the background task in Baseline-Ample requires no cognitive resources. Furthermore, in roughly 98% of cases, subjects in Baseline-Ample also solve the background task correctly.
in the background task; these spillovers are aggravated by a lower stock of cognitive resources, a more difficult decision task, or lower relative payoffs for the decision task. (4) The overall payoff consequences of the interventions are weakly negative. The discussion of our empirical results in the following sections will be structured according to these main predictions.

4 Results

In our empirical analysis, we first study how relative incentives affect the allocation of cognitive resources and individuals’ choices, by comparing behavior across the different treatments in the Baseline environment (the first column of Table 1). In a second step, we analyze differences in behavior between the Baseline and Intervention environment, focusing especially on the question whether interventions that steer attention to a particular task induce negative cognitive spillovers in other choice domains. We conclude the section by discussing overall payoff consequences of the interventions and the efficiency of cognitive resource allocation.

4.1 Cognitive Resource Allocation and Passivity

The different treatments in the Baseline environment allow us to directly test how relative incentives shape individuals’ allocation of cognitive resources across tasks. As our main measure of cognitive resource allocation, we examine how much visual attention subjects devote to the decision task—i.e., the total number of seconds that a subject dedicates to the decision task in a given round of the experiment. Comparing this outcome in Baseline-10, Baseline-20, and Baseline-40 shows how subjects re-allocate their attention when the relative payoffs for solving the decision task vs. the background task increase from 1:4 over 1:2 to 1:1. By analyzing attention spans in Baseline-Ample, we can further assess how individuals behave in a boundary condition in which the relative incentives to devote cognitive resources to the decision task are maximal.

Figure 2 depicts the cumulative distributions of attention spans devoted to the decision task across treatments. The empirically observed attention distributions are ranked in a first-order stochastic dominance sense according to the relative incentives for the decision task. The CDF in Baseline-Ample first-order stochastically dominates the CDF in Baseline-40, which itself dominates the one in Baseline-20, which in turn dominates the CDF in Baseline-10. In

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14Recall that this measure is only available for a subset of participants in the Baseline environment (cf. Section 2.2). Throughout Section 4.1, all figures and tests that require detailed attention data are, therefore, based on n=203 subjects (corresponding to N=4,060 subject-round observations). All remaining tests are based on the full sample of n=298 participants in the Baseline environment (N=5,960 subject-round observations).

15Stochastic dominance is also observed when considering the CDFs of subject-level averages instead of decision-level data (see Figure O.1 in the Online Appendix).
Figure 2: Attention Spans in Baseline

Note: The figure depicts cumulative distribution functions of the amount of attention devoted to the decision task for different treatments in the Baseline environment (based on measures of attention spans at the subject-round level).

In line with the pronounced visual differences, the distributions of attention spans differ significantly across treatments (Kolmogorov-Smirnov test, $p < 0.001$ for all pairwise treatment comparisons). The monotone relationship between the relative incentives for the decision task and the amount of attention devoted to the task is also reflected in the average length of attention spans. On average, participants in Baseline-10 enter the decision task for 14.1 seconds in each round of the experiment. This value increases to 15.9, 18.4, and 25.8 in Baseline-20, Baseline-40, and Baseline-Ample, respectively. The positive relationship between the relative incentives and the amount of attention devoted to the decision task is statistically significant (Spearman’s $\rho = 0.463$, $p < 0.001$). Higher relative incentives for the decision task, thus, cause subjects to devote more attention to this task, in line with the first part of Proposition 1.

When focusing on the left-most part of Figure 2, it is evident that the increase in relative incentives for the decision task also induces a shift in subjects’ propensity to devote no cognitive resources

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$^{16}$The p-value is based on a two-sided test of the relationship between relative payoffs and individual-level attention spans (averaged across the 20 rounds of the experiment). Regression results on pairwise treatment comparisons of attention spans, including specifications with wave fixed effects and further controls, can be found in Table B.1 in the appendix. While not all of the differences between “neighboring” treatments are statistically significant (e.g., when comparing Baseline-10 and Baseline-20), stronger differences in the experimentally induced incentives also lead to economically and statistically significant differences in attention allocation (e.g., the p-values of pairwise tests between Baseline-Ample and the remaining treatment cells all lie below 0.01).
to the task at all. Subjects completely ignore the decision task in 30.7% of cases in BASELINE-10, whereas they do so in only 24.8% of cases in BASELINE-20, 11.7% of cases in BASELINE-40, and 2.5% of cases in BASELINE-Ample. The decrease in subjects’ propensity to ignore the decision task under higher relative payoffs is statistically significant (Spearman’s ρ = 0.425, p < 0.001; see Appendix Table B.1 for further estimations with pairwise treatment comparisons).

Our data also show that the allocation of cognitive resources differs systematically across subgroups of participants. In line with our theoretical framework, we observe that the propensity to ignore the decision task is particularly high for subjects with a relatively small “stock” of cognitive resources. As a proxy for participants’ stock of cognitive resources, we use their performance in a 10-item version of Raven’s Progressive Matrices (see Section 2). Comparing treatment differences in the CDFs for subgroups with above- and below-median test scores shows that the extensive-margin reaction in response to a change in relative incentives is almost exclusively driven by subjects with below-median test scores (see Figure B.1 in the appendix).

Finally, in line with the second part of Proposition 1, the increase in attention devoted to the decision task under higher relative incentives also leads to better performance in this task. On average, the rate at which subjects correctly solve the decision task increases from 58.3% in BASELINE-10 to 61.4%, 68.9%, and 83.6% in BASELINE-20, BASELINE-40, and BASELINE-Ample, respectively (Spearman’s ρ = 0.496, p < 0.001; see Appendix Table B.1 for further estimations with pairwise treatment comparisons).

**Passive Behavior**

In a next step, we analyze how the observed shift in cognitive resource allocation affects participants’ propensity to stay passive in the decision task. As measure of passivity, we consider the rate at which subjects stick to the default option stipulated in the decision task. Panel (a) of Figure 3 compares default adherence rates across treatments in the BASELINE environment. In line with the third part of Proposition 1, the figure shows a negative relationship between the relative incentives for the decision task and the average rate at which subjects stick to the stipulated default option for the task (Spearman’s ρ = −0.439, p < 0.001; see Appendix Table B.2 for further estimations with pairwise treatment comparisons). Going from the treatment with the highest relative incentive to solve the decision task to the treatment with the lowest incentive, the default adherence rate almost

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17 The reported numbers are based on the full sample of the BASELINE environment and therefore differ slightly from the values in Figure 2 (recall that for a subset of participants, the detailed attention spans depicted in the figure were not recorded; cf. Section 2.2).
Figure 3: Passive Behavior in BASELINE

(a) Overall default adherence

(b) Default adherence conditional on default quality

Note: Panel (a) depicts average default adherence rates for the different treatments in the BASELINE environment. Panel (b) depicts treatment differences in default adherence rates separately for situations in which the stipulated default option was incorrect / correct. The figure depicts raw means. Corresponding estimation results are reported in Appendix Table B.2.

doubles (31.8% in BASELINE-Ample and 59.3% in BASELINE-10).\(^{18}\)

Panel (b) of Figure 3 further shows that treatment differences in default adherence predominantly arise in situations in which sticking to the default is a “bad” choice. Specifically, the figure depicts default adherence rates across treatments separately for situations in which the (randomly determined) default option did versus did not coincide with the correct solution to the decision task. If the default option corresponds to the correct solution, we find only small and insignificant differences in default adherence rates (Spearman’s $\rho = -0.071$, $p = 0.219$). In contrast, we observe a strong divergence in passive behavior if the stipulated default option is incorrect. In this case, subjects follow the default in 47.9% of cases in BASELINE-10 but only in 42.2%, 23.8%, and 9.4% in BASELINE-20, BASELINE-40, and BASELINE-Ample, respectively (Spearman’s $\rho = -0.528$, $p < 0.001$). This finding is a further indication that the differences in passivity are indeed driven by a reduction of cognitive resources devoted to the decision task.\(^{19}\)

Note that the randomly determined default option ended up being correct in 6 out of 20 rounds, i.e., in 30% of cases. Note further that subjects in our setting might either “passively” stick to a default as a result of paying little or no attention to the task, or they might “actively” decide to follow the default option after devoting a high amount of cognitive resources to solve the task. While we cannot fully differentiate between the two types of default adherence, the treatment differences in subjects’ propensity to completely ignore the decision task underlines the importance of passivity for the observed differences in default adherence (cp. Figure 2).

\(^{18}\)In Appendix A.3, we discuss in more detail why—according to our theoretical framework—one would also expect the effect of relative incentives on default adherence to be stronger for incorrect defaults.

\(^{19}\)
particularly strong for subjects with a relatively small stock of cognitive resources. Regression results are provided in Table B.2 in the appendix.

In sum, our findings on how relative incentives shape individuals’ allocation of cognitive resources, their propensity to stay passive, and the resulting quality of their choices are all in line with Proposition 1 of our theoretical framework.

**Result 1.** *An increase in relative incentives for the decision task leads to an increase in the amount of attention allocated to the task, a decrease in default adherence rates, and an increase in the quality of subjects’ choices in the decision task.*

### 4.2 Interventions and Cognitive Spillovers

The findings presented so far illustrate the interplay between relative incentives, cognitive resource allocation, and passive behavior. In a next step, we study how interventions that aim at promoting active decision-making by steering people’s attention to a particular task affect individuals’ choices and their allocation of cognitive resources. Given our findings discussed so far, two important questions emerge with respect to such interventions. First, do the interventions improve the quality of choices in the targeted domain? Second, if the interventions succeed in steering cognitive resources to the targeted choice domain, do they also reduce the resources devoted to other decisions and thereby impair the quality of choices in these other domains?

These questions are especially pertinent for the case in which cognitive resources are scarce and, thus, the cognitive resource constraint is binding. We will therefore focus our discussion on the set of treatments in which subjects face scarce cognitive resources. In contrast, in environments without scarcity in which individuals can devote ample amounts of resources to the decision task even in the absence of an intervention, we would expect interventions that steer attention to this task not to matter (cp. Section 3).  

To examine the consequences of choice-promoting interventions, we vary the choice environment such that—in contrast to the **Baseline** environment—the decision task is permanently displayed to subjects. We study two variants of the **Intervention** environment, which only differ in whether the decision task does (**Directed Attention** environment) or does not feature a default (**Forced Choice** environment). For all outcomes of interest, we start by analyzing the overall impact of the interventions, and then consider to what extent the two variants of the intervention differ in their implications.

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20We summarize the results of the **Intervention** treatments in the **Ample** environment in the Online Appendix.
## Table 2: Behavior in the Targeted Choice Domain

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<th></th>
<th>Panel (a): Choice Quality</th>
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<th>Panel (b): Default Adherence</th>
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Note: The table reports results of OLS regressions of treatment differences in choice quality and default adherence in the decision task. The dependent variable in Columns (1)–(4) is an indicator equal to 1 if a subject correctly solves the decision task in a given round of the experiment and 0 otherwise. The dependent variable in Columns (5)–(8) is an indicator equal to 1 if a subject’s choice in a given round of the experiment coincides with the (choice alternative corresponding to the) default option and 0 otherwise. Control variables in Columns (2), (4), (6), and (8) include subjects’ age, gender, field of study, high school GPA and last high school math grade, ability in the background and decision task, perceived multitasking ability, and indicator variables for subjects’ assigned payoff scheme (cp. Table 1) and experiment wave. The lower part of the table reports p-values from post-estimation tests of the equality of selected coefficients (Wald tests). Robust standard errors, clustered at the subject level, are reported in parentheses. ***, **, * indicates significance at the 1%, 5%, and 10% level, respectively.

### Behavior in the Targeted Choice Domain

In a first step, we examine the impact of choice-promoting interventions on behavior in the domain that is targeted by the intervention (i.e., in the decision task). The estimations presented in Panel (a) of Table 2 analyze how the interventions affect the quality of subjects’ choices in the decision task. In line with the idea that the interventions induce a shift of cognitive resources to the targeted task, the results in Column (1) demonstrate that subjects’ performance in the decision task increases significantly relative to the Baseline environment. While subjects in Baseline on average solve the decision task correctly in 59.9% of cases, their performance increases by 10.7 percentage points when the decision task is permanently displayed on their screens.\(^{21}\) Adding controls for relative incentives, wave fixed effects, baseline ability levels, and sociodemographic characteristics has only

\(^{21}\)Recall that subjects in Baseline had to press a button on their keyboard to access the decision task. Doing so might in itself require some cognitive resources. Note, however, that this amount of resources should be minimal and thus have little relevance for the observed treatment differences. Moreover, if the cognitive costs of pressing a button were substantive, we would expect higher overall payoffs in the Intervention environment than in Baseline, which is not what we find (see Section 4.3).
a negligible effect on the estimated treatment coefficient (see Column (2) of Table 2). Columns (3)–(4) further show that the performance-enhancing effect of the intervention occurs irrespective of whether the decision task does or does not feature a default. Effects of the DIRECTED and FORCED condition are very similar and do not differ significantly from each other.\footnote{Table O.2 in the Online Appendix depicts the effects of the interventions separately for the treatments with different relative incentives. Qualitatively, effects are similar across the different incentive conditions. Quantitatively, the performance increase is somewhat less pronounced in treatments with relatively high payoffs for the decision task, in which subjects already allocate relatively more cognitive resources to the decision task in the BASELINE environment (see Section 4.1). In Table O.3 in the Online Appendix, we further analyze how subjects with a large and small baseline stock of cognitive resources respond to the interventions. While choices in the decision task improve for both groups, the effect is more pronounced for subjects with a small stock of cognitive resource. This is in line with the notion that the interventions should alter the cognitive resource allocation of subjects with a relatively small stock of cognitive resources to a larger extent (cp. Section 3).}

Panel (b) of Table 2 illustrates that the observed performance increase in the decision task is associated with a strong decrease in passive decision-making for the task. The table compares the rates at which subjects follow the default in the BASELINE and INTERVENTION environment. For the FORCED condition, which featured no defaults, we consider the frequency with which subjects choose the option that happened to be the default in the exact same version of the decision task in BASELINE and DIRECTED, exploiting the fact that the order and content of tasks was identical across all subjects in the experiment (cp. Section 2).\footnote{A question of detail concerns the evaluation of cases in which subjects failed to make a decision in the FORCED environment (cp. Footnote 5). For sake of comparability, we treat these cases as “passive” choices—since the same behavior of ignoring the decision task entirely would lead a subject to stick to the default in the BASELINE and DIRECTED environment.} The estimates in Columns (5)–(6) of Table 2 show that default adherence decreases significantly relative to the BASELINE environment, indicating that the interventions indeed succeed in promoting more active decision-making.\footnote{Default adherence rates decrease in all incentive conditions (cp. Table O.2 in the Online Appendix). Yet, the effect is less pronounced and not statistically significant for conditions in which subjects already dedicate a high amount of resources to the decision task (and hence make more active choices) in the BASELINE environment.}

When considering the FORCED and DIRECTED environment separately, we find that the tendency to choose the (option corresponding to the) default is lowest in FORCED, intermediate in DIRECTED, and highest in BASELINE (see Columns (7)–(8)). The difference between the FORCED and DIRECTED environment is interesting, as it demonstrates that simply directing attention to the decision task does foster active decision-making, but at the same time it does not fully eliminate subjects’ propensity to stick to defaults.

Our findings on how choice-promoting interventions affect subjects’ behavior in the targeted domain are in line with Proposition 2 of our model and can be summarized as follows:

**Result 2.** *Encouraging active decision-making by steering individuals’ attention to the decision task improves individuals’ performance in the task and reduces passive behavior.*
Table 3: Cognitive Spillovers

<table>
<thead>
<tr>
<th></th>
<th>Lab Experiment</th>
<th>Online Experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Intervention</strong></td>
<td>-0.028*</td>
<td>-0.038***</td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>(0.014)</td>
</tr>
<tr>
<td><strong>Directed Attention</strong></td>
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<td>-0.032*</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.016)</td>
</tr>
<tr>
<td><strong>Forced Choice</strong></td>
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<td>-0.044***</td>
</tr>
<tr>
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<td>(0.020)</td>
<td>(0.016)</td>
</tr>
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<td><strong>Constant</strong></td>
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<td>0.519***</td>
</tr>
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<td>(0.014)</td>
<td>(0.014)</td>
</tr>
<tr>
<td><strong>Controls</strong></td>
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<td>Yes</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>12160</td>
<td>12160</td>
</tr>
<tr>
<td><strong>No. Subjects</strong></td>
<td>608</td>
<td>608</td>
</tr>
<tr>
<td><strong>$R^2$</strong></td>
<td>0.001</td>
<td>0.074</td>
</tr>
<tr>
<td><strong>DIRECTED=FORCED</strong></td>
<td>0.606</td>
<td>0.453</td>
</tr>
</tbody>
</table>

Note: The table reports results of OLS regressions of treatment differences in choice quality in the background task. The dependent variable in Columns (1)–(4) is an indicator equal to 1 if a subject correctly solves the background task in a given round of the laboratory experiment and 0 otherwise. The dependent variable in Columns (5)–(6) is the fraction of pairs that a subject uncovers in the Memory game in a given round of the online experiment. Columns (2) and (4) include the set of control variables specified in Table 2. Control variables in Column (6) include subjects’ age, gender, and screen size. The lower part of the table reports p-values from post-estimation tests of the equality of selected coefficients (Wald tests). Robust standard errors, clustered at the subject level, are reported in parentheses. ***, **, * indicates significance at the 1%, 5%, and 10% level, respectively.

**Cognitive Spillovers**

The results depicted in Table 2 indicate that choice-promoting interventions succeed in reaching their primary goal: subjects make more active and better decisions in the targeted choice domain. These positive effects in the targeted domain might, however, come at the cost of negative spillovers on the quality of people’s choices in other domains. To investigate whether this is the case, we analyze differences in background task performance between the Baseline and Intervention environment.

Table 3 shows that the interventions indeed have a negative effect on the quality of subjects’ decisions in the background task. Individuals’ likelihood to correctly solve the background task decreases from 76.0% in the Baseline environment to 73.2% in the Intervention environment. As shown in Columns (1)–(2) of Table 3, the average spillover effect is weakly significant in a regression framework without controls, but turns out to be significant at the one percent level when controlling for wave fixed effects and the additional covariates discussed above (t-test, $p = 0.007$). When comparing the Baseline environment separately to the behavior in the Directed and Forced environment, we see that the spillover effect is somewhat more pronounced (and also
statistically significant only) in the Forced environment. However, the difference in spillover effects induced by the Directed and Forced interventions is not statistically significant.

To further explore the relevance of cognitive spillovers and ensure that our spillover result represents a robust phenomenon, we complement our empirical analysis with evidence from an additional online experiment. The design of the online experiment—which is discussed in more detail in Section V in the Online Appendix—closely follows the paradigm of our main experiment. Subjects had to allocate their cognitive resources across two tasks. The decision task was identical to the laboratory experiment. As background task, subjects in the online experiment had to solve a Memory game instead of memorizing strings of numbers. This modification of the background task was necessary to prevent subjects from “solving” the background task by taking notes. To analyze the effects of choice-promoting interventions, we varied which task was displayed first when subjects entered a new round of the experiment. Specifically, upon entering a new round of the online experiment, subjects in the Baseline environment faced a screen displaying the background task. Their counterparts in the Intervention environment faced the screen on which the decision task was displayed—emulating the idea of promoting active choices in the decision task by steering subjects’ attention to the task. In both environments, however, subjects could freely navigate from one task to the other. The results depicted in Columns (5)–(6) of Table 3 show that steering individuals’ attention to the decision task leads to a highly significant reduction in subjects’ performance in the background task (t-test, \( p < 0.001 \))—a negative cognitive spillover. In the Online Appendix, we present further evidence that the spillover in the online experiment works through precisely the same mechanisms as in our main experiment. Subjects in the Intervention environment devote more attention to the decision task, they are less likely to stick to defaults, and make better decisions in the decision task. These positive effects, however, come at the cost of fewer resources devoted to and worse performance in the background task.

In sum, the findings from our laboratory and online experiments both indicate that interventions that steer individuals’ attention to one particular choice domain induce negative cognitive spillovers on other domains. To provide further insights on the determinants and scale of these spillovers, we next examine how the likelihood and strength of cognitive spillovers depend on the characteristics of decision makers and the decision environment. In particular, our theoretical framework points to a heterogeneity analysis along three dimensions: individuals’ baseline stock of cognitive resources, the difficulty of the decision task, and the relative incentives for the different tasks. In what follows, we test whether these factors alter the strength of cognitive spillovers in line with our theoretical
predictions from Proposition 3.

**Cognitive Spillovers and the Stock of Cognitive Resources**

We first examine how the strength of the spillover depends on individuals’ stock of cognitive resources. The evidence in Section 4.1 demonstrated that individuals with a smaller stock of cognitive resources are more likely to pay no attention to the decision task in the Baseline environment, and rather concentrate all their available resources on solving the background task. This behavior makes them particularly susceptible to adverse effects of interventions that steer individuals’ attention to the decision task. In contrast, for subjects with a large stock of cognitive resources, who already devote a relatively high amount of resources to the decision task in Baseline, we would expect the interventions to distort cognitive resource allocation less or not at all.

Figure 4 depicts the estimated cognitive spillovers separately for subjects with above- and below-median scores on the Raven test.\(^{25}\) Two points are worth noting about the depicted results. First,

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\(^{25}\)All figures in this section display estimated effect sizes and 95% confidence intervals based on regression models corresponding to Column (2) in Table 3, in which we additionally interact the Intervention dummy with a dummy indicating whether a subject’s Raven score lies below / above the median. The estimation includes the set of control variables specified in Table 2; detailed estimation results, including specifications that estimate separate effects for the Directed and Forced condition, are reported in Table O.4 in the Online Appendix.
Figure 5: Heterogeneity of Cognitive Spillovers: Task Difficulty

Note: The figure depicts treatment coefficients and 95% confidence intervals of a regression model corresponding to Column (2) in Table 3, in which we additionally interact the Intervention dummy with a dummy indicating whether the difficulty of the decision task in a given round of the experiment lies below / above the median difficulty. The estimation includes the set of control variables specified in Table 2; detailed estimation results, including specifications that estimate separate effects for the Directed and Forced condition, are reported in Table O.5 in the Online Appendix.

among subjects with below-median test scores, the intervention decreases performance in the background task by roughly six percentage points relative to the Baseline environment. For these subjects, the cognitive spillover is thus large and highly statistically significant (t-test, $p = 0.003$). Second, spillovers for subjects with below-median test scores are significantly stronger than for subjects with a relatively large stock of resources (t-test, $p = 0.024$). For the latter group, the interventions have essentially no effects on background task performance. In line with part (i) of Proposition 3, individuals’ baseline stock of cognitive resources thus seems to be a key determinant of the emergence and strength of cognitive spillovers.

**Cognitive Spillovers and Task Difficulty**

A second factor that should influence the strength of cognitive spillovers is the difficulty of the decision task. If the task becomes cognitively more demanding (while payoffs stay the same), more subjects will decide to allocate only small amounts of resources to the decision task in Baseline. As a consequence, the interventions will force more subjects to reallocate cognitive resources, leading to stronger spillovers. To test part (ii) of Proposition 3, we exploit the variation in task difficulty induced by our sampling of options for the decision task (see Section 2). Specifically, we measure
the difficulty of the decision task by comparing the option with the highest sum and the option with the second-highest sum. If the difference between the corresponding sums is large, a cursory glance may suffice to find the best option. If the difference between the two sums is small, however, it is harder to estimate which option is the best without precisely calculating the sum of each option.

Figure 5 illustrates how the strength of cognitive spillovers interacts with the difficulty of the decision task. We find that the spillover is sizable and highly significant for decision tasks with above-median difficulty. In rounds in which the decision task is difficult, subjects in the Intervention environment are 5.4 percentage points less likely to correctly solve the background task than their counterparts in Baseline (t-test, \( p = 0.001 \)). The size of the spillover decreases significantly \( (p = 0.017) \) and is not significantly different from zero \( (p = 0.230) \) for tasks with below-median difficulty.26

**Cognitive Spillovers and Incentives**

Finally, our theoretical framework predicts that the strength of cognitive spillovers should depend on the relative incentives for the different tasks. A decrease in the relative incentives for the decision task should lead to a decrease in the share of subjects who pay any attention to the task in the Baseline environment. We have already seen in Figure 2 that this prediction is supported by the data. Given the differences in cognitive resource allocation in Baseline, interventions are more likely to induce cognitive spillovers in environments in which relative incentives for the decision task are rather low.

Figure 6 depicts cognitive spillovers separately for the different incentive conditions. In the conditions with a relative-payoff ratio of 1:4, performance in the background task decreases by 4.0 percentage points in response to the intervention (t-test, \( p = 0.020 \)). When relative payoffs increase to 1:2, the estimated spillover becomes marginally smaller (3.8 percentage points), and it decreases further to 2.0 percentage points in the conditions with a 1:1 payoff ratio. For the conditions with a relative-payoff ratio of 1:2 and 1:1, the observed spillover is not significantly different from zero (t-test, \( p = 0.178 \) and \( p = 0.680 \), respectively). The estimates for the latter conditions and their lack of statistical significance, however, have to be taken with some caution, as they rely on relatively small samples (as also indicated by the wide error bands).

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26The derivation of the corresponding theoretical prediction relies on the idea that subjects can condition their allocation of cognitive resources on the task difficulty. As subjects in our experiment do not know the task difficulty in a given round, they need to be able to reallocate at least some of their cognitive resources dynamically in order for task difficulty to matter in our setting. In contrast, we would not expect to observe the heterogeneity in Figure 5 if subjects commit to an allocation of cognitive resources before entering the decision task. Our findings thus support the notion that the allocation of cognitive resources is a dynamic process that is malleable over time.
Figure 6: Heterogeneity of Cognitive Spillovers: Relative Incentives

Note: The figure depicts treatment coefficients and 95% confidence intervals of a regression model corresponding to Column (2) in Table 3, in which we additionally interact the Intervention dummy with indicators for the different incentive conditions. The estimation includes the set of control variables specified in Table 2; detailed estimation results, including specifications that estimate separate effects for the Directed and Forced condition, are reported in Table O.6 in the Online Appendix.

Overall, the observed differences in the strength of cognitive spillovers provide further support for the theoretical predictions from Proposition 3. The findings underline that choice-promoting interventions lead individuals to withdraw scarce cognitive resources from other tasks, and that the extent to which they do so depends systematically on the characteristics of the decision maker and the choice environment.

Result 3. Interventions that steer attention to the decision task lead to a decrease in the quality of choices in the background task. This cognitive spillover is particularly pronounced (i) for subjects with a smaller stock of cognitive resources, (ii) in situations in which the decision task is more difficult, and (iii) in conditions in which the incentives for the decision task are relatively small.

4.3 Payoffs and the Efficiency of Cognitive Resource Allocation

Our findings demonstrate that interventions that steer individuals’ attention to a particular task have two countervailing effects on the quality of individuals’ decisions. They improve decisions in the targeted choice domain, but they also lead to inferior decisions in other domains, as subjects withdraw scarce cognitive resources from the latter. To evaluate the overall consequences of such interventions, it is therefore crucial to examine whether the increase in payoffs in the targeted
Table 4: Effect of Interventions on Overall Payoffs

<table>
<thead>
<tr>
<th>Intervention</th>
<th>Payoff Share</th>
<th>Payoff Share</th>
<th>Payoff Share</th>
<th>Payoff Share</th>
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</thead>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Payoff Share</td>
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<tr>
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<td></td>
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<td>(0.011)</td>
<td>(0.016)</td>
<td>(0.012)</td>
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<td>Directed Attention</td>
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<td></td>
<td></td>
</tr>
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<td>0.001</td>
<td>0.006</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.012)</td>
<td>(0.015)</td>
<td>(0.012)</td>
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<tr>
<td>Forced Choice</td>
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<td>(0.011)</td>
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<td>(0.050)</td>
<td>(0.011)</td>
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<tr>
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<td>0.074</td>
<td>0.000</td>
<td>0.074</td>
</tr>
<tr>
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<td>0.928</td>
<td>0.936</td>
<td>0.928</td>
</tr>
</tbody>
</table>

Note: The table reports results of OLS regressions of treatment differences in subjects’ overall payoffs. The dependent variable is a subject’s realized share of the maximally attainable overall payoff in the subject’s treatment (calculated on the subject-round level). Specifications with controls include the set of control variables specified in Table 2. The lower part of the table reports p-values from post-estimation tests of the equality of selected coefficients (Wald tests). Robust standard errors, clustered at the subject level, are reported in parentheses. \(**, **, \*\) indicates significance at the 1%, 5%, and 10% level, respectively.

domain is “worth” the negative cognitive spillovers on other decisions. In our experiment, we can study this question by assessing differences in subjects’ overall payoff between the Baseline and Intervention environment. To account for differences in the maximally feasible payoffs across conditions (resulting from the variation in relative payoffs), we divide subjects’ realized overall payoff by the maximally attainable payoff in their assigned treatment condition.

We find that subjects in the Baseline condition, on average, reap 72.5% of the maximally attainable payoff. As illustrated in Table 4, the overall payoffs in the environments with choice-promoting interventions are almost identical to this value. For all regression models, the differences in overall payoffs across choice environments is close to zero and statistically insignificant. Hence, the positive impact of the interventions on the quality of choices in the decision task and the negative spillovers on the background task cancel each other out, such that the resulting net effect on subjects’ overall payoff is essentially zero.\(^{27}\) It is also informative to compare treatment differences in participants’ overall payoffs in the online experiment discussed in Section 4.2. In this setting, we find that subjects whose attention is steered towards the decision task earn significantly less

\(^{27}\) In Table O.8 in the Online Appendix, we examine the payoff effects of the interventions separately for the different incentive conditions. In line the average effects from Table 4, payoff differences are small and statistically insignificant for all conditions.
Figure 7: Heterogeneity of Payoff Effects: Raven Score

Note: The figure depicts treatment coefficients and 95% confidence intervals of a regression model corresponding to Column (2) in Table 4, in which we additionally interact the INTERVENTION dummy with a dummy indicating whether a subject’s Raven score lies below / above the median. The estimation includes the set of control variables specified in Table 2; detailed estimation results, including specifications that estimate separate effects for the DIRECTED and FORCED condition, are reported in Table O.9 in the Online Appendix.

than their counterparts in the respective BASELINE condition (overall payoff shares differ by 2.8 percentage points; see Table O.10 in the Online Appendix).

The impact of choice-promoting interventions on overall payoffs in our experiments is thus zero or slightly negative. While this finding is qualitatively in line with our theoretical model, it is perhaps surprising how precisely individuals’ reactions to the interventions offset each other in terms of overall payoffs. When facing the interventions, subjects seem to reallocate their resources rather efficiently, such that the exogenous shift in their attention is not accompanied by a substantial drop in profits.

These findings raise the question to what extent subjects in the experiment manage to efficiently allocate cognitive resources across tasks. Unfortunately, we cannot directly test this question, as a subject’s efficient allocation of resources depends on the slope and curvature of the functions $\pi_B$ and $\pi_D$ (the functions determining the probability to correctly solve each task). In our experiment, these functions are unobserved and they are also likely to differ across participants. We can, however, exploit the variation of the decision environment across treatments to derive a set of theory-based necessary conditions for the allocation of cognitive resources in BASELINE to be efficient (see Appendix A.5 for details).
The first test makes use of the induced variation in whether or not subjects face an intervention. As discussed in our theoretical framework in Section 3, the Intervention environment can be thought of as a constrained version of the Baseline condition. Hence, if subjects efficiently allocate resources in the latter, we should observe no subgroup that strictly benefits from the interventions. When considering average treatment differences in overall payoffs (Table 4), we have seen that the condition is indeed fulfilled for the full sample of participants. To challenge the condition more rigorously, we further study payoff effects of the interventions in different subgroups of participants. In this respect, the subgroups with smaller vs. larger stocks of cognitive resources are of particular interest. As we have shown above, the additional constraint imposed by the Intervention condition does not seem to be binding for subjects with larger stocks of cognitive resources, such that they exhibit little or no cognitive spillovers.

To examine whether subjects with high Raven scores may even reap net benefits from the interventions, Figure 7 depicts profit differences between the Intervention and Baseline condition, separately for subjects with below- vs. above-median Raven scores. For the latter group of participants, we indeed observe a mild (statistically insignificant) increase in payoffs in response to the interventions. If we zoom in further and focus only on subjects in the top quartile of the Raven-score distribution, the positive net effect of the interventions is more pronounced and also weakly statistically significant (t-test, $p = 0.058$; cp. Figure O.2 in the Online Appendix)—in violation of our first necessary condition for the efficiency of the allocation of cognitive resources. Our data thus indicates that, at least for individuals with a very large stock of cognitive resources, the Baseline allocation of cognitive resources may not be efficient.

Our second approach to test the efficiency of cognitive resource allocation exploits the treatment variation in relative payoffs for the two tasks. It builds on the premise that subjects optimally respond to changes in the relative incentives. To illustrate the implications of an optimal reallocation of cognitive resources, consider two incentive conditions, $Y$ and $Z$. Holding the behavior of subjects in condition $Y$ constant, we can calculate the hypothetical payoffs that their choices would have generated in incentive condition $Z$. Efficient (re)allocation implies that the actual payoffs of subjects in $Z$ should be at least as high as the hypothetical payoffs calculated on the basis of condition-$Y$ behavior (put differently, subjects in treatment $Z$ should be able to at least replicate the behavior of subjects in condition $Y$ and, therefore, never generate less payoff). The data from Baseline-10, Baseline-20, and Baseline-40 allow us to test whether this condition is fulfilled, as these treatments vary the relative incentives for the two tasks, while holding the order
Table 5: Hypothetical Payoffs Across Incentive Conditions

<table>
<thead>
<tr>
<th></th>
<th>Baseline-10</th>
<th>Baseline-20</th>
<th>Baseline-40</th>
</tr>
</thead>
<tbody>
<tr>
<td>Subjects in Baseline-10</td>
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<td>0.711</td>
<td>0.679</td>
</tr>
<tr>
<td></td>
<td>(0.161)</td>
<td>(0.146)</td>
<td>(0.140)</td>
</tr>
<tr>
<td>Subjects in Baseline-20</td>
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<td>0.711</td>
<td>0.687</td>
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<td></td>
<td>(0.129)</td>
<td>(0.122)</td>
<td>(0.132)</td>
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<tr>
<td>Subjects in Baseline-40</td>
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<td>0.661</td>
<td>0.668</td>
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<tr>
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<td>(0.206)</td>
<td>(0.176)</td>
<td>(0.146)</td>
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<tr>
<td>Difference to highest hypothetical payoff</td>
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<td>0.000</td>
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<td>p-values</td>
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<td>0.988</td>
<td>0.629</td>
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</table>

Note: The table reports empirically observed and hypothetical shares of overall payoffs in the experiment. Within a given row, the behavior of subjects in the respective treatment is held fixed. Bold values on the diagonal depict the empirically observed share of the maximally attainable overall payoff in a given treatment. Off-diagonal values depict the corresponding payoff share subjects would have earned in the treatments denoted at the top of each column. At the bottom of the table, we report the difference between the empirically observed payoff share and the highest hypothetical value in each column, and p-values from t-tests of equality of the empirically observed and highest hypothetical payoff share.

and content of tasks constant in all rounds of the experiment (cp. Section 2).

Table 5 depicts the actual payoffs of subjects in a given incentive condition (bold numbers) as well as the hypothetical payoffs that their decisions would have generated in the alternative incentive conditions. For instance, the first row shows that subjects in Baseline-10 achieved 73.6% of the maximally attainable payoffs in their treatment. The same decisions would have yielded 71.1% and 67.9% of the maximally possible payoffs in Baseline-20 and Baseline-40, respectively. By comparing the numbers within a given column of the table, we can assess whether the actual payoffs of subjects exposed to a specific incentive condition lie above or below the hypothetical payoffs, calculated based on the behavior of subjects in the other conditions. At the bottom of the table, we depict the difference between the actually attained payoffs and the highest hypothetical value in each column. Across all columns, the differences are close to zero and not statistically significant. Hence, our data cannot reject the second necessary condition for the allocation of cognitive resources to be efficient.

Overall, our data in combination with the theory-based necessary conditions suggest some limited deviations from the efficient allocation of cognitive resources in the Baseline condition. On the one hand, we cannot reject that subjects’ reallocation of cognitive resources in response to changes in relative incentives is optimal. On the other hand, choice-promoting interventions seem
to increase the overall decision quality for some subjects, indicating that their allocation of cognitive resource in the BASELINE condition is not efficient.

5 Conclusions

We conclude by discussing practical implications of our findings for the design and evaluation of behavioral policy interventions. Typically, the success of such policies is examined solely with respect to the outcomes in the decision domain that is the subject of the intervention. This approach presumes that the policies do not trigger negative spillovers to other domains. Our results indicate that this assumption might frequently be violated. Whenever different tasks or decisions compete for people's scarce cognitive resources, interventions that steer attention to one domain can induce negative cognitive spillovers on others, which may dilute or even fully offset the policies' positive effects. While for some interventions the net effects may still be positive, examining the existence and magnitude of cognitive spillovers is crucial in order not to systematically overestimate the benefits of behavioral policy interventions.

On a more positive note, our findings also suggest that appropriately designed policies may have the potential to generate positive cognitive spillovers on choices in other domains of decision makers' lives. One candidate for such a “positive-spillover policy” are high-quality defaults and high-quality recommendations (Kling et al. 2012, Kaufmann et al. 2018). Defaults in our experiment were chosen at random and, therefore, passive behavior resulted in relatively poor choices in the decision task. In many natural settings, however, defaults are chosen by policy makers with the intention to induce better-than-random outcomes. As we show more formally in an extension to our theoretical analysis, our framework implies that such well-chosen defaults might yield a double dividend when cognitive resources are scarce: they do not only improve outcomes for passive decision makers, but they also “free up” scarce cognitive resources and allow individuals to focus on other pressing tasks, which improves their decisions therein (cp. Appendix A.6). In a similar vein, policies that facilitate complex decisions (e.g., by simplifying forms, rules, or decision processes) may not only have direct positive consequences, but could also yield positive spillovers on choices in other domains. Analyzing the scope of such positive cognitive spillover is an interesting agenda for future research.

Data Availability Statement

The data and code underlying this research is available on Zenodo at https://doi.org/10.5281/zenodo.5652808
References


Appendix A  Proofs and Derivations

This section presents the formal arguments underlying the behavioral predictions discussed in Section 3 and Section 4.

Appendix A.1  Proofs of Propositions

Proof of Proposition 1

We start by deriving the efficient allocation of cognitive resources in the Baseline environment.

Since \( \pi_B(x_B) \) is strictly increasing, all cognitive resources will be exhausted. Thus, the maximization problem can be rewritten as

\[
\max_{x_D} \pi_B(X^j - x_D)u_B + \pi_D(x_D)u_D \\
\text{s.t. } x_D \in [0, X^j]
\]

where \( \pi_D(x_D) \equiv \frac{1}{3}\pi_D(x_D, c) + \frac{2}{3}\pi_D(x_D, inc) \). The objective function is strictly concave in \( x_D \). The derivative with respect to \( x_D \) yields

\[
-\pi'_B(X^j - x_D)u_B + \pi'_D(x_D)u_D.
\]

Due to part (iv) of Condition 1, \( x^*_{D,Base} \) will be strictly positive and \( x^*_{D,Base} < X^j \) for all levels of \( X^j \). Moreover, the concavity of \( \pi_B \) and \( \pi_D \) implies that, \( x^*_{D,Base} = 0 \) holds if and only if:

\[
\pi'_D(0)u_D \leq \pi'_B(X^j)u_B.
\]

Therefore, there exists a strictly positive threshold \( \bar{X} \in \mathbb{R}^+ \) such that individual \( j \) will abstain from devoting any cognitive resources to the decision task if and only if \( X^j \leq \bar{X} \). \( \bar{X} \) is implicitly defined by:

\[
\pi'_D(0)\frac{u_D}{u_B} = \pi'_B(\bar{X}).
\]

Next, consider how \( x^*_{D,Base} \) varies in \( \frac{u_D}{u_B} \). Since \( \pi_B \) is concave, it is clear from (2) that \( \bar{X} \) decreases with \( \frac{u_D}{u_B} \). For subjects with \( X^j < \bar{X} \) a marginal shift in incentives does not affect \( x^*_{D,Base} \), while for subjects with \( X^j \geq \bar{X} \), we have

\[
-\pi'_B(X^j - x^*_{D,Base}) + \pi'_D(x^*_{D,Base})\frac{u_D}{u_B} = 0,
\]
which implies:

\[ \frac{dx_{D,Base}^*}{d\frac{u_D}{u_B}} = -\frac{\pi_D'(x_{D,Base}^*)}{\pi_B(x_{B,Base}^*) + \pi_D'(x_{D,Base}^*)\frac{u_D}{u_B}} > 0. \]

Overall, \( x_{D,Base}^* \) is thus weakly increasing in \( \frac{u_D}{u_B} \) for all levels of \( X^j \). It immediately follows that the same holds for the quality of choices in the decision task, \( \pi_D(x_{D,Base}^*) \), which is increasing in \( x_D \).

This proves parts (i) and (ii) of Proposition 1.

For part (iii), recall that subjects who ignore the decision task automatically stick to the default. Default adherence rates for subjects with \( X^j \leq \bar{X} \) therefore weakly decrease in \( \frac{u_D}{u_B} \). Next consider the case of \( X^j > \bar{X} \) and denote the probability to follow the default option for this case by \( q(x_D) \), which is given by:

\[ q(x_D) = \frac{1}{3}\pi_D(x_D, c) + \frac{2}{3}\rho(1 - \pi_D(x_D, inc)) \]

where \( \rho \) is the probability of sticking to the default when \( x_D > 0 \), conditional on the default being incorrect and the subject choosing either the default or the other incorrect option. If \( \rho \geq \frac{1}{2} \), we get the following result for the change in default adherence when \( x_D \) increases:

\[ q'(x_D) = \frac{1}{3}\pi_D'(x_D, c) - \frac{2}{3}\rho\pi_D'(x_D, inc) \leq \frac{1}{3}[\pi_D'(x_D, c) - \pi_D'(x_D, inc)] \leq 0, \]

where the last inequality holds because of part (ii) of Condition 1. Thus, default adherence decreases with \( \frac{u_D}{u_B} \), yielding part (iii) of Proposition 1.

**Proof of Proposition 2**

We next analyze how the Intervention environment (with its two variants Directed Attention and Forced Choice) shapes individuals’ allocation of cognitive resources and the resulting decisions. Note first that, for a given amount of cognitive resources \( x_D \), the expected rate of solving the decision task correctly is identical in Forced and Directed because of the no-arbitrage condition (part (iii) of Condition 1). An individual’s decision problem in the Intervention condition can thus be stated as follows:

\[ \max_{x_B, x_D} u(x_B, x_D) = \pi_B(x_B)u_B + \left(\frac{1}{3}\pi_D(x_D, c) + \frac{2}{3}\pi_D(x_D, inc)\right)u_D \tag{4} \]

s.t. \( x_D \geq x_I \) and \( x_B + x_D \leq X^j \)

The intervention will thus increase \( x_D \) compared to Baseline for all subjects that would allocate fewer resources than \( x_I > 0 \) to the decision task in Baseline. With the same arguments as above, there exists a threshold \( \bar{X} > \bar{X} \) such that \( x_{D,Base}^* < x_I \) if and only if \( X^j < \bar{X} \). The threshold is
implicitly defined by:

\[ \pi'_D(x_I) \frac{u_D}{u_B} = \pi'_B(\tilde{X} - x_I). \]  

(5)

Hence, all \( j \) with \( X^j < \tilde{X} \) increase the amount of resources allocated to the decision task in response to the intervention, while the resource allocation of subjects with \( X^j \geq \tilde{X} \) is not affected. The associated overall increase in the amount of resources on the decision task implies a higher \( \pi_D \) and lower default adherence (see the proof of Proposition 1). Nevertheless, individuals in DIRECTED will, in expectation, stick to the default in more than one third of cases; their default adherence rate will, thus, be higher than the rate at which subjects in FORCED choose the corresponding option.

To see this, note that for \( x_D > 0 \) and \( \rho \geq \frac{1}{2} \),

\[
\frac{1}{3} \pi_D(x_D, c) + \frac{2}{3} \rho(1 - \pi_D(x_D, inc)) \geq \frac{1}{3} [\pi_D(x_D, c) + (1 - \pi_D(x_D, inc))] \geq \frac{1}{3},
\]

where the last inequality holds as a consequence of part (i) of Condition 1.

**Proof of Proposition 3**

As noted in the discussion of Proposition 1, subjects in the Baseline environment make use of all their available cognitive resources. Hence, the increase in resources devoted to the decision task in response to the intervention directly implies a reduction of resources devoted to the background task (i.e., \( x^*_B,\text{Inter} \leq x^*_B,\text{Base} \)) and, in turn, a lower likelihood of correctly solving this task. Hence, the spillover \( \Delta \pi_B = \pi_B(x^*_B,\text{Inter}) - \pi_B(x^*_B,\text{Base}) \) is weakly negative for all \( X^j \).

To see how the size of the cognitive spillover varies with \( X^j \), recall that the intervention will not have any impact on subjects with \( X^j > \tilde{X} \). Additionally, all subjects with \( X^j < \tilde{X} \) will increase their cognitive resources allocated to the decision task from zero to \( x_I \). Overall, the size of the cognitive spillover is thus given by:

\[
\Delta \pi_B = \begin{cases} 
0 & \text{if } X^j \geq \tilde{X} \\
\pi_B(X^j - x_I) - \pi_B(x^*_B,\text{Base}) & \text{if } \tilde{X} \geq X^j \geq \tilde{X} \\
\pi_B(X^j - x_I) - \pi_B(X^j) & \text{if } X^j \leq \tilde{X} 
\end{cases}
\]  

(6)

For subjects with \( X^j \leq \tilde{X} \), the absolute size of the spillover decreases if \( X^j \) increases because \( \pi'_B(X^j - x_I) - \pi'_B(X^j) > 0 \) due to the concavity of \( \pi_B \). For intermediate subjects with \( \tilde{X} < X^j < \tilde{X} \), \( x^*_D,\text{Base} \) is given by:

\[
\pi'_D(x^*_D,\text{Base})u_D = \pi'_B(X^j - x^*_D,\text{Base})u_B.
\]
which implies that if $X^j$ increases, these subjects will increase the resources devoted to the decision task. Formally,

$$
\frac{dx^*_D, \text{Base}}{dX^j} = \frac{\pi''_B(X^j - x^*_D, \text{Base})}{\pi''_B(X^j - x^*_D, \text{Base}) + \pi''_D(x^*_D, \text{Base}) \frac{u_D}{u_B}} > 0.
$$

Hence, for all agents with intermediate $X^j$, we get $\frac{dx^*_D, \text{Base}}{dX^j} = 1 - \frac{dx^*_D, \text{Base}}{dX^j} < 1$. For the size of the spillover, this implies:

$$
\frac{d\Delta \pi_B}{dX^j} = \frac{d}{dX^j} \left[ \pi_B(X^j - x_I) - \pi_B(x^*_B, \text{Base}) \right]
= \pi_B'(X^j - x_I) - \pi_B'(x^*_B, \text{Base}) \frac{dx^*_B, \text{Base}}{dX^j}
> \pi_B'(X^j - x_I) - \pi_B'(x^*_B, \text{Base}) > 0,
$$

where the last inequality holds due to the concavity of $\pi_B$ and because $x^*_B, \text{Base} > X^j - x_I$, which follows from the definition of $\tilde{X}$. As there is no effect on the cognitive spillover for subjects with $X^j \geq \tilde{X}$, this argument concludes the proof of part (i).

To analyze how the strength of the cognitive spillover depends on the difficulty of the decision task (part (ii) of Proposition 3), let parameter $\phi \in \mathbb{R}$ reflect the difficulty of the decision task such that:

$$
\frac{\partial^2 \pi_D(x_D, \phi)}{\partial \phi \partial x_D} < 0 \quad \forall \ x_D
$$

For subjects with $X^j \notin [\tilde{X}, \tilde{X}]$, there will be no effect on the size of the spillover (cp. (6)). For subjects with $\tilde{X} > X^j > \tilde{X}$, we get:

$$
\frac{dx^*_D, \text{Base}}{d\phi} = -\frac{\partial^2 \pi_D(x^*_D, \text{Base}, \phi)}{\partial x_D \partial \phi} \frac{\partial^2 \pi_D(x^*_D, \text{Base}, \phi)}{\partial x_D \partial \phi} \frac{u_D}{u_B} < 0
$$

Hence, for those subjects we get for the spillover:

$$
\frac{d\Delta \pi_B}{d\phi} = \frac{d}{d\phi} \left[ \pi_B(X^j - x_I) - \pi_B(x^*_B, \text{Base}) \right] = -\pi_B'(x^*_B, \text{Base}) \frac{dx^*_B, \text{Base}}{d\phi} < 0.
$$

For subjects with $X^j \in (\tilde{X}, \tilde{X})$, the cognitive spillover thus becomes more negative if the difficulty of the decision task increases. The same also holds true for the cutoff-types, as both cutoffs increase in $\phi$ (see (2) and (5)), which implies a larger cognitive spillover for them as defined by (6).

For part (iii) note that Proposition 1 already implies that $x^*_D, \text{Base}$ is increasing in the relative incentives for the decision task. With the same arguments as in part (ii), we thus get that $\frac{d\Delta \pi_B}{d(u_D/u_B)} >$
Appendix A.2 Discussion of Part (iv) of Condition 1

Recall that part (iv) of Condition 1 assumes that \( \pi'_D(0)u_D < \pi'_B(0)u_B \), i.e., the incentives of the background task are sufficiently large such that individuals with a minimal stock of cognitive resources would allocate their resources to the background task rather than the decision task. If this condition is violated, as for example in the environment with AMPLE resources, subjects always allocate positive amounts of cognitive resources to the decision task. An analysis along the same lines as the proof of Proposition 1 shows that there exist a threshold \( X' \geq 0 \) such that subjects dedicate all their available cognitive resources to the decision task if and only if \( X^j \leq X' \). Individuals with \( X^j > X' \) again allocate positive amounts of cognitive resources to both tasks according to (3). \( X' \) increases in the relative incentives for the decision task (cp. Appendix A.1). Therefore, all parts of Proposition 1 also hold under this alternative assumption. Similarly, all parts of Proposition 2 hold as long as \( x_I > X' \). Conversely, if—as a result of high relative incentives for the decision task—all subjects allocate more resources to the decision task than the amount that would be exogenously reallocated, there would be no effect of the INTERVENTION condition. If instead interventions do have bite (i.e., if \( x_I > X' \)), they also lead to negative cognitive spillovers as specified in Proposition 3. As higher relative incentives for the decision task again imply that fewer subjects are susceptible for a cognitive spillover, parts (i)–(iii) of Proposition 3 hold analogously for the alternative setting in which part (iv) of Condition 1 is not fulfilled.

Appendix A.3 Incentive Effect for Correct vs. Incorrect Defaults

As discussed in Footnote 19, our framework also indicates that the effect of an increase in \( u_D/u_B \) on default adherence is stronger for incorrect defaults than for correct defaults. Consider first the case of an incorrect default option. In this case, the likelihood to stick to the default (for subjects with \( X^j > X \)) is given by \( \rho(1 - \pi_D(x_D, inc)) \). This probability decreases in \( x_D \). For this case, an increase in the relative incentive to solve the decision task causes an increase in \( x_D \) by subjects with \( X^j > X \) as well as a decrease in \( X \), which both imply less default adherence. Second, consider the case of a correct default option. For subjects with \( X^j \geq X \), the likelihood of default adherence in this case is \( \pi_D(x_D, c) \). Hence, subjects with \( X^j \geq X \) may follow the default more often when allocating more resources to the decision task, because they make fewer mistakes. However, an increase in \( u_D/u_B \) causes \( X \) to decrease, which again leads to lower default adherence. If the default option coincides with the correct solution, the net effect is thus ambiguous.
Appendix A.4 Resource Allocation with Ample Resources

As indicated in the main body of the text, the condition with AMPLE cognitive resources rests on the idea that memorizing two-digit numbers essentially requires zero cognitive resources. This idea can be conceptualized by setting $\pi_B(x_B) = 1$ for all $x_B \geq 0$. The decision problem then becomes:

$$\max_{x_D} u(x_B, x_D) = \left( \frac{1}{3} \pi_D(x_D, c) + \frac{2}{3} \pi_D(x_D, inc) \right) u_D.$$ 

s.t. $x_D \leq X^j$

The solution to this decision problem is clearly to set $x_D = X^j$, which is identical to the solution of the decision problem in (1) if the relative incentives for the decision task become large enough such that $\pi_D'(X^j)u_D > \pi_B'(0)u_B$ holds for all $X^j$. The AMPLE condition is then behaviorally equivalent to the boundary case of a treatment with scarce cognitive resources in which relative incentives for the decision task are very large.

This argument also immediately implies that in the INTERVENTION environment with AMPLE resources, the constraint of devoting at least $x_I$ to the decision task will never be binding (recall that $x_I < X^j$). As all subjects have more cognitive resources available than the amount that is exogenously reallocated by the intervention, and as they already devote all their resources to the decision task in BASELINE, there will be no behavioral reaction to the treatment interventions.

Appendix A.5 Efficiency of the Allocation of Cognitive Resources

In Section 4.3, we make use of two conditions that are necessary for the allocation of cognitive resources in BASELINE to be efficient. First, no subgroup of the population should profit from the INTERVENTION condition. This condition is obviously necessary for the allocation in BASELINE to be efficient since the decision problem in INTERVENTION is a constrained version of the one in BASELINE (see (4)).

The second condition builds on the notion that subjects should optimally respond to incentives. To see what an optimal allocation of cognitive resources implies across incentive conditions, take two incentive conditions, $Y$ and $Z$, with relative payoffs $(u_Y^D/u_Y^B) \neq (u_Z^D/u_Z^B)$. Optimal behavior of subjects in conditions $Y$ and $Z$ is given by $(x_{D,Base}^Y, x_{B,Base}^Y)$ and $(x_{D,Base}^Z, x_{B,Base}^Z)$, respectively. Due to optimality we get:

$$\pi_B(x_{B,Base}^Y)u_B^Y + \left( \frac{1}{3} \pi_D(x_{D,Base}^Y, c) + \frac{2}{3} \pi_D(x_{D,Base}^Y, inc) \right) u_D^Y \geq \pi_B(x_{B,Base}^Z)u_B^Z + \left( \frac{1}{3} \pi_D(x_{D,Base}^Z, c) + \frac{2}{3} \pi_D(x_{D,Base}^Z, inc) \right) u_D^Z,$$
for all $X^j$. That is, the optimal behavior in condition $Y$ should yield higher payoffs than the optimal behavior from condition $Z$ given the relative incentives $u_D^Y / u_B^Y$. This is the second necessary condition that we use in Section 4.3.

Appendix A.6 Double Dividend of Good Defaults

As discussed in the introduction and concluding remarks of the paper, our framework also implies that well-chosen defaults yield a double dividend when cognitive resources are scarce: they do not only directly improve outcomes for decision makers who passively stick to defaults, but they also “free up” scarce cognitive resources. The latter implies that the quality of subjects’ decisions in the background task increases with the default quality in the decision task. To derive this prediction formally, denote the quality of the default option—the probability with which the default option is correct—by $\lambda$. Then, there exists a threshold $\hat{X}$ such that subjects ignore the decision task if and only if $X^j \leq \hat{X}$, following the same arguments as in the proof of Proposition 1. This threshold will increase in $\lambda$. Moreover, for subjects with $X^j > \hat{X}$, $x^*_{D,Base}$ is given by:

$$-\pi'_B(X_j - x^*_{D,Base})u_B + (\lambda \pi'_D(x^*_{D,Base}, c) + (1 - \lambda) \pi'_D(x^*_{D,Base}, inc))u_D = 0.$$ 

The derivative of $x^*_{D,Base}$ with respect to $\lambda$ is:

$$\frac{dx^*_{D,Base}}{d\lambda} = -\frac{u_D(\pi'(x^*_{D,Base}, c) - \pi'(x^*_{D,Base}, inc))}{\pi'_B(x^*_{D,Base})u_B + (\lambda \pi'_D(x^*_{D,Base}, c) + (1 - \lambda) \pi'_D(x^*_{D,Base}, inc))u_D} < 0.$$

Hence, with increasing $\lambda$, it is optimal for individuals to withdraw resources from the decision task. Since it is optimal for individuals to use all their available cognitive resources, this directly implies that they allocate more resources to the background task. As a consequence, $\pi_B$ will increase with $\lambda$. 

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Appendix B  Supplementary Empirical Analysis

This section contains additional material and supplementary analysis. Figure B.1 provides CDFs of attention spans, separately for subjects with above- vs. below-median Raven scores. Table B.1 reports regression results on attention allocation and choice quality in the decision task in the different treatments in the BASELINE environment. Table B.2 presents regression results on differences in default adherence rates across treatments in the BASELINE environment.

Figure B.1: Heterogeneity in Attention Allocation

(a) Low Raven Score  
(b) High Raven Score

Note: The figure depicts cumulative distribution functions of attention spans (measured at the subject-round level) for subjects with below-median Raven scores [Panel (a)] and above-median Raven scores [Panel (b)].
Table B.1: Attention and Choice Quality in Baseline

<table>
<thead>
<tr>
<th></th>
<th>Avg. Attention</th>
<th>Attention = 0</th>
<th>Choice Quality</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Baseline-20</td>
<td>1.798</td>
<td>1.251</td>
<td>-0.059</td>
</tr>
<tr>
<td></td>
<td>(1.810)</td>
<td>(1.984)</td>
<td>(0.066)</td>
</tr>
<tr>
<td>Baseline-40</td>
<td>4.223*</td>
<td>2.964</td>
<td>-0.190***</td>
</tr>
<tr>
<td></td>
<td>(2.227)</td>
<td>(2.429)</td>
<td>(0.060)</td>
</tr>
<tr>
<td>Baseline-Ample</td>
<td>11.627***</td>
<td>12.295***</td>
<td>-0.282***</td>
</tr>
<tr>
<td></td>
<td>(1.174)</td>
<td>(1.480)</td>
<td>(0.035)</td>
</tr>
<tr>
<td>Constant</td>
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<td>9.702</td>
<td>0.307***</td>
</tr>
<tr>
<td></td>
<td>(0.961)</td>
<td>(6.135)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>Controls</td>
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<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>N</td>
<td>4060</td>
<td>4060</td>
<td>5960</td>
</tr>
<tr>
<td>No. Subjects</td>
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<td>203</td>
<td>298</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.171</td>
<td>0.253</td>
<td>0.100</td>
</tr>
<tr>
<td>Baseline-20=Baseline-40</td>
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<td>0.475</td>
<td>0.090</td>
</tr>
<tr>
<td>Baseline-20=Baseline-A.</td>
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<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td>Baseline-40=Baseline-A.</td>
<td>0.001</td>
<td>0.001</td>
<td>0.086</td>
</tr>
</tbody>
</table>

Note: The table reports results of OLS regressions of attention devoted to the decision task and choice quality in this task for the treatments in the Baseline environment. The dependent variable in Columns (1)–(2) is the number of seconds that a subject enters the decision task in a given round of the experiment. The dependent variable in Columns (3)–(4) is an indicator equal to 1 if a subject does not enter the decision task in a given round of the experiment and 0 otherwise. The dependent variable in Columns (5)–(6) is an indicator equal to 1 if a subject correctly solves the decision task in a given round of the experiment and 0 otherwise. Specifications with controls include the set of control variables specified in Table 2. The lower part of the table reports p-values from post-estimation tests of the equality of selected coefficients (Wald tests). Robust standard errors, clustered at the subject level, are reported in parentheses. ***, **, * indicates significance at the 1%, 5%, and 10% level, respectively.
### Table B.2: Default Adherence in Baseline

<table>
<thead>
<tr>
<th></th>
<th>Overall</th>
<th>Default incorrect</th>
<th>Default correct</th>
<th>Raven Low</th>
<th>Raven High</th>
<th>Raven Low</th>
<th>Raven High</th>
<th>Raven Low</th>
<th>Raven High</th>
<th>Raven Low</th>
<th>Raven High</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
<td>(9)</td>
<td>(10)</td>
<td>(11)</td>
</tr>
<tr>
<td>Baseline-20</td>
<td>-0.048</td>
<td>-0.029</td>
<td>-0.054</td>
<td>-0.042</td>
<td>-0.033</td>
<td>0.000</td>
<td>-0.027</td>
<td>-0.052</td>
<td>-0.044</td>
<td>0.036</td>
<td>(0.052)</td>
</tr>
<tr>
<td>Baseline-40</td>
<td>-0.204***</td>
<td>-0.187***</td>
<td>-0.237***</td>
<td>-0.224***</td>
<td>-0.127**</td>
<td>-0.100*</td>
<td>-0.242***</td>
<td>-0.249***</td>
<td>-0.074</td>
<td>-0.035</td>
<td>(0.052)</td>
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<td>Baseline-Ample</td>
<td>-0.272***</td>
<td>-0.280***</td>
<td>-0.381***</td>
<td>-0.377***</td>
<td>-0.019</td>
<td>-0.054**</td>
<td>-0.309***</td>
<td>-0.317***</td>
<td>-0.108**</td>
<td>-0.208***</td>
<td>(0.027)</td>
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<tr>
<td>Constant</td>
<td>0.591***</td>
<td>0.693***</td>
<td>0.476***</td>
<td>0.684***</td>
<td>0.859***</td>
<td>0.714**</td>
<td>0.636***</td>
<td>0.711***</td>
<td>0.451***</td>
<td>0.647***</td>
<td>(0.024)</td>
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#### Controls

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<tr>
<td>Baseline-20=Baseline-40</td>
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<td>0.015</td>
<td>0.023</td>
<td>0.018</td>
<td>0.136</td>
<td>0.102</td>
<td>0.018</td>
<td>0.044</td>
<td>0.762</td>
<td>0.421</td>
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<tr>
<td>Baseline-20=Baseline-A.</td>
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<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.667</td>
<td>0.269</td>
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<td>0.003</td>
<td>0.424</td>
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<td>Baseline-40=Baseline-A.</td>
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<td>0.176</td>
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<td>0.065</td>
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<td>0.539</td>
<td>0.628</td>
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**Note:** The table reports results of OLS regressions of treatment differences in default adherence for the treatments in the Baseline environment. The dependent variable is an indicator equal to 1 if a subject’s choice in a given round of the experiment coincides with the default option in the decision task and 0 otherwise. Columns (1)–(2) report results for the overall sample of subjects in the Baseline conditions; Columns (3)–(6) report separate estimations for situations in which the stipulated default option was incorrect/correct; Columns (7)–(10) report separate estimations for subjects with below-/above-median Raven scores. Specifications with controls include the set of control variables specified in Table 2. The lower part of the table reports p-values from post-estimation tests of the equality of selected coefficients (Wald tests). Robust standard errors, clustered at the subject level, are reported in parentheses. ***, **, * indicates significance at the 1%, 5%, and 10% level, respectively.