

Unbundling Autocratic Capture: Bundled Reform, Selective Emphasis, and Democratic Accountability

Alper Şükrü Gençer

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Abstract

Why do voters support reforms that weaken democratic oversight? I argue that incumbents can make harmful provisions easier to accept by packaging them with attractive or technical components and emphasizing the attractive parts, making the trade-offs embedded in the package harder to identify. Support for such reforms can therefore look like voters are trading democracy for other benefits, even when the democratic downside was never clear enough to shape their choices. Bundling turns the voters' decision into a judgment about a package; complexity makes the package harder to parse; selective emphasis highlights benefits while obscuring democratic risks. This process is especially consequential in captured media environments, where government-aligned outlets amplify promised benefits and make democratic risks harder to encounter. I test this argument in Türkiye, a hard case because voters have extensive experience with omnibus reform politics. Across randomized abstract choice tasks and political evaluation vignettes, bundled presentation worsens choice quality, selective emphasis can distort evaluation when the task is demanding, and oversight-weakening proposals gain support while recognition of their democratic costs weakens. The results show that democratic accountability can fail even before voters make a transparent trade-off, because whether they see what is at stake depends on how reforms are assembled, communicated, and contested.

1 Introduction

Why do voters sometimes support reforms that weaken separation of powers and democratic oversight? Take the case of Türkiye’s 2010 constitutional referendum, for example. The government presented the package through broadly appealing claims about democratization, civilianization, and institutional reform, yet it also changed the composition and appointment rules of key judicial bodies, including the Constitutional Court and the High Council of Judges and Prosecutors (Somer, 2019; Esen, 2021). The referendum was one constitutional example of a broader pattern. Much of Türkiye’s institutional change under the Justice and Development Party (AKP) moved through *torba yasalar*¹ and other reform packages, where oversight-weakening provisions were placed alongside technical, administrative, or publicly attractive measures (Kadioğlu, 2021; Özcan and Kimya, 2024). Existing accounts explain support for such democracy-weakening reforms through partisan loyalty, polarization, charismatic attachment, weak democratic commitments, or strategic trade-offs between democracy and other outcomes (Svolik, 2018; Graham and Svolik, 2020; Hollyer, Klačnja and Titiunik, 2023; Grossman et al., 2022; Krishnarajan, 2023; Luo and Przeworski, 2019; Grillo and Prato, 2023; Gratton and Lee, 2024). These explanations remain important, but they often begin after a demanding prior step: voters must recognize that an otherwise attractive reform package weakens separation of powers or democratic oversight before the democratic cost can shape their judgment. This problem is not confined to Türkiye. Hungary’s post-2010 constitutional transformation, India’s 2020 farm-law episode, and Israel’s 2023 judicial overhaul show how consequential institutional or policy changes can reach citizens through multi-component packages whose public defense emphasizes reform, efficiency, public benefit, or democratic restoration (Bánkuti, Halmai and Scheppele, 2012; Behl, 2022; Navot, 2023).

I argue that this failure can arise from the way reforms are bundled and presented, before voters decide whether they are willing to trade democratic oversight for other benefits. The packaging at the center of this argument is not unique to Türkiye: legislative scholars have long shown how omnibus bills and multi-issue reform packages combine otherwise separate provisions into a single proposal (Krutz, 2001; Sinclair, 2012). When a reform reaches voters as a package, its democratic implications must be understood from the combined effect of several provisions rather than from a single institutional change; as the proposal includes more components, policy domains, or technical details, that judgment becomes harder (Gabaix and Graeber, 2024; Oprea, 2020; Kendall and Oprea, 2024). This evaluation

¹ *Torba yasa* literally means “bag law” in Turkish. I use the term to refer to omnibus legislation that combines amendments across multiple statutes or policy domains into a single legislative package (Sav, 2014).

burden makes presentation more consequential because selective emphasis—the highlighting of promised benefits such as efficiency, security, service delivery, or democratization—can shape which parts of the package voters notice and which risks to oversight they miss. This emphasis should matter more in captured media environments, where government-aligned outlets repeat favorable claims and make warnings about weakened oversight harder to encounter, because voters then face a package whose benefits are easier to see than its democratic downside (Tversky and Kahneman, 1981; Bordalo, Gennaioli and Shleifer, 2012, 2013; Gabaix, 2014; Matějka and Tabellini, 2021). I develop this argument as a formal theory of bundle evaluation.

I test this argument in Türkiye using four randomized studies that include abstract tasks with known payoffs as well as realistic candidate-choice and municipal reform vignettes. Across the studies, package presentation increases suboptimal choice in abstract tasks compared with simpler presentation, while selective emphasis can further distort evaluation when the task is demanding. In the politically realistic choice tasks, executive-aggrandizing platforms and oversight-weakening reform packages gain more support while recognition of their democratic costs weakens. The central implication is that democratic resistance can fail even when citizens would reject a clearly identified threat to oversight, because reforms may be assembled and communicated in ways that make their democratic downside harder to understand.

This misevaluation gap between the reform citizens are formally asked to judge and the reform they actually understand at the moment of choice does not require voters to be indifferent to democracy, hostile to constraints, or blindly loyal to the actor proposing reform. The same package can produce several kinds of failure. Some voters may miss the oversight-weakening implication altogether. Others may understand that something changes, but fail to connect the provisions to separation of powers or democratic oversight. Still others may recognize the risk, yet give it less weight once it appears alongside promised benefits such as efficiency, security, or service delivery. In each case, democratic resistance weakens because citizens evaluate the reform without a clear understanding of what it would do to oversight and institutional constraint.

Türkiye is a useful case because it is not an idiosyncratic outlier: it belongs to a broader family of cases in which democratic erosion proceeds through formally legal, reformist, and multi-component institutional change. The ingredients required by the argument are widespread: incumbents in many backsliding settings weaken oversight through constitutional amendments, ordinary legislation, administrative reforms, or emergency measures that are defended through claims about efficiency, stability, security, democratization, or public benefit (Sebők, Kiss and Kovács, 2023; Druckman, 2024; Grillo et al., 2024). Within this

broader family, Türkiye is a demanding case because bundled reform is familiar rather than novel. The repeated use of *torba yasalar*, constitutional packages, emergency decrees, and executive-centered administrative reforms means that Turkish voters have repeatedly encountered institutional change in packaged form (Kadioğlu, 2021; Özcan and Kimya, 2024). This prior exposure matters: if packaging still shapes evaluation in this setting, the result is harder to dismiss as confusion caused by an unfamiliar experimental format. Türkiye also helps connect selective emphasis to real politics. As media capture deepened, citizens were more likely to encounter favorable descriptions of reform through government-aligned mainstream outlets than warnings about weakened oversight (Esen, 2021). The case therefore helps study a broader problem in contemporary backsliding, where citizens encounter formally legal reform packages in captured media environments that make promised benefits easier to encounter than their democratic implications (Shirikov, 2024).

The empirical design begins by isolating the evaluative problem and then asks whether the same pattern appears in political choice. The abstract tasks do this by stripping away partisan attachment, policy agreement, candidate appeal, and other political motivations that could explain support for a harmful reform. Respondents choose between options with known payoffs, so accepting the worse option can be interpreted as suboptimal choice rather than ideological agreement, partisan loyalty, or a conscious democratic trade-off. I then move to candidate-choice and municipal reform modules, where the same presentational features are embedded in more realistic political settings: complexity is varied through shorter versus longer platforms or packages, and selective emphasis is varied through neutral versus favorable presentation of attractive components. These modules preserve a fixed oversight-weakening contrast while bringing back what makes real reform choices difficult: voters may support a proposal because they miss the democratic cost, understand it only partially, recognize it but give it less weight inside a broader package, or perceive other benefits from the proposal.

Study 1 establishes the benchmark result: package presentation alone increases suboptimal choice. Respondents are 17.0 percentage points more likely to choose the objectively worse option when its consequences must be inferred from a bundle of three changes rather than directly as a simple option (95% CI: 12.2, 21.8), and they are less likely to identify the objectively better option when its value must be reconstructed from those changes. Study 2 then varies complexity and selective emphasis while holding payoff-relevant consequences fixed. The full-sample interactions are positive but imprecisely estimated. The clearest pattern appears among lower-capacity respondents² evaluating harmful bundles. Relative

²I use “lower-capacity” to refer to the preregistered cognitive-type classification described in the research design section. Among retained respondents, higher-capacity respondents answered both simple CRT items

to the low-complexity, neutral-presentation condition (three policy changes without selective emphasis), the high-complexity, selective-emphasis condition increases harmful-bundle acceptance by 11.6 percentage points (95% CI: 3.4, 19.8). Together, the abstract choice tasks show that presentation can impair evaluation even when the stakes are objective and experimentally controlled.

The third and fourth studies move to politically realistic choice tasks. Study 3 examines complexity and selective emphasis in a candidate-choice setting. Relative to the low-complexity, neutral-presentation condition, the high-complexity, selective-emphasis condition increases support for the candidate associated with an executive-aggrandizing commitment by 17.0 percentage points (95% CI: 11.1, 22.8), while recognition of the harmful implication falls by 7.9 percentage points in the same condition (95% CI: -14.1, -1.6). Study 4 then moves to a multidimensional municipal reform package. Relative to the two-policy, neutral-presentation condition, the four-policy, selective-emphasis condition increases package acceptance by 8.1 percentage points in the full sample (95% CI: 1.8, 14.3), while consequence recognition declines by an estimated 4.1 percentage points (95% CI: -8.8, 0.5). The political studies show that package presentation can increase support and weaken recognition, while also leaving room for more than one evaluative path: some respondents may miss the democratic cost, others may understand it only partially, and others may recognize it but give it less weight inside a broader package.

The paper makes three main contributions. First, it develops a formal theory of bundled policy evaluation that connects work on complexity, rational inattention, sparse attention, and salience. Existing models of inattention explain why decision makers economize on costly information processing, while salience models explain why prominent attributes receive disproportionate weight (Sims, 2003; Gabaix, 2014, 2019; Maćkowiak, Matějka and Wiederholt, 2023; Bordalo, Gennaioli and Shleifer, 2012, 2022). I bring these ideas together by distinguishing the quantity of attention from its composition: voters may devote more or less attention to a reform package, but presentation shapes which provisions that attention reaches. Complexity raises the burden of understanding the package, while selective emphasis changes which consequences voters are most likely to notice and use. The main text develops the theory’s logic and empirical implications, while Appendix A presents the full model and proofs. Although the empirical application centers on democratic backsliding, the theory applies more broadly to political choices that arrive as packages: campaign platforms, omnibus legislation, policy reforms, and proposals that combine popular benefits with more

correctly and at least one harder reasoning item correctly; lower-capacity respondents are those who do not meet that threshold. The label is used only to examine heterogeneity in the evaluative burden of reconstructing bundles, not to rank respondents more broadly.

extreme or harder-to-evaluate components. In these settings, political communication can shape not only how voters feel about a proposal, but also what they understand the proposal to contain.

Second, the paper clarifies a recognition problem at the center of democratic accountability. Existing work shows that citizens may tolerate democratic violations because of partisan loyalty, polarization, weak democratic commitments, or trade-offs between democracy and other valued outcomes (Svolik, 2018; Graham and Svolik, 2020; Grossman et al., 2022; Grillo and Prato, 2023; Braley et al., 2023). Recent work on democratic backsliding also emphasizes that erosion depends on the weakening of constraints on elected executives, the actors who can restrain them, and the psychological processes through which citizens respond to democratic threats (Druckman, 2024; Grillo et al., 2024). This paper asks whether voters recognize the oversight-weakening or executive-aggrandizing provisions inside omnibus reforms clearly enough for accountability or democratic trade-off theories to operate. This shifts attention to a recognition condition that trade-off accounts often leave implicit: voters can only knowingly tolerate democratic costs when those costs have become visible enough to enter their judgment.

Third, the paper gives a demand-side account of why legislative packaging matters for democratic backsliding. Research on omnibus legislation and policy bundling emphasizes how elites assemble coalitions, protect controversial provisions, or move otherwise separate measures through a single proposal (Krutz, 2001; Sinclair, 2012; Lee, 2022). Work on legislative backsliding shows that democratic erosion can also proceed through ordinary law, including changes in policy content, legal-constitutional form, legislative procedure, and institutional stability (Sebők, Kiss and Kovács, 2023). I connect these perspectives by showing that packaging can also matter after a proposal reaches citizens. By changing how reform is encountered, bundling, complexity, and selective emphasis can alter whether voters recognize a proposal’s oversight-weakening consequences and how those consequences enter political judgment.

The remainder of the paper proceeds as follows. The next section develops the theoretical framework, with the full formal model and proofs in Appendix A. I then situate the argument in Türkiye’s reform politics and explain why Türkiye provides a demanding setting in which to study the proposed mechanism. The research design section explains the experimental sequence. The empirical sections move from the abstract bundling benchmark to the core complexity-by-emphasis test, then to candidate choice and municipal reform evaluation. The final section discusses what the results imply for democratic backsliding and reform evaluation.

2 Theoretical Framework

Citizens can punish democratic backsliding only when they identify what a proposal actually does to oversight and institutional constraint. Existing trade-off accounts usually begin after that step: voters see a democratic cost and decide whether to accept it for partisan, policy, or expressive reasons (Svolik, 2018; Graham and Svolik, 2020; Grossman et al., 2022; Grillo and Prato, 2023; Braley et al., 2023). This paper moves one step earlier. When a reform reaches citizens as a package, the democratic cost may have to be reconstructed from a larger set of provisions before it can shape what they learn and whether they support or oppose the reform. I use reconstruction to mean the evaluative step in which citizens infer the relevant consequences of a package from its components before forming a judgment. A voter who misses that cost, identifies it only partly, or sees it less clearly than the attractive parts of the package is not making the same choice as a voter who knowingly accepts democratic harm.

Bundling makes the prior step harder because it changes what voters are asked to judge. A stand-alone provision can be considered on its own. A package asks citizens to form a judgment over several provisions at once, and the burden grows as the package includes more issues, policy domains, and consequences. Legislative scholars show that elites use omnibus packages to build coalitions, protect controversial measures, and move provisions that might struggle on their own (Krutz, 2001; Sinclair, 2012). The demand-side implication is that packaging can make the consequences of a proposal harder to identify. A voter may understand some parts of the package while missing, misclassifying, or only partly identifying others, including provisions that alter oversight or executive discretion.

H1. *Relative to direct presentation of the same final consequences, bundled presentation increases suboptimal choice because citizens must reconstruct the package before evaluating it.*

Complexity captures how demanding the package is once reform is presented in bundled form. Some packages contain only a few clear provisions; others are longer, more technical, or spread consequences across several policy domains. Work on complexity, sparse attention, and rational inattention shows that more demanding choice environments can make otherwise engaged evaluation less complete or less reliable (Gabaix, 2014; Oprea, 2020; Kendall and Oprea, 2024; Gabaix and Graeber, 2024). In the model here, this happens because additional issues and domains give the same effort more to cover. Citizens may spend more time or seek more information, but that effort can still leave them with less clarity about each component.

H2. *Higher complexity reduces clarity about the components of a package. This can increase*

evaluation error when citizens lose sight of decision-relevant consequences, even if they spend more effort evaluating the package.

The quantity-composition distinction matters because greater effort is not the same as more complete understanding. A harder package may induce more time, more clicks, or greater effort, yet still leave citizens with a less complete understanding of what the reform does. As the package becomes longer, more technical, or more multidimensional, the same amount of effort buys less scrutiny per unit of burden. Under those conditions, presentation becomes more consequential: when attractive components are emphasized while institutional costs remain less salient, citizens may understand the attractive parts of the reform more clearly than its costs. The result is not necessarily disengagement. Citizens can work at evaluating a reform and still come away with a partial, presentation-shaped version of its consequences.

Selective emphasis changes which parts of a package citizens are most likely to identify. Governing actors, campaigns, and government-aligned media rarely need to deny that a reform contains many provisions. They can instead describe the package through the parts that are easiest to defend: service delivery, administrative repair, security, rights, efficiency, or material benefits. This logic is related to framing and shortcut-based accounts of opinion formation, but it operates one step earlier. Framing research shows that presentation can change which considerations citizens use when forming opinions (Tversky and Kahneman, 1981; Kahneman and Tversky, 1984; Druckman, 2001; Chong and Druckman, 2007), and work on information shortcuts shows how accessible signals can substitute for direct evaluation (Zaller, 1992; Lupia, 1994). In bundled reform, however, the prior problem is identifying what the package contains. Selective emphasis can shape which components enter judgment clearly enough to be weighed in the first place. If citizens identify the attractive components more clearly than the oversight cost, support can rise because the package they judge is incomplete.

H3. *Selective emphasis increases support for a harmful package when attractive components are made easier to identify than costly institutional components.*

The model places an important restriction on this prediction. Selective emphasis should have little room to matter when the package is simple enough for citizens to identify both the attractive components and the institutional cost. It should also weaken when the package is so difficult that citizens identify little meaningful content at all. The effect should be most visible in the engaged range: the package is difficult enough to produce uneven identification, but still clear enough for emphasized components to enter judgment.

Cognitive capacity also matters because evaluating a package requires effort. Some re-

spondents can inspect several provisions with less strain; others face greater difficulty when the same package contains multiple components, technical language, or consequences spread across domains. While citizens remain engaged, this difference should affect what they identify inside the package. Lower-capacity respondents should identify fewer components and rely more on the components made easiest to see. When governing actors, campaigns, or aligned media emphasize attractive parts of the package, those parts should therefore play a larger role in lower-capacity respondents' judgments.

H4. *Lower-capacity citizens are more vulnerable to selective emphasis in the engaged range, because the same package leaves them with less clarity about its components.*

The same logic also cautions against treating lower capacity as a fixed ranking of vulnerability. Under severe complexity, some lower-capacity respondents may disengage and stop identifying meaningful content, including the emphasized components. Higher-capacity respondents may continue trying to evaluate the package, but their understanding can still become uneven: they identify some components, especially the emphasized ones, while missing or only partly identifying others. For this reason, the subgroup analyses should be read together with engagement and recognition outcomes rather than as a simple test of which group is always more susceptible.

Appendix A formalizes this theory. A reform package has full consequences, denoted by V , but citizens act on the version of the package they identify at the moment of judgment, denoted by $\hat{V}$. Bundling introduces the reconstruction problem by requiring citizens to infer consequences from components; additional complexity raises the burden of that reconstruction; selective emphasis changes which components are easiest to identify; and capacity affects how costly evaluation is for different respondents. Support depends on $\hat{V}$, while recognition depends on whether the harmful component enters judgment clearly enough to be reported. The hypotheses above therefore describe when $\hat{V}$ should depart from V , and when that departure should make harmful packages more acceptable.

The empirical sequence tests these expectations in complementary ways. Study 1 isolates the benchmark implication of H1 by comparing bundled presentation with direct presentation of objectively ranked consequences. Study 2 provides the cleanest controlled tests of H2–H4 by varying complexity and selective emphasis while holding payoff-relevant consequences fixed and examining heterogeneity by cognitive capacity. Studies 3 and 4 then move these presentational features into political settings, where support and recognition can diverge and where respondents may recognize a cost only partially or give a recognized cost less weight in their final judgment. Table 1 summarizes the relationship between the theoretical expectations and the four studies.

Table 1: Theoretical expectations and empirical tests

Hypothesis	Expectation	Primary test	Political extension
H1	Bundled presentation impairs evaluation relative to direct presentation of the same consequences.	Study 1	Studies 3–4 examine the same evaluative problem once consequences are embedded in political platforms and reform packages.
H2	Greater complexity reduces clarity about the components of a package.	Study 2	Studies 3–4 examine whether more complex political presentations alter support and recognition.
H3	Selective emphasis makes harmful packages more acceptable when attractive components are easier to identify than costly institutional components.	Study 2	Studies 3–4 examine whether favorable emphasis changes support and recognition in political choices.
H4	Lower-capacity citizens are more vulnerable to selective emphasis while they remain engaged.	Study 2	Capacity comparisons in Studies 3–4 provide secondary evidence about heterogeneity in political evaluation.

The theory makes a specific claim about democratic accountability: citizens may spend effort evaluating a reform and still judge an incomplete version of it. Complexity affects how clearly they identify the components of a package; selective emphasis affects which components become easiest to identify; and capacity affects how strongly those burdens are felt. Support for a harmful reform can rise without requiring citizens to prefer institutional harm, because the package they evaluate may differ from the package’s full consequences.

3 Türkiye as a Scope-Condition Case

Türkiye is useful for this paper because, during the AKP period, governing actors often advanced changes to judicial oversight, executive discretion, emergency authority, policing, and media regulation through legal reform packages rather than openly anti-democratic measures. They packaged oversight-weakening provisions with technical, administrative, distributive, or rights-oriented changes, while government officials and government-aligned media described these packages through favorable language: modernization, security, efficiency, stability, democratization, or administrative necessity. Türkiye therefore belongs to a broader family of legalistic backsliding cases in which incumbents weaken oversight through constitutional amendments, omnibus laws, decree-laws, or administrative reforms. It is also a demanding case for the experiments because packaged reform is familiar rather than novel. The studies ask whether bundled presentation and selective emphasis still shape evaluation in a setting where citizens already know this kind of politics.

The key legislative form is the *torba yasa*, or omnibus law. The term refers to laws that gather amendments across multiple statutes or policy domains into a single legislative package. Legal scholarship describes this practice as a de facto lawmaking form that became especially important after 2001 and helped shift legislative initiative toward the executive (Sav, 2014; Haspolat, 2016). A provision that weakens oversight or expands discretion can appear inside a broader reform that also includes technical fixes, administrative changes, distributive benefits, or rights-oriented measures. Citizens then evaluate the package as a whole, while the institutional consequences of one provision must be separated from the surrounding measures before they can shape judgment.

Torba yasa was a routine feature of Turkish lawmaking rather than an occasional tactic. Drawing on information from the Turkish parliament, BBC Türkçe reported that only two omnibus laws were passed during the AKP’s first governing period from 2002 to 2007, while twenty-three were passed between November 2015 and November 2017; by late 2017, almost one in every two laws passed in that legislative period was an omnibus law (Erem, 2017). Critics objected to the practice because the package form made separation difficult: controversial provisions could be placed beside less controversial ones, and legislators could be forced to vote up or down on a package containing both provisions they supported and provisions they opposed. The same problem matters for citizens. When reform arrives as a package, recognizing its democratic cost requires connecting particular provisions to oversight, executive discretion, or institutional constraint.

The 2010 constitutional referendum gives a clear example. The ballot placed rights-oriented reforms and judicial-governance changes inside one constitutional package. Rights-

oriented components included individual application to the Constitutional Court; oversight-relevant components changed the composition and appointment rules of key judicial bodies, including the Constitutional Court and the HSYK (Somer, 2019; Esen, 2021). Citizens therefore evaluated judicial reform as an integrated package. Support for the rights-oriented parts could coexist with weaker attention to, uncertainty about, or disagreement with the judicial-governance changes. The democratic cost entered the vote as one part of a broader reform package rather than as a separable proposal.

Other episodes show how routine legalistic reform politics became. Law No. 6524 changed the governance of the HSYK in technical legal-administrative terms. The 2014–2015 internet-law packages expanded blocking powers through privacy and public-order language. The 2015 Internal Security Package expanded police authority through a broader security reform frame. Emergency decree-laws after 2016, including Decree-Law No. 674, made lasting institutional changes under emergency authority, while the 2017 constitutional amendment package consolidated executive power through a multi-article constitutional proposal defended partly through efficiency and stability. Together, these episodes show how changes to courts, police powers, internet regulation, emergency governance, and executive authority often reached citizens through reform packages, technical legal language, emergency authority, and favorable public-facing claims (Kadioğlu, 2021; Özcan and Kimya, 2024; Esen, Gumuscu and Yavuzylmaz, 2023).

The media environment made this evaluative task harder. During the AKP period, major newspapers and television channels increasingly came under the ownership or influence of business groups close to the government. Scholars describe this transformation as media capture, involving ownership changes, regulatory pressure, legal intimidation, and state-linked advertising used to reward supportive outlets and punish critical ones (Yeşil, 2016; Coşkun, 2020; Yanatma, 2021; Akser and Baybars, 2023). The 2018 sale of Doğan Media Group to Demirören Holding was especially important because it transferred major outlets such as *Hürriyet*, CNN Türk, Kanal D, and DHA to a conglomerate widely described as government-aligned (Reporters Without Borders, 2018). As media capture deepened, citizens were more likely to encounter favorable descriptions of reform through government-aligned outlets: efficiency, security, democratization, service delivery, administrative order, or crisis response. Detailed criticism of what a package did to courts, oversight bodies, local authority, or executive discretion became harder to encounter and more costly to verify (Yanatma, 2021; Çarkoğlu and Yıldırım, 2024). In this setting, selective emphasis matters because citizens first have to identify which parts of a broader package change accountability, and those parts may be less visible than the benefits repeatedly attached to the reform.

The experiments do not try to reproduce Turkish politics as a whole. They isolate the

presentation problem that recurs in this setting: governing actors place oversight-weakening provisions inside broader reform packages, and citizens encounter those packages through language that makes some components easier to notice than others. By varying presentation while holding the relevant consequences fixed where the design permits, the studies ask whether harmful options become more acceptable when citizens encounter them as reform packages rather than as isolated institutional costs. The Turkish case makes this a demanding test because packaged reform is politically familiar. If bundled presentation and selective emphasis shape evaluation in this setting, the results are harder to attribute to novelty or confusion about the experimental format. The case also clarifies where similar dynamics should matter: legalistic backsliding settings where oversight-weakening changes are embedded in broader reform packages and communicated through publicly defensible claims.

4 Research Design Overview

The first two studies deliberately begin with abstract, incentivized lottery choices. This abstraction makes it possible to isolate evaluation from political motivations that could otherwise explain support for a harmful option, including partisan attachment, policy agreement, candidate appeal, or a willingness to accept institutional costs for other benefits. Because the lottery payoffs are known and objectively ranked, choosing a strictly worse option can be interpreted as an evaluation error rather than as a difference in political preferences. Study 1 establishes the bundling benchmark by comparing direct presentation of the final consequences with a low-complexity bundle in which those same consequences must be inferred from three changes. Study 2 then holds bundling constant and increases complexity, comparing three-change bundles with five-change bundles while also varying selective emphasis. This sequence separates the effect of requiring respondents to reconstruct the final consequences from the additional effect of increasing the complexity of that task, while allowing Study 2 to test whether higher complexity becomes more consequential for evaluation under selective emphasis.

Studies 3 and 4 then reintroduce political content while preserving the same basic presentational logic. Study 3 embeds an oversight-weakening commitment in a candidate platform and varies platform complexity and selective emphasis. Study 4 moves to a multidimensional municipal reform package, again varying complexity and selective emphasis. These political tasks trade the objective error benchmark of the lottery studies for greater political realism. Support for a candidate or reform package can reflect failed recognition, partial recognition, a willingness to accept a recognized institutional cost, or some combination of these paths. I therefore examine support and recognition together rather than treating every instance of support for the harmful political option as an evaluation error.

Across the four studies, I measure final judgments alongside what respondents identify from the task. The main outcomes are suboptimal lottery choice, harmful-bundle acceptance, support for the candidate tied to an oversight-weakening commitment, acceptance of the municipal reform package, and recognition of the harmful implications in the political tasks. I also measure perceived difficulty, confidence, time, and clicks to characterize how respondents experience the evaluative burden created by the treatments. I use these measures as supporting evidence about task engagement and difficulty rather than as separate tests of the mechanism.

Table 2: Study sequence and inferential role

Study	Setting		Main comparison	Main judgment	Inferential role
Study 1	Incentivized choice	lottery	Bundled presentation vs. direct stand-alone presentation	Choice of the lower-value option	Tests whether bundling-induced reconstruction impairs evaluation.
Study 2	Incentivized choice	lottery	Higher vs. lower complexity; selective emphasis vs. neutral presentation	Choice of the worse option and acceptance of the harmful bundle	Tests whether complexity and favorable emphasis jointly change evaluation under fixed payoffs.
Study 3	Candidate choice		Longer vs. shorter candidate platforms; favorable vs. generic summary headline for the direct-sign-off candidate	Support for the candidate tied to a harmful institutional commitment; recognition of that commitment	Tests whether platform length and headline emphasis change electoral support and recognition.
Study 4	Municipal package	reform	Two- vs. four-policy package; neutral vs. favorable policy headlines	Acceptance of the reform package; recognition of harmful consequences	Tests the logic in a multidimensional reform task closer to real policy evaluation.

The studies were fielded as modules within the same survey. Respondents therefore come from the same recruitment pool and share the same pre-treatment measures as the tasks move from abstract lotteries to political reform evaluation. The studies should be read as an experimental sequence rather than as independent replications.

4.1 Sample, Recruitment, and Data Collection

The studies use original online data from adult Turkish citizens who could complete the survey in Turkish. Respondents were recruited through paid Meta advertisements and directed to a Qualtrics survey, where they provided consent before completing the randomized experimental modules and the pretreatment measures used for heterogeneity analyses. This recruitment strategy was designed to estimate treatment effects within a recruited online sample, not to estimate population prevalence of support for any particular reform, candidate, or institutional view.

The raw data export contains 9,904 records. Of these, 2,150 respondents provided permission for data use and completed the survey. The main analytic specification excludes

responses completed in less than 450 seconds, responses longer than 6,000 seconds, and respondents who failed the pretreatment CRT-based quality threshold, producing a restricted analytic sample of 1,871 respondents. The 450-second lower threshold and CRT screen were specified in the preregistration; the 6,000-second upper threshold was added to exclude sessions likely to contain extended interruptions or unattended browser time. Because total survey duration is measured after treatment exposure, Appendix D.5 reports parallel estimates under alternative inclusion rules, including a specification that removes the upper-duration cutoff. The substantive conclusions are unchanged across these specifications.

The final restricted analytic sample contains 1,871 respondents. Study 1 has a smaller sample of 1,642 respondents because the module was introduced after the pilot wave and therefore was not administered to respondents in that wave. Studies 2–4 share the restricted analytic sample of 1,871 respondents. Appendix D.5 reports the corresponding sample sizes under the alternative inclusion rules.

The randomized modules were fielded in a fixed sequence within the same survey. To reduce confounding between task order and complexity exposure, the design counterbalanced complexity across the abstract and political modules: respondents assigned to higher-complexity presentation in the lottery-bundle module were assigned to lower-complexity presentation in the candidate and municipal modules, and respondents assigned to lower-complexity lottery bundles were assigned to higher-complexity political modules. Selective-emphasis assignment is analyzed using the study-specific treatment indicators corresponding to the presentation shown in each module. The estimates therefore use the randomized presentation variation within each study, while the appendix reports balance and assignment diagnostics separately for each article-labeled study.

The main design and analysis plan were preregistered on OSF on March 28, 2026, under the registration titled *Unbundling Autocratic Capture* (OSF registry code `py2xc`; project link: osf.io/q4su5). The study was reviewed under NYU IRB protocol IRB-FY2025-10304. Rather than listing every preregistered design element here, I report the relevant departures from the registration directly. One sample-construction difference is the treatment of post-treatment duration restrictions: the main restricted sample remains the main specification, while Appendix D.5 reports estimates under alternative inclusion rules to assess whether the duration rule affects the results. Appendix D.13 summarizes how the analyses reported in the article map onto the preregistration, identifies departures from the registered plan, and notes preregistered components that are reported only in the appendix or fall outside the scope of the present article.

Pilot waves were used to inform instrument revisions. The main analyses pool only waves and modules whose assignment structure, treatment conditions, and outcome measures are

comparable.

The estimand is the average treatment effect within the recruited survey sample: the average difference between the potential outcome under one presentation and the potential outcome under the comparison presentation. Random assignment identifies this estimand by making the assigned presentation independent of respondents’ pretreatment characteristics. I therefore interpret the estimates as internally valid treatment effects for this sample, while treating external representativeness as a scope condition rather than a design assumption.

The survey recorded both final judgments and process measures. The final judgments are whether respondents choose the worse lottery option, accept a harmful lottery bundle, support a candidate tied to a harmful institutional commitment, accept the municipal reform package, and recognize the harmful implication of the proposal. The process measures are time spent on task, clicks for additional information, perceived difficulty, and confidence. I use these measures as supporting evidence about task engagement and difficulty. Full process-outcome estimates are reported in Appendix D.10.

4.2 Cognitive Types

The capacity analyses use a pretreatment cognitive-performance measure specified in the preregistration. The five Cognitive Reflection Test (CRT) items were administered before treatment assignment and before respondents entered the randomized experimental modules. They therefore cannot be affected by the complexity or selective-emphasis treatments. Two items are used as basic pretreatment screen items, and three items are harder reasoning items. Appendix D.12 reports the exact item wording, correct answers, and coding rule.

The preregistered CRT-based inclusion rule excludes respondents who missed both basic pretreatment screen items. This exclusion changes the respondent population to which the restricted-sample estimates apply, but because the items were measured before treatment assignment, it does not create treatment-induced selection. Among retained respondents, I classify respondents as higher-capacity if they answered both basic items correctly and at least one of the three harder reasoning items correctly. Respondents who passed the basic-CRT-item inclusion rule but do not meet this higher-capacity threshold are classified as lower-capacity.

I use this distinction only to examine heterogeneity in treatment response. It is not meant to rank respondents more broadly. The measure captures a pretreatment difference in performance on reasoning items relevant to the theory: how costly it is to identify the consequences of a package when those consequences are spread across several components. Because the preregistration treated the lower-cognition joint pattern as less certain in sign,

I interpret subgroup estimates as evidence about where the evaluative burden binds most clearly rather than as the paper's main confirmatory evidence.

5 Study 1: Bundling Benchmark

5.1 Design

Study 1 begins with the simplest version of the problem. Before introducing political content, selective emphasis, or variation in bundle size, it asks whether bundling itself creates enough evaluative complexity to reduce respondents’ ability to identify the better option. In the direct-comparison baseline, respondents see each lottery’s final composition. In the bundled conditions, one option must instead be reconstructed from a starting bowl and three changes (see Table 3). In this lottery task, reconstruction means calculating the final bowl implied by the starting bowl and the displayed changes before comparing it with the directly shown alternative. The treatment therefore captures the complexity introduced by bundling: the relevant consequences are no longer directly visible, even though the bundle is short and its size is held fixed. Study 2 then asks whether this problem deepens when the bundle itself becomes larger and respondents must process more changes.

Respondents choose between two bowls of colored balls. After they choose, one ball is drawn at random, and the color of the drawn ball determines their experimental credits: Purple pays 60 credits, Blue pays 30 credits, and Gray pays zero. Each option ultimately produces a final bowl with 20 balls. One final bowl is strictly better: it contains 4 Purple, 5 Blue, and 11 Gray balls, for an expected value of 19.5 credits. The other is strictly worse: it contains 3 Purple, 2 Blue, and 15 Gray balls, for an expected value of 12 credits. The lower-value bowl is not a safer alternative or a trade-off against risk; it has fewer high-paying balls, fewer intermediate-paying balls, and more zero-paying balls. A respondent therefore makes a suboptimal choice when they select the lower expected-value bowl.

The three treatment arms vary whether those same final lotteries are directly visible or must be understood from components. In the direct-comparison baseline, both final bowls are shown directly. In the lower-value bundled condition, the better lottery is shown directly, while the worse lottery is shown as a starting bowl plus three changes that produce the worse final bowl. In the higher-value bundled condition, the worse lottery is shown directly, while the better lottery is shown as a starting bowl plus three changes that produce the better final bowl. Table 3 summarizes the comparison. Full condition details and payoff calculations are reported in Appendix Tables 14 and 15; the full survey screen is reported in Appendix C.

This design distinguishes directional reactions to bundled presentation from an evaluation problem created by reconstruction. If respondents simply favor bundled options, they should become more likely to choose the bundled alternative regardless of whether it is better or worse. If they avoid bundled options because they dislike complexity, they should become

less likely to choose the bundled alternative regardless of its value. By contrast, if bundling makes evaluation harder by forcing respondents to reconstruct the final bowl, it should reduce choice quality in both directions: respondents should become more likely to accept a worse bundled option and less likely to identify a better bundled option.

Table 3: Study 1 treatment arms and treatment presentations

Treatment arm	Strictly better option	Strictly worse option
Direct comparison	Direct final bowl: 4P, 5B, 11G. Expected value = 19.5.	Direct final bowl: 3P, 2B, 15G. Expected value = 12.
Lower-value bundled	Direct final bowl: 4P, 5B, 11G. Expected value = 19.5.	Bundled presentation. <i>Start:</i> 4P, 5B, 11G. <i>Changes:</i> -2G, +3B, -2P; +4P, +1G, -4B; -2B, -3P, +5G. <i>Implied final:</i> 3P, 2B, 15G. Expected value = 12.
Higher-value bundled	Bundled presentation. <i>Start:</i> 3P, 2B, 15G. <i>Changes:</i> +2B, +3P, -5G; -4P, -1G, +4B; +2G, -3B, +2P. <i>Implied final:</i> 4P, 5B, 11G. Expected value = 19.5.	Direct final bowl: 3P, 2B, 15G. Expected value = 12.

Note: P = Purple, B = Blue, and G = Gray. The column labels identify the payoff ranking for the reader; respondents saw generic option labels rather than “better” and “worse.” Option placement was randomized. In the bundled conditions, respondents saw a starting bowl and a sequence of three changes, then had to infer the final bowl before comparing it with the directly shown alternative. The lower expected-value option is strictly worse, not a safer alternative: it contains fewer high-payoff balls, fewer medium-payoff balls, and more zero-payoff balls.

The main outcome is suboptimal choice. I estimate linear probability models in which the dependent variable equals one when the respondent chooses the lower expected-value lottery. The main specification compares the bundled arms to the direct-comparison baseline and includes the preregistered risk-aversion measure as a covariate. Because each respondent contributes one Study 1 decision, I use heteroskedasticity-robust standard errors and report effects in percentage points. I also examine perceived difficulty, confidence, time, and clicks as secondary process measures that characterize how respondents experienced the task.³

³Process outcomes are estimated with the same treatment specification. Treatment effects are reported in standardized outcome units; the Baseline column reports difficulty and confidence on their original 1–7 scales, time in the original recorded units, and clicks as raw counts. Full process-outcome estimates are reported in Appendix D.10. Because these measures are secondary and may reflect several aspects of task experience, I use them to characterize engagement and difficulty rather than as separate tests of the mechanism.

5.2 Results

Study 1 shows that bundling alone is enough to impair evaluation even when the underlying choice contains no payoff trade-off. In the direct-comparison baseline, where both options are visible as simple lotteries, 26.4 percent of respondents choose the objectively inferior option. When one of the same final lotteries must instead be inferred from a three-change bundle, suboptimal choice increases by 17.0 percentage points, with a 95 percent confidence interval from 12.2 to 21.8 points.

Figure 1 shows that when the lower-value option is bundled, respondents become more likely to accept it: suboptimal choice rises by 14.0 percentage points (95% CI: 8.3, 19.6). When the higher-value option is bundled, respondents become less likely to identify it as the better option: suboptimal choice rises by 20.0 percentage points (95% CI: 14.4, 25.7). Bundling therefore does more than make respondents favorable toward whichever option is bundled. It makes them worse at distinguishing better from worse options when the payoff-relevant final bowl must be inferred rather than inspected directly.

The capacity pattern is especially informative. Higher-capacity respondents perform much better in the direct-comparison baseline, but their advantage shrinks sharply once an option is bundled. For this group, bundling the lower-value option increases suboptimal choice by 22.1 percentage points (95% CI: 13.3, 30.8), and bundling the higher-value option increases suboptimal choice by 24.2 percentage points (95% CI: 15.4, 33.1). Among lower-capacity respondents, the corresponding estimates are 8.8 percentage points (95% CI: 1.4, 16.2) and 17.2 percentage points (95% CI: 9.7, 24.6). The smaller lower-capacity estimates should be read against a much higher baseline error rate. Higher-capacity respondents have more room to fall because they begin from a more accurate baseline.

This benchmark establishes the paper’s first claim: when citizens must infer the consequences of a package, the format of presentation can change the quality of evaluation before any political content, selective emphasis, or partisan signal enters the task. Bundling affects more than respondents who are already prone to error. It can also erode the advantage of respondents who evaluate simple alternatives accurately.

The process outcomes point in the same direction. Bundled options lead respondents to spend more time, click more, report greater difficulty, and express lower confidence. These patterns support an evaluative-burden interpretation: respondents are not merely ignoring the task, but the additional effort does not fully offset the difficulty of aggregating the option mentally. Study 1 supplies the baseline for the later experiments by showing that bundling alone creates an evaluative vulnerability. Study 2 asks whether that vulnerability deepens when the bundle becomes more complex and attractive components receive selective

emphasis.

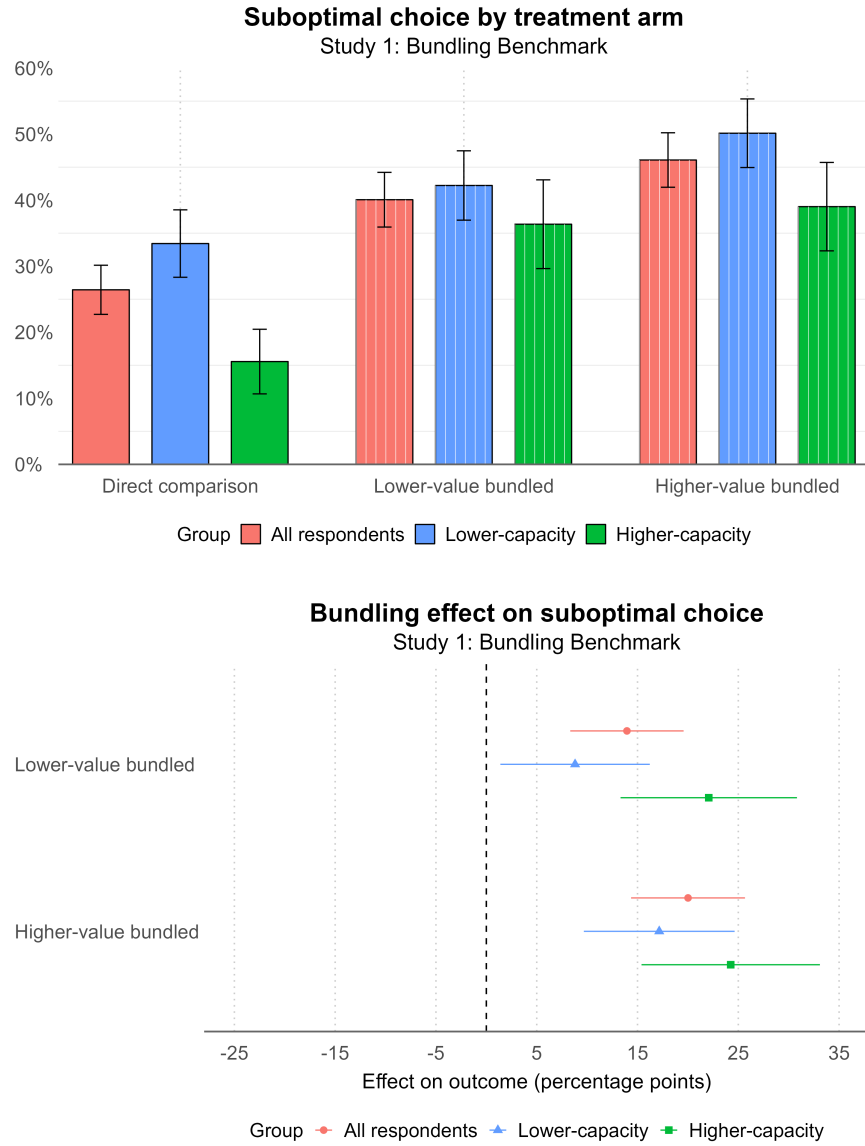


Figure 1: Study 1

Note: The analytic sample contains 1,642 respondents. The top panel reports treatment-arm means for suboptimal choice, coded as one when the respondent chooses the lower expected-value lottery. The bottom panel reports linear probability model estimates relative to the direct-comparison baseline. The specification includes the preregistered risk-aversion measure as a covariate; standard errors are heteroskedasticity-robust. Baseline suboptimal-choice rates in the direct-comparison condition are 26.4 percent among all respondents, 15.6 percent among higher-capacity respondents, and 33.4 percent among lower-capacity respondents.

6 Study 2: Complexity and Selective Emphasis

6.1 Design

Study 2 keeps the choice task abstract but varies how difficult it is to infer the value of a bundled option. Study 1 showed that bundling can impair evaluation because one option must be inferred from a starting point *and* a sequence of changes, raising the complexity and cost of evaluation. Study 2 asks whether that problem worsens when the inference task becomes more demanding and attractive changes are selectively emphasized.

Respondents again make incentivized lottery choices. In each task, they compare a directly shown status quo bowl with one bundled alternative. The bundled alternative is shown as a starting bowl plus a sequence of rule changes that respondents must use to infer the final bowl. The bundle can be harmful, meaning that its final outcome is strictly worse than the directly shown alternative, or beneficial, meaning that its final outcome is strictly better. This structure keeps the payoff ranking objective: there is no induced payoff trade-off between the two options.

The design varies two features of the bundled alternative. The first is complexity. Study 2 manipulates complexity by changing the number of rule lines respondents must process. Low-complexity bundles contain three rule lines; high-complexity bundles contain five. In both cases, respondents must infer the final payoff-relevant bowl from all rule changes before comparing it with the directly shown alternative.

The second feature is selective emphasis. In the neutral condition, all rule changes are displayed without visual emphasis. In the selective-emphasis condition, attractive changes are highlighted with bold, colored font while the full rule list remains available. This manipulation does not change the final payoff-relevant bowl. It changes which parts of the bundle are easiest to notice while respondents infer the outcome.

Table 4 summarizes the treatment structure. In every task, respondents compare a directly shown status-quo bowl with one bundled alternative. The treatment varies whether that bundled alternative is described with three or five rule lines and whether attractive changes are visually emphasized. In the harmful-bundle task, the question is whether a net-worse bundle becomes more acceptable when the inference task is harder and favorable changes are highlighted. In the beneficial-bundle task, the question is whether a strictly better bundle becomes harder to identify under the same presentation conditions.

Table 4: Study 2 treatment cells and rule changes

Treatment cell	Directly shown alternative	Rule changes shown for bundled alternative	Implied final bundle
Low complexity / neutral / harmful bundle	Task A: 2P, 6B, 12G. Task B: 2P, 4B, 14G.	1. +2B, -1P, +3G. 2. +2P, +3G, -5B. 3. -4G, +2B, -2P.	Task A: 1P, 5B, 14G. EV = 10.5. Task B: 1P, 3B, 16G. EV = 7.5.
Low complexity / selective emphasis / harmful bundle	Task A: 2P, 6B, 12G. Task B: 2P, 4B, 14G.	1. +2B , -1P, +3G. 2. +2P , +3G, -5B. 3. -4G , +2B , -2P.	Task A: 1P, 5B, 14G. EV = 10.5. Task B: 1P, 3B, 16G. EV = 7.5.
High complexity / neutral / harmful bundle	Task A: 2P, 6B, 12G. Task B: 2P, 4B, 14G.	1. +2B, -1P, +3G. 2. +3P, -1B, +2G. 3. +2P, +3G, -5B. 4. +1B, -2G, -3P. 5. -4G, +2B, -2P.	Task A: 1P, 5B, 14G. EV = 10.5. Task B: 1P, 3B, 16G. EV = 7.5.
High complexity / selective emphasis / harmful bundle	Task A: 2P, 6B, 12G. Task B: 2P, 4B, 14G.	1. +2B , -1P, +3G. 2. +3P , -1B, +2G. 3. +2P , +3G, -5B. 4. +1B , -2G , -3P. 5. -4G , +2B , -2P.	Task A: 1P, 5B, 14G. EV = 10.5. Task B: 1P, 3B, 16G. EV = 7.5.
Low complexity / neutral / beneficial bundle	Task A: 2P, 6B, 12G. Task B: 2P, 4B, 14G.	1. +3B, -6G, -1P. 2. +1P, -3B, +1G. 3. +2G, +1P, +2B.	Task A: 3P, 8B, 9G. EV = 21.0. Task B: 3P, 6B, 11G. EV = 18.0.
Low complexity / selective emphasis / beneficial bundle	Task A: 2P, 6B, 12G. Task B: 2P, 4B, 14G.	1. +3B , -6G , -1P. 2. +1P , -3B, +1G. 3. +2G, +1P , +2B .	Task A: 3P, 8B, 9G. EV = 21.0. Task B: 3P, 6B, 11G. EV = 18.0.
High complexity / neutral / beneficial bundle	Task A: 2P, 6B, 12G. Task B: 2P, 4B, 14G.	1. +3B, -6G, -1P. 2. +1P, -1B, +2G. 3. +1P, -3B, +1G. 4. -2G, -1P, +1B. 5. +2G, +1P, +2B.	Task A: 3P, 8B, 9G. EV = 21.0. Task B: 3P, 6B, 11G. EV = 18.0.
High complexity / selective emphasis / beneficial bundle	Task A: 2P, 6B, 12G. Task B: 2P, 4B, 14G.	1. +3B , -6G , -1P. 2. +1P , -1B, +2G. 3. +1P , -3B, +1G. 4. -2G , -1P, +1B . 5. +2G, +1P , +2B .	Task A: 3P, 8B, 9G. EV = 21.0. Task B: 3P, 6B, 11G. EV = 18.0.

Note: P = Purple, B = Blue, and G = Gray. Purple pays 60 credits, Blue pays 30 credits, and Gray pays zero. The directly shown alternative is the status-quo bowl. The bundled alternative starts from the same bowl and then applies the listed rule changes. Neutral and selective-emphasis versions use the same rule lists and imply the same final bowls. In the selective-emphasis condition, favorable changes were shown in bold colored font; all other changes were shown in regular black font.

The main outcomes are suboptimal choice and harmful bundle acceptance. Suboptimal

choice equals one when the respondent chooses the lower expected-value option. Harmful bundle acceptance equals one when the respondent accepts a bundled alternative that is worse than the directly shown status quo. I estimate linear probability models with indicators for high complexity, selective emphasis, and their interaction, using the low-complexity neutral condition as the baseline. Because respondents contribute repeated lottery decisions in Study 2, standard errors are clustered by respondent. Effects are reported in percentage points. I also examine perceived difficulty, confidence, time, and clicks as secondary process measures.⁴

6.2 Results

Study 2 shows that the complexity-by-emphasis pattern is concentrated among lower-capacity respondents. In the full sample, the interaction estimates are small because the lower- and higher-capacity patterns move in different directions. The aggregate estimate therefore does not summarize the main pattern well. I report the full-sample contrasts first, then turn to the preregistered capacity groups.

In the low-complexity, neutral-emphasis baseline, 41.5 percent of respondents in the full sample choose the suboptimal option and 42.0 percent accept the harmful bundle (see Figure 2). High complexity alone produces little change in either outcome in the full sample. The sharper pattern emerges in how complexity combines with selective emphasis, particularly among lower-capacity respondents. In the full sample, the complexity-by-selective-emphasis interaction is 3.7 percentage points for suboptimal choice (95% CI: -2.4, 9.8) and 4.8 percentage points for harmful bundle acceptance (95% CI: -4.1, 13.8). These interaction terms are difference-in-differences estimates: they ask whether the effect of high complexity is larger under selective emphasis than under neutral presentation.

The lower-capacity subgroup shows the clearest pattern. Among lower-capacity respondents, the interaction on suboptimal choice is 9.7 percentage points (95% CI: 2.1, 17.4). This means that high complexity increases suboptimal choice more under selective emphasis than under neutral presentation. The harmful-bundle outcome shows the same pattern. The interaction on harmful bundle acceptance is 12.6 percentage points (95% CI: 1.2, 24.1). In substantive terms, added complexity makes lower-capacity respondents more likely to accept a harmful bundle when attractive changes are highlighted.

⁴Process outcomes are estimated with the same treatment specification. Treatment effects are reported in standardized outcome units; the Baseline column reports difficulty and confidence on their original 1–7 scales, time in the original recorded units, and clicks as raw counts. Full process-outcome estimates are reported in Appendix D.10. Because these measures are secondary and may reflect several aspects of task experience, I use them to characterize engagement and difficulty rather than as separate tests of the mechanism.

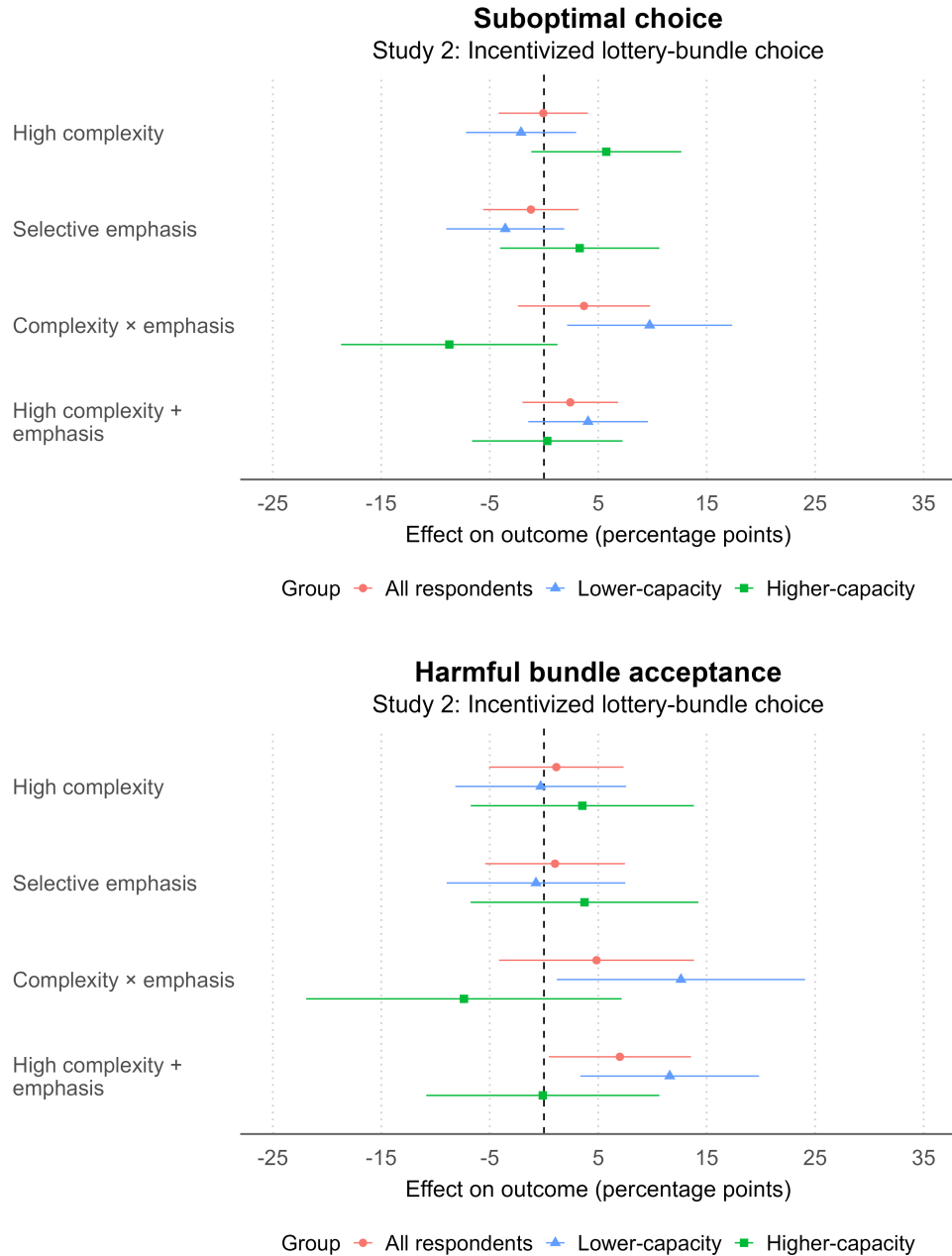


Figure 2: Study 2

Note: The analytic sample contains 1,871 respondents. Outcomes are coded as binary indicators and effects are reported in percentage points. The top panel reports treatment effects on suboptimal choice, coded as one when the respondent chooses the lower expected-value option. The bottom panel reports treatment effects on harmful bundle acceptance, coded as one when the respondent accepts a bundled alternative that is worse than the directly shown status quo. Linear probability models include indicators for high complexity, selective emphasis, and their interaction, with the low-complexity neutral condition as the baseline. Standard errors are clustered by respondent. Baseline rates are from the low-complexity, neutral-emphasis, no-advisor condition, matching the reported no-advisor simple-effect estimand. For suboptimal choice, baseline rates are 41.5 percent among all respondents, 32.8 percent among higher-capacity respondents, and 46.4 percent among lower-capacity respondents. For harmful bundle acceptance, baseline rates are 42.0, 36.7, and 45.1 percent, respectively.

The combined treatment contrast points in the same direction. Relative to the low-complexity neutral condition, the high-complexity selective-emphasis treatment increases harmful bundle acceptance among lower-capacity respondents by 11.6 percentage points (95% CI: 3.4, 19.8). This comparison is easier to read than the interaction: lower-capacity respondents are more likely to accept the harmful bundle when they face both a more complex inference task and highlighted attractive changes.

The higher-capacity pattern is different. These respondents are less moved by the interaction, and in some specifications the interaction moves in the opposite direction. That divergence helps explain why the full-sample interaction is small. The result therefore suggests a capacity-specific heterogeneity pattern: the combination of added complexity and selective emphasis matters most among lower-capacity respondents.

The process outcomes are informative but should be interpreted cautiously. I do not find clear evidence that added complexity produced a consistent increase in time, clicks, or perceived difficulty in Study 2. Clicks rise modestly with complexity in the suboptimal-choice task, but the pattern is not strong enough to support a separate search-effort account. Confidence provides the most suggestive process evidence: it declines most clearly in the high-complexity selective-emphasis condition for the suboptimal-choice task, especially among lower-capacity respondents. I therefore treat the process outcomes as supporting context rather than as a separate test of the mechanism.

Study 2 supports the argument on a specific margin. In a controlled lottery setting with objective payoffs, added complexity and selective emphasis make harmful bundles more acceptable among lower-capacity respondents. The study does not show that all bundle evaluation deteriorates in the same way, and it does not show a large full-sample interaction. Its contribution is narrower but important: when a harmful bundle must be inferred from several changes, highlighting attractive parts can make that bundle more acceptable among respondents for whom the task is most demanding. The next studies ask whether the same presentational logic appears in political evaluation.

7 Study 3: Candidate Choice

7.1 Design

Study 3 asks whether the same bundle-evaluation problem appears in electoral choice. Respondents compare two candidates running on proposals about small-business permits and inspections. The political studies deliberately use local administrative settings rather than nationally salient reforms. A national-level vignette in Türkiye would be more likely to activate prior knowledge, established partisan attachments, and existing views about prominent reforms, making it harder to separate responses to the experimental presentation from responses to familiar political actors or issues. The local setting preserves the institutional stakes central to the argument—executive discretion, procedural review, and public accountability—while reducing these preexisting national cues. Permitting, inspections, and discretionary approval also provide a setting in which speed and simplification can be attractive while weaker review and less public documentation increase executive discretion.

The candidates operate in the same policy domain, but one carries the oversight-weakening commitment. That candidate proposes faster permit approval through mayoral direct sign-off without committee review. The comparison candidate preserves committee approval and published minutes. The design therefore makes the institutional contrast concrete: one candidate offers speed and administrative flexibility while weakening review, while the other preserves a more procedural approval process.

The experiment varies two features of presentation. The first is complexity. In the low-complexity condition, respondents see two short three-item candidate platforms. In the high-complexity condition, both candidates' platforms expand to eight items. Five administrative items are added to each platform using closely matched language adapted to the candidate's proposal, while the original three core items remain unchanged. The institutional contrast therefore remains fixed across complexity conditions: one candidate proposes permit approval through the mayor's direct sign-off without committee review, while the comparison candidate preserves committee approval with published minutes. Complexity changes the amount of surrounding information respondents must process without changing the core institutional difference between the candidates.

Table 5: Study 3 treatment arms and full text of candidate platforms

Treatment arm	Candidate with direct mayoral sign-off	Comparison candidate
Low complexity / neutral	At a glance: Municipal Permit and Inspection Plan Set a clear timeline for permit decisions. Use an expedited lane for some applications by making exceptions. Approve permits through the mayor’s direct sign-off, without a committee.	At a glance: Municipal Permit and Inspection Plan Establish a fixed schedule for permit processing. Offer a fast option for applications that meet preset standards. Approve permits through a committee with published minutes.
Low complexity / selective emphasis	At a glance: Clear deadlines for permit decisions and Faster processing for routine applications Set a clear timeline for permit decisions. Use an expedited lane for some applications by making exceptions. Approve permits through the mayor’s direct sign-off, without a committee.	At a glance: Municipal Permit and Inspection Plan Establish a fixed schedule for permit processing. Offer a fast option for applications that meet preset standards. Approve permits through a committee with published minutes.
High complexity / neutral	At a glance: Municipal Permit and Inspection Plan Use a single standard application form for permits. Use a checklist to reduce missing documents. Assign a case owner for each application. Set internal time limits for department feedback. Use a shared tracking system to follow applications. Set a clear timeline for permit decisions. Use an expedited lane for some applications by making exceptions. Approve permits through the mayor’s direct sign-off, without a committee.	At a glance: Municipal Permit and Inspection Plan Use a standard application form for permits. Use a checklist to reduce missing documents. Assign a point person for each application. Set internal time limits for departments to respond. Use a shared record system to track applications. Establish a fixed schedule for permit processing. Offer a fast option for applications that meet preset standards. Approve permits through a committee with published minutes.
High complexity / selective emphasis	At a glance: Clear deadlines for permit decisions and Faster processing for routine applications Use a single standard application form for permits. Use a checklist to reduce missing documents. Assign a case owner for each application. Set internal time limits for department feedback. Use a shared tracking system to follow applications. Set a clear timeline for permit decisions. Use an expedited lane for some applications by making exceptions. Approve permits through the mayor’s direct sign-off, without a committee.	At a glance: Municipal Permit and Inspection Plan Use a standard application form for permits. Use a checklist to reduce missing documents. Assign a point person for each application. Set internal time limits for departments to respond. Use a shared record system to track applications. Establish a fixed schedule for permit processing. Offer a fast option for applications that meet preset standards. Approve permits through a committee with published minutes.

Note: Candidate order was randomized. The column labels identify the institutional contrast for the reader; respondents saw generic candidate labels. Complexity varies the length of both candidate platforms: both contain three items in the low-complexity condition and eight items in the high-complexity condition. The five additional items in the high-complexity condition describe closely matched administrative procedures, with minor wording differences adapted to each candidate’s proposal. Within each complexity condition, the underlying platform items are held fixed across neutral and selective-emphasis arms. In the neutral condition, both candidates receive the generic “At a glance: Municipal Permit and Inspection Plan” headline. In the selective-emphasis condition, the comparison candidate retains that headline, while the direct-sign-off candidate instead receives an “At a glance” headline emphasizing clear deadlines and faster processing for routine applications. In Turkish, “At a glance” was presented as “Kısaca.” The institutional contrast between mayoral direct sign-off without committee review and committee approval with published minutes remains unchanged across treatment arms.

The second feature is selective emphasis. Each candidate is presented with an “At a glance” headline. In the neutral condition, both candidates receive the generic headline “Municipal Permit and Inspection Plan.” In the selective-emphasis condition, the comparison candidate retains that generic headline, while the candidate proposing mayoral direct sign-off receives a favorable headline emphasizing “Clear deadlines for permit decisions and Faster processing for routine applications.” Within each complexity condition, the underlying platform items are held fixed across neutral and selective-emphasis arms. Selective emphasis therefore changes how the direct-sign-off candidate’s platform is summarized without changing the proposals themselves. In Turkish, “At a glance” was presented as “Kısaca.”

The main outcomes are harmful-option selection and recognition. Harmful-option selection is coded as one when the respondent supports the candidate proposing direct mayoral sign-off without committee review. This outcome is the political analogue of choosing or accepting a harmful option in the lottery studies. Recognition is coded as one when the respondent correctly identifies which candidate proposed approving permits through mayoral direct sign-off without a committee. I also examine perceived difficulty, confidence, time, and clicks as secondary process measures.

I estimate linear probability models with indicators for high complexity, selective emphasis, and their interaction, using the low-complexity neutral condition as the baseline. The models adjust for module order, municipal-module order, and survey date. Because each respondent contributes one Study 3 candidate-choice observation, I use heteroskedasticity-robust standard errors and report effects in percentage points. Recognition is a binary outcome and is analyzed with the same treatment specification.⁵

7.2 Results

When both candidates are shown with longer platforms and the candidate proposing direct mayoral sign-off receives the favorable summary headline, harmful-option selection rises and recognition of the institutional commitment falls. Both outcomes move in the direction predicted by the theory: respondents become more willing to support the candidate tied to weaker oversight, while they become less likely to identify the direct-sign-off commitment.

⁵Process outcomes are estimated with the same treatment specification. Treatment effects are reported in standardized outcome units; the Baseline column reports difficulty and confidence on their original 1–7 scales, time in the original recorded units, and clicks as raw counts. Full process-outcome estimates are reported in Appendix D.10. Because these measures are secondary and may reflect several aspects of task experience, I use them to characterize engagement and difficulty rather than as separate tests of the mechanism.

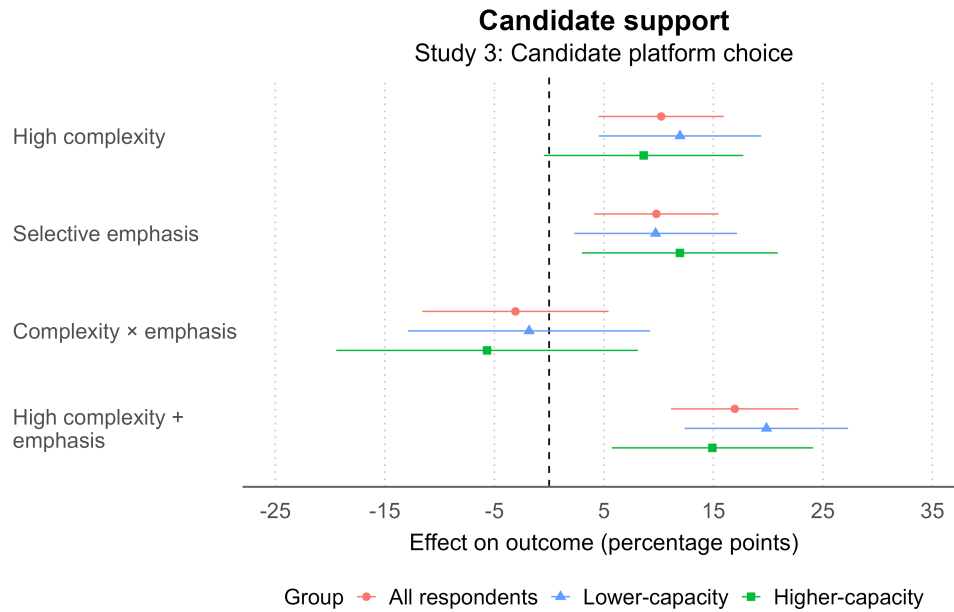
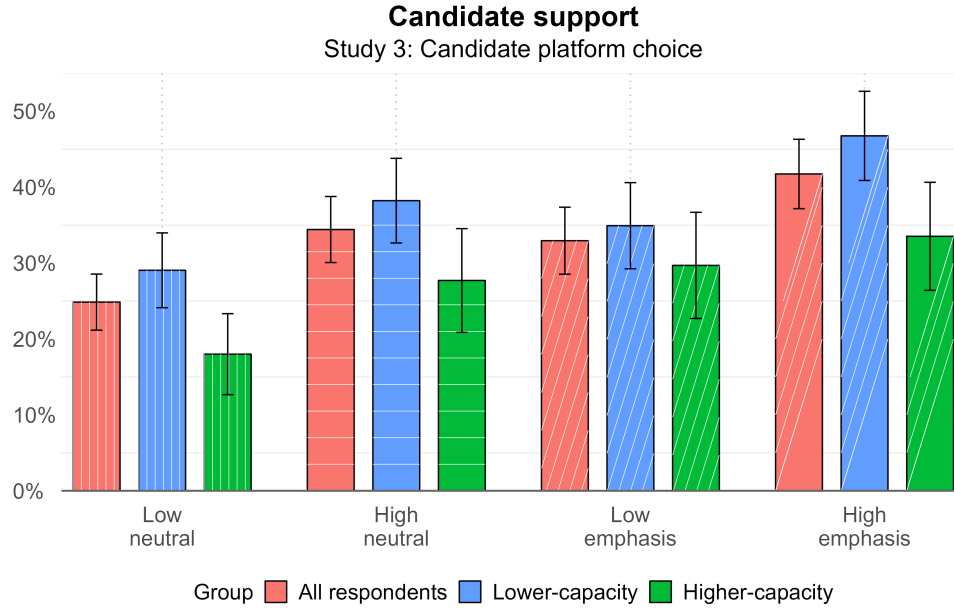


Figure 3: Study 3: Candidate support.

Note: The analytic sample contains 1,871 respondents. The top panel reports treatment-arm means for harmful-option selection, coded as one when the respondent supports the candidate proposing mayoral direct sign-off without committee review. The bottom panel reports linear probability model estimates relative to the low-complexity neutral baseline. The specification includes indicators for high complexity, selective emphasis, and their interaction, and adjusts for module order, municipal-module order, and survey date. Standard errors are heteroskedasticity-robust. Baseline candidate-support rates in the low-complexity, neutral-emphasis condition are 24.9 percent among all respondents, 18.0 percent among higher-capacity respondents, and 29.1 percent among lower-capacity respondents.

Recognition moves in the opposite direction. Relative to the low-complexity neutral baseline, the joint high-complexity and selective-emphasis condition reduces correct recognition of the oversight-weakening commitment by 7.9 percentage points (95% CI: -14.1, -1.6; Figure 4).

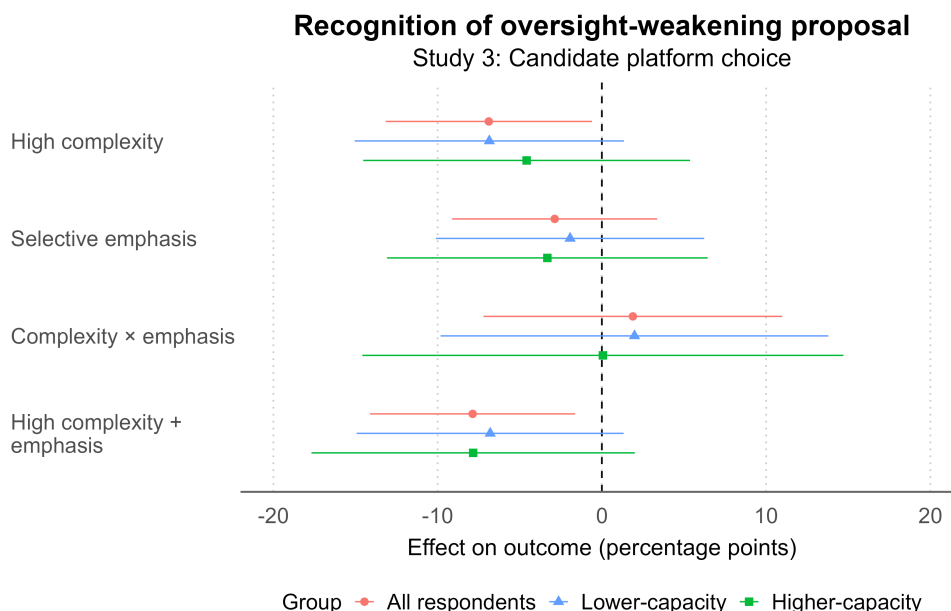


Figure 4: Study 3: Recognition of oversight-weakening proposal.

Note: The analytic sample contains 1,871 respondents. Recognition is coded as one when the respondent correctly identifies which candidate proposed approving permits through mayoral direct sign-off without a committee. The figure reports linear probability model estimates relative to the low-complexity neutral baseline. The specification includes indicators for high complexity, selective emphasis, and their interaction, and adjusts for module order, municipal-module order, and survey date. Standard errors are heteroskedasticity-robust. Baseline recognition rates in the low-complexity, neutral-emphasis condition are 63.4 percent among all respondents, 72.0 percent among higher-capacity respondents, and 58.1 percent among lower-capacity respondents.

In the low-complexity neutral baseline, 24.9 percent of respondents support the candidate proposing direct mayoral sign-off (see Figure 3). Relative to that baseline, high complexity increases support by 10.2 percentage points (95% CI: 4.5, 15.9), while selective emphasis increases support by 9.8 percentage points (95% CI: 4.1, 15.5). When both features are present, support rises by 17.0 percentage points (95% CI: 11.1, 22.8). The joint condition produces the largest shift in support, but the two presentational features do not amplify one another. The complexity-by-selective-emphasis interaction is -3.1 percentage points (95% CI: -11.6, 5.4).

Appendix D.8.9 further unpacks this decline by distinguishing correct identification, identification of the wrong candidate, and uncertainty. The decline in correct recognition is driven primarily by an increase in uncertainty rather than by respondents shifting toward the wrong candidate. These diagnostics are descriptive rather than a causal mediation analysis: they show how recognition changes under treatment, but they cannot establish how much of the increase in candidate support operates through recognition. Descriptively, support for the direct-sign-off candidate also rises among respondents who correctly identify the institutional commitment, indicating that failed recognition alone does not account for the overall support pattern.

The process outcomes help rule out a simple disengagement interpretation. High complexity increases time spent and clicks, and the joint condition also raises perceived difficulty. Confidence falls under complexity in the full sample, especially among lower-capacity respondents, although the joint confidence effect is weaker. Respondents therefore do not appear to ignore the longer platform. They spend more time with it, interact more with it, and experience it as more demanding. Yet harmful-option selection rises and recognition falls under the joint presentation. The candidate platform changes what respondents take away from the choice even when it induces more engagement rather than less.

Study 3 therefore extends the argument from direct bundle evaluation to candidate choice. A candidate tied to weaker oversight becomes more electorally acceptable when the choice involves longer platforms and that candidate receives a favorable summary headline. Harmful-option selection and recognition each move in the predicted direction: support for the direct-sign-off candidate rises, while identification of the direct-sign-off commitment declines.

8 Study 4: Municipal Reform Package

8.1 Design

Study 4 brings the argument back to direct reform evaluation in a setting closer to ordinary local politics. Respondents read a municipal reform package that affects employment, service capacity, and discretionary allocation. As in the lottery tasks, each component changes the consequences of the option, and respondents must combine those component-level changes before deciding whether to accept the package.

The package deliberately mixes attractive administrative aims with costly institutional consequences. Some components promise faster delivery, investment, or revitalization. Others increase administrative discretion, or reduce service capacity or employment opportunities. This structure mirrors the reform bundles that motivate the paper: institutional costs do not appear alone, but alongside provisions that may be useful, publicly defensible, or attractive on their face.

The experiment varies two features of presentation. The first is complexity. In the low-complexity condition, respondents see a shorter two-policy version of the package. In the high-complexity condition, they see the full four-policy version. The longer package asks respondents to track more moving parts and integrate consequences across several domains.

The second feature is selective emphasis. Within each complexity condition, the same measures appear in the neutral and selective-emphasis arms. What changes is how those measures are labeled. The neutral version uses more technocratic titles, while the selective-emphasis version uses favorable policy headlines that emphasize attractive aspects of the package, such as speed, revitalization, and delivery. The treatment therefore changes which components are easiest to notice, not the underlying measures within a given complexity level.

Table 6 summarizes the measures shown in each arm. Low-complexity respondents saw Measures 1 and 3; high-complexity respondents saw all four measures. The added measures in the high-complexity condition increase the amount of information respondents must process, but they do not change the package’s net consequences across the three measured domains. Within each complexity level, selective emphasis changes how the same measures are displayed: favorable policy titles are added, and good-direction numerical consequences are made visually prominent. The full text is reported in Appendix C.

Table 6: Study 4 municipal measures, selective emphasis, and domain consequences

Policy	Shown in	Neutral headline	Selective-emphasis headline	Emphasized consequences	Employment	Discretionary allocation	Service capacity
1	Low and high complexity	Regulation 1	Faster Project Delivery.	+300 jobs and +3,000 residents served.	+300	+200 million TL	+3,000
2	High complexity only	Regulation 2	Stronger Oversight.	+200 jobs and -250 million TL discretionary allocation.	+200	-250 million TL	0
3	Low and high complexity	Regulation 3	Downtown Revitalization Fund.	—	-700	+350 million TL	-6,000
4	High complexity only	Regulation 4	New Transparency Portal.	—	-200	+250 million TL	0
Low-complexity total (Measures 1 and 3)					-400	+550 million TL	-3,000
High-complexity total (Measures 1–4)					-400	+550 million TL	-3,000

Note: The table summarizes the municipal package shown in Study 4. Low-complexity respondents saw Measures 1 and 3; high-complexity respondents saw all four measures. Within each complexity level, neutral and selective-emphasis arms imply the same domain consequences. Selective emphasis changes the display of the same measures: favorable titles are added, and good-direction numerical consequences are bolded. Good-direction consequences are positive employment changes, reductions in discretionary allocation, and positive service-capacity changes. Positive discretionary allocation means more spending under municipal discretion.

The main outcomes are harmful-package acceptance and consequence recognition. Harmful-package acceptance is coded as one when respondents accept the municipal reform package, which is strictly worse than the status-quo in all three domains. Consequence recognition is coded as one when respondents identify that at least two relevant domains worsen under the package: employment, discretionary allocation, or service capacity. Recognition matters because the municipal task can fail in more than one way. Respondents may fail to integrate the domain-level consequences correctly, or they may recognize the costs but still accept the package when judging it as a whole.⁶ I also examine perceived difficulty, confidence, time,

⁶This measure is deliberately more demanding than the Study 3 recognition item. Study 3 asks respondents to recall which candidate proposed a specific oversight-weakening provision. Study 4 asks respondents to integrate consequences across several policy domains and identify whether multiple domains worsen. Consequence recognition in Study 4 therefore captures multidomain integration rather than recall of a single provision.

and clicks as secondary evidence about how respondents experienced the task.

I estimate linear probability models with indicators for high complexity, selective emphasis, and their interaction, using the low-complexity neutral condition as the baseline. The models adjust for module order, candidate-module order, survey date, and the preregistered risk-aversion measure. Because each respondent contributes one Study 4 reform-package decision, I use heteroskedasticity-robust standard errors and report effects in percentage points. Consequence recognition is a binary outcome and is analyzed with the same treatment specification.

8.2 Results

In the low-complexity neutral baseline, 59.1 percent of respondents accept the municipal package (Figure 5). Relative to that baseline, the high-complexity selective-emphasis condition increases harmful-package acceptance by 8.1 percentage points, with a 95 percent confidence interval from 1.8 to 14.3 points. The joint condition produces the largest shift in acceptance: respondents become more willing to accept the package when they see the longer version with favorable policy headlines. I therefore interpret the result as evidence that the combined presentation changes package acceptance, while treating the interaction term itself as secondary. The complexity-by-selective-emphasis interaction is -6.8 percentage points (95% CI: -15.6, 2.0), providing no evidence that the two presentational features amplify one another in this task.

Consequence recognition moves in the predicted direction, but the evidence is weaker than for package acceptance (Figure 6). In the low-complexity neutral baseline, 16.3 percent of respondents correctly identify that at least two relevant domains worsen. That low baseline reflects the difficulty of the recognition task and leaves limited room for further decline. Relative to that baseline, the high-complexity selective-emphasis condition lowers consequence recognition by 4.1 percentage points, with a 95 percent confidence interval from -8.8 to 0.5 points. The estimate is consistent with weaker recognition under the joint presentation, but it is also compatible with a small decline or no change.

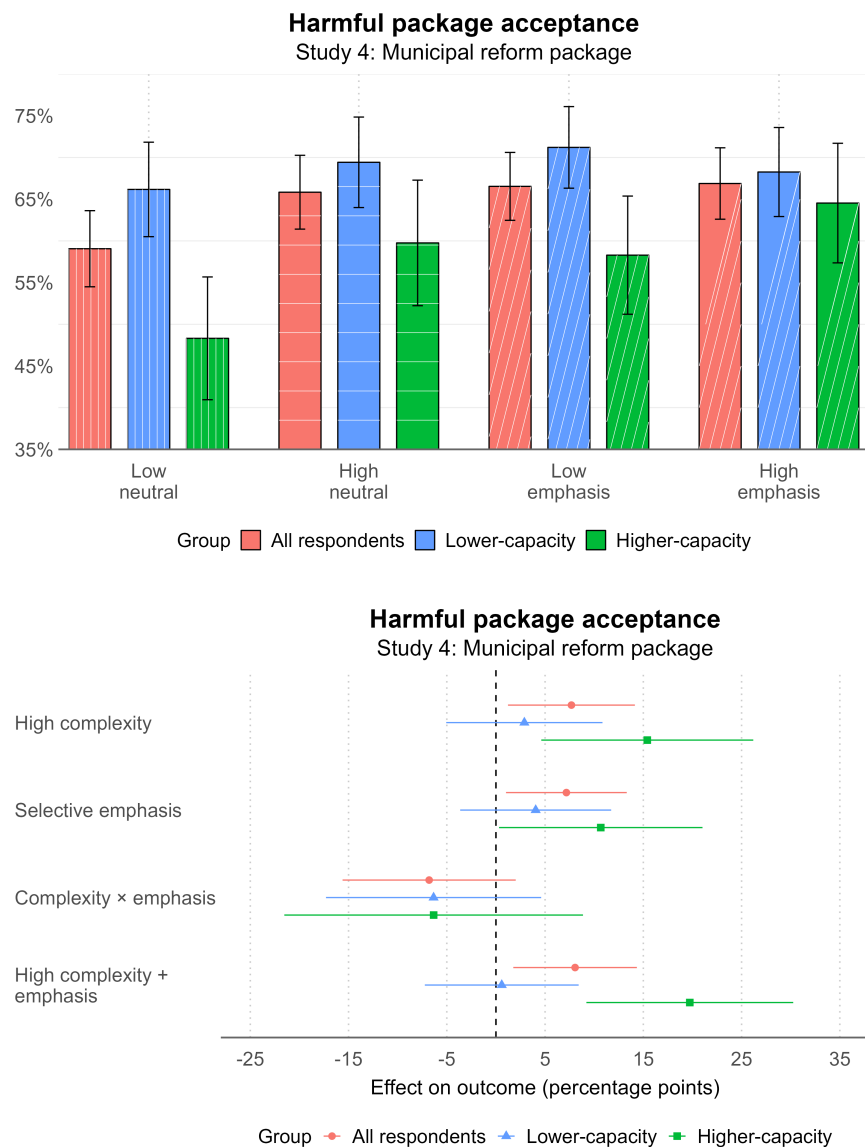


Figure 5: Study 4: Harmful-package acceptance.

Note: The analytic sample contains 1,871 respondents. The top panel reports treatment-arm means for harmful-package acceptance, coded as one when the respondent accepts the municipal reform package. The bottom panel reports linear probability model estimates relative to the low-complexity neutral baseline. The specification includes indicators for high complexity, selective emphasis, and their interaction, and adjusts for module order, candidate-module order, survey date, and the preregistered risk-aversion measure. Standard errors are heteroskedasticity-robust. Baseline municipal package-acceptance rates in the low-complexity, neutral-emphasis condition are 59.1 percent among all respondents, 48.3 percent among higher-capacity respondents, and 66.2 percent among lower-capacity respondents.

The subgroup estimates suggest that acceptance and recognition move on different margins. Acceptance rises most among higher-capacity respondents (Figure 5). For this group,

the joint condition increases package acceptance by 19.7 percentage points (95% CI: 9.2, 30.3). Among lower-capacity respondents, the corresponding estimate is 0.6 percentage points (95% CI: -7.2, 8.4). This difference should be read alongside the baseline rates. Lower-capacity respondents begin from a higher baseline acceptance rate of 66.2 percent, while higher-capacity respondents begin from a lower baseline of 48.3 percent. The combined presentation therefore has more room to increase acceptance among higher-capacity respondents, which may partly account for the larger estimated effect in that subgroup. The baseline difference, however, does not by itself explain the subgroup contrast.

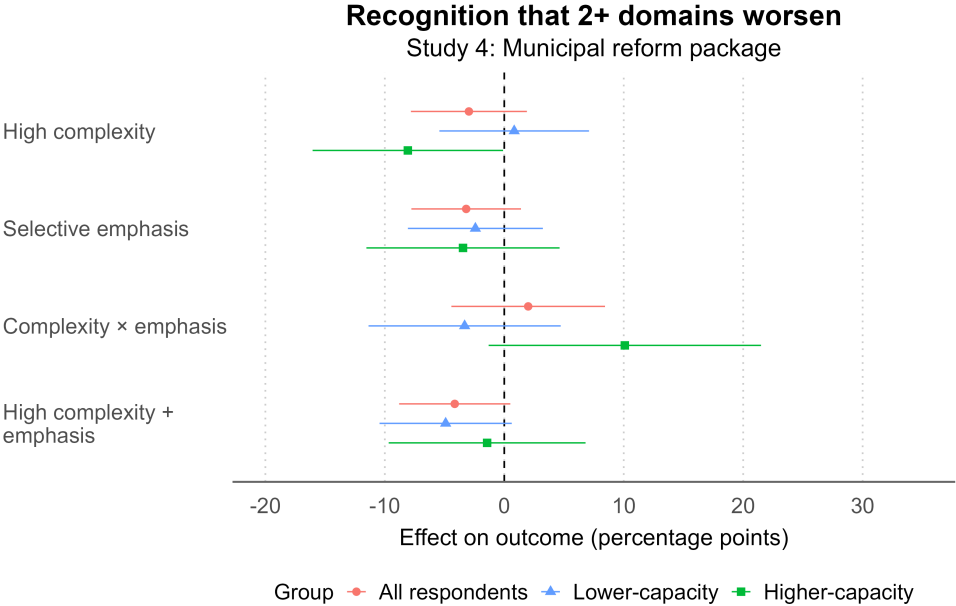


Figure 6: Study 4: Consequence recognition. The figure reports treatment effects on the indicator for correctly identifying that at least two relevant package domains worsen, relative to the low-complexity, neutral-emphasis baseline.

Note: The analytic sample contains 1,871 respondents. Consequence recognition is coded as one when the respondent correctly identifies that at least two relevant domains worsen under the municipal package. The figure reports linear probability model estimates relative to the low-complexity neutral baseline. The specification includes indicators for high complexity, selective emphasis, and their interaction, and adjusts for module order, candidate-module order, survey date, and the preregistered risk-aversion measure. Standard errors are heteroskedasticity-robust. Baseline consequence-recognition rates in the low-complexity, neutral-emphasis condition are 16.3 percent among all respondents, 19.7 percent among higher-capacity respondents, and 14.1 percent among lower-capacity respondents.

Recognition shows a different pattern. Higher-capacity respondents correctly identify

the multidomain consequences of the package only slightly more often than lower-capacity respondents in the baseline condition, 19.7 percent versus 14.1 percent (Figure 6). Among higher-capacity respondents, the joint condition changes consequence recognition by -1.4 percentage points (95% CI: -9.7, 6.8). Among lower-capacity respondents, the corresponding estimate is -4.9 percentage points (95% CI: -10.4, 0.6). Acceptance therefore rises most among higher-capacity respondents, who also begin from a lower baseline acceptance rate, while recognition declines more visibly among lower-capacity respondents.

This subgroup pattern fits the structure of the municipal task. The recognition item requires respondents to combine positive and negative consequences across employment, discretionary allocation, and service capacity. In that setting, presentation can matter in more than one way. Some respondents may fail to identify that multiple domains worsen. Others may recognize some costs but still accept the package because the favorable labels make the package more attractive overall. Study 4 cannot separate those paths, but it shows why acceptance and recognition need not move identically as the task becomes more demanding.

The process outcomes help rule out a pure non-engagement interpretation. Clicks increase under more complex presentation, indicating that respondents interact more with the municipal package rather than simply passing over it. Perceived difficulty moves in the same general direction, with the clearest increase in the high-complexity selective-emphasis condition. These measures do not show that respondents understood the package better; they show that higher acceptance is not simply a result of lower engagement. Respondents engage with a demanding reform package, but that engagement does not prevent higher acceptance or weaker consequence recognition.

Study 4 therefore extends the argument to a harder political setting. The longer package with favorable policy headlines increases harmful-package acceptance in the full sample. Consequence recognition moves downward, though the evidence is weaker. The subgroup results suggest that difficult multidomain packages can affect evaluation through more than one path: weakening recognition for some respondents, while changing how recognized or partially recognized costs enter final judgment for others.

9 Discussion

Support for reforms that weaken democratic oversight is often interpreted as evidence that voters knowingly accept democratic costs in exchange for partisan, policy, or material benefits. The findings in this paper suggest that this inference may sometimes be premature. When an oversight-weakening provision is embedded in a broader package and attractive components receive greater emphasis, citizens may support the proposal without fully understanding its democratic consequences. What appears to be a willingness to trade democracy for other benefits may therefore also reflect how the reform was assembled and presented.

When governments combine several provisions in an omnibus bill or reform package, citizens must assess those provisions together. This judgment becomes more demanding as the package grows longer, spans more policy domains, or relies on technical language whose institutional implications are not immediately clear. Selective emphasis by government officials, campaigns, and government-aligned media can further shape which parts of the package citizens notice and use. Promised benefits such as efficiency, security, or service delivery may become easier to identify than changes to oversight or executive discretion, leaving the democratic downside less clear at the moment of judgment.

The two abstract choice studies isolate this evaluation problem from partisan attachment, policy agreement, and other political motivations. Study 1 shows that bundled presentation increases suboptimal choice by 17.0 percentage points even when respondents choose between options with objectively ranked payoffs. Study 2 then examines whether selective emphasis becomes more consequential as the bundle becomes harder to evaluate. The pattern is concentrated among lower-capacity respondents: relative to the shorter, neutrally presented bundle, the longer bundle with attractive changes emphasized increases acceptance of the harmful option by 11.6 percentage points. Together, these findings show that presentation can impair evaluation even when the consequences are fixed and no partisan or ideological trade-off is present.

The political studies move the same argument into more realistic settings. In Study 3, relative to the low-complexity, neutral-presentation condition, the high-complexity, selective-emphasis condition increases support for the candidate associated with an oversight-weakening commitment by 17.0 percentage points, while recognition of that commitment declines by 7.9 percentage points. Study 4 extends the comparison to a municipal reform package. There, the larger package with favorable headlines increases acceptance by 8.1 percentage points, while consequence recognition moves downward by an estimated 4.1 points. Together, the political studies show that broader and more favorable presentation can increase support for oversight-weakening proposals. The decline in recognition is clearest in Study 3 and more

modest in Study 4.

The design addresses the trade-off interpretation directly, although the studies do so in different ways. In Studies 1 and 2, the harmful option is strictly worse in payoff terms, and in Study 4 the municipal package is worse than the status quo across all three measured domains. Increased acceptance in Studies 1 and 2 therefore cannot be explained by a payoff trade-off built into the alternatives. In Study 4, it cannot be explained by a compensating gain in any of the three experimentally measured domains. Study 3 allows more room for a perceived trade-off because the candidate who weakens committee oversight is also associated with attractive claims about speed and administrative flexibility. Because recognition is measured after respondents make their choice, the political studies cannot determine whether presentation increased support by making the harmful implication harder to identify, by reducing the weight respondents placed on a recognized cost, or by some combination of the two.

The subgroup patterns are substantively stable across the alternative inclusion rules. Most importantly, the Study 2 complexity-by-selective-emphasis pattern among lower-capacity respondents changes little as the sample is broadened. The interaction remains positive for both suboptimal choice and harmful-bundle acceptance under every inclusion rule, and the joint high-complexity and selective-emphasis contrast on harmful-bundle acceptance is similarly stable. Study 1 continues to show bundling effects in both capacity groups, and Study 3 candidate-support effects remain positive in both groups. In Study 4, the acceptance effect remains concentrated among higher-capacity respondents, while the lower-capacity estimate remains near zero. The Study 4 consequence-recognition estimates remain negative but imprecisely estimated and become smaller under broader inclusion rules.

The argument applies most clearly when provisions that weaken oversight are embedded in broader legal or policy packages, must be identified and evaluated alongside several other components, and have not already been made unmistakable through public debate. It should matter less when the harmful provision stands alone, the proposal openly weakens oversight, or opposition parties, independent media, courts, experts, and civil society have clearly explained how the package expands executive discretion or weakens institutional constraints.

Türkiye provides a demanding case because packaged reform is familiar rather than novel: citizens have repeatedly encountered institutional change through *torba yasalar*, constitutional packages, emergency decree-laws, and executive-centered administrative reforms. Finding presentation effects in this setting makes unfamiliarity with reform packages a less plausible explanation. Media capture makes the problem especially consequential by repeatedly emphasizing promised benefits while making criticism harder to encounter, although the experiments do not establish that voters misunderstood any particular Turkish reform.

The same scope conditions can arise beyond Türkiye. Hungary’s post-2010 constitutional transformation, India’s 2020 farm-law episode, and Israel’s 2023 judicial overhaul each placed major institutional or policy changes within multi-component packages (Bánkuti, Halmai and Scheppele, 2012; Behl, 2022; Navot, 2023). India and Israel also show how sustained opposition and mass mobilization can make the disputed costs central to public debate. The evaluation problem studied here should become less consequential once the contents and likely effects of a package are widely understood.

The same evaluation problem can extend beyond democratic backsliding. Governments often place costly or controversial provisions alongside popular benefits in omnibus legislation, ballot measures, campaign platforms, and administrative reforms. In these settings, the relevant harm may concern public spending, privacy, environmental risk, or access to services rather than democratic oversight. The argument is not that policy packages are inherently misleading. It is that when citizens must infer a proposal’s overall consequences from several components, selective emphasis in media coverage can make favorable consequences easier to identify than the costs embedded elsewhere in the package.

The findings also have implications for how researchers study democratic accountability. Experiments that state a democratic violation explicitly are well suited to testing whether citizens will tolerate a cost they clearly recognize. Many real reforms, however, do not present the democratic downside in that form. When institutional changes are embedded in longer platforms or multi-component packages, support alone cannot reveal whether citizens knowingly accepted the cost or failed to identify it clearly. Future research should therefore examine what citizens understood the proposal to contain alongside whether they supported it.

The distinction between conscious trade-offs and incomplete understanding also changes what opposition and civil society need to do. When voters recognize the institutional cost and accept it, opponents must explain why the promised benefits do not justify weaker oversight. When that cost remains unclear, they must first separate the package’s provisions and explain how those provisions alter oversight and executive discretion.

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A Formal Model and Proofs

A.1 The Decision Problem

A citizen chooses between a status quo S and a reform bundle R . Normalize the value of the status quo to zero, $U(S) = 0$. The bundle contains J components, $R = \{r_1, \dots, r_J\}$. Each component j contributes utility $v_j \in \mathbb{R}$, so the reform's true value is

$$V \equiv U(R) = \sum_{j=1}^J v_j. \quad (1)$$

A fully informed citizen supports the reform if and only if $V > 0$. If $V < 0$, the reform is harmful overall. The problem studied here begins when citizens do not recover V fully before making that support decision.

Citizens must reconstruct the bundle from its components. Let $k > 0$ denote the bundle's evaluative burden. It summarizes how hard the package is to parse, including the number of consequential provisions, the number of policy domains, the technical opacity of the language, and the difficulty of integrating the components into an overall judgment. Let $\tau \in \{L, H\}$ index citizens' cognitive type, where lower-capacity citizens face a higher cost of evaluating the same bundle.

Presentation of components is represented by $q = (q_1, \dots, q_J) \in \{0, 1\}^J$. A component with $q_j = 1$ is emphasized by the presentation; a component with $q_j = 0$ is not. The citizen observes both the burden of the bundle and its presentation before choosing how much attention to devote to evaluation.

Incomplete evaluation enters the model through component coverage. Let $s_j(a, k, q) \in [0, 1]$ denote how much of component j a citizen recovers after devoting attention $a \geq 0$ to a package with evaluative burden k and presentation q . Coverage means that the component is noticed, processed, and brought into the final judgment. When $s_j = 1$, the component is fully recovered; when $s_j = 0$, it does not enter evaluation at all. Cognitive type affects this process indirectly, by changing the attention level chosen in equilibrium, rather than by changing the primitive relationship between exposure and coverage.

The support decision is therefore based not on the reform's full value, V , but on the value the citizen recovers from the package:

$$\hat{V}(a^*, k, q, \tau) = \sum_{j=1}^J s_j(a^*(k, q, \tau), k, q) v_j, \quad (2)$$

where $a^*(k, q, \tau)$ is the attention level chosen by the citizen. The citizen supports the bundle when this recovered value is positive. A reform can therefore be harmful in truth,

$V < 0$, yet still receive support if attractive components are recovered more fully than the costly institutional components.

A.2 Attention, Saliency, and Coverage

The next step is to show how attention becomes component coverage. Citizens choose how much attention to devote to the package, but presentation shapes how that attention is distributed across components.

Let $\chi_j(q)$ denote the saliency weight attached to component j under presentation q :

$$\chi_j(q) = \frac{J(1 + \rho q_j)}{\sum_{\ell=1}^J (1 + \rho q_\ell)}, \quad \rho \geq 0. \quad (3)$$

Here, q_j records whether component j is emphasized, and ρ captures how strongly emphasis changes saliency. When $\rho = 0$, presentation has no effect and every component receives the same saliency weight, $\chi_j(q) = 1$. When $\rho > 0$, emphasized components receive higher saliency weights than nonemphasized components. The denominator rescales the weights after emphasis is applied, while the factor J keeps their sum equal to J . Presentation therefore redistributes saliency across components rather than increasing the citizen's total evaluative capacity by assumption.

This formulation follows the logic of saliency models, but adapts it to the setting studied here. In standard saliency models, an attribute becomes prominent because it stands out relative to attributes of other options. Here, saliency responds to presentation and operates within a single reform bundle rather than across competing alternatives (Bordalo, Gennaioli and Shleifer, 2012, 2013).⁷

Coverage is generated by a simple exposure technology. A citizen who devotes attention a to a bundle with burden k has scrutiny per unit of burden equal to a/k , and saliency determines how much of that scrutiny reaches each component:

$$s_j(a, k, q) = \phi \left(\frac{\chi_j(q)a}{k} \right), \quad (4)$$

where $\phi : \mathbb{R}_+ \rightarrow [0, 1]$, $\phi(0) = 0$, $\phi' > 0$, $\phi'' < 0$, and $\lim_{x \rightarrow \infty} \phi(x) = 1$. The function ϕ is increasing because additional exposure improves coverage, concave because coverage has diminishing returns, and bounded above by one because coverage eventually saturates.

⁷A fully rational-inattention model would allow citizens to choose both how much to learn and what to learn. Fixing the composition of attention through saliency is a substantive restriction: it captures the idea that presentation shapes which consequences become available for judgment before full deliberation takes place.

In this expression, complexity works through the denominator k : as the package becomes harder to parse, a given amount of effort is spread more thinly across the task of evaluation. Saliency works through the multiplier $\chi_j(q)$, directing more of that effort toward emphasized components and less toward the rest. A respondent may therefore spend more time with a complex package and still recover less of each component, because the burden being evaluated has grown.

The attention choice is modeled as an accuracy problem rather than as an immediate support decision. Citizens value attention because it helps them recover consequential components before forming $\hat{V}$, whether those components ultimately count for or against the reform. A costly provision is just as important to recognize as a beneficial one if the goal is to understand what the reform contains. I represent this value of accurate evaluation as the coverage-weighted sum of component magnitudes:

$$G(a, k, q) \equiv \sum_{j=1}^J \phi \left(\frac{\chi_j(q)a}{k} \right) |v_j|. \quad (5)$$

The absolute value reflects this accuracy interpretation. G is not the value of supporting the reform; it is the value of learning which consequences the reform contains. Support itself is determined by the signed recovered value $\hat{V}$. Thus G determines the incentive to acquire information, while $\hat{V}$ determines the support decision after that information has been recovered.

Attention is costly. Let $\lambda_\tau > 0$ denote the unit price of attention for type τ , with $\lambda_L > \lambda_H$. Lower-capacity citizens therefore face a higher price of evaluating the same bundle. The citizen chooses attention according to

$$a^*(k, q, \tau) = \arg \max_{a \geq 0} \{G(a, k, q) - \lambda_\tau a\}. \quad (6)$$

The useful summary measure is scrutiny per unit of burden,

$$b \equiv \frac{a}{k}. \quad (7)$$

Complexity and capacity both make scrutiny scarcer, though through different routes: complexity raises the burden across which attention must be spread, while lower capacity raises the cost of supplying attention in the first place. I use the term attention gap to describe the distance between the bundle's full content and the component coverage citizens recover from it. In the model, this gap corresponds to low average coverage, or equivalently to scarce scrutiny per unit of burden. The appendix proves the comparative static formally, but the intuition is straightforward: complexity need not reduce raw effort in order to impair

evaluation. It can instead reduce what effort buys, so that citizens spend more time, click more, or report greater difficulty while still recovering a more partial version of the package.

A.3 Selective Emphasis

Selective emphasis matters because scarce scrutiny has to land somewhere. Presentation does not change the components in the bundle or their true values v_j . It changes the mix of components that citizens are most likely to recover.

Let $Q = \{j : q_j = 1\}$ denote the emphasized components and $N = \{j : q_j = 0\}$ the components left in the background. For a nontrivial presentation, with $\rho > 0$ and Q neither empty nor equal to the full bundle, the salience weights satisfy

$$\chi_j(q) > 1 \quad \text{for } j \in Q, \quad \chi_j(q) < 1 \quad \text{for } j \in N. \quad (8)$$

At any fixed level of scrutiny $b > 0$, emphasized components therefore receive more coverage than they would under neutral presentation,

$$\phi(\chi_j(q)b) > \phi(b) \quad \text{for } j \in Q, \quad (9)$$

while backgrounded components receive less coverage,

$$\phi(\chi_j(q)b) < \phi(b) \quad \text{for } j \in N. \quad (10)$$

This is the basic compositional effect. Since the salience weights average to one, emphasis redistributes scrutiny rather than expanding the citizen's evaluative capacity. Under the concavity of ϕ , this redistribution can lower average coverage even as it raises coverage of the emphasized components:

$$\frac{1}{J} \sum_{j=1}^J \phi(\chi_j(q)b) \leq \phi(b), \quad (11)$$

with strict inequality when the salience weights are nonuniform and ϕ is strictly concave over the relevant range. Selective emphasis can therefore move recognition and support in different directions. Citizens may recover less of the bundle on average while recovering more of the parts made salient.

The support decision depends on how the salience shift lines up with the values of the components. Let q^0 denote neutral presentation and q^E a presentation that emphasizes Q . For this fixed-scrutiny comparison, it is useful to write recovered value directly as a function of b :

$$\hat{V}(b, q) = \sum_{j=1}^J \phi(\chi_j(q)b) v_j. \quad (12)$$

Relative to neutral presentation, selective emphasis changes recovered value by

$$\hat{V}(b, q^E) - \hat{V}(b, q^0) = \sum_{j=1}^J [\phi(\chi_j(q^E)b) - \phi(b)] v_j, \quad (13)$$

where the bracketed term is the change in coverage for component j caused by selective emphasis, and multiplying it by v_j shows whether that coverage shift makes the recovered reform value rise or fall. Selective emphasis raises recovered value when the weighted sum of these coverage changes is positive:

$$\sum_{j=1}^J [\phi(\chi_j(q^E)b) - \phi(b)] v_j > 0. \quad (14)$$

This condition is most apparent when the presentation emphasizes attractive components and leaves costly institutional components less visible. In that case, coverage rises for the parts of the package that make the reform look better and falls for the parts that make it look worse. The condition does not require every emphasized component to be beneficial or every backgrounded component to be costly. What matters is the weighted balance: the gains from recovering attractive components must outweigh any losses from backgrounding attractive components and any additional recovery of costly components. When that condition holds, a harmful reform can become more attractive even as recognition of its full consequences declines.

The fixed-scrutiny comparison separates the compositional effect from any change in the amount of attention the citizen chooses. In equilibrium, presentation may also change b^* , which can amplify or dampen the same logic. The appendix treats that response formally; the main point here is that selective emphasis can distort evaluation by changing the composition of recovered information, even when citizens remain engaged.

The effect should be largest when scrutiny is scarce but not collapsed. When scrutiny is abundant, most components are recovered and presentation has little room to redirect judgment. When scrutiny is extremely scarce, little content is recovered at all. The strongest effects arise in the intermediate range: evaluation is difficult enough that citizens cannot reconstruct the package evenly, but not so difficult that emphasized components fail to enter judgment.

The capacity implication comes from the same scarcity logic. Because lower-capacity citizens face a higher price of attention, the same bundle generally leaves them with less

scrutiny per unit of burden. They should therefore be more vulnerable to selective emphasis over the range in which they remain engaged. The ordering is not unconditional, however. If lower-capacity citizens approach disengagement while higher-capacity citizens continue to process the bundle, emphasis effects can attenuate among lower-capacity citizens and appear more strongly among higher-capacity citizens.

A.4 Decision Rule and Hypotheses

The citizen supports the reform when the recovered value of the bundle is positive:

$$\hat{V}(a^*, k, q, \tau) > 0. \quad (15)$$

The recovered value need not equal the reform's true value. True value depends on all components,

$$V = \sum_{j=1}^J v_j, \quad (16)$$

whereas recovered value depends on the components the citizen incorporates into judgment,

$$\hat{V}(a^*, k, q, \tau) = \sum_{j=1}^J s_j(a^*(k, q, \tau), k, q) v_j. \quad (17)$$

The hypotheses follow from the fact that citizens act on $\hat{V}$, not directly on V . Once a reform has to be reconstructed from a package, presentation can change which consequences enter judgment. Greater complexity makes reconstruction more incomplete by spreading scrutiny over more moving parts, while selective emphasis changes which parts of the package are most likely to be recovered. If those shifts lead some respondents to recover $\hat{V} > 0$ for a reform whose full value is $V < 0$, support for a harmful package should rise relative to a direct-comparison baseline. The same recovery problem also motivates the recognition predictions: citizens may support harmful reform because the institutional cost is missed, only partially recovered, or outweighed by more accessible attractive components.

A.5 Assumptions

(A1) Reform bundle. A reform bundle contains $J \geq 2$ components, indexed by $j \in \{1, \dots, J\}$. Each component has signed value $v_j \in \mathbb{R}$, and the true value of the bundle is

$$V = \sum_{j=1}^J v_j. \quad (18)$$

A reform is harmful overall when $V < 0$.

(A2) Saliency weights. Presentation is given by $q = (q_1, \dots, q_J) \in \{0, 1\}^J$. A component with $q_j = 1$ is emphasized by the presentation. Saliency weights are

$$\chi_j(q) = \frac{J(1 + \rho q_j)}{\sum_{\ell=1}^J (1 + \rho q_\ell)}, \quad \rho \geq 0. \quad (19)$$

The normalization implies $\sum_{j=1}^J \chi_j(q) = J$. If $\rho = 0$, presentation has no effect and $\chi_j(q) = 1$ for all j .

(A3) Coverage technology. Coverage of component j is

$$s_j(a, k, q) = \phi\left(\frac{\chi_j(q)a}{k}\right), \quad (20)$$

where $a \geq 0$ is attention and $k > 0$ is evaluative burden. The function $\phi : \mathbb{R}_+ \rightarrow [0, 1]$ satisfies

$$\phi(0) = 0, \quad \phi' > 0, \quad \phi'' < 0, \quad \lim_{x \rightarrow \infty} \phi(x) = 1, \quad (21)$$

with finite $\phi'(0)$. Thus coverage increases with exposure, has diminishing returns, and saturates at one.

(A4) Attention costs and capacity. Attention costs are linear in attention:

$$C(a, \tau) = \lambda_\tau a, \quad (22)$$

with

$$\lambda_L > \lambda_H > 0. \quad (23)$$

Lower-capacity citizens therefore face a higher marginal price of attention.

(A5) Attention choice. The value of accurate evaluation is

$$G(a, k, q) = \sum_{j=1}^J |v_j| \phi\left(\frac{\chi_j(q)a}{k}\right). \quad (24)$$

The citizen chooses attention according to

$$a^*(k, q, \tau) = \arg \max_{a \geq 0} \{G(a, k, q) - \lambda_\tau a\}. \quad (25)$$

(A6) Harmful selective-emphasis structure. For the selective-emphasis results, let

$$Q = \{j : q_j = 1\} \quad \text{and} \quad N = \{j : q_j = 0\}.$$

Both Q and N are nonempty. The emphasized components are attractive on net, while the non-emphasized components contain the net costs:

$$\sum_{j \in Q} v_j > 0, \quad \sum_{j \in N} v_j < 0. \quad (26)$$

A.6 Propositions

Proposition 1 (Bundling creates incomplete recovery). *Suppose the direct-comparison baseline allows full recovery, so that $s_j = 1$ for every component and $\hat{V} = V$. Under bundled presentation, if $s_j < 1$ for at least one policy-relevant component, then recovered value*

$$\hat{V} = \sum_{j=1}^J s_j v_j \quad (27)$$

can depart from the true value V . Bundling therefore introduces the possibility of suboptimal choice through incomplete component coverage.

Proposition 2 (Complexity and capacity reduce scrutiny per unit of burden). *Let*

$$b \equiv \frac{a}{k} \quad \text{and} \quad \mu_\tau(k) \equiv \lambda_\tau k. \quad (28)$$

Then the attention problem can be written as

$$b^*(\mu_\tau(k), q) = \arg \max_{b \geq 0} \left\{ \sum_{j=1}^J |v_j| \phi(\chi_j(q)b) - \mu_\tau(k)b \right\}. \quad (29)$$

The chosen scrutiny level $b^*(\mu_\tau(k), q)$ is weakly decreasing in $\mu_\tau(k)$, and strictly decreasing wherever the solution is interior. Since $\mu_\tau(k) = \lambda_\tau k$, higher complexity and lower capacity reduce scrutiny per unit of burden. Component coverage $s_j = \phi(\chi_j(q)b^*)$ is correspondingly weakly decreasing in $\mu_\tau(k)$. Raw attention,

$$a^*(k, q, \tau) = k b^*(\lambda_\tau k, q), \quad (30)$$

need not be decreasing in k .

Proposition 3 (Selective emphasis reallocates coverage). *Let q^0 denote neutral presentation, so that $\chi_j(q^0) = 1$ for every component, and let q^E denote a presentation that emphasizes the nonempty proper subset Q . If $\rho > 0$, then at any fixed $b > 0$,*

$$\phi(\chi_j(q^E)b) > \phi(b) \quad \text{for } j \in Q, \quad (31)$$

and

$$\phi(\chi_j(q^E)b) < \phi(b) \quad \text{for } j \in N. \quad (32)$$

Moreover,

$$\frac{1}{J} \sum_{j=1}^J \phi(\chi_j(q^E)b) \leq \phi(b), \quad (33)$$

with strict inequality when the salience weights are nonuniform and ϕ is strictly concave over the relevant range.

Proposition 4 (Selective emphasis raises perceived value at fixed scrutiny). *Under the harmful selective-emphasis structure in (A6), selective emphasis raises perceived value at any fixed $b > 0$:*

$$\hat{V}(b, q^E) > \hat{V}(b, q^0), \quad (34)$$

where

$$\hat{V}(b, q) = \sum_{j=1}^J \phi(\chi_j(q)b)v_j. \quad (35)$$

Thus, when attractive components are emphasized and costly components are left less salient, a harmful bundle can appear more valuable even though its true value V is unchanged.

Proposition 5 (Equilibrium scope condition). *Let*

$$b^0 = b^*(\mu_\tau(k), q^0) \quad \text{and} \quad b^E = b^*(\mu_\tau(k), q^E).$$

The equilibrium effect of selective emphasis can be written as

$$\hat{V}(b^E, q^E) - \hat{V}(b^0, q^0) = \underbrace{\left[\hat{V}(b^0, q^E) - \hat{V}(b^0, q^0) \right]}_{\text{fixed-scrutiny compositional effect}} + \underbrace{\left[\hat{V}(b^E, q^E) - \hat{V}(b^0, q^E) \right]}_{\text{endogenous scrutiny response}}. \quad (36)$$

The first term is positive under Proposition 4. The total equilibrium effect is positive whenever the endogenous scrutiny response does not offset the fixed-scrutiny compositional effect. The fixed-scrutiny effect is zero when $b = 0$, approaches zero as $b \rightarrow \infty$, and is positive for interior b . Selective emphasis is therefore most consequential when scrutiny is scarce but not collapsed.

Proposition 6 (Capacity shapes vulnerability). *For any $k > 0$, lower-capacity citizens face a higher effective price of scrutiny:*

$$\mu_L(k) = \lambda_L k > \lambda_H k = \mu_H(k). \quad (37)$$

Hence

$$b^*(\mu_L(k), q) \leq b^*(\mu_H(k), q). \quad (38)$$

Lower-capacity citizens are therefore more vulnerable to selective emphasis over ranges in which reduced scrutiny increases the fixed-scrutiny emphasis effect. This ordering is not unconditional: if lower-capacity citizens are pushed close to disengagement while higher-capacity citizens remain in the interior range, the emphasis effect can attenuate among lower-capacity citizens and appear more strongly among higher-capacity citizens.

A.7 Proofs

Proof of Proposition 1. If the direct-comparison baseline yields $s_j = 1$ for all j , then

$$\hat{V} = \sum_{j=1}^J s_j v_j = \sum_{j=1}^J v_j = V.$$

Under bundled presentation, if $s_j < 1$ for at least one policy-relevant component, then

$$\hat{V} = \sum_{j=1}^J s_j v_j$$

need not equal V . A decision based on $\hat{V}$ can therefore differ from the fully informed decision based on V . $\square$

Proof of Proposition 2. Substitute $a = kb$ into the attention problem:

$$a^*(k, q, \tau) = \arg \max_{a \geq 0} \left\{ \sum_{j=1}^J |v_j| \phi \left(\frac{\chi_j(q)a}{k} \right) - \lambda_\tau a \right\} \quad (39)$$

$$= k \arg \max_{b \geq 0} \left\{ \sum_{j=1}^J |v_j| \phi(\chi_j(q)b) - \lambda_\tau kb \right\}. \quad (40)$$

Let $\mu_\tau(k) = \lambda_\tau k$. The problem in b is

$$b^*(\mu_\tau(k), q) = \arg \max_{b \geq 0} \left\{ \sum_{j=1}^J |v_j| \phi(\chi_j(q)b) - \mu_\tau(k)b \right\}. \quad (41)$$

The objective is concave in b , since ϕ is concave and the cost term is linear. If the solution is interior, the first-order condition is

$$\sum_{j=1}^J |v_j| \chi_j(q) \phi'(\chi_j(q)b^*) = \mu_\tau(k). \quad (42)$$

The left-hand side is strictly decreasing in b^* , because

$$\frac{d}{db} \sum_{j=1}^J |v_j| \chi_j(q) \phi'(\chi_j(q)b) = \sum_{j=1}^J |v_j| \chi_j(q)^2 \phi''(\chi_j(q)b) < 0.$$

Thus a higher $\mu_\tau(k)$ implies a lower interior value of b^* . At the corner, $b^* = 0$, and further increases in $\mu_\tau(k)$ keep the solution at zero.

The corner threshold is

$$\bar{\mu}(q) = \phi'(0) \sum_{j=1}^J \chi_j(q) |v_j|. \quad (43)$$

If $\mu_\tau(k) \geq \bar{\mu}(q)$, the marginal value of scrutiny at zero is no greater than its marginal cost, so $b^* = 0$. If $\mu_\tau(k) < \bar{\mu}(q)$, the solution is interior and satisfies the first-order condition above.

Because $\mu_\tau(k) = \lambda_\tau k$, both higher k and higher λ_τ raise the effective price of scrutiny.

Since $s_j = \phi(\chi_j(q)b^*)$ and ϕ is increasing, coverage is weakly decreasing in $\mu_\tau(k)$. Finally,

$$a^*(k, q, \tau) = k b^*(\lambda_\tau k, q),$$

so the monotonicity of b^* does not imply that raw attention $a^*(k, q, \tau)$ must fall with k . Complexity reduces scrutiny per unit of burden, not necessarily raw effort. $\square$

Illustration. In the neutral case with $\chi_j(q) = 1$ for all j , let

$$W = \sum_{j=1}^J |v_j|$$

and suppose $\phi(x) = 1 - e^{-x}$. The interior first-order condition is

$$W e^{-b} = \lambda_\tau k,$$

so

$$b^*(\lambda_\tau k) = \log\left(\frac{W}{\lambda_\tau k}\right) \quad (44)$$

whenever $\lambda_\tau k < W$, and $b^* = 0$ otherwise. Raw attention is

$$a^*(k, q^0, \tau) = k \log\left(\frac{W}{\lambda_\tau k}\right) \quad (45)$$

over the interior range. This expression is hump-shaped in k : raw effort can initially rise with complexity even though scrutiny per unit of burden falls.

Proof of Proposition 3. Let $m = |Q|$, with $1 \leq m < J$. Under q^E , all emphasized components receive the same salience weight

$$\chi_Q = \frac{J(1 + \rho)}{J + \rho m}, \quad (46)$$

while all non-emphasized components receive

$$\chi_N = \frac{J}{J + \rho m}. \quad (47)$$

If $\rho > 0$, then $\chi_Q > 1$ and $\chi_N < 1$. Since ϕ is strictly increasing, for any $b > 0$,

$$\phi(\chi_Q b) > \phi(b)$$

and

$$\phi(\chi_N b) < \phi(b).$$

This proves the componentwise coverage result.

For the average-coverage result, use the normalization

$$\frac{1}{J} \sum_{j=1}^J \chi_j(q^E) = 1.$$

By Jensen's inequality and the concavity of ϕ ,

$$\frac{1}{J} \sum_{j=1}^J \phi(\chi_j(q^E)b) \leq \phi\left(\frac{1}{J} \sum_{j=1}^J \chi_j(q^E)b\right) \quad (48)$$

$$= \phi(b). \quad (49)$$

The inequality is strict when the salience weights are nonuniform and ϕ is strictly concave over the relevant range. $\square$

Proof of Proposition 4. At fixed b , the difference between selective emphasis and neutral presentation is

$$\begin{aligned} \hat{V}(b, q^E) - \hat{V}(b, q^0) &= \sum_{j \in Q} [\phi(\chi_Q b) - \phi(b)] v_j \\ &\quad + \sum_{j \in N} [\phi(\chi_N b) - \phi(b)] v_j. \end{aligned} \quad (50)$$

Because $\chi_Q > 1$ and ϕ is increasing,

$$\phi(\chi_Q b) - \phi(b) > 0.$$

Because $\chi_N < 1$ and ϕ is increasing,

$$\phi(\chi_N b) - \phi(b) < 0.$$

By (A6),

$$\sum_{j \in Q} v_j > 0 \quad \text{and} \quad \sum_{j \in N} v_j < 0.$$

Therefore the first term is positive and the second term is also positive, since it multiplies a

negative coverage change by a negative net value. Hence

$$\hat{V}(b, q^E) - \hat{V}(b, q^0) > 0.$$

□

Proof of Proposition 5. Add and subtract $\hat{V}(b^0, q^E)$:

$$\begin{aligned} \hat{V}(b^E, q^E) - \hat{V}(b^0, q^0) &= \hat{V}(b^0, q^E) - \hat{V}(b^0, q^0) \\ &\quad + \hat{V}(b^E, q^E) - \hat{V}(b^0, q^E). \end{aligned} \tag{51}$$

The first term is the fixed-scrutiny compositional effect. By Proposition 4, it is positive under (A6), with $\rho > 0$, Q nonempty and not equal to the full bundle, and $b^0 > 0$. The second term is the endogenous scrutiny response: presentation changes the attention problem and may therefore change b^* . The total equilibrium effect is positive if this second term is not negative enough to offset the fixed-scrutiny gain.

It remains to show the intermediate-range claim. Define the fixed-scrutiny effect

$$D(b) \equiv \hat{V}(b, q^E) - \hat{V}(b, q^0). \tag{52}$$

By Proposition 4, $D(b) > 0$ for all interior $b > 0$. At $b = 0$, $\phi(0) = 0$, so $D(0) = 0$. As $b \rightarrow \infty$, $\phi(\chi_j(q)b) \rightarrow 1$ and $\phi(b) \rightarrow 1$ for every j , so

$$\lim_{b \rightarrow \infty} D(b) = 0.$$

Thus the fixed-scrutiny effect is absent when scrutiny collapses and vanishes again when coverage saturates. Since $D(b)$ is continuous and positive for interior b , its largest effects occur between these extremes. Selective emphasis is therefore most consequential when scrutiny is scarce but not collapsed. □

Proof of Proposition 6. Since $\lambda_L > \lambda_H$, for any $k > 0$,

$$\mu_L(k) = \lambda_L k > \lambda_H k = \mu_H(k).$$

By Proposition 2, $b^*(\mu, q)$ is weakly decreasing in μ . Therefore

$$b^*(\mu_L(k), q) \leq b^*(\mu_H(k), q).$$

Lower-capacity citizens have less scrutiny per unit of burden for the same bundle.

The effect of this lower scrutiny on vulnerability depends on where each type lies on the fixed-scrutiny effect $D(b)$. If the lower-capacity type remains in the range where reduced scrutiny increases the compositional effect of emphasis, then lower capacity increases vulnerability. If the lower-capacity type is pushed close to $b = 0$, while the higher-capacity type remains in the interior range, the ordering can attenuate or reverse. This follows directly from Proposition 5, which shows that the emphasis effect is largest in the intermediate range and vanishes at the collapse and saturation limits. $\square$

B Experimental Design, Randomization, and Consequence Equivalence

This appendix documents the experimental design, treatment assignment, and consequence-equivalence logic for the four studies reported in the article. The appendix uses the article labels throughout: Study 1, Study 2, Study 3, and Study 4. The original Qualtrics export contains additional internal block names, but those labels are not used as article labels.

B.1 Study Sequence

Table 7: Study sequence

Study	Task	Role in the article
Study 1	Incentivized lottery benchmark	Tests whether bundled presentation alone worsens choice quality relative to isolated lotteries.
Study 2	Incentivized lottery-bundle choice	Tests whether complexity becomes more damaging when attractive components are selectively emphasized.
Study 3	Candidate platform choice	Tests whether presentation affects support for a candidate carrying a harmful institutional commitment and recognition of that commitment.
Study 4	Municipal reform-package evaluation	Tests the argument in a harder multi-dimensional reform task involving employment, service capacity, and administrative discretion.

B.2 Treatment-Cell Counts

Table 8: Treatment-cell counts by study

Study	Treatment cell	N observations	N respondents
Study 1	Direct comparison	541	541
Study 1	Higher-value bundled	562	562
Study 1	Lower-value bundled	539	539
Study 2	High complexity / Neutral	1054	527
Study 2	High complexity / Selective emphasis	874	437
Study 2	Low complexity / Neutral	918	459
Study 2	Low complexity / Selective emphasis	896	448
Study 3	High complexity / Neutral	459	459
Study 3	High complexity / Selective emphasis	448	448
Study 3	Low complexity / Neutral	527	527
Study 3	Low complexity / Selective emphasis	437	437
Study 4	High complexity / Neutral	442	442
Study 4	High complexity / Selective emphasis	465	465
Study 4	Low complexity / Neutral	447	447
Study 4	Low complexity / Selective emphasis	517	517

Note: Study 1 was added after the pilot wave and preregistered after that pilot. The Study 1 rows therefore report only respondents assigned to one of the three Study 1 arms. In the analytic file used here, 229 pilot respondents were not assigned to Study 1.

B.3 Randomization Balance

Tables 9–12 report randomization-balance diagnostics for each study. The tests are descriptive diagnostics of random assignment. The main estimates rely on randomized treatment assignment rather than covariate adjustment.

Table 9: Randomization balance: Study 1

Variable	Direct comparison	Higher-value bundled	Lower-value bundled	Joint p-value
Age	32.84	34.88	34.33	0.033
Risk aversion	0.68	0.71	0.68	0.484
Cognitive score	2.51	2.43	2.44	0.476
Higher-capacity indicator	0.39	0.36	0.37	0.595
Academic trust	0.01	-0.00	0.02	0.947
Sex, joint test				0.528
Education, joint test				0.386
City, joint test				0.566
Field date, joint test				

Note: Field-date balance is not reported for Study 1 because Study 1 was added after the pilot wave and assigned through a separate study block. The remaining rows report balance on pre-treatment covariates for respondents assigned to the Study 1 arms.

Table 10: Randomization balance: Study 2

Variable	High complexity / Neutral	High complexity / Selective emphasis	Low complexity / Neutral	Low complexity / Selective emphasis	Joint p-value
Age	33.97	33.51	34.59	34.56	0.566
Risk aversion	0.70	0.68	0.68	0.68	0.789
Cognitive score	2.51	2.44	2.42	2.45	0.687
Higher-capacity indicator	0.38	0.38	0.36	0.38	0.931
Academic trust	-0.02	-0.03	0.05	0.04	0.537
Sex, joint test					0.150
Education, joint test					0.910
City, joint test					0.185
Field date, joint test					0.602

Table 11: Randomization balance: Study 3

Variable	High complexity / Neutral	High complexity / Selective emphasis	Low complexity / Neutral	Low complexity / Selective emphasis	Joint p-value
Age	34.59	34.56	33.97	33.51	0.566
Risk aversion	0.68	0.68	0.70	0.68	0.789
Cognitive score	2.42	2.45	2.51	2.44	0.687
Higher-capacity indicator	0.36	0.38	0.38	0.38	0.931
Academic trust	0.05	0.04	-0.02	-0.03	0.537
Sex, joint test					0.130
Education, joint test					0.903
City, joint test					0.193
Field date, joint test					0.610

Table 12: Randomization balance: Study 4

Variable	High complexity / Neutral	High complexity / Selective emphasis	Low complexity / Neutral	Low complexity / Selective emphasis	Joint p-value
Age	35.11	34.07	33.56	33.93	0.349
Risk aversion	0.69	0.66	0.68	0.71	0.537
Cognitive score	2.42	2.46	2.50	2.46	0.807
Higher-capacity indicator	0.37	0.37	0.40	0.36	0.683
Academic trust	0.05	0.04	-0.02	-0.03	0.520
Sex, joint test					0.618
Education, joint test					0.946
City, joint test					0.185
Field date, joint test					0.531

Note: Balance tables are reported separately by study because treatment labels are study-specific. Studies that share the same respondent pool and related assignment structure can therefore have similar or identical covariate means across corresponding cells. The tables are retained separately to document the realized balance for each article-labeled study.

B.4 Randomization and Assignment

Table 13 summarizes the main randomization structure used in the article.

Study	Randomization	Notes
Study 1	Three-arm between-subject assignment	Respondents are assigned to a direct-comparison baseline, a lower-value bundled option, or a higher-value bundled option.
Study 2	2×2 between-subject assignment	Complexity is low versus high. Selective emphasis is neutral versus highlighted/favorably cued. Respondents complete incentivized lottery-bundle decisions.
Study 3	2×2 assignment	Complexity varies by platform length. Selective emphasis varies through a favorable summary headline for the reform candidate.
Study 4	2×2 assignment	Complexity varies by the number of municipal measures shown. Selective emphasis varies through neutral versus favorable measure labels.

The complexity assignment was counterbalanced across the abstract and political modules. Respondents assigned to higher-complexity presentation in the lottery-bundle module were assigned to lower-complexity presentation in the candidate and municipal modules, while respondents assigned to lower-complexity lottery bundles were assigned to higher-complexity political modules. This counterbalancing reduces the risk that cumulative exposure to high-complexity tasks is concentrated among the same respondents throughout the survey. The article therefore reports study-specific treatment indicators corresponding to the presentation actually shown in each module, and the balance tables are reported separately

for each study.

Where feasible, option placement is randomized within screens, task order is randomized within blocks, and rule or item order is randomized. The analysis uses the study-specific treatment indicators corresponding to the treatment actually shown in each study.

B.5 Study 1: Incentivized Lottery Benchmark

Study 1 compares choices over isolated lotteries and bundled lottery presentations. The direct-comparison baseline shows both lotteries in isolated form. The bundled conditions require respondents to reconstruct one option from a sequence of changes.

Table 14: Study 1 benchmark conditions

Condition	Option A		Option B		Main comparison
Direct-comparison baseline	High-EV	iso-lated lottery	Low-EV	iso-lated lottery	Baseline ability to identify the better option.
Lower-value bundled option	High-EV	iso-lated lottery	Low-EV	bun-dle	Whether an objectively inferior option becomes more acceptable when bundled.
Higher-value bundled option	Low-EV	iso-lated lottery	High-EV	bun-dle	Whether an objectively superior option becomes harder to identify when bundled.

Table 15: Study 1 lottery payoffs

Lottery	Final bowl (P, B, G)	Total balls	Expected value
Low EV	(3, 2, 15)	20	12
High EV	(4, 5, 11)	20	19.5

The outcome is suboptimal choice, coded as one when the respondent selects the objectively lower-EV option.

B.6 Study 2: Incentivized Lottery-Bundle Choice

Study 2 tests whether complexity becomes more damaging when attractive components of the lottery bundle are selectively emphasized. Complexity changes the number of rule lines respondents must process. Selective emphasis changes which components are visually highlighted. The payoff-relevant final bowl is fixed within each comparison.

Table 16: Study 2 treatment cells

Cell	Complexity	Selective emphasis	What changes
Low / neutral	Low	No	Shorter bundle, neutral presentation.
Low / selective emphasis	Low	Yes	Same final bowl as low/neutral, but favorable components are highlighted.
High / neutral	High	No	Longer bundle, neutral presentation.
High / selective emphasis	High	Yes	Same final bowl as high/neutral, but favorable components are highlighted.

Table 17: Study 2 payoff-relevant final bowls

Task	Option	Final bowl (P, B, G)	Total balls	Expected value
Task A	Status quo	(2, 6, 12)	20	15.0
Task A	Low EV	(1, 5, 14)	20	10.5
Task A	High EV	(3, 8, 9)	20	21.0
Task B	Status quo	(2, 4, 14)	20	12.0
Task B	Low EV	(1, 3, 16)	20	7.5
Task B	High EV	(3, 6, 11)	20	18.0

Selective-emphasis equivalence. In selective-emphasis arms, highlighted components are chosen so that attending only to the highlighted changes can create a favorable interim impression. The final bowl remains unchanged once all components are counted. Thus the highlighted tally is an attentional manipulation, not a different payoff-relevant lottery.

Table 18: Selective emphasis changes interim salience, not final consequences

Feature	Neutral presentation	Selective-emphasis presentation
Final payoff-relevant bowl	Same final bowl within comparison	Same final bowl within comparison
Rule content	All rule components shown without highlighting	All rule components shown, with favorable components highlighted
Interim impression if only highlighted parts are counted	Not applicable	Can resemble a higher-EV pattern
Correct evaluation rule	Count all rule components	Count all rule components

B.7 Study 3: Candidate Platform Choice

Study 3 tests whether the presentational logic extends to electoral evaluation. Respondents choose between two candidates. The reform candidate carries the harmful institutional commitment, while the comparison candidate preserves oversight.

Table 19: Study 3 treatment structure

Cell	Complexity	Selective emphasis		What changes	
Low / neutral	Two 3-item platforms	Generic headline	summary	Harmful institutional commitment appears within short, neutrally presented candidate platforms.	
Low / selective emphasis	Two 3-item platforms	Favorable candidate headline	reform-summary	Same short platform structure, but the reform candidate receives a favorable summary headline.	
High / neutral	Two 8-item platforms	Generic headline	summary	Both candidate platforms are longer, with the harmful institutional commitment embedded in the reform candidate's platform.	
High / selective emphasis	Two 8-item platforms	Favorable candidate headline	reform-summary	Both candidate platforms are longer, and the reform candidate receives a favorable summary headline.	

Table 20: Study 3 core institutional contrast

Dimension	Reform candidate	Comparison candidate
Permit processing	Faster or expedited processing	Scheduled or standards-based processing
Oversight feature	Mayor's direct sign-off without committee review	Committee approval with published minutes
Institutional implication	Weaker oversight and greater executive discretion	Preserved review and public accountability

Across Study 3 arms, the core institutional contrast is held fixed: the reform candidate is associated with faster permit approval through mayoral direct sign-off without committee review, while the comparison candidate preserves committee approval and published

minutes. The treatments change platform length and the reform candidate’s summary headline, not the institutional implication being evaluated. The exact candidate texts shown to respondents are reported in Appendix C.

B.8 Study 4: Municipal Reform Package

Study 4 presents respondents with a municipal reform package whose measures affect employment, discretionary allocation, and service capacity. Complexity varies by the number of measures shown. Selective emphasis varies through favorable versus neutral measure titles.

Table 21: Study 4 treatment structure

Cell	Complexity	Selective emphasis	What changes
Low / neutral	Two measures	Neutral titles	Respondents evaluate a shorter municipal package with neutral measure labels.
Low / selective emphasis	Two measures	Favorable titles	Same complexity level, with attractive elements emphasized through favorable labels.
High / neutral	Four measures	Neutral titles	Respondents evaluate the full municipal package with neutral measure labels.
High / selective emphasis	Four measures	Favorable titles	Respondents evaluate the full package with attractive elements emphasized.

The full municipal package is harmful relative to the status quo. It combines some attractive measures with a net reduction in employment, an increase in discretionary allocation, and a reduction in service capacity.

Table 22: Study 4 municipal package consequences

Measure	Employment jobs	Discretionary allocation TL; + means more discre- tion	Service capacity residents served
1	+300	+200,000,000	+3,000
2	+200	-250,000,000	0
3	-700	+350,000,000	-6,000
4	-200	+250,000,000	0
Total	-400	+550,000,000	-3,000

Within each complexity level, the selective-emphasis manipulation changes measure labels and salience, not the underlying municipal consequences. In the full package, the consequences sum to 400 fewer jobs, 550 million TL more discretionary allocation, and 3,000 fewer residents served.

Study 4 applies the design to a harder political setting. The high-complexity condition shows more policy information, while the selective-emphasis condition changes the labels attached to the measures. Its purpose is to test whether the presentational logic appears in a realistic multidimensional package.

B.9 Design Equivalence Across Studies

This appendix documents the design features needed to interpret the four studies as a cumulative experimental sequence. Study 1 and Study 2 separate payoff-relevant consequences from presentation, allowing clean tests of bundling, complexity, and selective emphasis in incentivized lottery tasks. Study 3 and Study 4 preserve the central harmful institutional contrast while embedding it in more realistic political settings. Across the sequence, the designs vary how respondents encounter the relevant consequences rather than treating support for harmful options as a direct expression of underlying preferences alone.

C Full Survey Instrument and Treatment Materials

This appendix reports the respondent-facing materials for the four studies analyzed in the article. The original Qualtrics export contains internal block names and embedded fields. I replace those internal labels with the article labels, Study 1 through Study 4, and report the substantive text shown to respondents. Dynamic fields such as the respondent’s city, candidate order, and candidate labels are shown in brackets.

Note on language. The survey was fielded in Turkish. English versions below are translations for readers. Dynamic fields such as city name, candidate order, and candidate labels are shown in brackets.

C.1 Common Incentivized Lottery Instructions

The following instructions were shown before the incentivized lottery tasks.

C.1.1 English version

In this section, you will answer a set of questions.

During the experiment, you will answer a set of questions by choosing one bowl each time. At the end, the computer randomly picks one of the bowls you chose. From that chosen bowl, a ball is drawn at random. Your credits equal the payout for the color drawn.

Purple = 60 credits. Blue = 30 credits. Gray = 0 credits.

If your total credits place you in the top 15 percent, you get one extra raffle entry. Do your best.

Some options will simply list the number of balls in the bowl. If you choose such an option, a ball will be drawn from the bowl as described.

Other options will describe an initial bowl and a set of changes. If you choose this option, the initial bowl will be modified by implementing the changes in the order listed. Afterwards, a ball will be drawn from the final bowl.

Note that the initial and final configurations of bowls are composed of 20 balls.

C.1.2 Turkish version

Bu bölümde, bir dizi soruyu cevaplayacaksınız.

Anket sırasında, her seferinde bir kase seçerek bir dizi soruyu yanıtlayacaksınız. Bittiğinde, bilgisayar seçtiğiniz kaselerden rastgele birini seçecek. Seçilen bu kaseden rastgele bir bilye çekilecek. Anket krediniz, çekilen bilyenin rengine karşılık gelen ödeme tutarına eşit olacak.

Mor = 60 kredi. Mavi = 30 kredi. Gri = 0 kredi.

Toplam krediniz ile tüm katılımcılar arasında ilk yüzde 15 içinde yer alırsanız, 1 ek çekiliş hakkı kazanırsınız. Elinizden gelenin en iyisini yapın.

Bazı seçeneklerde kasedeki bilye sayıları doğrudan listelenir. Bu tür bir seçeneği seçerseniz, açıklamada belirtildiği gibi kaseden bir bilye çekilir.

Diğer seçeneklerde ise başlangıç kasesi ve bir değişim listesi verilir. Bu seçeneği seçerseniz, başlangıç kasası listelenen sırayla bu değişiklikler uygulanarak güncellenir. Ardından nihai kaseden bir bilye çekilir.

Not: Başlangıçta ve nihai halinde her kasede 20 bilye bulunur.

C.2 Study 1: Incentivized Lottery Benchmark

Study 1 uses the same lottery instructions above and assigns respondents to one of three benchmark conditions: direct-comparison baseline, lower-value bundled option, or higher-value bundled option. The treatment structure and final payoff-relevant bowls are reported in Appendix Tables 14 and 15.

C.2.1 Task prompt

English version

Task overview. You will pick one bowl filled with colored balls. After you decide which bowl you want, we will draw one ball at random. You will earn experimental credits proportional to the value of the color drawn.

Payouts: Purple Ball = 60, Blue Ball = 30, Gray Ball = 0.

Important:

- Some options will display the number of balls in the bowl. If you choose one of these, a ball will be drawn from that bowl.
- Other options will show a starting bowl and a list of changes. If you choose one of these, the changes will be applied in order to form the final bowl, and then a ball will be drawn from it.

Turkish version

Görevin özeti. Renkli bilyelerle dolu bir kase seçeceksiniz. Hangi kaseyi istediğinize karar verdikten sonra, bu kaseden rastgele bir bilye çekilecektir. Kazancınız, çekilen bilyenin rengine karşılık gelen anket kredisine göre belirlenecektir.

Ödemeler: Mor Bilye = 60, Mavi Bilye = 30, Gri Bilye = 0.

Önemli:

- Bazı seçeneklerde kasedeki nihai bilye sayıları doğrudan gösterilir. Bu tür bir seçeneği seçerseniz, o nihai kaseden bir bilye çekilir.
- Diğer seçeneklerde bir başlangıç kasesi ve bir değişiklik listesi gösterilir. Bu seçeneği seçerseniz, değişiklikler sırasıyla başlangıç kasesine uygulanarak nihai kase oluşturulur; ardından bu nihai kaseden bir bilye çekilir.

C.2.2 Study 1 process questions

Difficulty

How difficult did you find the question above?

1 = Very easy. 7 = Very difficult.

Yukarıdaki soruyu ne kadar zor buldunuz?

1 = Çok kolay. 7 = Çok zor.

Confidence

How confident do you feel about your choice above?

1 = Not confident at all. 7 = Very confident.

Yukarıdaki seçiminizden ne kadar eminsiniz?

1 = Hiç emin değilim. 7 = Çok eminim.

C.3 Study 2: Incentivized Lottery-Bundle Choice

Study 2 uses the same incentivized lottery instructions and varies complexity and selective emphasis in a 2×2 design. The low-complexity condition uses shorter rule lists. The high-complexity condition uses longer rule lists. In the selective-emphasis condition, favorable components are highlighted, but the final payoff-relevant bowl remains unchanged once all rule components are counted.

C.3.1 Study 2 task prompt

English version

Task overview. You will pick one bowl filled with colored balls. After you decide which bowl you want, we will draw one ball at random. You will earn experimental credits proportional to the value of the color drawn.

Payouts: Purple Ball = 60, Blue Ball = 30, Gray Ball = 0.

Important:

- Some options will display the number of balls in the bowl. If you choose one of these, a ball will be drawn from that bowl.
- Other options will show a starting bowl and a list of changes. If you choose one of these, the changes will be applied in order to form the final bowl, and then a ball will be drawn from it.

Turkish version

Yapacaklarınızın Özeti. Renkli bilyelerle dolu bir kase seçeceksiniz. Hangi kaseyi istediğinize karar verdikten sonra, içinden rastgele bir bilye çekilecek. Anket krediniz, çekilen bilyenin değerine orantılı olacaktır.

Ödemeler: Mor Bilye = 60, Mavi Bilye = 30, Gri Bilye = 0.

Önemli:

- Bazı seçeneklerde kasedeki nihai bilye sayıları doğrudan gösterilir. Bu tür bir seçeneği seçerseniz, o nihai kaseden bir bilye çekilir.
- Diğer seçeneklerde bir başlangıç kasesi ve bir değişiklik listesi gösterilir. Bu seçeneği seçerseniz, değişiklikler sırasıyla başlangıç kasesine uygulanarak nihai kase oluşturulur; ardından bu nihai kaseden bir bilye çekilir.

C.3.2 Study 2 treatment-specific rule lists

The table reports the rendered rule-change templates used in Study 2. The neutral and selective-emphasis arms used the same rule content. The selective-emphasis arm changed only the visual presentation of selected favorable components, shown below in bold/color. In the neutral arms, the same rule lists appeared without bold/color emphasis. The payoff-relevant starting and final bowls are reported in the consequence-equivalence appendix tables.

Condition	English rule list
Low complexity, low-EV bundle, neutral	1. Add 2 Blue, Remove 1 Purple, Add 3 Gray 2. Add 2 Purple, Add 3 Gray, Remove 5 Blue 3. Remove 4 Gray, Add 2 Blue, Remove 2 Purple
Low complexity, low-EV bundle, selective emphasis	1. Add 2 Blue, Remove 1 Purple, Add 3 Gray 2. Add 2 Purple, Add 3 Gray, Remove 5 Blue 3. Remove 4 Gray, Add 2 Blue, Remove 2 Purple
Low complexity, high-EV bundle, neutral	1. Add 3 Blue, Remove 6 Gray, Remove 1 Purple 2. Add 1 Purple, Remove 3 Blue, Add 1 Gray 3. Add 2 Gray, Add 1 Purple, Add 2 Blue
Low complexity, high-EV bundle, selective emphasis	1. Add 3 Blue, Remove 6 Gray, Remove 1 Purple 2. Add 1 Purple, Remove 3 Blue, Add 1 Gray 3. Add 2 Gray, Add 1 Purple, Add 2 Blue
High complexity, low-EV bundle, neutral	1. Add 2 Blue, Remove 1 Purple, Add 3 Gray 2. Add 3 Purple, Remove 1 Blue, Add 2 Gray 3. Add 2 Purple, Add 3 Gray, Remove 5 Blue 4. Add 1 Blue, Remove 2 Gray, Remove 3 Purple 5. Remove 4 Gray, Add 2 Blue, Remove 2 Purple
High complexity, low-EV bundle, selective emphasis	1. Add 2 Blue, Remove 1 Purple, Add 3 Gray 2. Add 3 Purple, Remove 1 Blue, Add 2 Gray 3. Add 2 Purple, Add 3 Gray, Remove 5 Blue 4. Add 1 Blue, Remove 2 Gray, Remove 3 Purple 5. Remove 4 Gray, Add 2 Blue, Remove 2 Purple
High complexity, high-EV bundle, neutral	1. Add 3 Blue, Remove 6 Gray, Remove 1 Purple 2. Add 1 Purple, Remove 1 Blue, Add 2 Gray 3. Add 1 Purple, Remove 3 Blue, Add 1 Gray 4. Remove 2 Gray, Remove 1 Purple, Add 1 Blue 5. Add 2 Gray, Add 1 Purple, Add 2 Blue
High complexity, high-EV bundle, selective emphasis	1. Add 3 Blue, Remove 6 Gray, Remove 1 Purple 2. Add 1 Purple, Remove 1 Blue, Add 2 Gray 3. Add 1 Purple, Remove 3 Blue, Add 1 Gray 4. Remove 2 Gray, Remove 1 Purple, Add 1 Blue 5. Add 2 Gray, Add 1 Purple, Add 2 Blue

Table 23: Study 2 English rule templates by complexity, expected-value condition, and selective emphasis.

Condition	Turkish rule list
Düşük karmaşıklık, düşük EV paket, nötr sunum	1. 2 Mavi Ekle, 1 Mor Çıkar, 3 Gri Ekle 2. 2 Mor Ekle, 3 Gri Ekle, 5 Mavi Çıkar 3. 4 Gri Çıkar, 2 Mavi Ekle, 2 Mor Çıkar
Düşük karmaşıklık, düşük EV paket, seçici vurgu	1. 2 Mavi Ekle, 1 Mor Çıkar, 3 Gri Ekle 2. 2 Mor Ekle, 3 Gri Ekle, 5 Mavi Çıkar 3. 4 Gri Çıkar, 2 Mavi Ekle, 2 Mor Çıkar
Düşük karmaşıklık, yüksek EV paket, nötr sunum	1. 3 Mavi Ekle, 6 Gri Çıkar, 1 Mor Çıkar 2. 1 Mor Ekle, 3 Mavi Çıkar, 1 Gri Ekle 3. 2 Gri Ekle, 1 Mor Ekle, 2 Mavi Ekle
Düşük karmaşıklık, yüksek EV paket, seçici vurgu	1. 3 Mavi Ekle, 6 Gri Çıkar, 1 Mor Çıkar 2. 1 Mor Ekle, 3 Mavi Çıkar, 1 Gri Ekle 3. 2 Gri Ekle, 1 Mor Ekle, 2 Mavi Ekle
Yüksek karmaşıklık, düşük EV paket, nötr sunum	1. 2 Mavi Ekle, 1 Mor Çıkar, 3 Gri Ekle 2. 3 Mor Ekle, 1 Mavi Çıkar, 2 Gri Ekle 3. 2 Mor Ekle, 3 Gri Ekle, 5 Mavi Çıkar 4. 1 Mavi Ekle, 2 Gri Çıkar, 3 Mor Çıkar 5. 4 Gri Çıkar, 2 Mavi Ekle, 2 Mor Çıkar
Yüksek karmaşıklık, düşük EV paket, seçici vurgu	1. 2 Mavi Ekle, 1 Mor Çıkar, 3 Gri Ekle 2. 3 Mor Ekle, 1 Mavi Çıkar, 2 Gri Ekle 3. 2 Mor Ekle, 3 Gri Ekle, 5 Mavi Çıkar 4. 1 Mavi Ekle, 2 Gri Çıkar, 3 Mor Çıkar 5. 4 Gri Çıkar, 2 Mavi Ekle, 2 Mor Çıkar
Yüksek karmaşıklık, yüksek EV paket, nötr sunum	1. 3 Mavi Ekle, 6 Gri Çıkar, 1 Mor Çıkar 2. 1 Mor Ekle, 1 Mavi Çıkar, 2 Gri Ekle 3. 1 Mor Ekle, 3 Mavi Çıkar, 1 Gri Ekle 4. 2 Gri Çıkar, 1 Mor Çıkar, 1 Mavi Ekle 5. 2 Gri Ekle, 1 Mor Ekle, 2 Mavi Ekle
Yüksek karmaşıklık, yüksek EV paket, seçici vurgu	1. 3 Mavi Ekle, 6 Gri Çıkar, 1 Mor Çıkar 2. 1 Mor Ekle, 1 Mavi Çıkar, 2 Gri Ekle 3. 1 Mor Ekle, 3 Mavi Çıkar, 1 Gri Ekle 4. 2 Gri Çıkar, 1 Mor Çıkar, 1 Mavi Ekle 5. 2 Gri Ekle, 1 Mor Ekle, 2 Mavi Ekle

Table 24: Study 2 Turkish rule templates by complexity, expected-value condition, and selective emphasis.

C.3.3 Study 2 process questions

Difficulty

How difficult did you find the question above?

1 = Very easy. 7 = Very difficult.

Yukarıdaki soruyu ne kadar zor buldunuz?

1 = Çok kolay. 7 = Çok zor.

Confidence

How confident do you feel about your choice above?

1 = Not confident at all. 7 = Very confident.

Yukarıdaki seçiminizden ne kadar eminsiniz?

1 = Hiç emin değilim. 7 = Çok eminim.

C.4 Study 3: Candidate Platform Choice

C.4.1 Introductory screen

English version

In this task, you will read two candidates' proposals for how the city should handle small business permits and inspections. Then you will answer a few questions about your views.

Please read the proposal carefully and answer as thoughtfully as you can.

I understand. I will read the candidates' proposals carefully and answer the questions as thoughtfully as I can.

Turkish version

Bu bölümde, iki adayın belediyede küçük işletme ruhsatları ve denetimlerin nasıl yürütüleceğine dair önerilerini okuyacaksınız. Ardından görüşlerinizi soran birkaç soruyu yanıtlayacaksınız.

Lütfen önerileri dikkatle okuyun ve soruları mümkün olduğunca düşünerek yanıtlayın.

Anladım. Adayların önerilerini dikkatle okuyacak ve soruları mümkün olduğunca düşünerek yanıtlayacağım.

C.4.2 Candidate platform screen

The candidate order was randomized. In the text below, the reform candidate is the candidate carrying the oversight-weakening provision. The summary headings above the candidate platforms were generated through embedded fields. In the neutral condition, both candidates

received the generic heading “At a glance: Municipal Permit and Inspection Plan.” In the selective-emphasis condition, the comparison candidate retained this generic heading, while the reform candidate instead received the favorable heading “At a glance: Clear deadlines for permit decisions and Faster processing for routine applications.” Within each complexity condition, the underlying platform items were identical across neutral and selective-emphasis arms. In Turkish, “At a glance” was presented as “Kısaca.”

English version

Candidate Choice

Small business permits and inspections

Please read the two candidates’ proposals below. Then you will be asked which candidate you would support.

Table 25: Study 3 candidate platforms shown to respondents, English version

Complexity	Reform candidate	Comparison candidate
Low	• Set a clear timeline for permit decisions. • Use an expedited lane for some applications by making exceptions. • Approve permits through the mayor's direct sign-off, without a committee.	• Establish a fixed schedule for permit processing. • Offer a fast option for applications that meet preset standards. • Approve permits through a committee with published minutes.
High	• Use a single standard application form for permits. • Use a checklist to reduce missing documents. • Assign a case owner for each application. • Set internal time limits for department feedback. • Use a shared tracking system to follow applications. • Set a clear timeline for permit decisions. • Use an expedited lane for some applications by making exceptions. • Approve permits through the mayor's direct sign-off, without a committee.	• Use a standard application form for permits. • Use a checklist to reduce missing documents. • Assign a point person for each application. • Set internal time limits for departments to respond. • Use a shared record system to track applications. • Establish a fixed schedule for permit processing. • Offer a fast option for applications that meet preset standards. • Approve permits through a committee with published minutes.

Turkish version

Aday Seçimi

Küçük işletme ruhsatları ve denetimler

Lütfen iki adayın önerilerini okuyun. Ardından hangi adayı destekleyeceğiniz sorulacaktır.

Table 26: Study 3 candidate platforms shown to respondents, Turkish version

Karmaşıklık	Reform adayı	Karşılaştırma adayı
Düşük	- Ruhsat kararları için net bir takvim belirlemek. - Bazı başvurulara istisna yapıp hızlandırılmış bir yol kullanmak. - Ruhsatları, komisyon olmadan belediye başkanının doğrudan onayıyla vermek.	- Ruhsat işlemleri için sabit bir takvim oluşturmak. - Önceden belirlenmiş standartları karşılayan başvurular için hızlı bir seçenek sunmak. - Ruhsatları, tutanakları yayımlanan bir komisyonla onaylamak.
Yüksek	- Ruhsat başvuruları için tek bir standart form kullanmak. - Eksik evrakı azaltmak için kontrol listesi kullanmak. - Her başvuru için sorumlu kişi belirlemek. - Birimlerden geri dönüş için iç süre sınırları koymak. - Başvuruları takip etmek için ortak takip sistemi kullanmak. - Ruhsat kararları için net bir takvim belirlemek. - Bazı başvurulara istisna yapıp hızlandırılmış bir yol kullanmak. - Ruhsatları, komisyon olmadan belediye başkanının doğrudan onayıyla vermek.	- Ruhsat başvuruları için standart bir form uygulamak. - Eksikleri azaltmak için kontrol listesi kullanmak. - Her başvuru için ilgili kişi belirlemek. - Birimlerin yanıt vermesi için iç zaman sınırları belirlemek. - Başvuru sürecini takip etmek için ortak kayıt sistemi kullanmak. - Ruhsat işlemleri için sabit bir takvim oluşturmak. - Önceden belirlenmiş standartları karşılayan başvurular için hızlı bir seçenek sunmak. - Ruhsatları, tutanakları yayımlanan bir komisyonla onaylamak.

C.4.3 Study 3 outcome questions

Candidate choice

Which candidate would you vote for?

Candidate A. Candidate B.

Bugün karar vermeniz gerekse hangi adayı desteklerdiniz?

Aday A. Aday B.

Vote likelihood

How likely are you to vote for this candidate in a real election?

1 = Very unlikely. 7 = Very likely.

Gerçek bir seçimde bu adaya oy verme olasılığınız nedir?

1 = Hiç olası değil. 7 = Çok olası.

Difficulty

How difficult did you find the candidate comparison you read?

1 = Very easy. 7 = Very difficult.

Okuduğunuz aday karşılaştırmasını ne kadar zor buldunuz?

1 = Çok kolay. 7 = Çok zor.

Confidence

How confident do you feel about your support choice above?

1 = Not confident at all. 7 = Very confident.

Yukarıdaki oylama seçiminiz konusunda ne kadar eminsiniz?

1 = Hiç emin değilim. 7 = Çok eminim.

Open-ended reason

It is important for us to understand your reasoning. Please briefly explain why you chose this candidate. Write 1 to 2 sentences. Minimum 15 characters.

Gerekçenizi anlamak bizim için önemli. Lütfen bu adayı neden seçtiğinizi 1 ya da 2 cümleyle kısaca yazın. En az 15 karakter.

Recognition

According to the text, which candidate proposed approving permits through the mayor's direct sign-off, without a committee?

Candidate A. Candidate B. Not sure / I do not remember.

Metne göre, ruhsatların komisyon olmadan belediye başkanı onayıyla verilmesini hangi aday öneriyor?

Aday A. Aday B. Emin değilim / Hatırlamıyorum.

C.5 Study 4: Municipal Reform Package

C.5.1 Introductory screen

English version

In this task, you will read an example city policy proposal and then evaluate it. Your answers will be used to understand what kinds of local policies people support, and this evidence can inform future policy-making in neighborhoods like yours.

Please read the proposal carefully and answer as thoughtfully as you can.

I understand. I will read the city policy proposal carefully and answer the questions as thoughtfully as I can.

Turkish version

Bu bölümde, örnek bir şehir politikası tasarısı okuyacak ve ardından değerlendireceksiniz. Yanıtlarınız, insanların ne tür yerel politikaları desteklediğini anlamak için akademik çalışmalarda kullanılabilir. Elde edilen bulgular, sizin mahalleniz gibi mahallelerde gelecekteki politika yapımına ışık tutabilir.

Lütfen öneriyi dikkatlice okuyun ve elinizden geldiğince özenli yanıt verin.

Anlıyorum. Bir politika tasarısını dikkatlice okuyacağım ve sorulan soruları bildiğince özenli bir şekilde yanıtlayacağım.

C.5.2 Municipal reform article

The respondent's city was piped into the newspaper name. The article headline and explanatory box were held constant across Study 4 conditions. Complexity changed the number of regulations shown. Selective emphasis added favorable regulation titles and bolded good-direction numerical consequences: positive employment changes, reductions in discretionary spending, and positive service-capacity changes.

English version

[Respondent's city] City Gazette – Politics

Mayor Proposes a Single “Reform Package” for City Projects

News Desk – January 19, 2026

According to a draft submitted to the city council this week, the mayor’s proposal would implement multiple regulations as one package. If the package is adopted, all regulations would take effect at the same time.

Expert report (12 months)

- Employment: expected change in the number of jobs.
- Discretionary spending (TRY): spending where the city can decide more freely.
- Service capacity: expected change in the number of people who can receive the service.

Turkish version

[Katılımcının şehri] Şehir Gazetesi – Siyaset

Belediye Başkanı Şehir Projeleri İçin Tek Bir “Reform Paketi” Sunuyor

Haber Merkezi – 19 Ocak 2026

Belediye başkanı birden fazla düzenlemeyi tek bir paket olarak sunuyor. Paket kabul edilirse, tüm düzenlemeler aynı anda yürürlüğe girecek.

Uzman raporu (12 ay)

- İstihdam: iş sayısındaki değişim.
- İnisiyatife bağlı harcama (TL): belediyenin daha serbest karar verebildiği harcama.
- Hizmet kapasitesi: hizmet alabilecek kişi sayısındaki değişim.

C.5.3 Municipal regulations

The low-complexity condition displayed two regulations. The high-complexity condition displayed four regulations. Regulation numbering below follows the rendered screen in each condition: the downtown revitalization fund appeared as Regulation 2 in the low-complexity condition and as Regulation 3 in the high-complexity condition.

Table 27: Study 4 municipal regulations, English version

Condition	Regulation title	Text shown or translated text
Low/control and high/control	Regulation 1	What changes: For selected investment projects, the city would be allowed to procure faster using a negotiated procedure instead of an open tender. Expert estimate (12 months): Employment +300 Discretionary spending +200 million TRY Service capacity +3,000.
Low/selective emphasis and high/selective emphasis	Regulation 1 – Faster Project Delivery	What changes: For selected investment projects, the city would be allowed to procure faster using a negotiated procedure instead of an open tender. Expert estimate (12 months): Employment +300 Discretionary spending +200 million TRY Service capacity +3,000 .
High/control only	Regulation 2	What changes: The city would fund an independent procurement audit unit that reviews major contracts and publishes a compliance report every three months. Contracts that fail the audit could not be renewed through exceptions and would have to be re-tendered through an open process. Expert estimate (12 months): Employment +200 Discretionary spending -250 million TRY.
High/selective emphasis only	Regulation 2 – Stronger Oversight	What changes: The city would fund an independent procurement audit unit that reviews major contracts and publishes a compliance report every three months. Contracts that fail the audit could not be renewed through exceptions and would have to be re-tendered through an open process. Expert estimate (12 months): Employment +200 Discretionary spending -250 million TRY .
Low/control and high/control	Regulation 2 in low complexity; Regulation 3 in high complexity	What changes: The city would create a downtown revitalization fund that issues discretionary grants to selected projects. The fund would be financed by reallocating the operating budget currently used for after-school programs across the city. Expert estimate (12 months): Employment -700 Discretionary spending +350 million TRY Service capacity -6,000.
Low/selective emphasis and high/selective emphasis	Regulation 2 – Downtown Revitalization Fund in low complexity; Regulation 3 – Downtown Revitalization Fund in high complexity	What changes: The city would create a fund that issues discretionary grants to selected downtown projects. The fund would be financed by reallocating the operating budget currently used for after-school programs across the city. Expert estimate (12 months): Employment -700 Discretionary spending +350 million TRY Service capacity -6,000.
High/control only	Regulation 4	What changes: The city would create a single public portal for contracts (tender documents, amendments, subcontractor notices). In addition, procurement rules would be amended to expand the use of negotiated procedures for certain contracts. Expert estimate (12 months): Employment -200 Discretionary spending +250 million TRY.
High/selective emphasis only	Regulation 4 – New Transparency Portal	What changes: The city would create a single public portal for contracts (tender documents, amendments, subcontractor notices). In addition, procurement rules would be amended to expand the use of negotiated procedures for certain contracts. Expert estimate (12 months): Employment -200 Discretionary spending +250 million TRY.

Table 28: Study 4 municipal regulations, Turkish version

Condition	Regulation title	Text shown
Düşük/kontrol ve yüksek/kontrol	Düzenleme 1	Ne değişiyor: Belirli yatırım projelerinde, açık ihale yerine pazarlık usulüyle daha hızlı satın alma yapılabilecek. Uzman tahmini (12 ay): İstihdam +300 İnisiyatife bağlı harcama +200 milyon TL Hizmet kapasitesi +3.000.
Düşük/seçici vurgu ve yüksek/seçici vurgu	Düzenleme 1 – Daha Hızlı Proje Teslimi	Ne değişiyor: Belirli yatırım projelerinde, açık ihale yerine pazarlık usulüyle daha hızlı satın alma yapılabilecek. Uzman tahmini (12 ay): İstihdam +300 İnisiyatife bağlı harcama +200 milyon TL Hizmet kapasitesi +3.000.
Yalnızca yüksek/kontrol	Düzenleme 2	Ne değişiyor: Büyük sözleşmeleri inceleyen bağımsız bir ihale denetim birimi kurulacak. Denetimden geçemeyen sözleşmeler açık ihaleyle yeniden ihaleye çıkacak. Uzman tahmini (12 ay): İstihdam +200 İnisiyatife bağlı harcama -250 milyon TL.
Yalnızca yüksek/seçici vurgu	Düzenleme 2 – Daha Güçlü Denetim	Ne değişiyor: Büyük sözleşmeleri inceleyen bağımsız bir ihale denetim birimi kurulacak. Denetimden geçemeyen sözleşmeler açık ihaleyle yeniden ihaleye çıkacak. Uzman tahmini (12 ay): İstihdam +200 İnisiyatife bağlı harcama -250 milyon TL.
Düşük/kontrol ve yüksek/kontrol	Düşük karmaşıklıkta Düzenleme 2; yüksek karmaşıklıkta Düzenleme 3	Ne değişiyor: Seçilen projelere inisiyatife bağlı hibeler veren bir şehir merkezi canlandırma fonu kurulacak. Fon, şehir genelindeki okul sonrası program bütçesinden finanse edilecek. Uzman tahmini (12 ay): İstihdam -700 İnisiyatife bağlı harcama +350 milyon TL Hizmet kapasitesi -6.000.
Düşük/seçici vurgu ve yüksek/seçici vurgu	Düşük karmaşıklıkta Düzenleme 2 – Şehir Merkezine Canlılık Fonu; yüksek karmaşıklıkta Düzenleme 3 – Şehir Merkezine Canlılık Fonu	Ne değişiyor: Seçilen projelere inisiyatife bağlı hibeler veren bir fon kurulacak. Fon, okul sonrası program bütçesinden finanse edilecek. Uzman tahmini (12 ay): İstihdam -700 İnisiyatife bağlı harcama +350 milyon TL Hizmet kapasitesi -6.000.
Yalnızca yüksek/kontrol	Düzenleme 4	Ne değişiyor: Kamu sözleşmeleri için tek bir portal kurulacak (ihale belgeleri, değişiklikler, taşeron bildirimleri). Ayrıca bazı sözleşmelerde pazarlık usulünün kapsamı genişletilecek. Uzman tahmini (12 ay): İstihdam -200 İnisiyatife bağlı harcama +250 milyon TL.
Yalnızca yüksek/seçici vurgu	Düzenleme 4 – Yeni Şeffaflık Portalı	Ne değişiyor: Kamu sözleşmeleri için tek bir portal kurulacak (ihale belgeleri, değişiklikler, taşeron bildirimleri). Ayrıca bazı sözleşmelerde pazarlık usulünün kapsamı genişletilecek. Uzman tahmini (12 ay): İstihdam -200 İnisiyatife bağlı harcama +250 milyon TL.

C.5.4 Study 4 outcome questions

Policy consequence perceptions

Overall, based on the article above, how do you think the reform package would

affect each of the following in your city? Even if a topic was not mentioned, please answer based on your best guess.

1 = Worsen a lot. 7 = Improve a lot.

Employment (jobs). Discretionary allocation (TL). Service capacity (residents served). Property tax rates. Police and public safety spending. Garbage collection and waste management.

Genel olarak, yukarıdaki metne göre, reform paketinin şehrinizde aşağıdaki alanları nasıl etkileyeceğini düşünüyorsunuz? Bir konu metinde geçmemiş olsa bile, lütfen en iyi tahmininize göre yanıtlayın.

1 = Çok kötüleştirecek. 7 = Çok iyileştirecek.

İstihdam (iş sayısı). İnisiyatife bağlı harcama (TL). Hizmet kapasitesi (hizmet alabilecek kişi sayısı). Emlak vergisi oranları. Polis ve kamu güvenliği harcamaları. Çöp toplama ve atık yönetimi.

Package versus separate vote

Should the council vote on this as one package or vote on each measure separately?

Vote as one package. Vote separately, measure by measure. No preference.

Belediye meclisi bu teklifi paket olarak mı oylamalı yoksa her düzenlemeyi tek tek mi oylamalı?

Tek bir paket olarak oylamalı. Her bir düzenleme ayrı ayrı oylanmalı. Fark etmez, her ikisi de olur.

Open-ended response

It is important that your voice is heard. Please write 1 to 2 sentences to the city council sharing your view. Minimum 15 characters.

Sesinizin duyulması önemli. Lütfen belediye meclisine görüşünüzü paylaşan 1 ya da 2 cümle yazın. En az 15 karakter.

Recognition

In the article, what does “discretionary allocation” mean?

Spending awarded via discretionary grants or negotiated procurement procedures.

Spending that must be approved item by item by the city council.

The city’s total tax revenue.

Spending used only for after school programs.

Metinde “inisiyatifte bağılı harcama” neyi ifade ediyor?

Belediye yönetiminin sabit kurallara bağılı olmadan, gerektiğinde istisna yaparak hangi projeye ne kadar kaynak ayrılacağına karar verebildiği harcamalar.

Belediye meclisinin kalem kalem onaylamak zorunda olduğu harcamalar.

Belediyenin toplam vergi geliri.

Sadece okul sonrası programlara ayrılan harcamalar.

C.6 Post-Study Elicitation

The main section of the survey is complete. Now please answer the next questions so we can understand how you make decisions. Your raffle entry comes right after this.

Have you experienced any difficulty in any specific part of the survey? Minimum 15 characters.

Son aşamadayız. Görüşlerinizi almak istiyoruz. Çekiliş hakkınız bir sonraki adımda.

Anketin herhangi bir bölümünde özel bir zorluk yaşadınız mı? En az 15 karakter.

D Outcome Coding, Sample Construction, and Full Results

D.1 Outcome Coding

Table 29 defines the main outcomes used in the article.

Table 29: Outcome definitions

Outcome	Coding	Study	Interpretation
Suboptimal choice	Indicator equal to 1 if the respondent chooses the objectively lower expected-value option.	Studies 1–2	Choice error
Harmful bundle acceptance	Indicator equal to 1 if the respondent accepts the harmful lottery bundle.	Study 2	Harmful support
Candidate support	Indicator equal to 1 if the respondent chooses the candidate carrying the harmful institutional commitment.	Study 3	Political support
Candidate recognition	Indicator equal to 1 if the respondent correctly identifies the candidate associated with the oversight-weakening provision.	Study 3	Recognition
Municipal package acceptance	Indicator equal to 1 if the respondent accepts or supports the harmful municipal reform package.	Study 4	Reform support
Consequence recognition	Indicator equal to 1 if the respondent identifies that at least two relevant package domains worsen: employment, discretionary allocation, and service capacity.	Study 4	Consequence recognition
Perceived difficulty	Rating of how difficult the task was, coded so higher values indicate greater perceived difficulty.	Studies 1–4	Subjective burden
Confidence	Rating of confidence in the respondent’s choice, coded so higher values indicate greater confidence.	Studies 1–4	Subjective certainty
Time spent	Time spent on the task page.	Studies 1–4	Behavioral process measure
Clicks	Number of clicks recorded on the task page.	Studies 1–4	Behavioral process measure

D.2 Recognition Measures and Answer Keys

Table 30 reports the recognition measures used in the political studies. Study 4 includes both the main consequence-recognition outcome used in the analysis and a separate comprehension item about discretionary allocation.

Table 30: Recognition measures and answer keys

Study	Measure	Coding or correct answer
Study 3	Which candidate proposed approving permits through the mayor’s direct sign-off, without a committee?	The reform candidate carrying the oversight-weakening commitment.
Study 4: Main consequence recognition	Respondents assess the package’s consequences for employment, discretionary allocation, and service capacity.	Coded as one when the respondent identifies that at least two of the three relevant domains worsen.
Study 4: Discretionary-allocation comprehension	What does discretionary allocation mean in the article?	Spending where the city can decide more freely, including discretionary grants or negotiated procurement procedures.

D.3 Sample Flow and Inclusion Sensitivity

The target population is adult Turkish citizens who can complete the survey in Turkish. Respondents were recruited online and completed the survey in Qualtrics. The main analytic sample applies the following exclusion rules: respondents must provide informed consent, complete the survey, spend at least 450 seconds and less than 6000 seconds on the survey, and pass the basic CRT/engagement threshold used for inclusion in the analytic sample.

Table 31 reports the cumulative sample-construction sequence from the raw imported survey records to the final analytic respondent sample.

Table 31: Sequential sample construction

Step	Removed	Remaining	Share of raw records
Raw imported survey records	NA	9904	100.0
Provided permission for data use	7754	2150	21.7
Completed survey	0	2150	21.7
Duration greater than 450 seconds	57	2093	21.1
Duration less than 6000 seconds	8	2085	21.1
Passed CRT/engagement rule	214	1871	18.9
Final analytic respondent sample	0	1871	18.9

Table 32 reports standalone exclusion-reason counts. These categories are not mutually exclusive: the same respondent can fail more than one rule. The table is therefore an audit of each exclusion criterion, not a sequential sample-flow table.

Table 32: Standalone exclusion-reason audit

Exclusion reason	N	Share of raw records
Did not provide permission for data use	7754	78.3
Did not complete survey	7164	72.3
Duration missing	0	0.0
Duration at or below 450 seconds	5674	57.3
Duration at or above 6000 seconds	25	0.3
Failed CRT/engagement rule	4957	50.1
Potential duplicate flag present, not excluded in main rule	773	7.8

Table 33 reports study-level reach and analytic inclusion. Because the raw imported file includes incomplete survey starts and respondents who attrited before later modules, the module-reach and observed-outcome columns are the relevant diagnostics for study-specific missingness.

Table 33: Study reach and analytic inclusion

Study	Raw survey-start N	Reached task N	Observed main outcome N	Included analytic N	Observed share	Included share
Study 1	9904	4175	4175	1642	42.2	16.6
Study 2	9904	4346	4346	1871	43.9	18.9
Study 3	9904	2861	2654	1871	26.8	18.9
Study 4	9904	2881	2758	1871	27.8	18.9

D.4 Treatment-Cell Attrition and Outcome Missingness

Table 34 reports treatment-cell attrition and analytic inclusion for Studies 2–4. Study 1 is omitted from this table because it was added after the pilot wave and has a separate three-arm assignment structure; its treatment-cell counts are reported in Appendix Table 8.

Table 34: Treatment-cell attrition and analytic inclusion

Study	Outcome	Treatment cell	Raw treatment-cell N	Reached task N	Observed outcome N	Included analytic N	Missing before outcome pct	Excluded after observed pct
Study 2	Study 2 high-value bundle, first lottery module	Low complexity / neutral	2420	975	975	459	59.7	52.9
Study 2	Study 2 high-value bundle, first lottery module	Low complexity / selective emphasis	2491	1001	1001	448	59.8	55.2
Study 2	Study 2 high-value bundle, first lottery module	High complexity / neutral	2478	1038	1038	527	58.1	49.2
Study 2	Study 2 high-value bundle, first lottery module	High complexity / selective emphasis	2515	997	997	437	60.4	56.2
Study 2	Study 2 high-value bundle, second lottery module	Low complexity / neutral	2420	992	992	459	59.0	53.7
Study 2	Study 2 high-value bundle, second lottery module	Low complexity / selective emphasis	2491	992	992	448	60.2	54.8
Study 2	Study 2 high-value bundle, second lottery module	High complexity / neutral	2478	1034	1034	527	58.3	49.0
Study 2	Study 2 high-value bundle, second lottery module	High complexity / selective emphasis	2515	987	987	437	60.8	55.7
Study 2	Study 2 low-value bundle, first lottery module	Low complexity / neutral	2420	969	969	459	60.0	52.6
Study 2	Study 2 low-value bundle, first lottery module	Low complexity / selective emphasis	2491	996	996	448	60.0	55.0
Study 2	Study 2 low-value bundle, first lottery module	High complexity / neutral	2478	1046	1046	527	57.8	49.6
Study 2	Study 2 low-value bundle, first lottery module	High complexity / selective emphasis	2515	1004	1004	437	60.1	56.5
Study 2	Study 2 low-value bundle, second lottery module	Low complexity / neutral	2420	982	982	459	59.4	53.3
Study 2	Study 2 low-value bundle, second lottery module	Low complexity / selective emphasis	2491	991	991	448	60.2	54.8
Study 2	Study 2 low-value bundle, second lottery module	High complexity / neutral	2478	1041	1041	527	58.0	49.4
Study 2	Study 2 low-value bundle, second lottery module	High complexity / selective emphasis	2515	991	991	437	60.6	55.9
Study 3	Study 3 candidate rating	Low complexity / neutral	2478	763	718	527	71.0	26.6
Study 3	Study 3 candidate rating	Low complexity / selective emphasis	2515	693	632	437	74.9	30.9
Study 3	Study 3 candidate rating	High complexity / neutral	2420	693	641	459	73.5	28.4
Study 3	Study 3 candidate rating	High complexity / selective emphasis	2491	712	663	448	73.4	32.4
Study 3	Study 3 candidate support	Low complexity / neutral	2478	763	718	527	71.0	26.6
Study 3	Study 3 candidate support	Low complexity / selective emphasis	2515	693	632	437	74.9	30.9
Study 3	Study 3 candidate support	High complexity / neutral	2420	693	641	459	73.5	28.4
Study 3	Study 3 candidate support	High complexity / selective emphasis	2491	712	663	448	73.4	32.4
Study 4	Study 4 package acceptance	Low complexity / neutral	2520	704	668	447	73.5	33.1
Study 4	Study 4 package acceptance	Low complexity / selective emphasis	2473	765	740	517	70.1	30.1
Study 4	Study 4 package acceptance	High complexity / neutral	2487	705	671	442	73.0	34.1
Study 4	Study 4 package acceptance	High complexity / selective emphasis	2424	707	679	465	72.0	31.5
Study 4	Study 4 package-vote preference	Low complexity / neutral	2520	704	636	447	74.8	29.7
Study 4	Study 4 package-vote preference	Low complexity / selective emphasis	2473	765	712	517	71.2	27.4
Study 4	Study 4 package-vote preference	High complexity / neutral	2487	705	638	442	74.3	30.7
Study 4	Study 4 package-vote preference	High complexity / selective emphasis	2424	707	654	465	73.0	28.9

Table 35 reports missingness for choice, recognition, and process outcomes among analytically eligible respondents. This table documents denominator changes across the main outcomes and secondary process measures.

Table 35: Outcome missingness among analytically eligible respondents

Study	Outcome type	Outcome	Eligible N	Observed N	Missing N	Missing pct
Study 1	Choice/support	Study 1 lottery choice	1871	1642	229	12.2
Study 1	Process	Study 1 lottery choice: Clicks	1871	1642	229	12.2
Study 1	Process	Study 1 lottery choice: Confidence	1871	1642	229	12.2
Study 1	Process	Study 1 lottery choice: Perceived difficulty	1871	1642	229	12.2
Study 1	Process	Study 1 lottery choice: Time	1871	1642	229	12.2
Study 2	Choice/support	Study 2 high-value bundle, first lottery module	1871	1871	0	0.0
Study 2	Choice/support	Study 2 high-value bundle, second lottery module	1871	1871	0	0.0
Study 2	Choice/support	Study 2 low-value bundle, first lottery module	1871	1871	0	0.0
Study 2	Choice/support	Study 2 low-value bundle, second lottery module	1871	1871	0	0.0
Study 2	Process	Study 2 high-value bundle, first lottery module: Clicks	1871	1871	0	0.0
Study 2	Process	Study 2 high-value bundle, first lottery module: Confidence	1871	1871	0	0.0
Study 2	Process	Study 2 high-value bundle, first lottery module: Perceived difficulty	1871	1871	0	0.0
Study 2	Process	Study 2 high-value bundle, first lottery module: Time	1871	1871	0	0.0
Study 2	Process	Study 2 high-value bundle, second lottery module: Clicks	1871	1871	0	0.0
Study 2	Process	Study 2 high-value bundle, second lottery module: Confidence	1871	1871	0	0.0
Study 2	Process	Study 2 high-value bundle, second lottery module: Perceived difficulty	1871	1871	0	0.0
Study 2	Process	Study 2 high-value bundle, second lottery module: Time	1871	1871	0	0.0
Study 2	Process	Study 2 low-value bundle, first lottery module: Clicks	1871	1871	0	0.0
Study 2	Process	Study 2 low-value bundle, first lottery module: Confidence	1871	1871	0	0.0
Study 2	Process	Study 2 low-value bundle, first lottery module: Perceived difficulty	1871	1871	0	0.0
Study 2	Process	Study 2 low-value bundle, first lottery module: Time	1871	1871	0	0.0
Study 2	Process	Study 2 low-value bundle, second lottery module: Clicks	1871	1871	0	0.0
Study 2	Process	Study 2 low-value bundle, second lottery module: Confidence	1871	1871	0	0.0
Study 2	Process	Study 2 low-value bundle, second lottery module: Perceived difficulty	1871	1871	0	0.0
Study 2	Process	Study 2 low-value bundle, second lottery module: Time	1871	1871	0	0.0
Study 3	Choice/support	Study 3 candidate rating	1871	1871	0	0.0
Study 3	Choice/support	Study 3 candidate support	1871	1871	0	0.0
Study 3	Process	Study 3 candidate rating: Clicks	1871	1871	0	0.0
Study 3	Process	Study 3 candidate rating: Confidence	1871	1871	0	0.0
Study 3	Process	Study 3 candidate rating: Perceived difficulty	1871	1871	0	0.0
Study 3	Process	Study 3 candidate rating: Time	1871	1871	0	0.0
Study 3	Process	Study 3 candidate support: Clicks	1871	1871	0	0.0
Study 3	Process	Study 3 candidate support: Confidence	1871	1871	0	0.0
Study 3	Process	Study 3 candidate support: Perceived difficulty	1871	1871	0	0.0
Study 3	Process	Study 3 candidate support: Time	1871	1871	0	0.0
Study 3	Recognition	Study 3 candidate-platform recognition	1871	1871	0	0.0
Study 4	Choice/support	Study 4 package acceptance	1871	1871	0	0.0
Study 4	Choice/support	Study 4 package-vote preference	1871	1871	0	0.0
Study 4	Process	Study 4 package acceptance: Clicks	1871	1871	0	0.0
Study 4	Process	Study 4 package acceptance: Confidence	1871	1871	0	0.0
Study 4	Process	Study 4 package acceptance: Perceived difficulty	1871	1871	0	0.0
Study 4	Process	Study 4 package acceptance: Time	1871	1871	0	0.0
Study 4	Process	Study 4 package-vote preference: Clicks	1871	1871	0	0.0
Study 4	Process	Study 4 package-vote preference: Confidence	1871	1871	0	0.0
Study 4	Process	Study 4 package-vote preference: Perceived difficulty	1871	1871	0	0.0
Study 4	Process	Study 4 package-vote preference: Time	1871	1871	0	0.0
Study 4	Recognition	Study 4 consequence recognition	1871	1871	0	0.0

D.5 Exclusion-Sensitivity Sample Sizes

Table 36 reports analytic sample sizes under alternative exclusion rules. Because the 6,000-second upper-duration cutoff was added after preregistration, I first report a specification that removes only that upper cutoff while retaining the preregistered 450-second lower threshold and pretreatment CRT screen. I then report broader specifications that remove the duration restriction entirely, relax the CRT/engagement screen, or relax both restrictions.

Table 36: Analytic sample size under alternative exclusion rules

Study	Outcome	Main analytic rule	No upper-duration cutoff	No duration exclusion	No CRT/engagement exclusion	No duration or CRT/engagement exclusion	Completed-only sample
Study 1	Study 1 lottery choice	1642	1648	1677	1836	1890	2125
Study 2	Study 2 high-value bundle, first lottery module	1871	1878	1912	2085	2150	2403
Study 2	Study 2 high-value bundle, second lottery module	1871	1878	1912	2085	2150	2403
Study 2	Study 2 low-value bundle, first lottery module	1871	1878	1912	2085	2150	2403
Study 2	Study 2 low-value bundle, second lottery module	1871	1878	1912	2085	2150	2403
Study 3	Study 3 candidate rating	1871	1878	1912	2085	2150	2403
Study 3	Study 3 candidate support	1871	1878	1912	2085	2150	2403
Study 4	Study 4 package acceptance	1871	1878	1912	2085	2150	2403
Study 4	Study 4 package-vote preference	1871	1878	1912	2085	2150	2403

Tables 37 and 38 examine whether the main analytic inclusion rules affect the paper’s estimates. Table 37 first re-estimates the focal treatment contrasts after removing only the 6,000-second upper-duration cutoff while retaining the preregistered lower-duration and pretreatment CRT rules. It then reports broader samples that remove the duration restriction entirely, relax the pretreatment CRT rule, and relax both restrictions together. The focal estimates remain similar in sign and magnitude across these samples. The main exception is consequence recognition in Study 4, which becomes smaller under broader inclusion rules; this is consistent with the cautious interpretation of that outcome in the main text. Table 38 then reports whether treatment assignment predicts reaching the task, answering the focal outcome, entering the restricted analytic sample, passing the duration window among respondents who answered, or passing the pretreatment CRT rule among respondents who answered. Because the duration window is measured after treatment exposure, the duration diagnostic is the central post-treatment selection check. Treatment-cell differences in passing that rule are small among respondents who answered the focal outcome. Broader reach, answer, and restricted-inclusion rates show some treatment-cell differences, especially in later modules, so the sensitivity estimates in Table 37 are reported to show that the substantive conclusions do not depend on the main restricted sample.

Table 37: Sensitivity of focal treatment effects to inclusion rules

Study	Outcome	Contrast	Sample	Estimate	SE	95% CI	Baseline	N respondents
Study 1	Choice of worse lottery option	Bundled vs direct presentation	Main restricted sample	17.02	2.44	[12.23, 21.81]	26.43	1642
Study 1	Choice of worse lottery option	Bundled vs direct presentation	No upper-duration cutoff	17.20	2.44	[12.43, 21.97]	26.34	1648
Study 1	Choice of worse lottery option	Bundled vs direct presentation	No duration restriction	16.64	2.42	[11.89, 21.39]	27.16	1677
Study 1	Choice of worse lottery option	Bundled vs direct presentation	No CRT restriction	16.05	2.34	[11.47, 20.63]	27.51	1836
Study 1	Choice of worse lottery option	Bundled vs direct presentation	No duration or CRT restriction	15.61	2.31	[11.07, 20.14]	28.39	1890
Study 2	Choice of worse lottery option	Complexity x selective emphasis	Main restricted sample	3.69	3.11	[-2.41, 9.79]	41.50	1871
Study 2	Choice of worse lottery option	Joint: high complexity + selective emphasis	Main restricted sample	2.43	2.25	[-1.98, 6.84]	41.50	1871
Study 2	Choice of worse lottery option	Complexity x selective emphasis	No upper-duration cutoff	3.59	3.11	[-2.50, 9.69]	41.32	1878
Study 2	Choice of worse lottery option	Joint: high complexity + selective emphasis	No upper-duration cutoff	2.63	2.25	[-1.77, 7.04]	41.32	1878
Study 2	Choice of worse lottery option	Complexity x selective emphasis	No duration restriction	3.63	3.08	[-2.40, 9.67]	41.42	1912
Study 2	Choice of worse lottery option	Joint: high complexity + selective emphasis	No duration restriction	2.82	2.23	[-1.56, 7.20]	41.42	1912
Study 2	Choice of worse lottery option	Complexity x selective emphasis	No CRT restriction	4.10	2.93	[-1.65, 9.85]	42.59	2085
Study 2	Choice of worse lottery option	Joint: high complexity + selective emphasis	No CRT restriction	2.15	2.12	[-2.00, 6.30]	42.59	2085
Study 2	Choice of worse lottery option	Complexity x selective emphasis	No duration or CRT restriction	3.60	2.90	[-2.08, 9.28]	42.62	2150
Study 2	Choice of worse lottery option	Joint: high complexity + selective emphasis	No duration or CRT restriction	2.44	2.10	[-1.67, 6.55]	42.62	2150
Study 2	Harmful lottery-bundle acceptance	Complexity x selective emphasis	Main restricted sample	4.85	4.59	[-4.14, 13.83]	42.05	1871
Study 2	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	Main restricted sample	7.00	3.34	[0.45, 13.55]	42.05	1871
Study 2	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No upper-duration cutoff	4.76	4.58	[-4.21, 13.73]	41.87	1878
Study 2	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No upper-duration cutoff	7.14	3.33	[0.61, 13.68]	41.87	1878
Study 2	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No duration restriction	4.62	4.54	[-4.28, 13.52]	42.06	1912
Study 2	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No duration restriction	7.13	3.32	[0.64, 13.63]	42.06	1912
Study 2	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No CRT restriction	5.42	4.35	[-3.11, 13.94]	43.28	2085
Study 2	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No CRT restriction	7.06	3.15	[0.89, 13.24]	43.28	2085
Study 2	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No duration or CRT restriction	4.96	4.29	[-3.44, 13.37]	43.10	2150
Study 2	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No duration or CRT restriction	7.35	3.11	[1.26, 13.45]	43.10	2150
Study 3	Candidate support	Joint: longer platform + attractive headline	Main restricted sample	16.96	2.97	[11.14, 22.78]	24.86	1871
Study 3	Candidate support	Joint: longer platform + attractive headline	No upper-duration cutoff	16.88	2.96	[11.07, 22.68]	24.91	1878
Study 3	Candidate support	Joint: longer platform + attractive headline	No duration restriction	15.69	2.94	[9.92, 21.45]	26.06	1912
Study 3	Candidate support	Joint: longer platform + attractive headline	No CRT restriction	15.55	2.83	[10.00, 21.09]	26.66	2085
Study 3	Candidate support	Joint: longer platform + attractive headline	No duration or CRT restriction	13.52	2.80	[8.04, 19.00]	28.36	2150
Study 3	Recognition of harmful commitment	Joint: longer platform + attractive headline	Main restricted sample	-7.87	3.18	[-14.11, -1.63]	63.38	1871
Study 3	Recognition of harmful commitment	Joint: longer platform + attractive headline	No upper-duration cutoff	-7.95	3.18	[-14.18, -1.73]	63.58	1878
Study 3	Recognition of harmful commitment	Joint: longer platform + attractive headline	No duration restriction	-7.28	3.15	[-13.45, -1.11]	62.66	1912
Study 3	Recognition of harmful commitment	Joint: longer platform + attractive headline	No CRT restriction	-6.89	3.05	[-12.87, -0.91]	60.63	2085
Study 3	Recognition of harmful commitment	Joint: longer platform + attractive headline	No duration or CRT restriction	-6.23	3.00	[-12.11, -0.35]	59.73	2150
Study 4	Municipal package acceptance	Joint: four-policy package + favorable headlines	Main restricted sample	8.05	3.21	[1.77, 14.34]	59.06	1871
Study 4	Municipal package acceptance	Joint: four-policy package + favorable headlines	No upper-duration cutoff	7.91	3.20	[1.63, 14.19]	59.15	1878
Study 4	Municipal package acceptance	Joint: four-policy package + favorable headlines	No duration restriction	7.23	3.17	[1.01, 13.45]	59.69	1912
Study 4	Municipal package acceptance	Joint: four-policy package + favorable headlines	No CRT restriction	7.54	3.02	[1.61, 13.46]	59.96	2085
Study 4	Municipal package acceptance	Joint: four-policy package + favorable headlines	No duration or CRT restriction	6.96	2.98	[1.12, 12.79]	60.61	2150
Study 4	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	Main restricted sample	-4.15	2.37	[-8.80, 0.51]	16.33	1871
Study 4	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No upper-duration cutoff	-4.18	2.37	[-8.82, 0.46]	16.29	1878
Study 4	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No duration restriction	-3.64	2.35	[-8.25, 0.96]	16.08	1912
Study 4	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No CRT restriction	-3.05	2.22	[-7.41, 1.31]	15.62	2085
Study 4	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No duration or CRT restriction	-2.45	2.19	[-6.74, 1.85]	15.30	2150

Table 38: Treatment assignment, inclusion, and attrition diagnostics

Study	Outcome	Selection outcome	Min cell rate	Max cell rate	Range pp	Treatment joint p	Treatment x covariates p	N respondents
Study 1	Choice of worse lottery option	Reached task	45.5	46.8	1.4	0.465	0.977	9003
Study 1	Choice of worse lottery option	Answered focal outcome	45.5	46.8	1.4	0.465	0.977	9003
Study 1	Choice of worse lottery option	Entered restricted analytic sample	18.1	18.4	0.3	0.937	0.689	9003
Study 1	Choice of worse lottery option	Passed duration window among answered	97.3	98.5	1.2	0.358	0.678	1677
Study 1	Choice of worse lottery option	Passed CRT rule among answered	89.1	89.9	0.8	0.887	0.979	1836
Study 2	Choice of worse lottery option	Reached task	43.0	45.3	2.3	0.366	0.141	9904
Study 2	Choice of worse lottery option	Answered focal outcome	43.0	45.3	2.3	0.366	0.141	9904
Study 2	Choice of worse lottery option	Entered restricted analytic sample	17.4	21.3	3.9	0.003	0.961	9904
Study 2	Choice of worse lottery option	Passed duration window among answered	97.4	98.5	1.1	0.542	0.453	1912
Study 2	Choice of worse lottery option	Passed CRT rule among answered	86.9	91.8	4.9	0.050	0.090	2085
Study 2	Harmful lottery-bundle acceptance	Reached task	43.0	45.3	2.3	0.366	0.141	9904
Study 2	Harmful lottery-bundle acceptance	Answered focal outcome	43.0	45.3	2.3	0.366	0.141	9904
Study 2	Harmful lottery-bundle acceptance	Entered restricted analytic sample	17.4	21.3	3.9	0.003	0.961	9904
Study 2	Harmful lottery-bundle acceptance	Passed duration window among answered	97.4	98.5	1.1	0.542	0.453	1912
Study 2	Harmful lottery-bundle acceptance	Passed CRT rule among answered	86.9	91.8	4.9	0.050	0.090	2085
Study 3	Candidate support	Reached task	27.6	30.8	3.2	0.081	0.846	9904
Study 3	Candidate support	Answered focal outcome	25.1	29.0	3.8	0.021	0.822	9904
Study 3	Candidate support	Entered restricted analytic sample	17.4	21.3	3.9	0.003	0.961	9904
Study 3	Candidate support	Passed duration window among answered	97.4	98.5	1.1	0.542	0.453	1912
Study 3	Candidate support	Passed CRT rule among answered	86.9	91.8	4.9	0.050	0.090	2085
Study 3	Recognition of harmful commitment	Reached task	27.6	30.8	3.2	0.081	0.846	9904
Study 3	Recognition of harmful commitment	Answered focal outcome	100.0	100.0	0.0	0.392	0.639	9904
Study 3	Recognition of harmful commitment	Entered restricted analytic sample	17.4	21.3	3.9	0.003	0.961	9904
Study 3	Recognition of harmful commitment	Passed duration window among answered	97.4	98.5	1.1	0.542	0.453	1912
Study 3	Recognition of harmful commitment	Passed CRT rule among answered	86.9	91.8	4.9	0.050	0.090	2085
Study 4	Municipal package acceptance	Reached task	27.9	30.9	3.0	0.095	0.662	9904
Study 4	Municipal package acceptance	Answered focal outcome	26.5	29.9	3.4	0.036	0.670	9904
Study 4	Municipal package acceptance	Entered restricted analytic sample	17.7	20.9	3.2	0.012	0.991	9904
Study 4	Municipal package acceptance	Passed duration window among answered	97.2	98.5	1.3	0.462	0.840	1912
Study 4	Municipal package acceptance	Passed CRT rule among answered	87.3	91.5	4.2	0.136	0.138	2085
Study 4	Recognition of harmful consequences	Reached task	27.9	30.9	3.0	0.095	0.662	9904
Study 4	Recognition of harmful consequences	Answered focal outcome	25.2	28.8	3.6	0.021	0.676	9904
Study 4	Recognition of harmful consequences	Entered restricted analytic sample	17.7	20.9	3.2	0.012	0.991	9904
Study 4	Recognition of harmful consequences	Passed duration window among answered	97.2	98.5	1.3	0.462	0.840	1912
Study 4	Recognition of harmful consequences	Passed CRT rule among answered	87.3	91.5	4.2	0.136	0.138	2085

D.6 Main Estimation Strategy

For binary outcomes, the main analyses use linear probability models. Treatment effects are reported in percentage points. For continuous outcomes, the main analyses use ordinary least squares. Standard errors are clustered at the respondent level when the outcome pools repeated decisions from the same respondent. For single-decision modules, robust standard errors are reported unless otherwise noted.

The main specifications estimate treatment effects relative to the relevant baseline condition. For Study 2, the baseline is the low-complexity, neutral-emphasis condition. The main specification includes indicators for high complexity, selective emphasis, and their interaction:

$$Y_i = \alpha + \beta_1 HighComplexity_i + \beta_2 SelectiveEmphasis_i + \beta_3 HighComplexity_i \times SelectiveEmphasis_i + \varepsilon_i.$$

The coefficient β_3 estimates the interaction between complexity and selective emphasis.

Substantively, the paper interprets this interaction as whether complexity becomes more consequential when attractive components are selectively emphasized. The joint effect of high complexity and selective emphasis relative to the low-complexity, neutral-emphasis baseline is:

$$\beta_1 + \beta_2 + \beta_3.$$

For Study 1, the main comparisons estimate differences in suboptimal choice between the direct-comparison baseline and the two bundled-presentation conditions. For Studies 3 and 4, the main specifications follow the same 2×2 treatment structure used in Study 2, with candidate support, candidate recognition, municipal package acceptance, and municipal recognition as the main outcomes.

D.7 Capacity-Subgroup Inclusion Sensitivity

The main heterogeneity results use the main restricted sample. To assess whether those patterns depend on the analytic inclusion rules, I re-estimate the focal treatment contrasts separately for lower- and higher-capacity respondents under the same five samples used in the full-sample sensitivity analysis: the main restricted sample, a sample that removes only the 6,000-second upper-duration cutoff, a sample without any duration restriction, a sample without the pretreatment CRT restriction, and a sample that removes both the duration and pretreatment CRT restrictions while retaining completion and consent.

The capacity criterion itself is held fixed across these analyses. Respondents are classified as higher-capacity if they answered both basic items correctly and at least one of the three harder reasoning items correctly. Under specifications that retain the preregistered CRT inclusion rule, lower-capacity respondents are therefore those who passed the basic-item screen but did not meet the higher-capacity threshold. When the CRT inclusion rule is relaxed, the lower-capacity category additionally includes respondents who did not pass the basic-item screen. The latter specifications therefore test robustness to broadening the lower-performing population as well as to changing the sample inclusion rule.

Tables 39–42 report the subgroup sensitivity estimates for the focal outcomes in each study. Each table re-estimates the same treatment contrasts reported in the corresponding sensitivity analysis above, separately by cognitive capacity and under each inclusion rule.

Table 39: Study 1: sensitivity of focal treatment effects to inclusion rules, by cognitive capacity

Capacity	Outcome	Contrast	Sample	Estimate	SE	95% CI	Baseline	N respondents
Lower capacity	Choice of worse lottery option	Bundled vs direct presentation	Main restricted sample	13.02	3.27	[6.62, 19.42]	33.43	1027
Higher capacity	Choice of worse lottery option	Bundled vs direct presentation	Main restricted sample	23.14	3.73	[15.82, 30.45]	15.57	615
Lower capacity	Choice of worse lottery option	Bundled vs direct presentation	No upper-duration cutoff	13.22	3.26	[6.83, 19.60]	33.33	1030
Higher capacity	Choice of worse lottery option	Bundled vs direct presentation	No upper-duration cutoff	23.13	3.72	[15.85, 30.42]	15.49	618
Lower capacity	Choice of worse lottery option	Bundled vs direct presentation	No duration restriction	12.36	3.23	[6.03, 18.69]	34.40	1057
Higher capacity	Choice of worse lottery option	Bundled vs direct presentation	No duration restriction	23.15	3.71	[15.88, 30.41]	15.49	620
Lower capacity	Choice of worse lottery option	Bundled vs direct presentation	No CRT restriction	12.24	3.00	[6.35, 18.12]	33.92	1221
Higher capacity	Choice of worse lottery option	Bundled vs direct presentation	No CRT restriction	23.14	3.73	[15.82, 30.45]	15.57	615
Lower capacity	Choice of worse lottery option	Bundled vs direct presentation	No duration or CRT restriction	11.68	2.95	[5.89, 17.47]	35.02	1270
Higher capacity	Choice of worse lottery option	Bundled vs direct presentation	No duration or CRT restriction	23.15	3.71	[15.88, 30.41]	15.49	620

Table 40: Study 2: sensitivity of focal treatment effects to inclusion rules, by cognitive capacity

Capacity	Outcome	Contrast	Sample	Estimate	SE	95% CI	Baseline	N respondents
Lower capacity	Choice of worse lottery option	Complexity x selective emphasis	Main restricted sample	9.75	3.88	[2.14, 17.35]	46.42	1170
Lower capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	Main restricted sample	4.06	2.81	[-1.45, 9.58]	46.42	1170
Higher capacity	Choice of worse lottery option	Complexity x selective emphasis	Main restricted sample	-8.73	5.09	[-18.70, 1.25]	32.83	701
Higher capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	Main restricted sample	0.30	3.54	[-6.63, 7.24]	32.83	701
Lower capacity	Choice of worse lottery option	Complexity x selective emphasis	No upper-duration cutoff	9.68	3.88	[2.08, 17.28]	46.42	1174
Lower capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	No upper-duration cutoff	4.10	2.81	[-1.40, 9.61]	46.42	1174
Higher capacity	Choice of worse lottery option	Complexity x selective emphasis	No upper-duration cutoff	-8.77	5.07	[-18.71, 1.17]	32.44	704
Higher capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	No upper-duration cutoff	0.68	3.52	[-6.21, 7.57]	32.44	704
Lower capacity	Choice of worse lottery option	Complexity x selective emphasis	No duration restriction	9.68	3.82	[2.18, 17.17]	46.48	1206
Lower capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	No duration restriction	4.19	2.79	[-1.27, 9.65]	46.48	1206
Higher capacity	Choice of worse lottery option	Complexity x selective emphasis	No duration restriction	-8.69	5.07	[-18.62, 1.24]	32.44	706
Higher capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	No duration restriction	0.96	3.51	[-5.92, 7.85]	32.44	706
Lower capacity	Choice of worse lottery option	Complexity x selective emphasis	No CRT restriction	9.33	3.54	[2.39, 16.27]	47.35	1384
Lower capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	No CRT restriction	3.13	2.55	[-1.88, 8.13]	47.35	1384
Higher capacity	Choice of worse lottery option	Complexity x selective emphasis	No CRT restriction	-8.73	5.09	[-18.70, 1.25]	32.83	701
Higher capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	No CRT restriction	0.30	3.54	[-6.63, 7.24]	32.83	701
Lower capacity	Choice of worse lottery option	Complexity x selective emphasis	No duration or CRT restriction	8.55	3.48	[1.73, 15.38]	47.46	1444
Lower capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	No duration or CRT restriction	3.26	2.52	[-1.68, 8.19]	47.46	1444
Higher capacity	Choice of worse lottery option	Complexity x selective emphasis	No duration or CRT restriction	-8.69	5.07	[-18.62, 1.24]	32.44	706
Higher capacity	Choice of worse lottery option	Joint: high complexity + selective emphasis	No duration or CRT restriction	0.96	3.51	[-5.92, 7.85]	32.44	706
Lower capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	Main restricted sample	12.63	5.84	[1.18, 24.08]	45.05	1170
Lower capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	Main restricted sample	11.59	4.21	[3.35, 19.84]	45.05	1170
Higher capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	Main restricted sample	-7.38	7.42	[-21.92, 7.16]	36.75	701
Higher capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	Main restricted sample	-0.11	5.48	[-10.85, 10.64]	36.75	701
Lower capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No upper-duration cutoff	12.62	5.83	[1.19, 24.04]	45.05	1174
Lower capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No upper-duration cutoff	11.48	4.20	[3.25, 19.70]	45.05	1174
Higher capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No upper-duration cutoff	-7.58	7.40	[-22.08, 6.93]	36.31	704
Higher capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No upper-duration cutoff	0.45	5.46	[-10.25, 11.15]	36.31	704
Lower capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No duration restriction	12.38	5.76	[1.09, 23.67]	45.30	1206
Lower capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No duration restriction	11.17	4.16	[3.01, 19.34]	45.30	1206
Higher capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No duration restriction	-7.43	7.41	[-21.95, 7.10]	36.31	706
Higher capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No duration restriction	0.88	5.46	[-9.82, 11.59]	36.31	706
Lower capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No CRT restriction	11.87	5.34	[1.39, 22.34]	46.47	1384
Lower capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No CRT restriction	10.56	3.83	[3.05, 18.06]	46.47	1384
Higher capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No CRT restriction	-7.38	7.42	[-21.92, 7.16]	36.75	701
Higher capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No CRT restriction	-0.11	5.48	[-10.85, 10.64]	36.75	701
Lower capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No duration or CRT restriction	11.21	5.24	[0.95, 21.48]	46.33	1444
Lower capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No duration or CRT restriction	10.49	3.76	[3.12, 17.87]	46.33	1444
Higher capacity	Harmful lottery-bundle acceptance	Complexity x selective emphasis	No duration or CRT restriction	-7.43	7.41	[-21.95, 7.10]	36.31	706
Higher capacity	Harmful lottery-bundle acceptance	Joint: high complexity + selective emphasis	No duration or CRT restriction	0.88	5.46	[-9.82, 11.59]	36.31	706

Table 41: Study 3: sensitivity of focal treatment effects to inclusion rules, by cognitive capacity

Capacity	Outcome	Contrast	Sample	Estimate	SE	95% CI	Baseline	N respondents
Lower capacity	Candidate support	Joint: longer platform + attractive headline	Main restricted sample	19.85	3.81	[12.39, 27.31]	29.05	1170
Higher capacity	Candidate support	Joint: longer platform + attractive headline	Main restricted sample	14.91	4.69	[5.72, 24.11]	18.00	701
Lower capacity	Candidate support	Joint: longer platform + attractive headline	No upper-duration cutoff	19.85	3.80	[12.39, 27.30]	29.09	1174
Higher capacity	Candidate support	Joint: longer platform + attractive headline	No upper-duration cutoff	14.77	4.68	[5.60, 23.93]	18.00	704
Lower capacity	Candidate support	Joint: longer platform + attractive headline	No duration restriction	18.10	3.75	[10.75, 25.46]	30.79	1206
Higher capacity	Candidate support	Joint: longer platform + attractive headline	No duration restriction	14.53	4.66	[5.39, 23.66]	18.00	706
Lower capacity	Candidate support	Joint: longer platform + attractive headline	No CRT restriction	17.03	3.52	[10.13, 23.94]	31.28	1384
Higher capacity	Candidate support	Joint: longer platform + attractive headline	No CRT restriction	14.91	4.69	[5.72, 24.11]	18.00	701
Lower capacity	Candidate support	Joint: longer platform + attractive headline	No duration or CRT restriction	14.12	3.47	[7.33, 20.92]	33.59	1444
Higher capacity	Candidate support	Joint: longer platform + attractive headline	No duration or CRT restriction	14.53	4.66	[5.39, 23.66]	18.00	706
Lower capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	Main restricted sample	-6.80	4.15	[-14.93, 1.33]	58.10	1170
Higher capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	Main restricted sample	-7.84	5.02	[-17.68, 2.01]	72.00	701
Lower capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	No upper-duration cutoff	-7.18	4.14	[-15.29, 0.93]	58.48	1174
Higher capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	No upper-duration cutoff	-7.68	5.00	[-17.48, 2.13]	72.00	704
Lower capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	No duration restriction	-6.01	4.08	[-14.00, 1.98]	57.18	1206
Higher capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	No duration restriction	-8.07	5.00	[-17.87, 1.74]	72.00	706
Lower capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	No CRT restriction	-5.21	3.84	[-12.74, 2.33]	54.55	1384
Higher capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	No CRT restriction	-7.84	5.02	[-17.68, 2.01]	72.00	701
Lower capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	No duration or CRT restriction	-4.38	3.75	[-11.72, 2.97]	53.54	1444
Higher capacity	Recognition of harmful commitment	Joint: longer platform + attractive headline	No duration or CRT restriction	-8.07	5.00	[-17.87, 1.74]	72.00	706

Table 42: Study 4: sensitivity of focal treatment effects to inclusion rules, by cognitive capacity

Capacity	Outcome	Contrast	Sample	Estimate	SE	95% CI	Baseline	N respondents
Lower capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	Main restricted sample	0.59	3.99	[-7.24, 8.42]	66.17	1170
Higher capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	Main restricted sample	19.73	5.37	[9.21, 30.25]	48.31	701
Lower capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	No upper-duration cutoff	0.51	3.99	[-7.30, 8.33]	66.30	1174
Higher capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	No upper-duration cutoff	19.34	5.37	[8.82, 29.85]	48.31	704
Lower capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	No duration restriction	-0.41	3.94	[-8.13, 7.31]	66.91	1206
Higher capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	No duration restriction	19.01	5.35	[8.51, 29.50]	48.60	706
Lower capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	No CRT restriction	0.99	3.67	[-6.20, 8.18]	66.17	1384
Higher capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	No CRT restriction	19.73	5.37	[9.21, 30.25]	48.31	701
Lower capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	No duration or CRT restriction	0.44	3.60	[-6.61, 7.49]	66.86	1444
Higher capacity	Municipal package acceptance	Joint: four-policy package + favorable headlines	No duration or CRT restriction	19.01	5.35	[8.51, 29.50]	48.60	706
Lower capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	Main restricted sample	-4.91	2.82	[-10.44, 0.63]	14.13	1170
Higher capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	Main restricted sample	-1.44	4.20	[-9.68, 6.80]	19.66	701
Lower capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No upper-duration cutoff	-4.90	2.82	[-10.42, 0.63]	14.07	1174
Higher capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No upper-duration cutoff	-1.65	4.17	[-9.83, 6.53]	19.66	704
Lower capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No duration restriction	-3.99	2.81	[-9.49, 1.52]	13.82	1206
Higher capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No duration restriction	-1.57	4.16	[-9.71, 6.58]	19.55	706
Lower capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No CRT restriction	-3.17	2.57	[-8.21, 1.88]	13.47	1384
Higher capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No CRT restriction	-1.44	4.20	[-9.68, 6.80]	19.66	701
Lower capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No duration or CRT restriction	-2.23	2.54	[-7.21, 2.74]	13.08	1444
Higher capacity	Recognition of harmful consequences	Joint: four-policy package + favorable headlines	No duration or CRT restriction	-1.57	4.16	[-9.71, 6.58]	19.55	706

The subgroup patterns are substantively stable across the alternative inclusion rules. Most importantly, the Study 2 complexity-by-selective-emphasis pattern among lower-capacity respondents changes little as the sample is broadened. The interaction remains positive for both suboptimal choice and harmful-bundle acceptance under every inclusion rule, and the joint high-complexity and selective-emphasis contrast on harmful-bundle acceptance is similarly stable. Study 1 continues to show bundling effects in both capacity groups, and Study 3 candidate-support effects remain positive in both groups. In Study 4, the acceptance effect

remains concentrated among higher-capacity respondents, while the lower-capacity estimate remains near zero. The Study 4 consequence-recognition estimates remain negative but imprecisely estimated and become smaller under broader inclusion rules.

D.8 Full Main-Outcome Results

The following tables report the main outcome estimates used in the article.

D.8.1 Study 1: Bundling Benchmark

Table 43: Study 1: Bundling benchmark

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complex Bundles	17.02	2.44	[12.23, 21.81]	26.43	1642
High Capacity	Complex Bundles	23.14	3.73	[15.82, 30.45]	15.57	615
Low Capacity	Complex Bundles	13.02	3.27	[6.62, 19.42]	33.43	1027

Table 44: Study 1: Low- and high-value bundled options

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Low EV Complex	13.95	2.87	[8.33, 19.57]	26.43	1642
All sample	High EV Complex	20.01	2.88	[14.36, 25.66]	26.43	1642
High Capacity	Low EV Complex	22.07	4.47	[13.32, 30.82]	15.57	615
High Capacity	High EV Complex	24.25	4.52	[15.40, 33.10]	15.57	615
Low Capacity	Low EV Complex	8.80	3.78	[1.39, 16.22]	33.43	1027
Low Capacity	High EV Complex	17.15	3.82	[9.67, 24.63]	33.43	1027

D.8.2 Study 2: Estimand and Advisor Factor

Study 2 was implemented as a crossed design that also included an advisor factor. The article reports the no-advisor simple effects from this full specification. In the interacted models, the coefficients on high complexity, selective emphasis, and their interaction correspond to effects when the advisor factor is set to zero. This matches the article’s focus on complexity and selective emphasis. As a robustness check, Table 48 compares these estimates to models restricted only to no-advisor observations. The results are substantively unchanged.

D.8.3 Study 2: Suboptimal Choice

Table 45: Study 2: Suboptimal choice

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-0.06	2.10	[-4.18, 4.06]	41.50	1871
All sample	Selective emphasis (1 vs 0)	-1.20	2.24	[-5.60, 3.19]	41.50	1871
All sample	Complexity x selective emphasis	3.69	3.11	[-2.41, 9.79]	41.50	1871
All sample	Joint effect: high complexity + selective emphasis	2.43	2.25	[-1.98, 6.84]	41.50	1871
High Capacity	Complexity (1 vs 0)	5.74	3.53	[-1.17, 12.66]	32.83	701
High Capacity	Selective emphasis (1 vs 0)	3.29	3.75	[-4.05, 10.63]	32.83	701
High Capacity	Complexity x selective emphasis	-8.73	5.09	[-18.70, 1.25]	32.83	701
High Capacity	Joint effect: high complexity + selective emphasis	0.30	3.54	[-6.63, 7.24]	32.83	701
Low Capacity	Complexity (1 vs 0)	-2.11	2.59	[-7.19, 2.96]	46.42	1170
Low Capacity	Selective emphasis (1 vs 0)	-3.57	2.77	[-9.00, 1.87]	46.42	1170
Low Capacity	Complexity x selective emphasis	9.75	3.88	[2.14, 17.35]	46.42	1170
Low Capacity	Joint effect: high complexity + selective emphasis	4.06	2.81	[-1.45, 9.58]	46.42	1170

D.8.4 Study 2: Harmful Bundle Acceptance

Table 46: Study 2: Harmful bundle acceptance

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	1.14	3.16	[-5.07, 7.34]	42.05	1871
All sample	Selective emphasis (1 vs 0)	1.02	3.29	[-5.43, 7.46]	42.05	1871
All sample	Complexity x selective emphasis	4.85	4.59	[-4.14, 13.83]	42.05	1871
All sample	Joint effect: high complexity + selective emphasis	7.00	3.34	[0.45, 13.55]	42.05	1871
High Capacity	Complexity (1 vs 0)	3.53	5.25	[-6.75, 13.82]	36.75	701
High Capacity	Selective emphasis (1 vs 0)	3.74	5.35	[-6.76, 14.23]	36.75	701
High Capacity	Complexity x selective emphasis	-7.38	7.42	[-21.92, 7.16]	36.75	701
High Capacity	Joint effect: high complexity + selective emphasis	-0.11	5.48	[-10.85, 10.64]	36.75	701
Low Capacity	Complexity (1 vs 0)	-0.30	4.01	[-8.17, 7.57]	45.05	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.74	4.20	[-8.97, 7.50]	45.05	1170
Low Capacity	Complexity x selective emphasis	12.63	5.84	[1.18, 24.08]	45.05	1170
Low Capacity	Joint effect: high complexity + selective emphasis	11.59	4.21	[3.35, 19.84]	45.05	1170

D.8.5 Study 2: High-Value Diagnostic

Table 47: Study 2: High-value diagnostic

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-1.98	3.77	[-9.37, 5.41]	41.29	1871
All sample	Selective emphasis (1 vs 0)	-2.64	3.86	[-10.21, 4.92]	41.29	1871
All sample	Complexity x selective emphasis	3.09	5.38	[-7.46, 13.65]	41.29	1871
All sample	Joint effect: high complexity + selective emphasis	-1.53	3.80	[-8.98, 5.93]	41.29	1871
High Capacity	Complexity (1 vs 0)	4.74	6.05	[-7.12, 16.60]	30.12	701
High Capacity	Selective emphasis (1 vs 0)	4.36	5.95	[-7.30, 16.02]	30.12	701
High Capacity	Complexity x selective emphasis	-4.92	8.62	[-21.82, 11.98]	30.12	701
High Capacity	Joint effect: high complexity + selective emphasis	4.18	6.10	[-7.78, 16.14]	30.12	701
Low Capacity	Complexity (1 vs 0)	-5.76	4.83	[-15.24, 3.71]	47.61	1170
Low Capacity	Selective emphasis (1 vs 0)	-6.60	5.05	[-16.50, 3.31]	47.61	1170
Low Capacity	Complexity x selective emphasis	7.66	6.93	[-5.92, 21.24]	47.61	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-4.70	4.87	[-14.25, 4.85]	47.61	1170

D.8.6 Study 2: Advisor-Factor Robustness

Table 48: Study 2 robustness: full crossed specification versus no-advisor-only specification

Analysis	Outcome	Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
Full crossed specification	Harmful bundle acceptance	All respondents	Selective emphasis	1.02	3.29	[-5.43, 7.46]	42.05	1871
No-advisor-only specification	Harmful bundle acceptance	All respondents	Selective emphasis	0.99	3.30	[-5.48, 7.47]	42.05	1871
Full crossed specification	Harmful bundle acceptance	All respondents	High complexity	1.14	3.16	[-5.07, 7.34]	42.05	1871
No-advisor-only specification	Harmful bundle acceptance	All respondents	High complexity	1.44	3.20	[-4.82, 7.71]	42.05	1871
Full crossed specification	Harmful bundle acceptance	All respondents	Interaction	4.85	4.59	[-4.14, 13.83]	42.05	1871
No-advisor-only specification	Harmful bundle acceptance	All respondents	Interaction	4.96	4.61	[-4.07, 13.99]	42.05	1871
Full crossed specification	Harmful bundle acceptance	All respondents	Joint effect	7.00	3.34	[0.45, 13.55]	42.05	1871
No-advisor-only specification	Harmful bundle acceptance	All respondents	Joint effect	7.40	3.34	[0.84, 13.95]	42.05	1871
Full crossed specification	Harmful bundle acceptance	Higher-capacity	Selective emphasis	3.74	5.35	[-6.76, 14.23]	36.75	701
No-advisor-only specification	Harmful bundle acceptance	Higher-capacity	Selective emphasis	4.32	5.38	[-6.21, 14.86]	36.75	701
Full crossed specification	Harmful bundle acceptance	Higher-capacity	High complexity	3.53	5.25	[-6.75, 13.82]	36.75	701
No-advisor-only specification	Harmful bundle acceptance	Higher-capacity	High complexity	5.20	5.29	[-5.17, 15.56]	36.75	701
Full crossed specification	Harmful bundle acceptance	Higher-capacity	Interaction	-7.38	7.42	[-21.92, 7.16]	36.75	701
No-advisor-only specification	Harmful bundle acceptance	Higher-capacity	Interaction	-8.62	7.47	[-23.27, 6.02]	36.75	701
Full crossed specification	Harmful bundle acceptance	Higher-capacity	Joint effect	-0.11	5.48	[-10.85, 10.64]	36.75	701
No-advisor-only specification	Harmful bundle acceptance	Higher-capacity	Joint effect	0.89	5.43	[-9.74, 11.53]	36.75	701
Full crossed specification	Harmful bundle acceptance	Lower-capacity	Selective emphasis	-0.74	4.20	[-8.97, 7.50]	45.05	1170
No-advisor-only specification	Harmful bundle acceptance	Lower-capacity	Selective emphasis	-0.96	4.23	[-9.26, 7.34]	45.05	1170
Full crossed specification	Harmful bundle acceptance	Lower-capacity	High complexity	-0.30	4.01	[-8.17, 7.57]	45.05	1170
No-advisor-only specification	Harmful bundle acceptance	Lower-capacity	High complexity	-0.27	4.09	[-8.28, 7.75]	45.05	1170
Full crossed specification	Harmful bundle acceptance	Lower-capacity	Interaction	12.63	5.84	[1.18, 24.08]	45.05	1170
No-advisor-only specification	Harmful bundle acceptance	Lower-capacity	Interaction	12.88	5.90	[1.33, 24.44]	45.05	1170
Full crossed specification	Harmful bundle acceptance	Lower-capacity	Joint effect	11.59	4.21	[3.35, 19.84]	45.05	1170
No-advisor-only specification	Harmful bundle acceptance	Lower-capacity	Joint effect	11.66	4.22	[3.38, 19.93]	45.05	1170
Full crossed specification	Suboptimal choice	All respondents	Selective emphasis	-1.20	2.24	[-5.60, 3.19]	41.50	1871
No-advisor-only specification	Suboptimal choice	All respondents	Selective emphasis	-1.69	2.26	[-6.12, 2.74]	41.50	1871
Full crossed specification	Suboptimal choice	All respondents	High complexity	-0.06	2.10	[-4.18, 4.06]	41.50	1871
No-advisor-only specification	Suboptimal choice	All respondents	High complexity	-0.31	2.12	[-4.47, 3.85]	41.50	1871
Full crossed specification	Suboptimal choice	All respondents	Interaction	3.69	3.11	[-2.41, 9.79]	41.50	1871
No-advisor-only specification	Suboptimal choice	All respondents	Interaction	4.18	3.14	[-1.98, 10.34]	41.50	1871
Full crossed specification	Suboptimal choice	All respondents	Joint effect	2.43	2.25	[-1.98, 6.84]	41.50	1871
No-advisor-only specification	Suboptimal choice	All respondents	Joint effect	2.18	2.25	[-2.23, 6.59]	41.50	1871
Full crossed specification	Suboptimal choice	Higher-capacity	Selective emphasis	3.29	3.75	[-4.05, 10.63]	32.83	701
No-advisor-only specification	Suboptimal choice	Higher-capacity	Selective emphasis	3.13	3.81	[-4.33, 10.59]	32.83	701
Full crossed specification	Suboptimal choice	Higher-capacity	High complexity	5.74	3.53	[-1.17, 12.66]	32.83	701
No-advisor-only specification	Suboptimal choice	Higher-capacity	High complexity	5.92	3.62	[-1.17, 13.01]	32.83	701
Full crossed specification	Suboptimal choice	Higher-capacity	Interaction	-8.73	5.09	[-18.70, 1.25]	32.83	701
No-advisor-only specification	Suboptimal choice	Higher-capacity	Interaction	-8.95	5.22	[-19.18, 1.28]	32.83	701
Full crossed specification	Suboptimal choice	Higher-capacity	Joint effect	0.30	3.54	[-6.63, 7.24]	32.83	701
No-advisor-only specification	Suboptimal choice	Higher-capacity	Joint effect	0.09	3.57	[-6.90, 7.09]	32.83	701
Full crossed specification	Suboptimal choice	Lower-capacity	Selective emphasis	-3.57	2.77	[-9.00, 1.87]	46.42	1170
No-advisor-only specification	Suboptimal choice	Lower-capacity	Selective emphasis	-4.16	2.78	[-9.62, 1.30]	46.42	1170
Full crossed specification	Suboptimal choice	Lower-capacity	High complexity	-2.11	2.59	[-7.19, 2.96]	46.42	1170
No-advisor-only specification	Suboptimal choice	Lower-capacity	High complexity	-2.23	2.60	[-7.33, 2.86]	46.42	1170
Full crossed specification	Suboptimal choice	Lower-capacity	Interaction	9.75	3.88	[2.14, 17.35]	46.42	1170
No-advisor-only specification	Suboptimal choice	Lower-capacity	Interaction	10.47	3.92	[2.79, 18.14]	46.42	1170
Full crossed specification	Suboptimal choice	Lower-capacity	Joint effect	4.06	2.81	[-1.45, 9.58]	46.42	1170
No-advisor-only specification	Suboptimal choice	Lower-capacity	Joint effect	4.07	2.82	[-1.46, 9.61]	46.42	1170

D.8.7 Study 3: Candidate Support

Table 49: Study 3: Candidate support

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	10.24	2.91	[4.53, 15.95]	24.86	1871
All sample	Selective emphasis (1 vs 0)	9.80	2.90	[4.12, 15.48]	24.86	1871
All sample	Complexity x selective emphasis	-3.07	4.34	[-11.58, 5.43]	24.86	1871
All sample	Joint effect: high complexity + selective emphasis	16.96	2.97	[11.14, 22.78]	24.86	1871
High Capacity	Complexity (1 vs 0)	8.63	4.64	[-0.46, 17.73]	18.00	701
High Capacity	Selective emphasis (1 vs 0)	11.95	4.57	[2.99, 20.90]	18.00	701
High Capacity	Complexity x selective emphasis	-5.67	7.03	[-19.44, 8.10]	18.00	701
High Capacity	Joint effect: high complexity + selective emphasis	14.91	4.69	[5.72, 24.11]	18.00	701
Low Capacity	Complexity (1 vs 0)	11.95	3.79	[4.53, 19.37]	29.05	1170
Low Capacity	Selective emphasis (1 vs 0)	9.72	3.79	[2.29, 17.16]	29.05	1170
Low Capacity	Complexity x selective emphasis	-1.82	5.64	[-12.87, 9.22]	29.05	1170
Low Capacity	Joint effect: high complexity + selective emphasis	19.85	3.81	[12.39, 27.31]	29.05	1170

D.8.8 Study 3: Candidate Recognition

Table 50: Study 3: Candidate recognition

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-6.88	3.20	[-13.16, -0.61]	63.38	1871
All sample	Selective emphasis (1 vs 0)	-2.87	3.19	[-9.12, 3.37]	63.38	1871
All sample	Complexity x selective emphasis	1.89	4.64	[-7.20, 10.98]	63.38	1871
All sample	Joint effect: high complexity + selective emphasis	-7.87	3.18	[-14.11, -1.63]	63.38	1871
High Capacity	Complexity (1 vs 0)	-4.58	5.08	[-14.53, 5.37]	72.00	701
High Capacity	Selective emphasis (1 vs 0)	-3.32	4.98	[-13.08, 6.44]	72.00	701
High Capacity	Complexity x selective emphasis	0.06	7.47	[-14.58, 14.71]	72.00	701
High Capacity	Joint effect: high complexity + selective emphasis	-7.84	5.02	[-17.68, 2.01]	72.00	701
Low Capacity	Complexity (1 vs 0)	-6.85	4.18	[-15.05, 1.35]	58.10	1170
Low Capacity	Selective emphasis (1 vs 0)	-1.93	4.17	[-10.10, 6.23]	58.10	1170
Low Capacity	Complexity x selective emphasis	1.99	6.02	[-9.82, 13.79]	58.10	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-6.80	4.15	[-14.93, 1.33]	58.10	1170

D.8.9 Study 3 Recognition Diagnostics

Study 3 shows that the joint high-complexity and selective-emphasis condition increases support for the reform candidate while reducing recognition of the institutional implication. This appendix unpacks that recognition result by separating correct identification from two distinct forms of recognition failure: attributing the oversight-weakening provision to the wrong candidate and reporting uncertainty.

Appendix Table 51 shows that the recognition decline is driven primarily by increased uncertainty rather than by a large increase in wrong-candidate attribution. In the low-complexity, neutral-emphasis baseline, 63.4 percent of respondents correctly identify the reform candidate, 23.9 percent select the wrong candidate, and 12.7 percent report that they are not sure. In the high-complexity, selective-emphasis condition, correct recognition falls to 56.0 percent, wrong-candidate attribution is 25.2 percent, and uncertainty rises to 18.8 percent.

Appendix Table 52 re-estimates the recognition result after excluding respondents who selected “not sure.” The joint estimate is smaller and imprecisely estimated, which indicates

Table 51: Study 3 recognition-response distribution by treatment cell

Treatment cell	Recognition response	N	Percent
Low complexity / neutral	Correct	334	63.4
Low complexity / neutral	Wrong candidate	126	23.9
Low complexity / neutral	Not sure	67	12.7
High complexity / neutral	Correct	260	56.6
High complexity / neutral	Wrong candidate	119	25.9
High complexity / neutral	Not sure	80	17.4
Low complexity / selective emphasis	Correct	263	60.2
Low complexity / selective emphasis	Wrong candidate	112	25.6
Low complexity / selective emphasis	Not sure	62	14.2
High complexity / selective emphasis	Correct	251	56.0
High complexity / selective emphasis	Wrong candidate	113	25.2
High complexity / selective emphasis	Not sure	84	18.8

that the main recognition decline partly reflects movement into uncertainty. Appendix Tables 53 and 54 then describe how support for the reform candidate varies by recognition response. These patterns show that the joint condition increases support even among respondents who correctly identify the institutional implication, while support is also high among respondents who are uncertain.

These analyses are descriptive. Recognition is measured after treatment, so the association between recognition and support should not be interpreted as a causal mediation estimate. The purpose is narrower: to show that the Study 3 result combines two evaluative margins. The joint condition reduces clear recognition by increasing uncertainty, and it also increases support in ways that are not confined to respondents who fail the recognition item.

Table 52: Study 3 recognition robustness excluding not-sure responses

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-4.01	3.17	[-10.22, 2.2]	72.61	1578
All sample	Selective emphasis (1 vs 0)	-2.48	3.15	[-8.65, 3.7]	72.61	1578
All sample	Complexity x selective emphasis	2.83	4.64	[-6.26, 11.92]	72.61	1578
All sample	Joint effect: high complexity + selective emphasis	-3.65	3.20	[-9.92, 2.62]	72.61	1578
High Capacity	Complexity (1 vs 0)	-3.13	4.60	[-12.14, 5.88]	80.00	618
High Capacity	Selective emphasis (1 vs 0)	-0.95	4.50	[-9.76, 7.86]	80.00	618
High Capacity	Complexity x selective emphasis	2.40	6.66	[-10.65, 15.45]	80.00	618
High Capacity	Joint effect: high complexity + selective emphasis	-1.68	4.57	[-10.64, 7.28]	80.00	618
Low Capacity	Complexity (1 vs 0)	-4.50	4.23	[-12.78, 3.79]	67.86	960
Low Capacity	Selective emphasis (1 vs 0)	-3.54	4.24	[-11.85, 4.77]	67.86	960
Low Capacity	Complexity x selective emphasis	3.07	6.22	[-9.11, 15.26]	67.86	960
Low Capacity	Joint effect: high complexity + selective emphasis	-4.96	4.29	[-13.37, 3.45]	67.86	960

Table 53: Study 3 harmful-candidate support by recognition response

Treatment cell	Recognition response	N respondents	Harmful-candidate support pct
Low complexity / neutral	Correct	334	20.7
Low complexity / neutral	Wrong candidate	126	23.0
Low complexity / neutral	Not sure	67	49.3
High complexity / neutral	Correct	260	32.3
High complexity / neutral	Wrong candidate	119	30.3
High complexity / neutral	Not sure	80	47.5
Low complexity / selective emphasis	Correct	263	35.4
Low complexity / selective emphasis	Wrong candidate	112	25.9
Low complexity / selective emphasis	Not sure	62	35.5
High complexity / selective emphasis	Correct	251	40.2
High complexity / selective emphasis	Wrong candidate	113	34.5
High complexity / selective emphasis	Not sure	84	56.0

Table 54: Study 3 support-recognition diagnostic association

Term	Estimate	SE	95% CI	N respondents
Baseline support among correct recognizers	20.66	2.22	[16.3, 25.01]	975
Joint condition among correct recognizers	19.58	3.82	[12.1, 27.06]	975
Wrong candidate vs correct, baseline	2.36	4.37	[-6.21, 10.92]	975
Not sure vs correct, baseline	28.60	6.52	[15.82, 41.37]	975
Joint condition x wrong candidate	-8.08	6.99	[-21.78, 5.62]	975
Joint condition x not sure	-12.88	9.04	[-30.59, 4.83]	975

D.8.10 Study 4: Municipal Package Acceptance

Table 55: Study 4: Municipal package acceptance

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	7.68	3.29	[1.23, 14.14]	59.06	1871
All sample	Selective emphasis (1 vs 0)	7.17	3.13	[1.03, 13.31]	59.06	1871
All sample	Complexity x selective emphasis	-6.80	4.50	[-15.61, 2.01]	59.06	1871
All sample	Joint effect: high complexity + selective emphasis	8.05	3.21	[1.77, 14.34]	59.06	1871
High Capacity	Complexity (1 vs 0)	15.40	5.50	[4.61, 26.19]	48.31	701
High Capacity	Selective emphasis (1 vs 0)	10.67	5.28	[0.32, 21.02]	48.31	701
High Capacity	Complexity x selective emphasis	-6.34	7.75	[-21.54, 8.86]	48.31	701
High Capacity	Joint effect: high complexity + selective emphasis	19.73	5.37	[9.21, 30.25]	48.31	701
Low Capacity	Complexity (1 vs 0)	2.89	4.06	[-5.06, 10.84]	66.17	1170
Low Capacity	Selective emphasis (1 vs 0)	4.04	3.92	[-3.64, 11.73]	66.17	1170
Low Capacity	Complexity x selective emphasis	-6.35	5.59	[-17.29, 4.60]	66.17	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.59	3.99	[-7.24, 8.42]	66.17	1170

D.8.11 Study 4: Municipal Recognition

Table 56: Study 4: Consequence recognition (2+ domains worsening)

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-2.96	2.48	[-7.82, 1.89]	16.33	1871
All sample	Selective emphasis (1 vs 0)	-3.19	2.34	[-7.78, 1.40]	16.33	1871
All sample	Complexity x selective emphasis	2.00	3.28	[-4.43, 8.43]	16.33	1871
All sample	Joint effect: high complexity + selective emphasis	-4.15	2.37	[-8.80, 0.51]	16.33	1871
High Capacity	Complexity (1 vs 0)	-8.07	4.07	[-16.05, -0.10]	19.66	701
High Capacity	Selective emphasis (1 vs 0)	-3.46	4.13	[-11.54, 4.63]	19.66	701
High Capacity	Complexity x selective emphasis	10.09	5.82	[-1.31, 21.49]	19.66	701
High Capacity	Joint effect: high complexity + selective emphasis	-1.44	4.20	[-9.68, 6.80]	19.66	701
Low Capacity	Complexity (1 vs 0)	0.83	3.20	[-5.43, 7.09]	14.13	1170
Low Capacity	Selective emphasis (1 vs 0)	-2.42	2.88	[-8.07, 3.23]	14.13	1170
Low Capacity	Complexity x selective emphasis	-3.32	4.10	[-11.36, 4.73]	14.13	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-4.91	2.82	[-10.44, 0.63]	14.13	1170

D.9 Study 4 Perceived-Consequence Diagnostics

The following tables report post-treatment perceived-consequence diagnostics for the municipal reform package. Outcomes use a 1–7 scale, where 1 means “worsen a lot” and 7 means “improve a lot.” Employment, discretionary allocation, and service capacity are package-relevant domains. Property tax rates, police and public safety spending, and garbage collection are placebo domains not directly affected by the package. These diagnostics are post-treatment outcomes and should not be interpreted as causal mediation estimates.

Table 57: Study 4: Perceived effect on employment

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	High complexity	0.26	0.11	[0.04, 0.48]	5.03	1871
All sample	Selective emphasis	0.24	0.11	[0.03, 0.45]	5.03	1871
All sample	Interaction	-0.37	0.15	[-0.67, -0.08]	5.03	1871
All sample	Joint effect	0.13	0.11	[-0.08, 0.35]	5.03	1871
High Capacity	High complexity	0.49	0.18	[0.13, 0.85]	4.83	701
High Capacity	Selective emphasis	0.21	0.18	[-0.15, 0.57]	4.83	701
High Capacity	Interaction	-0.62	0.26	[-1.12, -0.12]	4.83	701
High Capacity	Joint effect	0.09	0.19	[-0.29, 0.46]	4.83	701
Low Capacity	High complexity	0.09	0.14	[-0.19, 0.37]	5.16	1170
Low Capacity	Selective emphasis	0.23	0.13	[-0.04, 0.49]	5.16	1170
Low Capacity	Interaction	-0.23	0.19	[-0.60, 0.14]	5.16	1170
Low Capacity	Joint effect	0.09	0.14	[-0.18, 0.36]	5.16	1170

Table 58: Study 4: Perceived effect on discretionary allocation

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	High complexity	-0.05	0.12	[-0.28, 0.18]	4.97	1871
All sample	Selective emphasis	-0.04	0.11	[-0.27, 0.18]	4.97	1871
All sample	Interaction	0.05	0.16	[-0.28, 0.37]	4.97	1871
All sample	Joint effect	-0.05	0.11	[-0.27, 0.17]	4.97	1871
High Capacity	High complexity	-0.01	0.19	[-0.39, 0.36]	5.13	701
High Capacity	Selective emphasis	-0.35	0.19	[-0.73, 0.03]	5.13	701
High Capacity	Interaction	-0.10	0.29	[-0.66, 0.46]	5.13	701
High Capacity	Joint effect	-0.47	0.19	[-0.85, -0.09]	5.13	701
Low Capacity	High complexity	-0.04	0.15	[-0.34, 0.26]	4.86	1170
Low Capacity	Selective emphasis	0.11	0.14	[-0.18, 0.39]	4.86	1170
Low Capacity	Interaction	0.10	0.21	[-0.31, 0.51]	4.86	1170
Low Capacity	Joint effect	0.17	0.14	[-0.10, 0.44]	4.86	1170

Table 59: Study 4: Perceived effect on service capacity

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	High complexity	0.08	0.11	[-0.13, 0.29]	5.11	1871
All sample	Selective emphasis	0.13	0.10	[-0.07, 0.33]	5.11	1871
All sample	Interaction	-0.14	0.15	[-0.43, 0.15]	5.11	1871
All sample	Joint effect	0.07	0.10	[-0.14, 0.27]	5.11	1871
High Capacity	High complexity	0.12	0.17	[-0.22, 0.45]	5.10	701
High Capacity	Selective emphasis	0.06	0.17	[-0.28, 0.41]	5.10	701
High Capacity	Interaction	-0.35	0.24	[-0.83, 0.12]	5.10	701
High Capacity	Joint effect	-0.17	0.17	[-0.51, 0.17]	5.10	701
Low Capacity	High complexity	0.03	0.14	[-0.25, 0.30]	5.12	1170
Low Capacity	Selective emphasis	0.17	0.13	[-0.08, 0.43]	5.12	1170
Low Capacity	Interaction	-0.05	0.19	[-0.42, 0.32]	5.12	1170
Low Capacity	Joint effect	0.15	0.13	[-0.11, 0.40]	5.12	1170

Table 60: Study 4: Perceived effect on property tax rates

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	High complexity	0.03	0.12	[-0.20, 0.26]	4.45	1871
All sample	Selective emphasis	-0.04	0.11	[-0.26, 0.18]	4.45	1871
All sample	Interaction	0.01	0.16	[-0.30, 0.33]	4.45	1871
All sample	Joint effect	-0.00	0.11	[-0.22, 0.22]	4.45	1871
High Capacity	High complexity	0.12	0.19	[-0.25, 0.49]	4.35	701
High Capacity	Selective emphasis	-0.21	0.18	[-0.56, 0.15]	4.35	701
High Capacity	Interaction	0.01	0.26	[-0.50, 0.52]	4.35	701
High Capacity	Joint effect	-0.08	0.18	[-0.43, 0.27]	4.35	701
Low Capacity	High complexity	-0.02	0.15	[-0.33, 0.28]	4.52	1170
Low Capacity	Selective emphasis	0.04	0.14	[-0.24, 0.32]	4.52	1170
Low Capacity	Interaction	0.01	0.21	[-0.39, 0.41]	4.52	1170
Low Capacity	Joint effect	0.03	0.14	[-0.25, 0.31]	4.52	1170

Table 61: Study 4: Perceived effect on police and public safety spending

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	High complexity	0.10	0.12	[-0.13, 0.34]	4.33	1871
All sample	Selective emphasis	0.14	0.11	[-0.08, 0.37]	4.33	1871
All sample	Interaction	-0.09	0.16	[-0.41, 0.23]	4.33	1871
All sample	Joint effect	0.15	0.11	[-0.07, 0.38]	4.33	1871
High Capacity	High complexity	0.07	0.20	[-0.32, 0.45]	4.20	701
High Capacity	Selective emphasis	0.02	0.19	[-0.35, 0.39]	4.20	701
High Capacity	Interaction	-0.14	0.28	[-0.68, 0.40]	4.20	701
High Capacity	Joint effect	-0.06	0.18	[-0.41, 0.30]	4.20	701
Low Capacity	High complexity	0.06	0.15	[-0.24, 0.36]	4.42	1170
Low Capacity	Selective emphasis	0.20	0.15	[-0.09, 0.49]	4.42	1170
Low Capacity	Interaction	0.01	0.21	[-0.40, 0.41]	4.42	1170
Low Capacity	Joint effect	0.27	0.14	[-0.02, 0.55]	4.42	1170

Table 62: Study 4: Perceived effect on garbage collection

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	High complexity	0.18	0.12	[-0.05, 0.40]	4.59	1871
All sample	Selective emphasis	0.22	0.11	[0.01, 0.44]	4.59	1871
All sample	Interaction	-0.26	0.16	[-0.57, 0.04]	4.59	1871
All sample	Joint effect	0.14	0.11	[-0.08, 0.35]	4.59	1871
High Capacity	High complexity	0.33	0.19	[-0.05, 0.71]	4.39	701
High Capacity	Selective emphasis	0.06	0.19	[-0.31, 0.43]	4.39	701
High Capacity	Interaction	-0.25	0.27	[-0.78, 0.29]	4.39	701
High Capacity	Joint effect	0.14	0.18	[-0.22, 0.49]	4.39	701
Low Capacity	High complexity	0.10	0.15	[-0.19, 0.40]	4.72	1170
Low Capacity	Selective emphasis	0.33	0.14	[0.06, 0.59]	4.72	1170
Low Capacity	Interaction	-0.31	0.20	[-0.71, 0.08]	4.72	1170
Low Capacity	Joint effect	0.11	0.14	[-0.16, 0.39]	4.72	1170

The final diagnostic table asks whether measured consequence perceptions are associated with package acceptance differently in the joint high-complexity and selective-emphasis condition. The table uses only the low-complexity, neutral-emphasis baseline and the high-complexity, selective-emphasis joint condition. Perceived-consequence predictors are standardized within the Study 4 analytic sample. Relevant consequence perceptions combine employment and service capacity. Placebo-domain perceptions combine property tax rates, police and public safety spending, and garbage collection.

Table 63: Study 4: Perceived consequences and package acceptance

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Joint condition	5.70	2.84	[0.14, 11.27]	59.06	912
All sample	Relevant consequence perceptions	20.33	2.43	[15.57, 25.09]	59.06	912
All sample	Discretionary allocation perception	5.10	2.27	[0.65, 9.55]	59.06	912
All sample	Placebo-domain perceptions	10.86	2.83	[5.32, 16.40]	59.06	912
All sample	Joint x relevant consequence perceptions	-4.54	3.77	[-11.93, 2.85]	59.06	912
All sample	Joint x discretionary allocation perception	-1.95	3.28	[-8.38, 4.48]	59.06	912
All sample	Joint x placebo-domain perceptions	1.61	4.38	[-6.98, 10.20]	59.06	912
High Capacity	Joint condition	19.44	5.06	[9.52, 29.36]	48.31	350
High Capacity	Relevant consequence perceptions	21.19	3.67	[14.00, 28.38]	48.31	350
High Capacity	Discretionary allocation perception	3.94	3.57	[-3.05, 10.93]	48.31	350
High Capacity	Placebo-domain perceptions	8.06	4.57	[-0.91, 17.02]	48.31	350
High Capacity	Joint x relevant consequence perceptions	-3.86	5.87	[-15.37, 7.65]	48.31	350
High Capacity	Joint x discretionary allocation perception	0.90	5.05	[-8.99, 10.79]	48.31	350
High Capacity	Joint x placebo-domain perceptions	-5.08	7.61	[-19.99, 9.84]	48.31	350
Low Capacity	Joint condition	-3.49	3.58	[-10.50, 3.52]	66.17	562
Low Capacity	Relevant consequence perceptions	19.33	3.52	[12.43, 26.23]	66.17	562
Low Capacity	Discretionary allocation perception	8.15	3.14	[2.00, 14.30]	66.17	562
Low Capacity	Placebo-domain perceptions	8.81	3.92	[1.13, 16.48]	66.17	562
Low Capacity	Joint x relevant consequence perceptions	-3.10	5.42	[-13.72, 7.53]	66.17	562
Low Capacity	Joint x discretionary allocation perception	-6.17	4.65	[-15.28, 2.94]	66.17	562
Low Capacity	Joint x placebo-domain perceptions	9.42	5.75	[-1.84, 20.69]	66.17	562

Note: These models are diagnostic and should not be interpreted as causal mediation estimates. The perceived-consequence outcomes are post-treatment measures. The perception-acceptance diagnostic tests whether measured perceptions predict acceptance differently in the joint condition, not whether the treatment causally changes weighting.

D.10 Process Outcomes

The following tables report secondary process outcomes. These measures describe task experience and scrutiny. They should not be read as independent mechanisms without the corresponding choice and recognition evidence.

D.10.1 Perceived Difficulty

Table 64: Study 1: Difficulty

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Low EV Complex	0.13	0.06	[0.00, 0.25]	3.96	1642
All sample	High EV Complex	0.13	0.06	[0.01, 0.25]	3.96	1642
High Capacity	Low EV Complex	0.07	0.11	[-0.13, 0.28]	3.67	615
High Capacity	High EV Complex	0.12	0.10	[-0.08, 0.32]	3.67	615
Low Capacity	Low EV Complex	0.17	0.08	[0.01, 0.32]	4.15	1027
Low Capacity	High EV Complex	0.15	0.08	[0.00, 0.30]	4.15	1027

Table 65: Study 2: Difficulty, suboptimal-choice task

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.05	0.06	[-0.06, 0.17]	4.29	1871
All sample	Selective emphasis (1 vs 0)	0.03	0.06	[-0.09, 0.15]	4.29	1871
All sample	Complexity x selective emphasis	-0.12	0.09	[-0.29, 0.05]	4.29	1871
All sample	Joint effect: high complexity + selective emphasis	-0.03	0.06	[-0.15, 0.09]	4.29	1871
High Capacity	Complexity (1 vs 0)	0.10	0.10	[-0.09, 0.30]	3.89	701
High Capacity	Selective emphasis (1 vs 0)	0.19	0.10	[-0.01, 0.40]	3.89	701
High Capacity	Complexity x selective emphasis	-0.28	0.15	[-0.57, 0.01]	3.89	701
High Capacity	Joint effect: high complexity + selective emphasis	0.02	0.11	[-0.19, 0.23]	3.89	701
Low Capacity	Complexity (1 vs 0)	0.06	0.07	[-0.08, 0.20]	4.52	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.01	0.07	[-0.15, 0.14]	4.52	1170
Low Capacity	Complexity x selective emphasis	-0.07	0.11	[-0.27, 0.14]	4.52	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-0.02	0.08	[-0.16, 0.13]	4.52	1170

Table 66: Study 2: Difficulty, harmful-bundle task

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.02	0.06	[-0.11, 0.15]	4.29	1871
All sample	Selective emphasis (1 vs 0)	0.03	0.07	[-0.10, 0.16]	4.29	1871
All sample	Complexity x selective emphasis	-0.12	0.09	[-0.30, 0.06]	4.29	1871
All sample	Joint effect: high complexity + selective emphasis	-0.07	0.07	[-0.20, 0.06]	4.29	1871
High Capacity	Complexity (1 vs 0)	0.10	0.11	[-0.10, 0.31]	3.89	701
High Capacity	Selective emphasis (1 vs 0)	0.23	0.11	[0.02, 0.45]	3.89	701
High Capacity	Complexity x selective emphasis	-0.26	0.16	[-0.57, 0.05]	3.89	701
High Capacity	Joint effect: high complexity + selective emphasis	0.08	0.11	[-0.14, 0.29]	3.89	701
Low Capacity	Complexity (1 vs 0)	0.01	0.08	[-0.15, 0.16]	4.52	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.04	0.08	[-0.20, 0.12]	4.52	1170
Low Capacity	Complexity x selective emphasis	-0.08	0.12	[-0.31, 0.15]	4.52	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-0.11	0.08	[-0.28, 0.05]	4.52	1170

Table 67: Study 3: Difficulty

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.11	0.06	[-0.02, 0.24]	3.82	1871
All sample	Selective emphasis (1 vs 0)	0.13	0.07	[0.00, 0.26]	3.82	1871
All sample	Complexity x selective emphasis	-0.06	0.09	[-0.24, 0.12]	3.82	1871
All sample	Joint effect: high complexity + selective emphasis	0.18	0.07	[0.06, 0.31]	3.82	1871
High Capacity	Complexity (1 vs 0)	0.07	0.11	[-0.14, 0.29]	3.46	701
High Capacity	Selective emphasis (1 vs 0)	0.08	0.11	[-0.13, 0.30]	3.46	701
High Capacity	Complexity x selective emphasis	0.14	0.16	[-0.17, 0.44]	3.46	701
High Capacity	Joint effect: high complexity + selective emphasis	0.30	0.11	[0.08, 0.51]	3.46	701
Low Capacity	Complexity (1 vs 0)	0.09	0.08	[-0.06, 0.25]	4.03	1170
Low Capacity	Selective emphasis (1 vs 0)	0.16	0.08	[-0.01, 0.32]	4.03	1170
Low Capacity	Complexity x selective emphasis	-0.14	0.12	[-0.37, 0.09]	4.03	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.11	0.08	[-0.05, 0.27]	4.03	1170

Table 68: Study 4: Difficulty

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.12	0.07	[-0.02, 0.25]	4.12	1871
All sample	Selective emphasis (1 vs 0)	0.05	0.07	[-0.08, 0.18]	4.12	1871
All sample	Complexity x selective emphasis	-0.02	0.09	[-0.21, 0.16]	4.12	1871
All sample	Joint effect: high complexity + selective emphasis	0.14	0.07	[0.01, 0.27]	4.12	1871
High Capacity	Complexity (1 vs 0)	0.17	0.11	[-0.04, 0.39]	4.01	701
High Capacity	Selective emphasis (1 vs 0)	-0.02	0.11	[-0.24, 0.20]	4.01	701
High Capacity	Complexity x selective emphasis	-0.01	0.16	[-0.32, 0.30]	4.01	701
High Capacity	Joint effect: high complexity + selective emphasis	0.14	0.11	[-0.07, 0.36]	4.01	701
Low Capacity	Complexity (1 vs 0)	0.08	0.09	[-0.09, 0.25]	4.19	1170
Low Capacity	Selective emphasis (1 vs 0)	0.07	0.08	[-0.10, 0.24]	4.19	1170
Low Capacity	Complexity x selective emphasis	-0.01	0.12	[-0.24, 0.23]	4.19	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.14	0.09	[-0.03, 0.31]	4.19	1170

D.10.2 Confidence

Table 69: Study 1: Confidence

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Low EV Complex	-0.08	0.06	[-0.20, 0.04]	5.32	1642
All sample	High EV Complex	-0.11	0.06	[-0.23, 0.01]	5.32	1642
High Capacity	Low EV Complex	-0.05	0.10	[-0.25, 0.15]	5.45	615
High Capacity	High EV Complex	-0.04	0.11	[-0.25, 0.17]	5.45	615
Low Capacity	Low EV Complex	-0.09	0.08	[-0.25, 0.07]	5.24	1027
Low Capacity	High EV Complex	-0.11	0.08	[-0.27, 0.04]	5.24	1027

Table 70: Study 2: Confidence, suboptimal-choice task

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-0.02	0.06	[-0.14, 0.09]	5.23	1871
All sample	Selective emphasis (1 vs 0)	-0.04	0.06	[-0.16, 0.07]	5.23	1871
All sample	Complexity x selective emphasis	-0.06	0.08	[-0.23, 0.10]	5.23	1871
All sample	Joint effect: high complexity + selective emphasis	-0.13	0.06	[-0.25, -0.01]	5.23	1871
High Capacity	Complexity (1 vs 0)	-0.13	0.10	[-0.33, 0.06]	5.44	701
High Capacity	Selective emphasis (1 vs 0)	-0.08	0.10	[-0.27, 0.11]	5.44	701
High Capacity	Complexity x selective emphasis	0.20	0.14	[-0.08, 0.47]	5.44	701
High Capacity	Joint effect: high complexity + selective emphasis	-0.02	0.10	[-0.21, 0.18]	5.44	701
Low Capacity	Complexity (1 vs 0)	0.02	0.07	[-0.12, 0.16]	5.10	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.04	0.07	[-0.18, 0.11]	5.10	1170
Low Capacity	Complexity x selective emphasis	-0.17	0.11	[-0.38, 0.04]	5.10	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-0.18	0.08	[-0.34, -0.03]	5.10	1170

Table 71: Study 2: Confidence, harmful-bundle task

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-0.02	0.06	[-0.15, 0.11]	5.23	1871
All sample	Selective emphasis (1 vs 0)	-0.08	0.07	[-0.21, 0.05]	5.23	1871
All sample	Complexity x selective emphasis	-0.01	0.09	[-0.19, 0.18]	5.23	1871
All sample	Joint effect: high complexity + selective emphasis	-0.11	0.07	[-0.24, 0.03]	5.23	1871
High Capacity	Complexity (1 vs 0)	-0.11	0.11	[-0.32, 0.10]	5.44	701
High Capacity	Selective emphasis (1 vs 0)	-0.10	0.11	[-0.31, 0.10]	5.44	701
High Capacity	Complexity x selective emphasis	0.19	0.15	[-0.11, 0.48]	5.44	701
High Capacity	Joint effect: high complexity + selective emphasis	-0.03	0.11	[-0.25, 0.18]	5.44	701
Low Capacity	Complexity (1 vs 0)	0.01	0.08	[-0.15, 0.17]	5.10	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.08	0.08	[-0.24, 0.08]	5.10	1170
Low Capacity	Complexity x selective emphasis	-0.07	0.12	[-0.30, 0.16]	5.10	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-0.14	0.09	[-0.31, 0.03]	5.10	1170

Table 72: Study 3: Confidence

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-0.17	0.07	[-0.29, -0.04]	5.77	1871
All sample	Selective emphasis (1 vs 0)	-0.12	0.07	[-0.25, 0.00]	5.77	1871
All sample	Complexity x selective emphasis	0.20	0.09	[0.01, 0.38]	5.77	1871
All sample	Joint effect: high complexity + selective emphasis	-0.10	0.06	[-0.22, 0.03]	5.77	1871
High Capacity	Complexity (1 vs 0)	-0.07	0.11	[-0.29, 0.15]	5.80	701
High Capacity	Selective emphasis (1 vs 0)	-0.03	0.10	[-0.23, 0.17]	5.80	701
High Capacity	Complexity x selective emphasis	0.04	0.15	[-0.26, 0.34]	5.80	701
High Capacity	Joint effect: high complexity + selective emphasis	-0.06	0.10	[-0.26, 0.14]	5.80	701
Low Capacity	Complexity (1 vs 0)	-0.24	0.08	[-0.40, -0.07]	5.76	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.19	0.09	[-0.36, -0.01]	5.76	1170
Low Capacity	Complexity x selective emphasis	0.29	0.12	[0.05, 0.54]	5.76	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-0.13	0.08	[-0.30, 0.04]	5.76	1170

Table 73: Study 4: Confidence

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	-0.03	0.07	[-0.17, 0.11]	5.34	1871
All sample	Selective emphasis (1 vs 0)	0.11	0.06	[-0.01, 0.24]	5.34	1871
All sample	Complexity x selective emphasis	-0.10	0.09	[-0.29, 0.08]	5.34	1871
All sample	Joint effect: high complexity + selective emphasis	-0.02	0.07	[-0.15, 0.11]	5.34	1871
High Capacity	Complexity (1 vs 0)	-0.13	0.11	[-0.34, 0.09]	5.51	701
High Capacity	Selective emphasis (1 vs 0)	-0.04	0.10	[-0.24, 0.15]	5.51	701
High Capacity	Complexity x selective emphasis	0.05	0.15	[-0.26, 0.35]	5.51	701
High Capacity	Joint effect: high complexity + selective emphasis	-0.12	0.11	[-0.34, 0.09]	5.51	701
Low Capacity	Complexity (1 vs 0)	0.04	0.09	[-0.14, 0.22]	5.24	1170
Low Capacity	Selective emphasis (1 vs 0)	0.23	0.08	[0.07, 0.40]	5.24	1170
Low Capacity	Complexity x selective emphasis	-0.22	0.12	[-0.46, 0.02]	5.24	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.05	0.09	[-0.11, 0.22]	5.24	1170

D.10.3 Time Spent

Table 74: Study 1: Time spent

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Low EV Complex	0.31	0.06	[0.19, 0.43]	25.80	1642
All sample	High EV Complex	0.36	0.06	[0.25, 0.47]	25.80	1642
High Capacity	Low EV Complex	0.43	0.10	[0.24, 0.62]	29.16	615
High Capacity	High EV Complex	0.50	0.09	[0.32, 0.68]	29.16	615
Low Capacity	Low EV Complex	0.28	0.08	[0.13, 0.43]	23.62	1027
Low Capacity	High EV Complex	0.32	0.07	[0.17, 0.46]	23.62	1027

Table 75: Study 2: Time spent, suboptimal-choice task

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.03	0.06	[-0.08, 0.15]	32.46	1871
All sample	Selective emphasis (1 vs 0)	-0.08	0.06	[-0.19, 0.04]	32.46	1871
All sample	Complexity x selective emphasis	-0.02	0.08	[-0.19, 0.14]	32.46	1871
All sample	Joint effect: high complexity + selective emphasis	-0.07	0.06	[-0.19, 0.05]	32.46	1871
High Capacity	Complexity (1 vs 0)	0.17	0.10	[-0.03, 0.37]	39.75	701
High Capacity	Selective emphasis (1 vs 0)	-0.07	0.10	[-0.28, 0.13]	39.75	701
High Capacity	Complexity x selective emphasis	-0.09	0.15	[-0.38, 0.20]	39.75	701
High Capacity	Joint effect: high complexity + selective emphasis	0.01	0.11	[-0.21, 0.22]	39.75	701
Low Capacity	Complexity (1 vs 0)	-0.06	0.07	[-0.19, 0.07]	28.32	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.09	0.07	[-0.23, 0.05]	28.32	1170
Low Capacity	Complexity x selective emphasis	0.01	0.10	[-0.18, 0.20]	28.32	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-0.14	0.07	[-0.28, -0.01]	28.32	1170

Table 76: Study 2: Time spent, harmful-bundle task

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.00	0.06	[-0.12, 0.13]	32.46	1871
All sample	Selective emphasis (1 vs 0)	-0.09	0.07	[-0.22, 0.04]	32.46	1871
All sample	Complexity x selective emphasis	-0.02	0.09	[-0.20, 0.16]	32.46	1871
All sample	Joint effect: high complexity + selective emphasis	-0.12	0.07	[-0.25, 0.01]	32.46	1871
High Capacity	Complexity (1 vs 0)	0.09	0.11	[-0.13, 0.31]	39.75	701
High Capacity	Selective emphasis (1 vs 0)	-0.11	0.11	[-0.33, 0.12]	39.75	701
High Capacity	Complexity x selective emphasis	-0.04	0.16	[-0.36, 0.28]	39.75	701
High Capacity	Joint effect: high complexity + selective emphasis	-0.05	0.12	[-0.29, 0.18]	39.75	701
Low Capacity	Complexity (1 vs 0)	-0.07	0.07	[-0.22, 0.08]	28.32	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.09	0.08	[-0.25, 0.06]	28.32	1170
Low Capacity	Complexity x selective emphasis	-0.02	0.11	[-0.23, 0.19]	28.32	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-0.18	0.08	[-0.33, -0.03]	28.32	1170

Table 77: Study 3: Time spent

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.36	0.06	[0.24, 0.49]	22.60	1871
All sample	Selective emphasis (1 vs 0)	-0.08	0.05	[-0.19, 0.02]	22.60	1871
All sample	Complexity x selective emphasis	0.01	0.09	[-0.17, 0.19]	22.60	1871
All sample	Joint effect: high complexity + selective emphasis	0.29	0.06	[0.16, 0.41]	22.60	1871
High Capacity	Complexity (1 vs 0)	0.57	0.11	[0.36, 0.78]	25.05	701
High Capacity	Selective emphasis (1 vs 0)	-0.16	0.08	[-0.32, -0.00]	25.05	701
High Capacity	Complexity x selective emphasis	-0.08	0.15	[-0.37, 0.20]	25.05	701
High Capacity	Joint effect: high complexity + selective emphasis	0.33	0.10	[0.13, 0.53]	25.05	701
Low Capacity	Complexity (1 vs 0)	0.26	0.08	[0.10, 0.42]	21.10	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.03	0.07	[-0.16, 0.11]	21.10	1170
Low Capacity	Complexity x selective emphasis	0.03	0.12	[-0.20, 0.26]	21.10	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.27	0.08	[0.11, 0.43]	21.10	1170

Table 78: Study 4: Time spent

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.07	0.07	[-0.06, 0.20]	45.12	1871
All sample	Selective emphasis (1 vs 0)	-0.00	0.06	[-0.12, 0.11]	45.12	1871
All sample	Complexity x selective emphasis	0.06	0.09	[-0.13, 0.24]	45.12	1871
All sample	Joint effect: high complexity + selective emphasis	0.12	0.07	[-0.00, 0.25]	45.12	1871
High Capacity	Complexity (1 vs 0)	0.01	0.11	[-0.20, 0.22]	52.40	701
High Capacity	Selective emphasis (1 vs 0)	-0.08	0.09	[-0.26, 0.11]	52.40	701
High Capacity	Complexity x selective emphasis	0.26	0.16	[-0.05, 0.57]	52.40	701
High Capacity	Joint effect: high complexity + selective emphasis	0.19	0.11	[-0.02, 0.40]	52.40	701
Low Capacity	Complexity (1 vs 0)	0.12	0.08	[-0.05, 0.28]	40.31	1170
Low Capacity	Selective emphasis (1 vs 0)	0.07	0.08	[-0.08, 0.22]	40.31	1170
Low Capacity	Complexity x selective emphasis	-0.08	0.11	[-0.30, 0.14]	40.31	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.11	0.08	[-0.05, 0.26]	40.31	1170

D.10.4 Clicks

Table 79: Study 1: Clicks

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Low EV Complex	0.35	0.06	[0.24, 0.46]	7.30	1642
All sample	High EV Complex	0.44	0.06	[0.32, 0.55]	7.30	1642
High Capacity	Low EV Complex	0.35	0.09	[0.17, 0.52]	7.78	615
High Capacity	High EV Complex	0.48	0.10	[0.29, 0.67]	7.78	615
Low Capacity	Low EV Complex	0.35	0.07	[0.21, 0.49]	6.99	1027
Low Capacity	High EV Complex	0.44	0.08	[0.29, 0.59]	6.99	1027

Table 80: Study 2: Clicks, suboptimal-choice task

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.12	0.06	[0.01, 0.23]	13.28	1871
All sample	Selective emphasis (1 vs 0)	-0.01	0.06	[-0.12, 0.10]	13.28	1871
All sample	Complexity x selective emphasis	-0.06	0.08	[-0.22, 0.10]	13.28	1871
All sample	Joint effect: high complexity + selective emphasis	0.05	0.06	[-0.06, 0.16]	13.28	1871
High Capacity	Complexity (1 vs 0)	0.21	0.10	[0.02, 0.40]	13.85	701
High Capacity	Selective emphasis (1 vs 0)	0.03	0.09	[-0.14, 0.20]	13.85	701
High Capacity	Complexity x selective emphasis	-0.17	0.14	[-0.44, 0.09]	13.85	701
High Capacity	Joint effect: high complexity + selective emphasis	0.06	0.09	[-0.11, 0.24]	13.85	701
Low Capacity	Complexity (1 vs 0)	0.07	0.07	[-0.07, 0.21]	12.97	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.00	0.07	[-0.14, 0.13]	12.97	1170
Low Capacity	Complexity x selective emphasis	-0.04	0.10	[-0.23, 0.16]	12.97	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.02	0.07	[-0.12, 0.16]	12.97	1170

Table 81: Study 2: Clicks, harmful-bundle task

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.09	0.07	[-0.04, 0.22]	13.28	1871
All sample	Selective emphasis (1 vs 0)	-0.01	0.06	[-0.13, 0.12]	13.28	1871
All sample	Complexity x selective emphasis	-0.07	0.09	[-0.25, 0.12]	13.28	1871
All sample	Joint effect: high complexity + selective emphasis	0.02	0.06	[-0.11, 0.15]	13.28	1871
High Capacity	Complexity (1 vs 0)	0.16	0.11	[-0.05, 0.38]	13.85	701
High Capacity	Selective emphasis (1 vs 0)	0.04	0.11	[-0.17, 0.25]	13.85	701
High Capacity	Complexity x selective emphasis	-0.17	0.16	[-0.47, 0.14]	13.85	701
High Capacity	Joint effect: high complexity + selective emphasis	0.03	0.10	[-0.16, 0.23]	13.85	701
Low Capacity	Complexity (1 vs 0)	0.05	0.08	[-0.12, 0.21]	12.97	1170
Low Capacity	Selective emphasis (1 vs 0)	0.00	0.08	[-0.16, 0.16]	12.97	1170
Low Capacity	Complexity x selective emphasis	-0.05	0.12	[-0.28, 0.17]	12.97	1170
Low Capacity	Joint effect: high complexity + selective emphasis	-0.01	0.08	[-0.17, 0.16]	12.97	1170

Table 82: Study 3: Clicks

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.64	0.07	[0.51, 0.76]	8.26	1871
All sample	Selective emphasis (1 vs 0)	-0.01	0.05	[-0.11, 0.09]	8.26	1871
All sample	Complexity x selective emphasis	-0.03	0.09	[-0.20, 0.15]	8.26	1871
All sample	Joint effect: high complexity + selective emphasis	0.60	0.06	[0.48, 0.72]	8.26	1871
High Capacity	Complexity (1 vs 0)	0.81	0.10	[0.61, 1.01]	7.87	701
High Capacity	Selective emphasis (1 vs 0)	-0.03	0.07	[-0.17, 0.12]	7.87	701
High Capacity	Complexity x selective emphasis	-0.00	0.14	[-0.28, 0.28]	7.87	701
High Capacity	Joint effect: high complexity + selective emphasis	0.78	0.10	[0.59, 0.97]	7.87	701
Low Capacity	Complexity (1 vs 0)	0.53	0.08	[0.36, 0.69]	8.51	1170
Low Capacity	Selective emphasis (1 vs 0)	-0.02	0.07	[-0.16, 0.12]	8.51	1170
Low Capacity	Complexity x selective emphasis	0.03	0.11	[-0.20, 0.25]	8.51	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.53	0.08	[0.38, 0.69]	8.51	1170

Table 83: Study 4: Clicks

Subsample	Contrast	Estimate	SE	95% CI	Baseline	N respondents
All sample	Complexity (1 vs 0)	0.32	0.07	[0.19, 0.46]	12.77	1871
All sample	Selective emphasis (1 vs 0)	0.08	0.05	[-0.03, 0.18]	12.77	1871
All sample	Complexity x selective emphasis	-0.07	0.10	[-0.26, 0.12]	12.77	1871
All sample	Joint effect: high complexity + selective emphasis	0.33	0.06	[0.21, 0.45]	12.77	1871
High Capacity	Complexity (1 vs 0)	0.44	0.12	[0.20, 0.67]	13.36	701
High Capacity	Selective emphasis (1 vs 0)	0.05	0.09	[-0.12, 0.22]	13.36	701
High Capacity	Complexity x selective emphasis	-0.13	0.16	[-0.44, 0.18]	13.36	701
High Capacity	Joint effect: high complexity + selective emphasis	0.35	0.10	[0.16, 0.54]	13.36	701
Low Capacity	Complexity (1 vs 0)	0.30	0.09	[0.13, 0.47]	12.37	1170
Low Capacity	Selective emphasis (1 vs 0)	0.10	0.07	[-0.04, 0.24]	12.37	1170
Low Capacity	Complexity x selective emphasis	-0.07	0.12	[-0.31, 0.16]	12.37	1170
Low Capacity	Joint effect: high complexity + selective emphasis	0.33	0.08	[0.17, 0.48]	12.37	1170

D.11 Reproducibility Materials

The replication package will include:

- final survey instrument;
- Qualtrics QSF export;
- cleaned analysis data;
- codebook;
- analysis scripts;
- generated result files, including `jmp_master_results.csv`;
- generated LaTeX number and table files.

D.12 Cognitive Items and Capacity Classification

The cognitive-type classification uses five pretreatment reasoning items administered before treatment assignment and before the randomized experimental modules. Two items are treated as basic CRT/engagement items. Respondents who missed both basic items are excluded under the preregistered CRT rule. The three remaining items are harder reasoning items used to distinguish higher-capacity respondents among those who passed the basic-item rule. Higher-capacity respondents answered both basic items correctly and at least one harder item correctly. Lower-capacity respondents passed the basic-item inclusion rule but did not meet the higher-capacity threshold. Table 84 reports the item classification and coding rule.

The retained sample satisfies:

$$CC_2 = 1 \quad \text{or} \quad CC_4 = 1.$$

Among retained respondents, the higher-capacity indicator is coded as:

$$\text{Higher-capacity}_i = 1\{CC_2 = 1, CC_4 = 1, CC_1 + CC_3 + CC_5 \geq 1\}.$$

All retained respondents who do not satisfy this rule are coded as lower-capacity.

Table 84: Pretreatment cognitive items and capacity classification

Item	Item wording	Correct answer	Use in coding
CC_2	Which of the following is most unlike the others? Tulip, Rose, Bud, Daisy, Lily, or “I am not sure.”	Bud	Basic pretreatment screen item. Respondents must answer at least one of CC_2 or CC_4 correctly for inclusion under the preregistered CRT rule.
CC_4	You flip a fair coin 10 times. What is the expected number of heads?	5	Basic pretreatment screen item. Respondents must answer at least one of CC_2 or CC_4 correctly for inclusion under the preregistered CRT rule.
CC_1	What is the next letter in the following series? J – F – M – A – M – ?	J	Harder pretreatment reasoning item. Counts toward the higher-capacity threshold among retained respondents.
CC_3	A bat and a ball cost \$1.10 in total. The bat costs \$1.00 more than the ball. How much does the ball cost?	\$0.05	Harder pretreatment reasoning item. Counts toward the higher-capacity threshold among retained respondents.
CC_5	A patch of algae doubles in size every day. It takes 30 days for the algae to cover the whole pond. On which day is the pond half covered?	Day 29	Harder pretreatment reasoning item. Counts toward the higher-capacity threshold among retained respondents.

Note: All five items were administered before treatment assignment and before the randomized experimental modules. The preregistered CRT rule retains respondents who answered at least one of the two basic screen items correctly. Among retained respondents, higher-capacity respondents are those who answered both basic screen items correctly and at least one of the three harder reasoning items correctly. All retained respondents who do not meet this threshold are classified as lower-capacity.

D.13 Preregistration, Deviations, and Appendix-Only Analyses

The experiment and main analysis plan were preregistered on OSF on March 28, 2026. The registration describes a multi-module experiment covering bundling, complexity, selective emphasis, endorsement or advice, information acquisition, political reform evaluation, recognition, and heterogeneity by cognitive performance. The present article focuses on the subset of those analyses that bear directly on the paper’s argument about bundled evaluation, complexity, and selective emphasis. Table 85 summarizes the correspondence between the registered plan and the analyses reported in the article, as well as the main departures from the registration.

Table 85: Preregistration mapping and departures

Article component	Preregistered plan	Implementation in the article
Study 1: bundling benchmark	The registration predicts that bundling will worsen choice quality relative to isolated presentation of the same underlying alternatives.	Reported as Study 1. The article retains the preregistered three-arm benchmark design and uses suboptimal choice as the main outcome.
Study 2: complexity and selective emphasis	The registration specifies a factorial design varying complexity and favorable or biased cues, with suboptimal choice and harmful-bundle acceptance as primary outcomes.	Reported as Study 2. The article focuses on complexity, selective emphasis, their interaction, and the joint high-complexity/selective-emphasis contrast. The underlying payoff consequences remain fixed within the relevant comparisons.
Advisor or endorsement factor	The registration also includes endorsement or advice and hypotheses concerning its independent and interactive effects.	The article does not treat endorsement as part of its central theoretical contribution. Because the Study 2 experimental implementation includes the advisor factor, the main Study 2 specification accounts for that crossed factor and Appendix D.8 reports a robustness comparison with the no-advisor-only specification.
Active information acquisition	The registration includes a module in which respondents can request additional information and specifies outcomes concerning information demand.	This module is not part of the four-study sequence analyzed in the present article. The article instead uses clicks, time, difficulty, and confidence from the reported studies as secondary descriptions of task engagement.
Studies 3–4: political outcomes	The registration predicts that complexity and favorable presentation will increase acceptance of harmful political options and reduce recognition of harmful institutional features.	These predictions are examined in Studies 3 and 4 using candidate support, municipal-package acceptance, and recognition outcomes.
Cognitive heterogeneity	The registration specifies the higher-cognition indicator and predicts heterogeneous responses by cognitive performance. It also states that the sign of the joint complexity-by-cue effect among lower-cognition respondents is sufficiently uncertain to treat that comparison as exploratory.	The article retains the preregistered cognitive classification. Capacity comparisons are therefore treated as heterogeneity evidence rather than as the paper's primary confirmatory evidence, and the theory explicitly allows the ordering of vulnerability to attenuate or reverse when evaluation becomes sufficiently demanding.
Study 4 recognition	The registration describes recognition in the political modules as identifying an institutional-cost feature, with expanded negotiated-procurement discretion given as an example.	The article's main Study 4 recognition outcome is a broader consequence-recognition measure, coded as correctly identifying that at least two of three package-relevant domains worsen. The survey also contains the separate preregistered-style comprehension item concerning discretionary allocation; both measures and their coding are documented in Table 30.
Analytic exclusions	The registration specifies exclusion for lack of consent, failure of both basic CRT items, survey completion below 450 seconds, and duplicate or invalid participation identified through survey or panel checks.	The main analytic specification retains the consent, 450-second, and CRT rules and additionally excludes sessions longer than 6,000 seconds. The upper-duration threshold was not preregistered and is therefore treated as an analytic deviation. Potential duplicate flags are documented in the sample audit but are not used as a stand-alone exclusion in the main analytic rule.
Process outcomes	Difficulty, confidence, time, and clicks were preregistered as secondary or exploratory measures of engagement and task experience. The registration also contemplated standardized continuous outcomes and transformed time measures.	The article reports these outcomes as secondary process evidence and presents difficulty, confidence, time, and clicks in their original units for ease of interpretation. They are not treated as causal mediators.

The article also reorganizes the preregistered hypotheses to match the narrower theoretical argument developed here. Article H1 corresponds to the preregistered bundling benchmark. Article H2–H3 summarize the registered expectations concerning complexity, biased or favorable cues, and their joint role in bundle evaluation. Article H4 develops the preregistered cognition heterogeneity into a scope-conditioned prediction: lower-capacity

respondents should be especially vulnerable while they remain engaged, but the ordering need not persist when complexity becomes sufficiently severe. The political applications in Studies 3 and 4 correspond to the preregistered expectations concerning harmful-option acceptance and recognition in the municipal and candidate modules.

Several preregistered analyses are therefore not used as independent confirmatory tests in the present article. In particular, endorsement or advice and active information acquisition were part of the broader registered experiment but are not central to the article’s four-study argument. Where the advisor factor enters the data used for Study 2, Appendix D.8 shows that the focal complexity and selective-emphasis estimates are substantively similar when the analysis is restricted to no-advisor observations. Process outcomes and capacity subgroup analyses are likewise reported as secondary evidence rather than additional primary outcomes.

The most consequential sample-construction departure is the addition of the 6,000-second upper-duration cutoff. Because total duration is measured after treatment exposure, Appendices D.3–D.5 report the sample construction, treatment-selection diagnostics, and sensitivity of the focal estimates to alternative inclusion rules. The main text therefore does not describe the final analytic sample as a fully preregistered restricted sample.