

Accountability in the Technocratic State: Public Opinion and Regulatory Enforcement in Antitrust

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Abstract

Can citizens hold the technocratic state accountable when partisan cues offer little guidance? I study this question through antitrust enforcement, a domain governing trillions in economic activity yet rarely central to partisan debate. Using a survey experiment, I show citizens form systematic merger preferences from economically intuitive signals: deal size, industry overlap, and familiarity. Notably, revealing that a firm owns well-known brands increases support for intervention rather than generating leniency, an effect operating primarily through perceptions of acquiror corporate scale. A supplementary survey validates the inference: given nothing but the firms' identities, respondents use brand information to correctly classify whether the merging firms compete and to calibrate scrutiny to each deal's economics, easing it for unrelated pairs while preserving it for true competitors. Decisions aligned with respondent preferences increase confidence in the antitrust agencies and presidential approval; enforcement failures reduce both. Complementing the experimental evidence, I construct an original dataset linking Hart-Scott-Rodino reviews from 2001 to 2020 to firm-level financial and brand data, showing that agency enforcement tracks these same dimensions, familiarity included. The alignment does not operate in the dark: nearly two-thirds of respondents name a real merger unprompted, and knowledge of enforcement concentrates where visible firms and agency action intersect. Together, the results show that citizens use familiarity as information rather than sentiment; democratic accountability can reach technical domains where partisan cues offer little guidance, to the extent that government's choices are legible in the terms of everyday economic life.

KEYWORDS: democratic accountability; voter competence; antitrust; corporate consolidation; business & politics

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Introduction

Democratic accountability rests on a deceptively simple premise that voters can observe what government does and hold officials responsible for it. In practice, much of what government does happens far from the electoral arena, in regulatory agencies and administrative bodies staffed by technical experts and specialists making complex discretionary decisions that have major implications for voters' welfare. These institutions often justify their authority on the basis of expertise and neutrality rather than partisan contestation, insisting that public judgment is therefore beside the point (Wilson 2019; Carpenter and Krause 2015). Yet public opinion can be perceptive and revealing. For example, research shows that citizens are able to detect ideological shifts in the Supreme Court despite its claims of legal neutrality (Jessee et al. 2022). The problem is sharpest when decisions are highly technical, and partisan cues are weak or ambiguous. In these domains, the informational shortcuts that typically aid political judgment such as party labels, ideological signals, elite or interest group endorsement offer little guidance (e.g. Converse 2006; Lupia and McCubbins 1998; Lenz 2012; Achen and Bartels 2017). Whether citizens can evaluate government performance under these conditions is a foundational question for democratic theory that remains unresolved (Dahl 1961).

Mergers and acquisitions (M&A) are among the most consequential forces shaping the structure of modern economies, and how government responds to them has direct implications for democratic accountability. Every year, US companies engage in M&A worth over two trillion dollars in total.¹ These events reshape markets and, by extension, the everyday economic life of ordinary people. A large body of empirical research links rising consolidation to higher prices, reduced quality of goods and services, diminishing innovation, increasing monopsony power over workers, as well as increasing political influence (e.g. De Loecker et al. 2020; Stavins 2001; Cunningham et al. 2021; Autor et al. 2020; Callander et al. 2022). These are not abstract shifts: they register in the grocery bills, healthcare costs, and job options that citizens encounter repeatedly. When government acts or fails to act on corporate consolidation, such decisions send a signal about whose interests the state is willing to protect, with plausible consequences for how citizens evaluate government performance and the officials responsible for it.

However, the institutions charged with overseeing M&A, the Federal Trade Commission (FTC) and the Department of Justice's (DOJ) Antitrust Division, have operated at the margins of public attention since the 1970s. Antitrust has been treated as a bipartisan, technocratic domain governed by economic and legal expertise, with debates confined largely

¹Estimates are my calculations based on LSEG SDC Platinum records.

to specialists (e.g. Kovacic and Fox 2003; Crane 2007). Antitrust does carry a modest partisan tilt that Democrats are somewhat more supportive of enforcement than Republicans (Brutger and Pond 2025b), but that tilt reflects general dispositions toward government regulatory interferences more than judgments about actual merger reviews. Unlike healthcare or taxation, antitrust offers citizens few partisan anchors for evaluating whether a specific enforcement decision is warranted. It thus becomes a particularly revealing setting for a fundamental question in democratic governance: whether accountability can reach a technical domain where partisan cues offer little guidance.

In this paper, I examine this question through the lens of antitrust enforcement. Using a survey experiment, I study which features of merger transactions lead citizens to support government intervention, and how regulatory outcomes shape public confidence in enforcement agencies and elected officials. The merger profiles vary the features a citizen could actually observe, such as deal size, whether the merging parties compete in the same market, and how recognizable they are. Whether people can infer a deal’s likely consequences from these features is itself a central question. Complementing the experimental evidence, I construct an original dataset linking two decades of Hart-Scott-Rodino (HSR) premerger reviews² to financial and brand data to examine whether agency enforcement patterns track public intuitions. Together, the two approaches allow me to assess not only whether citizens reason about these decisions in economically meaningful ways, but whether those judgments connect to real regulatory behavior.

The survey experiment shows that citizens respond systematically to the economic features of a merger. Support for government intervention increases sharply with deal size and industry overlap between the merging parties, both standard indicators of anticompetitive risk. These preferences are not arbitrary reactions to dollar figures: much of the effect operates through respondents’ perceived price effects and scale; additionally in their open-ended explanations, through explicit reasoning about firm size, market structure, and competition. More strikingly, revealing that a firm owns well-known brands substantially increases support for blocking the transaction: rather than generating leniency toward familiar companies, brand recognition operates as an informative signal for corporate market power and scale. Familiarity also connects the experiment to the information environment outside it: deal size and industry overlap are facts a profile must supply, but brand recognition is knowledge

²Hart-Scott-Rodino Antitrust Improvements Act of 1976: <https://www.ftc.gov/legal-library/browse/statutes/hart-scott-rodino-antitrust-improvements-act-1976>. The main change this piece of legislation installed was mandating that, for certain transactions, the merging parties file with the FTC and DOJ and wait for the agencies to review before executing the transaction. This gives the government agencies a chance to intervene in a harmful corporate consolidation ex ante.

respondents carry with them from everyday economic life: a signal that exists inside and outside the survey.

A supplementary survey shows the shortcut is not only portable but accurate. Respondents who know a firm’s brands well have typically never heard of the firm’s legal name. Withholding or revealing the brands thus turns a household-name company into an anonymous-sounding one, with no deception involved. Revealing the brand information produces more accurate judgments on market overlap between merging parties. The same information also moves support for blocking toward the economically appropriate response for each pair, easing scrutiny of unrelated pairs while leaving scrutiny of true competitors in place.

These preferences carry direct accountability consequences. When agency decisions align with respondent preferences, confidence in the FTC and DOJ rises; enforcement failures — whether approving a harmful merger or losing a block attempt in court — produce significant declines. These patterns extend to presidential approval, suggesting antitrust enforcement decisions carry electoral implications beyond the agencies themselves and that voters are able to attribute agency decisions to the political principal responsible.

Public preferences are generally reflected in agency behavior. Utilizing the rules of the HSR process where parties to transactions meeting certain criteria need to file with the FTC and DOJ before consummating the merger, I build an original dataset linking two decades of HSR actions³ to financial and brand data. I find that the FTC and DOJ are more likely to challenge large, same-industry transactions, and firms that own recognizable brands attract greater scrutiny.⁴ The effects of brands are more prominent among large or same-industry transactions. This result is especially meaningful since block attempts are exceedingly rare, occurring in around 0.6-3.4% of eligible filings each year. One can reasonably assume that the agencies could have intervened more if allowed more time and resources (Bhattacharya et al. 2023), but in reality need to make limited picks even among the more risky mergers. While the experimental and observational approaches differ in design and information environment, their convergence on the same underlying dimensions suggests a shared logic governing how both citizens and agencies evaluate corporate consolidation.

³By taking action, I mean that FTC or DOJ either issued consent orders for public comment, or initiated administrative or federal court litigation. Throughout the paper, I use terms such as “action”, “flag” interchangeably. The universe of these actions is taken from FTC and DOJ cases and proceedings, available on their websites. Both agencies denied my FOIA requests on the grounds that transaction level information was meant to be proprietary and private. Scraping the agency websites and building the data myself is to my knowledge the best bet at getting a comprehensive, workable dataset.

⁴This is in line with Coate et al. (1990), who use internal, nonpublic FTC case files on 70 horizontal merger investigations from 1982–1987 to show that challenge decisions track the merger guidelines’ concentration criteria as well as political pressure, measured by congressional oversight and press coverage.

Nor does this alignment operate in the dark. Citizens’ knowledge of real enforcement, measured through recognition and unprompted recall of actual transactions, concentrates precisely where visible firms and agency action intersect. The accountability the alignment supports is therefore conditional on what reaches public attention.

This paper speaks to a growing body of work studying antitrust as a question of mass opinion. Brutger and Pond (2025a) and Brutger and Pond (2025b) show that support for antitrust as general policy moves with broad political arguments: framing enforcement as a handicap to American firms in international competition lowers support, fairness arguments raise it, and the price argument that has anchored federal enforcement since the 1970s does not make the public more in favor of government intervention. Short et al. (2025) bring the analysis down to hypothetical merger profiles: in a conjoint whose profiles state each deal’s consequences, the public weighs job losses and product quality degradation far more heavily than price savings, and antitrust lawyers weigh them very differently, a cleavage the authors read as ideological capture. Together, these studies establish that the public holds structured views about what antitrust should protect. They say less about whether citizens can evaluate enforcement decisions, because merger review is largely an *ex ante* problem. At the time a transaction takes place, its effects on prices, employment, and quality are not yet realized. The HSR process requires the agencies to evaluate them from the deal characteristics observable at filing — a task hard enough that economists dispute the effects of individual mergers even in retrospect (Peters 2006; Kwoka 2015; Vita and Osinski 2018). A design that stipulates outcomes identifies the public’s preferences when consequences are certain and knowable, but it cannot say whether the public can evaluate a merger as well as government actions related to it while its consequences are uncertain and not yet realized, which is when enforcement decisions are actually made. This capacity determines whether antitrust enforcement can be an object of public opinion at all, rather than of expert judgment alone.

This paper isolates the inferential step an actual enforcement decision requires: judging a deal from its observable features before its consequences are realized. The profiles put respondents in the informational position of a citizen reading about a proposed deal, and elicit rather than stipulate two beliefs that track the poles of the antitrust debate: the deal’s expected effect on prices — the consumer welfare standard’s central question, and the firm scale — a concern with corporate bigness that goes beyond the narrow scope of short-term price effects (Khan 2016; Wu 2018). The contribution is a chain that prior work leaves in pieces. Citizens in this technocratic domain are neither disengaged nor guessing: they draw on intuitive economic signals such as deal size and industry overlap as well as

an informative shortcut of familiarity to assess anticompetitive risk. When that shortcut is tested on firm pairs, the inferences it supports are largely correct. While the government agencies supposedly have more information on the transactions and firms, descriptively they rely on similar dimensions when deciding which mergers to intervene in. Familiarity is what makes this comparison informative: convergence on deal size and market overlap can be interpreted as intuitive, but familiarity is not obvious, yet it raises public and agency scrutiny alike, a pattern no policy framework prescribes and whose welfare implications I leave open.

The accountability documented here runs through an indirect circuit. The agencies face no election; but when citizens learn how an enforcement decision turned out, their confidence in the agencies moves, and so do their evaluations of the president who appointed the agency head. Enforcement itself falls along the dimensions the public watches: a congruence that carries the question of democratic responsiveness beyond the electoral arena. These findings arrive as antitrust resurfaces as a political issue, with legal scholars and policymakers calling for reforms⁵, and the unprecedented firing of the Democratic FTC commissioners in March 2025 signaling a growing partisan divide among political leaders⁶, even as enforcement itself has remained stable across administrations and the new salience has not yet hardened into partisan cues over specific enforcement decisions. The broader suggestion concerns the conditions under which democratic accountability can operate. Citizens evaluated this technical domain without partisan anchors, drawing on the economic legibility of the decisions themselves. The reach of accountability into the technocratic state may be limited less by voter capacity than by whether government’s choices are legible in the terms of everyday economic life and the relevant information environment. Whether that holds in other technical domains, and what degree of legibility it requires, are questions this paper opens rather than settles.

1 Public Opinion without Partisan Anchors

Decades of research in public opinion and voter competence suggest that forming meaningful policy preferences requires both knowledge and interest that average citizens often lack (Lupia 2016; Carpini and Keeter 1996; Campbell et al. 1980). Researchers argue that individuals rely on shortcuts based on ideological alignment to simplify such tasks: for instance,

⁵For example, see <https://www.americanbar.org/content/dam/aba/publications/antitrust/journal/84/3/finding-common-ground-among-antitrust-reformers.pdf>.

⁶See a summary of the timeline here: <https://www.epi.org/policywatch/firing-ftc-commissioners/>.

an imperfect proxy would be partisan-based alignment (Downs 1957; Sniderman et al. 1991; Converse 2006). Even then, voters might not hold ideologically coherent positions across issues, and their expressed preferences frequently shift with context or elite cues (Achen and Bartels 2017; Lenz 2012). In other words, voters might not have “ideology”, defined by Kinder and Kalmoe (2017) as an abstract system of consistent beliefs, but rather, voters have partisan identities that help sort policy views into left and right buckets or encourage them to adopt certain kinds of views (Leeper and Slothuus 2014). Such reliance on party cues could lead to biases even among the more attentive and educated voters (e.g. Dancey and Sheagley 2013). Previous studies have found that factors such as political knowledge, issue salience, and information environment — essentially either educating voters or calling voters’ attention to a specific issue — could moderate the effect of party cues (e.g. Popkin 1991; Gilens and Murakawa 2002; Slothuus and De Vreese 2010; Boudreau and MacKenzie 2014; Peterson 2017). Another strand of literature, on the other hand, argues that even with more information, sometimes voters do not overcome partisan cues (e.g. Rahn 1993; Kalla and Broockman 2018).

When it comes to economic policies, a different dimension of voter sophistication becomes relevant: apart from general political knowledge, voters might need a basic understanding of economic principles, and depending on the context, more specialized information. Prior research in political science is generally more concerned with changes in personal or national economic conditions as a way to judge the competence of political leaders (e.g. Kramer 1971). The vagueness and complexity of this question naturally invite noisy voter perceptions and responses, as Louis Uchitelle summarized (quoted in Achen and Bartels 2017): “[...] If the economy is flourishing in the final weeks of a campaign, when the music stops, the incumbent is likely to be re-elected.”⁷ Researchers have also studied how voters evaluate specific policies in trade, taxation, and welfare. These studies tend to explore how additional information or experience can shift voters’ support, and to what extent these shifts are more compatible with a rational agent’s self-interest. For example, Rho and Tomz (2017) show how most voters misunderstand consequences of protectionism and can update their preferences after they are exposed to information. Bartels (2005) demonstrates that voters fail to understand the implications of tax cuts and hence support tax cuts for the wealthy. Stantcheva (2021) finds that respondents primarily evaluate taxation policies through the lens of social preferences that are highly correlated to partisan identifications. Margalit (2013) shows that voters experiencing economic hardship are more supportive of welfare policies, though such effects are short-lived. Nall (2025) finds that the public skepticism on housing supply is unstable,

⁷Are Elections Too Much Like Musical Chairs? <https://archive.nytimes.com/campaignstops.blog.s.nytimes.com/2012/04/12/are-elections-too-much-like-musical-chairs/>

and often induced by personalized market interactions and naive beliefs about high housing prices.

Antitrust enforcement presents a connected yet different configuration. Individual enforcement decisions are highly technical and discretionary, yet partisan signals surrounding these decisions are often weak or ambiguous. In such settings, it is not obvious *ex ante* how citizens should process information or which aspects of these economic activities they would find most concerning. While the technical complexity of antitrust could limit meaningful evaluation, enforcement decisions also regulate a range of very visible market outcomes — prices, products, businesses — that citizens encounter repeatedly in everyday life. These interactions are high-frequency and relatively more accessible, compared to economic policies that shape citizens’ lives through episodic or infrequent financial decisions.

A recent literature has begun to measure public opinion on antitrust, and its findings are instructive. Brutger and Pond (2025a) and Brutger and Pond (2025b) study general support for antitrust policy. They find that fairness arguments that antitrust protects hard work and an equal chance to compete raise support, while the consumer price argument that has anchored decades of federal enforcement does not. Framing enforcement as a handicap to American firms in international competition lowers support. They observe a modest baseline partisan gap: Democrats are about eight percentage points more supportive of strengthening antitrust, but most arguments move respondents from both parties similarly. Short et al. (2025) shift the lens from policy to transactions. In a conjoint where respondents evaluate hypothetical mergers whose consequences are spelled out in the profile, the public weighs stipulated job losses and quality deterioration far more heavily than price savings, and antitrust lawyers evaluate the same profiles very differently.

Read side by side, these findings are puzzling: the public seems deaf to the consideration of price effects around which enforcement is organized. The two designs also share a feature that limits what they can say about enforcement itself: both tell respondents what to weigh. Brutger and Pond (2025a) and Brutger and Pond (2025b) focus on framings around general policy outcomes. Short et al. (2025) hand the respondents definitive consequences. In both, the hard part of a real enforcement decision is resolved by the certainty of implications. The difficulty is not merely that a merger’s consequences are generally unknown when it is announced; they are often never established with any consensus: the counterfactual world in which a deal was blocked, or cleared, is not observed, and economists dispute what individual mergers actually did even decades after the fact (Peters 2006; Kwoka 2015; Vita and Osinski 2018). A profile that stipulates a merger’s effects therefore identifies what the public values under certainty, a condition enforcement hardly supplies. A public deaf to “antitrust lowers

prices” as a slogan might nonetheless, when confronted with a specific deal, form its own expectations about what the deal would do to prices. And observable features that do little work when consequences are stipulated — hence there is nothing left to infer — might become the entire basis of judgment when consequences are uncertain and contested, which is the situation almost every actual merger review presents. Whether citizens can make that evaluation is the question this paper seeks to answer.

Moreover, weak partisan cues leave open fundamental questions about accountability. Without clear party signals, it is unclear how citizens evaluate regulatory outcomes, whether they distinguish between policy disagreement and enforcement failure, or how they assign responsibility to agencies and elected officials.

In this context, citizens might turn to alternative anchors for evaluating corporate power. One natural candidate is familiarity — the recognizability of firms and brands through everyday experience in the economy that can serve as an intuitive shortcut for corporate scale and market presence. Familiarity is theoretically ambiguous *ex ante*: brand recognition could heighten concern about corporate power, or it could generate leniency through the positive associations that repeated exposure tends to produce. Its direction, moreover, is not the only open question. A shortcut is only as good as the inference it licenses, and whether recognition tracks anything real about a firm’s scale and market position is itself an empirical matter.

2 The Politics of Antitrust

Antitrust as a policy domain originates from a prominent, divisive political contest between capital and labor. The “trust” in antitrust refers to corporate trusts such as the Standard Oil Trust that could dominate an entire industry, control prices, and bend markets to their will. Trusts were or aspired to be monopolies: by concentrating power in the hands of a capable few, they claimed that they could save the US economy from ruinous competition and make production more efficient and innovation more rapid than ever.

The public and policymakers, however, did not agree. The Trust Movement was accompanied by a growing dissatisfied middle class and an explosion in economic inequality. By the early 1900s, antitrust had become a prominent political issue, championed by the “Trustbusters”. Presidents such as Theodore Roosevelt, William Taft, and Woodrow Wilson campaigned on breaking up monopolies, framing concentrated corporate power as a political and moral threat to democracy itself. From the groundbreaking Northern Securities case to the Federal Trade Commission Act that created the FTC, Trustbusters once stood at the center of

American politics. Antitrust enforcement during this time was often framed as part of a broader struggle over economic equality and the proper limits of private corporations. Louis Brandeis, who later became Woodrow Wilson’s advisor and was then nominated to the Supreme Court, articulated a moral and democratic rationale for limiting corporate scale, warning that “the curse of bigness” threatened not only competition but also the freedom and liberty of every citizen.⁸

Over the course of the twentieth century, antitrust gradually shifted from a popular cause to a domain of expert judgment. Beginning in the 1960s, law and economics scholars, many of whom were associated with the University of Chicago, recast competition policy as a question of efficiency, and a narrowly defined issue of consumer welfare, centered primarily on prices (e.g. Stigler 1983; Bork 1978; Posner 1976). This framework prioritized measurable economic outcomes and sought to make enforcement more rigorous and predictable, but in doing so, it de-emphasized the political dimensions. By the early 2000s, antitrust had largely faded from the public agenda, treated as a specialized policy domain rather than a political battleground. Enforcement decisions came to rely heavily on economic analysis that few outside the expert community could meaningfully engage with. Yet as corporate consolidation accelerated and a handful of firms came to dominate technology, media, and healthcare, the same questions once thought to be settled have returned: what makes a merger more harmful than others, how much concentration is too much, and who gets to decide.

In recent years, antitrust began to move toward the political center stage again. Wu (2018) draws the comparison between the contemporary times and the early 1900s in terms of high level of corporate consolidation, increasing economic inequality, and lack of proper government oversight in the private sector. Such views are echoed by Joe Biden’s FTC Chair Lina Khan, who has criticized the oversimplification of analyzing merger outcomes in the framework of consumer welfare and myopic price effects (Khan 2016).

Aggregate records on the Hart–Scott–Rodino (HSR) merger review process since 2001 indicate that antitrust enforcement occurs at consistently low rates across administrations.⁹ They point to a relatively stable enforcement environment, echoing the former FTC commissioner Thomas B. Leary: “*There really is no such thing as a ‘Republican’ or a ‘Democratic’ antitrust agenda today. People may have different views on the facts of individual cases for a variety of reasons, but there is a broad mainstream consensus on the basic approach*

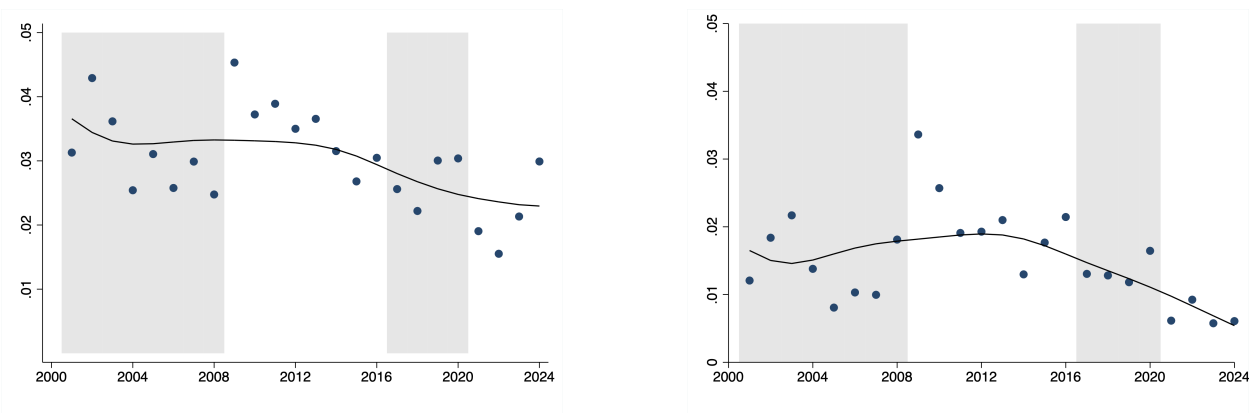
⁸See summary of Brandeis’s argument in articles such as <https://orionmagazine.org/article/the-curse-of-bigness/>.

⁹For institutional details on the HSR process, see Appendix A.

to antitrust issues.”¹⁰ Each year, the FTC and DOJ issue Second Requests (a request for further information before merging parties could consummate the deal) for around 1.5% to 4.5% of eligible HSR filings, and attempt to block around 0.6% to 3.4%.¹¹ Figure 1 shows that although enforcement intensity fluctuates over time — reflecting economic conditions, composition of cases received, agency capacity — there are no sharp or persistent shifts in enforcement rates associated with changes in presidential administration. Figures 2 and 3 show the breakdown by agency.

This pattern complicates the common portrayal of antitrust enforcement as a straightforward partisan problem, with Democratic administrations presumed to be more interventionist and Republican administrations more permissive. For instance, despite widespread expectations of more aggressive enforcement under the Biden administration, the aggregate rate of Second Requests and block attempts does not display a pronounced increase. Instead, recent years show a modest decline, prolonging the downward trend since the mid-2010s. Statements from FTC, along with a higher share of transactions abandoned or restructured before agencies formally challenge the merger proposals — see Figure OA8 in Appendix C — could suggest a change in tactics, though the descriptive pattern alone is not conclusive enough to determine agency efficiency.¹²

Figure 1: Overall Rate of Second Requests (left) and Block Attempts (right) by FTC and DOJ, Fiscal Years 2001-2024



Notes: Graph areas shaded by partisanship of the sitting president.

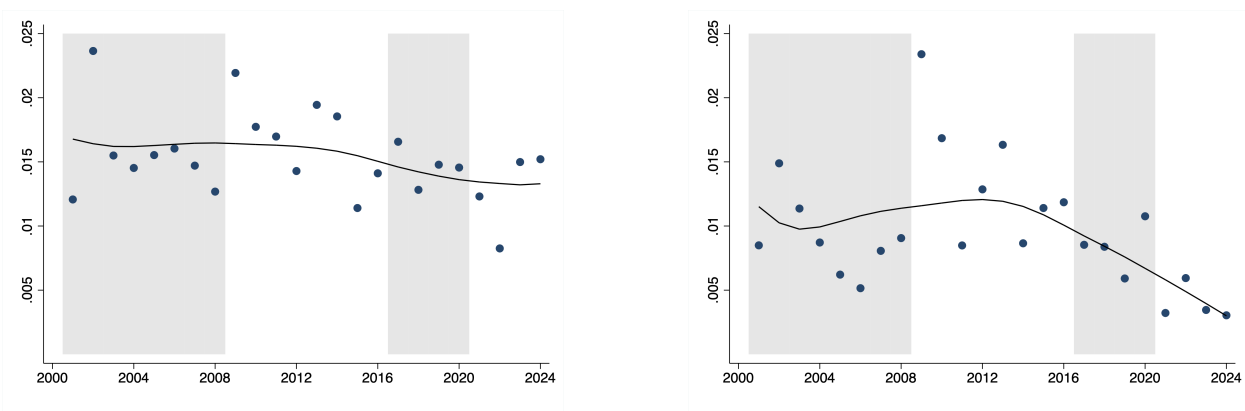
The lack of partisanship in enforcement decisions could also be by design. The FTC Commissioner Board, notably, is set up in a way that encourages partisan balance: there are

¹⁰<https://www.tulanelawreview.org/pub/volume80/issue2/the-bipartisan-legacy>.

¹¹Specifically, attempts to block here refer to cases where the agencies issue consent orders for public comment, as well as cases where the agencies initiate administrative or federal court litigation.

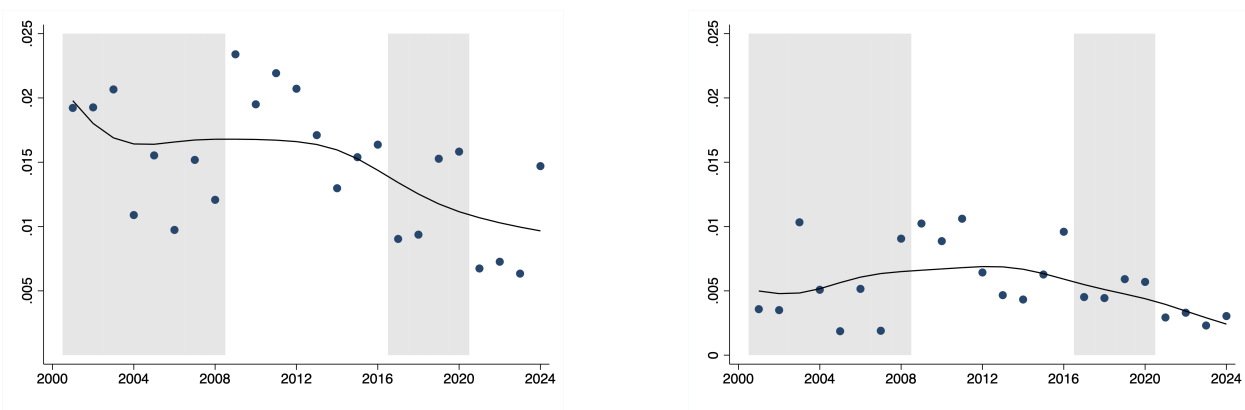
¹²The FTC attributes these abandonments to its enforcement actions, but this claim cannot be independently verified. Merging parties can abandon transactions for reasons unrelated to regulatory pressure, such as changing market conditions or financing constraints.

Figure 2: Rate of Second Requests (left) and Block Attempts (right) by FTC, Fiscal Years 2001-2024



Notes: Graph areas shaded by partisanship of the sitting president.

Figure 3: Rate of Second Requests (left) and Block Attempts (right) by DOJ, Fiscal Years 2001-2024



Notes: Graph areas shaded by partisanship of the sitting president.

in total five Commissioners, among whom at most three could be from the same party. This essentially ensures that the partisan split is at most 3-2. Each commissioner is nominated by the President and confirmed by the Senate, and serves a staggered seven-year term, which helps insulate the agency from short-term political pressures and presidential turnover. There have also been cases where a president echoes his predecessor’s nomination, further suggesting independence from partisan labels in antitrust matters: some examples are Jon Leibowitz, first nominated by GW Bush, later by Obama; Rebecca Kelly Slaughter, first nominated by Trump during his first presidency, later by Biden¹³.

3 Citizen Preferences over M&A Enforcement

The recent revival of antitrust as a political issue has largely been driven by policymakers and scholars, and this paper seeks to understand how ordinary citizens perceive corporate consolidation and the regulation of M&A. After all, unlike many economic policies that operate in the background, consolidation shapes the prices people pay, the choices they see on shelves, and the size and prominence of the firms they encounter in daily life. These everyday experiences provide citizens with intuitive reference points. Even though antitrust has not been a core issue for average voters in contemporary times, voters certainly care about economic consequences.

I conduct an original survey experiment to examine public opinion on M&A. The survey seeks to understand what kind of corporate consolidation an average citizen considers more relevant or harmful, and whether and under what conditions government interventions are deemed necessary. The survey also includes questions that measure respondents’ general economic well-being.

The survey is administered on Prolific from October 15, 2025 to November 13, 2025.¹⁴ I use quota sampling to construct a representative US population sample in the following dimensions: geographical region, gender, race, age, political party identification, and education level.¹⁵ The final survey sample has 1,969 respondents. See Table OA1 in Appendix C for descriptive statistics of the sample.

¹³Though Slaughter was fired by Trump during his second presidency in March of 2025, the back and forth, if anything, shows that the individual’s political identification as a label rather than her actual handling of antitrust matters is the “partisan” issue here. Courts are still deciding whether Trump’s firing is illegal.

¹⁴Prior to administering the survey, I filed a pre-analysis plan; the survey is approved by the Stanford University IRB Board.

¹⁵Because the survey is quota sampled to match the general U.S. population on these demographic characteristics, we do not apply post-stratification weights to these data.

3.1 Main Experiment

The general economic logic of antitrust is to spot and avoid increases in market concentration. Higher market concentration is associated with firms’ ability to raise prices over the competitively possible threshold (i.e. market power). Traditionally, M&A within the same market (i.e. horizontal mergers) are considered more problematic because merging directly reduces the number of players and increases the relative share of the new firm in that industry.¹⁶ M&A across different markets (i.e. vertical mergers) started attracting more attention from the agencies in recent years, culminating in the first standalone Vertical Merger Guidelines issued in 2020. Though not definitive, the current standards of the agency doctrines would predict that large transactions within the same industry are more likely to be harmful.

In the main experiment, respondents are shown merger profiles and asked whether the merger in question should be stopped by government agencies. Each respondent gets six merger profiles, each with information on the acquiror firm, target firm relative size, target firm industry, and transaction size. The acquiror firm contexts are fixed, each with two versions and assigned randomly. The other attribute values are randomly assigned. In other words, every respondent gets all six acquiror firm profiles that are randomized in order.

In addition to the more intuitive economic factors, I vary how familiar certain mergers would appear to voters by showing merger profiles where the main firm (the acquiror) is presented as either the “branded” or “not branded” version of the same company. This experimental design signals how familiar the firm and its relevant market would appear to respondents. The contrast mirrors a measured asymmetry in what people actually know: in an independently-run, supplementary survey introduced with the results below, respondents rate familiarity with companies and brands on a four-point scale, from having never heard of the name (1) to being very familiar with it (4). Under their legal names, the same class of firms averages 1.6, between never having heard of the name and having merely heard of it; the brands those firms own average 3.3, between somewhat and very familiar, see detailed results in Appendix D. Whether a description of a company mentions its brands is close to the difference between naming a company people know and naming one they have never heard of. Conceptually, familiarity could cut in either direction. On one hand, recognizable brands can signal scale and market power, leading respondents to favor greater scrutiny. On the other hand, familiarity can generate leniency: people often hold positive associations with well-known brands, and prior work in consumer psychology shows that brand exposure and loyalty can foster positive affect (e.g. Zajonc 1968; Ahluwalia et al. 2000). This design

¹⁶See for example, the 2015 version of Horizontal Merger Guidelines, published by the FTC and DOJ: <https://www.justice.gov/atr/horizontal-merger-guidelines-0>.

does not attempt to adjudicate whether respondents are rational, but it does allow us to test which of these competing intuitions dominates when people evaluate mergers.

3.1.1 Profile Attributes

Example profile: *Suppose that Newell Brands Inc, a consumer product company that owns brands such as Rubbermaid, Yankee Candles, and Sharpie, is trying to merge with a similar-sized company in the same industry. The merger is valued at 500 million dollars.*

Acquiror Firm: one out of the two versions is randomly assigned to each profile. Every respondent gets all six profiles. The main difference between the two versions is whether information on relatively more well-known brands that the acquiror firm owns is revealed to the respondents. I consider the branding information as a loose instrument for how close or familiar the acquiror firm appears to an average person. See Table 1 for the attribute values.

Deal Size: categorical variable with values “50 million dollars”, “100 million dollars”, “200 million dollars”, “500 million dollars”, “over a billion dollars”.

Other Firm Industry: categorical variable with values “a different industry”, “the same industry”.

Other Firm Size: categorical variable with values “bigger”, “similar-sized”, “smaller”.

Outcome Variables: there are three outcome variables per profile, and an additional outcome variable per respondent.

Perception of price increase: Do you think this proposed deal would increase or decrease the prices of [products]¹⁷? 5-point scale from “Significantly increase prices” to “Significantly decrease prices”.

Perception of bigness: How big of a company do you think [acquiror] is? 5-point scale from “Very small” to “Very large”.

Preference on government action: Do you think the US government should approve or stop this deal? 5-point scale from “Definitely approve” to “Definitely stop”.

Ranked choice: Please rank all 6 deals from most important to investigate further to least important to investigate further by dragging them into order.

The perception of price increase and perception of bigness questions are asked right after the

¹⁷The exact wording is adjusted by profile, for instance, profiles with Mondelez would say “snack food”. For exact wording, see the accompanying document that shows the full questionnaire.

Table 1: Attribute Values for the Acquiror Firm

Acquiror Firm	Not Branded	Branded
Mondelez	Mondelez International, a snack food company	Mondelez International, a snack food company that owns brands such as Oreo, Ritz, and Trident
Haleon	Haleon PLC, a pharmaceutical company	Haleon PLC, a pharmaceutical company that owns brands such as Advil, Sensodyne, and Emergen-C
Newell	Newell Brands Inc., a consumer product company	Newell Brands Inc., a consumer product company that owns brands such as Rubbermaid, Yankee Candles, and Sharpie
Kenvue	Kenvue Inc., a consumer health and beauty product company	Kenvue Inc., a consumer health and beauty product company that owns brands such as Band-Aid, Tylenol, and Neutrogena
Early Warning Services	Early Warning Services, a digital payment service company	Early Warning Services, a digital payment service company that owns and operates Zelle
Alphabet	Alphabet, a technology company	Alphabet, a technology company that owns services such as Google and YouTube

respondent has seen each of the profiles, in a random order, then followed by the preference on government action question. After the respondents have gone through all the individual profiles, I bring back all six profiles again and ask them to rank the profiles from “the most important to investigate” to “the least important to investigate”.

3.2 Other Experiments

The other important aspect of voter responsiveness concerns how citizens update their support for government institutions and political representatives following policy actions. To examine this, I implement a second set of experiments. After the main experiment where respondents answer questions about the merger profiles, one of the profiles is randomly picked.

A government action (either approve the merger or stop the merger) is randomly assigned at the respondent level to this profile. Respondents are then asked for their confidence in the government agencies, as well as likelihood to re-elect a president who appointed the agency leaders. Lastly, a treatment of enforcement failure is randomly assigned to half of the respondents, followed by questions asking about their confidence in the government agencies, as well as likelihood to re-elect a president who appointed these agency leaders. To make the failures believable and knowable, the wordings differ by the randomly assigned agency action treatment. See below for details:

- Agency action = stop: The Federal Trade Commission (FTC) and the Department of Justice (DOJ) looked at this transaction involving [acquiror], where you responded [respondent preference for this merger profile], when asked what the government should do. The FTC and DOJ decided to **stop** the merger.
 1. **Control:** Suppose that because the FTC and DOJ stopped this merger, the two firms did not complete the transaction.
 2. **Failure:** Suppose that after the FTC and DOJ decided to stop this merger, the two firms filed a lawsuit in federal court challenging this decision. The court sided with the firms, allowing them to ultimately merge.
- Agency action = approve: The Federal Trade Commission (FTC) and the Department of Justice (DOJ) looked at this transaction involving [acquiror], where you responded [respondent preference for this merger profile], when asked what the government should do. The FTC and DOJ decided to **approve** the merger.
 1. **Control:** Suppose that after the FTC and DOJ approved this merger, the two firms completed the transaction, and prices of their products did not go up.
 2. **Failure:** Suppose that after the FTC and DOJ approved this merger, the two firms completed the transaction, and later raised prices of their products.

This design allows me to assess not only whether voters reward or punish agencies for actions that align with their own preferences, but also whether they distinguish between mere disagreement and actual regulatory failure.

Outcome Variables: there are two sets of two outcome variables — in total four — per respondent.

Confidence in government agencies: Does this action affect your confidence in the agencies? 5-point scale from “Significantly increase my confidence in FTC/DOJ” to “Significantly decrease my confidence in FTC/DOJ”.

Electoral outcome: Does FTC/DOJ’s decision mentioned above make you more or less likely to support re-electing the president who appointed those agency leaders?¹⁸ 5-point scale from “Much more likely” to “Much less likely”.

3.3 Other Survey Measures

Prior to any experiments, I draw a baseline for how citizens view the economic conditions they are in. This is an area where prior research has found large partisan gaps (e.g. Gerber and Huber 2009). My results are consistent with previous research that respondents exhibit a pattern of partisan cheerleading where Republicans have a more positive view on the nation’s economy than Democrats and Independents, see Figures OA9 and OA10 in Appendix C. Such partisan divides are not as prominent when it comes to evaluating one’s personal economic well-being, see Figure OA11 in Appendix C.

I also measure their baseline attitudes toward M&A by asking them how favorably they think of M&A activities, and whether they think M&A activities should be monitored by the government.

Favorability: In general do you think of firms’ mergers and acquisitions (M&A) favorably or unfavorably? 5-point scale from “Very favorably” to “Very unfavorably”.

Gov Involvement: In general, do you think firms’ M&A activities should be left to the market and firms, or closely monitored by the government? 3-point scale: 1 = “Left to the market and firms entirely”, 2 = “Left to the market and firms mostly but monitored by the government”, 3 = “Monitored by the government closely”.

I document a large partisan divide in general favorability, where Republicans are much more likely to hold a favorable view on firms merging with each other, see Figures OA5 and OA6. A majority of the respondents, across the partisan aisle, would prefer some level of government supervision of mergers and acquisitions.

After the experiments, I measure the immutable traits of respondents, including age, gender, race and ethnicity, education, income, geographical location, financial literacy, and financial

¹⁸To avoid/lessen association to current sitting President, I open this question with the following prompt: “In the US, presidents play a direct role in shaping the leadership of FTC and DOJ, by nominating their top officials (e.g., commissioners and assistant attorney generals), subject to Senate confirmation.”

asset mix.

3.4 Results

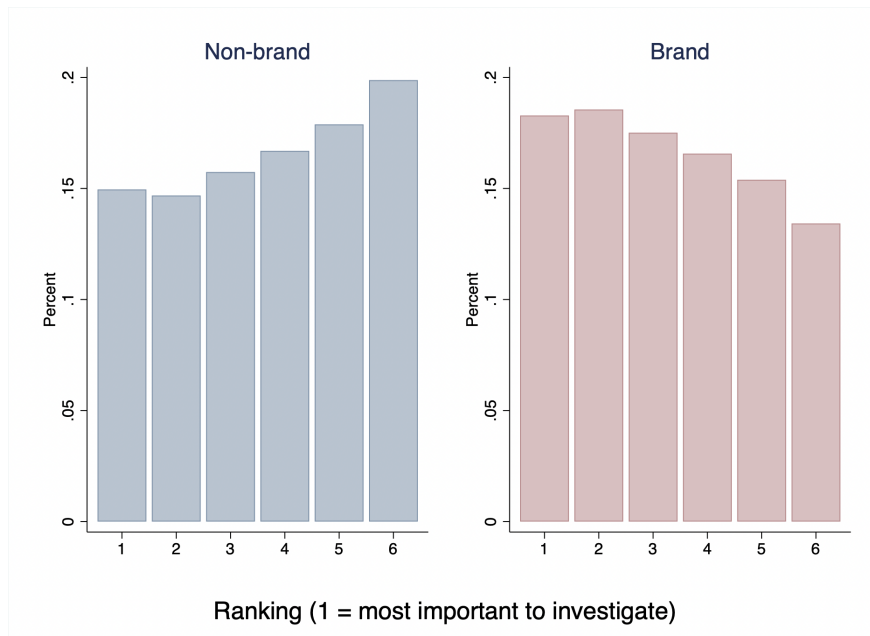
The survey results show that respondents want more government intervention when transactions are large, occur within the same industry, or involve well-known brands. These characteristics increase respondents' perception of both market power and the sheer bigness of the firm involved. I also find that respondents react to government taking an action that is in congruence or not in congruence with what they preferred. Unsurprisingly, respondents reward congruent agency behaviors and punish incongruent ones. However, there is a baseline positive association between the agencies stopping a merger and higher confidence in government as well as higher likelihood to re-elect a president. Finally, respondents decrease their confidence in government and likelihood to re-elect a president when they learn about an enforcement failure.

3.4.1 Evaluation of Mergers

Table 2 shows how observable merger characteristics shape respondents' preferences for government intervention. The dependent variable is a 5-point scale from 0 = definitely approve to 1 = definitely stop and captures the degree of support for the government blocking a proposed merger. The patterns are consistent across specifications that employ various combinations of fixed effects or controls. The scale can be loosely interpreted as from 0 to 100 percent, how much the respondent would like the government to stop the said merger. Transaction size matters substantially: compared with a \$50 million deal, respondents are 13–15 percentage points more likely to favor stopping a merger valued at over \$1 billion. Compared to the benchmark merger profile, respondents roughly want the same level of government intervention between a horizontal \$50 million merger and a vertical \$200 million merger. Substantively, I find that branded mergers are consistently associated with greater support for government intervention. Revealing brand information increases respondents' support for government putting a definitive block on the proposed merger by around 3.5 percentage points. This effect size is non-trivial. For example, the effect of simply revealing the brands is comparable to the effect of doubling the transaction size from \$50 to \$100 million: respondents would want the same level of government intervention between a \$50 million merger with revealed brands and a \$100 million merger if they don't know about the brands. Even when having a second look at all the profiles, respondents are more likely to rank the branded mergers as more important for the government to investigate. See Figure

4. Imposing a ranked choice naturally asks respondents to differentiate between levels of priority.

Figure 4: Ranked Choice as Outcome: distribution of rankings (1 = most important to investigate)



I also discuss possible channels through which ordinary citizens evaluate M&A. Deciding whether a merger is potentially harmful is a complicated economic decision even among trained experts and professionals. It's not uncommon to find champions on both sides of approve versus stop for a given merger. I explore two possibilities. The first is about market power: I ask respondents how they think prices of the relevant products would change if the merger in question goes through. This question is a close proxy for asking about market power while avoiding the economic jargon. The second is about the sheer bigness: I ask respondents how big they think the acquiror firm is. This measure attempts to capture respondents' perceived harm of concentration that is beyond the imminent price increases. I report the results in Tables 3 and 4.

Table 3 shows respondents' expectations of post-merger price changes, where the dependent variable is a 5-point scale from 0 = "significantly decrease prices" to 1 = "significantly increase prices". Respondents expect to see price increases when the merger is large in transactional value, and happens within the same industry between sizable firms. Mergers involving branded firms lead respondents to expect higher prices, net of other deal characteristics, though the effect size is moderate compared to other attributes. Table 4 shifts the focus to respondents' broader attitudes toward bigness. Here, the dependent variable is a

Table 2: Respondents want branded, larger, horizontal mergers to be prosecuted more by the government

	Gov should... (0 = def approve, 1 = def stop)					
	(1)	(2)	(3)	(4)	(5)	(6)
Branded	0.034*** (0.005)	0.033*** (0.004)	0.033*** (0.004)	0.033*** (0.004)	0.035*** (0.005)	0.033*** (0.005)
Deal Size: 100 million	0.029*** (0.007)	0.036*** (0.007)	0.036*** (0.007)	0.036*** (0.007)	0.028*** (0.007)	0.032*** (0.007)
Deal Size: 200 million	0.054*** (0.008)	0.064*** (0.007)	0.062*** (0.007)	0.064*** (0.007)	0.053*** (0.007)	0.057*** (0.007)
Deal Size: 500 million	0.081*** (0.008)	0.096*** (0.007)	0.096*** (0.007)	0.096*** (0.007)	0.081*** (0.008)	0.084*** (0.007)
Deal Size: over a billion	0.126*** (0.008)	0.154*** (0.007)	0.154*** (0.007)	0.154*** (0.007)	0.128*** (0.008)	0.132*** (0.008)
Same Industry	0.054*** (0.005)	0.058*** (0.004)	0.058*** (0.004)	0.058*** (0.004)	0.053*** (0.005)	0.054*** (0.005)
Other Firm Size: similar_sized	0.028*** (0.006)	0.033*** (0.005)	0.032*** (0.005)	0.033*** (0.005)	0.027*** (0.006)	0.028*** (0.006)
Other Firm Size: bigger	0.045*** (0.006)	0.050*** (0.005)	0.050*** (0.005)	0.050*** (0.005)	0.046*** (0.006)	0.048*** (0.006)
Favorability towards M&A						0.318*** (0.017)
Prefer Gov Oversight						0.086*** (0.012)
Respondent FE		Yes	Yes	Yes		
Profile FE			Yes			
Profile Order FE				Yes		
Demographic Controls					Yes	Yes
M&A Baseline View Controls						Yes
R-square	0.043	0.496	0.514	0.500	0.080	0.174
Observations	11814	11814	11814	11814	11808	11808

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: SE clustered by respondent. Six profiles per respondent. Demographic controls include age, gender, race and ethnicity, education level, income group, political party identification. “Favorability towards M&A” is a 5-point scale where 0 = respondent thinks of M&A very favorably in general, and 1 = respondent thinks of M&A very unfavorably in general. “Prefer Gov Oversight” is a 3-point scale where 0 = respondent thinks M&A activities should be left to the market and firms, and 1 = respondent thinks M&A activities should be closely monitored by the government.

Table 3: Respondents expect large horizontal transactions between sizable firms to increase prices

	Expected Changes in Prices (0 = decrease significantly, 1 = increase significantly)					
	(1)	(2)	(3)	(4)	(5)	(6)
Branded	0.008** (0.004)	0.010*** (0.003)	0.010*** (0.003)	0.010*** (0.003)	0.008** (0.004)	0.007* (0.004)
Deal Size: 100 million	0.018*** (0.006)	0.021*** (0.005)	0.021*** (0.005)	0.021*** (0.005)	0.018*** (0.006)	0.020*** (0.006)
Deal Size: 200 million	0.033*** (0.006)	0.039*** (0.005)	0.039*** (0.005)	0.039*** (0.005)	0.033*** (0.006)	0.034*** (0.006)
Deal Size: 500 million	0.045*** (0.006)	0.050*** (0.005)	0.049*** (0.005)	0.050*** (0.005)	0.044*** (0.006)	0.046*** (0.006)
Deal Size: over a billion	0.068*** (0.006)	0.080*** (0.005)	0.080*** (0.005)	0.080*** (0.005)	0.069*** (0.006)	0.070*** (0.006)
Same Industry	0.035*** (0.004)	0.037*** (0.003)	0.037*** (0.003)	0.037*** (0.003)	0.035*** (0.004)	0.036*** (0.004)
Other Firm Size: similar_sized	0.014*** (0.005)	0.015*** (0.004)	0.015*** (0.004)	0.015*** (0.004)	0.013*** (0.005)	0.014*** (0.005)
Other Firm Size: bigger	0.025*** (0.005)	0.030*** (0.004)	0.030*** (0.004)	0.030*** (0.004)	0.025*** (0.005)	0.026*** (0.005)
Favorability towards M&A						0.146*** (0.014)
Prefer Gov Oversight						0.040*** (0.011)
Respondent FE		Yes	Yes	Yes		
Profile FE			Yes			
Profile Order FE				Yes		
Demographic Controls					Yes	Yes
M&A Baseline View Controls						Yes
R-square	0.022	0.468	0.474	0.468	0.037	0.071
Observations	11812	11812	11812	11812	11806	11806

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: SE clustered by respondent. Six profiles per respondent. Demographic controls include age, gender, race and ethnicity, education level, income group, political party identification. “Favorability towards M&A” is a 5-point scale where 0 = respondent thinks of M&A very favorably in general, and 1 = respondent thinks of M&A very unfavorably in general. “Prefer Gov Oversight” is a 3-point scale where 0 = respondent thinks M&A activities should be left to the market and firms, and 1 = respondent thinks M&A activities should be closely monitored by the government.

Table 4: Respondents think large branded mergers involve very large companies

	Perceived Bigness (0 = very small, 1 = very large)					
	(1)	(2)	(3)	(4)	(5)	(6)
Branded	0.119*** (0.004)	0.115*** (0.004)	0.115*** (0.004)	0.115*** (0.004)	0.117*** (0.004)	0.117*** (0.004)
Deal Size: 100 million	0.067*** (0.007)	0.066*** (0.006)	0.065*** (0.006)	0.067*** (0.006)	0.066*** (0.007)	0.066*** (0.007)
Deal Size: 200 million	0.094*** (0.007)	0.100*** (0.006)	0.099*** (0.006)	0.100*** (0.006)	0.095*** (0.007)	0.095*** (0.007)
Deal Size: 500 million	0.144*** (0.007)	0.162*** (0.006)	0.161*** (0.006)	0.162*** (0.006)	0.143*** (0.007)	0.144*** (0.007)
Deal Size: over a billion	0.214*** (0.007)	0.238*** (0.006)	0.237*** (0.006)	0.238*** (0.006)	0.215*** (0.007)	0.215*** (0.007)
Same Industry	-0.005 (0.004)	-0.001 (0.004)	-0.000 (0.004)	-0.002 (0.004)	-0.005 (0.004)	-0.005 (0.004)
Other Firm Size: similar_sized	-0.013*** (0.005)	-0.011** (0.005)	-0.010** (0.005)	-0.011** (0.005)	-0.013*** (0.005)	-0.013*** (0.005)
Other Firm Size: bigger	-0.048*** (0.005)	-0.042*** (0.005)	-0.040*** (0.005)	-0.042*** (0.005)	-0.047*** (0.005)	-0.047*** (0.005)
Favorability towards M&A						0.020 (0.013)
Prefer Gov Oversight						0.032*** (0.011)
Respondent FE		Yes	Yes	Yes		
Profile FE			Yes			
Profile Order FE				Yes		
Demographic Controls					Yes	Yes
M&A Baseline View Controls						Yes
R-square	0.156	0.463	0.512	0.464	0.164	0.166
Observations	11814	11814	11814	11814	11808	11808

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: SE clustered by respondent. Six profiles per respondent. Demographic controls include age, gender, race and ethnicity, education level, income group, political party identification. “Favorability towards M&A” is a 5-point scale where 0 = respondent thinks of M&A very favorably in general, and 1 = respondent thinks of M&A very unfavorably in general. “Prefer Gov Oversight” is a 3-point scale where 0 = respondent thinks M&A activities should be left to the market and firms, and 1 = respondent thinks M&A activities should be closely monitored by the government.

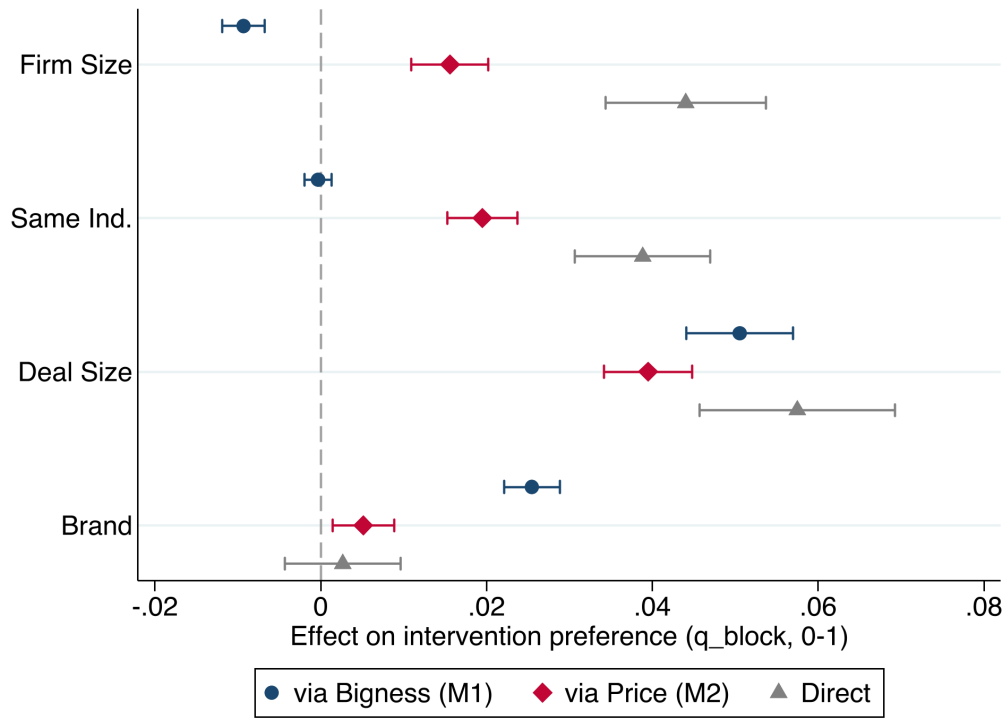
5-point scale from 0 = “very small” to 1 = “very large”. Respondents perceive firms with brands that attempt to acquire smaller firms with large transactional values to be much larger. The effect of revealing brands is remarkable: it is larger than the effect of quadrupling the deal size from \$50 million to \$200 million; its effect size is over half of the effect of increasing deal size from \$50 million to over a billion dollars. In other words, simply priming on how familiar a firm appears through brands produces a shift in perceived bigness comparable to increasing the deal’s monetary value by an order of magnitude. In both outcomes, respondents are quite reasonable. For instance, a horizontal merger directly increases the concentration within the industry, and respondents associate it with an increase in market power; however, large firms can make deals within or across industries so respondents don’t necessarily think horizontal mergers signal bigness.

Taken together, the results suggest that respondents think about both market power and bigness beyond price increases when evaluating firms’ M&A activities. The public interprets mergers through a reasonable economic logic — large transactions that combine sizable firms within the same industry predict a greater likelihood of price increases in a relevant product space. The public is also concerned about the sheer bigness. While economists, legal scholars, and corporate executives may argue that even as firms expand, certain mergers lack the intent or capacity (at least in the short run) to eliminate competition, raise prices, or monopolize an industry, the public favors a more precautionary approach. Corporate scale itself is a potential source of risk, and the public might not want to rely on the benevolence of large firms. My results also show that respondents use familiarity as a cue to corporate scale and market power. When the acquiror firm is presented with its well-recognized brands, respondents perceive the merger as posing a greater threat: they expect higher post-merger prices and view the firm as substantially larger in scale. These patterns indicate that branding activates intuitions about corporate reach and market power more than brand loyalty and other positive associations. Rather than generating leniency, familiarity heightens perceived risk.

So far I have treated perceived prices and bigness as outcomes in their own right. A final question is whether they account for the support-for-intervention results in Table 2: do deal features raise support for government action because they shift these perceptions, and do different features work through different perceptions? I decompose each feature’s total effect on support for intervention into the share mediated by expected price increases, the share mediated by perceived bigness, and a residual direct effect, estimating the decomposition with respondent fixed effects and bootstrapping confidence intervals by respondent.¹⁹

¹⁹Because the merger attributes are randomized, the feature-to-perception and total effects are cleanly

Figure 5: Channel decomposition of feature effects on support for intervention



Notes: Each feature's total effect on support for intervention (Table 2) is decomposed into the portion mediated by perceived bigness (Table 4), the portion mediated by expected price increases (Table 3), and a residual direct effect. Estimates include respondent fixed effects; 95% confidence intervals are from a respondent-clustered bootstrap.

Figure 5 reports the decomposition. The economic features retain large direct effects: about two-thirds of the industry-overlap effect and 40% of the deal-size effect operate outside respondents’ measured price and bigness perceptions, through considerations the two items do not capture. Where these features are mediated, however, the price channel does the work: industry overlap is mediated through expected prices and essentially not through perceived bigness (Table 4), and for deal size expected prices carry a share comparable to bigness.

Familiarity is more fully accounted for, and works through a different channel. Nearly the entire brand effect on support for intervention is mediated — about 93% — but overwhelmingly through perceived bigness (roughly three-quarters of its total effect) rather than price. Brand familiarity thus operates almost wholly as a threat from “bigness” — a general idea of corporate power that is in addition to the ability to manipulate prices.

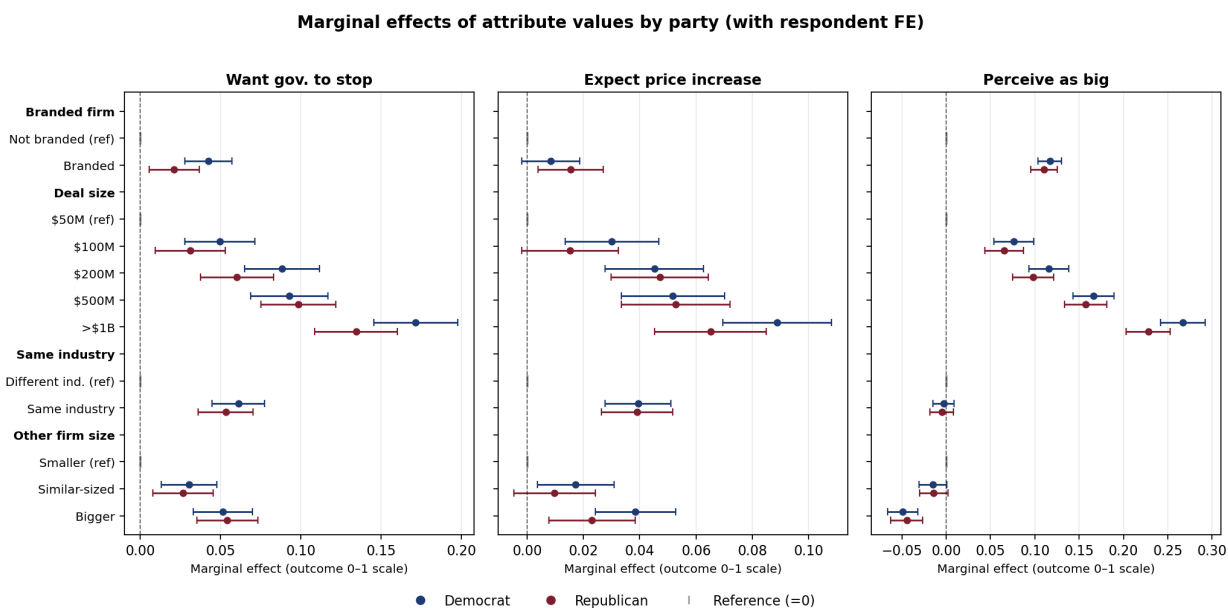
The mediation rests on two closed-ended perception measures. As an independent check on whether they capture how respondents actually reason, I also asked respondents to explain their rankings in their own words. The economically relevant features of a deal dominate these explanations: about 71% of respondents invoke a structural feature—firm size, transaction value, or industry overlap—and 58% invoke a welfare consideration, namely effects on consumers or on competition and markets, with 17% using technical antitrust vocabulary unprompted and nearly half raising two or more distinct dimensions. This is the systematic, economically grounded reasoning the experiment also recovers, and competition and market-structure reasoning in particular is a natural candidate for the direct effects the mediation leaves unexplained. Familiarity surfaces less often in people’s own words—about 12% name a specific firm. The broad pattern nonetheless fits how recognition-based cues are understood to operate: recognition can serve as an intuitive shortcut for attributes such as corporate scale and market power (Goldstein and Gigerenzer 2002), and people often cannot report the considerations that actually shaped their judgments (Nisbett and Wilson 1977; Lodge et al. 1989). This is consistent with the mediation analysis while familiarity is a dimension that is most tied to the outside information environment.

The same bipartisan pattern appears on the demand side. Just as enforcement patterns are stable on the supply side as previously discussed (Figures 2 and 3), Democratic and

identified; the perception-to-preference link rests on a sequential-ignorability assumption (Imai et al. 2010), mitigated by the respondent fixed effects and by asking the two perception items in randomized order. For the familiarity effect, a controlled-direct-effect estimate that holds respondents’ price expectations fixed: the brand effect is essentially undiminished, confirming it does not run through the price channel. Because perceived bigness is itself affected by branding, I estimate this by sequential g-estimation (Acharya et al. 2018), which remains valid when bigness acts as a treatment-induced intermediate confounder. All these results are included in Appendix C.

Republican respondents weigh the same merger features when forming their views. Figure 6 re-estimates the marginal effect of each merger attribute separately for Democratic and Republican respondents, across all three outcomes.

Figure 6: Democrats and Republicans have a similar consideration process when evaluating the merger profiles



Partisanship moves the overall level of pro-enforcement sentiment, but not the decision rule. Democrats are about 5.6 percentage points more likely than Republicans to want an identical merger stopped and about 2.8 points more likely to expect a price increase, with no difference in how big the merging firms are perceived to be. The unadjusted gap on the stop outcome is roughly 9 percentage points, consistent with the descriptive party differences reported before. In contrast, the slopes are similar, as shown in Figure 6; see Figure OA7 in the Appendix for a plot of partisan differences. Both parties respond alike to familiar brands, same-market mergers, and most deal-size buckets, and these features move their price and bigness perceptions in a similar way. The partisan gap emerges mainly in one attribute: Democrats react somewhat more strongly to transactions exceeding one billion dollars.

Validation: brand revelation alone produces accurate and calibrated judgments

The conjoint profiles are what make the decomposition above clean: attributes are randomized. The profiles also hand respondents a fair amount to work with: deal value, the industry match, relative firm size. Coverage of real deals reports such details as well, but there is no guarantee that voters register much beyond the names involved: M&A is plausibly a

low-information domain for most citizens, and the informative test is at the floor. A supplementary survey pushes the evaluation problem to that extreme. The scenarios present nothing but the merging parties’ identities, with brand revelation again randomized to isolate the effect of familiarity. In this most stringent information environment, the design can test not only whether brand information moves beliefs and preferences, but whether the beliefs are accurate and the preferences respond to the economics of the specific deal. The survey is fielded on Prolific from August 13 to 17, 2026, with a pre-analysis plan filed before launch; the final sample has 806 respondents (see Appendix D for details).

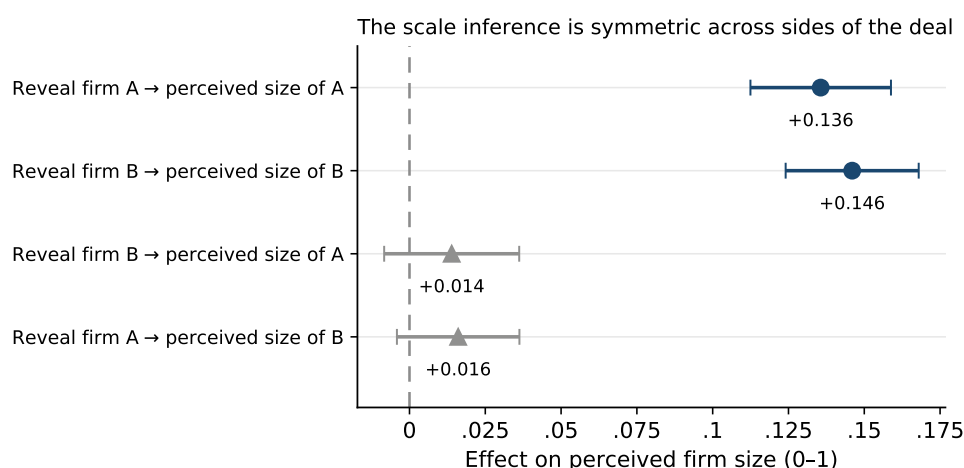
Respondents evaluate proposed mergers between pairs of firms randomly selected from a roster of nine firm pairs; each respondent gets three. Five pairs compete in the same line of business, the other four are in unrelated businesses; see the full list in Appendix D. The outcome questions mirror the main experiment — perceived size of each firm and the approve–stop preference on the same five-point scales, along with expected effects on prices, jobs, and product quality — with one addition: respondents classify the pair’s market relationship as competitors in the same line of business, different but related businesses, largely unrelated, or not sure. This classification is the item scored against the pair’s true relationship. For each side of the pairing, I randomly assign whether the firm appears under its legal name alone or together with the well-recognized brands it owns; here brand information varies on both sides of the deal, not only for the acquiror. Because the firms, the brands, and the ownership are all real, the design turns a household-name company into an anonymous-sounding one without any deception.

The scale inference from the main experiment replicates and is symmetric across sides of the deal: revealing a firm’s brands raises its own perceived size by 0.136 and 0.146 on the 0–1 scale for the two sides respectively, with cross-side effects near zero (Figure 7), magnitudes closely in line with the branded effect on perceived bigness in the main experiment. The beliefs it produces are also accurate. The share of respondents correctly classifying whether the two firms compete rises from 23 percent when neither side is revealed to 74 percent when both are, while “not sure” answers fall from 60 percent to 11 percent. Even with nothing but identity to work from, respondents use the revealed portfolios to work out, pair by pair, whether the merging parties share a market, see detailed results in Appendix D.

The same information calibrates scrutiny to the deal’s economics. Figure 8 splits the effect of brand reveals on support for blocking by the pair’s true relationship. On pairs in unrelated businesses, each reveal significantly lowers support for blocking (-0.027 per revealed side); on pairs that actually compete, reveals leave scrutiny essentially unchanged; and the difference between the two slopes is significant ($+0.034$). Recognizing the firms behind the legal names

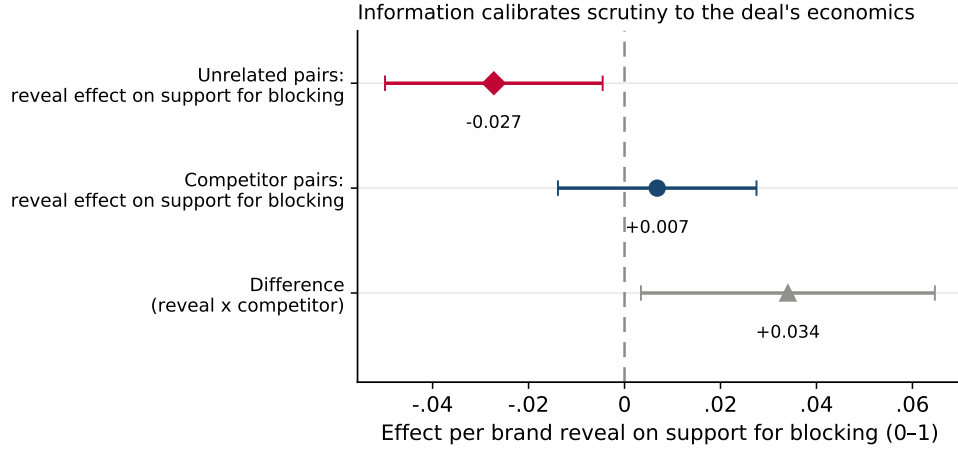
moves respondents toward the economically appropriate response. This pattern complements the main experiment: there, market structure was part of the profile, so brand information could only convey scale, and scrutiny rose; here structure is unknown, so the same cue conveys structure as well. What differs is not how respondents treat brands but what there is left for brands to teach. Had familiarity worked through affect — warm feelings toward known brands producing leniency, or mere salience producing alarm — the reveal would shift both types of pairs in the same direction, which is not what happens; nor is the pattern bigness alarm, since reveals raise perceived scale on both pair types while moving support for blocking in opposite directions. Brand revelation alone, with every other detail withheld, is enough for respondents to recover a deal’s market structure and adjust their preferred response to it.

Figure 7: The scale inference from the main experiment replicates symmetrically on both sides of the deal



Notes: Supplementary survey. Effect of revealing each side’s brand portfolio on the perceived size of each firm (0–1); pair fixed effects, standard errors clustered by respondent, 95 percent confidence intervals. Own-side effects (navy) are of similar magnitude to the branded effect on perceived bigness in the main experiment (Table 4); cross-side effects (gray) are near zero.

Figure 8: Information calibrates scrutiny to the deal's economics



Notes: Supplementary survey. Effect of each brand reveal (0–2 revealed sides) on support for blocking (0–1), estimated separately for pairs in unrelated businesses and pairs that compete in the same line of business, and their difference. Pair fixed effects; standard errors clustered by respondent; 95 percent confidence intervals. Results are similar excluding the pre-registered data-quality tier and without pair fixed effects; see Appendix D.

3.4.2 Evaluation of the Government

If citizens' evaluations of mergers carry real information, the natural next question is whether they hold government to them. I now show that respondents are indeed responsive to agency decisions: government responses to M&A have consequences in accountability and electoral outcomes. For a randomly picked merger profile out of the six profiles shown, respondents are told that the FTC and DOJ decided to either approve or stop the merger, which naturally translates to one of the three randomly assigned conditions: congruence, where the agency action matches the respondent's preferred action, incongruence, where the agency action goes against the respondent's preferred action, and neutral, where the respondent indicated "neither approve nor stop" when evaluating the merger profile. All regressions control for partisanship, demographic characteristics, and pre-treatment attitudes toward M&A, ensuring that the observed effects are not mere reflections of partisan or identity-based loyalties.

Unsurprisingly, respondents increase their confidence in the FTC and DOJ when the agencies act congruently, and decrease their confidence when the agencies act incongruently. See Column (1) of Table 5. Interestingly, there is a baseline positive association between agencies stopping a merger and increasing confidence in the agencies. Column (2) shows that while incongruent behaviors decrease respondents' confidence, their effects are entirely offset in the cases where the agencies decided to stop a merger that the respondents thought

the government should have approved. Allowing different responses to agencies stopping a merger by respondent reported preferences does not erase this baseline positive effect, see Column (3). Table 6 shows a further breakdown by respondent reported preferences into three subsamples. Similar to the previous table, agency decisions to stop a merger shift respondents’ confidence in the government in opposite directions depending on their priors: confidence decreases among those who prefer approval, and increases among those who prefer the government to block the merger. The magnitudes across the two groups — Column (1) versus Column (3) in Table 6 — are noticeably asymmetric. Importantly, even among respondents who initially expressed neutral preferences (i.e. “Neither approve nor stop”), stopping a merger leads to a boost in confidence. Across both tables, the results suggest that there is a general preference for government intervention: stopping a merger is broadly viewed as a sign of good governance and effective agency supervision.

Table 5: Respondents reward congruence and punish incongruence, with an average baseline revealed preference for agencies stopping a merger

	Confidence in FTC/DOJ (0 = decrease significantly, 1 = increase significantly)		
	(1)	(2)	(3)
Congruence	0.105*** (0.012)	0.120*** (0.011)	0.086*** (0.016)
Incongruence	-0.108*** (0.012)	-0.110*** (0.011)	-0.185*** (0.017)
Agency Action: Stop		0.149*** (0.009)	0.076*** (0.017)
Agency Action: Stop X Congruence			0.059*** (0.022)
Agency Action: Stop X Incongruence			0.135*** (0.022)
R-square	0.215	0.321	0.335
Observations	1967	1967	1967

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: “Stop” is a dummy for randomly assigned government action = “stop the merger”. “Congruence” is a dummy for government action matching the respondent’s evaluation of the merger profile.

“Incongruence” is a dummy for government action opposite to the respondent’s evaluation of the merger profile. The omitted comparison group is when respondents reported “Neither approve nor stop” when evaluating the merger profile. All regressions include demographic controls (age, gender, race and ethnicity, education level, income group, political party identification) as well as pre-treatment variables that measure respondent baseline attitudes toward M&A.

Tables 7 and 8 show the same specifications with a dependent variable that is related to electoral outcomes. Respondents first read about how, in the US, presidents can directly pick agency heads, and are asked about their likelihood to re-elect a president who appointed the agency heads that made the aforementioned decision (i.e. either approve or stop the merger). The results corroborate the previous findings where the outcome is confidence in the FTC and DOJ, further demonstrating that perceptions of merger decisions could extend their

Table 6: Further breakdown by respondent reported preferences

	Confidence in FTC/DOJ (0 = decrease significantly, 1 = increase significantly)		
	(1)	(2)	(3)
Agency Action: Stop	-0.061*** (0.013)	0.076*** (0.015)	0.403*** (0.016)
Respondent Preference	Approve	Neutral	Stop
R-square	0.064	0.100	0.545
Observations	813	487	664

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: “Stop” is a dummy for randomly assigned government action = “stop the merger”. “Congruence” is a dummy for government action matching the respondent’s evaluation of the merger profile.

“Incongruence” is a dummy for government action opposite to the respondent’s evaluation of the merger profile. The omitted comparison group is when respondents reported “Neither approve nor stop” when evaluating the merger profile. All regressions include demographic controls (age, gender, race and ethnicity, education level, income group, political party identification) as well as pre-treatment variables that measure respondent baseline attitudes toward M&A.

effects to electoral consequences. While antitrust might not be the core issue for an average citizen, the results suggest that information on how the agencies have been handling specific mergers — if it reaches voters — could shift their evaluations of the president responsible.

Table 7: Respondents reward congruence and punish incongruence in electoral outcomes as well, with an average baseline revealed preference for agencies stopping a merger

	Likelihood to re-elect president (0 = decrease significantly, 1 = increase significantly)		
	(1)	(2)	(3)
Congruence	0.061*** (0.011)	0.070*** (0.011)	0.056*** (0.016)
Incongruence	-0.106*** (0.011)	-0.107*** (0.011)	-0.175*** (0.016)
Agency Action: Stop		0.084*** (0.008)	0.031* (0.017)
Agency Action: Stop X Congruence			0.019 (0.022)
Agency Action: Stop X Incongruence			0.122*** (0.022)
R-square	0.219	0.257	0.272
Observations	1967	1967	1967

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: “Stop” is a dummy for randomly assigned government action = “stop the merger”. “Congruence” is a dummy for government action matching the respondent’s evaluation of the merger profile.

“Incongruence” is a dummy for government action opposite to the respondent’s evaluation of the merger profile. The omitted comparison group is when respondents reported “Neither approve nor stop” when evaluating the merger profile. All regressions include demographic controls (age, gender, race and ethnicity, education level, income group, political party identification) as well as pre-treatment variables that measure respondent baseline attitudes toward M&A.

Yet in reality, blocking a merger is an exceptionally rare event (see Section 4 for details), due to the agencies’ limited capacity. This divergence between public sentiment and institutional

Table 8: Further breakdown of electoral outcomes by respondent reported preferences

	Likelihood to re-elect president (0 = decrease significantly, 1 = increase significantly)		
	(1)	(2)	(3)
Agency Action: Stop	-0.076*** (0.013)	0.032* (0.016)	0.278*** (0.015)
Respondent Preference	Approve	Neutral	Stop
R-square	0.157	0.087	0.424
Observations	813	487	664

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: “Stop” is a dummy for randomly assigned government action = “stop the merger”. “Congruence” is a dummy for government action matching the respondent’s evaluation of the merger profile.

“Incongruence” is a dummy for government action opposite to the respondent’s evaluation of the merger profile. The omitted comparison group is when respondents reported “Neither approve nor stop” when evaluating the merger profile. All regressions include demographic controls (age, gender, race and ethnicity, education level, income group, political party identification) as well as pre-treatment variables that measure respondent baseline attitudes toward M&A.

capacity has two implications. First, it suggests that the public may support expanding the budgets or personnel of agencies like the FTC and DOJ to better align enforcement capacity with expectations for oversight. Second, for highly visible mergers (for instance, those that involve well-known brands), the heightened salience could generate public pressure for regulators to act more, potentially nudging agencies toward investigating certain types of transactions at a higher rate.

Lastly, I investigate how citizens respond to agency enforcement failures. When the agencies block a merger, they could lose the case in court, effectively failing to successfully stop the merger; when they approve a merger which then leads to price increases, the agencies have effectively failed to intervene in a harmful merger. The two conditions are intentionally asymmetric to avoid speculation about counterfactuals²⁰. Instead, both scenarios provide respondents with observable information that could update their assessments of agency competence or judgment through different inferential pathways: a court loss could signal excessive or ineffective enforcement, while a failure to intervene in a harmful merger could signal under-enforcement or incompetence in reviewing the merger ex ante.

Tables 9 and 10 show results in agency confidence split by assigned agency action. Tables 11 and 12 show results in electoral responses by assigned agency action.

Overall, respondents react to agency enforcement failures negatively. Magnitude-wise, approving a harmful merger induces large decreases in confidence, even among respondents who ex ante prefer government approval of the merger or hold neutral opinions. When reacting

²⁰For example, should the agencies intervene in an approved merger, whether they would have secured a successful block in court; should the agencies approve a stopped merger, whether prices would have gone up post merger. These counterfactuals often induce disagreements even among antitrust experts.

Table 9: Respondent reaction to price increases after agencies approve the merger

	Confidence in FTC/DOJ (0 = decrease significantly, 1 = increase significantly)			
	(1)	(2)	(3)	(4)
Agency Failure	-0.321*** (0.013)	-0.302*** (0.019)	-0.243*** (0.027)	-0.409*** (0.021)
Respondent Preference		Approve	Neutral	Stop
R-square	0.461	0.455	0.404	0.588
Observations	967	399	221	343

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: “Agency Failure” is a dummy for the randomly assigned failure treatment: after the FTC and DOJ approved the merger, the two firms completed the transaction and later raised prices of their products. The sample is restricted to respondents randomly assigned the government action “approve the merger”. Column (1) pools these respondents; Columns (2) to (4) split the sample by the respondent’s evaluation of the merger profile (approve, neutral, stop). All regressions include demographic controls (age, gender, race and ethnicity, education level, income group, political party identification) as well as pre-treatment variables that measure respondent baseline attitudes toward M&A.

Table 10: Respondent reaction to agency failure in blocking the merger after the attempt

	Confidence in FTC/DOJ (0 = decrease significantly, 1 = increase significantly)			
	(1)	(2)	(3)	(4)
Agency Failure	-0.150*** (0.012)	-0.052*** (0.020)	-0.128*** (0.022)	-0.290*** (0.022)
Respondent Preference		Approve	Neutral	Stop
R-square	0.185	0.139	0.221	0.420
Observations	1000	411	265	321

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: “Agency Failure” is a dummy for the randomly assigned failure treatment: after the FTC and DOJ decided to stop the merger, the two firms challenged the decision in federal court and the court sided with the firms, allowing them to ultimately merge. The sample is restricted to respondents randomly assigned the government action “stop the merger”. Column (1) pools these respondents; Columns (2) to (4) split the sample by the respondent’s evaluation of the merger profile (approve, neutral, stop). All regressions include demographic controls (age, gender, race and ethnicity, education level, income group, political party identification) as well as pre-treatment variables that measure respondent baseline attitudes toward M&A.

to failure in blocking the merger, the negative association is generally smaller in magnitude. Importantly, Column (2) of both Table 10 and Table 12 show that even when respondents believe that the government should have just approved the merger, they update their view on the agencies accordingly when the agencies intervened but failed to prevail in court, and such updates have electoral consequences as well. This is reassuring — average citizens are able to separate agency competence and effectiveness from their personally preferred government actions. The same logic goes with the subsample with prior preference of stopping the merger, see Column (4) of both tables. Though the two failure conditions are not exactly opposite to each other, the setup mimics what kind of information is possible to convey to the general public in real life. Both price increases of a merger that did happen and the government failure to secure a block from courts are observable, and could be mentioned in congressional reports, political campaign speeches, and everyday news outlets, whereas how prices would have changed had the merger taken place, or how effective the block would have been had the agencies intervened, are complicated and oftentimes unknowable counterfactuals. One immediate implication for antitrust policies and enforcement is that the public does care about what the government is doing with mergers and acquisitions, and that providing clear, accessible information about agency activity and outcomes can improve public understanding and engagement.

Table 11: Respondent reaction to price increases after agencies approve the merger, electoral outcome

	Likelihood to re-elect president (0 = decrease significantly, 1 = increase significantly)			
	(1)	(2)	(3)	(4)
Agency Failure	-0.218*** (0.013)	-0.207*** (0.018)	-0.125*** (0.030)	-0.298*** (0.021)
Respondent Preference		Approve	Neutral	Stop
R-square	0.358	0.358	0.247	0.491
Observations	967	399	221	343

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: “Agency Failure” is a dummy for the randomly assigned failure treatment: after the FTC and DOJ approved the merger, the two firms completed the transaction and later raised prices of their products. The sample is restricted to respondents randomly assigned the government action “approve the merger”. Column (1) pools these respondents; Columns (2) to (4) split the sample by the respondent’s evaluation of the merger profile (approve, neutral, stop). All regressions include demographic controls (age, gender, race and ethnicity, education level, income group, political party identification) as well as pre-treatment variables that measure respondent baseline attitudes toward M&A.

3.4.3 What the Public Sees in the Real World

The experimental results assemble the citizen side of the accountability chain. Respondents weigh the features that antitrust doctrine treats as the standard indicators of anticompetitive

Table 12: Respondent reaction to agency failure in blocking the merger after the attempt, electoral outcome

	Likelihood to re-elect president (0 = decrease significantly, 1 = increase significantly)			
	(1)	(2)	(3)	(4)
Agency Failure	-0.087*** (0.012)	-0.069*** (0.019)	-0.028 (0.024)	-0.141*** (0.021)
Respondent Preference		Approve	Neutral	Stop
R-square	0.144	0.217	0.175	0.223
Observations	1000	411	265	321

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: “Agency Failure” is a dummy for the randomly assigned failure treatment: after the FTC and DOJ decided to stop the merger, the two firms challenged the decision in federal court and the court sided with the firms, allowing them to ultimately merge. The sample is restricted to respondents randomly assigned the government action “stop the merger”. Column (1) pools these respondents; Columns (2) to (4) split the sample by the respondent’s evaluation of the merger profile (approve, neutral, stop). All regressions include demographic controls (age, gender, race and ethnicity, education level, income group, political party identification) as well as pre-treatment variables that measure respondent baseline attitudes toward M&A.

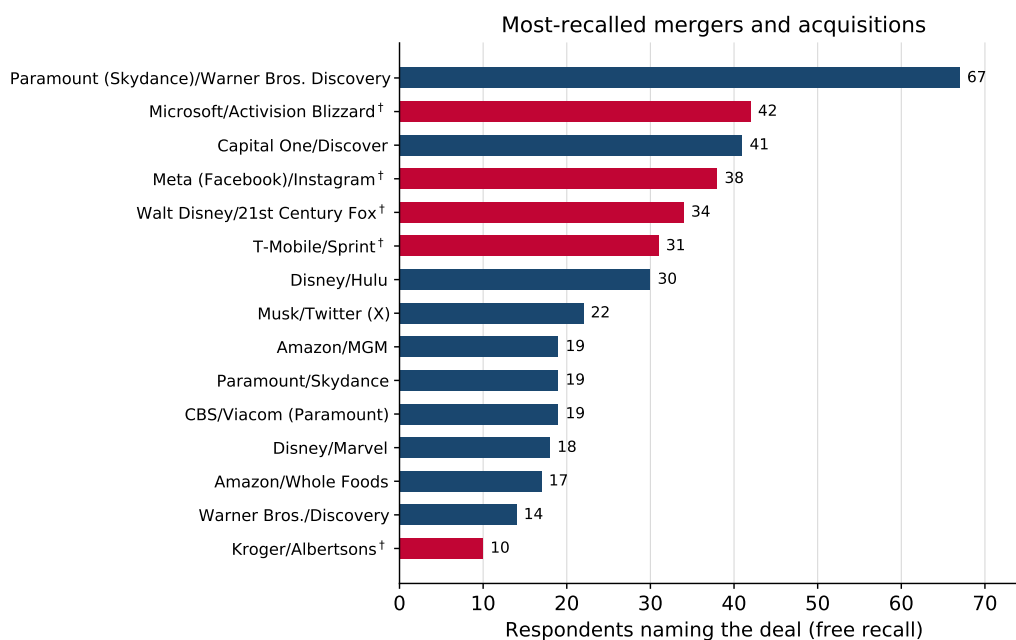
risk, such as transaction size and market overlap, and they use familiar brands as a shortcut; their preferences move through beliefs about prices and corporate scale. Even in the barest information environment, they reconstruct whether the parties compete from brand information alone and shift scrutiny in the direction the deal’s economics warrant. When told what the agencies did, they attribute the outcome upstream, holding accountable both the agencies and the political principal who appointed their leadership, while separating competence from their own policy preferences. Each link, however, operates on information the survey itself supplies. How far these results carry outside the survey depends on the information ordinary citizens actually hold. In the supplementary survey, I measure that stock directly, recording what the public knows about real transactions both unprompted and prompted.

Unprompted knowledge is broader than the technocratic framing of antitrust would suggest. Asked to name mergers or acquisitions from recent years in an open-ended format, 64 percent of respondents produce at least one real, publicly announced transaction, and the recalled deals are both current and dispersed. The most-named transaction is the Paramount/Warner Bros. (Discovery) deal unfolding at the time of fielding, followed by Microsoft/Activision Blizzard, Capital One/Discover, and Facebook’s acquisition of Instagram. In total, respondents name more than two hundred distinct transactions, with the two most-cited accounting for about 13 percent of valid mentions.²¹ Recall is not a proxy for general sophistication —

²¹The open-ended screens carry keystroke, copy-paste, tab-switch, and response-latency telemetry, along with an instructed-response check that flags automated agents. A pre-registered robustness tier marks responses consistent with lookups or AI assistance (18.6 percent of the sample); excluding it, 61 percent of respondents still name a real transaction and the ranking of the most-recalled deals is essentially unchanged.

the gradient in naming at least one real deal is modest across education levels and political-knowledge scores, see detailed tabulation in Appendix D.

Figure 9: Unprompted recall of real mergers is current, dispersed, and includes enforcement targets



[†] challenged by FTC/DOJ

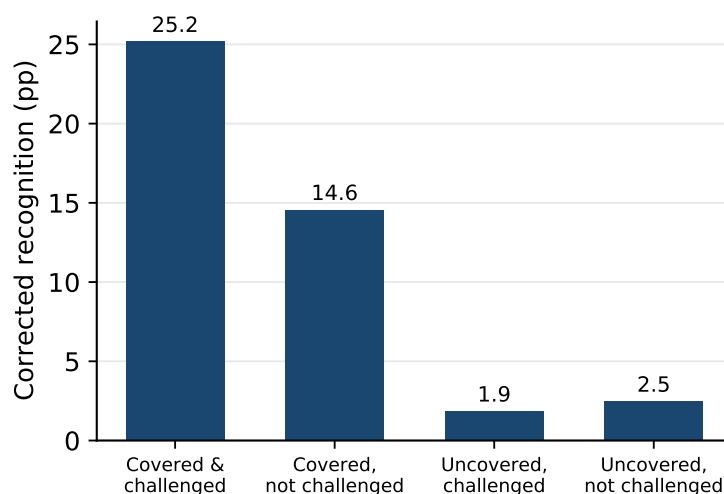
Notes: Most-named transactions in the open-ended recall questions, supplementary survey. Counts are respondents; a deal counts once per respondent across the two free-recall prompts. Vermilion bars mark deals publicly challenged by the FTC or DOJ.

Prompted recognition shows where that knowledge comes from. Respondents see a set of actual transactions drawn from a roster stratified by general-interest news coverage (high versus low public visibility) and by enforcement status, alongside fictitious distractor deals that net out over-claiming. Corrected recognition is 25 percentage points for high visibility deals the government challenged, 15 points for high visibility deals it did not challenge, and only 2 to 3 points for low visibility deals. Recognition also carries content rather than being a feeling of having seen a name before: among deals a respondent recognizes, genuinely challenged transactions draw a report that the government challenged them 26.5 percent of the time, against 11.0 percent for unchallenged ones, a gap of 15.5 points, while among unrecognized deals the gap nearly disappears (7.1 versus 4.9 percent).

What recognition carries is the deal's observable structure, not its consequences. The same follow-up questions ask what each deal would likely do to prices, jobs, and product quality. Respondents rarely decline: among recognized deals, not-sure rates on the consequence items

See Appendix D.

Figure 10: Recognition of real deals concentrates where visibility and enforcement intersect



Notes: Corrected recognition = share of real deals recognized minus the share of fictitious distractor deals “recognized,” netting out over-claiming; distractors are matched to each cell’s visibility tier. Cells cross general-interest news coverage with whether the government publicly challenged the deal; supplementary survey.

are about as low as for market relationship. Yet the answers carry no recognition-borne content. Within the same deal, recognition improves relationship accuracy and sharply cuts uncertainty, while expected consequences are statistically indistinguishable between respondents who recognize the deal and respondents who have never heard of it. The one thing that moves consequence beliefs is enforcement itself: among respondents who recognize a deal, challenged transactions draw expectations of price increases, job cuts, and quality harm 11 to 20 percentage points more often than unchallenged ones, gaps that shrink or vanish among respondents who do not recognize the deal, see Appendix D. Citizens track who the firms are, whether they compete, and what the government did with the deal. Their beliefs about a deal’s consequences look less like independent forecasts than like inferences drawn from the alarm itself.

Public knowledge of merger enforcement is therefore real but concentrated: it follows a fire-alarm pattern (McCubbins and Schwartz 1984) in which attention clusters where visible firms and enforcement activity intersect. This completes the citizen side of the accountability chain: citizens can evaluate mergers from the information they hold; they reward and punish government for enforcement outcomes when those outcomes reach them; and enforcement information does reach them selectively along the visibility track. It also means the public pressure conjectured above has a real audience: an agency that clears a visible, branded merger does so in front of citizens who notice such deals and, at meaningful rates, learn

what the government did with them. The channel runs in both directions. Enforcement is itself more likely to be heard — among high visibility deals, challenged transactions are recognized at nearly twice the rate of unchallenged ones. As shown above, respondents reward intervention with confidence in the agencies; in front of this audience, inaction on a visible deal carries a cost and action carries a credit.

4 FTC and DOJ in Action

Having examined how citizens evaluate mergers in a survey environment, I now show that many of the same considerations appear in how federal agencies enforce antitrust laws. To do so, I analyze the actions of the FTC and DOJ under the Hart–Scott–Rodino (HSR) Act. These two agencies are jointly responsible for enforcing federal antitrust law and systematically reviewing pre-merger filings. The HSR framework requires merging parties whose proposed transaction meets certain thresholds to notify the government before consummating the said transaction. See more details in Appendix A.

The HSR process offers a systematic window into how the agencies review mergers at the point of filing. Combining FTC and DOJ records with comprehensive financial records of all mergers and acquisitions in the US, I am able to study what types of cases attract FTC and DOJ attention among a pool of all possible targets. The observational analyses serve as a real-world counterpart to the experimental results. Together, they help assess whether the public’s concerns are reflected in regulatory patterns.

4.1 Data on Transactions

I use both government agency records and financial records to build the main transaction-level data. The FTC and DOJ publish records of consent orders and complaints submitted to administrative or federal courts. For more details, see Appendix B.

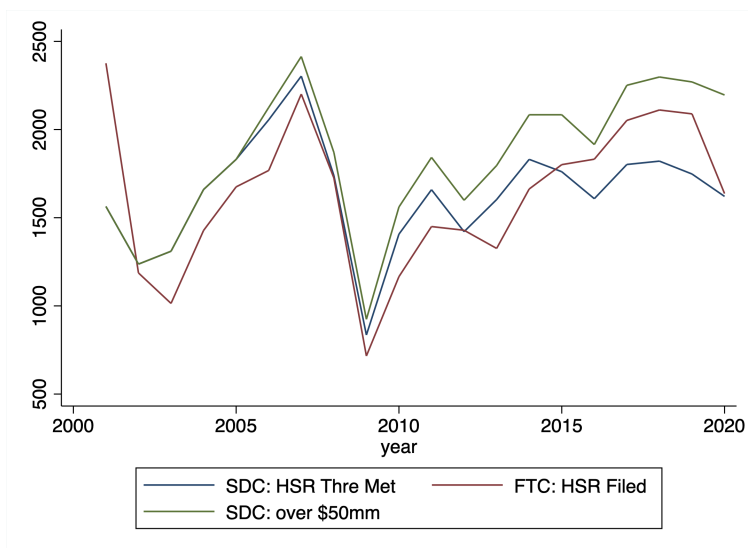
However, these transaction-level records only show part of what the FTC and DOJ have done. One important question is how they pick these cases out of all the cases that they could have investigated. To proxy for the whole pool of M&A activities, I use LSEG SDC Platinum to get all records of M&A transactions with US involvement — that is, at least one party involved in the transaction has a US entity.²² Then, I match this database to

²²While relying on SDC as the foundation of my databuild, I use CRSP/Compustat Merged Database to complement firm level information such as annual net sales.

transaction-level information from FTC and DOJ. The main comparison group would be cases that have met the HSR size of transaction threshold but were not flagged by the FTC/DOJ according to the agencies' public records.

I remove any transaction that involves a REIT (Real Estate Investment Trust) since REITs are not subject to HSR reviews; I also remove mergers between banks because banks are primarily overseen by other federal agencies such as the Federal Deposit Insurance Corporation (FDIC) and the Office of the Comptroller of the Currency (OCC). I remove self-purchases, share buybacks, as well as leveraged buyouts. As a sanity check, I compare the volume of transactions against the aggregate data from FTC/DOJ. The proxy pool tracks FTC/DOJ official statistics closely, see Figure 11 below.

Figure 11: Number of transactions in proxy pool from SDC tracks official government statistics



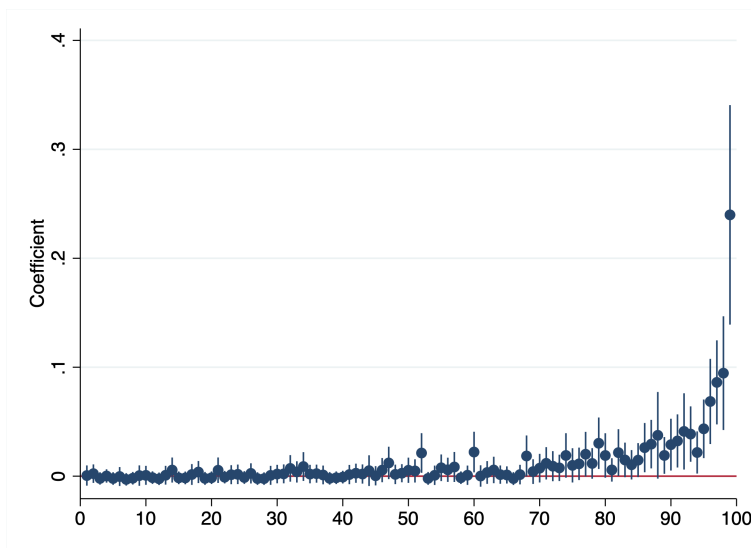
Lastly, I use YouGov's BrandIndex as a proxy for well-known brands. YouGov keeps track of close to 2,000 brands in the US that cover a wide range of industries, from beverages, clothing stores, online brands, to insurance carriers, medical products, and telecom providers. Matching BrandIndex to the SDC base dataset, I am able to categorize companies by whether a given company owns a tracked brand or not.

Due to different limitations of data coverage, the aggregate data covers the period of 2001 to 2024, and the regression base data covers the period of 2001 to 2020. Regressions that involve YouGov-based measures cover the period of 2006 to 2020.

4.2 What drives agency actions?

In this section, I show descriptively what kind of mergers are more likely to be blocked by the agencies. Firstly, larger transactions are flagged more often.²³ Interestingly, deal sizes influence the chance of getting flagged in a very non-linear way — the top 10% of transactions in a given year are more likely to get flagged, and the top 1% attracts a lot more attention from the agencies. This is true in both settings of within-year comparisons and within-acquiror comparisons. See Figures 12 and 13 where coefficients of the deal size percentiles are plotted, and the dependent variable is $\text{dummy} = 1$ for transactions that are flagged. Same industry (horizontal) mergers are more likely to get flagged; the magnitude is around 1.2 to 1.5 percentage points.

Figure 12: Coefficients of Deal Size Percentiles, within Year Comparison

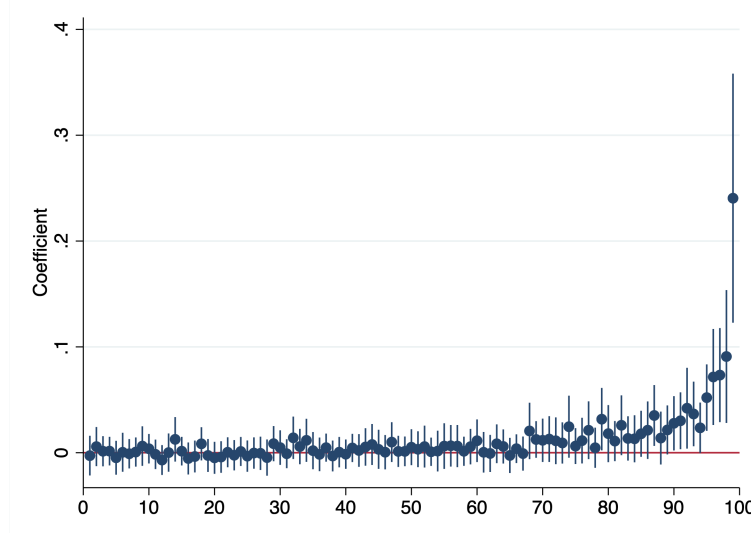


Notes: Regression includes year FE, industry level concentration (defined as whether an industry has HHI that is considered “highly concentrated” per the agencies’ Merger Guidelines), indicator for horizontal mergers at 6-digit NAICS level. Standard errors are clustered at acquiror firm 6-digit NAICS code level.

Next, I show how more publicly visible and familiar cases are more likely to get flagged. Descriptively, when acquiror firms have a tracked brand, the agencies are more likely to block the transaction, see Table 13. The magnitude — around 0.8-0.9 percentage points in columns (1) and (2) — might not seem large, but recall that the average flagging rate at the aggregate level is between 0.6% and 3.4% in a given year; in the regression sample, the average annual flag rate is around 1.52%. The effect of having a tracked brand is

²³I note that around 60% of the records in SDC have missing values for deal sizes, and hence are excluded from analysis. While this is not ideal, SDC is the best data source for such measures to my knowledge, and one can reasonably assume that if there is a selection bias in reporting, the bias tends to be downward — i.e. the deals with missing deal sizes are likely to be smaller on average.

Figure 13: Coefficients of Deal Size Percentiles, within Acquiror Firm Comparison



Notes: Regression includes acquiror firm FE, industry level concentration (defined as whether an industry has HHI that is considered “highly concentrated” per the agencies’ Merger Guidelines), indicator for horizontal mergers at 6-digit NAICS level. Standard errors are clustered at acquiror firm 6-digit NAICS code level.

amplified when the merger is within the same industry, see column (3). Conditioned on such horizontal transactions, deals with a visible and familiar acquiror are more than twice as likely to face regulatory actions. Columns (4) and (5) narrow the descriptive comparison by holding constant different dimensions of transaction similarity. Column (4) includes a target-industry fixed effect, effectively comparing transactions involving target firms operating in the same industry. Acquisitions by firms with recognizable brands remain more likely to attract scrutiny — around 0.6 percentage point. Column (5) includes an acquiror fixed effect, comparing different acquisitions made by the same firm over time. Within a given acquiror’s acquisition history, averaged over all acquirors who have made multiple acquisitions in the time period of 2001-2020, transactions involving more visible target firms are 0.7 percentage points more likely to get flagged.

Transactions where both companies have tracked brands are also more likely to get flagged, see Table 14. Measuring both merging parties’ brand status differs from the survey experiment design for several reasons. In the survey environment, varying whether to reveal brands for either or both firms would have introduced substantially more information and additional noise, while also constraining other attributes such as industry match or relative firm size. By contrast, in the real HSR filings, regulators observe complete information on both parties, and public visibility is naturally shaped by both firms involved. The goal here is not to replicate the experimental treatment mechanically, but to capture the broader

Table 13: Acquiror firm having a tracked brand would attract more attention

	Flagged (0/1)				
	(1)	(2)	(3)	(4)	(5)
Acq Has Tracked Brand	0.008*** (0.002)	0.009*** (0.002)	0.005** (0.003)	0.006*** (0.002)	
Same Market		0.022*** (0.002)	0.020*** (0.002)	0.021*** (0.002)	0.017*** (0.003)
Acq Tracked X Same Market			0.017*** (0.006)		
Tar Has Tracked Brand					0.007** (0.003)
Year FE	X	X	X	X	
Acq Firm FE					X
Tar Industry FE				X	
R-square	0.002	0.008	0.008	0.025	0.313
Observations	22525	22525	22525	22520	17938

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Same Industry transactions are defined as acquiror and target firms having the same primary 6-digit NAICS codes. Acq firm is identified at the ultimate parent company's 6-digit CUSIP code level. Tar industry is identified at the 3-digit NAICS code level. Due to BrandIndex's coverage, regressions in this table are constrained to the period of 2006-2020.

empirical construct of how visible and familiar a merger would appear to the public. Introducing both merging parties' brand information therefore captures transaction-level, rather than firm-level, visibility. Column (1) shows that more visible transactions, where both companies have tracked brands, are on average about 2.4-2.9 percentage points more likely to get flagged by the FTC and DOJ. Column (2) shows that this branding effect is stronger among large transactions—the top decile by deal size in a given year—that are already more likely to attract scrutiny. Column (3) includes controls for deal size, industry concentration, and firm sizes. While the magnitude shrinks, the net effect of having tracked brands on both sides of the merger remains positive. Columns (4) and (5) introduce more stringent fixed effects. By including acquiror-target industry pair fixed effects, these specifications compare transactions occurring within the same relevant market. The results are similar under this comparison: agency scrutiny increases when both parties have visible brands. Throughout all specifications, the interaction term for both merging parties having tracked brands remains positive and non-trivial in size relative to the coefficient of same market.²⁴

Enforcement patterns may also operate at the level of industry selection rather than individual firms or transactions. Regulators often emphasize coverage across sectors, and some industries are more publicly visible than others. To examine whether visibility shapes en-

²⁴In the interacted specifications, the lower-order brand coefficients capture the association for transactions in which only one party has a tracked brand, conditional on the other party being untracked. The estimates therefore speak most directly to whether scrutiny is elevated for transactions in which both firms are publicly visible.

Table 14: Transactions that involve brands on both sides are more likely to get flagged

	Flagged (0/1)				
	(1)	(2)	(3)	(4)	(5)
Acq Has Tracked Brand	0.004 (0.003)	0.007 (0.014)	-0.005** (0.003)	-0.001 (0.004)	-0.011*** (0.004)
Tar Has Tracked Brand	0.001 (0.003)	-0.019 (0.016)	-0.007** (0.003)	-0.003 (0.005)	-0.009* (0.005)
Acq Tracked X Tar Tracked	0.024*** (0.006)	0.053** (0.027)	0.015** (0.006)	0.031*** (0.009)	0.024*** (0.009)
Same Market	0.022*** (0.002)	0.072*** (0.011)	0.016*** (0.002)	0.017*** (0.003)	0.014*** (0.003)
Year FE	X	X	X	X	X
Acq-Tar Industry Pair FE				X	X
Subsample		Top 10%			
Controls			X		X
R-square	0.009	0.030	0.068	0.089	0.145
Observations	22525	2607	22525	11625	11625

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Same Industry transactions are defined as acquiror and target firms having the same primary 6-digit NAICS codes. Acq-Tar industry pair is defined at the 3-digit NAICS code level. Controls include transaction sizes in percentile cut by year (as a categorical variable), industry level concentration (defined as whether an industry has HHI that is considered “highly concentrated” per the agencies’ Merger Guidelines), Acquiror last 12 months net sales, Target last 12 months net sales. Due to BrandIndex’s coverage, regressions in this table are constrained to the period of 2006-2020.

forcement at this broader level, I collapse the data to the acquiror–target industry pair and analyze whether more publicly visible industry pairs are more likely to experience at least one flagged transaction in a given year. Consistent with the transaction-level results, industry pairs with more tracked brands are also more likely to get flagged, particularly among same industry pairs. These results are included in Appendix C — see Tables OA12 and OA13.

Overall, the patterns indicate that mergers involving brands are more likely to attract antitrust attention, and this additional attention is more pronounced for transactions that already carry anticompetitive risks — larger deals, horizontal mergers. Visibility does not appear to substitute for economic signals; rather, scrutiny is heightened where economic risk and public familiarity coincide. This is the same intersection where public knowledge of enforcement concentrates (Section 3.4.3). The agencies act most where the public watches most. That symmetry is consistent with regulators internalizing the fire-alarm audience documented on the demand side, though the enforcement records alone cannot separate anticipation of public scrutiny from visibility proxying for competitive stakes.

5 Conclusion and Discussion

This paper provides systematic evidence on two fronts: how citizens evaluate specific merger enforcement decisions and translate those evaluations into accountability consequences for agencies and elected officials, and how real agency enforcement patterns mirror the dimensions that structure public preferences. I show that average voters would like more government intervention in large transactions within the same industry; they also prefer more intervention upon learning that the firm involved in the transaction owns well-known brands. Revealing brand information has strong, positive causal effects on respondent perception of the firm’s market power and “bigness”. I conclude that average voters are able to evaluate mergers through systematic economic reasoning, and use familiarity as an informative proxy for corporate scale and market power. I also find that individual preferences have consequences in their confidence in the government and evaluation of the president responsible for appointing agency leadership. Respondents reward agencies for actions that align with their own preferences and penalize them when the decisions diverge, but they also exhibit a general tendency to view merger blocks as a signal of competence. These patterns extend to presidential approval, suggesting that antitrust enforcement decisions carry electoral implications beyond the agencies themselves. The observational analysis using FTC and DOJ records as well as financial records and data on brands complements survey findings. Investigations and block attempts are rare events that exhibit stable bipartisan patterns for the past two decades. FTC and DOJ are more likely to block extremely large (the top 1%), horizontal transactions. The agencies are also more likely to block transactions where the firms involved own well-recognized brands. Overall, agency behaviors are generally aligned with public preferences, suggesting that average citizens are able to form meaningful opinions on complicated economic issues like M&A regulatory reviews, in the absence of elite or partisan cues.

The alignment on familiarity admits several interpretations, and they carry different welfare implications. Regulators may anticipate which mergers attract public concern, consistent with earlier evidence linking merger challenges to political pressure (Coate et al. 1990); familiarity may be a reasonable proxy for real competitive stakes, since building a widely recognized brand takes resources and often market position; or regulators may overweight visibility itself, a bias that only internal records of what they observe could quantify. The survey results narrow this menu, though only on the citizen side and only so far. Public concern about familiar deals is not indiscriminate alarm: put to the test against firm pairs with a knowable market relationship, the familiarity shortcut supports accurate classification of market structure and intervention preferences that track the deal’s economics. The

knowledge results establish the audience the anticipation reading requires, and in both directions: the public differentially monitors visible deals and learns what the government does with them, and enforcement is itself heard and rewarded, so visible action carries a credit while visible inaction carries a cost. What remains open is the agency side and the welfare question that turns on it. If regulators respond to this audience, they respond to informed concern rather than noise; but whether the resulting enforcement pattern reflects healthy democratic accountability, an efficient use of a correlated signal, or bureaucratic overweighting of visibility is a question these data cannot settle, and I do not claim to settle it. What the results do establish is that the public half of the alignment rests on inferences that hold up when scored against the real market relationships of the firms involved.

These interpretations are not mutually exclusive, and each highlights the importance of understanding how public perceptions and institutional incentives jointly shape antitrust regulations and enforcement. For instance, there could be broader implications for firms' non-market strategies as they seek to minimize government attention when conducting M&A.²⁵ One implication for businesses that are primarily business-facing is to balance the risk and gain when they invest in public campaigns to promote their brand name.²⁶ Moreover, public sentiment could be influenced and even abused. Firms that are able to manipulate narratives, obscure ownership, or exert influence on political figures who act as principals to the agencies, can face a systematically lighter regulatory burden — a lighter burden that comes across as legitimate in the public eye as well.

This paper demonstrates that antitrust enforcement is not merely a technical exercise reserved for experts and elites but also one within the reach of public judgment. Ordinary citizens are capable of forming systematic, economically grounded judgments about corporate consolidation. They likely draw on everyday experiences as consumers and workers: they notice when prices rise, when product choices narrow, and when a handful of firms seem to dominate more of their economic lives. These lived experiences provide intuitive but meaningful insights for how the government should regulate M&A. As corporate consolidation accelerates and public concerns over economic conditions grow, understanding how voters make sense of these dynamics is essential.

While this paper focuses on antitrust, the findings speak more broadly to how voters can

²⁵For example, Wollmann (2019) has identified bunching around the HSR filing threshold, which suggests that firms would manipulate transactional values to avoid HSR filing. DOJ and FTC have retrospectively investigated failures to file for HSR before, for example, see <https://www.ftc.gov/news-events/news/press-releases/2021/12/ftc-fines-biglari-holdings-inc-repeatedly-violating-antitrust-laws>.

²⁶For example, Intel is not consumer-facing but for intentional and unintentional reasons became a well-recognized brand name.

form structured policy preferences in the absence of strong partisan or elite cues. Competition policy is a domain where citizens receive little direct messaging from political leaders. Whether the patterns here arise because the underlying economic conditions such as prices and bigness are readily observable and experienced, or because weak partisan signals prompt citizens to think more independently, remains an open question that is worth further research. The answer matters beyond antitrust. If the constraint on democratic accountability lies less in citizen capacity than in the legibility of government's choices, then the boundary of the technocratic state is not fixed: any domain whose decisions can be rendered in the terms of everyday life is potentially subject to the same quiet form of public judgment documented here. That conditionality is visible in the data. Public knowledge of real enforcement follows a fire-alarm pattern, concentrated where visible firms and agency action intersect. Respondents who recognize a deal know substantially more about its enforcement history than guessing can explain. On this evidence, citizens understand more than the technocratic view assumes. The reach of democratic accountability is hence set not by what they can understand, but by what government's choices let them see.

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Online Appendix

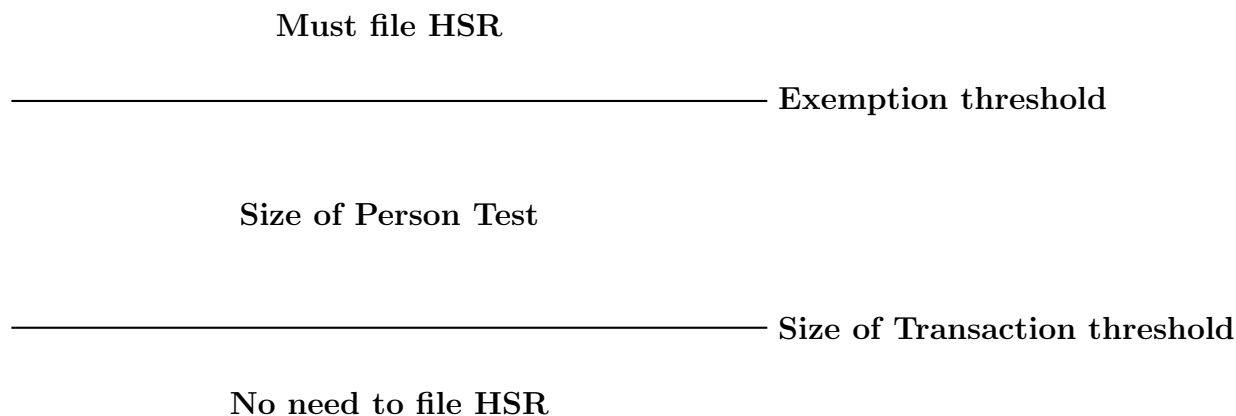
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A Institutional Details of HSR Review Process

The HSR Improvements Act of 1976 asks companies to file premerger notices with the Federal Trade Commission (FTC) and the Department of Justice (DOJ) for mergers larger than a certain size threshold. The most commonly used criterion is the size of transaction — namely, how much the acquiror firm pays to acquire the target company. Bill Clinton signed an amendment in 2000 to increase this threshold from \$15 million to \$50 million. Since 2005, the HSR deal size threshold also gets adjusted every year — in 2024, for example, the number is \$119.5 million. The other more complicated threshold is based on the size of person — that is, whether the firms involved in the transaction are large enough to warrant government attention.¹ This category of thresholds is harder to incorporate into empirical research due to lack of accurate firm level information that is often proprietary and private. For any transaction of a certain large size, the parties must file an HSR notice with the FTC, regardless of whether they meet the size of person test. The original threshold was \$200 million; similar to the size of transaction threshold, this threshold has been adjusted every year since 2005. In this article, I primarily rely on the size of transaction threshold.

Figure OA1: HSR Filing Requirement



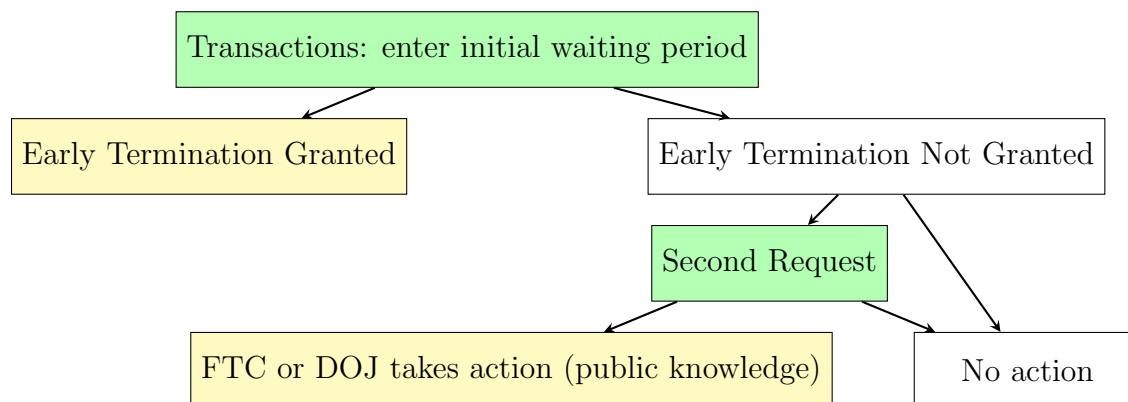
When companies file HSR filings for a potential merger, the transaction first enters an initial waiting period of 30 days. The companies could file a request for Early Termination that could end the waiting period earlier (thus to consummate the merger earlier).² If a transaction is not granted Early Termination, at the end of the waiting period, FTC/DOJ

¹Original threshold: acquiror firm with sales larger than \$100 million and target firm with sales larger than \$10 million.

²When I talked to FTC economists, I was told that FTC usually initiates these Early Termination grants, implying that FTC/DOJ might initiate the Early Terminations rather than firms applying for them.

have the option to issue an additional information request (Second Request). At the end of the Second Request, FTC/DOJ have the option to launch a formal investigation that seeks to block the merger. I illustrate the flow of the HSR review process in Figure OA2.

Figure OA2: HSR Review Process



B Details on Data

I use FTC and DOJ annual reports by fiscal year to build the aggregate datasets. From these reports, I observe the total number of HSR filings and those issued with information requests — these cells are the boxes highlighted in green in Figure OA2. Transaction-level data is not disclosed to the public.³ I am able to get transaction-level information for cases either granted Early Termination or those FTC and DOJ tried to stop (boxes highlighted in yellow in Figure OA2) by scraping FTC and DOJ websites. I show some examples of how this information looks on websites in Figures OA3 and OA4. Second Request information is considered proprietary and not disclosed at transaction level.

However, these transaction-level records only show part of what the FTC and DOJ have done. One important question is how they pick these cases out of all the cases that they could have investigated. To proxy for the whole pool of M&A activities, I use LSEG SDC Platinum to get all records of M&A transactions with US involvement — that is, at least one party involved in the transaction has a US entity.⁴ Then, I match this database to transaction-level information from FTC and DOJ via acquiror and target names. The main comparison group would be cases that have met the HSR size of transaction threshold but were not flagged by the FTC/DOJ according to the agencies' public records. Essentially, I

³Both FTC and DOJ denied my FOIA requests on the ground that this information was meant to be protected and proprietary, and hence they were not obligated to share.

⁴Self purchases, share buybacks, mergers involving REITs, leveraged buyouts are excluded.

Figure OA3: Example of FTC Early Termination Notice

20210616: FedEx Corporation; ShopRunner, Inc.

Date: December 18, 2020
Transaction Number: 20210616
Acquiring Party: FedEx Corporation
Acquired Party: ShopRunner, Inc.
Granting Status: Granted

Acquired Entities

ShopRunner, Inc.

Figure OA4: Example of DOJ Action

Case Open Date

November 20, 2017

Case Name

United States v. AT&T Inc., DirecTV Group Holdings, LLC, and Time Warner Inc.

Case Type

Civil Merger

Case Violation(s)

Vertical Merger

Industry Code(s)

Cable and Other Subscription Programming

Wired Telecommunications Carriers

Wireless Telecommunications Carriers (except Satellite)

Component

[Antitrust Division](#)

compare the cases FTC/DOJ attempted to block to cases with a deal size meeting the HSR threshold, for which it can be reasonably assumed that they submitted an HSR filing before consummating the merger, but which FTC/DOJ did not attempt to block.

I use YouGov's BrandIndex to measure whether a firm has a well-recognized brand. YouGov tracks brands rather than firms, so brand names are mapped to company names, then matched to the SDC base via company names. I use fields of company name, immediate company parent name, and ultimate company parent name. Both steps use an iterative process combining LLM-assisted name matching and manual verification. I clean up matches at

the CUSIP level to make sure the matching is consistent across different company subsidiary and name variations within the same entity.

C Additional Tables and Figures

Figure OA5: Pre-treatment Baseline of Respondent Attitude toward M&A: General Favorability

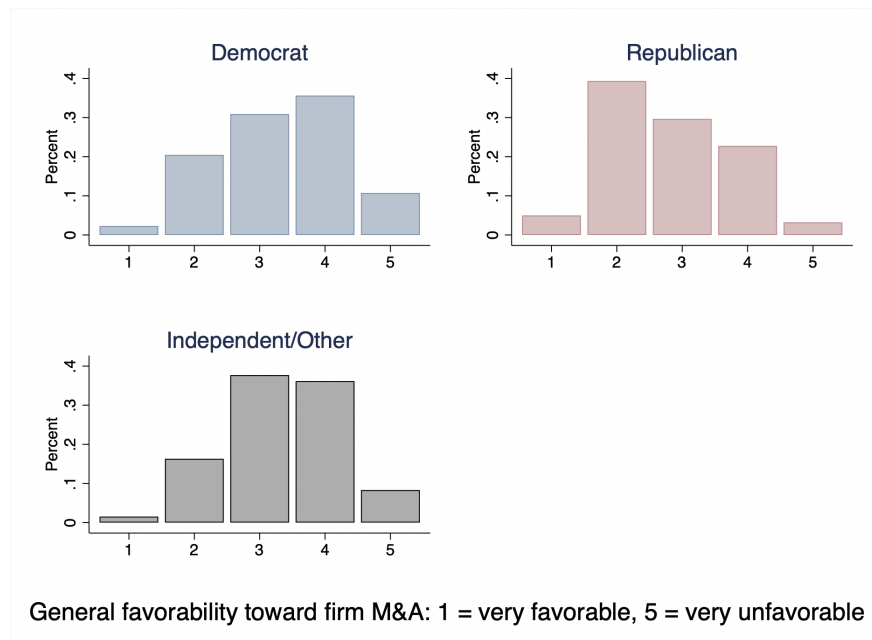


Table OA1: Descriptive Statistics of Survey Sample

	Percent $N = 1,969$
<i>Region</i>	
Midwest	19.7
Northeast	16.6
South	41.1
West	22.7
<i>Political Party</i>	
Democrat	35.5
Republican	34.6
Independent	29.2
Other	0.8
<i>Age</i>	
18 to 34	24.7
35 to 54	46.8
55 and above	28.5
<i>Gender</i>	
Male	46.7
Female	53.3
Other	0.1
<i>Race</i>	
White	75.5
Black	13.0
Asian	4.5
Mixed Race	4.8
Other	2.3
<i>Hispanic</i>	
Yes	8.6
No	91.4
<i>Education Level</i>	
Less than high school	0.3
High school	14.9
Some college	25.9
Associate's degree	14.7
Bachelor's degree	30.6
Graduate degree	13.5
<i>Income Level</i>	
Less than 25k	26.3
25k – 50k	25.1
50k – 75k	19.1
75k – 100k	14.1
100k – 150k	9.8
150k – 250k	4.3
250k – 500k	0.9
More than 500k	0.4

Figure OA6: Pre-treatment Baseline of Respondent Attitude toward M&A: Views on Supervision

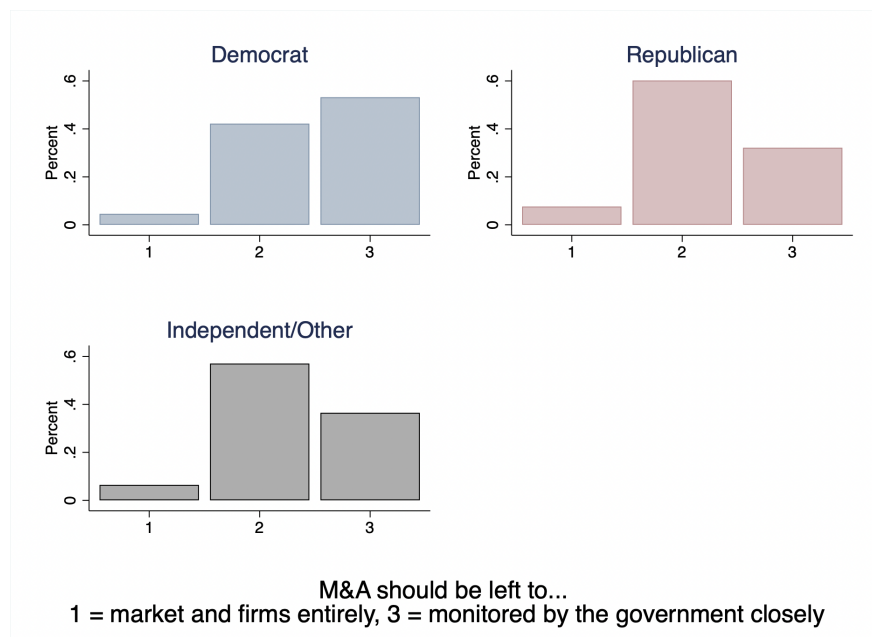


Table OA2: Mediation Analysis — Path Coefficients

	(1)	(2)	(3)	(4)
	Perc. Bigness (M1)	Exp. Price (M2)	Stop (Y)	Stop (Y), total
Branded	0.115*** (0.004)	0.010*** (0.003)	0.003 (0.004)	0.033*** (0.005)
Deal Size (0-1)	0.229*** (0.007)	0.075*** (0.005)	0.057*** (0.006)	0.147*** (0.007)
Same Industry	-0.001 (0.004)	0.037*** (0.004)	0.039*** (0.004)	0.058*** (0.005)
Other Firm Size (0-1)	-0.042*** (0.005)	0.030*** (0.004)	0.044*** (0.005)	0.050*** (0.006)
Perceived Bigness			0.221*** (0.012)	
Expected Price Increase			0.525*** (0.016)	
R-square	0.461	0.468	0.613	0.495
Observations	11814	11812	11812	11814

* p < 0.10, ** p < 0.05, *** p < 0.01

Table OA3: Mediation Analysis — Decomposition

	Total (X→Y)	Indirect (X→M→Y)	via Bigness (M1)	via Price (M2)	% Mediated
Branded	0.033	0.031	0.025	0.005	0.928
Deal Size	0.147	0.090	0.051	0.040	0.613
Same Industry	0.058	0.019	-0.000	0.019	0.332
Other Firm Size	0.050	0.006	-0.009	0.016	0.126

Table OA4: Mediation Analysis — Robustness Check on Decomposition

	(1)	(2)
	Pooled	+ Demographics
prop_brand	0.847*** (0.100)	0.817*** (0.119)
propPr_brand	0.131** (0.059)	0.127** (0.058)
prop_dealsize	0.657*** (0.042)	0.638*** (0.038)
propPr_dealsize	0.313*** (0.026)	0.302*** (0.024)
prop_same_ind	0.362*** (0.046)	0.360*** (0.044)
propPr_same_ind	0.383*** (0.044)	0.380*** (0.043)
prop_tgtsize	0.104 (0.069)	0.100 (0.061)
propPr_tgtsize	0.328*** (0.060)	0.310*** (0.057)
N	11814	11808

* p < 0.10, ** p < 0.05, *** p < 0.01

Figure OA7: Democrats and Republicans have a similar consideration process when evaluating the merger profiles

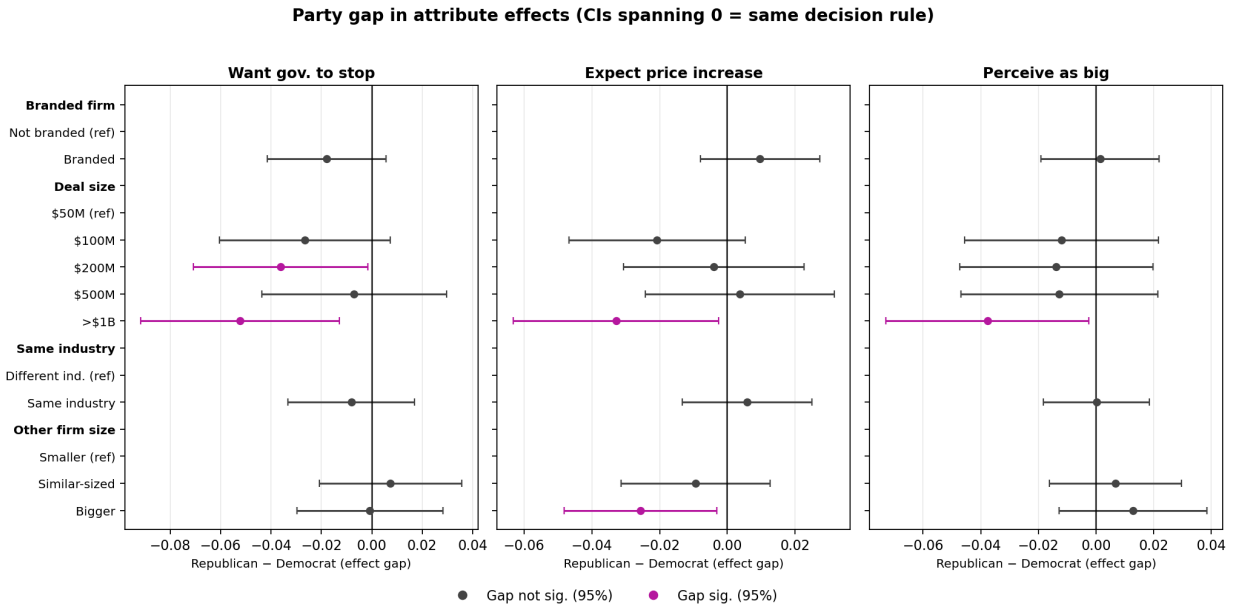


Table OA5: Robustness Check: incl. Deal Size and Other Firm Size Interactions

	Preference on Government Action			
	(1)	(2)	(3)	(4)
Branded	0.033*** (0.004)	0.033*** (0.005)	0.033*** (0.004)	0.033*** (0.005)
Deal Size: 100 million	0.036*** (0.007)	0.032*** (0.007)	0.015 (0.012)	0.017 (0.012)
Deal Size: 200 million	0.064*** (0.007)	0.057*** (0.007)	0.058*** (0.012)	0.052*** (0.012)
Deal Size: 500 million	0.096*** (0.007)	0.084*** (0.007)	0.085*** (0.012)	0.079*** (0.012)
Deal Size: over a billion	0.154*** (0.007)	0.132*** (0.008)	0.126*** (0.012)	0.110*** (0.013)
Same Industry	0.058*** (0.004)	0.054*** (0.005)	0.058*** (0.004)	0.054*** (0.005)
Other Firm Size: similar_sized	0.033*** (0.005)	0.028*** (0.006)	0.003 (0.012)	0.004 (0.012)
Other Firm Size: bigger	0.050*** (0.005)	0.048*** (0.006)	0.040*** (0.012)	0.043*** (0.012)
Favorability towards M&A		0.318*** (0.017)		0.318*** (0.017)
Prefer Gov Oversight		0.086*** (0.012)		0.086*** (0.012)
Deal Size: 100 million × Other Firm Size: similar_sized			0.043*** (0.017)	0.034** (0.017)
Deal Size: 100 million × Other Firm Size: bigger			0.019 (0.017)	0.012 (0.017)
Deal Size: 200 million × Other Firm Size: similar_sized			0.019 (0.017)	0.020 (0.017)
Deal Size: 200 million × Other Firm Size: bigger			-0.002 (0.017)	-0.007 (0.017)
Deal Size: 500 million × Other Firm Size: similar_sized			0.031* (0.017)	0.020 (0.017)
Deal Size: 500 million × Other Firm Size: bigger			0.002 (0.017)	-0.005 (0.017)
Deal Size: over a billion × Other Firm Size: similar_sized			0.053*** (0.017)	0.041** (0.018)
Deal Size: over a billion × Other Firm Size: bigger			0.030* (0.017)	0.024 (0.018)
Respondent FE	Yes		Yes	
Demographic Controls		Yes		Yes
M&A Baseline View Controls		Yes		Yes
R-square	0.496	0.174	0.496	0.175
Observations	11814	11808	11814	11808

Standard errors in parentheses

Outcome scaled 0 (definitely approve) to 1 (definitely stop).

* p < 0.10, ** p < 0.05, *** p < 0.01

Table OA6: Robustness Check: incl. Deal Size and Other Firm Size Interactions

	Expected Changes in Prices			
	(1)	(2)	(3)	(4)
Branded	0.010*** (0.003)	0.007* (0.004)	0.010*** (0.003)	0.007* (0.004)
Deal Size: 100 million	0.021*** (0.005)	0.020*** (0.006)	0.019** (0.009)	0.019** (0.009)
Deal Size: 200 million	0.039*** (0.005)	0.034*** (0.006)	0.041*** (0.009)	0.042*** (0.010)
Deal Size: 500 million	0.050*** (0.005)	0.046*** (0.006)	0.052*** (0.009)	0.052*** (0.010)
Deal Size: over a billion	0.080*** (0.005)	0.070*** (0.006)	0.069*** (0.009)	0.062*** (0.010)
Same Industry	0.037*** (0.003)	0.036*** (0.004)	0.037*** (0.003)	0.036*** (0.004)
Other Firm Size: similar_sized	0.015*** (0.004)	0.014*** (0.005)	0.010 (0.009)	0.014 (0.010)
Other Firm Size: bigger	0.030*** (0.004)	0.026*** (0.005)	0.030*** (0.009)	0.028*** (0.010)
Favorability towards M&A		0.146*** (0.014)		0.146*** (0.014)
Prefer Gov Oversight		0.040*** (0.011)		0.040*** (0.011)
Deal Size: 100 million × Other Firm Size: similar_sized			0.002 (0.013)	0.001 (0.013)
Deal Size: 100 million × Other Firm Size: bigger			0.004 (0.013)	0.002 (0.013)
Deal Size: 200 million × Other Firm Size: similar_sized			-0.001 (0.013)	-0.016 (0.013)
Deal Size: 200 million × Other Firm Size: bigger			-0.005 (0.013)	-0.008 (0.014)
Deal Size: 500 million × Other Firm Size: similar_sized			-0.000 (0.013)	-0.008 (0.014)
Deal Size: 500 million × Other Firm Size: bigger			-0.008 (0.013)	-0.009 (0.014)
Deal Size: over a billion × Other Firm Size: similar_sized			0.025* (0.013)	0.022 (0.014)
Deal Size: over a billion × Other Firm Size: bigger			0.007 (0.013)	0.003 (0.015)
Respondent FE	Yes		Yes	
Demographic Controls		Yes		Yes
M&A Baseline View Controls		Yes		Yes
R-square	0.468	0.071	0.468	0.071
Observations	11812	11806	11812	11806

Standard errors in parentheses

Outcome scaled 0 (decrease significantly) to 1 (increase significantly).

* p < 0.10, ** p < 0.05, *** p < 0.01

Table OA7: Robustness Check: incl. Deal Size and Other Firm Size Interactions

	Perceived Bigness			
	(1)	(2)	(3)	(4)
Branded	0.115*** (0.004)	0.117*** (0.004)	0.115*** (0.004)	0.117*** (0.004)
Deal Size: 100 million	0.066*** (0.006)	0.066*** (0.007)	0.041*** (0.011)	0.040*** (0.013)
Deal Size: 200 million	0.100*** (0.006)	0.095*** (0.007)	0.076*** (0.011)	0.062*** (0.012)
Deal Size: 500 million	0.162*** (0.006)	0.144*** (0.007)	0.141*** (0.011)	0.118*** (0.012)
Deal Size: over a billion	0.238*** (0.006)	0.215*** (0.007)	0.204*** (0.011)	0.177*** (0.012)
Same Industry	-0.001 (0.004)	-0.005 (0.004)	-0.001 (0.004)	-0.005 (0.004)
Other Firm Size: similar_sized	-0.011** (0.005)	-0.013*** (0.005)	-0.046*** (0.011)	-0.052*** (0.013)
Other Firm Size: bigger	-0.042*** (0.005)	-0.047*** (0.005)	-0.069*** (0.011)	-0.081*** (0.014)
Favorability towards M&A		0.020 (0.013)		0.020 (0.013)
Prefer Gov Oversight		0.032*** (0.011)		0.032*** (0.011)
Deal Size: 100 million × Other Firm Size: similar_sized			0.037** (0.015)	0.041** (0.017)
Deal Size: 100 million × Other Firm Size: bigger			0.036** (0.015)	0.036** (0.017)
Deal Size: 200 million × Other Firm Size: similar_sized			0.036** (0.015)	0.042** (0.017)
Deal Size: 200 million × Other Firm Size: bigger			0.035** (0.015)	0.054*** (0.018)
Deal Size: 500 million × Other Firm Size: similar_sized			0.040*** (0.015)	0.042*** (0.016)
Deal Size: 500 million × Other Firm Size: bigger			0.022 (0.015)	0.036** (0.017)
Deal Size: over a billion × Other Firm Size: similar_sized			0.059*** (0.015)	0.070*** (0.016)
Deal Size: over a billion × Other Firm Size: bigger			0.042*** (0.015)	0.043** (0.017)
Respondent FE	Yes		Yes	
Demographic Controls		Yes		Yes
M&A Baseline View Controls		Yes		Yes
R-square	0.463	0.166	0.464	0.168
Observations	11814	11808	11814	11808

Standard errors in parentheses

Outcome scaled 0 (very small) to 1 (very large).

* p < 0.10, ** p < 0.05, *** p < 0.01

Table OA8: Robustness Check: incl. Deal Size and Brand Interactions

	Preference on Government Action			
	(1)	(2)	(3)	(4)
Branded	0.033*** (0.004)	0.033*** (0.005)		
Deal Size: 100 million	0.036*** (0.007)	0.032*** (0.007)	0.046*** (0.010)	0.041*** (0.010)
Deal Size: 200 million	0.064*** (0.007)	0.057*** (0.007)	0.067*** (0.010)	0.060*** (0.010)
Deal Size: 500 million	0.096*** (0.007)	0.084*** (0.007)	0.103*** (0.010)	0.096*** (0.010)
Deal Size: over a billion	0.154*** (0.007)	0.132*** (0.008)	0.176*** (0.010)	0.158*** (0.011)
Same Industry	0.058*** (0.004)	0.054*** (0.005)	0.058*** (0.004)	0.054*** (0.005)
Other Firm Size: similar_sized	0.033*** (0.005)	0.028*** (0.006)	0.033*** (0.005)	0.028*** (0.006)
Other Firm Size: bigger	0.050*** (0.005)	0.048*** (0.006)	0.050*** (0.005)	0.048*** (0.006)
Favorability towards M&A		0.318*** (0.017)		0.318*** (0.017)
Prefer Gov Oversight		0.086*** (0.012)		0.086*** (0.012)
Branded=1			0.049*** (0.010)	0.053*** (0.010)
Branded=1 × Deal Size: 100 million			-0.019 (0.014)	-0.016 (0.014)
Branded=1 × Deal Size: 200 million			-0.007 (0.014)	-0.006 (0.014)
Branded=1 × Deal Size: 500 million			-0.012 (0.013)	-0.023 (0.014)
Branded=1 × Deal Size: over a billion			-0.043*** (0.014)	-0.052*** (0.015)
Respondent FE	Yes		Yes	
Demographic Controls		Yes		Yes
M&A Baseline View Controls		Yes		Yes
R-square	0.496	0.174	0.496	0.175
Observations	11814	11808	11814	11808

Standard errors in parentheses

Outcome scaled 0 (definitely approve) to 1 (definitely stop).

* p < 0.10, ** p < 0.05, *** p < 0.01

Table OA9: Robustness Check: incl. Deal Size and Brand Interactions

	Expected Changes in Prices			
	(1)	(2)	(3)	(4)
Branded	0.010*** (0.003)	0.007* (0.004)		
Deal Size: 100 million	0.021*** (0.005)	0.020*** (0.006)	0.030*** (0.008)	0.029*** (0.008)
Deal Size: 200 million	0.039*** (0.005)	0.034*** (0.006)	0.045*** (0.008)	0.039*** (0.008)
Deal Size: 500 million	0.050*** (0.005)	0.046*** (0.006)	0.059*** (0.007)	0.052*** (0.008)
Deal Size: over a billion	0.080*** (0.005)	0.070*** (0.006)	0.090*** (0.008)	0.083*** (0.009)
Same Industry	0.037*** (0.003)	0.036*** (0.004)	0.037*** (0.003)	0.036*** (0.004)
Other Firm Size: similar_sized	0.015*** (0.004)	0.014*** (0.005)	0.015*** (0.004)	0.014*** (0.005)
Other Firm Size: bigger	0.030*** (0.004)	0.026*** (0.005)	0.030*** (0.004)	0.026*** (0.005)
Favorability towards M&A		0.146*** (0.014)		0.146*** (0.014)
Prefer Gov Oversight		0.040*** (0.011)		0.040*** (0.011)
Branded=1			0.023*** (0.008)	0.019** (0.008)
Branded=1 × Deal Size: 100 million			-0.017* (0.011)	-0.017 (0.011)
Branded=1 × Deal Size: 200 million			-0.011 (0.011)	-0.008 (0.011)
Branded=1 × Deal Size: 500 million			-0.018* (0.011)	-0.012 (0.011)
Branded=1 × Deal Size: over a billion			-0.019* (0.011)	-0.024** (0.012)
Respondent FE	Yes		Yes	
Demographic Controls		Yes		Yes
M&A Baseline View Controls		Yes		Yes
R-square	0.468	0.071	0.468	0.071
Observations	11812	11806	11812	11806

Standard errors in parentheses

Outcome scaled 0 (decrease significantly) to 1 (increase significantly).

* p < 0.10, ** p < 0.05, *** p < 0.01

Table OA10: Robustness Check: incl. Deal Size and Brand Interactions

	Perceived Bigness			
	(1)	(2)	(3)	(4)
Branded	0.115*** (0.004)	0.117*** (0.004)		
Deal Size: 100 million	0.066*** (0.006)	0.066*** (0.007)	0.087*** (0.009)	0.086*** (0.010)
Deal Size: 200 million	0.100*** (0.006)	0.095*** (0.007)	0.124*** (0.009)	0.124*** (0.010)
Deal Size: 500 million	0.162*** (0.006)	0.144*** (0.007)	0.198*** (0.009)	0.179*** (0.010)
Deal Size: over a billion	0.238*** (0.006)	0.215*** (0.007)	0.296*** (0.009)	0.269*** (0.010)
Same Industry	-0.001 (0.004)	-0.005 (0.004)	-0.001 (0.004)	-0.004 (0.004)
Other Firm Size: similar_sized	-0.011** (0.005)	-0.013*** (0.005)	-0.010** (0.005)	-0.012** (0.005)
Other Firm Size: bigger	-0.042*** (0.005)	-0.047*** (0.005)	-0.042*** (0.005)	-0.047*** (0.005)
Favorability towards M&A		0.020 (0.013)		0.020 (0.013)
Prefer Gov Oversight		0.032*** (0.011)		0.031*** (0.011)
Branded=1			0.169*** (0.009)	0.171*** (0.011)
Branded=1 × Deal Size: 100 million			-0.040*** (0.012)	-0.038*** (0.014)
Branded=1 × Deal Size: 200 million			-0.045*** (0.012)	-0.055*** (0.014)
Branded=1 × Deal Size: 500 million			-0.070*** (0.012)	-0.069*** (0.013)
Branded=1 × Deal Size: over a billion			-0.111*** (0.012)	-0.104*** (0.013)
Respondent FE	Yes		Yes	
Demographic Controls		Yes		Yes
M&A Baseline View Controls		Yes		Yes
R-square	0.463	0.166	0.468	0.171
Observations	11814	11808	11814	11808

Standard errors in parentheses

Outcome scaled 0 (very small) to 1 (very large).

* p < 0.10, ** p < 0.05, *** p < 0.01

Table OA11: Mediation Analysis — Controlled Direct Effects

	(1)
cde.brand	0.028*** (0.004)
cde.dealsize	0.108*** (0.006)
cde.same_ind	0.038*** (0.004)
cde.tgtsize	0.035*** (0.005)
N	11812

* p < 0.10, ** p < 0.05, *** p < 0.01

Table OA12: Agencies tend to flag industries with more tracked brands, especially when target is in the same industry (brand counts in quantiles)

	Ind Flagged (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Avg Deal Size (log)	0.069*** (0.004)	0.071*** (0.004)	0.144*** (0.019)	0.025*** (0.002)	0.060*** (0.004)	0.059*** (0.004)
Acq Brand Quantile 2	0.004 (0.011)	-0.015 (0.014)	0.042 (0.036)	0.006 (0.007)	0.020* (0.010)	
Acq Brand Quantile 3	0.018* (0.011)	-0.052*** (0.012)	0.134*** (0.038)	-0.021*** (0.006)	0.010 (0.010)	
Acq Brand Quantile 4	0.047*** (0.015)	-0.048*** (0.017)	0.311*** (0.065)	-0.037*** (0.008)	0.025* (0.015)	
Tar Brand Quantile 2	-0.019* (0.011)			0.009 (0.006)		-0.007 (0.010)
Tar Brand Quantile 3	0.028*** (0.011)			-0.006 (0.006)		0.039*** (0.010)
Tar Brand Quantile 4	0.059*** (0.011)			-0.000 (0.006)		0.021** (0.010)
Same Industry	0.165*** (0.010)	0.058*** (0.016)			0.164*** (0.010)	0.156*** (0.010)
Acq Brand Quantile 2 X Same Ind		0.067*** (0.024)				
Acq Brand Quantile 3 X Same Ind		0.201*** (0.020)				
Acq Brand Quantile 4 X Same Ind		0.364*** (0.032)				
Subsample			Same Ind	Diff Ind		
Ind FE					Tar Ind FE	Acq Ind FE
R-square	0.326	0.350	0.349	0.047	0.411	0.435
Observations	6381	6381	887	5494	6376	6376

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: All regressions include year FE. All regressions include controls on industry level concentration (defined as whether the industry HHI is considered “highly concentrated” per the agencies’ Merger Guidelines), Acquiror last 12 months net sales, Target last 12 months net sales. Number of transactions is used as the analytical weight. Same Industry pairs are tagged at the primary 3-digit NAICS code level. Due to BrandIndex’s coverage, regressions in this table are constrained to the period of 2006-2020.

Table OA13: Agencies tend to flag industries with more tracked brands, especially when target is in the same industry (brand counts in logs)

	Ind Flagged (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Avg Deal Size (log)	0.080*** (0.005)	0.072*** (0.005)	0.161*** (0.023)	0.026*** (0.003)	0.062*** (0.005)	0.064*** (0.005)
Acq Brand (log)	0.007 (0.004)	-0.010** (0.004)	0.040*** (0.015)	-0.007*** (0.002)	-0.011*** (0.004)	
Tar Brand (log)	0.016*** (0.004)			-0.000 (0.002)		0.001 (0.004)
Same Industry	0.172*** (0.012)	-0.022 (0.023)			0.031 (0.023)	0.084*** (0.023)
Acq Brand (log) X Same Ind		0.066*** (0.007)			0.048*** (0.007)	
Tar Brand (log) X Same Ind						0.028*** (0.007)
Subsample			Same Ind	Diff Ind	Tar Ind FE	Acq Ind FE
Ind FE						
R-square	0.343	0.345	0.346	0.047	0.429	0.449
Observations	4602	5430	676	3926	5424	5212

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: All regressions include year FE. All regressions include controls on industry level concentration (defined as whether the industry HHI is considered “highly concentrated” per the agencies’ Merger Guidelines), Acquiror last 12 months net sales, Target last 12 months net sales. Number of transactions is used as the analytical weight. Same Industry pairs are tagged at the primary 3-digit NAICS code level. Due to BrandIndex’s coverage, regressions in this table are constrained to the period of 2006-2020.

Figure OA8: FTC Enforcement Actions, Fiscal Years 1996-2024

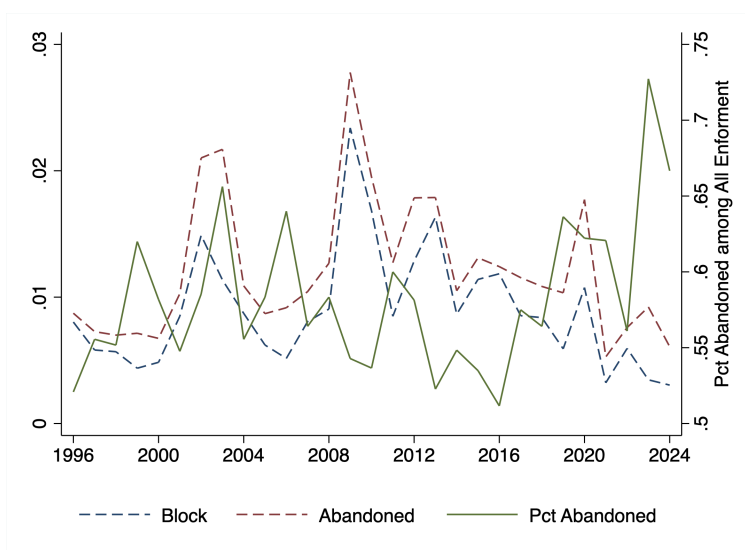


Figure OA9: Respondent View on State of US Economy

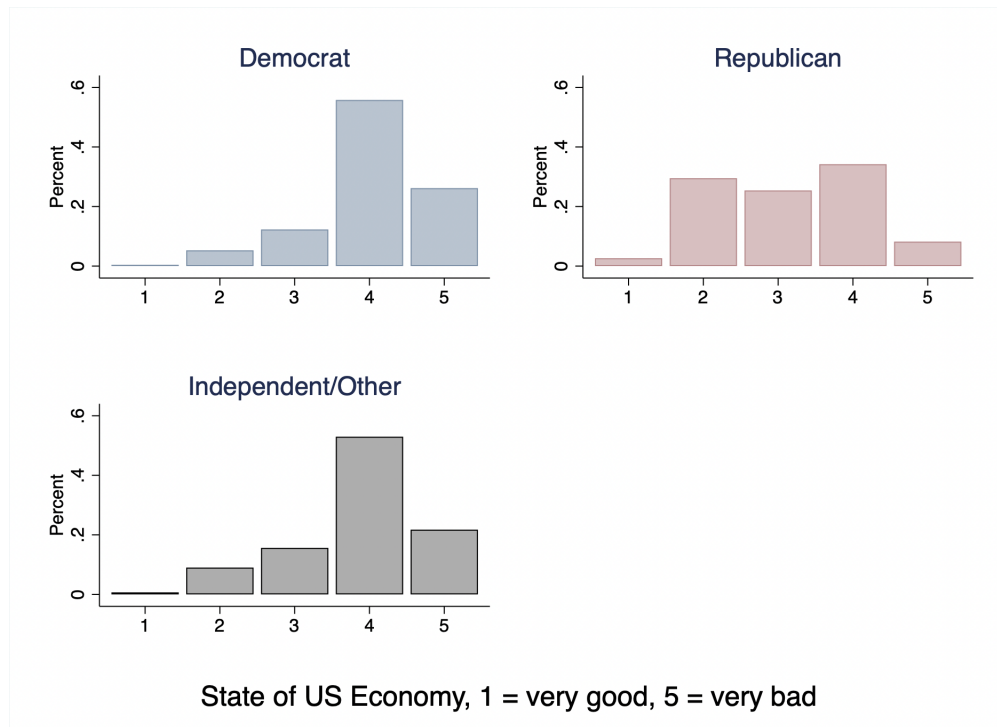


Figure OA10: Respondent View on Trajectory of US Economy

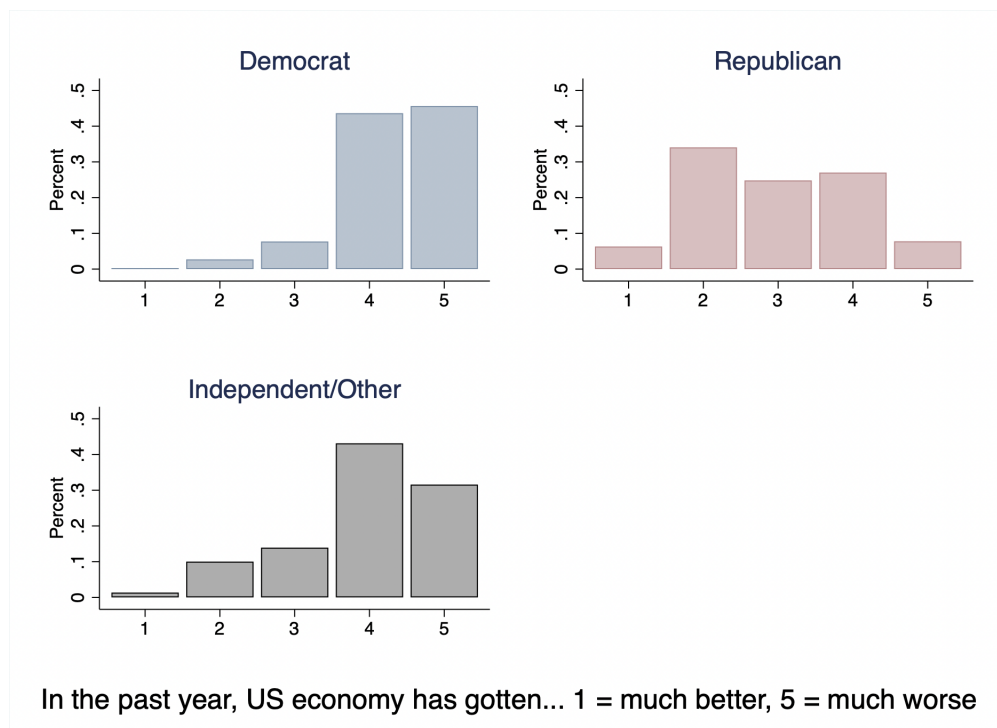
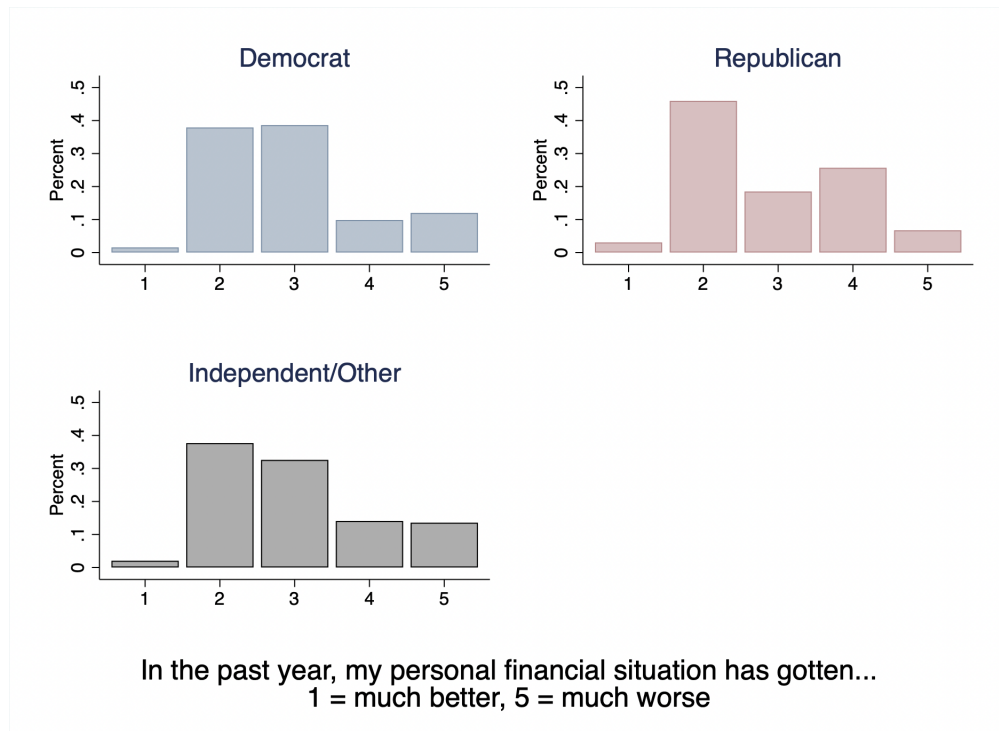


Figure OA11: Respondent View on Personal Financial Situation



D Supplementary Survey

The supplementary survey was administered on Prolific from August 13 to August 17, 2026. A pre-analysis plan was filed before any data collection. Recruitment used Prolific’s demographic screeners to target Census benchmarks on age, sex, and education, with a three-way partisanship target across postings. The final survey has $N = 806$.⁵

After consent, respondents first answered the open-ended block: two free-recall prompts asking them to name mergers or acquisitions from recent years, an attribution item on which government bodies can review and block a deal, and a third open-ended prompt asking for cases in which the government blocked or tried to block a deal. Next came the recognition block built on actual transactions: twelve items per respondent, eight real deals drawn two at random from each cell of a roster stratified by high versus low public visibility and by enforcement status, plus four fictitious distractor deals shown to everyone, matched to the two visibility tiers. For a subset of the shown deals, recognized ones plus a backfill of unrecognized ones to create four in total per respondent, follow-up questions then asked for the deal’s approximate size, the merging parties’ market relationship, whether the government challenged the deal, and its expected effects on prices, jobs, and quality. Items on where respondents encounter merger news, political attitudes and knowledge, and standard demographics followed. Lastly, respondents reached their randomly assigned arm task, so that neither arm’s materials could contaminate the recall or recognition answers.

The arm task was assigned at random with 75/25 probability. In the scenario arm (75% chance), respondents evaluated three hypothetical mergers drawn from a roster of nine real firm pairs, five that compete in the same line of business and four in unrelated businesses. The scenarios disclosed nothing about the deal beyond the parties’ identities. For each side of each pairing, I randomly assigned whether the firm appeared under its legal name alone or together with the well-recognized brands it owns. Outcome questions asked for the perceived size of each firm, the pair’s market relationship (competitors in the same line of business, different but related businesses, largely unrelated, or not sure), expected effects on product prices, product quality, and jobs, and whether the government should approve or stop the deal, on the same five-point scales as the main experiment. In the familiarity arm (25% chance), respondents instead rated their familiarity with 62 items, the 18 firms appearing in the scenario roster as well as in the main experiment’s conjoint profiles, and 44 of the brands

⁵The survey collected 829 completes. 23 respondents (2.8% of 829) met the pre-specified hard-exclusion rule described in Section D.6: open-ended text that contained the instructed-response token or was majority-pasted, with pasted characters accounting for at least half of the response text. All analyses use the remaining valid 806.

Table OA14: Recognition roster: real deals by visibility and enforcement cell

	Flagged	Not flagged
High visibility	<i>Cell A:</i> ExxonMobil/Pioneer Natural Resources Chevron/Hess AT&T/Time Warner CVS Health/Aetna Bayer/Monsanto Walt Disney/21st Century Fox T-Mobile/Sprint Microsoft/Activision Blizzard Kroger/Albertsons JetBlue Airways/Spirit Airlines Facebook/WhatsApp	<i>Cell B:</i> Walmart/Vizio Capital One/Discover Pfizer/Seagen Heinz/Kraft Foods Microsoft/LinkedIn Amazon/Whole Foods Market Salesforce/Slack Budweiser/SABMiller IBM/Red Hat
Low visibility	<i>Cell C:</i> Synopsys/Ansys Omnicom/Interpublic Group Vistra/Energy Harbor TopBuild/Specialty Products and Insulation Novant Health/Community Health Systems United Technologies/Raytheon Praxair/Linde AG Bristol-Myers Squibb/Celgene	<i>Cell D:</i> Broadcom/Qualcomm CDW Corp/Sirius Computer Solutions Wesco International/Anixter International Diamondback Energy/Endeavor Energy Resources International Paper/DS Smith Berry Global/RPC Group Arthur J. Gallagher/AssuredPartners Globus Medical/NuVasive

Notes: The 36 real deals in the recognition block, by roster cell. Cells cross public visibility (high: deals involving widely recognized firms, covered in the general-interest press) with enforcement status (flagged: a public FTC or DOJ challenge). Each respondent saw two deals drawn at random from each cell, in randomized order, alongside the four fictitious distractors in Table OA15.

Table OA15: Fictitious distractor deals

Distractor	Visibility tier	Calibrates cells
Costco/Petco	High	A, B
Eli Lilly/Moderna	High	A, B
Eaton/Hubbell Incorporated	Low	C, D
Emerson Electric/Roper Technologies	Low	C, D

Notes: Fictitious deals shown to every respondent, constructed to be plausible for their visibility tier. Corrected recognition subtracts each tier's distractor "recognition" rate from the real-deal recognition rate of that tier's cells.

they own, presented in randomized order with no pairing of firms to brands.

D.1 Sample

Table OA16 compares the final sample to population benchmarks. The sample tracks the benchmarks on sex, age, and region, with the familiar online-panel shortfalls at the lowest education tier and among respondents 65 and older. Because the analyses are experimental or within-survey comparisons, and because recruitment targeted these demographic margins directly, I do not apply post-stratification weights. As a sensitivity check, raking to the benchmarks in Table OA16 yields a design effect of 1.31 (an effective sample size of about 469) and leaves the headline estimates similar.

Table OA16: Sample composition and population benchmarks

	Sample (%)	Benchmark (%)
Age 18–34	33.5	29.2
Age 35–64	46.7	48.0
Age 65+	19.9	22.9
Woman	53.1	51.5
HS or less	21.6	37.4
Some college / associate	35.2	28.7
BA or more	43.2	33.9
Democrat	40.4	27.0
Republican	26.3	27.0
Independent	30.8	45.0
Hispanic or Latino	12.7	17.0
Northeast	18.0	17.1
Midwest	16.9	20.6
South	34.4	38.8
West	30.8	23.5
Respondents	806	

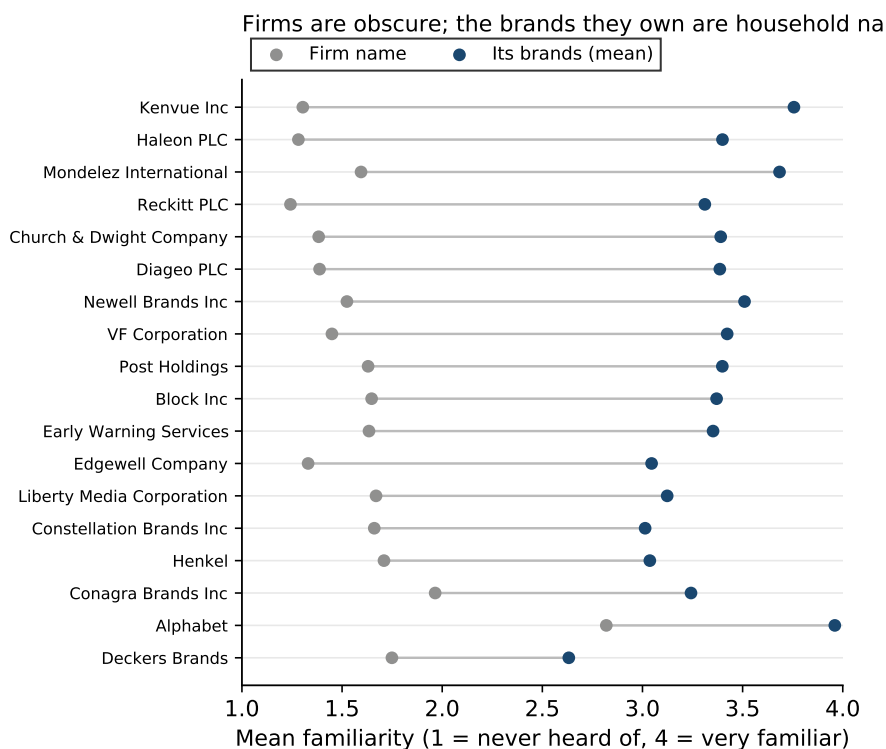
Notes: Approved sample. Benchmarks: age, sex, education, Hispanic origin, and region from ACS 2024 1-year estimates (adults 18+); party identification from Gallup 2025 (Democrat/Republican/independent self-identification). Analyses are unweighted.

D.2 Recognition of Firms, Brands, and Real Deals

Figure OA12 reports the familiarity arm. On the four-point familiarity scale, from never having heard of the name (1) to being very familiar with it (4), the firms in the scenario

roster average 1.61 under their legal names; the brands they own average 3.34, and the gap holds for nearly every firm on the list. This is the measured asymmetry that the main experiment’s branded and not-branded profile versions manipulate, and it is what allows the scenario arm to render a household-name company anonymous-sounding without deception.

Figure OA12: Firms are obscure; the brands they own are household names

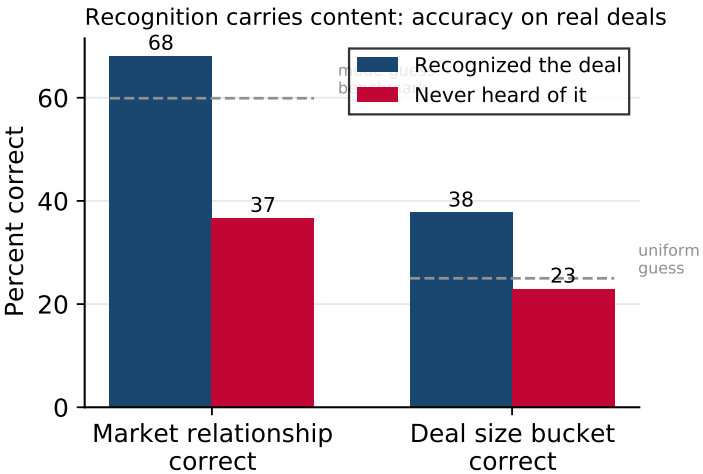


Notes: Familiarity arm of the supplementary survey. Each row is a firm from the scenario roster; gray dots show mean familiarity with the firm’s legal name, navy dots the mean across its listed brands (1 = never heard of it, 4 = very familiar). Items are presented in randomized order, with no pairing of firms to their brands.

Figures OA13 and OA14 report the accuracy content of prompted recognition. Among deals a respondent recognizes, 68 percent of market-relationship answers are correct, against a 60 percent benchmark from always guessing the most common true relationship; among unrecognized deals, accuracy is 37 percent. Size answers show the same pattern at 38 versus 23 percent, against a uniform 25 percent benchmark. Figure OA14 breaks the answers down by roster cell and recognition status. Table OA17 reports the enforcement-knowledge result discussed in Section 3.4.3: among recognized deals, genuinely challenged transactions draw a report of a government challenge 26.5 percent of the time against 11.0 percent for unchallenged ones, while among unrecognized deals the gap nearly disappears.

Table OA18 contrasts the observable follow-up items with the consequence items, the com-

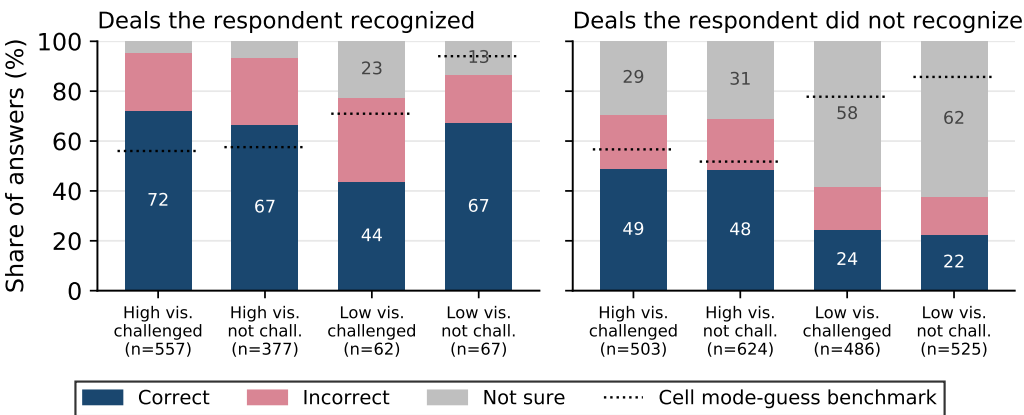
Figure OA13: Recognition carries content: accuracy on real deals against guessing benchmarks



Notes: Follow-up questions on real deals from the recognition roster. The mode-guess benchmark is the accuracy of always answering the most common true relationship among the shown deals; the uniform benchmark for the four size buckets is 25 percent.

Figure OA14: Answer composition by roster cell and recognition status

Market-relationship answers by deal visibility and recognition



Notes: Market-relationship answers on real deals, split by roster cell and by whether the respondent recognized the deal. The dotted line is each cell’s mode-guess benchmark: the accuracy of always answering that cell’s most common true relationship. The benchmark grants a deal-blind guesser the cell’s marginal truth distribution, so exceeding it is conservative evidence of deal-specific knowledge; “not sure” answers count against the correct share.

Table OA17: Reported government challenge by the deal’s enforcement status (H3)

	Challenged deals (cells A/C)	Unchallenged deals (cells B/D)	Difference (clustered SE)
Recognized deals	26.5	11.0	15.5*** (2.3)
Unrecognized deals	7.1	4.9	2.2** (1.0)

Notes: Share (%) answering that the government challenged the deal, among follow-up items. Enforcement status per the locked roster design (SDC flag); standard errors clustered by respondent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

parison discussed in Section 3.4.3. Panels A and B implement the registered comparison. Respondents rarely decline the consequence items: among recognized deals, “not sure” rates on prices, jobs, and quality run 9 to 10 percent, about as low as for market relationship. But their substantive answers disagree far more: the modal answer draws 75 percent of recognized-deal responses for market relationship against 48 to 57 percent for the three consequence items, on the same number of substantive options. Panels C and D are exploratory. Within the same deal, recognition improves relationship accuracy and sharply cuts uncertainty on every item, yet substantive consequence answers among those answering are statistically indistinguishable between respondents who recognize the deal and respondents who have never heard of it. The one covariate that moves consequence beliefs is enforcement: among recognized high-visibility deals, challenged transactions draw expectations of price increases, job cuts, and quality harm 11 to 20 percentage points more often than unchallenged ones, gradients that shrink or vanish among respondents who do not recognize the deal. Consistent with the scripts reading, expected price harm tracks the deal’s true horizontal status no better among respondents who recognize the deal (+3.9 points, se 3.3) than among those who do not (+10.2 points, se 2.7): whatever differentiation the consequence answers show is available from the deal’s names alone.

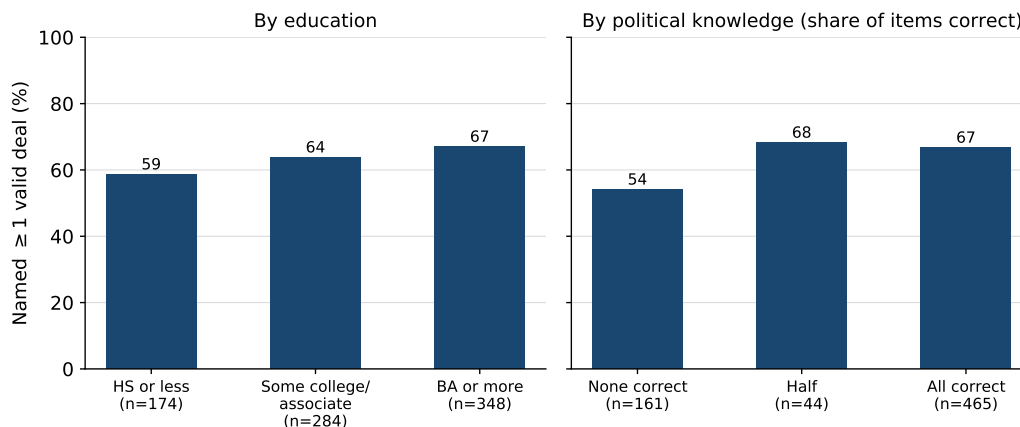
D.3 Open-Ended Recall of Real Transactions

The open-ended responses were coded against a ledger of canonical transactions. A mention counts as a real deal if it resolves to a publicly announced transaction, whatever its outcome; respondents’ claims about a deal’s status are never scored as errors. Mentions naming only a firm, with no transaction, are recorded separately, as are vague and off-topic responses. A deal counts once per respondent. On the enforcement question, a correct recall is a deal the FTC or DOJ publicly challenged: a consent order published for public comment, administrative or federal litigation, or abandonment after a public attempt to block. Every challenge designation is anchored to the survey’s roster design or to an FTC or DOJ primary

source, and the deals shown in the main text were additionally re-verified by independent blind checks. The full codebook, the coding audit trail, and the verification log are included in the replication package.

Table OA19 summarizes the recall results, and Table OA20 lists the most-named deals, the challenged deals named on the enforcement question, and the most common invalid claims. Figure OA15 shows that naming at least one real deal varies only modestly with education and political knowledge. No open-ended mention matched any of the four fictitious distractor deals.

Figure OA15: Naming a real deal varies only modestly with education and political knowledge



Political-knowledge items (House majority, veto override) each fielded to a random subset; score = share correct of items received.

Notes: Share of respondents naming at least one real deal in free recall. The two political-knowledge items (House majority and veto override) were each fielded to a random subset of respondents; the score is the share correct of the items a respondent received.

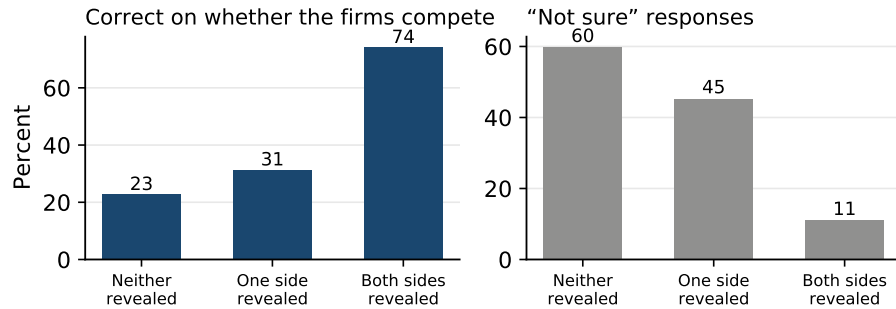
D.4 Scenario Experiment: Full Results

Table OA21 reports the scenario-arm estimates underlying the validation results in Section 3.4.1: the effect of each side’s brand reveal on perceived size, on correct classification of the pair’s market relationship, and on support for blocking, together with the reveal-by-competitor interaction. All specifications include pair fixed effects with standard errors clustered by respondent. Figure OA16 shows the classification result by the number of revealed sides. Table OA22 reports the consequence expectations: reveals do not move expected prices, jobs, or product quality. Table OA23 shows that the calibration result is similar excluding the lookup-or-AI tier and without pair fixed effects, and attenuates with respondent fixed effects, which are identified only from the 59 percent of respondents whose blocking an-

swer varies across their three scenarios. Table OA24 reports the full respondent-fixed-effects versions.

Figure OA16: Brand reveals produce accurate classification of the pair’s market relationship

Brands are information: revealing what real firms own produces accuracy



Notes: Scenario arm. Share of scenarios in which the respondent correctly classifies whether the two firms compete, and the share answering “not sure,” by the number of revealed sides.

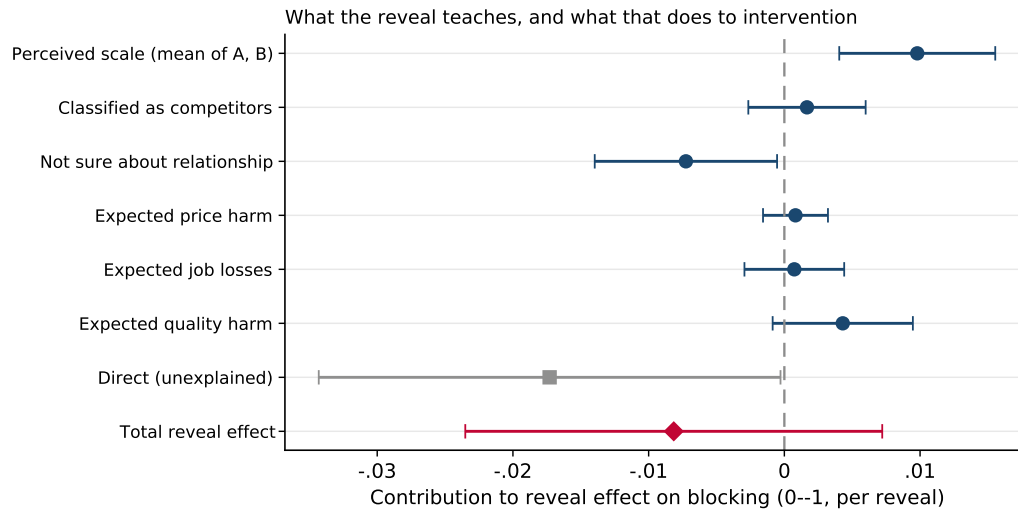
D.5 Decomposition of the Reveal Effect

Figure OA17 decomposes the pooled effect of a brand reveal on support for blocking into the parts running through measured beliefs. The reveal-to-belief paths are experimental; the belief-to-preference paths are observational, so the decomposition is descriptive. Two channels stand out. Perceived scale contributes a positive component, mirroring the main experiment’s mediation result. Uncertainty resolution contributes a negative component: each reveal cuts “not sure” answers about the pair’s relationship by about 24 percentage points, and uncertain respondents lean toward blocking. The competitor classification channel is near zero in the pooled decomposition because it is calibrated rather than inert: reveals raise the probability of classifying the pair as competitors by 0.261 on truly competing pairs and leave it essentially unchanged on unrelated pairs. Consequence beliefs contribute nothing, consistent with Table OA22.

D.6 Data Quality

The open-ended questions carried keystroke, copy-paste, tab-switch, and response-latency telemetry, along with an instructed-response check that flags automated agents. Two pre-specified tiers use this information. First, a hard exclusion removes respondents whose open-ended text contained the instructed-response token or was majority-pasted; 23 respondents

Figure OA17: Decomposition of the reveal effect on support for blocking



Indirect = (reveal→mediator) × (mediator→block | reveal); pair FE, respondent-clustered; a-paths causal, b-paths observational.

Notes: Scenario arm. Each indirect component is the product of the reveal effect on the belief and the belief's association with blocking conditional on the reveal; pair fixed effects, standard errors clustered by respondent. Reveal-to-belief paths are experimental; belief-to-blocking paths are observational.

(2.8 percent) met this rule and are excluded everywhere. Second, a robustness tier flags responses consistent with lookups or AI assistance: any paste, more than 20 seconds of tab-outs, or a page time above the 95th percentile on the open-ended questions. This tier covers 18.6 percent of the final sample. It never excludes anyone from the main estimates; instead, every headline quantity is re-reported excluding it, in the second columns of Tables OA19 and OA20 and in Table OA23.

The open-ended coding followed a written codebook with a fixed decision rule for firm mentions, and was audited three ways: an author audit of a 75-mention stratified random sample against the codebook (97 percent agreement, with disagreements resolved into documented rules), a full verification of every challenge designation against FTC and DOJ primary sources, and an independent blind re-verification of the deals shown in the main text.

Table OA18: What recognition carries: observable deal features versus expected consequences

	Recognized	Unrecognized	Difference
<i>Panel A: “not sure” rate (%)</i>			
Deal size	12.1	31.8	-19.7*** (1.5)
Market relationship	6.9	44.5	-37.7*** (1.4)
Government challenge	50.9	78.8	-27.9*** (1.7)
Prices	10.3	31.2	-20.9*** (1.4)
Jobs	9.4	27.9	-18.5*** (1.4)
Product quality	8.7	33.4	-24.8*** (1.4)
<i>Panel B: consensus among recognized deals — modal-answer share (%)</i>			
Deal size	44.9		
Market relationship	75.0		
Prices	56.7		
Jobs	55.9		
Quality	48.3		
<i>Panel C: substantive answers (% among answered; difference within deal)</i>			
Relationship: correct	73.1	66.1	+3.6* (1.9)
Prices: raise	44.8	47.8	+0.7 (2.3)
Jobs: cut	51.8	55.5	-3.0 (2.4)
Quality: worsen	21.4	19.3	-0.4 (1.9)
<i>Panel D: challenged — unchallenged, high-visibility deals (pp)</i>			
Prices: raise	+17.7*** (3.3)	+9.8*** (3.2)	
Jobs: cut	+19.9*** (3.3)	+0.6 (3.1)	
Quality: worsen	+11.0*** (2.8)	-1.5 (2.6)	

Notes: Follow-up questions on real deals from the recognition roster; recognized/unrecognized per the respondent’s earlier answer (recognition is not randomized); SEs clustered by respondent. Panels A and B are the registered R3 comparison; Panels C and D are exploratory. Per the pre-analysis plan, no accuracy score is defined for the consequence items: realized per-deal price, job, and quality effects are not attributable ex post, even by experts. Panel B: count-weighted mean, over deals with at least 15 recognized substantive answers, of the share giving the deal’s modal answer (higher = more agreement); size has four substantive options, the others three. Panel C differences are estimated within deal (deal fixed effects). Panel D compares challenged (cell A) with unchallenged (cell B) high-visibility deals; deal composition differs across the two cells, so the contrast is descriptive. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table OA19: Open-ended recall of real transactions: summary

	All	Excl. flagged
Named ≥ 1 valid deal (%)	64.0	61.1
Named ≥ 2 valid deals (%)	26.9	23.9
Valid deals per namer (mean)	1.6	1.5
Distinct valid deals named	201	161
Top-2 deal share of valid mentions (%)	13.2	12.4
Only invalid claims (%)	7.4	7.8
Only firm names, no deal (%)	6.5	7.0
Only vague deal mentions (%)	4.2	4.3
Only generic deal talk (%)	2.0	2.1
Only off-topic or unintelligible (%)	1.0	0.9
No mention (none/don't know) (%)	14.9	16.8
Named ≥ 1 challenged deal, enforcement screen (%)	17.1	13.4
Respondents	806	656
Flagged (lookup-or-AI tier, %)	18.6	—

Notes: Free recall pools the two open-ended naming questions; a deal counts once per respondent. A valid deal is a publicly announced transaction, whatever its outcome. The enforcement row uses the separate question asking for deals the government blocked or tried to block, scored against public FTC/DOJ challenges. The second column excludes the pre-registered lookup-or-AI robustness tier described in Section D.6.

Table OA20: Most-named transactions and invalid claims in open-ended recall

Deal	Status	All	Excl. flagged
<i>Panel A: most-named valid deals (free recall)</i>			
Paramount (Skydance)/Warner Bros. Discovery	pending at fielding	67	44
Microsoft/Activision Blizzard	challenged; completed 2023	42	26
Capital One/Discover	completed 2025	41	29
Meta (Facebook)/Instagram	challenged; completed 2012	38	31
Walt Disney/21st Century Fox	challenged; completed 2019	34	25
T-Mobile/Sprint	challenged; completed 2020	31	24
Disney/Hulu	completed 2023	30	26
Musk/Twitter (X)	completed 2022	22	18
Amazon/MGM	completed 2022	19	13
Paramount/Skydance	completed 2025	19	14
CBS/Viacom (Paramount)	completed 2019	19	19
Disney/Marvel	completed 2009	18	13
Amazon/Whole Foods	completed 2017	17	12
Warner Bros./Discovery	completed 2022	14	11
Kroger/Albertsons	blocked; terminated 2024	10	7
<i>Panel B: genuinely challenged deals named on the enforcement screen (H3b)</i>			
JetBlue/Spirit Airlines	blocked 2024	54	33
Microsoft/Activision Blizzard	challenged; completed 2023	29	20
Kroger/Albertsons	blocked; terminated 2024	20	11
AT&T/T-Mobile	agreement 2011; abandoned 2011	17	11
AT&T/Time Warner	challenged; completed 2018	11	6
NVIDIA/Arm	agreement 2020; abandoned 2022	7	3
Adobe/Figma	agreement 2022; abandoned 2023	4	1
Staples/Office Depot	blocked 2016	4	1
Visa/Plaid	agreement 2020; abandoned 2021	2	2
Meta (Facebook)/WhatsApp	challenged; completed 2014	2	2
Meta (Facebook)/Instagram	challenged; completed 2012	2	2
Anthem/Cigna	blocked 2017	2	2
Microsoft/Intuit	agreement 1994; abandoned 1995	1	0
Comcast/NBCUniversal	challenged; completed 2011	1	1
Penguin Random House/Simon & Schuster	blocked 2022	1	1
T-Mobile/Sprint	challenged; completed 2020	1	1
Albertsons/Safeway	challenged; completed 2015	1	1
Walt Disney/21st Century Fox	challenged; completed 2019	1	1
<i>Panel C: most-common invalid claims (all screens)</i>			
Warner Bros./Netflix	bid reported at fielding; no agreement	34	24
TikTok divestiture	not a merger (divestiture)	22	15
Kroger/Giant Eagle	no such transaction	11	9
Facebook/Meta rebrand	rebrand, not a merger	10	8
Paramount/Netflix	no such bid; direction reversed	7	7
Paramount/Disney	no such transaction	6	6
Tesla/Twitter	ownership error (Musk, not Tesla, acquired Twitter)	5	4
Amazon/Microsoft	no such transaction	4	1
Warner Bros./HBO	ownership error (HBO is a WBD unit)	4	4
Disney/Warner Bros.	no such transaction	3	3
Paramount/CNN	no such transaction (CNN is WBD-owned; WBD-bid confusion)	3	3
HBO/Paramount	no such transaction	3	3

Notes: Counts are respondents. Panel A lists the most-named real deals in free recall; Panel B lists deals with a public FTC/DOJ challenge named on the enforcement question; Panel C lists the most common claims that do not correspond to an announced transaction. Status entries reflect each deal's state as of fielding (August 2026).

Table OA21: Brand reveals in hypothetical merger scenarios: size, accuracy, and intervention

	(1)	(2)	(3)	(4)	(5)
	Size of A	Size of B	Relationship correct	Block	Block
Reveal firm A	0.136*** (0.012)	0.016 (0.010)	0.329*** (0.022)	-0.002 (0.011)	
Reveal firm B	0.014 (0.011)	0.146*** (0.011)	0.188*** (0.022)	-0.015 (0.011)	
Reveals (0–2)					-0.027** (0.012)
Reveals × competitor pair					0.034** (0.016)
Pair FE	Yes	Yes	Yes	Yes	Yes
Respondent FE	No	No	No	No	No
Observations	1737	1737	1737	1737	1737
Respondents	579	579	579	579	579

Notes: Hypothetical-merger scenarios, approved sample. Outcomes rescaled to 0–1: perceived size $((x-1)/4)$ from the five-point scale), correct classification of the pair’s true relationship (“not sure” coded incorrect), support for blocking $((x-1)/4)$, 1 = definitely stop). Standard errors clustered by respondent in parentheses. All specifications include pair fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table OA22: Brand reveals and expected consequences: prices, jobs, quality (H6, R5)

	Price harm		Job-loss harm		Quality harm	
	(1)	(2)	(3)	(4)	(5)	(6)
Reveal firm A	0.001 (0.009)		0.007 (0.012)		0.014 (0.009)	
Reveal firm B	0.008 (0.009)		-0.001 (0.012)		0.010 (0.010)	
Reveals (0–2)		-0.004 (0.010)		-0.005 (0.012)		0.010 (0.010)
Reveals × competitor pair		0.015 (0.013)		0.014 (0.017)		0.003 (0.014)
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1737	1737	1737	1737	1737	1737

Notes: Outcomes harm-coded to 0–1 $((5-x)/4)$: 1 = prices significantly increase / jobs significantly cut / quality significantly worsens. Pair fixed effects; SEs clustered by respondent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table OA23: Robustness of the calibration result (support for blocking)

	Main (pair FE)	Excl. flagged	Respondent FE	No pair FE
Reveals (0–2)	-0.027** (0.012)	-0.030** (0.013)	-0.010 (0.010)	-0.027** (0.012)
Reveals $\times$ competitor pair	0.034** (0.016)	0.039** (0.017)	0.027* (0.014)	0.034** (0.016)
Observations	1737	1401	1737	1737
Respondents	579	467	579	579

Notes: SEs clustered by respondent. Excl. flagged drops the pre-registered lookup-or-AI robustness tier. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table OA24: Robustness: respondent fixed effects

	(1) Size of A	(2) Size of B	(3) Relationship correct	(4) Block	(5) Block
Reveal firm A	0.139*** (0.012)	0.021* (0.011)	0.331*** (0.025)	0.018* (0.010)	
Reveal firm B	0.001 (0.011)	0.135*** (0.012)	0.163*** (0.025)	-0.008 (0.010)	
Reveals (0–2)					-0.010 (0.010)
Reveals $\times$ competitor pair					0.027* (0.014)
Pair FE	Yes	Yes	Yes	Yes	Yes
Respondent FE	Yes	Yes	Yes	Yes	Yes
Observations	1737	1737	1737	1737	1737
Respondents	579	579	579	579	579

Notes: Hypothetical-merger scenarios, approved sample. Outcomes rescaled to 0–1: perceived size $((x-1)/4$ from the five-point scale), correct classification of the pair’s true relationship (“not sure” coded incorrect), support for blocking $((x-1)/4, 1 = \text{definitely stop})$. Standard errors clustered by respondent in parentheses. All specifications add respondent fixed effects (within-respondent variation across the three scenarios). Respondents whose outcome does not vary across their scenarios contribute no identifying variation; on the blocking outcome, 41 percent of respondents give an identical answer in all three scenarios, so the within-respondent estimates are identified from the remaining 59 percent.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.