The Magical Number 2 (Minus Two):

An Empirical Analysis on the Efficacy of Choice Screens to Increase Competition in Digital Markets

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Abstract

This work examines the efficacy of choice screens as a regulatory tool for reducing market concentration in digital markets. Competition authorities have increasingly relied on choice screens as a primary legal remedy to reduce the dominance of incumbent search engines and internet browsers, yet empirical evidence on their effectiveness remains limited.

The paper reviews existing literature on status quo bias, forced choice, and previous regulatory interventions, focusing on the Microsoft and Android choice screen implementations. It also empirically assesses the impact of the Digital Markets Act on Google Search and Chrome's market share.

The findings indicate that while choice screens produce measurable effects, their impact on market dominance is minimal, with market share shifts typically below 2%. These results raise questions about policymakers' reliance on choice screens as a primary competition policy remedy and highlight the need for alternative or complementary measures to meaningfully affect entrenched market dynamics when antitrust intervention is socially desirable. Ultimately, however, this work sheds light on the limited effectiveness of choice screens and does not argue for or against more intrusive interventions.

1 Introduction

George A. Miller's "*The Magical Number Seven Plus and Minus Two*" is one of the most cited papers in cognitive psychology (Miller 1956). It is about our limited capacity to process information, showing that people tend to perceive seven discrete categories of stimuli (such as noise or sweetness) even when there's a much wider range of stimulus intensity (e.g., a dozen different sugar levels). This work is somewhat related to Miller's. However, rather than examining our cognitive processing limitations, it focuses on our

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engagement with forced choices designed to shift entrenched market dynamics. Similar to Miller's discovery of a cognitive "magic number," this research identifies an empirical regularity: forced-choice remedies typically change the behavior of no more than 2% of individuals subjected to them.

Many of the most important digital markets are highly concentrated. The search engines market is a canonical example of market dominance. In fact, Google holds over 90% of the market in the *mobile* segment of search and over 80% in the *desktop* segment. These statistics not only depict Google's dominance in the U.S. but in virtually every western country. The internet browsers market is still somewhat concentrated. Yet Internet Explorer (IE) was a monopolist for several years. And the parallel between Google and IE is particularly intriguing because while Google is regarded as the most accurate search engine, IE had a much worse reputation—as someone put it, "IE was the best browser to download a better browser with" (James 2020). Nevertheless, there is a common element in both Google and IE's cases: both came preset as people's default application—a feature that triggered many of the most significant antitrust cases and competition regulations of the last 30 years.

It is tempting to assume that this common feature partially explains market dominance and that its elimination would enhance competition among applications. While network effects are perhaps the most significant source of market power in many digital markets—including search—it seems plausible to hypothesize that status quo bias also contributes to market dominance. This was to some extent one of the reasons that led the U.S. Department of Justice (DOJ) to sue Microsoft in the late 1990s (see, e.g., Melamed and Rubinfeld 2007), an example European authorities closely followed a few years later. While the DOJ Microsoft settlement mostly focused on code transparency, Europeans mandated Microsoft to force its users to choose their default internet browser in 2010. A remedy the whole antitrust community merely assumed had been "effective"—more on what "effective" means below—for over ten years, which led European authorities to use it again in the Android case in 2020 and policymakers to give this remedy a starring role in the novel Digital Markets Act (DMA).

The empirical evidence on choice screens is limited. However, two studies have measured the impact of the Microsoft (Vasquez Duque 2023) and Android (Decarolis, Li, and Paternollo 2023) choice screens. The Android study has been misinterpreted as evidence that this remedy can significantly lower a dominant firm's market share. This misinterpretation stems from inappropriate extrapolations of interventions in Russia and Turkey, with the Turkish case not even involving forced application choices. This work reviews this literature, delves deeper into the behavioral theory underlying choice as a regulatory tool and the policy objectives of choice screens, and provides an empirical assessment of the DMA on Google and Chrome's market share. By assessing these three interventions, this work shows that relying on people's choices to police market dominance does not make a meaningful difference on market concentration. In fact, when choice screens have changed a dominant actor's market share, the effect size is, at most, a 2% change. This work proceeds as follows: Section 2 of this work summarizes the psychology and economics of status quo bias and forced choice, reviewing the relevant literature and discussing behavioral models that explain consumer inertia. Section 3 sheds light on the empirical strategy this work employs for identifying the DMA's impact on Google and Chrome's market share. Section 4 presents the findings, showing that the DMA lowered Google and Chrome's market share less than 1% and 2.5%, respectively. Section 5 discusses them. Finally, Section 6 concludes suggesting that while choice screens seem like an inexpensive and practical intervention, they are based on a poor behavioral theory and thus are unlikely to have a meaningful impact on the market.

2 Status Quo Bias, Exclusivity, and Forced Choice

2.1 The Social Science of Status Quo Bias

One of the most famous findings of behavioral science is that people tend to stick to the status quo (Samuelson and Zeckhauser 1988). For instance, the number of organ donors is much higher in countries where people automatically become donors compared with those in which people must actively opt in (Johnson and Goldstein 2004). Defaults too radically influence whether and how much people save for retirement, since those automatically enrolled are unlikely to opt-out or change plans (Beshears et al. 2009). This is also the case of car insurance plans (Johnson et al. 1993). And there are many other examples in which automatic opt-in increases the uptake of whatever a choice architect sets as people's default.

More formally, a default effect is the difference in the probability of seeing a particular outcome, let's say a, when option a is people's default (i.e., $P(a|d)$) compared with another state of the world in which a is not people's default. The normative benchmark is what people would choose ($P(a|c)$, where c is "choice"). Status quo is a "bias" because of people's tendency to adhere to the status quo more often than the canonical rational choice model predicts. If transaction costs were minimal, defaults should have a negligible effect on behavior. Agents with clear preferences would avoid any default that does not optimize their utility, regardless of the default's nature. But we know that defaults tend to have a large influence on behavior (i.e., $P(a|d) > P(a|c)$).

In addition to coining the "status quo bias" term and providing important empirical evidence to support their theory of inertia, Samuelson and Zeckhauser hypothesized that default effects could stem from three causes: (i) rational choice, (ii) cognitive misperceptions, and (iii) psychological commitment (Samuelson and Zeckhauser 1988, 33). As noted, defaults should be irrelevant to explain behavior when transaction costs are low. Thus, a natural explanation for status quo bias is "high" switching costs (i.e., higher than the benefit of opting out). For instance, understanding the terms of a complex insurance contract is costly. In addition to the time investment (t), people may need to hire an expert (h) to understand the value of a plan and assess the potential benefit of switching (b). For a perfectly informed person, sticking to the status quo is rational as long as $t + e > b$. For an imperfectly informed individual, the rationality of inertia depends on the subjective probabilities assigned to costs and benefits. This is why sticking to a default is not necessarily a mistake. However, some authors define status quo effects as the "suboptimal acceptance of a default choice option" (Fleming, Thomas, and Dolan 2010, 6005). This is because of the common belief that default effects reflect a primacy of people's System 1 over System 2, is not necessarily the case. In fact, in many cases the benefit of switching may be zero or even negative. These are cases of "benign" defaults (Goldstein et al. 2008), in which defaults don't affect behavior.

Defaults do distort choice when cognitive misperceptions affect our judgment. A canonical example is loss aversion (Kahneman, Knetsch, and Thaler 1990). In the late seventies, Kahneman and Tversky stated that people's subjective valuation of gains and losses deviated from their objective monetary values, with losses typically weighted more heavily than equivalent gains (Kahneman and Tversky 1979). This is why credit cards lobby to ensure potential discounts for buyers who pay by cash—or debit cards—are perceived as a saving rather than having their clients paying a higher price for credit card purchases, which would be perceived as a loss (Thaler and Sunstein 2008, 36). Empirical research then validated their theory, finding that losses tend to weight twice as much as the gains' monetary value (see, e.g., Brown et al. 2024). As noted people are more likely to save for retirement if they automatically enroll in a pension plan. In part, the effect may stem from how the default affects the frames of gains and losses (i.e., how losing savings for a pension to get a larger paycheck compares to earning pension savings to lose a part of someone's paycheck). In fact, one of Thaler's main contributions to behavioral economics was to postulate the endowment effect (i.e., assigning a higher value to an item just because of owning it), which builds upon Kahneman and Tversky's prospect theory. Other examples of cognitive misperceptions are anchoring and incomplete consideration of choice sets (Samuelson and Zeckhauser 1988, 36).

Psychological commitment is closely related to cognitive misperceptions. The main examples are sunk cost fallacies and regret avoidance. The former refers to people's desire to justify previous actions with future commitments. And the greater the investment in the status quo, the more likely it will be retained. An example would be a person refusing a job offer unrelated to their college degree just because it has nothing to do with the person's education. The latter points out that people feel greater regret for bad outcomes resulting from new actions taken than for bad consequences stemming from inaction. An example is the same person not switching to a new job related to their degree despite disliking their current job because of the prospect of disliking the new one as well.

Scholars in behavioral law and economics have generally failed to distinguish between varying cognitive states when examining default effects, grouping *inertia* and *procrastination* together in their analyses. This is problematic because procrastination involves a significant degree of conscious awareness. Default effects can emerge when individuals *seek* to minimize effort through either (i) avoiding decision-making altogether, or (ii) reluctance to develop a preference in the first place. However, another explanation for inertia is people's inattention (see, e.g., Vasquez Duque 2024). In this respect the marketing literature is particularly insightful. It points out that consumers may act in three different cognitive states: autopilot, copilot, or pilot modes (see, Martin and Morich 2011). The former "is the state of being that enables a person to complete tasks not linked to conscious intent, needs or goals" (Martin and Morich 2011, 494–95). It represents habitual purchase and usage behavior, such as buying coffee for breakfast in the same coffee shop near one's house. In contrast, pilot mode is conscious attendance to the purchase or use of a product or service (Martin and Morich 2011, 405). When consumers face new purchasing scenarios or encounter changes in routine buying factors like price, features, or where products are sold, they enter "pilot mode," which activates their conscious awareness and may involve evaluating costs against benefits while comparing different products and purchase options. By contrast, "co-pilot mode" occurs in familiar situations with a modest range of choices that are too complex for purely habitual selection but don't warrant full conscious deliberation. In these intermediate scenarios, consumers typically employ heuristics—straightforward mental shortcuts—to partially automate their decision-making process rather than engaging in comprehensive evaluation (Martin and Morich 2011, 496). The pilot, co-pilot and auto-pilot taxonomy is compatible with a behavioral theory according to which our brains economize attention in various degrees (see, e.g., Kruglanski and Gigerenzer 2011).

2.2 Exclusivity and Forced Choice

Why is exclusivity relevant for this work? The main idea is that through default agreements firms may exploit people's inertia to achieve an effect functionally equivalent to an exclusivity contract.[1] And exclusivity contracts that foreclose the market to actual or potential competitors may be illegal. It is important to note, however, that exclusivity is not illegal per se. In fact, exclusive dealing contracts are typically procompetitive (see, e.g., Melamed 2005). Courts evaluate these arrangements by examining the defendant's market power, the extent of market foreclosure, contract duration, and the balance between anticompetitive effects and efficiency justifications. Usually the defendant pays a fine and courts mandate behavioral remedies (e.g., code transparency to facilitate the development of new applications). However, since the 2010s European courts have innovated in this respect, mandating users to select their default applications. A strategy that is meant to debias users' choice.

2.2.1 Microsoft

Microsoft pre-installed Internet Explorer (IE) on Windows and made it practically impossible to uninstall, effectively integrating the browser with its operating system. This strategy aimed to eliminate Netscape, which Microsoft viewed as an existential threat to its operating system monopoly. Microsoft feared that Netscape could evolve into a platform-neutral middleware that would enable applications to run across different operating systems, potentially undermining Windows' market dominance (see, e.g., Gavil

[1] The legal analysis gets technical quickly. The interested reader may find a much more detailed explanation of the applicable law to exclusivity agreements and equivalent practices in another piece the author recently published in a law review (Vasquez Duque 2024).

and First 2014). Since Microsoft owned both Windows and IE, the case was not about exclusivity contracts but tying. The strategy proved successful, as Netscape's market share plummeted from over 70% to under 1% within a few years, effectively eliminating what Microsoft had identified as a competitive threat to its core business (Campbell 2015). These facts led to antitrust cases in both the US and EU. The US case ended with a settlement that focused on API disclosure and behavioral remedies. In contrast, the EU took a more aggressive approach,[2] ordering Microsoft to implement a browser choice screen in 2009 (see Figure 1).

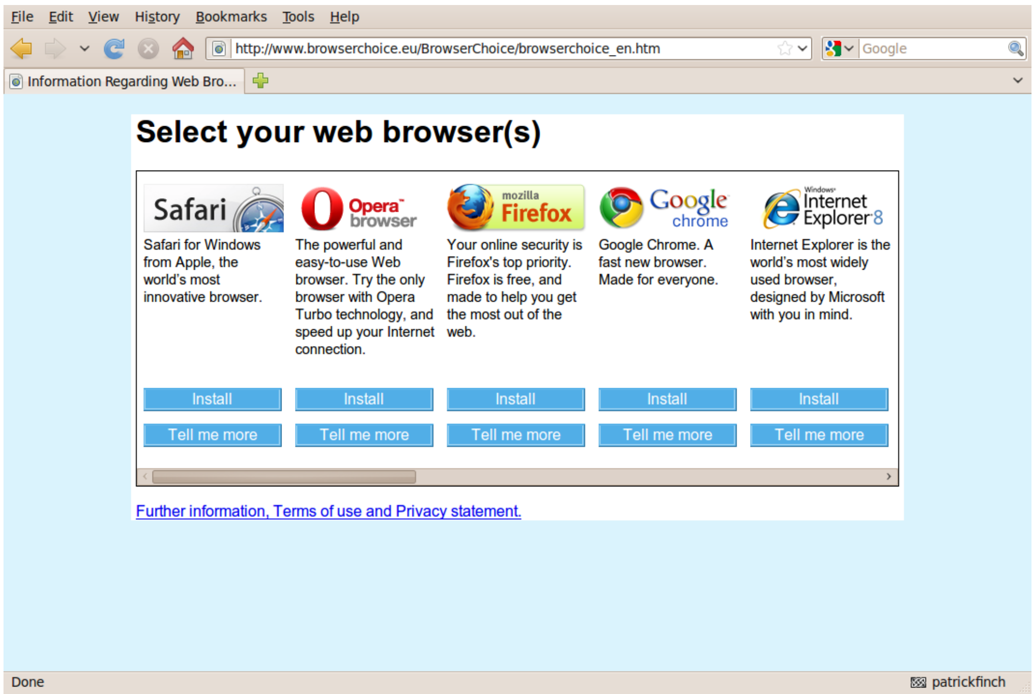

Figure 1: Microsoft Choice Screen

For over a decade European authorities merely assumed this remedy had had a meaningful impact on the internet browsers market. When discussing the remedies to be applied in the Android case, former European Commissioner for Competition, Margrethe Vestager, stated that Europe had seen "that choice screens [could] be an effective way to promote competition" (European Commission 2019). However, the author showed that when considering U.S., Canada, Australia, and New Zealand as a comparison group, the choice screen's impact was at most a 2% decrease in IE's market share (Vasquez Duque (2023)). Table 8 in the Appendix shows how matrix completion provides almost identical results when assessing the one-year impact of this intervention including all high-income countries of 2010 in the donor pool (ATT=2.13, 95% CI: [-3.64, -0.62], p <0.006).

[2]The EU's first major case against Microsoft actually concerned Windows Media Player, resulting in a €497 million fine in 2004 and requiring Microsoft to offer a version of Windows without the media player. See, e.g., Ayres and Nalebuff (2005), who criticize the remedy.

2.2.2 Google

Unlike enforcement actions against Microsoft, which the US's Justice Department started, European authorities took the initiative this time. Google required device manufacturers to pre-install Google Search and Chrome browser as a condition for licensing its Play Store, prevented manufacturers from selling devices running alternative ("forked") versions of Android, and provided financial incentives to exclusively pre-install Google Search. The European authorities concluded that Google's practices exploited users' status quo bias to strengthen and maintain its dominance in mobile search. The General Court explicitly acknowledged this "status quo bias" in its judgment, noting that pre-installation could significantly increase the usage of Google's services due to users' tendency to stick with available apps. In July 2018, the European Commission imposed a record €4.34 billion fine (later reduced to €4.125 billion in 2022). The Court upheld most of the Commission's findings but annulled the portion related to revenue-sharing agreements.

As a remedy, Google was required to implement a choice screen for European Android users to select their preferred browser and search engine (see Figure 2). The case remains under appeal at the European Court of Justice. The DOJ's 2020 complaint against Google is based on similar facts. Judge Metha recently ruled on Google's liability but a separate trial is taking place to decide on the applicable remedies. The DOJ expressly indicated that a choice screen could be a way to deal with Google's illegal distribution (see, e.g., Elias 2024).

According to Decarolis, Li, and Paternollo (2023) changes to default settings in the EEA, Russia, and Turkey have consistently led to a decline in Google's market share. However, they note that effects vary across regions. In the EEA, the impact was less than 1 percentage point, whereas in Russia and Turkey, the decline exceeded 10 percentage points. They claim that the differences are influenced by factors such as the scope of the intervention, local market dynamics, and the specific design of the regulatory measures. While the authors group changes to default settings into a single category, only the EEA and Russia mandated choice screens. The impact of the choice screen in the EEA was negligible, as the authors acknowledge.

In Russia, the choice screen was a part of a much bigger intervention. Google had to terminate its exclusive agreements with OEMs, permit the preinstallation of rival search engines, cease exclusively promoting Google Search, and present alternative search engines through a choice window (later replaced by a choice widget). However, only Yandex and Mail.ru were offered as alternatives to Google. As Cooper et al note, "[c]ultural and political considerations may have ... supported the uptake of Yandex as a"national champion" firm" (Cooper, Alisa, van den Boom, Jasper, and Arnao, Zander 2024, 5).

Turkey prohibited Google from requiring or implying exclusive preinstallation or home-screen placement of Google Search, default assignment of Google Search to all search points, or circumventing these rules through financial incentives. Huawei, a key player in Turkey's market, switched its default search to Yandex, contributing to an estimated 10% market share shift toward Yandex. Turkey's strategy allowed OEMs freedom to choose exclusive defaults without a choice screen; Yandex's strong local presence and Huawei's popularity amid US export restrictions further supported this shift (Decarolis, Li, and Paternollo 2023, 8). To be clear, Turkey changed the incentive structures between Google and the OEMs. This triggered default chnange, which is a different type of intervention, one that is more likely to lower Google's market share Allcott et al. (2020).

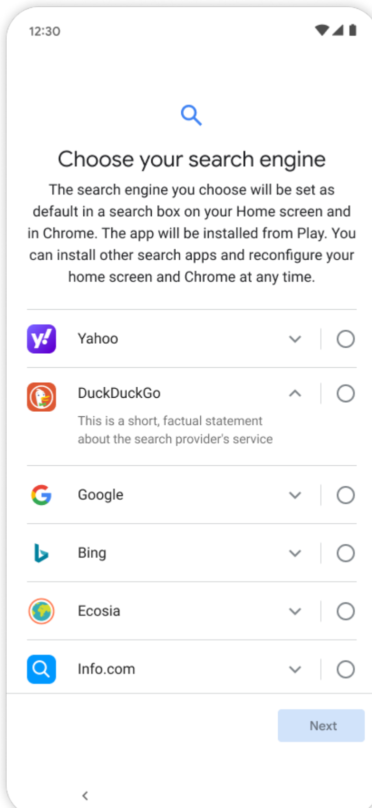

Figure 2: Android Choice Screen

2.2.3 Digital Markets Act

The Digital Markets Act (DMA), which became enforceable in March 2024, intends to "make the markets in the digital sector fairer and more contestable."[3] With this aim, it introduced choice screens as a key mechanism to promote competition in digital markets dominated by tech designated gatekeepers (Alphabet, Amazon, Apple, ByteDance, Meta, and Microsoft). Under Article 6(3) of the DMA, gatekeepers must prompt users during first use to select their preferred search engine, web browser, and virtual assistant from a list of main available service providers, rather than defaulting to the gatekeeper's choice. Non-compliance can result in fines of up to 10% of a company's worldwide turnover. One of the main differences with the Android choice screen is that the DMA's also applies

[3]European Commission. About the Digital Markets Act. Available at: https://digital-markets-act.ec.europa.eu/about-dma_en (last visit March 9, 2025).

to Apple. Initially Apple only showed the choice screen the "first time a user in the EU open[ed] Safari on their iPhone or iPad running a minimum of iOS 17.4 or iPadOS 18.2".[4] As of iOS 18.2 and iPadOS 18.2, Apple introduced a few design and functionality modifications.

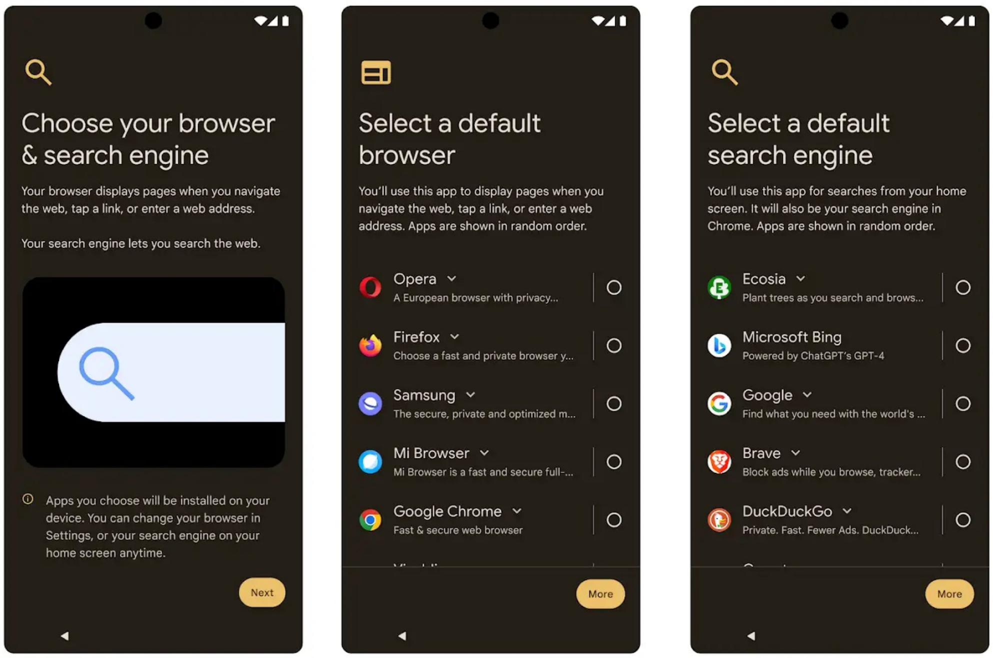

Figure 3: DMA Choice Screen

The main hypothesis to test is that Google and Chrome's market share should decrease as a consequence of the DMA. However, the effect is expected to be close to zero, since current market shares mostly reflect people's preferences rather than inertia. This hypothesis follows from (i) the minimal impact of the Microsoft choice screen, which shows that IE's market share was declining all over the world; and (2) the negligible impact of the Android Choice Screen, which suggests that either people have a strong preference for Google and Chrome and/or that very few people are interested in experimenting with applications they do not use.

3 Methods

Whether a law had an effect, intended or not, is always an empirical question. This work assesses the DMA's impact with time-series cross-sectional data from StatCounter.

[4] Apple Developer. *About the browser choice screen in the EU*. https://developer.apple.com/support/browser-choice-screen/ (last visit March 9, 2025).

3.1 Data

StatCounter Global Stats,[5] is a web analytics service that tracks over 5 billion page views monthly across more than 1.5 million websites globally. StatCounter collects browsing data by analyzing page views rather than unique visitors, allowing it to account for browsing frequency patterns. The service provides comprehensive statistics on browser usage, search engines, and device types across diverse geographic regions. Unlike some competitors, StatCounter does not apply geo-weighting adjustments to its worldwide statistics, meaning the raw data is not modified to compensate for potential sampling biases across different regions. This methodological choice stems from StatCounter's position that there is an absence of reliable and current internet usage data sources necessary for accurate weighting calculations. For this project, monthly data on search engine market share across members of the European Economic Area (EEA) and high-income countries (following the World Bank's income classification)[6] from 2017 through early 2024 was extracted, providing approximately 90 pre-treatment observations before the intervention point.

Members of the EEA that are not members of the European Union were filtered out because they implement their own enforcement mechanisms. This analysis also excluded Russia and Turkey due to the interventions discussed in the previous section, which significantly impacted Google's market share. For the *search market* analysis, countries in which Google's market share followed a clearly different trend than the average for the whole group of developed countries between 2017 and 2015 were filtered out. Different methods were considered to set a threshold. However, all of them included countries with a national search engine (i.e., Czech Republic and Korea) and those where Yahoo's market share exceeded 10% in 2017 (i.e., Japan and Hong Kong). Finally, three countries had month-to-month variations that exceeded 15%, which were clearly outliers after performing an IQR analysis (i.e., American Samoa, Greenland, and Seychelles). Said countries were removed as well.

3.2 Estimand and Estimators

The fundamental challenge in causal inference is the estimation of counterfactual outcomes—what would have happened to treated units had they not received treatment. Following Rubin's potential outcomes framework (Rubin 2005), the treatment effect for an individual unit is defined as the difference between the observed outcome under treatment ($Y_i(1)$) and the unobserved counterfactual outcome under non-treatment ($Y_i(0)$): $Y_i(1) - Y_i(0)$.

The validity of ATT estimation depends on identification strategies that address selection bias and confounding. Various econometric approaches, including difference-in-differences (DID), synthetic control methods, and matrix completion techniques, have

[5] https://gs.statcounter.com/

[6] "The World Bank classifies economies for analytical purposes into four income groups: low, lower-middle, upper-middle, and high income. For this purpose it uses gross national income (GNI) per capita data in U.S. dollars, converted from local currency using the World Bank Atlas method, which is applied to smooth exchange rate fluctuations." See, https://datatopics.worldbank.org/world-development-indicators/the-world-by-income-and-region.html

been developed to estimate ATT under different assumptions about treatment assignment and unobserved heterogeneity (see, e.g., Athey and Imbens 2017).

DiD represents the foundational approach for causal inference with panel data. In its simplest form, DiD compares the change in outcomes for treated units to the change for untreated units. The key identifying assumption is that treated and control units would have followed parallel trends in the absence of treatment.

When extending to multiple time periods and units, the two-way fixed effects (TWFE) model becomes standard:

$$Y(it) = \alpha(i) + \gamma(t) + \tau D(it) + X(it)\beta + \varepsilon(it)$$

where $\alpha(i)$ represents unit fixed effects, capturing time-invariant heterogeneity across units, and $\gamma(t)$ represents time fixed effects, accounting for common shocks affecting all units in a given period. The term $D(it)$ is the treatment indicator, which equals 1 if unit i is treated at time t and 0 otherwise, while $X(it)\beta$ represents a vector of time-varying covariates with associated coefficients. The error term $\varepsilon(it)$ captures unobserved factors that vary across both units and time, including any idiosyncratic shocks or omitted variables not accounted for by the fixed effects or covariates.

However, recent literature has identified significant limitations of TWFE, particularly when treatment timing varies across units and treatment effects are heterogeneous (see, e.g., Liu, Wang, and Xu 2024). In such settings, TWFE estimates can be biased and difficult to interpret causally.

Synthetic control methods (SCM) were developed to address some limitations of DiD approaches, particularly for settings with few treated units (Abadie, Diamond, and Hainmueller 2010). SCM constructs a counterfactual for each treated unit as a weighted combination of control units, where weights are chosen to minimize pre-treatment differences. While SCM offers advantages over traditional DiD by allowing for time-varying effects of unobserved confounders (rather than requiring parallel trends), the method faces important limitations, most importantly challenges in accommodating multiple treated units with heterogeneous effects.

Matrix completion represents a significant methodological advancement that addresses many limitations of DiD and SCM approaches (Athey et al. 2021). Matrix completion treats the panel data as a partially observed matrix where entries corresponding to treated unit-periods are "missing" (representing counterfactual outcomes that must be imputed). The approach assumes that the complete matrix of potential outcomes under no treatment has a low-rank structure, reflecting latent factors that drive market shares across countries and time.

This framework operates on two $N \times T$ matrices: the outcome matrix $\mathbf{Y}$ (e.g., market shares), which contains missing values for treated unit-periods, and the covariate matrix $\mathbf{X}$ (e.g., median age), which is fully observed.

The potential outcomes under control are modeled as:

$$\mathbf{Y}(0) = \mathbf{X}\beta + \mathbf{L}^* + \varepsilon$$

where $\mathbf{L}^*$ is an $N \times T$ matrix of unobserved latent factors with low-rank structure. Formally, a matrix has rank r if all its columns can be expressed as linear combinations of r basis vectors. The low-rank assumption ($r \ll \min(N, T)$) implies that the $N \times T$ outcomes are driven by a small set of r common factors that simultaneously influence both units and time periods. These latent factors capture systematic patterns not explained by observed covariates, such as cross-country economic interdependencies, temporally evolving consumer preferences, and unmeasured technological diffusion processes.

The latent factor model accommodates time-varying unobserved confounders, overcoming a key limitation of fixed effects models. Unlike SCM, matrix completion imposes no constraints on weights. Instead, regularization encourages parsimonious representations while allowing flexible, data-driven patterns.

This work implements matrix completion using the *fect* R package (Liu et al. 2022), specifying the *mc* option to estimate low-rank structure via nuclear norm minimization. Models include two-way fixed effects and use a parametric bootstrap with 1,000 iterations to quantify uncertainty. The regularization parameter is selected via cross-validation to minimize prediction error on held-out data. Two predictors were used in the model specification: median age and iOS's market share of each country.

4 Results

4.1 Google

Google's market share has not changed much in the last 15 years in most of the western world. Google is particularly dominant in the mobile segment of the market, where its average market share has been above 96% between 2017 and 2022 in both EU and countries in the donor pool. Figure 4 illustrates Google's market share trends in both treated (EU) and comparison (non-EU high-income) countries from 2017 to early 2025. The graph reveals several important patterns. First, Google maintained extraordinary dominance across both groups, with average market share consistently above 96% in the mobile search segment. Second, a notable baseline difference exists during 2017-2021, with Google's share actually increasing in the comparison group while remaining relatively stable in the EU. This divergent pre-treatment trend presents challenges for traditional difference-in-differences analysis, which motivated the matrix completion approach. Around 2021, both groups experienced a modest decline of approximately 1%, followed by convergence in early 2025 where the initial baseline difference disappeared. Prior to the DMA implementation in March 2024, Google's market share was higher in the EU than the comparison group.

Figure 5 presents the key results from the matrix completion analysis estimating the

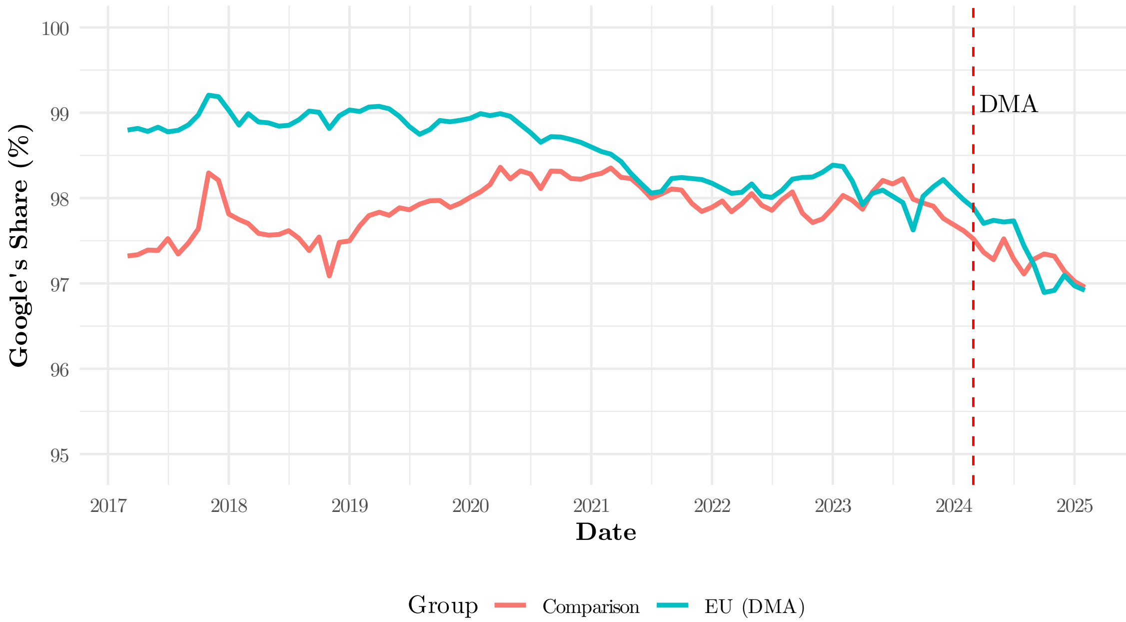

Figure 4: Google in Treated and Comparison Groups

impact of the DMA on Google's market share. This visualization displays the estimated average treatment effect on the treated (ATT) $(Y_i(1) - Y_i(0))$ for each month, showing that Google's market share declined by approximately 0.56% following the DMA implementation (95% CI: [-0.71, -0.39], p<0.0001). The pre-treatment fit is not perfect but all the differences are very close to zero.

The figure illustrates both the point estimate and the confidence interval derived from parametric bootstrap with 1,000 iterations. While statistically significant, this effect size demonstrates that the DMA's impact on Google's dominant position remains modest—just over half a percentage point reduction from a market share above 95%. This finding aligns with the central hypothesis that choice screens produce minimal effects on entrenched market dynamics, falling below the 2% threshold observed in prior choice screen interventions like the Microsoft case.

4.2 Chrome

Figure 6 displays Chrome's market share trends across treated and comparison countries from 2017 through early 2025. The most striking feature is the remarkably parallel pre-treatment trends between the two groups, with Chrome consistently maintaining approximately 10% higher market share in the EU compared to the comparison group. This stable difference persisted through various market fluctuations, including two notable increases between 2017-2020 followed by a gradual decline until 2024. After the DMA implementation in March 2024 (marked by the vertical line), a differential pattern emerges: Chrome's market share initially increased in both groups but with a steeper rise in the

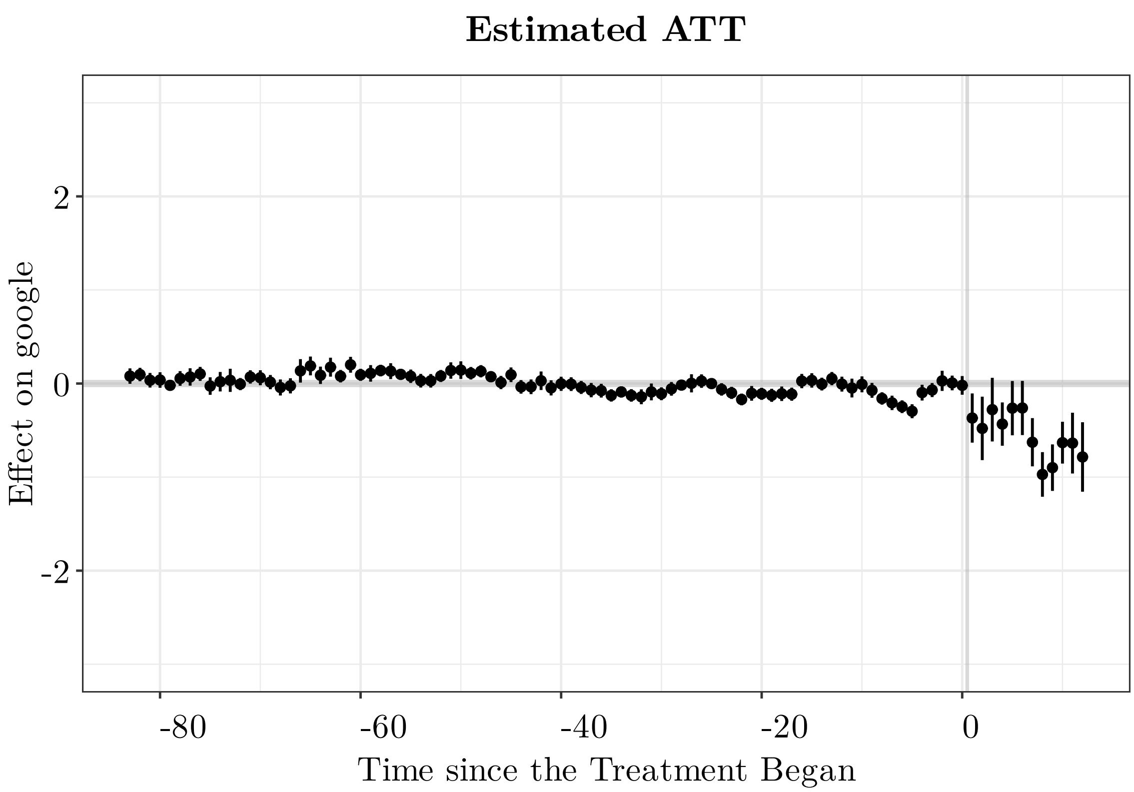

Figure 5: DMA's Effect on Google's Market Share

comparison group, followed by a decline in the EU while the comparison group remained stable. In recent months, Chrome's share has begun rising again in both groups, with a more pronounced increase in the EU. These divergent post-treatment patterns suggest a temporary negative effect on Chrome's market share that appears to be diminishing over time, consistent with the quantitative finding of a small treatment effect.

Figure 7 shows the estimated impact of the DMA on Chrome's market share. The ATT is -1.80 (95% CI = [-2.20, -1.41], p <.0001), an effect that is substantially larger than the DMA's impact on Google, yet still below 2%.

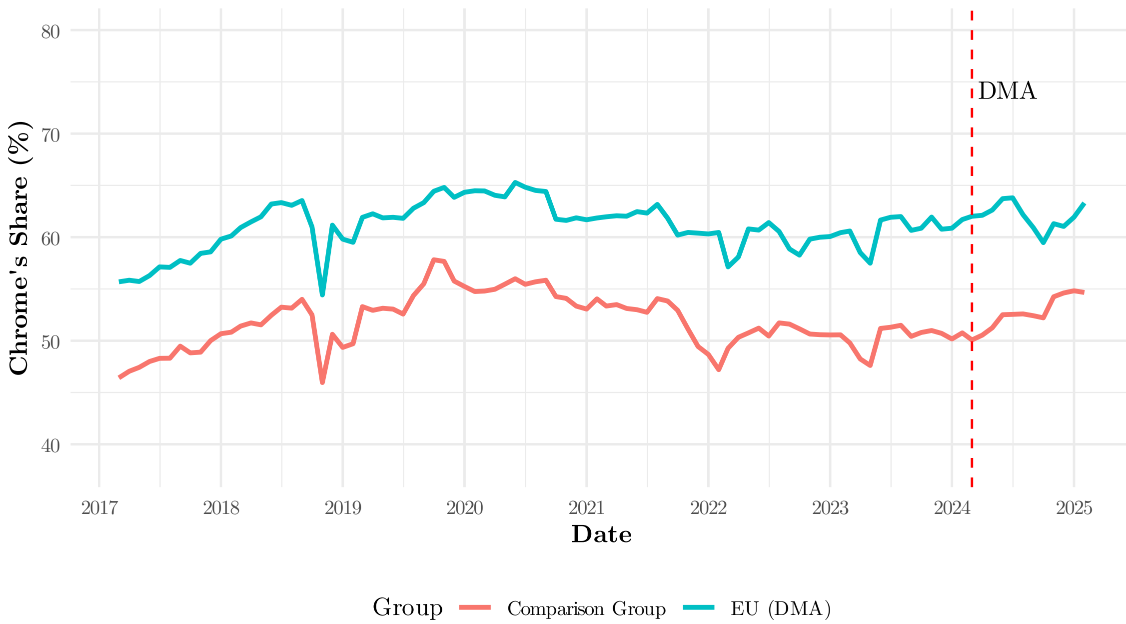

Figure 6: Chrome in Treated and Comparison Groups

5 Discussion

The findings of this study reinforce a familiar pattern in digital market regulation: while choice screens are introduced with great expectations, their actual impact on competition remains minimal. The results from the DMA intervention suggest that, consistent with previous cases, choice screens do not meaningfully alter users' preferences. The observed effects—marginal reductions in market share for Google Search and Chrome—highlight the limitations of relying on consumer choice alone to drive competition.

No single competitor captured substantial market share. Firefox and Opera showed the largest gain among browsers, but this increase remained below 0.5%—far from transformative or meaningful (see Figures 9 and 10 in the Appendix). Other alternative search engines and browsers showed no clear changes following the DMA implementation.

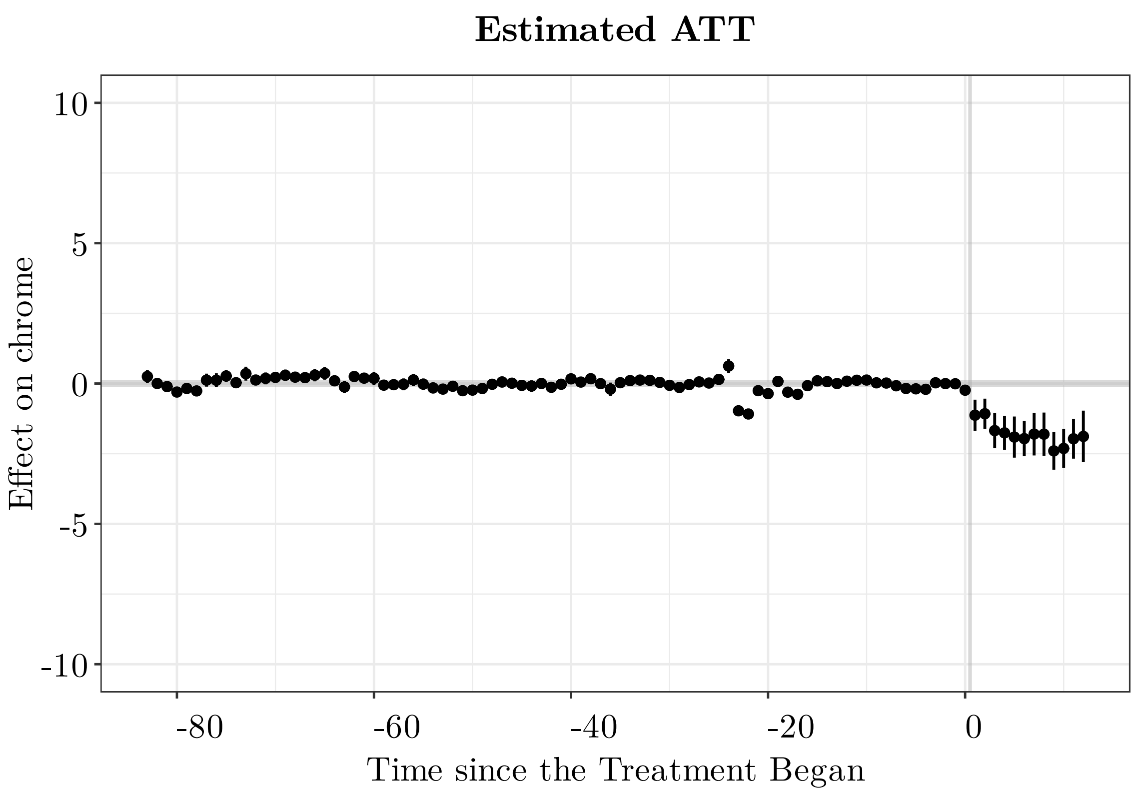

Figure 7: Chrome in Treated and Comparison Groups

Among search engines, only Ecosia appears to have increased it market share. While the effect is statistically significant, it is a trivial difference below 0.01%.

A key assumption behind choice screens is that consumer inertia sustains market dominance. However, the findings here suggest that consumers may not be as inert as conventionally assumed. The Microsoft case demonstrated that users were switching to Firefox and Chrome virtually everywhere, regardless of whether they saw a choice screen or not. Just like the Android intervention, the DMA's impact indicates that even when presented with alternative options, most users simply reaffirm their prior choices. Unlike IE, Google and Chrome are well regarded by most users. It is hard to hypothesize why people would switch to another product they consider inferior.

This raises questions about the "true" effectiveness of choice screens. Firefox and Opera report higher downloads (oddly before March 2024), but we see very slight changes in their market shares. In 2010, right after the Microsoft choice screen was displayed, Opera also reported increased downloads of its browser (Ostrovsky 2023). This may mean two things: either those who choose to experiment with alternative options switch back to their previous browser, or the share of users who are willing to experiment is negligible. If effectiveness is defined as "encouraging users to consider alternative options" and the browsers' assessment is correct, then choice screens may be seen as partially successful. However, if the goal is to increase market contestability, the evidence suggests that choice screens alone are ineffective.

Interventions in Russia and Turkey resulted in significantly larger shifts in market share, but these cases did not rely on choice screens alone—actually Turkey did not use a choice screen at all. Instead, they directly altered default settings, changing the competitive landscape in ways that choice screens do not. These structural changes created stronger incentives—especially for manufacturers—to shift away from Google, leading to market share declines exceeding 10%. The contrast between these interventions and the DMA suggests that change of default is far more effective than simply giving users a choice—a result that aligns with experimental studies the author conducted himself (Vasquez Duque 2024) as well as newer and more involved experiments (Allcott et al. 2020).

Several factors complicate the interpretation of these results. First, allegations of non-compliance with the DMA could mean that the choice screen implementation was not fully effective. Second, the timing of observed effects is unusual—market share shifts appear strongest in June rather than immediately after the March 2024 intervention. While this delayed effect suggests other market dynamics may have influenced the trends, it could also reflect the time required for new devices featuring the choice screens to reach consumers

Here it is also important to discuss the findings of survey and interview studies that have tested different designs for choice screens. These studies claim that the choice screens' lack of impact results from their poor design. When the experimental task itself is the engagement with a choice screen it is clear that people will pay attention to different designs, and those differences may affect their behavior. However, for those results to translate into the real world, people would have to engage with real-world choice screens with the same *interest* and *attention* they had in a paid study. The data show that is not the case.

6 Conclusions

This study provides compelling evidence that choice screens, as implemented in the DMA and previous regulatory interventions, have limited efficacy in altering entrenched market dynamics in digital markets. The findings reveal that while choice screens produce statistically significant effects on market share, these effects are minimal—typically below 2%—and do not meaningfully increase market contestability or reduce the dominance of incumbent firms. The results align with previous research on the Microsoft and Android choice screens, suggesting a consistent pattern across different interventions and time periods. While choice screens may encourage some users to consider alternatives, they fail to address the underlying factors that sustain market concentration, such as product quality, network effects, and ingrained user preferences. These findings raise important questions about the reliance on choice screens as a primary competition policy remedy and highlight the need for policymakers to consider more robust or complementary measures when antitrust intervention is deemed necessary.

7 Appendix

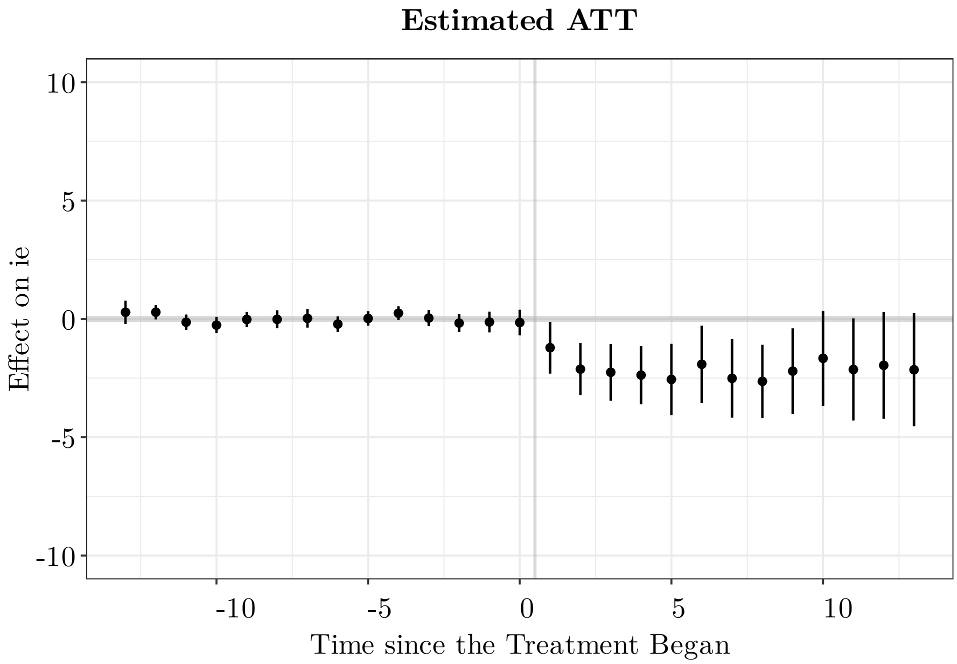

Figure 8: Microsoft Choice Screen's Impact

Figure 7 presents the matrix completion analysis of the 2010 Microsoft choice screen's impact on Internet Explorer's market share. This historical comparison provides important context for interpreting the DMA's effects. The visualization follows the same format as Figure 5, showing the average difference between the estimated controls and each treated unit for each period. The analysis includes all high-income countries from 2010 in the donor pool and reveals an average treatment effect of approximately 2.13% (95% CI: [-3.64, -0.62], $p<0.006$). This finding aligns with previous research using different comparison groups (Vasquez Duque 2023) and represents the upper bound of choice screen effectiveness observed across multiple regulatory interventions.

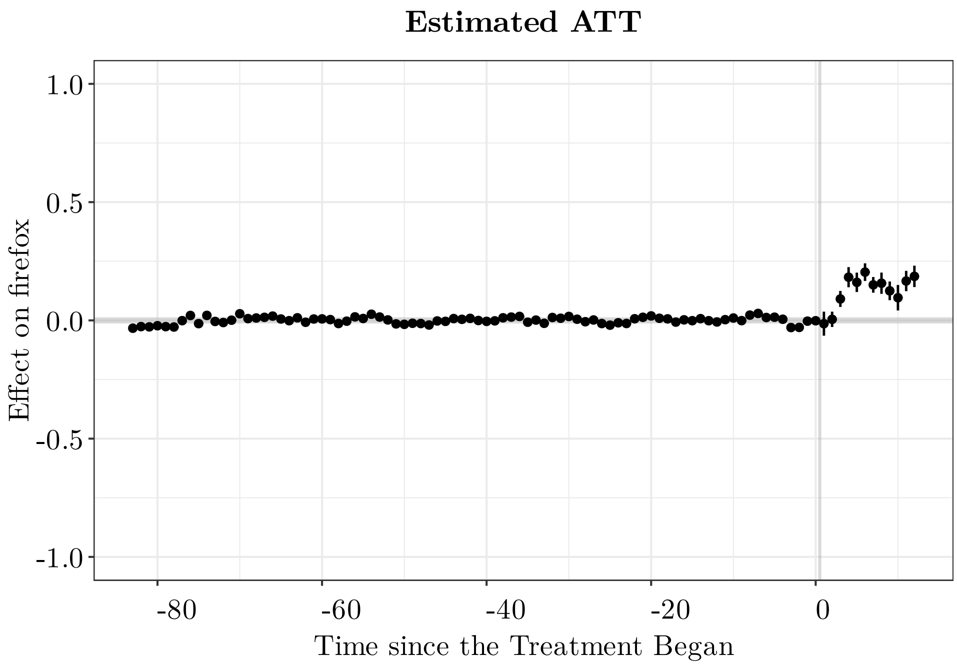

Figure 9: DMA's Impact on Firefox

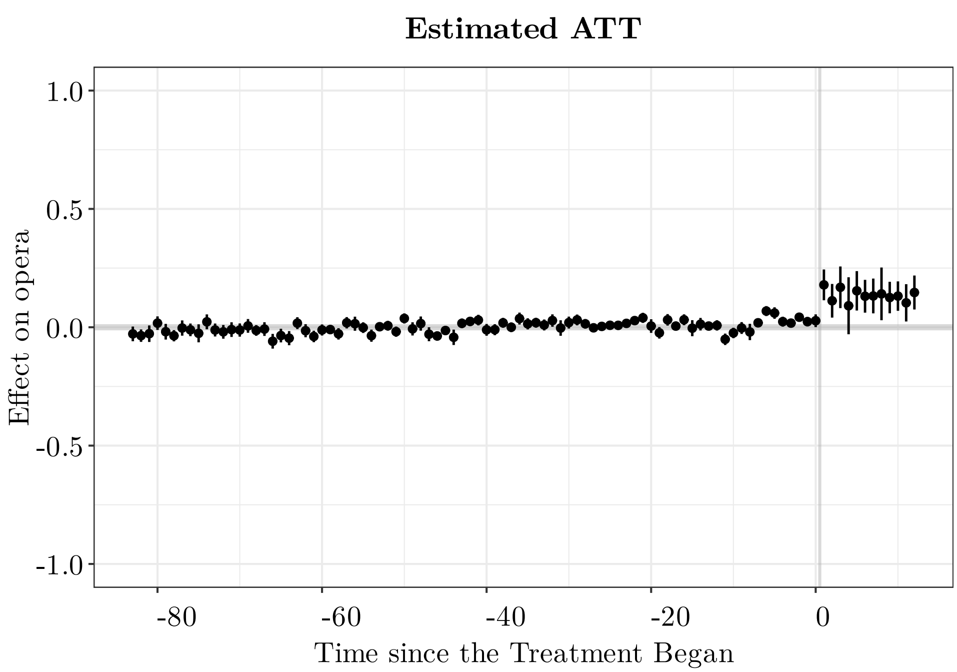

Figure 10: DMA's Impact on Opera

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