Designing Consent: Choice Architecture and Consumer Welfare in Data Sharing

Chiara Farronato, Andrey Fradkin, Tesary Lin

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Abstract

We study the welfare consequences of choice architecture for online privacy using a field experiment that randomizes cookie consent banners. We study three ways in which firms or policymakers can influence choices: (1) nudging users through banner design to encourage acceptance of cookie tracking; (2) setting defaults when users dismiss banners; and (3) implementing consent decisions at the website versus browser level. Absent design manipulation, users accept all cookies more than half of the time. Placing cookie options behind extra clicks strongly influences choices, shifting users toward more easily accessible alternatives. Many users dismiss banners without making an explicit choice, underscoring the importance of default settings. Survey evidence further reveals substantial confusion about default settings. Using a structural model, we find that among consent policies requiring site-specific decisions, consumer surplus is maximized when consent interfaces clearly display all options and default to acceptance in the absence of an explicit choice. However, the welfare gains from optimizing banner design are much smaller than those from adopting browser-level consent, which eliminates the time costs of repeated decisions.

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1. Introduction

Consumer data is a key input into the digital economy, powering everything from algorithmic recommendations to targeted advertising. Yet, the collection and use of this data have sparked privacy concerns, prompting regulators from the European Union to California to demand that firms obtain explicit user consent before data collection and use. In response, firms now regularly solicit consent to use consumer data, most commonly through cookie consent banners.

Data consent requirements are controversial for at least three reasons. First, firms routinely design interfaces to steer users toward broad data collection and use, potentially preventing consumers from making their preferred choice. These choice architectures, often labeled *dark patterns* in public discourse,[1] include deliberate obstruction (e.g., hiding rejection options behind extra clicks), reordering options to favor consent, or using differential visual salience. Second, requiring each firm to obtain user consent can lead to frequent consent banners, disrupting user experience, and increasing cognitive burden. Lastly, consent requirements risk benefiting large and prominent firms due to consumers' higher propensity to consent to data use when interacting with familiar brands.[2]

In this paper, we examine how three key features of choice architecture (the design of consent interfaces, the use of default settings, and the frequency of consent prompts) shape online cookie consent decisions and influence consumer welfare. We conduct a large-scale field experiment in which we randomize cookie consent interfaces presented to users as they visit thousands of websites over one week. Our design captures both organic browsing behavior and website visits prompted by survey participation. The induced variation allows us to identify the causal effects of banner design on user consent decisions. We then estimate a structural model of consumer choice, and use it to quantify welfare effects. We evaluate counterfactual policies that regulate design elements, set default settings, and mandate a uniform browser-level choice instead of site-by-site choices.

Our results reveal large differences in the effectiveness of choice architecture. Deliberate obstruction effectively deters users from choosing hidden options, whereas purely visual manipulations like reordering or highlighting with different colors have minimal effects. More popular websites achieve higher consent rates. However, choice architecture does not amplify data collection advantages for these sites. Users exhibit considerable heterogeneity in their baseline data-sharing preferences, and many users frequently close banners without choosing, underscoring the critical role of website default settings.

[1] https://www.deceptive.design/

[2] For example, the FTC's report to OECD discusses how data privacy and competition interact: https://one.oecd.org/document/DAF/COMP/WD(2024)29/en/pdf

Our welfare analysis shows that policies designed to reduce choice burdens improve consumer welfare. With site-by-site consent interactions, the optimal consent interface is one that presents all options without obstruction and defaults to "accept all cookies" when users close the banner without making a choice. This design substantially increases consumer surplus compared to the most common design in the U.S., which hides rejection options while also defaulting users to accepting cookies. A browser-level consent mechanism delivers the largest welfare gains, even compared to the optimal banner design, because the time cost of repeated consent interactions substantially outweighs the benefit of site-specific customization.

Our experiment is enabled by Cookie Manager, a customized browser extension built on the Webmunk framework (Farronato, Fradkin, and Karr 2024) that randomly assigns different consent interfaces to users while they browse the web. The extension automatically enforces users' consent choices whenever technically feasible, making user choices incentive-compatible. We use Prolific to recruit U.S. consumers who consent to install Cookie Manager.

We design experimental consent interfaces that vary the ease of choosing among three core options: accept all cookies, reject all (non-essential) cookies, and customize preferences by cookie purpose. We test three types of choice architectures. *Deliberate obstruction* removes an option from the main banner, forcing users to make additional clicks to select that option. *Reordering* emphasizes an option by placing it at the top of the choice list. *Differential highlighting* uses different colors to make a particular option more salient than the others. Our experiment randomizes six different interfaces across both users and web domains.

The study consists of two phases. In the first phase (*survey browsing*), we direct participants to visit specific websites. This structured browsing allows us to evaluate users' privacy preferences across the same set of websites, regardless of whether they would organically visit them. In the second phase (*organic browsing*), we observe participants' natural browsing behavior for a week. In both phases, we randomize consent interface designs as users encounter new web domains.[3] Together, these two phases allow us to identify preferences over cookie tracking and to estimate the effects of interface design on consumer choice. Note that throughout the paper, we use the terms "cookie tracking" and "data sharing" interchangeably.

[2]The extension identifies common HTML, CSS, and textual patterns indicating options to accept or reject cookies. It then selects those options detected based on user choice.

[3]In the organic phase, we also randomly vary banner frequency: 50% of users experience consent banners at most every 10 minutes (the "10-minute" frequency treatment), whereas the other 50% of users experience consent banners at most every 60 minutes (the "60-minute" treatment). This randomization, detailed in Appendix B, allows us to test for choice fatigue induced by more frequent popups. As discussed in the appendix, we find no significant effects of banner frequency on consent choices.

When participants encounter the *baseline interface*—an interface without any nudging towards accepting or rejecting— 65% choose to accept all cookies during survey browsing, and 61% do so during organic browsing. Designs with nudges significantly increase choice variability. Indeed, the share of participants who vary their privacy preferences across websites increases from 30.8% to 68.9% when we introduce design variations.

We find that deliberate obstruction has the strongest influence on privacy choices, while visual changes have modest effects. Hiding the "reject all cookies" option reduces cookie rejection rates by 17.8% in survey visits and 9.4% in organic browsing. The sizable effect of deliberate obstruction is consistent with websites' strategic choices: deliberate obstruction is present in 78.5% of cookie banners, making it the most commonly used design manipulation (Utz et al. 2019). In comparison, reordering options to prioritize "accept all cookies" only increases consent rates by up to 3.8%. Graying out options other than "accept all cookies" increases the acceptance probability by less than 2%.

Perhaps surprisingly, the effect of choice architecture does not vary substantially with website characteristics such as popularity or user familiarity. While users are on average more likely to share data with familiar or popular sites, choice architecture does not systematically increase this tendency. If anything, it slightly reduces the advantage that popular sites enjoy during survey browsing. These findings challenge the hypothesis that choice architecture increases entry barriers or amplifies data-enabled network effects (Hagiu and Wright 2023), which would otherwise reinforce incumbent advantages in the data economy.

Beyond creating choice distortions, consent interfaces impose significant time costs on users. Participants spend an average of 7.34 seconds interacting with a baseline banner during organic browsing. Extrapolating this number to scenarios where consent banners are present on every domain and valuing a user's time at $36/hour (the average U.S. hourly wage), we estimate that consent interactions cost a user $4 per week on average.[4] Having to make an additional click to access options in the "settings" menu increases the time cost by over 50%.

Closing the banner without making a selection is the second most common choice during organic browsing, after accepting all cookies. Yet, 52.2% of participants mistakenly believe that doing so results in rejecting cookies, even though most websites default to acceptance in the U.S. This mismatch leads to systematic discrepancies between users' intended and actual data-sharing outcomes, an issue we explicitly incorporate into our welfare analysis.

[4]Our participants interact with 53 unique web domains on any given week.

To quantify welfare implications, we estimate a structural model of consent decisions using our experimental variation in interface design. In the model, heterogeneous consumers make discrete choices over cookie tracking decisions. We account for consumer heterogeneity by estimating a distribution over three latent user types. The model identifies three segments: users with a strong preference for always accepting cookies, users who consistently prefer to reject them, and a third group with intermediate or context-dependent preferences. To translate our estimates into monetary values, we use the time participants spend interacting with consent banners, combined with their annual income, to quantify their opportunity cost of time.[5] We also use data on participants' beliefs about the consequences of closing the consent banner without making an explicit choice to account for mistaken beliefs in the welfare calculation (Train 2015).

Our welfare analysis allows us to compare several regulatory approaches to consent interfaces. First, we consider banning design manipulations, as mandated by the EU's Digital Services Act.[6] In this regime, website defaults could matter, as many users close banners without making an explicit choice. We find that combining manipulation-free interfaces with an "accept all" default maximizes consumer surplus among banner-based approaches. This policy improves welfare by $0.6 per user-week compared to current U.S. practices, where strategic choice architectures are predominant. It also slightly outperforms the EU norm, which bans deliberate obstruction but defaults users to rejecting cookies. Clarifying defaults, so that consumers have correct beliefs about the implications of closing consent banners, would further increase welfare by $0.2 per user-week.

We also evaluate an alternative policy approach that eliminates site-by-site consent entirely in favor of browser-level privacy settings. The browser-level choice outperforms consent-based policies by a wide margin, improving consumer welfare by $3.7 per user-week compared to the optimal site-by-site banner with correct consumer beliefs. The primary driver of the welfare gain comes from the elimination of time costs associated with repeated choice. Interestingly, even users with heterogeneous preferences across websites would benefit from a browser-level choice. This result is supported by our survey data: 62.4% of participants indicate a preference for browser-level control tools, rather than interacting with cookie banners on each site. Our findings point to a general issue with consent-based policies, which can impose substantial time costs on consumers.

These findings speak directly to the current policy debate on choice architecture related to online privacy. For example, in the EU, both the Digital Services Act and the

[5]This approach relies on the assumption that disutility is linear in time spent interacting with consent banners.

[6]https://commission.europa.eu/strategy-and-policy/priorities-2019-2024/europe-fit-digital-age/digital-services-act_en.

Artificial Intelligence Act regulate choice architecture;[7] in the U.S., several states and the Federal Trade Commission have taken action against deceptive interface designs.[8] Our results support these efforts: designs that deliberately obstruct choices reduce welfare and undermine meaningful choice. However, our analysis also indicates that design regulations alone are insufficient. Browser-level consent mechanisms—already implemented by Mozilla and supported by the California Privacy Rights Act—could deliver much larger welfare gains while preserving user choice.[9]

Our work contributes to the growing literature on the economics of privacy, particularly studies that measure privacy preferences (Lin 2022; Collis et al. 2021; Ke and Sudhir 2023; Tomaino, Wertenbroch, and Walters 2023; Tang 2023; Acquisti, John, and Loewenstein 2013) and examine the consumer impact of data collection practices (Goldfarb and Tucker 2011; Miller and Tucker 2018; Tang 2019; Zhao, Yildirim, and Chintagunta 2021; Bian et al. 2023). Importantly, we highlight that privacy choices in our setting are shaped not only by consumers' preferences over data sharing but also by their perceived benefits from cookie-based personalization and functionality. Our contribution lies in measuring these choices in a naturalistic, real-world setting and using them to estimate consumer surplus under alternative data collection regimes, thus offering a policy-relevant view of privacy trade-offs.

Our analyses relate to the recent theoretical work examining the economics of information markets and privacy (Bergemann and Bonatti 2019, 2024; Chen 2025). A central theme in this literature is the presence of externalities, as individual data-sharing decisions influence the privacy and welfare of others (Acemoglu et al. 2022; Rhodes and Zhou 2024; Miklós-Thal et al. 2024) by affecting firms' inference abilities. In this work, we abstract away from these spillover effects and instead look at individual choices, in line with existing regulatory frameworks (Goldfarb and Tucker 2024).

We also contribute to the broader literature on choice architecture, particularly in the context of privacy decisions. Most empirical work has focused on documenting the prevalence of manipulative consent designs online (Mathur et al. 2019; Di Geronimo et al. 2020; Nouwens et al. 2020; Warberg et al. 2023). Attempts to quantify their effects on user

[7] https://www.europarl.europa.eu/RegData/etudes/ATAG/2025/767191/EPRS_ATA(2025)767191_EN.pdf

[8] The Federal Trade Commission has fined Epic Games and Amazon for user interface designs that induce accidental purchases and obstruct subscription cancellation (https://www.ftc.gov/news-events/news/press-releases/2023/03/ftc-finalizes-order-requiring-fortnite-maker-epic-games-pay-245-million-tricking-users-making and https://www.ftc.gov/news-events/news/press-releases/2023/06/ftc-takes-action-against-amazon-enrolling-consumers-amazon-prime-without-consent-sabotaging-their). U.S. states such as California, Colorado, and Connecticut have enacted privacy regulations that ban companies from manipulating interface designs to increase data collection. (https://insightplus.bakermckenzie.com/bm/technology-media-telecommunications_1/united-states-consumer-protection-regulators-set-sights-on-dark-patterns.).

[9] See: https://blog.mozilla.org/netpolicy/2021/10/28/implementing-global-privacy-control/; https://globalprivacycontrol.org/press-release/20201007.html

behavior have typically relied on lab experiments or artificial settings (Acquisti, John, and Loewenstein 2013; Utz et al. 2019; Luguri and Strahilevitz 2021; Habib et al. 2022; Lin and Strulov-Shlain 2023; Bielova et al. 2024; Baviskar et al. 2024). A few exceptions, such as D'Assergio et al. (2022) and Müller-Tribbensee, Miller, and Skiera (2024), examine persuasive design in real-world contexts. Our study extends this literature by capturing privacy choices made during everyday browsing, thus offering greater real-world relevance. In addition, our analysis covers a wide range of websites, allowing us to assess how design manipulations interact with domain familiarity and popularity.

Finally, our work is related to the existing literature on behavioral biases and their implications for firm competition (Huck and Zhou 2011; Spiegler 2014; Ho, Hogan, and Scott Morton 2017; Decarolis, Li, and Paternollo 2023). This literature examines how factors such as switching costs and obfuscation strategies can limit competition in product markets. Our findings extend this analysis to the domain of data collection strategies. Interestingly, we find the effectiveness of choice architecture to be uniform across websites, regardless of popularity. This suggests that policies targeting choice architecture might not reduce incumbents' data advantage. In fact, they could even harm competition (Campbell, Goldfarb, and Tucker 2015) if smaller firms derive higher marginal value from additional user data, as suggested by Aridor et al. (2024) and Johnson et al. (2024).

The rest of the paper is structured as follows. Section 2 presents our experimental design and describes the study participants. We discuss our descriptive findings and experimental treatment effects in Section 3, and our model of user privacy preferences in Section 4. Section 5 evaluates the welfare effects of policies that regulate consent choice architecture, and Section 6 discusses the policy implications of our findings.

2. Study Design

In this section, we introduce the consent interfaces in our experiment, then discuss the study phases and randomization, and describe the sample of participants.

Consent Interfaces. Our experiment aims to identify how consumers make cookie tracking choices across websites and choice architectures. To do this, we use Cookie Manager, a browser extension based on the Webmunk framework for browsing-based experiments (Farronato, Fradkin, and Karr 2024). Participants install the extension on their Chrome browser. The extension randomizes choice architecture by displaying different consent interfaces that prompt users to make consequential cookie tracking choices.

The design of our consent interfaces is motivated by prior work documenting how companies use choice architecture to encourage data sharing (Habib et al. 2022; Bielova, Santos, and Gray 2024). These practices broadly fall into three categories. Obstruction tactics create friction around privacy-protecting choices–for example, by hiding "reject all cookies" behind extra clicks or defaulting to "accept all cookies." These are among the most common patterns observed online (Habib et al. 2022). Visual manipulation alters option salience through layout, color, or ordering, such as placing "accept all" first or graying out "reject all." Persuasion tactics frame data sharing in a more favorable light using non-neutral language. We focus on obstruction and visual manipulation, excluding persuasion tactics due to their high-dimensional nature and the resulting requirement for more treatment arms than our participant pool allows.

Figure 1 displays our six consent interfaces. We treat Design C ("Set-Acc-Rej") as the *baseline interface*. The other interfaces implement obstruction and visual manipulation. For example, Design A ("Acc-Set") hides the reject option, making rejection only possible through "Cookie Settings" (*deliberate obstruction*); Design D ("Acc-Rej-Set") places the accept option on top (*reordering options*); and Design F ("Acc-GrayRej-GraySet") emphasizes the accept button with a brighter color while graying out the other options (*differential salience*).

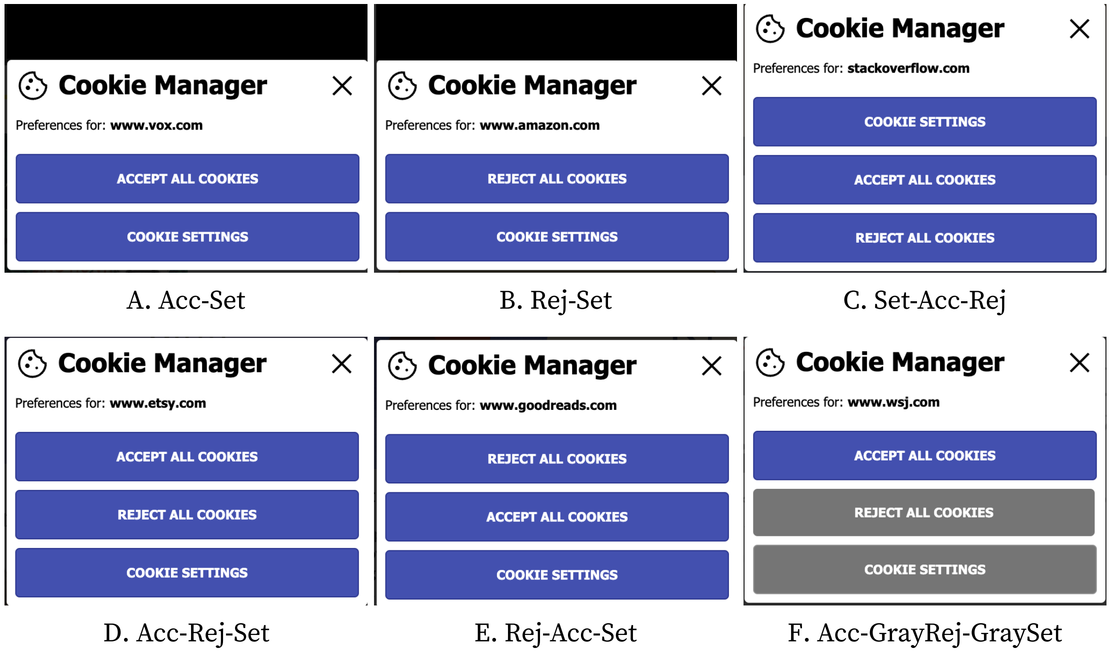
For the deliberate obstruction and reordering options, we also include designs that nudge users towards privacy-friendly options (Design B, "Rej-Set"; and Design E, "Rej-Acc-Set"). Though these are not designs that companies would prefer for the sake of maximizing data sharing, they have been featured in policy discussions (e.g., the "privacy by design" requirement in GDPR and CPRA) and actual platform implementations (e.g., Apple's App Tracking Transparency framework uses both Design E and persuasive tactics).

Users can select any displayed option, or close the banner without making an explicit choice by clicking the *X* in the top right corner of the banner. If they click "X," the website will implement its default data-sharing setting, which is normally "accept all" for U.S. websites. When users click on "Cookie Settings," they are presented with six cookie categories to choose from, such as information storage & access, performance & analytics, and ad selection delivery & reporting (see Appendix Table C.1). Selecting all options is equivalent to accepting all cookies; selecting none is equivalent to rejecting all (non-essential) cookies. To minimize choice friction under "Cookie Settings," we allow consumers to either accept all cookies with only one additional click, or reject all cookies with similar ease (as the default on this page is selecting none of the category-specific cookies).

Our banner can appear on any website, regardless of whether the site asks for consent. When there is an organic cookie choice interface, our banner replaces it.[10] To ensure data-

[10]Our participants are U.S. residents, so most of them do not see consent banners as often due to the lack of federal privacy regulations that require consent.

FIGURE 1. Consent Interface Design across Treatment Groups



Notes: The figure provides screenshots of the six cookie preference interfaces. Captions correspond to the labels used throughout the paper to refer to the treatment conditions. "Acc-Set": accept-settings; "Rej-Set": reject-settings; "Set-Acc-Rej": settings-accept-reject; "Acc-Rej-Set": accept-reject-settings; "Rej-Acc-Set": reject-accept-settings; "Acc-GraRej-GraySet": accept-gray reject-gray settings.

sharing choices are incentive-compatible, Cookie Manager enforces participants' choices by detecting and interacting with websites' native consent forms. The extension uses detection rules adapted from open-source packages (e.g., DuckDuckGo's AutoConsent) and custom scripts to automatically select the corresponding option on each website's original consent interface. When participants close the consent banner without making a choice, Cookie Manager does not modify any website settings. We explain this enforcement mechanism to participants during the onboarding process.[11]

Study Phases and Randomization. We recruit participants through Prolific, screen for eligibility, and instruct qualified and consenting users to install Cookie Manager on their Chrome browser. The study proceeds in two phases: a structured survey phase followed by an organic browsing phase.

[11]We tell users "After installing the extension, you will see a consent-request popup window whenever you visit a website for the first time. If you make a choice, the extension will try to pass on your choices to the website. In most cases, if the website has already been collecting consent from users, it will recognize your choice and decide whether to continue tracking you based on your choice."

In the survey phase, participants visit 20 pre-selected websites spanning different categories and popularity levels (see Appendix C.1 for the full list), presented in randomized order. As participants browse each site, the extension displays a randomly assigned cookie consent banner and records users' selection. This phase serves two purposes: first, it ensures sufficient observations per web domain to estimate choice architecture effects using fixed-effect specifications; second, it complements the organic phase data by providing consent choices when website visits are exogenously given.

In the organic phase, the extension remains active for one week while participants organically browse the web. During this period, the extension shows consent banners on domains where participants have not already interacted with the experimental consent banners. To reduce disruption to browsing, banners appear at most every 10 minutes, rather than on all domains that participants visit during this phase.[12] At the end of the week, participants complete an exit survey and uninstall the extension. Each participant receives $7.50 upon study completion. Survey instruments are provided in Appendix D.

Consent interface randomization occurs at the user-by-domain level: the extension randomly selects a banner design when a participant visits a domain for the first time since enrolling in the study, and records the corresponding choice. In pilot testing, we found no evidence of carryover effects from prior exposures to specific banner designs, so we chose this approach because it increases statistical power by leveraging within-user variation across sites.

Sample Description. We recruited participants on Prolific,[13] and restricted our participants to adults residing in the U.S. who primarily speak English and use Google Chrome as their main browser. We pre-registered recruiting 800 participants and expected 640 of them to be included in the main study sample.[14] Our actual participants are close to the pre-registered numbers (see Appendix Table C.2 for the conversion funnel). A total of 1,227 Prolific users started the study, of whom 74.7% were eligible. Among these, 877 consented to the study, and 563 generated valid data points.

Our final sample of 563 users includes participants who completed the baseline survey and generated valid data during the organic browsing phase, regardless of whether they completed the exit survey. For our main analysis, we exclude participants who, due to an

[12] In this second phase, we also randomize users into two frequency treatments: frequent banners, where banners appear at most every 10 minutes; and infrequent banners, where banners appear at most every 60 minutes. To implement this, a countdown starts after each banner interaction, and a new banner appears after the 10- or 60-minute threshold is crossed and a new domain is visited, whichever occurs later. We do not find differences in consumer behavior across these two treatments (see Appendix B).

[13] https://www.prolific.com/.

[14] https://www.socialscienceregistry.org/trials/12862.

implementation issue, continued to receive a consent interface on every new domain visit rather than at the intended less frequent interval.[15] We also exclude a small number of participants who reported making choices based on their expectations about the study rather than genuine privacy preferences. These restrictions ensure a consistent sample across both survey and organic phases.

Table 1 presents descriptive statistics for the main sample. We have a balanced sample between men (55%) and women (45%), with an average age of 38 years. The median household income is $50,000-$74,999, with substantial variation, including 11% of households with an annual income above $150,000. Participants visited an average of 53 unique domains prior to the study. During the study week, participants visited an average of 53 unique domains (excluding the survey-assigned websites), suggesting minimal change in their browsing behavior in response to being part of the study. Our consent interfaces appear on 41.9% of the organically visited domains.

TABLE 1. Summary Statistics

		Mean	Median	Std. Dev.
During Survey	Unique Domains in Prior Week	53.27	49.00	39.69
	Domains w. Banner	19.67	20.00	0.82
Post-Survey	Domains w. Banner	22.40	15.00	21.94
	Unique Domains Visited	53.45	38.00	49.64
	Unique URLs	653.93	385.00	799.79
	End Survey Completed	0.86	1.00	0.35
Demographics	Age	37.97	36.00	12.82
	Female	0.45	0.00	0.50
	Bachelor's or Above	0.52	1.00	0.50
	Income > $75,000	0.44	0.00	0.50
Cookie Behavior	Accept-All Rate	0.54	0.64	0.36
	Close-Window Rate	0.26	0.14	0.32
	Reject-All Rate	0.16	0.00	0.28

Notes: The table shows user-level descriptive statistics for the final study sample. Number of observations: 563.

We verify effective randomization of consent banners in two ways. First, we run a proportion test on the distribution of banner designs per website. The proportion test across the survey websites has a p-value of 0.96, which fails to reject the null hypothesis of balanced proportions across treatments. Second, we perform covariate balance tests

[15] Approximately 3% of users were not assigned to the frequency condition and saw banners for every new domain throughout the organic phase.

by regressing user- and domain-level covariates on treatment conditions (Appendix Table C.3). We find no statistically significant difference across designs.

3. Experimental Results

In this section, we present reduced-form evidence on the causal effects of choice architecture, explore heterogeneity in privacy choices across users and domains, and examine participants' beliefs about cookie tracking. We find that the majority of users accept all cookies when presented with a baseline interface, and designs that increase choice friction significantly shift consent behavior. While domain-level factors such as popularity have modest effects, user-level heterogeneity is substantial. Survey responses indicate that participants generally understand cookie functionality and the consequences of data sharing, suggesting that consent decisions are reasonably informed. However, beliefs vary considerably regarding default settings when they close the consent banner without making an explicit choice. We conclude by quantifying the time costs of cookie preference decisions.

3.1. The Effect of Choice Architecture on Data Sharing Choices

Figure 2 presents the choice distribution across treatment conditions, separately for the survey (Panel *A*) and organic phases (Panel *B*). We highlight three findings. First, participants share their data with websites more than 50% of the time absent deliberate nudging, with accept-all rates at 65% during the survey phase and 61% during the organic phase.[16] The exception is the banner condition where "accept all" is hidden from the main screen, where 19% of consumers or less during both phases choose to accept all cookies through "cookie settings."

Second, granular choices, defined as accepting only specific cookie categories, are infrequent across all conditions. Granular choices occur at a rate from 3% in the baseline interface to 8% when "reject all" is deliberately hidden during the survey phase.

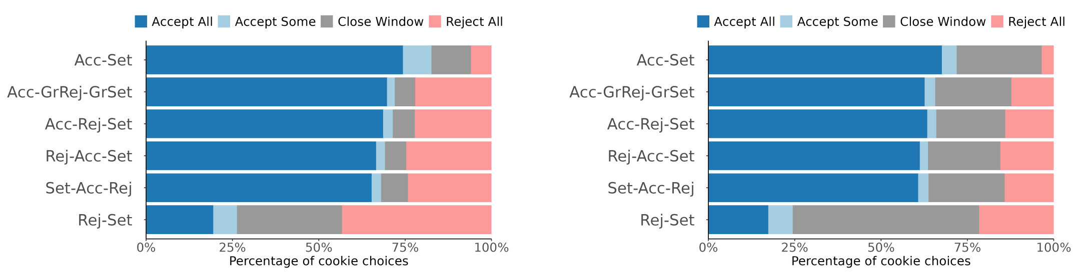
Third, participants respond similarly to choice architecture across both survey and organic phases, with one key exception: they close consent windows more frequently during organic browsing. This difference in behavior highlights the role of attention over

[16]Our acceptance rates are high but lower than prior evidence in lab studies that also examine privacy choices absent nudging designs, which document the probability of choosing "accept all" to be 77% (Vásquez Duque 2024) to 83% (Bielova et al. 2024). Existing reports on ATT consent rates are often lower, at around 44% as of 2024; see: https://www.appsflyer.com/company/newsroom/pr/att-data-findings/. However, Apple uses wording that discourages sharing (Baviskar et al. 2024), and we suspect that some mobile data (such as location) can be more sensitive compared to web behavior data.

repeated consent prompts and the importance of default settings when users organically browse the web.

Despite different propensities to close the consent banner, incorporating users' beliefs about what happens when they close the window (discussed in Section 3.2) reveals similar underlying preferences across the two phases. After imputing passive choices using belief data, we estimate that 63% of users in the survey phase and 62% in the organic phase intend to accept cookies. Similarly, 30% in both phases intend to reject them. Together, these findings underscore two points. First, survey-based choices, though potentially more artificial, still reliably capture choice architecture effects. Second, default settings play a critical role when users shift their attention from privacy decisions to other browsing activities.

FIGURE 2. Cookie Choices by Experimental Condition



A. Survey Choices

B. Organic Choices

Notes: This figure displays the proportions of cookie choices across banner design treatments. The possible choices are: accept all cookies, accept some cookies (i.e., a user clicks on settings and selects a subset of cookie types), close window (i.e., the user clicks on the *X* of the banner to close it), and reject all cookies. "Accept all" includes instances where a participant clicks into the "settings" page and manually selects all cookies. "Reject all" is similarly defined. Each row corresponds to a treatment condition. The mapping of the labels to each interface is presented in Figure 1.

To quantify the causal effects of banner design on consumer choice, we estimate the following type of regressions:[17]

$$y_{ij} = \beta_{acc-set_{ij}} + \beta_{acc-grayrej-grayset_{ij}} + \beta_{acc-rej-set_{ij}} + \beta_{rej-acc-set_{ij}} + \beta_{rej-set_{ij}} + \mu_i + \nu_{c(j)} + \epsilon_{ij}. \quad (1)$$

Here, i denotes the participant and j denotes the website. We include participant fixed effects μ_i and website category fixed effects $\nu_{c(j)}$, where categories are obtained using

[17] Our treatments often combine different types of manipulations into a single interface. For example, "acc-set" simultaneously prioritizes "accept all" and hides "reject all." Appendix A shows the separate effects of individual choice architecture (re-ordering, obstruction, and highlighting) on consumer choices.

large language models.[18] Each β coefficient measures the effect of a specific treatment condition relative to the baseline interface (Condition C in Figure 1).[19]

TABLE 2. Cookie Choices by Experimental Condition

	Survey			Organic		
	Accept All (1)	Reject All (2)	Close Window (3)	Accept All (4)	Reject All (5)	Close Window (6)
Acc-Set	0.083***	-0.178***	0.039***	0.054***	-0.094***	0.019
	(0.013)	(0.015)	(0.009)	(0.012)	(0.013)	(0.013)
Acc-GrRej-GrSet	0.035***	-0.017	-0.012	0.031**	-0.016	-0.020*
	(0.010)	(0.010)	(0.006)	(0.011)	(0.008)	(0.010)
Acc-Rej-Set	0.020	-0.007	-0.010	0.038**	-0.001	-0.041***
	(0.011)	(0.009)	(0.006)	(0.011)	(0.007)	(0.010)
Rej-Acc-Set	0.003	0.014	-0.012*	0.004	0.020*	-0.023*
	(0.010)	(0.010)	(0.006)	(0.011)	(0.009)	(0.010)
Rej-Set	-0.464***	0.193***	0.233***	-0.427***	0.086***	0.302***
	(0.020)	(0.017)	(0.018)	(0.024)	(0.012)	(0.023)
Benchmark group mean	0.65	0.24	0.08	0.61	0.14	0.22
R^2	0.646	0.579	0.562	0.571	0.522	0.494
Observations	11,075	11,075	11,075	12,610	12,610	12,610
Participant fixed effects	✓	✓	✓	✓	✓	✓
Domain Cat. fixed effects	✓	✓	✓	✓	✓	✓

Notes: Regression results of Equation 1 for three outcomes: accept all cookies, reject all cookies, and close the window without making a choice. The results are presented separately for two different sets of choices: survey choices (columns 1 through 3) and organic choices (columns 4 through 6). Standard errors clustered at the participant level. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

Table 2 displays our main results, with standard errors clustered at the participant level. We focus on three outcomes: accepting all cookies, rejecting all cookies, and closing the window without choosing, and separate the analysis for survey (columns 1 through 3) and organic choices (columns 4 through 6).[20]

Treatments involving deliberate obstruction are the most effective at steering consumer choices. Hiding "reject all" (the *Acc-Set* treatment) decreases rejection rates by 17.8 percentage points (a 74% decline). Acceptance rates increase by 8 percentage points in

[18]We use ChatGPT 4o to classify the websites via the following prompt: "Classify the website domains listed below into one of the following major categories (and only one of the following, do not include categories not in this list and try to limit how often other is selected): 'Reference Website', 'Entertainment Website', 'Business Website', 'E Commerce Website', 'Adult Website', 'News and Portals Website', 'Recreation Website', 'Banking Website', 'Government Website', 'Political Website', 'Other'. Each domain is separated by '+ – – – +'. Please always return a 10-element sequence of classifications separated by '– – –'. The list of domains is: [the list of domains follows]."

[19]Relative to the pre-registered specification, we have changed the comparison benchmark to be a design without nudges.).

[20]Given its small share, the analysis that focuses on granular choice as the outcome is left to Appendix Table C.6.

the survey and 5 percentage points in the organic phase. Closing the window increases modestly in the survey and remains flat in the organic phase.[21]

Removing the "accept all" option (the *Rej-Set* treatment) has even bigger effects, given that acceptance is the most frequent choice at baseline. Indeed, this treatment decreases acceptance rates by 46 percentage points in the survey phase (a 71% decrease) and by 43 percentage points in the organic phase (a 70% decrease). In the survey, this leads to large shifts toward both rejecting cookies (up 19 percentage points) and closing the window (up 23 percentage points). In the organic phase, the dominant response is again to close the window, which increases by 30 percentage points, more than doubling the baseline close rate.

Visual manipulations produce modest effects, concentrated in the organic phase, perhaps because users pay less attention to cookie decisions while naturally browsing the web. In the organic phase, placing "accept all" at the top (*Acc-Rej-Set*) increases acceptance rates by 3.8 percentage points (a 6% increase), while placing "reject all" at the top (*Rej-Acc-Set*) slightly increases rejections (up 2 percentage points, a 14% increase); neither design has a meaningful effect in the survey phase. The treatment that highlights "accept all" and ranks it on top (*Acc-GrRej-GrSet*) modestly increases acceptance in the organic phase by 3.1 percentage points, but this effect is indistinguishable from the effect of simply placing "accept all" at the top without additional visual cues. Most of the substitutions appear to come from users who would otherwise have closed the window.

Appendix Table C.6 shows that consumers rarely make granular cookie choices unless prompted by design interventions. In the baseline condition, only 3% of users accept a subset of cookie categories. However, hiding either the "accept all" or "reject all" option from the main screen nudges users to explore the settings menu, increasing the likelihood of granular choices. Among those making selective choices, 83% accept cookies for *preferences and functionality*, while only 7% consent to *ad selection, delivery, and reporting* (see Appendix Table C.5). This pattern suggests that targeted advertising represents consumers' least preferred data use, at least among users who selectively consent to cookie tracking.

These results point to three conclusions. First, users often accept cookie tracking when deliberate nudging is absent. Second, obstruction-based choice architecture is more

[21]Our consent banners that incorporate deliberate obstruction also position specific options more prominently, making it difficult for the regressions in Table 2 to isolate the effects of each individual choice architecture element. Appendix Table A.1 addresses this by providing separate estimates, and shows that the effects in the main analysis stem primarily from the deliberate obstruction component.

effective than visual manipulation.[22] Third, dismissal of consent windows without an active choice occurs frequently, even more so in the wild than in survey-based settings.

3.2. Heterogeneity across Websites and Consumers

In addition to the effects of choice architecture, we examine how cookie choices vary across websites and individuals to assess the potential competitive implications of consent design. Prior work shows that privacy decisions are highly context-dependent (Nissenbaum 2004; Lin 2022), and our results extend this literature by documenting substantial user-level heterogeneity even holding contexts fixed. Overall, 68.9% of participants change their cookie-sharing decisions across sites at least once. This variation reflects a combination of choice architecture effects and systematic variation in preferences across websites and users.

Website-Level Heterogeneity. We examine whether consent behavior varies across websites by content category, prior familiarity, and domain popularity, and whether choice architecture amplifies these differences. Figure 3 displays consent rates by site category, using e-commerce as the reference group. The estimates correspond to the category fixed effects from Equation 1, where the outcome is "accept all." Users are substantially less likely to accept cookies on adult, political, and government websites. This pattern suggests that participants are more hesitant to share data with websites perceived as more sensitive compared to those in the e-commerce and entertainment categories.

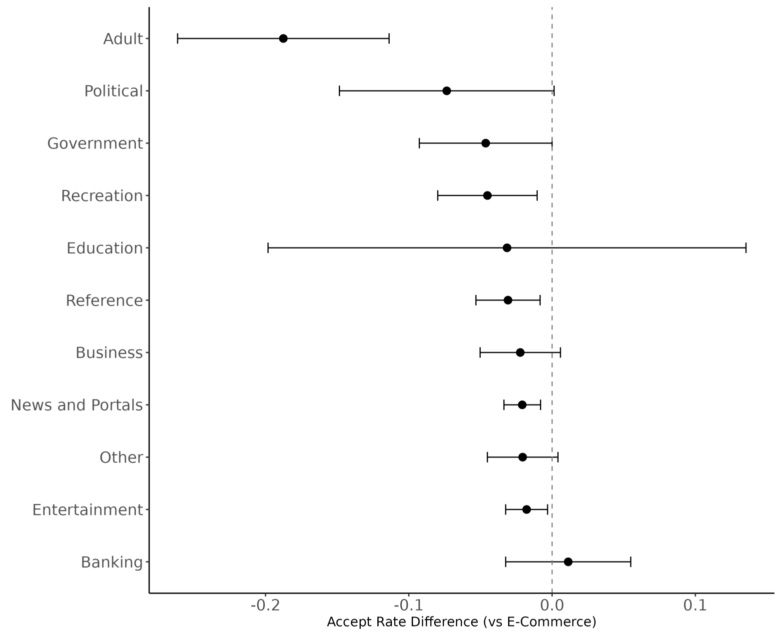
Next, we examine how a user's familiarity with a website or the website's popularity shapes consent decisions. We draw on two data sources to measure participants' familiarity with each site. First, we use participants' stated reports about whether they had heard of and regularly visited each of the 20 assigned survey websites. Second, we use browsing history data to determine whether a given domain had been visited in the two weeks prior to enrollment in the study. To capture site popularity, we use log domain ranks from Tranco,[23] which aggregates rankings from multiple public sources.

To examine how cookie choices vary with user familiarity, we add explanatory variables to Equation 1 and report their coefficient estimates in Table 3. Panel *a* focuses on the survey phase, using self-reported measures of website experience. Participants are 3

[22]These causal effects are broadly in line with existing findings in artefactual or survey experiments. For instance, Habib et al. (2022) compare a design where the reject option is hidden with a design where rejecting is the default, and find a sizable difference in choices among the two groups. Both Utz et al. (2019) and Vásquez Duque (2024) examine the effect of differential salience designs and find minimal effect on choices.

[23]https://tranco-list.eu/.

FIGURE 3. Consent Rates by Site Category



Notes: Differences in cookie acceptance rates by website category. The plot shows the estimated category fixed effects from Equation 1, where the outcome is "accept all" and the baseline category is e-commerce websites. Bars denote 95% confidence intervals.

percentage points more likely to accept cookies on websites they have heard of, and an additional 7 percentage points more likely on websites they normally visit.

Panel *b* combines data from both the survey and organic phases, using observed behavioral proxies for experience. Participants are 1.8 percentage points more likely to accept cookies on websites they had visited prior to the experiment. Additionally, domain popularity is associated with greater willingness to accept cookies, suggesting greater willingness to share data with well-known sites.

Together, these results confirm that user familiarity, whether self-reported or inferred from usage data, is associated with higher cookie acceptance rates. This is consistent with our open-ended survey responses, in which participants frequently cite trust in the website or brand as a motivation for acceptance.

We see limited heterogeneity in the effectiveness of choice architecture across site popularity and familiarity (see Appendix Figure C.3 and Appendix Tables C.7 and C.8). If anything, nudges appear to dampen participants' tendency to share more data with popular and familiar websites, suggesting they are unlikely to reinforce data-driven competitive advantages.

TABLE 3. Heterogeneity in Cookie Choices Across Websites and Users

	Accept All (1)	Reject All (2)	Close Window (3)
Panel a: Experience based on survey answers (survey data only)			
Normally Visit	0.071***	-0.072***	-0.010
	(0.009)	(0.010)	(0.006)
Heard Of	0.026**	-0.026**	-0.002
	(0.009)	(0.009)	(0.006)
R^2	0.652	0.587	0.562
Observations	11,075	11,075	11,075
Panel b: Experience based on browsing history and site popularity			
Pre-Exp Visit	0.018*	-0.031***	0.011
	(0.008)	(0.006)	(0.006)
Domain Rank (Log 10)	-0.010***	0.002	0.007***
	(0.002)	(0.002)	(0.002)
R^2	0.518	0.470	0.422
Observations	23,685	23,685	23,685
Panel c: User characteristics			
Bachelor's or Above	-0.086	0.017	0.056
	(0.044)	(0.034)	(0.039)
Income > $75,000	-0.004	-0.001	0.001
	(0.031)	(0.023)	(0.023)
Age	0.001	-0.001	-0.002**
	(0.001)	(0.001)	(0.001)
Female	-0.083**	0.048*	0.042
	(0.029)	(0.023)	(0.023)
Prior Domains Visits (Log)	-0.012	-0.013	0.019*
	(0.013)	(0.011)	(0.009)
R^2	0.147	0.069	0.122
Observations	22,460	22,460	22,460
Panel d: Beliefs about privacy			
Privacy: Meaningful Decision	0.025	-0.005	-0.030
	(0.035)	(0.028)	(0.024)
High Value Privacy	-0.013	0.070*	-0.078
	(0.049)	(0.028)	(0.043)
Outtake Missing	-0.060	0.074	-0.047
	(0.057)	(0.041)	(0.047)
R^2	0.135	0.063	0.113
Observations	23,685	23,685	23,685

Notes: Regression results of Equation 1, in which we add explanatory variables to explore heterogeneity in cookie tracking choices across websites. In Panel *a*, we add two dummies to indicate whether the study participant has heard of the website and whether the study participant normally visits the website (both questions are answered as part of the intake survey, so we only include consent choices during the survey phase). In Panel *b*, we aggregate across browsing and survey data, allowing for a level shift between the two. The dimensions of heterogeneity are proxied by a dummy to indicate whether the study participant visited the website in the two weeks preceding the study (we obtain this information by collecting their Chrome browsing history) and the website's popularity rank (in logs) from Tranco. Panel *c* includes user characteristics (age, gender, income, education, and the number of prior domain visits from their browsing history). Panel *d* focuses on self-reported and observed privacy attitudes. We include indicators of participants' beliefs about the meaningfulness of privacy decisions, whether they highly value privacy, and whether they left the study before completing the final survey. Regressions in Panels *c* and *d* require us to exclude individual fixed effects. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

Individual-Level Heterogeneity and User Beliefs. Next, we examine variations in data-sharing preferences across users. While earlier sections documented heterogeneity in behavior across websites, Panel c of Table 3 shifts the focus to user characteristics as predictors of privacy choices. Overall, most demographic factors have limited predictive power, but some meaningful patterns emerge. Women are modestly less likely to accept all cookies, and older participants are slightly less likely to close the banner without making a choice. Users who browse the internet more frequently are more likely to close consent windows.

While these patterns suggest some demographic tendencies in privacy behavior, they raise a further question: to what extent are users' choices affected by an understanding of what cookies do and how they affect data sharing? A common concern in studies of online privacy behavior is that users may lack the information needed to make meaningful choices. To address this, our endline survey asked participants to explain their reasons for accepting or rejecting cookies during the study, and to describe their broader beliefs about the consequences of data sharing.

Survey responses suggest that participants made reasonably informed decisions. Among those who accepted all cookies, the most common motivations were website trust (38.6% reported "It was a website I totally trusted"), convenience (21.9%: "It was just easier to do so. I didn't want to have to spend time looking through my options every time I opened a new site"), and functionality (17.6%: "To make the websites function well"). Cookie rejection reflected similar reasoning: participants cited concerns about unfamiliar or untrustworthy websites (26.2%: "Did not trust site"), tracking or advertising concerns (7.4%: "I didn't want to be tracked by advertisers on that website"), and general privacy preferences (13.2%: "in some website i felt not to accept some cookies because i may not want the website to have my data").

When asked about the benefits and costs of sharing data (regardless of their preferred choice), participants demonstrated an intuitive grasp of the core trade-offs. Roughly 70% identified personalized ads and 15% identified better site functionality as benefits, while over 75% cited privacy loss (43%), security risks (18%), or increased advertising (15%) as key concerns. Only less than 4% reported being unsure about what cookies do.

However, participants held varying beliefs about what would happen if they dismissed consent banners. 52.2% believed that closing the banner using the "X" button meant rejecting cookies, while 21.7% believed it meant accepting cookies. The remaining respondents expressed uncertainty. Given that U.S. websites typically default to collecting cookies, most users misunderstood actual website practices. We formally account for these belief differences in our structural model and welfare calculation in the next section.

Stated preferences for privacy are weakly correlated with revealed preferences. Panel d of Table 3 shows that participants who say they highly value privacy are 7 percentage points more likely to reject all cookies. Still, the majority of coefficients are indistinguishable from zero.[24]

Despite observable characteristics explaining some of the variation in cookie decisions, a large share of the variation remains unexplained. To disentangle user-driven preferences from domain-specific effects, we estimate a random effects model in which we regress the probability of accepting all cookies on treatment indicators, participant random effects, and domain random effects, both with and without the covariates used in the panels of Figure 3. Appendix Table C.11 shows that the standard deviation of the participant random effect is more than five times greater than that of the domain effect, even after adjusting for all covariates. This highlights that individual user differences contribute far more to cookie acceptance behavior than differences between websites.

3.3. The Time Cost of Consent

A common criticism of consent-based privacy regulations is that repeated consent banners degrade user experience and impose time costs. Here, we quantify these time costs and how they vary across interface designs.

We measure time spent on consent banners as the elapsed time between banner appearance and final action, including intermediate clicks and any back-and-forth interactions. We censor the time spent at 60 seconds, well above the 99th percentile, to exclude likely task-switching behavior rather than genuine interaction time.

Like in Section 3.1, we estimate Equation 1 but instead use the time spent interacting with the banner as the outcome. Table 4 displays the results. In the baseline design condition, consumers on average spend 5.25 seconds per banner in the survey phase (column 1) and 7.34 seconds in the organic phase (column 2). The only design that substantially increases interaction time is the *rej-set* design, which hides "accept all," the most commonly chosen option.[25]

Additional clicks substantially increase time costs. Table 5 shows that accessing the settings menu, required when preferred options are hidden, accounts for over 50% of average banner interaction time. This finding explains why hiding options behind the

[24] Additional analyses of survey-based privacy preferences and decisions are presented in Appendix Tables C.10 and C.9).

[25] This additional time cost may stem from user confusion or unfamiliarity, since interfaces where "accept all" is hidden are not used by websites.

TABLE 4. Time Spent by Experimental Condition

	Survey	Organic
	Time Spent (Seconds)	
	(1)	(2)
Acc-Set	0.497	-0.524
	(0.291)	(0.382)
Acc-GrRej-GrSet	-0.144	-0.192
	(0.259)	(0.393)
Acc-Rej-Set	-0.426	0.144
	(0.234)	(0.421)
Rej-Acc-Set	-0.257	-0.363
	(0.242)	(0.379)
Rej-Set	2.125***	1.110**
	(0.281)	(0.423)
Benchmark group mean	5.25	7.34
R^2	0.193	0.120
Observations	11,075	12,565
Participant fixed effects	✓	✓
Domain Cat. fixed effects	✓	✓

Notes: Regression results of Equation 1 where the outcome is the time spent interacting with the cookie consent banner. The results are presented separately for two different sets of choices: survey choices (column 1) and organic choices (column 2). Standard errors clustered at the participant level. * $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

main screen creates substantial choice friction that deters users from their preferred choices.

To translate the time costs into monetary terms, we perform a back-of-the-envelope calculation using the U.S. average hourly wage of $36 as a baseline.[26] With users spending 7.34 seconds per domain and visiting 53 unique domains per week on average, this implies a weekly time cost of approximately $4 per user. This calculation previews the analysis in the next section, which incorporates both choice architecture effects and time costs into an evaluation of consumer welfare.

4. Structural Model and Estimation

To quantify the welfare implications of different consent choice architectures, we develop a structural model of consumer data sharing decisions that incorporates the influence

[26]The $36/hour estimate is based on the average U.S. wage reported by the Bureau of Labor Statistics (https://www.bls.gov/news.release/empsit.t19.htm)

TABLE 5. Extra Time Spent When Clicking Settings

	Time Spent (Seconds)
	(1)
User Clicks Settings	4.069***
	(0.442)
Close Window	1.825***
	(0.409)
Accept Selected	3.282***
	(0.673)
R^2	0.102
Observations	22,842
Condition fixed effects	✓
Participant fixed effects	✓
Domain Cat. fixed effects	✓

Notes: The table presents regression estimates of time spent to make a decision as a function of whether the user's action involved clicking settings. The baseline is the time users take to accept or reject cookies when those options are available on the main banner. The variable of interest (*User Clicks Settings*) is equal to 1 if a user's final choice includes partial cookie selection, if it is "accept all" when that option is hidden, or if it is "reject all" when that option is hidden. Treatment, user, and domain category fixed effects are included as controls. The observations include all choices in the survey and organic data. Standard errors are clustered at the user level. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

of consent design. The model translates observed choices into welfare measures and quantifies the costs imposed by design frictions. More importantly, it enables us to evaluate policy interventions not directly tested in our experiments, such as policies that require alternative default settings or browser-level consent solutions.

4.1. Model Setup

Given a cookie consent banner, a consumer can choose among four actions: {accept all, reject all, customize settings, close window}. We model consumers' decision utility as a function of the selected option and the frictions introduced by the design of the consent interface. Here, we use the notion of "decision utility" (Kahneman and Thaler 1991) to recognize the fact that consumers may not always choose the option that maximizes their welfare, since consumers may have mistaken beliefs and can be temporarily affected by salience manipulations. For notational simplicity, we omit consumer-specific subscripts below.

Let θ_k denote the value that the consumer assigns to data-sharing option k. Specifically, θ_{acc} corresponds to the value of accepting all cookies, θ_{rej} to rejecting all cookies (normalized to zero for identification), and θ_{set} to customizing settings.[27]

The perceived value of closing the consent banner depends on a consumer's belief about the website's data collection default. Our endline survey elicits each participant's subjective probability ρ_{belief} that closing the window results in accepting all cookies. Their perceived utility is thus $\rho_{\text{belief}}\theta_{acc}$ (recall $\theta_{rej} = 0$). For respondents who believe that closing the window means accepting cookies (52%), we set $\rho_{\text{acc}} = 1$; for those who believe it means rejecting cookies (22%), we set $\rho_{\text{rej}} = 0$. For participants who report uncertain beliefs (12%) or did not answer the belief question (14%), we estimate their beliefs as ρ_{unc} and ρ_{miss}, respectively. Under the assumption of risk neutrality, ρ_{unc} and ρ_{miss} can be interpreted as the probability that these participants assign to the event that closing the window implies full cookie acceptance.[28]

We incorporate a cost of closing the consent banner because people are more likely to make an explicit choice rather than closing the window. The cost consists of two terms: a constant term κ_1 and a time-dependent term κ_2/t_i, where t_i represents the number of days since user i's enrollment in the study (with the enrollment day counted as day 1). The time-dependent term is motivated by the fact that users increase the frequency with which they close consent banners over time (Appendix Table B.1).

Choice architecture affects decisions through two channels. First, deliberate obstruction creates friction through click costs: consumers incur an additive cost C_{set} to access the settings menu when options are hidden. Second, visual manipulations alter salience and thus the decision utility of affected options. We model their effects as multiplicative factors applied to the baseline utility. For positive baseline utilities $\theta_k > 0$, placing the "accept all" option at the top increases its utility to $(1 + \delta_{\text{top}})\theta_k$, while graying out an option reduces its utility to $(1 + \delta_{\text{gray}})\theta_k$, where $\delta_{\text{gray}} < 0$. To accommodate potentially negative baseline utilities while ensuring proper estimation of the δ parameters, we specify the salience-adjusted utilities as $\theta_k + \delta_{\text{top}}\theta_k^2$ for top placement and $\theta_k + \delta_{\text{gray}}\theta_k^2$ for graying out. This formulation ensures that salience effects scale proportionally with the magnitude of baseline utility, regardless of sign.

For ease of comparison, Table 6 summarizes the complete utility specification across all treatment conditions.

[27] Our design does not separately identify the benefit of granular cookie selection from the hassle cost of doing so. As a result, θ_{set} is the net utility of the customization benefit and the hassle cost of making granular selections.

[28] We do not apply any discount for participants who believe that closing the window results in either full acceptance or full rejection. Our data (see Section 3.1) indicate that these users substitute between closing the window and their belief-consistent action at roughly a 1:1 ratio.

TABLE 6. Decision Utility of Each Option Across Treatment Conditions

	Option			
Treatment Design	Accept All	Reject All	Customize Settings	Close Window
Acc-Set	$\theta_{acc} + \delta_{top}\theta^2_{acc}$	$-C_{set}$	$\theta_{set} - C_{set}$	$\rho_{belief}\theta_{acc} + \kappa_1 + \frac{\kappa_2}{t_i}$
Rej-Set	$\theta_{acc} - C_{set}$	0	$\theta_{set} - C_{set}$	$\rho_{belief}\theta_{acc} + \kappa_1 + \frac{\kappa_2}{t_i}$
Set-Acc-Rej	θ_{acc}	0	$\theta_{set} + \delta_{top}\theta^2_{set} - C_{set}$	$\rho_{belief}\theta_{acc} + \kappa_1 + \frac{\kappa_2}{t_i}$
Acc-Rej-Set	$\theta_{acc} + \delta_{top}\theta^2_{acc}$	0	$\theta_{set} - C_{set}$	$\rho_{belief}\theta_{acc} + \kappa_1 + \frac{\kappa_2}{t_i}$
Rej-Acc-Set	θ_{acc}	0	$\theta_{set} - C_{set}$	$\rho_{belief}\theta_{acc} + \kappa_1 + \frac{\kappa_2}{t_i}$
Acc-GrRej-GrSet	$\theta_{acc} + \delta_{top}\theta^2_{acc}$	0	$\theta_{set} + \delta_{gray}\theta^2_{set} - C_{set}$	$\rho_{belief}\theta_{acc} + \kappa_1 + \frac{\kappa_2}{t_i}$

Notes: The table reports the decision utility associated with each option (Accept All, Reject All, Customize Settings, and Close Window) across the six treatment conditions. Although not explicitly indicated with subscripts, the parameters ($\theta_{\text{acc}}, \theta_{\text{set}}, C_{\text{set}}, \delta_{\text{top}}, \delta_{\text{gray}}$) are functions of the consumer type ι. In contrast, the costs of closing the window (κ_1, κ_2) and the belief parameters $\rho_{\text{belief}} = (\rho_{\text{unc}}, \rho_{\text{mis}})$ are assumed to be constant across consumer types.

A consumer i picks the option k that maximizes their utility given the banner design on website j:

$$\underset{k\in\{acc,rej,set,close\}}{Max} U_{ijk|design_{ij}},$$

where $U_{ijk|design_{ij}} = u_{k|design_{ij}} + \epsilon_{ijk}$. The first component is the user's decision utility from option k (which varies by treatment banner design as presented in Table 6), whereas the random component ϵ_{ijk} is i.i.d. distributed according to a Type-1 extreme value distribution.

To capture the preference heterogeneity documented in Section 3.2, we estimate a latent class model (Kamakura and Russell 1989), where we allow for three latent consumer types ι with shares below:

$$s_{i\iota} = \frac{exp(\lambda_{i\iota})}{\sum_{\iota'\in\{1,2,3\}} exp(\lambda_{i\iota'})}. \tag{2}$$

We let the shares depend on the user's education and browsing activity in the week preceding the study: $\lambda_{i\iota} = \lambda^1_\iota + \lambda^2_\iota \mathbf{1}\{\text{education}_i > \text{bachelor degree}\} + \lambda^3_\iota log(1 + \text{prior domain visits}_i)$, with normalization $\lambda_3 = 0$.

Each consumer type ι has distinct preferences and choice architecture sensitivity parameters ($\theta_{acc}, \theta_{set}, C_{set}, \delta_{top}, \delta_{gray}$), while belief parameters (ρ_{unc}, ρ_{mis}) and closing window costs (κ_1, κ_2) are common across types. Conditional on a user being type ι, the probability of choosing option k is

$$P_{\iota jk} = \frac{exp(u_{\iota k|design_{ij}})}{\sum_{k\in\{acc,rej,set,close\}} exp(u_{\iota k|design_{ij}})}. \tag{3}$$

The unconditional probability of selecting option k by consumer i is thus its probability of being a certain type and the type-specific choice probability:

$$P_{ijk} = \sum_{\iota\in\{1,2,3\}} s_{i\iota} P_{\iota jk}. \tag{4}$$

4.2. Identification and Estimation

Identification of the model parameters relies on our experimental variation, which randomly assigns consent banners at the user-by-domain level. Preferences for specific data sharing options are identified from choices under the baseline treatment without deliberate nudging. Friction costs are identified by comparing choice probabilities when options are hidden versus visible on the main screen. Salience effects are identified by comparing choices between interfaces that reorder or gray out options while maintaining the same choice set. Beliefs about what happens upon banner closure are separately elicited through the endline survey, which helps us infer the costs associated with closing the window. The panel structure across dates helps pin down any time-varying component of the utility parameters.

We use Equation 4 to form our likelihood function, and estimate the model parameters by maximum likelihood using data from both the survey and organic phases. We cluster the standard errors at the participant level.

4.3. Results

Panel *A* of Table 7 presents the estimates for consumer utility parameters. Type-1 users have the highest utility from accepting all cookies ($\theta_{acc} = 2.66$), so we label them "acceptors." Type-2 users strongly dislike data sharing ($\theta_{acc} = -4.96$), so we label them "rejectors." Type-3 users ("discerners") have a moderate utility for accepting all cookies ($\theta_{acc} = 1.21$) and a greater willingness to customize settings compared to the other two groups ($\theta_{set} = 0.62$).

The friction parameters reveal that interface designs impose substantial behavioral costs. The cost of clicking on the "settings" button (C_{set}) ranges from 1.7 to 2.9 across consumer types, matching the magnitude (in absolute terms) of the utility from accepting cookies for acceptors and discerners. In comparison, visual salience effects are more

TABLE 7. Parameter Estimates Across Consumer Subsets

Parameter	Explanation	Estimates		
Panel A: Utility Parameters				
Type-Specific Parameters		Type 1	Type 2	Type 3
θ_{acc}	Utility of accepting all	2.660***	-4.960***	1.210***
		(0.157)	(0.736)	(0.085)
θ_{set}	Utility of granular choice	0.407	-0.280*	0.615***
		(0.355)	(0.118)	(0.158)
C_{set}	Cost of clicking settings	2.900***	2.650***	1.660***
		(0.200)	(0.166)	(0.116)
δ_{top}	Effect of ranking on top	0.422***	0.029	0.076
		(0.082)	(0.042)	(0.077)
δ_{gray}	Effect of being grayed out	-1.610	-4.410	-0.041
		(10.200)	(5.030)	(0.412)
Pooled Parameters				
κ_1	Utility of closing window		0.752***	
			(0.045)	
κ_2	Inclination of closing over time		-1.670***	
			(0.070)	
ρ_{unc}	Pr(accept | close) for uncertain		0.352***	
			(0.025)	
ρ_{miss}	Pr(accept | close) for missing		0.469***	
			(0.138)	
Panel B: Latent Class Probability Parameters				
λ^1	Intercept	0.573***	-0.510**	
		(0.165)	(0.194)	
λ^2	Bachelor degree or higher	-1.720***	-0.874**	
		(0.426)	(0.305)	
λ^3	Num. prior domain visits	-1.470***	-1.520***	
		(0.238)	(0.214)	
Share of Users		0.454	0.176	0.369

Notes: The table presents parameter estimates from the utility model. Panel A reports estimates for consumer utility parameters, while Panel B reports estimates for the latent model parameters. Given the parameter estimates from Panel A, we refer to Type 1 users as "acceptors," Type 2 users as "rejectors," and Type 3 users as "discerners." Standard errors are clustered at the participant level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

modest. While the signs of the parameters indicate that ranking an option on top increases its choice probability and graying out an option reduces it, the effects are not statistically significant for most types, except for the effect of top-ranking for acceptors. Overall, these results align with our reduced-form findings that obstruction dominates visual manipulation.

The cost of closing the banner without making a choice ($\kappa_1 + \frac{\kappa_2}{t_i}$) is initially high relative to rejecting cookies, estimated as -0.92 on the first day of the study. However, the closing option becomes increasingly attractive over time as the time-dependent component $\frac{\kappa_2}{t_i}$ increases. By day 3, closing the banner becomes more attractive than explicitly rejecting cookies. Finally, the belief parameters (ρ) suggest that respondents who are uncertain about the website closing default assign a 35% probability that closing the window implies accepting cookies, while for participants who did not complete the endline, this probability is 47%.

Panel *B* of Table 7 shows how consumer types vary across the population. Type-1 users (acceptors) form the largest group at 45% of users, followed by type-3 (discerners) at 37% and type-2 (rejectors) at 18%. Both education and prior browsing intensity significantly predict consumer type. College-educated users are the least likely to be acceptors and most likely to be discerners (as indicated by the estimates of λ^2). Similarly, users with more extensive browsing histories (higher number of domains visited) are significantly less likely to be classified as acceptors or rejectors than discerners (from the λ^3 estimates).

5. Consumer Surplus under Counterfactual Policies

The structural model estimates enable us to assess the welfare implications of various consent policy designs. Crucially, the counterfactual policy simulations allow us to separately identify the effects of three key factors: manipulation of available options on the consent screen, the role of defaults, and the frequency with which users are asked to consent. We find that choice frictions significantly deter users from selecting their preferred option, justifying policies that reduce these frictions. One approach is to mandate interfaces free of deliberate obstruction. In these cases, the website's default behavior becomes critical, as many users close the banner without making a choice. We therefore compare designs that default to either accepting or rejecting cookies upon banner closure. A different policy, browser-level consent, reduces the frequency of consent decisions from the status quo of site-by-site consent. This policy, also known as *global privacy control*,[29] reduces interaction costs but limits customization, making its welfare impact ex-ante ambiguous.

[29] See https://globalprivacycontrol.org for details.

We evaluate consumer welfare under the following policies:

1. **U.S. status quo**: An "acc-set" design, with websites defaulting to collect all cookies when the user does not make an explicit choice;
2. **EU norm**: An interface without deliberate nudging ("set-acc-rej"), with websites defaulting to no cookie tracking upon inaction;
3. **Optimal Banner Interface**: An interface without nudging ("set-acc-rej"), with the default set to collect all cookies upon inaction;
4. **Optimal Banner Interface with Correct Beliefs**: An interface without nudging ("set-acc-rej"), with the default set to collect all cookies upon inaction, and consumers with correct beliefs about the default settings;
5. **Global Accept**: A browser-level choice that automatically accepts all cookies across websites;[30]
6. **Global Reject**: A browser-level choice that automatically rejects all cookies across websites;
7. **Global Privacy Control**: A browser-level setting that enables users to make one choice that applies to all websites, with options to accept all cookies, reject all cookies, or apply a single customized selection across all websites.

For site-by-site consent policies (policies 1-4), the welfare calculation must account for two sources of distortion: consumers' potentially incorrect beliefs about defaults, and the discrepancy between actual (aka. the "welfare utility") and perceived utility (aka. the "decision utility") induced by visual manipulations. To this end, we allow choice probabilities to be guided by consumer beliefs and salience parameters, but compute the actual utility ignoring salience parameters and using the actual website default upon consumer inaction. Based on Train (2015) and given our latent-type model, the average consumer surplus per person is as follows:

$$CS(\text{banner-level policy}) = \frac{1}{N}\sum_i \sum_\iota s_{i\iota}\left[log\left(\sum_k e^{u_{ik\iota\,|policy}}\right) + \left(\sum_k P_{ik\iota\,|policy} \cdot d_{ik\iota\,|policy}\right)\right].$$

In this equation, $\iota \in \{1, 2, 3\}$ denotes the latent consumer type, and $k \in \{acc, rej, set, close\}$ denotes the choices. The first term captures standard logit consumer surplus based on perceived utilities $u_{ik\iota|policy}$, which vary across individuals due to latent types and beliefs about default settings. $P_{ik\iota\,|policy}$ is the choice probability for option k, and $d_{ik\iota\,|policy}$ is the difference between the actual and the perceived utility. The difference arises from two sources: mistaken beliefs over the default upon closing the window (ρ_{belief}) and salience effects when consent banners visually vary how options are displayed (δ_{top} and δ_{gray}).

[30]Note that global accept and reject policies could be interpreted as the extreme case when global privacy control is given but hidden (thus hard for consumers to choose), and websites default to collecting cookies.

Under the U.S. status quo, utilities are based on the banner that hides "reject all." Since websites default to data sharing when users close windows, we adjust welfare for consumers who believe the default is otherwise. We also correct for δ_{top}, which increases the probability of selecting "accept." Under the EU norm, utilities are based on the banner without any manipulations, with a no-sharing default. Therefore, we adjust the welfare calculations for the consumers who think the default may result in cookie acceptance. The optimal banner design combines the EU's manipulation-free interface with the U.S. default of data sharing upon inaction. Finally, correcting consumer beliefs ensures that individuals who close the banner understand its consequences, i.e., acceptance of all data sharing.

Browser-level policies (policies 4-6) eliminate repeated banner interactions but restrict site-by-site customization. Consumer surplus becomes:

$$CS(\text{browser-level policy}) = \frac{1}{N}\sum_i \sum_\iota s_{i\iota} u_{ik^*\iota|policy} + V_{\text{banner time}},$$

where $k^* = \text{acc}$ for global accept, $k^* = \text{rej}$ for global reject, and $k^* = \arg\max_k u_{ik\iota}$ for individualized global privacy control. The consumer surplus reflects only the mean utility of the selected option, ignoring idiosyncratic preferences since users make choices before interacting with specific websites. In computing the surplus, we also assume that browser-level interfaces are free of nudging patterns, and consumers cannot close the browser-level consent without making a choice.

The consumer surplus from global privacy choices thus reflects three components: utility loss from the inability to customize cookie preferences for each website ($u_{ik^*\iota|policy} < log\left(\sum_k e^{u_{ik\iota\,|policy}}\right)$); gains from avoiding repeated interactions with banners ($V_{\text{banner time}} > 0$); and the absence of choice distortions induced by choice architecture ($\sum_k P_{ik\iota\,|policy} \cdot d_{ik\iota\,|policy} < 0$). To calculate the utility of time saving from bypassing banner interaction, we assume that the (dis)utility is linear in time spent, and use the C_{set} estimate to back out this utility term: $V_{\text{banner time}} = C_{\text{set}} \times$ (time spent per banner/time to click "settings").[31]

Lastly, to express welfare estimates in dollar terms, we need to find a way to map utility to monetary values. This translation is inherently challenging, as our data does not have consumers' willingness to pay for privacy or to avoid additional clicks. To address

[31] For time spent per banner, we use the average interaction time during the organic phase under the baseline design, which reflects the time cost of making a choice absent deliberate obstruction. Note that by invoking the assumption that disutility is linear in time spent, we ignore annoyance costs that may also be induced by deliberate obstruction and the banners themselves (see Bernheim, Kim, and Taubinsky (2024) for a comprehensive discussion), knowing that this term is harder to quantify in dollar terms.

this challenge, we use participants' self-reported annual household income to estimate their dollar value of time spent clicking settings.[32]

Specifically, we define the monetary value of the clicking cost (C_{set}) as the average time spent clicking on settings multiplied by a calibrated opportunity cost of time. Table 5 shows that users take 4.07 additional seconds per domain when clicking on settings. To get the opportunity cost of time, we use participants' self-reported annual household income to assign hourly wage values based on bracket midpoints. We then multiply this value by the time it takes to click on settings per domain and the number of unique domains visited per week to convert C_{set} into a weekly dollar value. For example, a participant in the \$75–100k income bracket (\$42 implied hourly wage) would incur a weekly time cost C_{set} of \$2.6, calculated as (\$42/hour) × (4.07/3,600 hours/domain) × 53 domains. We then use the ratio between the dollar-value opportunity costs of time and C_{set} to convert consumer welfare values to dollars.

Table 8 reports our consumer surplus estimates in dollar terms. The first column displays results averaged across types, while the remaining columns highlight the heterogeneity among the three consumer types.[33] Although the per-user-week effects appear modest in absolute terms, they represent meaningful welfare changes that compound substantially over time.

TABLE 8. Consumer Surplus Under Counterfactual Policies ($, Weekly)

Counterfactual	Average	Type 1 Acceptors	Type 2 Rejectors	Type 3 Discerners
U.S. Status Quo	2.03	2.69	-2.63	3.43
EU Norm	2.58	2.66	0.91	3.28
Optimal Banner Interface	2.65	2.92	-0.12	3.63
Optimal Banner Interface (with correct beliefs)	2.89	3.1	0.6	3.73
Global Accept	5.81	6.94	0.37	7.01
Global Reject	4.98	4.76	5.03	5.24
Global Privacy Control	6.63	6.94	5.03	7.01

Notes: The table reports consumer surplus per week per user in dollar units under various counterfactual policies. The U.S. status quo refers to an *acc-set* interface, combined with an "accept all" default when consumers close window; the EU norm refers to an interface without nudging designs with a "reject all" default when consumers close window; the optimal banner interface refers to an interface without nudging with an "accept all" default when consumers close window; the optimal banner interface with correct beliefs ensures that people closing the window have correct beliefs about default settings. "Global Accept" and "Global Reject" force each individual to either always accept or always reject all cookies. The last row, "Global Privacy Control," allows each individual to make their preferred global choice. The average column refers to the estimate across all subjects, and the other columns correspond to estimates by user type.

[32]During the intake survey, participants report their household income over the past 12 months, selecting from six predefined brackets: \$0–25k, \$25–50k, \$50–75k, \$75–100k, \$100–150k, and \$150k or more.

[33]Appendix Table C.12 reports welfare in utility units.

We start by comparing consent-based policies. Since consumer welfare is estimated up to a constant (Train 2009), we only discuss level differences. The difference between the U.S. status quo and the optimal banner interface reflects the impact of choice architecture, while the difference between the EU norm and the optimal banner interface reflects the impact of default settings. The optimal banner interface, which is a no-nudging design with an accept-all default, yields an average consumer surplus of $2.65 per person-week. Relative to the optimal banner interface, the U.S. status quo delivers $0.6 lower consumer surplus per user-week, with much larger losses for type-2 users ("rejectors"). Correcting beliefs about default settings boosts surplus by $0.2 per user-week, beyond the gains from removing deliberate obstruction alone. Rejectors benefit the most, since they no longer believe that closing the banner rejects cookies.

The EU Norm, defined as a no-nudging interface paired with a reject-all default, results in a small difference ($0.1 lower consumer surplus per week) compared to the optimal banner interface. Defaulting users to rejecting cookies has two opposing effects on consumer welfare. On one hand, it may improve outcomes for users who expect or prefer a reject-all default. On the other hand, it may reduce welfare for the majority of users who prefer to share data. Our analysis suggests that the net effect is slightly negative across consumers, though it benefits the rejectors over alternative policies, who gain from having the default aligned with their preferences. The EU norm also outperforms the U.S. status quo, increasing average surplus by $0.6 per user-week, due to its use of a baseline design that reduces choice friction.

Remarkably, all global privacy control policies (last three rows in Table 8) substantially outperform even the optimal banner-based approach with correct beliefs. Acceptors and discerners benefit the most from a global accept policy, with surplus gains increasing by $3.3-3.8 per week compared to the optimal consent-based policy. Rejectors benefit the most from a global reject policy, enjoying a surplus of $5 per week compared to $0.9 under the EU norm, their preferred banner policy. Global choice benefits even the discerners, who would customize data sharing decisions when site-specific banners are given, because the time-saving benefits from avoiding repeated interactions outweigh the costs of uniform choices. The large magnitude of the time-saving gains is consistent with our reduced-form evidence, where removing an option from the main consent interface decreases selection of that option by at least 70%. On average, the surplus gain from adopting the global privacy control is $3.7 per week compared to the optimal banner interface.

We conduct two additional analyses related to global privacy control. First, we calculate the banner interaction time that would make the optimal consent-based policy (with existing consumer beliefs) welfare-equivalent to the global privacy control. This break-even point occurs at just 0.64 seconds per banner—approximately 8.7% of users' current average interaction time.

Second, we validate these policy evaluation results using stated preferences for global privacy control tools in the endline survey. 62.4% of participants say they prefer such a tool over site-by-site choice, while only 13.4% prefer the site-by-site choice (Appendix Figure C.2). The average valuation for a global privacy control tool is at $4, which falls within our estimated surplus gain moving from the U.S. status quo to global privacy control: accepters gain $4.2, rejectors gain $7.7, and discerners gain $3.6. While stated and revealed preferences need not coincide perfectly, the similarity in their magnitudes gives reassurance that our surplus estimates are sensible.

Overall, these results indicate substantial welfare gains from allowing users to specify global privacy preferences that match their individual data-sharing attitudes. Current regulatory approaches focusing on banner design improvements, while beneficial, may miss larger opportunities for welfare enhancement through architectural changes to how consent is collected and managed.

6. Discussion

The debate over privacy policy is characterized by competing interests: businesses championing increased data collection versus regulators who assume consumers prioritize data minimization. Contrary to the assumptions underlying much of this discourse, our evidence reveals that many consumers willingly share their data even without nudging.

That said, consumers' choices to share data are sensitive to how consent is presented. Deliberate obstruction, which makes privacy-preserving options harder to access, is the design pattern with the largest effects among the designs we study. Visual manipulations such as reordering or highlighting have limited effects. Our evidence suggests that even though many websites use both obstruction and visual manipulations (Utz et al. (2019)), the benefits of visual manipulations may be small.

Many consumers misunderstand what happens when they close consent banners. This confusion matters because consumers often avoid explicit choices and close consent banners instead. Ambiguity about defaults extends beyond consumers to regulatory enforcement. Data Protection Authorities across EU member states interpret user inaction differently. For example, Italy and Sweden view it as indecision while France and Luxembourg consider it rejection (Bielova, Santos, and Gray 2024).

Our welfare analysis reveals large benefits from eliminating site-specific consent banners in favor of browser-level consent management. Browser-level consent management is endorsed by the CPRA and has been implemented by Mozilla's Firefox and the Brave browser. We find this policy change would boost consumer welfare by $3.7 per user-week relative to the optimal site-specific banner and $4.6 relative to the U.S. status quo. While seemingly modest at the individual-week level, these gains scale to substantial societal benefits across all internet users.

Our revealed preference approach assumes that observed decisions reflect true utility. This assumption may not hold if users lack a complete understanding of the consequences of sharing cookies. While we address this limitation by incorporating consumers' beliefs about banner closure outcomes into welfare calculations, broader information gaps may remain. Consumers' confusion about defaults reveals a fundamental problem in digital privacy markets: consumers make consequential privacy decisions without fully understanding the stakes. While our survey results suggest that participants have a basic understanding of cookie functionality, additional information has the potential to further alter consumer privacy choices.

Our experimental methodology abstracts from a fully natural environment. Participants' awareness of being studied and their self-selection into our experiment may introduce biases that limit external validity. Additionally, our focus on short-term responses leaves the potential for longer-term adaptation due to consumer learning and inattention unexplored.

Nevertheless, our findings underscore the need for privacy policies grounded in empirical evidence of actual consumer behavior rather than assumptions about privacy preferences. As digital markets evolve, understanding how consumers actually make privacy decisions–rather than how they should make them–will prove essential for designing effective and welfare-enhancing privacy regulations.

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Appendix to "Designing Consent: Choice Architecture and Consumer Welfare in Data Sharing" by Farronato, Fradkin, and Lin

Appendix A. Effects on Consumer Choice by Type of Dark Pattern

This appendix presents reduced-form results on the effects of specific types of dark patterns on consumer choices.

To quantify the effect of dark patterns on privacy choices, we identify the presence of reordering, highlighting, and obstructions in our treatments. This means estimating the following regression:

$$y_{ij} = \gamma_{reject\ hidden_{ij}} + \gamma_{accept\ hidden_{ij}} + \gamma_{accept\ on\ top_{ij}} + \gamma_{reject\ on\ top_{ij}} + \gamma_{highlight\ accept_{ij}} + \mu_i + \nu_{c(j)} + \epsilon_{ij}, \tag{A.1}$$

where i indexes study participants and j indexes websites. We include participant fixed effects μ_i and website category fixed effects $\nu_{c(j)}$, where categories are based on Ghostery's taxonomy[34] and extended to the full set of websites in our study using ChatGPT.[35]

The γ indicators capture the specific nudges present in the consent interface for user i on website j: which option is placed at the top (accept or reject), which option is hidden (accept or reject), and whether the "accept all" option is visually highlighted. To interpret these coefficients relative to our treatment conditions in Figure 1, $\gamma_{accept\ on\ top_{ij}}$ represents the effect of placing "accept all" first, relative to the baseline interface (Treatment C), while $\gamma_{reject\ on\ top_{ij}}$ captures the analogous effect for prioritizing "reject all." The remaining γ coefficients reflect the incremental impact of additional nudges— hiding or highlighting— beyond the positioning of "accept all" or "reject all" at the top of the banner.

We focus on three outcomes: accepting all cookies, rejecting all cookies, and closing the window without making an active choice. Table A.1 presents the results. Columns 1–3 jointly capture how each type of dark pattern affects substitution across consent choices in the survey phase; columns 4–6 provide the corresponding analysis for the organic phase.

Deliberate obstruction emerges as the most effective dark pattern. Hiding the "reject all" button from the main screen reduces rejection rates by 17 percentage points in the survey phase (a 71% decrease) and by 9 percentage points in the organic phase (a 66% decrease). In the survey phase, participants shift both to accepting cookies, up 6.3 percentage

[34] https://www.ghostery.com/.

[35] We used ChatGPT 4o to classify the websites. We used the following prompt: *Classify the website domains listed below into one of the following major categories (and only one of the following, do not include categories not in this list and try to limit how often other is selected): 'Reference Website', 'Entertainment Website', 'Business Website', 'E Commerce Website', 'Adult Website', 'News and Portals Website', 'Recreation Website', 'Banking Website', 'Government Website', 'Political Website', 'Other'.*

TABLE A.1. Cookie Choices by Dark Pattern

	Survey			Organic		
	Accept All (1)	Reject All (2)	Close Window (3)	Accept All (4)	Reject All (5)	Close Window (6)
Reject Hidden	0.063***	-0.171***	0.049***	0.017	-0.093***	0.060***
	(0.013)	(0.015)	(0.008)	(0.012)	(0.012)	(0.012)
Accept Hidden	-0.467***	0.179***	0.246***	-0.431***	0.065***	0.325***
	(0.020)	(0.017)	(0.018)	(0.023)	(0.012)	(0.023)
Accept Top	0.020	-0.007	-0.010	0.038**	-0.001	-0.041***
	(0.011)	(0.009)	(0.006)	(0.011)	(0.007)	(0.010)
Reject Top	0.003	0.014	-0.012*	0.004	0.020*	-0.023*
	(0.010)	(0.010)	(0.006)	(0.011)	(0.009)	(0.010)
Highlight Accept	0.015	-0.010	-0.002	-0.007	-0.014	0.021*
	(0.010)	(0.010)	(0.006)	(0.011)	(0.007)	(0.009)
Benchmark group mean	0.65	0.24	0.08	0.61	0.14	0.22
R^2	0.646	0.579	0.562	0.571	0.522	0.494
Observations	11,075	11,075	11,075	12,610	12,610	12,610
Participant fixed effects	✓	✓	✓	✓	✓	✓
Domain Cat. fixed effects	✓	✓	✓	✓	✓	✓

Notes: Regression results of Equation A.1 for three outcomes: accept all cookies, reject all cookies, and close window without making a choice. The results are presented separately for two different sets of choices: survey choices (columns 1 through 3) and organic choices (columns 4 through 6). Appendix Table A.2 presents similar results for the decision to accept a subset of cookie types. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

points (a 10% increase), and to closing the window, which rises by 4.9 percentage points (a 61% increase). In the organic phase, the dominant response is to close the window, which increases by 6 percentage points (a 27% rise from an already high baseline). Hiding the "accept all" button has an even more pronounced effect, reducing acceptance rates by 47 percentage points in the survey phase (a 72% decline) and by 43 percentage points during organic browsing (a 71% decline). Participants shift primarily toward closing the window—53% do so in the survey phase and 75% in the organic phase—with a smaller share substituting toward rejecting cookies (38% in survey, 15% in organic).

In contrast, visual manipulations—i.e., reordering options and highlighting "accept all"—have more limited effects on user choices. Most coefficients are small and statistically insignificant, particularly in the survey phase. These nudges have somewhat larger effects during organic browsing, perhaps because users are not as focused on cookie preferences, but the magnitudes remain modest. For example, placing "accept all" at the top increases acceptance by only 3.8 percentage points (column 4, a 6% increase), while highlighting has no additional impact on acceptance. Similarly, placing "reject all" at the top marginally increases rejections by 2 percentages points (column 5, a 14% increase).

TABLE A.2. Selective Cookie Choice by Dark Pattern

	Survey	Organic
	Accept Some	
	(1)	(2)
Reject Hidden	0.059***	0.016*
	(0.010)	(0.007)
Accept Hidden	0.043***	0.041***
	(0.008)	(0.008)
Accept Top	-0.003	0.005
	(0.005)	(0.003)
Reject Top	-0.005	-0.002
	(0.005)	(0.004)
Highlight Accept	-0.004	0.000
	(0.004)	(0.003)
Benchmark group mean:	0.03	0.03
R^2	0.413	0.499
Observations	11,075	12,610
Participant fixed effects	✓	✓
Domain Cat. fixed effects	✓	✓

Notes: The table regressions of Equation A.1, where the outcome is whether the user selects a subset of cookies. Otherwise the table is identical to Table A.1. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

Table A.2 indicates that consumers tend not to make granular cookie choices, and would rather opt out of making choices altogether by closing the consent window. In the baseline condition, only 3% of participants accept a subset of cookie types; deliberately hiding either “accept all” or “reject all” options from the main screen encourages participants to check out the settings menu, increasing the probability of granular choices by 2-6 percentage points. Among those who make granular selections, 83% choose to accept cookies for *preferences and functionality*, while only 7% accept cookies for *ad selection, delivery, and reporting* (see Appendix Table C.5). This result suggests that targeted advertising is the least preferred use of consumer data, at least among the few users who make selective choices.

Appendix B. Choice Fatigue

Next, we examine whether the attention users pay to choices changes as they see more consent banners. We compare the differences in choices between our 10-minute and 60-minute treatments to show that there isn't choice fatigue when we increase the frequency of banners.

The 10-minute treatment sees our banners in 53% of the domains they visited, while the 60-minute treatment sees these banners in 30% of the domains. Given this difference, we can see whether the frequency of choice types varies between these two conditions. We estimate the effects of this treatment in the following regression specification.

$$y_{ij} = \beta_{10\ minutes} + \gamma * time\ in\ study_{ij} + \nu_{c(j)} + \epsilon_{ij}. \tag{B.1}$$

The baseline is the condition where a user sees the banner every 60 minutes, while the alternative condition displays a banner every 10 minutes. We also control for the time a user has been in the study (post-survey), since this may be correlated with their overall engagement with the study.[36]

Table B.1 displays the results. We highlight two findings. First, we do not find a differential impact of banner frequency on data-sharing choices, whether it is the acceptance rate or the inclination to close banners. Users make similar choices, whether they see a banner every 10 or 60 minutes. These null effects are precisely estimated, as the 95% confidence interval excludes effects greater than 7%. However, we acknowledge the caveat that the difference between exposing to banners 30% vs. 53% of the time may not be large enough compared to, say, comparing banner exposure between 30% and 100% of the time.

Second, time spent in the study has an effect on choice. Each additional day in the study increases the share of people closing the banner by two percentage points. Since study participants remain in the study for 7 days, this implies that they are 14 percentage points more likely to close the window at the end of the study compared to the first day.

It is tempting to directly interpret the time in the study as another measure of choice fatigue, but it is not randomly allocated and could be correlated with underlying consumer characteristics and privacy preferences. To address this concern, in Table B.2 we add individual and hour-of-the-day fixed effects, as well as control for the order of the domain visit. Even with these covariates, we see that time in the study reduces acceptance and increases close-out. The most likely explanation for this effect is that participants reduce their engagement with the study over time.

[36] Adding this covariate does not affect whether we detect any treatment effects.

TABLE B.1. Fatigue in Cookie Choices During Organic Browsing

	Accept All (1)	Reject All (2)	Close Window (3)
10 Min Pop-up	0.009	-0.005	-0.003
	(0.037)	(0.024)	(0.031)
Time in Study (Days)	-0.009*	-0.008**	0.017***
	(0.004)	(0.003)	(0.004)
Domain Rank (Log 10)	0.010**	-0.008**	-0.002
	(0.004)	(0.003)	(0.003)
Pre-Exp Visit	0.041**	-0.031*	-0.015
	(0.015)	(0.013)	(0.013)
R^2	0.008	0.007	0.009
Observations	12,610	12,610	12,610
Domain Cat. fixed effects	✓	✓	✓

Notes: This table shows estimates of Equation B.1, where '10 Min Pop-up' is an indicator for whether the user was in the treatment where banners occurred at a frequency of once every 10 minutes. $^*p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

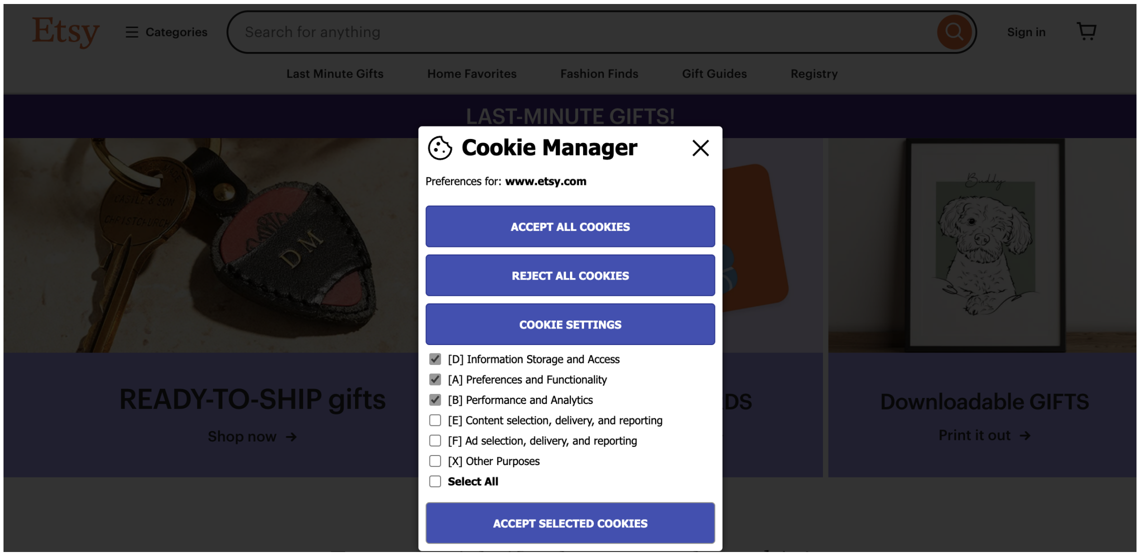
TABLE B.2. Fatigue in Cookie Choices During Organic Browsing (Additional Fixed Effects)

	Accept All (1)	Reject All (2)	Close Window (3)
Visit Order / 10	-0.020*	-0.004	0.023*
	(0.009)	(0.004)	(0.009)
Time in Study (Days)	0.000	-0.003	0.005
	(0.005)	(0.003)	(0.005)
R^2	0.469	0.501	0.439
Observations	12,610	12,610	12,610
Domain Cat. fixed effects	✓	✓	✓
Participant fixed effects	✓	✓	✓
Hour fixed effects	✓	✓	✓

Notes: This table estimates a variant of Equation B.1, which removes the banner frequency treatment and adds the order of which a domain is visited ("Visit Order") and additional fixed effects. $^*p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

Appendix C. Additional Exhibits

FIGURE C.1. Cookie Manager's User Interface



Notes: Consent interface when a user clicks "cookie settings". The available blue buttons vary by treatment: for example, in the "accept-settings" design, the blue "Reject All Cookies" button does not show up when a user clicks "settings." However, the list of specific cookies within the settings menu is identical across treatments, meaning they can always choose to reject or select all cookies upon coming to the "settings" menu.

TABLE C.1. Websites Browsed during the Survey Phase

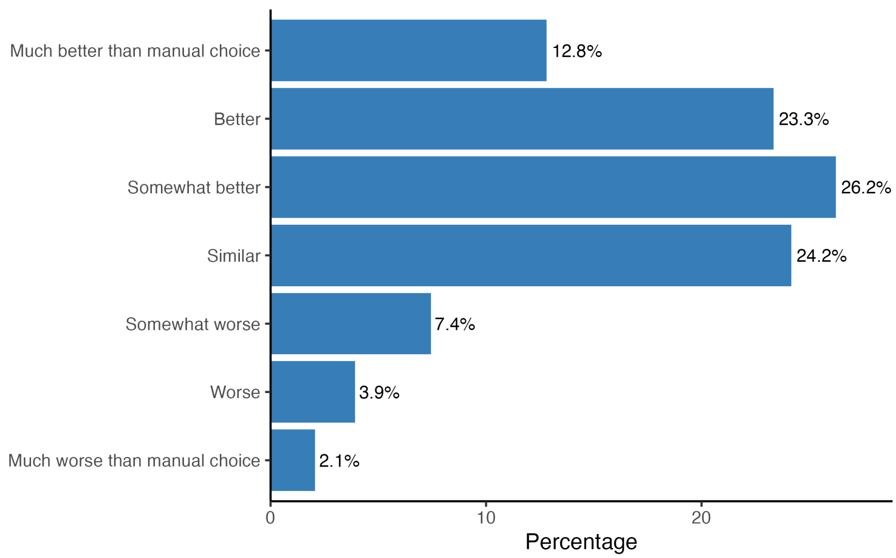
Domain	Domain Rank
facebook.com	3
youtube.com	8
amazon.com	28
yahoo.com	41
ebay.com	185
weather.com	325
duckduckgo.com	413
target.com	631
espn.com	278
etsy.com	301
nytimes.com	119
appleinsider.com	6319
seattletimes.com	3349
stockx.com	4547
funnyordie.com	16437
turo.com	16272
semafor.com	28266
thomannmusic.com	90809
truewerk.com	348372
merrysky.net	1000001

TABLE C.2. Number of Participants across the Experimental Funnel

Stage	N	Percent	10 min	Percentage	60 min	Percentage
1) Start Survey	1227	100				
2) Eligible for Study	917	74.74				
3) Study Consent	877	71.48				
4) Finished Survey	807	65.77				
5) Clicked All Links	808	65.85	359	100.00	418	100.00
6) Have Cookie Choice Data	767	62.51	350	97.49	410	98.09
7) After 15+ Domains Filter	687	55.99	316	88.02	371	88.76
8) After Mutual Presence Filter	602	49.06	282	78.55	320	76.56
9) Main Analysis Sample*	563	45.88	260	72.42	303	72.49
10) Finished Endline Survey	484	39.45	218	60.72	266	63.64

Notes: This table presents the number of study participants at every step of the study. After completing the initial survey, participants are randomly allocated to two treatment conditions: 10 minutes (where cookie banners appear every 10 minutes of browsing), and 60 minutes (where cookie banners appear every 60 minutes. Due to an implementation glitch, not all users are randomized into either the 10- or 60- minute treatment; 3% of participants kept seeing a banner for every new domain visited.
$*$: The main analysis sample in the second-to-last line restricts attention to users who have treatment assignment to either the 10-Minute or 60-Minute group, and for whom we observe at least one cookie selection both during and after the survey.

FIGURE C.2. User Preferences for Global Privacy Control



Notes: The figure plots the distribution of answers to the following question in the endline survey: Consider a tool that allows you to specify how you would like to answer cookie consent questions online. This tool will then automatically hide all cookie banners and answer them in the way you specified. For example, if you stated that you wanted to accept cookies for all websites, the tool would do so. Please select how much better or worse the tool is than manually answering the cookie consent form for each website.

TABLE C.3. Covariate Balance Check for Dark Pattern Randomization

	Age (1)	Female (2)	Bachelor's or Above (3)	Domain Rank (Log 10) (4)
Constant	38.720***	0.438***	0.182***	3.579***
	(0.196)	(0.007)	(0.006)	(0.023)
Acc-GrRej-GrSet	0.061	-0.004	-0.009	-0.034
	(0.281)	(0.011)	(0.008)	(0.033)
Acc-Rej-Set	-0.040	0.013	0.005	-0.004
	(0.281)	(0.011)	(0.008)	(0.033)
Acc-Set	-0.023	-0.001	-0.007	-0.023
	(0.280)	(0.010)	(0.008)	(0.033)
Rej-Acc-Set	0.343	0.011	-0.008	0.013
	(0.284)	(0.011)	(0.008)	(0.033)
Rej-Set	0.312	-0.008	-0.011	-0.045
	(0.286)	(0.011)	(0.008)	(0.033)
R^2	0.000	0.000	0.000	0.000
Observations	26,278	26,278	26,773	26,773

Notes: Banner design is randomized at the user X site level. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

TABLE C.4. Covariate Balance Check for Banner Frequency Randomization

	# Survey Domains (1)	Age (2)	Female (3)	Bachelor's or Above (4)
Constant	18.537***	37.460***	0.443***	0.563***
	(0.222)	(0.703)	(0.027)	(0.027)
10 Min Pop-up	0.123	1.560	0.039	-0.076$^{\cdot}$
	(0.307)	(1.037)	(0.040)	(0.039)
R^2	0.000	0.004	0.001	0.006
Observations	656	638	638	656

Notes: Banner frequency is randomized at the user level. We therefore exclude domain rank during the organic browsing, but include the number of banners exposed at the survey stage for covariate balance checks. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

TABLE C.5. Types of Cookies Accepted among People Making Granular Choices

Cookie Type	Percentage Selected
Preferences and functionality	0.826
Information storage and access	0.627
Performance and analytics	0.601
Content selection, delivery, and reporting	0.390
Ad selection, delivery, and reporting	0.070
Other purposes	0.048

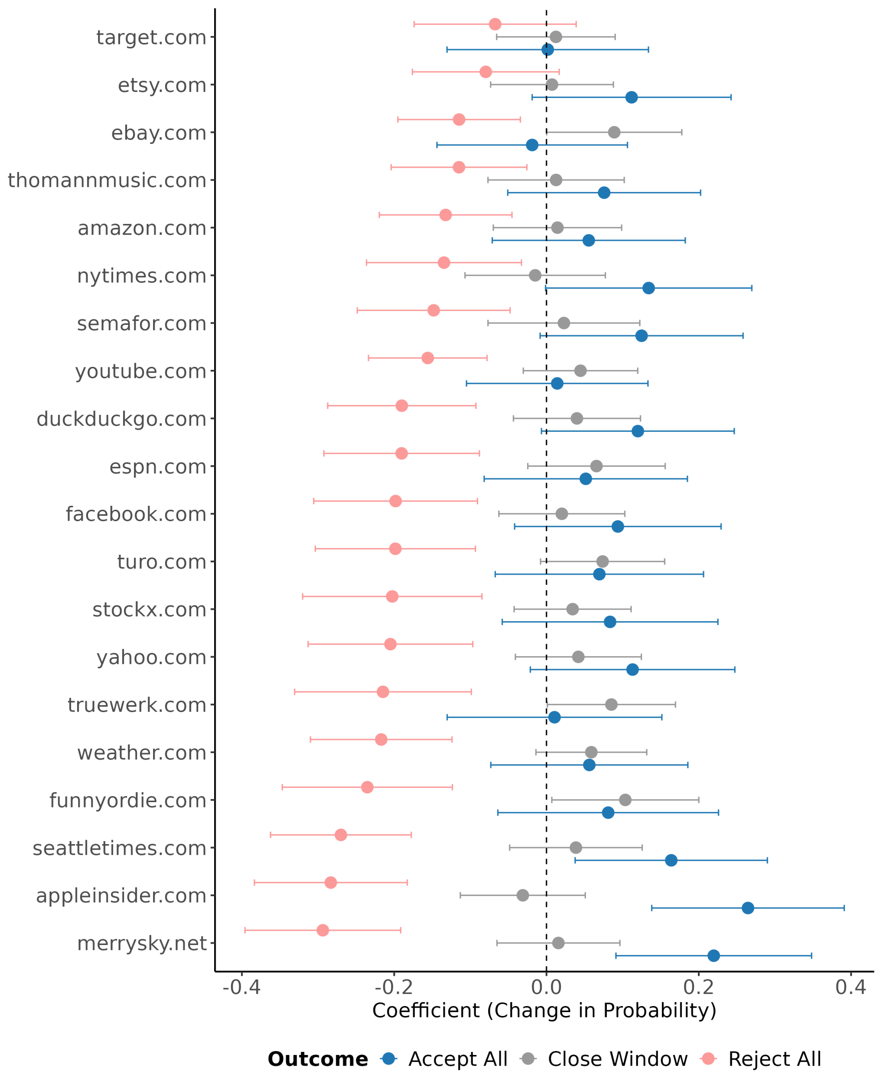
Notes: Percentage of different types of cookies selected among those who selectively accept some cookies but not all.

TABLE C.6. Selective Cookie Choice by Experimental Condition

	Survey	Organic
	Accept Some	
	(1)	(2)
Acc-Set	0.056***	0.021**
	(0.011)	(0.007)
Acc-GrRej-GrSet	-0.007	0.005
	(0.004)	(0.003)
Acc-Rej-Set	-0.003	0.005
	(0.005)	(0.003)
Rej-Acc-Set	-0.005	-0.002
	(0.005)	(0.004)
Rej-Set	0.038***	0.039***
	(0.008)	(0.007)
Benchmark group mean:	0.03	0.03
R^2	0.413	0.499
Observations	11,075	12,610
Participant fixed effects	✓	✓
Domain Cat. fixed effects	✓	✓

Notes: The table regressions of Equation 1, where the outcome is whether the user selects a subset of cookies. Otherwise, the table is identical to Table 2. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

FIGURE C.3. Treatment Effects by Survey Domain (Acc-Set vs Baseline Banner)



Notes: The figure displays the treatment effects (point estimates and 95% confidence intervals) of the `Acc-Set` condition relative to the baseline interface for each domain included in the survey phase. Estimates are obtained from separate regressions of Equation 1, conducted individually for each domain in our survey. Each color (pink, blue, and gray) denotes a different outcome.

TABLE C.7. Heterogeneity of Dark Pattern Effect by Prior Visit

	Accept All (1)	Reject All (2)	Close Window (3)
Has Prior Visit × Rej-Acc-Set	0.028	-0.009	-0.002
	(0.020)	(0.015)	(0.015)
Has Prior Visit × Acc-Set	0.007	0.038**	-0.030
	(0.018)	(0.014)	(0.016)
Has Prior Visit × Acc-GrRej-GrSet	0.011	-0.005	0.001
	(0.018)	(0.013)	(0.015)
Has Prior Visit × Acc-Rej-Set	0.017	-0.008	-0.008
	(0.019)	(0.014)	(0.016)
Has Prior Visit × Rej-Set	0.011	-0.044*	0.022
	(0.022)	(0.018)	(0.020)
Has Prior Visit	0.015	-0.029*	0.006
	(0.015)	(0.012)	(0.012)
R^2	0.517	0.471	0.421
Observations	23,685	23,685	23,685
Condition fixed effects	✓	✓	✓
Participant fixed effects	✓	✓	✓
Domain Cat. fixed effects	✓	✓	✓

Notes: The table shows regression estimates similar to Table 3, Panel *b*, except that the dummy for whether the participant visited the website in the days preceding the experiment is interacted with the banner design treatment dummies. "Condition fixed effect" refers to indicator variables for the 6 banner design conditions. * $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

TABLE C.8. Heterogeneity of Dark Pattern Effect by Domain Popularity

	Accept All (1)	Reject All (2)	Close Window (3)
Domain Rank (Log 10) × Rej-Acc-Set	-0.002	-0.001	0.001
	(0.005)	(0.004)	(0.004)
Domain Rank (Log 10) × Acc-Set	-0.007	0.004	0.005
	(0.005)	(0.004)	(0.004)
Domain Rank (Log 10) × Acc-GrRej-GrSet	0.000	-0.003	0.002
	(0.005)	(0.004)	(0.004)
Domain Rank (Log 10) × Acc-Rej-Set	-0.002	0.002	0.000
	(0.005)	(0.004)	(0.004)
Domain Rank (Log 10) × Rej-Set	0.005	-0.015**	0.013*
	(0.006)	(0.005)	(0.006)
Domain Rank (Log 10)	-0.010*	0.007*	0.003
	(0.004)	(0.003)	(0.003)
R^2	0.518	0.470	0.422
Observations	23,685	23,685	23,685
Condition fixed effects	✓	✓	✓
Participant fixed effects	✓	✓	✓
Domain Cat. fixed effects	✓	✓	✓

Notes: The table shows regression estimates similar to Table 3, Panel *b*, except that the domain rank (in logs) is interacted with the banner design treatment dummies. "Condition fixed effect" refers to indicator variables for the 6 banner design conditions. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

TABLE C.9. Choice by Stated Privacy Choice Pattern

	Accept All (1)	Reject All (2)	Close Window (3)
Accepted Most Cookies	0.273***	-0.141***	-0.059*
	(0.024)	(0.015)	(0.023)
Rejected Most Cookies	-0.314***	0.363***	0.007
	(0.030)	(0.038)	(0.036)
R^2	0.302	0.242	0.114
Observations	23,685	23,685	23,685
Condition fixed effects	✓	✓	✓
Domain Cat. fixed effects	✓	✓	✓
Sample fixed effects	✓	✓	✓

Notes: This table presents regression estimates of cookie choices as in Equation 1, while adding participants' stated reasons for accepting or rejecting cookies, as reported in the endline survey. Each row corresponds to a binary indicator for a stated motivation (e.g., trust, functionality, distrust, unfamiliarity, privacy concerns). All regressions include fixed effects for interface condition, domain category, and study phase. Standard errors are clustered at the participant level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

TABLE C.10. Choice by Stated Reasons to Accept/Reject

	Accept All (1)	Reject All (2)	Close Window (3)
Accepted for Trust	0.011	-0.032	-0.014
	(0.038)	(0.028)	(0.027)
Accepted for Functionality	0.045	-0.018	-0.062*
	(0.044)	(0.033)	(0.029)
Rejected for Distrust	-0.048	-0.006	0.077**
	(0.042)	(0.028)	(0.030)
Rejected for Unfamiliarity	-0.092*	0.007	0.106**
	(0.045)	(0.031)	(0.038)
Rejected for Privacy	-0.116*	0.066	0.093**
	(0.049)	(0.045)	(0.034)
R^2	0.143	0.068	0.123
Observations	21,410	21,410	21,410
Condition fixed effects	✓	✓	✓
Domain Cat. fixed effects	✓	✓	✓
Sample fixed effects	✓	✓	✓

Notes: This table presents regression estimates of cookie choices as in Equation 1, while adding participants' stated choices, as reported in the endline survey. The self-reported behavior aligns closely with actual choices: those who said they accepted most cookies are significantly more likely to accept and less likely to reject, while the reverse is true for those who reported rejecting most. All models include fixed effects for interface condition, domain category, and sample phase. Standard errors are clustered at the participant level. *$p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

TABLE C.11. Choice Variation Decomposition (Outcome: "Accept All")

	Base	With Covariates
Intercept	0.625***	0.510***
	(0.014)	(0.072)
Acc-Rej-Set	-0.002	-0.005
	(0.008)	(0.008)
Acc-Set	0.036***	0.033***
	(0.008)	(0.008)
Rej-Acc-Set	-0.026**	-0.030***
	(0.008)	(0.008)
Rej-Set	-0.475***	-0.480***
	(0.008)	(0.008)
Set-Acc-Rej	-0.031***	-0.032***
	(0.008)	(0.008)
SD (Participant)	0.304	0.304
SD (Domain)	0.058	0.046
SD (Residual)	0.343	0.342
Num.Obs.	23685	23190

Notes: This table presents estimates of treatment effects models where random effects for participant and domain are included in the regression. The second column adds controls for website characteristics, demographics, and privacy beliefs presented in Table 3. The outcome is a dummy for whether a user accepts all cookies.

TABLE C.12. Consumer Surplus Under Counterfactual Policies (Utility Scale Results)

Counterfactual	Average	Type 1 Acceptors	Type 2 Rejectors	Type 3 Discerners
U.S. Status Quo	1.87	3.29	-2.79	2.35
EU Norm	2.47	3.23	0.97	2.24
Optimal Banner Interface	2.52	3.57	-0.14	2.49
Optimal Banner Interface (with correct beliefs)	2.78	3.79	0.64	2.55
Global Accept	5.69	8.47	0.4	4.79
Global Reject	4.9	5.81	5.36	3.58
Global Privacy Control	6.56	8.47	5.36	4.79

Notes: The values represent unscaled consumer surplus *per choice* under various counterfactual policies in the utility scale. "Pooled estimate" refers to the estimate across all subjects, and the other columns correspond to subset-specific estimates. "U.S. status quo" refers to an accept-settings interface, combined with an accept-all default when consumers close window; "CS maximizing" refers to a baseline interface with an accept-all default when consumers close window; "EU norm" refers to a baseline interface with a reject-all default when consumers close window. "Global Accept" and "Global Reject" force each individual to either always accept or always reject all cookies. The last row, "Global Privacy Control," allows each individual to make their preferred global choice.

Appendix D. Survey Questions

This appendix presents the Qualtrics surveys used in the study:

- Intake.
- Outtake.

Device Transfer

The rest of the survey needs to be done on a Chrome browser. Please copy the link of the survey and reopen it in a Chrome browser to continue.

First Page

Would you like to help us understand online behavior and privacy choices? We are a team of Harvard and Boston University researchers who study the internet and how it affects society.

The study will take 30 minutes of your time over the course of the next day. We will ask you to fill out two surveys, clear the cookie data stored in your browser, install a browser extension vetted by Harvard and Boston University and keep it installed for seven days, and share information about your online behavior. Click below if you want to know more and discover if you qualify!

Eligibility Questions

Do you live in the United States?

No

Yes

Are you over 18 years old?

Yes

No

What is 12 minus 4? Regardless of the correct answer, you should always select the option with the value "seven". This is an attention check question.

6

8

7

5

What is the language you primarily speak?

Spanish

English

Other (please specify)

Which browser do you primarily use?

Others

Internet Explorer

Chrome

Microsoft Edge

Safari

Firefox

What was your total household income before taxes during the past 12 months?

Less than $25,000

$25,000-$49,999

$50,000-$74,999

$75,000-$99,999

$100,000-$149,999

$150,000 or more

Prefer not to say

What is the highest level of education you have completed?

Some high school or less

High school diploma or GED

Some college, but no degree

Associates or technical degree

Bachelor’s degree

Graduate or professional degree (MA, MS, MBA, PhD, JD, MD, DDS etc.)

Prefer not to say

Not Eligible

Thank you for your answers! Unfortunately, you do not qualify to participate in our study. **Can you please return your submission on Prolific?**

Consent

Congratulations! You are qualified to participate in our study.

Study Overview

The following is a summary with key information to help you decide whether you want to participate.

Why am I being invited to take part in a research study?
We invite you to take part in this research study because you are an English-speaking resident of the United States who uses Chrome to browse the web.

What should I know about a research study?
Research studies are conducted to better understand the choices we make. Whether or not you take part is completely up to you. Your decision will not be held against you. You can ask all the questions you want before you decide. You can even agree to take part and later change your mind.

Why is this research being done?

We want to better understand the online experience of people like you, how companies obtain user consent for the collection and use of their data, and how this affects user browsing experience. We hope that the results of this research will help inform data privacy policy.

How long will the research last and what will I need to do?

The study will last several days, but we will only ask you for 30 minutes of your time. Everything we ask you to do to participate in this research can be done from the comfort of your home. If you choose to participate, we'll ask you to:

- Complete two surveys:
    - The first survey will ask you some questions about yourself and your online browsing behavior. It will also ask you to visit some websites and make privacy choices.
    - The second survey will ask you about your experience during the study.
- Install the Cookie Manager browser extension, which is an application we developed for this study. We'll have instructions for you. The Cookie Manager extension will record your behavior and may tweak the interfaces through which you make cookie selections.
- Keep the extension installed for seven days, until the extension prompts you to uninstall it.

Will I be compensated for participating in this research?

Yes. You will be paid $7.50 after completing the two surveys and keeping the Cookie Manager extension installed for several days.

Is there any way being in this study could be bad for me?

Since we may collect personal information, there is a risk of breach of confidentiality. We have worked hard to minimize this risk. For example, we will encrypt any data before storing it. Before accessing the data for analysis, we will also permanently delete all personal information that we may intentionally or unintentionally collect.

Will being in this study help me in any way?

We cannot promise any benefits to you or others from your taking part in this research. It is possible, however, that our tweaks to your online browsing lead to a better (or worse) online experience.

Detailed Information

Withdrawing from the Study.
You can leave the research at any time; your decision will not be held against you. We may use the data you have shared with us prior to withdrawing as part of the study. We will provide simple instructions for how you can withdraw. Researchers can remove you from the research study without your approval. Possible reasons for removal include not complying with instructions to install the browser extension or intentionally avoiding data tracking through the extension.

Privacy.
Data security and privacy are important to us. During the course of the study we may collect personal information. The personal information that we know we are collecting will be deleted immediately. Other personal information that we inadvertently collect will be stored but removed after we finish collecting data.

We cannot promise complete secrecy, although efforts will be made to limit the use and disclosure of your personal information. Data will be encrypted and stored on secure servers and cannot be accessed by anyone outside the research team. At no time will study information be available over any public or private network in an unencrypted state.

In the future, when we publish our research, we will post anonymized data from this study in a data repository so that other researchers can reproduce our results. By then, no information that can identify you personally will be available, to us or others. We will not sell data from the study or share data for any commercial or marketing purposes.

Who can I talk to?
If you have questions, concerns, or complaints, or think the research has hurt you, do not hesitate to reach the research team on Prolific or cookie.manager.study@gmail.com.

Please indicate below whether you agree to participate in the study. Agreeing to participate means you are willing to install Cookie Manager (our web browser extension) for seven days, and complete the two surveys.

I agree to participate

I do not agree to participate

Not consent

Thank you for letting us know you do not want to participate. **Can you please return your submission on Prolific?**

Email

Thank you for your willingness to participate in our study!

Next, we will ask you to install *Cookie Manager*, a browser extension we developed to identify website tracking and to enable simplified privacy consent dialogs.

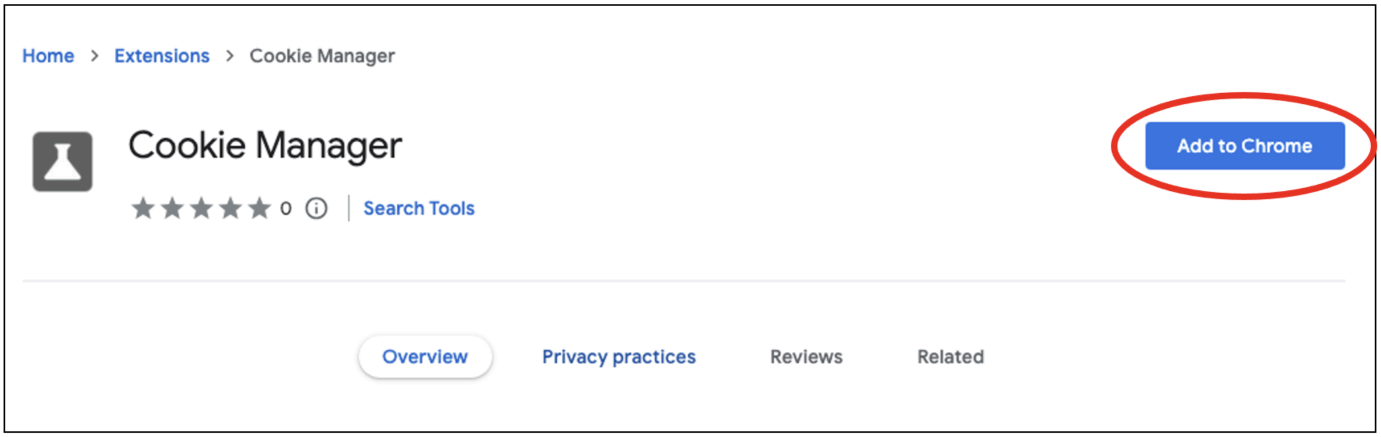
After installing the extension, you will see a consent-request popup window whenever you visit a website for the first time. If you make a choice, the extension will try to pass on your choices to the website. In most cases, if the website has already been collecting consent from users, it will recognize your choice and decide whether to continue tracking you based on your choice.

App Installation

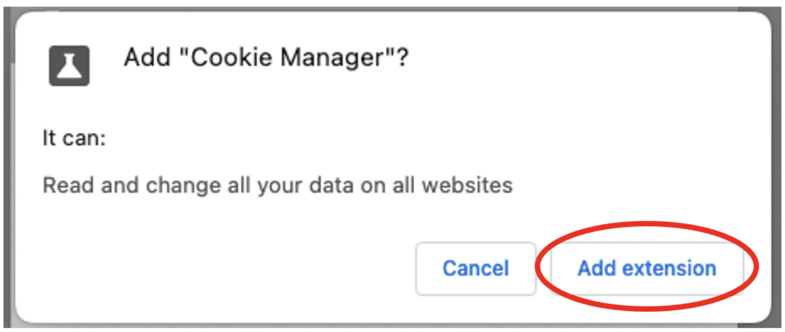
Cookie Manager Installation Instructions.

To install Cookie Manager, please **use Chrome** on the computer that you are using for online shopping:

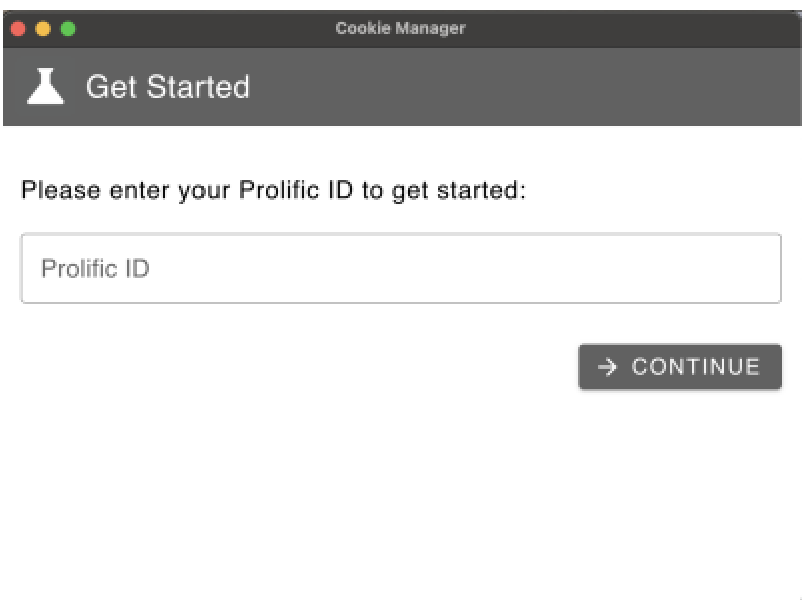
- Click here.
- Click “Add to Chrome.”



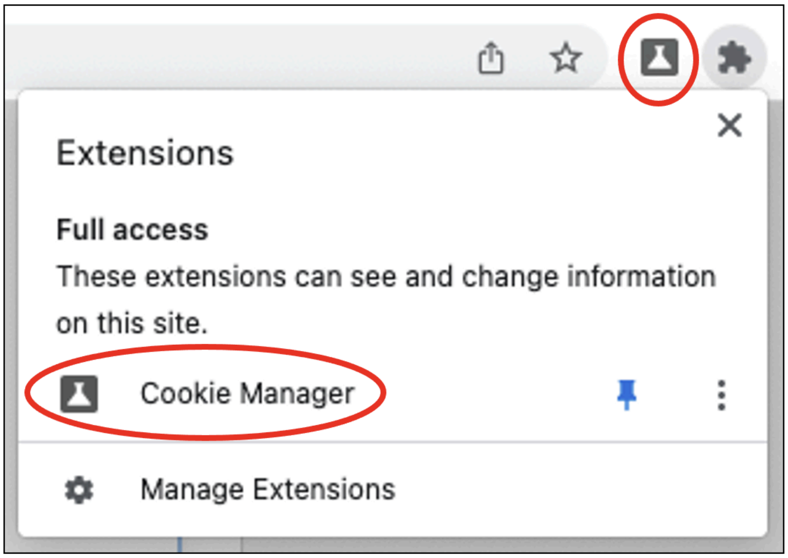
- When prompted, click “Add Extension.”



- You will be prompted to add your prolific id.



- You should now see the Cookie Manager icon on the top right corner of your browser. If you don’t see it, it may be hidden under the puzzle icon, which is visible in the upper right corner of the screenshot below.



- You are all set.

If you have trouble installing Cookie Manager, email us at cookie.manager.study@gmail.com and we will help you with additional instructions.

Were you able to successfully install the extension?

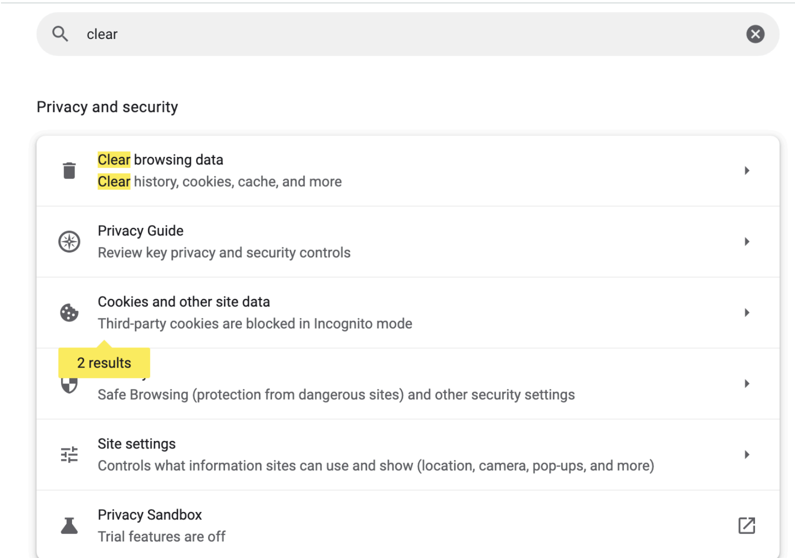
Yes

No

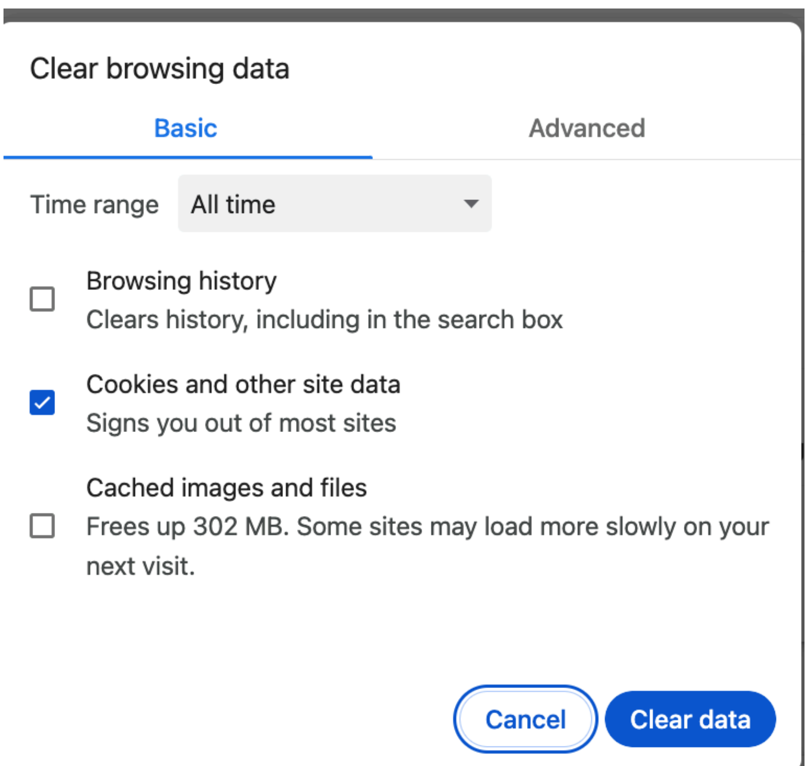
What difficulties have your encountered when installing the extension?

Clear Browsing History

Next, we will ask you to clear your cookie data. Please navigate to chrome://settings/privacy?search=clear (copy and paste the address directly on your search bar), and click on "Clear Browsing Data". Then select **only 'cookies and other site data'**, and click clear data.



Select the time range to be **"All Time"** and select the cookies and other data check box, as seen below. Then click "Clear Data."



Were you able to clear your cookie data?

Yes

No

Intro to website navigation

Now that you have *Cookie Manager* installed, we will ask you to visit a list of 20 websites. Please wait until a banner shows up for each site and interact with the banner as you normally would. We will ask you to answer a few questions after each visit. After you finish the survey task, the frequency of pop-ups will drastically decrease.

Note: for your browsing action to be correctly registered in our database, please directly left-click on the link on the survey page to navigate to the website. If instead you right-click on the link and select "open on a new tab", a warning will continue showing up, meaning that our database has not recognized your click action.

YouTube

Please use Chrome to navigate to youtube.com. Please wait until a banner shows up. Search for a video of your choice.
You haven't clicked on the link

Do you normally visit Youtube?

Yes

No

Have you ever heard of Youtube?

Yes

No

How often do you normally visit Youtube?

At least once a day

At least once a week

Less than once a week

Never

New York Times

Please use Chrome to navigate to nytimes.com. Please wait until a banner shows up. Click on an article of your choice.

You haven't clicked on the link

Do you normally visit NYTimes?

Yes

No

Have you ever heard of New York Times?

Yes

No

How often do you normally visit New York Times?

At least once a day

At least once a week

Less than once a week

Never

Apple Insider

Please use Chrome to navigate to appleinsider.com. Please wait until a banner shows up. Click on an article of your choice.

You haven't clicked on the link

Do you normally visit Apple Insider?

Yes

No

Have you ever heard of Apple Insider?

Yes

No

How often do you normally visit Apple Insider?

At least once a day

At least once a week

Less than once a week

Never

Yahoo

Please use Chrome to navigate to yahoo.com. Please wait until a banner shows up. Click on an article of your choice.
You haven't clicked on the link

Do you normally visit Yahoo?

Yes

No

Have you ever heard of Yahoo?

Yes

No

How often do you normally visit Yahoo?

At least once a day

At least once a week

Less than once a week

Never

Amazon

Please use Chrome to navigate to amazon.com. Please wait until a banner shows up. Search for a product of your choice.
You haven't clicked on the link

Do you normally visit Amazon?

Yes

No

Have you ever heard of Amazon?

Yes

No

How often do you normally visit Amazon?

At least once a day

At least once a week

Less than once a week

Never

eBay

Please use Chrome to navigate to ebay.com. Please wait until a banner shows up. Search for a product of your choice.
You haven't clicked on the link

Do you normally visit eBay?

Yes

No

Have you ever heard of eBay?

Yes

No

How often do you normally visit eBay?

At least once a day

At least once a week

Less than once a week

Never

What is 6 divided by 2? Regardless of the correct answer, you should always select the option with the value "one". This is an attention check question.

2

3

1

Target

Please use Chrome to navigate to target.com. Please wait until a banner shows up. Search for a product of your choice.
You haven't clicked on the link

Do you normally visit Target?

Yes

No

Have you ever heard of Target?

Yes

No

How often do you normally visit Target?

At least once a day

At least once a week

Less than once a week

Never

Etsy

Please use Chrome to navigate to etsy.com. Please wait until a banner shows up. Search for a product of your choice.
You haven't clicked on the link

Do you normally visit Etsy?

Yes

No

Have you ever heard of Etsy?

Yes

No

How often do you normally visit Etsy?

At least once a day

At least once a week

Less than once a week

Never

Turro

Please use Chrome to navigate to turo.com. Please wait until a banner shows up. Click on a car of your choice.
You haven't clicked on the link

Do you normally visit Turo?

Yes

No

Have you ever heard of Turo?

Yes

No

How often do you normally visit Turo?

At least once a day

At least once a week

Less than once a week

Never

StockX

Please use Chrome to navigate to stockx.com. Please wait until a banner shows up. Search for a product of your choice.
You haven't clicked on the link

Do you normally visit StockX?

Yes

No

Have you ever heard of StockX?

Yes

No

How often do you normally visit StockX?

At least once a day

At least once a week

Less than once a week

Never

ESPN

Please use Chrome to navigate to espn.com. Please wait until a banner shows up. Click on an article of your choice.
You haven't clicked on the link

Do you normally visit ESPN?

Yes

No

Have you ever heard of ESPN?

Yes

No

How often do you normally visit ESPN?

At least once a day

At least once a week

Less than once a week

Never

Facebook

Please use Chrome to navigate to facebook.com. Please wait until a banner shows up. Scroll down.

You haven't clicked on the link

Do you normally visit Facebook?

Yes

No

Have you ever heard of Facebook?

Yes

No

How often do you normally visit Facebook?

At least once a day

At least once a week

Less than once a week

Never

Funny Or Die

Please use Chrome to navigate to funnyordie.com. Please wait until a banner shows up. Click on an article of your choice.

You haven't clicked on the link

Do you normally visit Funny Or Die?

Yes

No

Have you ever heard of Funny Or Die?

Yes

No

How often do you normally visit Funny Or Die?

At least once a day

At least once a week

Less than once a week

Never

Weather

Please use Chrome to navigate to weather.com. Please wait until a banner shows up. Search for a location.

You haven't clicked on the link

Do you normally visit Weather.com?

Yes

No

Have you ever heard of Weather.com?

Yes

No

How often do you normally visit Weather.com?

At least once a day

At least once a week

Less than once a week

Never

DuckDuckGo

Please use Chrome to navigate to duckduckgo.com. Please wait until a banner shows up. Search for a product of your choice.
You haven't clicked on the link

Do you normally visit DuckDuckGo?

Yes

No

Have you ever heard of DuckDuckGo?

Yes

No

How often do you normally visit DuckDuckGo?

At least once a day

At least once a week

Less than once a week

Never

Truewerk

Please use Chrome to navigate to truewerk.com. Please wait until a banner shows up and navigate to an item.
You haven't clicked on the link

Do you normally visit Truewerk?

Yes

No

Have you ever heard of Truewerk?

Yes

No

How often do you normally visit Truewerk?

At least once a day

At least once a week

Less than once a week

Never

Thomann

Please use Chrome to navigate to thomannmusic.com. Please wait until a banner shows up and navigate to an item.
You haven't clicked on the link

Do you normally visit Thomann Music?

Yes

No

Have you ever heard of Thomann Music?

Yes

No

How often do you normally visit Thomann Music?

At least once a day

At least once a week

Less than once a week

Never

MerrySky

Please use Chrome to navigate to merrysky.com. Please wait until a banner shows up and search for a location.
You haven't clicked on the link

Do you normally visit Merry Sky?

Yes

No

Have you ever heard of Merry Sky?

Yes

No

How often do you normally visit Merry Sky?

At least once a day

At least once a week

Less than once a week

Never

Seattle Times

Please use Chrome to navigate to seattletimes.com. Please wait until a banner shows up and then click on an article.

You haven't clicked on the link

Do you normally visit Seattle Times?

Yes

No

Have you ever heard of Seattle Times?

Yes

No

How often do you normally visit Seattle Times?

At least once a day

At least once a week

Less than once a week

Never

Semafor

Please use Chrome to navigate to semafor.com. Please wait until a banner shows up and then click on an article.
You haven't clicked on the link

Do you normally visit Semafor?

Yes

No

Have you ever heard of Semafor?

Yes

No

How often do you normally visit Semafor?

At least once a day

At least once a week

Less than once a week

Never

Favorite website

Navigate to your favorite e-commerce website. Please wait until a banner shows up. Search for a product of your choice.

Paste the URL of the product below:

Did you see a cookie consent banner?

Yes

No

Did you make a choice on whether to allow for cookie sharing?

Yes, I allowed my preferred cookies and blocked unwanted cookies

Yes, I chose the default cookie sharing

No, I closed the cookie consent banner

No, I left the website without interacting with the consent banner

Questionnaire

Think about your browsing experiences on a typical day. Overall, how frequently do you encounter cookie consent banners?

Too frequently

A bit more frequently than ideal

Just right

A bit less frequently than ideal

Too infrequently

Overall, how would you rate the ease of navigation of the cookie consent interfaces on the websites you visit?

Very easy to navigate

Moderately easy to navigate

Neither easy nor hard to navigate

Moderately hard to navigate

Very hard to navigate

Overall, how would you rate the ease of making your preferred choices regarding cookie sharing on the websites you visit?

Very easy

Moderately easy

Neither easy nor hard

Moderately hard

Very hard

Which of the following best describes your behavior when deciding whether to share cookies online?

I reject most cookies

I consider both the website that is asking and the types of cookies involved before deciding whether to share them

I accept most cookies

I decide whether to share cookies based on what type of cookies they are

I decide whether to share cookies based on which website is asking

Part1-conclude

Thank you! To finish the rest of the study, we ask you to keep Cookie Manager installed for another seven days. You can continue your browsing activities as usual during this time. The frequency of pop-ups will drastically decrease over time. After the seven days have passed, the extension will prompt you with a survey and the instructions on how to uninstall the extension.

There is no completion code, since our system will detect completion automatically. Please make sure to click the next button below so that we register your response.

Intro Page

Thank you for finishing our web browsing task! Now we will walk you through the uninstallation process of the browser extension. To complete the study, we just need to ask you a few more questions about the web browsing and cookie-sharing experiences while using our extension and in general.

Block 1

Think back about your browsing experiences after completing our 20-website visit task while Cookie Manager is installed. Overall, what do you think of the frequency with which cookie consent banners appear during that time?

Too frequent

A bit more frequent than ideal

Just right

A bit less frequent than ideal

Too infrequent

Block 2

Overall, how will you rate the ease of navigation of the cookie consent interface created by our browser extension?

Very easy to navigate

Moderately easy to navigate

Neither easy nor hard to navigate

Moderately hard to navigate

Very hard to navigate

Block 3

Overall, how will you rate the ease of making your preferred cookie sharing choices created by our browser extension?

Very easy

Moderately easy

Neither easy nor hard

Moderately hard

Very hard

Block 4

In the past week, which of the following statement best describes your behavior when deciding whether to share cookies online?

I accepted most cookies

I rejected most cookies

I chose whether to share cookies based on which website is asking

I chose whether to share cookies based on what types of cookie it is

I chose whether to share cookies based on what website is asking and what types of cookie it is

Why choice

Think back to a case when you accepted all cookies during the course of the study. Why did you do so?

Think back to a case when you chose **not** to accept all cookies during the course of the study. Why did you do so?

Block 5

Overall, how do you think the Cookie Manager extension changes your web browsing experience?

It improves my browsing experience by a lot

It improves my browsing experience slightly

It neither improves nor degrades my browsing experience

It degrades my browsing experience slightly

It degrades my browsing experience a lot

Block 12

Consider the cookie consent form below.

One option is to hit the 'x' button in the upper right. If you were to click this 'x', what do you think will happen?

All cookies are accepted.

None of the cookies are accepted.

Other, please explain:

Block 8

During the study period, did you take any actions to change how you browse the internet?

No

Yes, I used a different browser or device.

Yes, I browsed the internet less.

Yes, I did something else. Please specify.

Block 9

As you browse the internet, which information do you think advertisers have about you? Check all that apply.

Your demographic information

Your prior website visits

Your interests

Your prior purchases

Your social media posts

Your address

Your credit score

Block 10

Thinking about privacy polices you might come across online or on your smartphone. Which of the following comes closer to your view, even if neither is exactly right?

Just something I have to get past in order to use a product or service.

A meaningful part of my decision to use a product or service.

Privacy means different things to different people today. In thinking about all of your online browsing, please state how important it is for you to be in control of who can get info about you.

Not all imporant	Not very imporant	Somewhat Important	Very Imporant
1	2	3	4
○	○	○	○

Block 11

What do you think are the benefits of sharing the data listed above with the advertisers?

What do you think are the costs of sharing the data listed above with the advertisers?

Block 6

Do you have any suggestions to help us improve the design of the Cookie Manager extension or the design of our study in general?

Block 14

Consider a tool that that allows you to specify how you would like to answer cookie consent questions online. This tool will then automatically hide all cookie pop-ups and answer them in they way you specified. For example, if you stated that you wanted to accept cookies for all websites, the tool would do so.

Please select how much better or worse the tool is than manually answering the cookie consent form for each website.

Much worse than manual choice	Worse	Somewhat worse	Similar	Somewhat better	Better	Much better than manual choice
○	○	○	○	○	○	○

How much would you be willing to pay for the tool?

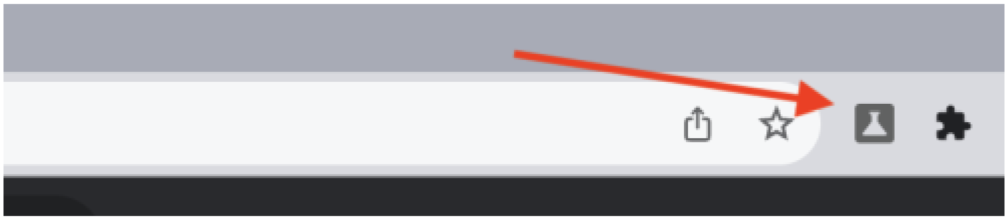
Please enter the price in the text box below.

Instructions for how to download and configure the tool, called Consent-O-Matic, are available here.

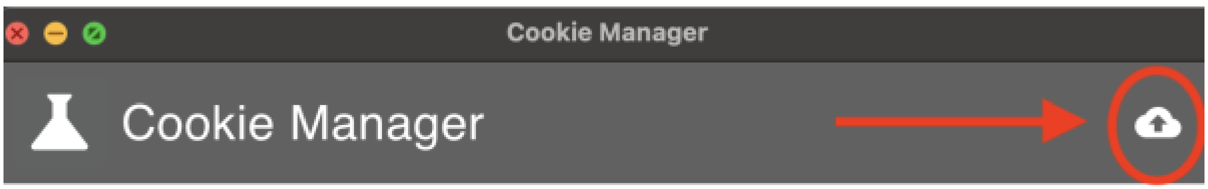
Please click the arrow below to continue the survey.

Block 7

Thank you! We will ask you to upload your data to us prior to uninstalling the extension. Please click on the Cookie Managert extension icon in your Chrome browser.



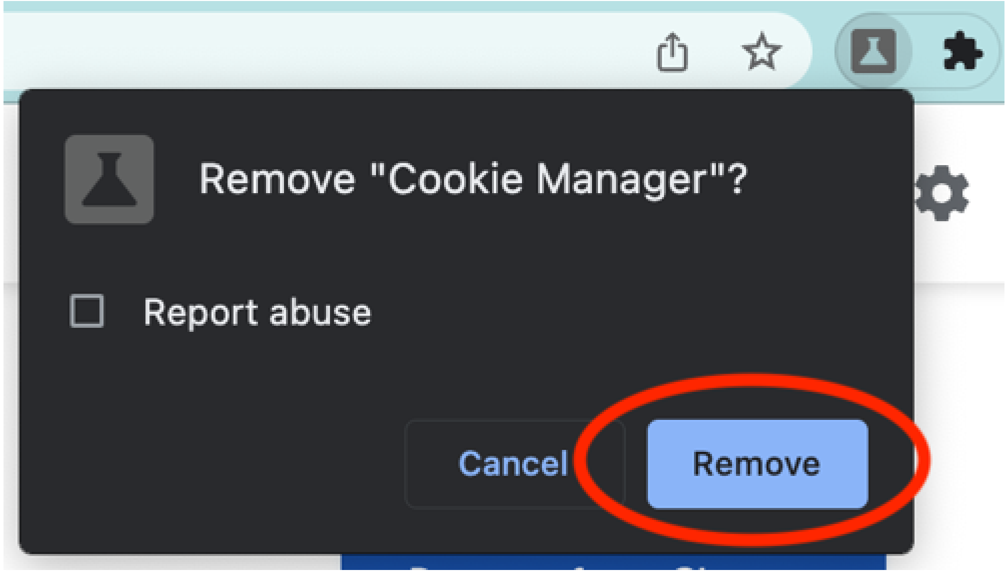
You should see a pop-up. Please click on the cloud button with an arrow. Completing this step ensures that your participation in our study and the associated data are properly recorded.



Now that you've clicked the cloud button, you can now proceed to uninstall the extension. Completing this step ensures that we stop collecting your browsing data going forward.

Here's how you can remove the cookie manager extension:
- Click [here](#).
- Click “Remove from Chrome.”

- Confirm by clicking "Remove" on the pop-up window appearing on the top right corner of your browser.



- You're all set.

If you have trouble uninstalling Cookie Manager, email us at cookie.manager.study@gmail.com and we will help you with additional instructions.

Please click the arrow below to finish the survey.

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