America's Digital Shield:
A New Online Monitoring System Is Making Google and Other Tech Companies Accountable to the Public

Written Testimony by

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Table of Contents

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It is my honor to speak to representatives of the great state of Texas today about the nationwide software system my team and I have developed to stop Big Tech companies from manipulating our elections and indoctrinating our children. This system is now running in all 50 states. A real-time dashboard summarizing the massive amount of data we are preserving and analyzing can be accessed at:

https://AmericasDigitalShield.com

I have been to Texas many times because my beautiful wife Misti, who died under suspicious circumstances the day after Christmas in 2019, was from Corpus Christi (see 2-min. TV news report at http://MistisDeath.com).

Since 2013, I have been conducting rigorous controlled experiments that have identified 10 new forms of influence that the internet has made possible and that are controlled exclusively by the Big Tech companies. We publish our findings in peer-reviewed journals.

These new techniques are among the most powerful forms of influence ever discovered in the behavioral sciences, and they are almost entirely invisible, which makes them especially dangerous.

I do not exaggerate when I tell you that our great nation unknowingly turned over its elections to Big Tech companies in 2012. I do not exaggerate when I tell you that the 2020 Presidential election was only one of hundreds of elections that Google has flipped without people's knowledge. I do not exaggerate when I tell you that Google has the power this year to shift between 6.4 and 25.5 million votes in the Presidential election.

In 2019, I told a committee of the Senate Judiciary, chaired by Ted Cruz, about the threat Google posed to our democracy and about two methods for stopping them. The first would be to declare their index – the database they use to generate search results – to be a public commons; that's light-touch regulation that has been repeatedly applied to essential commodities and services in the US for over a century.

The second method is to set up a large-scale monitoring system that will preserve and analyze the *actual data* they are sending to real voters – in other words, to *track* them – to do to Google what they do to us and our children 24 hours a day.

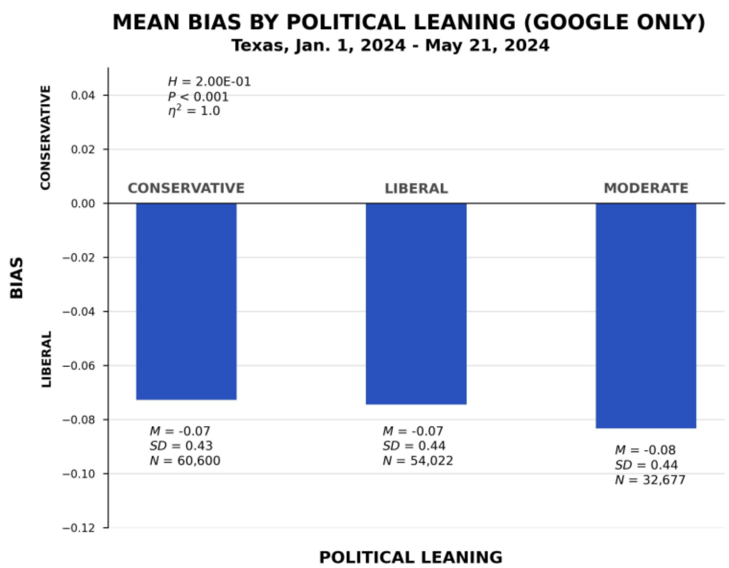
By the time I testified again before Congress in December of last year (2023), we had set up our first nationwide system, which is now preserving data Google and other companies are sending to our panel of more than 14,500 registered voters in all 50 states.

We have now preserved more than 90 million of what Google employees call "ephemeral experiences" – fleeting experiences like search results and YouTube recommendations that impact people – especially undecided voters – and then disappear, normally leaving no paper trail.

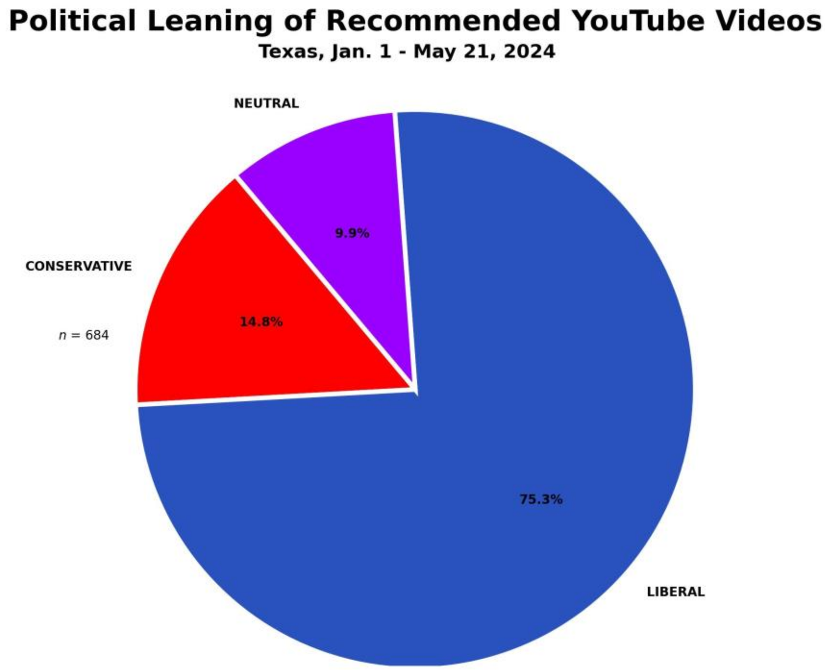
At this very moment, our monitoring system is revealing a wealth of disturbing examples of how Google-and-the-Gang are quietly manipulating our society. For example, right now Google is sending "register-to-vote" reminders to Democrats at 2 1/2 times the rate at which they're sending them to Republicans.

Right now, Google's YouTube is recommending shockingly violent and sexual videos to children and teens. Check https://AmericasDigitalShield.com for graphic examples.

Right now, Google is sending liberally biased search results to liberals, moderates, *and conservatives* in Texas:

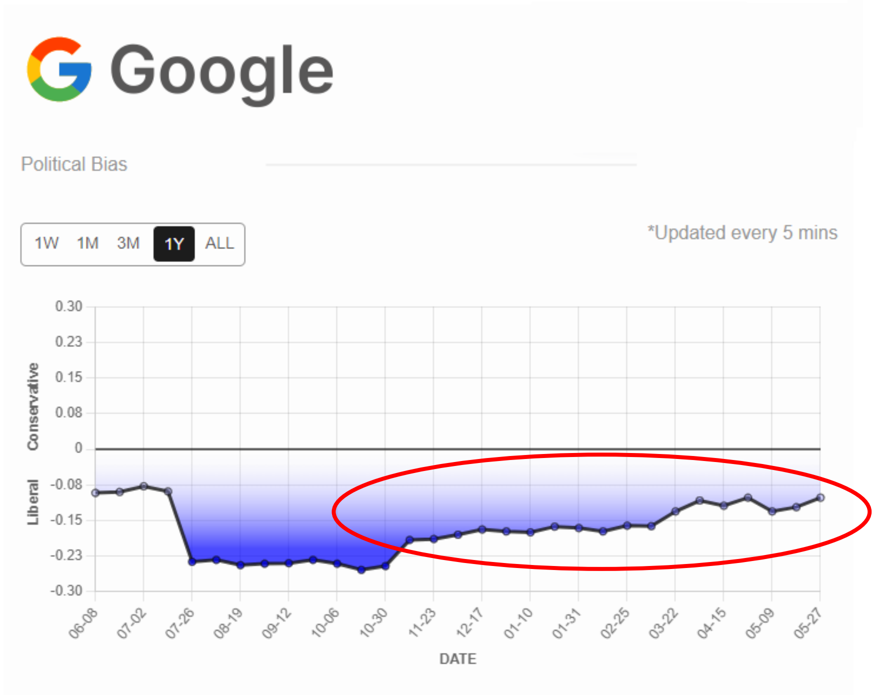


Right now, Google's YouTube is recommending liberally biased videos to registered voters in Texas at *twice the rate* that one would expect by chance:



But monitoring can stop them. In November 2020, after Senator Cruz sent a threatening letter to the CEO of Google about my research findings – Google immediately *stopped* its election interference in the Georgia runoff elections (see https://LetterToGoogleCEO.com).

And since we went public with our new nationwide monitoring system in November 2023, over the past six months Google search results have been steadily and gradually becoming less politically biased:



I'm working now with DC attorneys to file a complaint against Google with the FEC, and I'm working with AGs, election integrity groups, parenting groups, and others to develop ways of using our monitoring data to force Big Tech companies to stand down.

We have court-admissible data now in 15 states. That's how far $4 million in donations have gotten us. Now we need to rapidly expand the system so that we have court admissible data in all 50 states. It will take $50 million to make this system permanent.

Without a permanent monitoring system in place, we will be handing over our democracy and the minds of our children to the new Tech Lords. We will, quite literally, have no idea what they're doing.

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APPENDIX I

APPENDIX I: Links of Possible Interest

https://2023EpsteinTestimony.com – December 13, 2023 Epstein testimony before a subcommittee of the Senate Judiciary Committee, 6-min. video

https://AmericasDigitalShield.com – a live dashboard that documents Big Tech manipulation, bias, and indoctrination in real time.

https://2023WrittenTestimony.com – 480-page written testimony of Dr. Epstein entered into the Congressional Record on December 13, 2023

https://EpsteinInTheNewYorkPost.com – May 2023 article in the *New York Post* about Dr. Epstein's research by reporter Miranda Devine. It ends, "Only Epstein is standing in the way."

https://TechWatchProject.org – a new website about Dr. Epstein's election monitoring project.

https://HowGoogleStoppedTheRedWave.com – a 2022 article by Dr. Epstein in *The Epoch Times*.

https://MyGoogleResearch.com – a webpage where you can learn more about Dr. Epstein's research on online influence and where you can also support that research with donations to the American Institute for Behavioral Research and Technology, a nonprofit, nonpartisan 501(c)(3) public charity.

https://EpsteinOnRogan.com – a 160-minute video recording of Dr. Epstein's 2022 appearance on The Joe Rogan Experience.

https://EpsteinOnTuckerCarlson.com – Dr. Epstein on Tucker Carlson in 2022, 56-min. video

https://EpsteinOnSteveBannon.com – the first of three appearances by Dr. Epstein on Steve Bannon's show in September 2023

https://EpsteinOnAmericanThoughtLeaders.com (90 min. video, Epoch Times interview with Dr. Epstein on his research on Big Tech

https://EpsteinOnSTEMTalks – 90-min. biographical audio interview with Dr. Epstein)

https://MyPrivacyTips.com – an essay by Dr. Epstein about how you can protect yourself and your children from surveillance by Google-and-the-Gang.

https://EpsteinTestimony.com – Dr. Epstein's 2019 Congressional testimony about the threat Google-and-the-Gang pose to democracy (7-minute video).

https://EpsteinOnSTEMTalks – a 90-minute biographical audio interview with Dr. Epstein.

https://TamingBigTech.com – an essay by Dr. Epstein about the development of his first election monitoring system, deployed before the 2016 Presidential election.

https://CreepyLine.org – an 80-minute documentary film – "The Creepy Line" – featuring Dr. Epstein's research. It warns about surveillance, censorship, and manipulation by Google-and-the-Gang. It also features Dr. Jordan Peterson and other experts.

https://TheCaseForMonitoring.com – a 15-minute video in which Dr. Epstein summarizes findings from his online monitoring in the days leading up to the 2020 Presidential Election and the 2021 Senate runoff elections in Georgia.

https://DrRobertEpstein.com – Dr. Epstein's personal website.

https://AIBRT.org – website of the American Institute for Behavioral Research and Technology.

https://TheNewCensorship.com – Dr. Epstein on Google's blacklists, in *US News & World Report*.

https://TamingBigTech.com – article by Dr. Epstein on AIBRT's 2016 election monitoring project.

https://LetterToGoogleCEO.com – Nov. 5, 2020 letter from three US Senators to Google CEO about Epstein's findings in the 2020 Presidential race.

https://SearchEngineManipulationEffect.com – SEME: 2015 seminal paper on the power that search engines have to shift opinions and votes, published in the *Proceedings of the National Academy of Sciences USA*, downloaded or accessed from the website of the National Academy of Sciences more than 250,000 times.

https://SEMEandOperantConditioning.com – 2024 peer-reviewed study in *Behavior and Social Issues*

https://TargetedMessagingEffect.com – TME: 2023 peer-reviewed study in *PLOS ONE* showing the power that targeted messages on Twitter have to shift opinions and votes https://AnswerBotEffect.com – ABE: 2021 peer-reviewed study in *PLOS ONE* reporting new research on the power that personal assistants and answer boxes (and hence AIs) have to shift opinions and votes.

https://MultipleTopicsResearch.com – peer-reviewed research report in *PLOS ONE* on the power that search engines have to shift opinions and votes about perhaps any topic at all.

https://VideoManipulationEffect.com – VME: peer-reviewed research report in press in *PLOS ONE* on the power that YouTube has to shift opinions and votes

https://SearchSuggestionEffect.com – SSE: preprint of a research report on the power that Google search suggestions have to shift opinions and votes, currently under review.

https://OpinionMatchingEffect.com – OME: preprint of a research report on the power that online quizzes have to shift opinions and votes, currently under review.

https://MultipleExposureEffect.com – MEE: preprint of new report on the additive impact of repeated exposures to similarly biased content, currently under review.

https://DigitalPersonalizationEffect.com – DPE: new research on the power that personalization has to increase the impact of biased content, submitted for presentation.

APPENDIX II

APPENDIX II:
The Methodology of SEME Experiments

The methodology of SEME experiments adheres to the highest standards of research in the social and behavioral sciences. All experiments are randomized, controlled, double-blind, and counterbalanced (Epstein and Robertson, 2015a). Multiple SEME experiments conducted over a period of more than five years have involved more than 10,000 participants and five national elections in four countries. Reasonable efforts have been made to assure that participants are diverse across multiple demographic characteristics, and, when possible, representative of the voting population. When samples are not representative of the voting population, adjustments are made statistically or by examining subsamples.

In most experiments, participants are selected who are "undecided," by which I mean either that they haven't yet made up their minds, or, in some cases, that we are deliberately showing them materials from an election they are not familiar with (for example, when we show people from the U.S. materials from an election in Australia).

All search results and web pages used in the experiments are real, drawn from the internet and from Google's search engine. The elections we have examined are also real: the 2010 election for Prime Minister of Australia; the 2014 Lok Sabha election in India; the 2015 national election in the UK, and the 2016 and 2018 elections in the U.S.

Search results are presented to participants using a mock search engine called Kadoodle, which looks and functions almost exactly like Google. The difference between Google and Kadoodle is that with Kadoodle, we control what search results we show and the order in which those results are shown. Our search results link to copies of real web pages, but links on those pages have been disabled so we can keep our research participants in a closed online environment.

In the basic procedure, participants are randomly assigned to one of three groups: a group in which search results favor Candidate A – which means that high-ranking results link to web pages that make Candidate A look better than his or her opponent – a group favoring Candidate B, and a group in which neither candidate is favored in search results (the control group).

Participants are told they will be asked to use our custom search engine, Kadoodle, to conduct research on political candidates. They are first asked to read short paragraphs about each candidate and then asked several questions about each candidate: How much they like each candidate, trust each candidate, and so on. They are also asked, both in a binary fashion and on a scale, which candidate they would vote for if they had to vote today. These are all "pre-search questions."

Then, typically, they are given up to fifteen minutes in which to use the Kadoodle search engine to conduct further research about the candidates. They are typically given access to five pages of search results, with six results per page (30 in total), and they can navigate through the search results and the web pages exactly as they would on Google. They can stop searching when they please.

Then they are asked those same questions about the candidates; now these are "post-search questions."

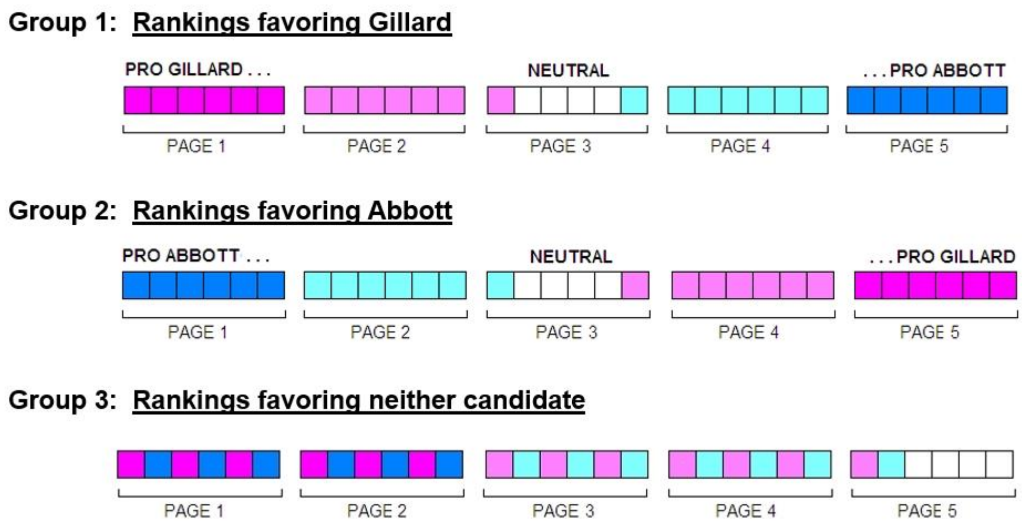

Figure 1. In a typical SEME experiment, in one group, search results are ordered in a way that favors Candidate A (Gillard, above). In a second group, the ordering is reversed, so it favors Candidate B (Abbott, above). And in a control group, the ordering alternates, so neither candidate is favored.

Remember that the only difference between the three groups is the order in which the search results are shown. All participants in all three groups have full access to all the search results and all the web pages.

The typical findings are as follows:

- Prior to search, all three groups tend to answer the pre-search questions the same way.
- After the search, the opinions and voting preferences of people in the control group shift very little or not at all.
- After the search, both the opinions and the voting preferences of people in the two bias groups shift fairly dramatically in the direction of the favored candidate. In other words, opinions and votes shift in opposite directions in the two groups.
- A shift of 20 percent or more is typical. In large studies in which we have enough participants to look at demographic differences, we have found shifts in the 60-to-

80 percent range in some demographic groups. In other words, some people are especially trusting of search results.

- Typically, very few people show any awareness of the bias they have seen. In a large study we conducted in India in 2014, for example – a study with more than 2,000 undecided voters throughout India in the midst of an intense election – 99.5 percent of our participants showed no awareness of bias in the search results we showed them.
- The very few people who do detect the bias tend, on average, to shift even farther in the direction of the bias.

Some of my SEME research attempts to explain why the effect is so large. One reason appears to be that people trust algorithmic output, believing that because it is computer-generated, it is inherently objective and unbiased.

Research I have conducted also suggests that SEME is a large effect because people are conditioned – very much like rats in a Skinner box – to believe that results at the top of the list are better and truer than results farther down the list (Epstein et al., in press). This is because most searches we conduct are for simple facts, such as “Who is the governor of Texas?” The correct answer always turns up at the top of the list, which is one reason 50 percent of all clicks go to the top two search positions.

But then that day comes when we search for something with a less certain answer: What is the best sushi restaurant in town? Who is the best candidate? Again, we are most likely to believe the highest-ranking answers.

When, in one experiment, we changed people’s beliefs about high-ranking search results by placing answers to simple questions in random positions in lists of search results, politically-biased search results had less impact on them.

APPENDIX III

APPENDIX III
Article from *Bloomberg Businessweek*, July 15, 2019

https://www.bloomberg.com/news/articles/2019-07-15/to-break-google-s-monopoly-on-search-make-its-index-public
Entered into The Congressional Record, July 16, 2019

Bloomberg Businessweek

To Break Google's Monopoly on Search, Make Its Index Public

The tech giant doesn't have to be dismantled. Sharing its crown jewel might reshape the internet.

By
Robert Epstein
July 15, 2019, 3:00 AM PDT

PHOTO ILLUSTRATION: 731; PHOTO: GETTY IMAGES

1

Recognition is growing worldwide that something big needs to be done about Big Tech, and fast.

More than $8 billion in fines have been levied against Google by the European Union since 2017. Facebook Inc., facing an onslaught of investigations, has dropped in reputation to almost rock bottom among the 100 most visible companies in the U.S. Former employees of Google and Facebook have warned that these companies are "ripping apart the social fabric" and can "hijack the mind."

Adding substance to the concerns, documents and videos have been leaking from Big Tech companies, supporting fears—most often expressed by conservatives—about political manipulations and even aspirations to engineer human values.

Fixes on the table include forcing the tech titans to divest themselves of some of the companies they've bought (more than 250 by Google and Facebook alone) and guaranteeing that user data are transportable.

But these and a dozen other proposals never get to the heart of the problem, and that is that Google's search engine and Facebook's

2

social network platform have value only if they are intact. Breaking up Google's search engine would give us a smattering of search engines that yield inferior results (the larger the search engine, the wider the range of results it can give you), and breaking up Facebook's platform would be like building an immensely long Berlin Wall that would splinter millions of relationships.

With those basic platforms intact, the three biggest threats that Google and Facebook pose to societies worldwide are barely affected by almost any intervention: the aggressive surveillance, the suppression of content, and the subtle manipulation of the thinking and behavior of more than 2.5 billion people.

Different tech companies pose different kinds of threats. I'm focused here on Google, which I've been studying for more than six years through both experimental research and monitoring projects. (Google is well aware of my work and not entirely happy with me. The company did not respond to requests for comment.) Google is especially worrisome because it has maintained an unopposed monopoly on search worldwide for nearly a decade. It controls 92 percent of search, with the next largest competitor, Microsoft's Bing, drawing only 2.5%.

Fortunately, there is a simple way to end the company's monopoly without breaking up its search engine, and that is to turn its "index"—the mammoth and ever-growing database it maintains of internet content—into a kind of public commons.

There is precedent for this both in law and in Google's business practices. When private ownership of essential resources and services—water, electricity, telecommunications, and so on—no longer serves the public interest, governments often step in to control them. One particular government intervention is especially relevant to the Big Tech dilemma: the 1956 consent decree in the U.S. in which AT&T agreed to share all its patents with other companies free of charge. As tech investor Roger McNamee and others have pointed out, that sharing reverberated around the world, leading to a significant increase in technological competition and innovation.

4

Doesn't Google already share its index with everyone in the world? Yes, but only for single searches. I'm talking about requiring Google to share its entire index with outside entities—businesses, nonprofit organizations, even individuals—through what programmers call an application programming interface, or API.

Google already allows this kind of sharing with a chosen few, most notably a small but ingenious company called Startpage, which is based in the Netherlands. In 2009, Google granted Startpage access to its index in return for fees generated by ads placed near Startpage search results.

With access to Google's index—the most extensive in the world, by far—Startpage gives you great search results, but with a difference. Google tracks your searches and also monitors you in other ways, so it gives you personalized results. Startpage doesn't track you—it respects and guarantees your privacy—so it gives you generic results. Some people like customized results; others treasure their privacy. (You might have heard of another privacy-oriented alternative to Google.com called DuckDuckGo, which aggregates information obtained from 400 other non-Google sources, including its own modest crawler.)

If entities worldwide were given unlimited access to Google's index, dozens of Startpage variants would turn up within months; within a year or two, thousands of new search platforms might emerge, each with different strengths and weaknesses. Many would target niche audiences—some small, perhaps, like high-end shoppers, and some huge, like all the world's women, and most of these platforms would do a better job of serving their constituencies than Google ever could.

These aren't just alternatives to Google, they are competitors—thousands of search platforms, each with its special focus and emphasis, each drawing on different subsets of information from Google's ever-expanding index, and each using different rules to decide how to organize the search results they display. Different platforms would likely have different business models, too, and business models that have never been tried before would quickly be tested.

This system replicates the competitive ecology we now have of both traditional and online media sources—newspapers, magazines, television channels, and so on—each drawing on roughly the same body of knowledge, serving niche audiences, and prioritizing information as it sees fit.

But what about those nasty filter bubbles that trap people in narrow worlds of information? Making Google's index public doesn't solve that problem, but it shrinks it to nonthreatening proportions. At the moment, it's entirely up to Google to determine which bubble you're in, which search suggestions you receive, and which search results appear at the top of the list; that's the stuff of worldwide mind control. But with thousands of search platforms vying for your attention, the power is back in your hands. You pick your platform or platforms and shift to others when they draw your attention, as they will all be trying to do continuously.

If that happens, what becomes of Google? At first, not much. It should be allowed, I believe, to retain ownership and control of its index. That will assure it continues to do a

7

great job maintaining and updating it. And even with competition looming, change will take time. Serious competitors will need months to gather resources and generate traffic. Eventually, though, Google will likely become a smaller, leaner, more diversified company, especially if some of the other proposals out there for taming Big Tech are eventually implemented. If, over time, Google wants to continue to spy on people through its search engine, it will have to work like hell to keep them. It will no longer be able to rest on its laurels, as it has for most of the past 20 years; it's going to have to hustle, and we will all benefit from its energy.

My kids think Google was the world's first search engine, but it was actually the 21st. I can remember when search was highly competitive—when Yahoo! was the big kid on the block and engines such as Ask Jeeves and Lycos were hot commodities. Founded in 1998 amid a crowded field of competitors, Google didn't begin to dominate search until 2003, by which time it still handled only about a third of searches in the U.S. Search can be competitive again—this time with a massive, authoritative,

8

rapidly expanding index available to all parties.

The alternative is frightening. If Google retains its monopoly on search, or even if a government steps in and makes Google a public utility, the obscene power to decide what information humanity can see and how that information should be ordered will remain in the hands of a single authority. Democracy will be an illusion, human autonomy will be compromised, and competition in search—with all the innovation that implies—might never emerge. With internet penetration increasing rapidly worldwide, do we really want a single player, no matter how benign it appears to be, to control the gateway to all information?

For the system I propose to work fairly and efficiently, we'll need rules. Here are some obvious ones to think about:

Access. There might have to be limits on who can access the API. We might not want every high school hacker to be able to build his or her own search platform. On the other hand, imagine thousands of Mark Zuckerbergs battling each other to find better ways of organizing the world's information.

Speed. Google must not be allowed to throttle access to its index, especially in ways that give it a performance advantage or that favor one search platform over another.

Content. To prevent Google from engineering humanity by being selective about what content it adds to its index, all parties with API access must be able to add content.

Visibility. For people using Google to seek information about other search platforms, Google must be forbidden from driving people to itself or its affiliated platforms.

Removal. Google must be prohibited from removing content from its index. The only exception will be when a web page no longer exists. An accurate, up-to-date record of such deletions must be accessible through the API.

Logging. Google must log all visits to its index, and that log must be accessible through the API.

Fees. Low-volume external platforms (think: high school hackers) should be able to access the index free of charge. High-volume users (think: Microsoft Corp.'s Bing) should pay Google nominal fees set by regulators. That gives Google another incentive for maintaining a superior index.

Can we really justify bludgeoning one of the world's biggest and most successful companies? When governments have regulated, dismembered, or, in some cases, taken ownership of private water or electricity companies, they have done so to serve the public interest, even when the company in question has developed new technologies or resources at great expense. The rationale is straightforward: You may have built the pipelines, but water is a "common" resource that belongs to everyone, as David Bollier reminded us in his seminal book, Silent Theft: The Private Plunder of Our Common Wealth.

In Google's case, it would be absurd for the company to claim ownership rights over the contents of its index for the simple reason that it gathered almost all those contents. Google scraped the content by roaming the internet, examining webpages, and copying both the address of a page and language used on that page. None of those websites or any external authority ever gave Google permission to do this copying.

Did any external authority give Google permission to demote a website in its search results or to remove a website from its index? No, which is why both individuals and even top business leaders are sometimes traumatized when Google demotes or delists a website.

But when Google's index becomes public, people won't care as much about its machinations. If conservatives think Google is messing with them, they'll soon switch to other search platforms, where they'll still get potentially excellent results. Given the possibility of a mass migration, Google will likely stop playing God, treating users and constituencies with new respect and humility.

Who will implement this plan? In the U.S., Congress, the Federal Trade Commission, and the Department of Justice all have the power to make this happen. Because Google is a global company with, at this writing, 16 data centers—eight in the U.S., one in Chile, five in the EU, one in Taiwan, and one in Singapore—countries outside the U.S. could also declare its index to be a public commons. The EU is a prime candidate for taking such action.

But there is another possibility—namely, that Google itself will step up. This isn't as crazy as you might think. Likely prompted by the EU antitrust investigations, the company has quietly gone through two corporate reorganizations since 2015, and experts I've talked to in both the U.S. and the U.K. say the main effect of these reorganizations has been to distance Google's major shareholders from any calamities that might befall the Google search engine. The company's lawyers have also undoubtedly been taking a close look at the turbulent years during which Microsoft unsuccessfully fought U.S. antitrust investigators.

Google's leaders have been preparing for an uncertain future in which the search engine might be made a public utility, fined into bankruptcy, frozen by court orders, or even seized by governments. It might be able to avoid ugly scenarios simply by posting the specs for its new public API and inviting people and companies around the world to compete with its search platform. Google could do this tomorrow—and generate glowing headlines worldwide. Google's data analysts know how to run numbers better than anyone. If the models predict that the company will make more money, minimize risk, and optimize its brand in coming years by making its index public, Google will make this happen long before the roof caves in.

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APPENDIX IV

The search engine manipulation effect (SEME) and its possible impact on the outcomes of elections

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Edited by Jacob N. Shapiro, Princeton University, Princeton, NJ, and accepted by the Editorial Board July 8, 2015 (received for review October 16, 2014)

Internet search rankings have a significant impact on consumer choices, mainly because users trust and choose higher-ranked results more than lower-ranked results. Given the apparent power of search rankings, we asked whether they could be manipulated to alter the preferences of undecided voters in democratic elections. Here we report the results of five relevant double-blind, randomized controlled experiments, using a total of 4,556 undecided voters representing diverse demographic characteristics of the voting populations of the United States and India. The fifth experiment is especially notable in that it was conducted with eligible voters throughout India in the midst of India's 2014 Lok Sabha elections just before the final votes were cast. The results of these experiments demonstrate that (*i*) biased search rankings can shift the voting preferences of undecided voters by 20% or more, (*ii*) the shift can be much higher in some demographic groups, and (*iii*) search ranking bias can be masked so that people show no awareness of the manipulation. We call this type of influence, which might be applicable to a variety of attitudes and beliefs, the search engine manipulation effect. Given that many elections are won by small margins, our results suggest that a search engine company has the power to influence the results of a substantial number of elections with impunity. The impact of such manipulations would be especially large in countries dominated by a single search engine company.

search engine manipulation effect | search rankings | Internet influence | voter manipulation | digital bandwagon effect

Recent research has demonstrated that the rankings of search results provided by search engine companies have a dramatic impact on consumer attitudes, preferences, and behavior (1–12); this is presumably why North American companies now spend more than 20 billion US dollars annually on efforts to place results at the top of rankings (13, 14). Studies using eye-tracking technology have shown that people generally scan search engine results in the order in which the results appear and then fixate on the results that rank highest, even when lower-ranked results are more relevant to their search (1–5). Higher-ranked links also draw more clicks, and consequently people spend more time on Web pages associated with higher-ranked search results (1–9). A recent analysis of ∼300 million clicks on one search engine found that 91.5% of those clicks were on the first page of search results, with 32.5% on the first result and 17.6% on the second (7). The study also reported that the bottom item on the first page of results drew 140% more clicks than the first item on the second page (7). These phenomena occur apparently because people trust search engine companies to assign higher ranks to the results best suited to their needs (1–4, 11), even though users generally have no idea how results get ranked (15).

Why do search rankings elicit such consistent browsing behavior? Part of the answer lies in the basic design of a search engine results page: the list. For more than a century, research has shown that an item's position on a list has a powerful and persuasive impact on subjects' recollection and evaluation of that item (16–18). Specific order effects, such as primacy and recency, show that the first and last items presented on a list, respectively, are more likely to be recalled than items in the middle (16, 17).

Primacy effects in particular have been shown to have a favorable influence on the formation of attitudes and beliefs (18–20), enhance perceptions of corporate performance (21), improve ratings of items on a survey (22–24), and increase purchasing behavior (25). More troubling, however, is the finding that primacy effects have a significant impact on voting behavior, resulting in more votes for the candidate whose name is listed first on a ballot (26–32). In one recent experimental study, primacy accounted for a 15% gain in votes for the candidate listed first (30). Although primacy effects have been shown to extend to hyperlink clicking behavior in online environments (33–35), no study that we are aware of has yet examined whether the deliberate manipulation of search engine rankings can be leveraged as a form of persuasive technology in elections. Given the power of order effects and the impact that search rankings have on consumer attitudes and behavior, we asked whether the deliberate manipulation of search rankings pertinent to candidates in political elections could alter the attitudes, beliefs, and behavior of undecided voters.

It is already well established that biased media sources such as newspapers (36–38), political polls (39), and television (40) sway voters (41, 42). A 2007 study by DellaVigna and Kaplan found, for example, that whenever the conservative-leaning Fox television network moved into a new market in the United States, conservative votes increased, a phenomenon they labeled the Fox News Effect (40). These researchers estimated that biased coverage by Fox News was sufficient to shift 10,757 votes in Florida during the 2000 US Presidential election: more than enough to flip the deciding state in the election, which was carried by the Republican presidential candidate by only 537 votes. The Fox News Effect was also found to be smaller in television markets that were more competitive.

We believe, however, that the impact of biased search rankings on voter preferences is potentially much greater than the influence of traditional media sources (43), where parties compete in

Significance

We present evidence from five experiments in two countries suggesting the power and robustness of the search engine manipulation effect (SEME). Specifically, we show that (*i*) biased search rankings can shift the voting preferences of undecided voters by 20% or more, (*ii*) the shift can be much higher in some demographic groups, and (*iii*) such rankings can be masked so that people show no awareness of the manipulation. Knowing the proportion of undecided voters in a population who have Internet access, along with the proportion of those voters who can be influenced using SEME, allows one to calculate the win margin below which SEME might be able to determine an election outcome.

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an open marketplace for voter allegiance. Search rankings are controlled in most countries today by a single company. If, with or without intervention by company employees, the algorithm that ranked election-related information favored one candidate over another, competing candidates would have no way of compensating for the bias. It would be as if Fox News were the only television network in the country. Biased search rankings would, in effect, be an entirely new type of social influence, and it would be occurring on an unprecedented scale. Massive experiments conducted recently by social media giant Facebook have already introduced other unprecedented types of influence made possible by the Internet. Notably, an experiment reported recently suggested that flashing "VOTE" ads to 61 million Facebook users caused more than 340,000 people to vote that day who otherwise would not have done so (44). Zittrain has pointed out that if Facebook executives chose to prompt only those people who favored a particular candidate or party, they could easily flip an election in favor of that candidate, performing a kind of "digital gerrymandering" (45).

We evaluated the potential impact of biased search rankings on voter preferences in a series of experiments with the same general design. Subjects were asked for their opinions and voting preferences both before and after they were allowed to conduct research on candidates using a mock search engine we had created for this purpose. Subjects were randomly assigned to groups in which the search results they were shown were biased in favor of one candidate or another, or, in a control condition, in favor of neither candidate. Would biased search results change the opinions and voting preferences of undecided voters, and, if so, by how much? Would some demographic groups be more vulnerable to such a manipulation? Would people be aware that they were viewing biased rankings? Finally, what impact would familiarity with the candidates have on the manipulation?

Study 1: Three Experiments in San Diego, CA

To determine the potential for voter manipulation using biased search rankings, we initially conducted three laboratory-based experiments in the United States, each using a double-blind control group design with random assignment. For each of the experiments, we recruited 102 eligible voters through newspaper and online advertisements, as well through notices in senior recreation centers, in the San Diego, CA, area.* The advertisements offered USD$25 for each subject's participation, and subjects were prescreened in an attempt to match diverse demographic characteristics of the US voting population (46).

Each of the three experiments used 30 actual search results and corresponding Web pages relating to the 2010 election to determine the prime minister of Australia. The candidates were Tony Abbott and Julia Gillard, and the order in which their names were presented was counterbalanced in all conditions. This election was used to minimize possible preexisting biases by US study participants and thus to try to guarantee that our subjects would be truly "undecided." In each experiment, subjects were randomly assigned to one of three groups: (*i*) rankings favoring Gillard (which means that higher-ranked search results linked to Web pages that portrayed Gillard as the better candidate), (*ii*) rankings favoring Abbott, or (*iii*) rankings favoring neither (Fig. 1 *A*–*C*). The order of these rankings was determined based on ratings of Web pages provided by three independent observers. Neither the subjects nor the research assistants who supervised them knew either the hypothesis of the experiment or the groups to which subjects were assigned.

Initially, subjects read brief biographies of the candidates and rated them on 10-point Likert scales with respect to their overall impression of each candidate, how much they trusted each candidate, and how much they liked each candidate. They were also asked how likely they would be to vote for one candidate or the other on an 11-point scale ranging from −5 to +5, as well as to indicate which of the two candidates they would vote for if the election were held that day.

The subjects then spent up to 15 min gathering more information about the candidates using a mock search engine we had created (called Kadoodle), which gave subjects access to five pages of search results with six results per page. As is usual with search engines, subjects could click on any search result to view the corresponding Web page, or they could click on numbers at the bottom of each results page to view other results pages. The same search results and Web pages were used for all subjects in each experiment; only the order of the search results was varied (Fig. 1). Subjects had the option to end the search whenever they felt they had acquired sufficient information to make a sound decision. At the conclusion of the search, subjects rated the candidates again. When their ratings were complete, subjects were asked (on their computer screens) whether anything about the search rankings they had viewed "bothered" them; they were then given an opportunity to write at length about what, if anything, had bothered them. We did not ask specifically whether the search rankings appeared to be "biased" to avoid false positives typically generated by leading or suggestive questions (47).

Regarding the ethics of our study, our manipulation could have no impact on a past election, and we were also not concerned that it could affect the outcome of future elections, because the number of subjects we recruited was small and, to our knowledge, included no Australian voters. Moreover, our study was designed so that it did not favor any one candidate, so there was no overall bias. The study presented no more than minimal risk to subjects and was approved by the Institutional Review Board (IRB) of the American Institute for Behavioral Research and Technology (AIBRT). Informed consent was obtained from all subjects.

In aggregate for the first three experiments in San Diego, CA, the demographic characteristics of our subjects (mean age, 42.5 y; SD = 18.1 y; range, 18–95 y) did not differ from characteristics of the US voting population by more than the following

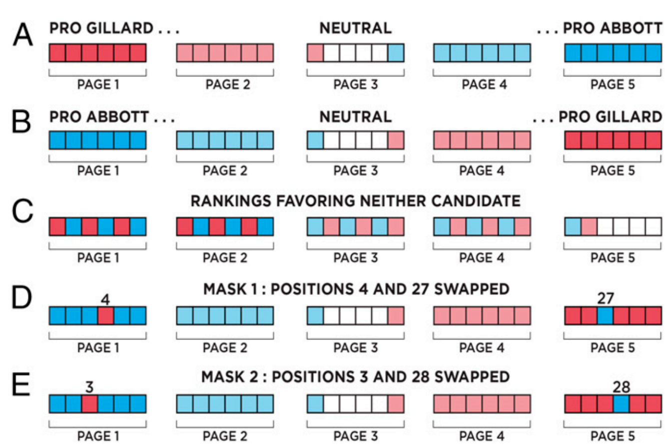

Fig. 1. Search rankings for the three experiments in study 1. (*A*) For subjects in group 1 of experiment 1, 30 search results that linked to 30 corresponding Web pages were ranked in a fixed order that favored candidate Julia Gillard, as follows: those favoring Gillard (from highest to lowest rated pages), then those favoring neither candidate, then those favoring Abbott (from lowest to highest rated pages). (*B*) For subjects in group 2 of experiment 1, the search results were displayed in precisely the opposite order so that they favored the opposing candidate, Tony Abbott. (*C*) For subjects in group 3 of experiment 1 (the control group), the ranking favored neither candidate. (*D*) For subjects in groups 1 and 2 of experiment 2, the rankings bias was masked slightly by swapping results that had originally appeared in positions 4 and 27. Thus, on the first page of search results, five of the six results—all but the one in the fourth position—favored one candidate. (*E*) For subjects in groups 1 and 2 of experiment 3, a more aggressive mask was used by swapping results that had originally appeared in positions 3 and 28.

*Although all participants claimed to be eligible voters in the prescreening, we later discovered that 6.9% of subjects marked "I don't know" and 5.2% of subjects marked "No" in response to a question asking "If you are not currently registered, are you eligible to register for elections?"

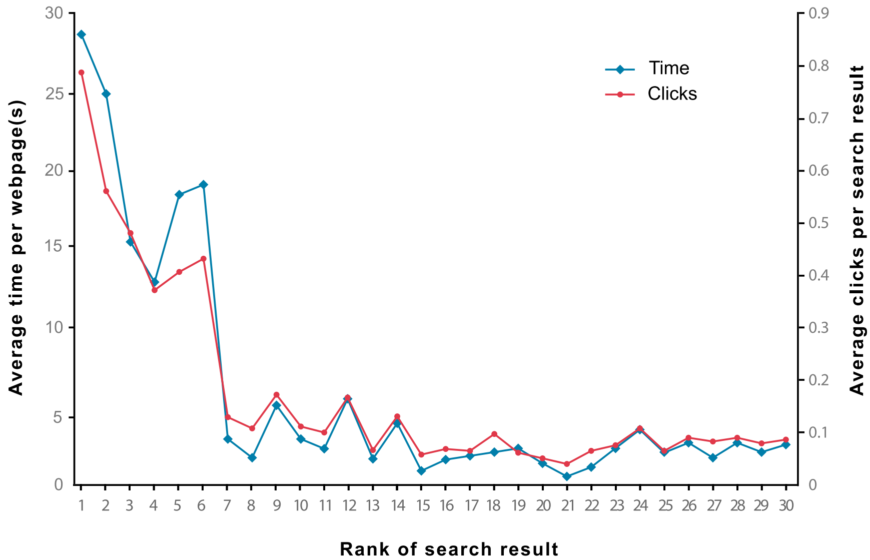

Fig. 2. Clicks on search results and time allocated to Web pages as a function of search result rank, aggregated across the three experiments in study 1. Subjects spent less time on Web pages corresponding to lower-ranked search results (blue curve) and were less likely to click on lower-ranked results (red curve). This pattern is found routinely in studies of Internet search engine use (1–12).

margins: 6.4% within any category of the age or sex measures; 14.1% within any category of the race measure; 18.7% within any category of the income or education measures; and 21.1% within any category of the employment status measure (Table S1). Subjects' political inclinations were fairly balanced, with 20.3% identifying themselves as conservative, 28.8% as moderate, 22.5% as liberal, and 28.4% as indifferent. Political party affiliation, however, was less balanced, with 21.6% identifying as Republican, 19.6% as Independent, 44.8% as Democrat, 6.2% as Libertarian, and 7.8% as other. In aggregate, subjects reported conducting an average of 7.9 searches (SD = 17.5) per day using search engines, and 52.3% reported having conducted searches to learn about political candidates. They also reported having little or no familiarity with the candidates (mean familiarity on a scale of 1–10, 1.4; SD = 0.99). On average, subjects in the first three experiments spent 635.9 s (SD = 307.0) using our mock search engine.

As expected, higher search rankings drew more clicks, and the pattern of clicks for the first three experiments correlated strongly with the pattern found in a recent analysis of ~300 million clicks [$r(13) = 0.90$, $P < 0.001$; Kolmogorov–Smirnov test of differences in distributions: $D = 0.033$, $P = 0.31$; Fig. 2] (7). In addition, subjects spent more time on Web pages associated with higher-ranked results (Fig. 2), as well as substantially more time on earlier search pages (Fig. 3).

In experiment 1, we found no significant differences among the three groups with respect to subjects' ratings of the candidates before Web research (Table S2). Following the Web research, all candidate ratings in the bias groups shifted in the predicted directions compared with candidate ratings in the control group (Table 1).

Before Web research, we found no significant differences among the three groups with respect to the proportions of people who said that they would vote for one candidate or the other if the election were held today (Table 2). Following Web research, significant differences emerged among the three groups for this measure (Table 2), and the number of subjects who said they would vote for the favored candidate in the two bias groups combined increased by 48.4% (95% CI, 30.8–66.0%; McNemar's test, $P < 0.01$).

We define the latter percentage as vote manipulation power (VMP). Thus, before the Web search, if a total of x subjects in the bias groups said they would vote for the target candidate, and if, following the Web search, a total of x' subjects in the bias groups said they would vote for the target candidate, VMP = $(x' - x)/x$. The VMP is, we believe, the key measure that an administrator would want to know if he or she were trying to manipulate an election using SEME.

Using a more sensitive measure than forced binary choice, we also asked subjects to estimate the likelihood, on an 11-point scale from −5 to +5, that they would vote for one candidate or the other if the election were held today. Before Web research, we found no significant differences among the three groups with respect to the likelihood of voting for one candidate or the other [Kruskal–Wallis (K–W) test: $\chi^2(2) = 1.384$, $P = 0.501$]. Following Web research, the likelihood of voting for either candidate in the bias groups diverged from their initial scale values by 3.71 points in the predicted directions [Mann–Whitney (M–W) test: u = 300.5, $P < 0.01$]. Notably, 75% of subjects in the bias groups showed no awareness of the manipulation. We counted subjects as showing awareness of the manipulation if (i) they had clicked on the box indicating that something bothered them about the rankings and (ii) we found specific terms or phrases in their open-ended comments suggesting that they were aware of bias in the rankings (*SI Text*).

In experiment 2, we sought to determine whether the proportion of subjects who were unaware of the manipulation could be increased with voter preferences still shifting in the predicted directions. We accomplished this by masking our manipulation to some extent. Specifically, the search result that had appeared in the fourth position on the first page of the search results favoring Abbott in experiment 1 was swapped with the corresponding search result favoring Gillard (Fig. 1*D*). Before Web research, we found no significant differences among the three

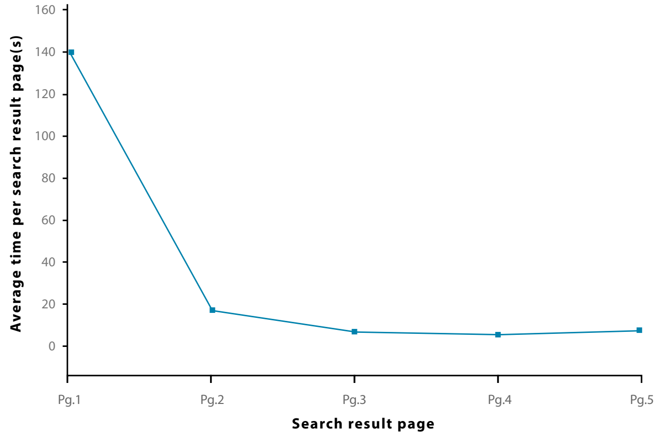

Fig. 3. Amount of time, aggregated across the three experiments in study 1, that subjects spent on each of the five search pages. Subjects spent most of their time on the first search page, a common finding in Internet search engine research (1–12).

Table 1. Postsearch shifts in voting preferences for study 1

			Mean deviation from control (SE)			
Experiment	Candidate	Rating	Gillard bias	u	Abbott bias	u
1	Gillard	Impression	1.44 (0.56)*	761.0	−1.52 (0.56)**	380.5
		Trust	1.26 (0.53)**	779.0	−1.85 (0.48)**	330.5
		Like	0.26 (0.54)	615.5	−1.73 (0.65)**	387.0
	Abbott	Impression	−2.29 (0.73)**	373.0	1.11 (0.72)**	766.5
		Trust	−2.02 (0.63)**	384.0	0.67 (0.76)	679.0
		Like	−1.55 (0.71)	460.5	1.17 (0.64)*	733.0
2	Gillard	Impression	0.97 (0.65)	704.0	−2.38 (0.79)***	325.0
		Trust	0.94 (0.72)	691.5	−2.17 (0.74)**	332.5
		Like	0.55 (0.76)	639.5	−1.82 (0.66)**	378.0
	Abbott	Impression	−1.44 (0.81)*	395.5	1.17 (0.75)*	742.0
		Trust	−0.79 (0.81)	453.5	1.85 (0.72)**	774.5
		Like	−1.44 (0.70)*	429.0	0.64 (0.71)	690.0
3	Gillard	Impression	1.44 (0.73)*	717.5	−0.55 (0.69)	507.5
		Trust	0.47 (0.70)	620.0	−0.23 (0.56)	466.5
		Like	0.44 (0.65)	623.5	−0.41 (0.70)	528.5
	Abbott	Impression	−0.32 (0.70)	534.0	1.26 (0.60)*	750.5
		Trust	−0.73 (0.65)	498.5	1.50 (0.58)**	795.0
		Like	−0.50 (0.61)	496.0	0.88 (0.62)	681.5

*$P < 0.05$, **$P < 0.01$, and ***$P < 0.001$: Mann–Whitney u tests were conducted between the control group and each of the bias groups.

groups with respect to subjects' ratings of the candidates (Table S2). Following the Web research, all candidate ratings in the bias groups shifted in the predicted directions compared with candidate ratings in the control group (Table 1).

Before Web research, we found no significant differences among the three groups with respect to voting proportions (Table 2). Following Web research, significant differences emerged among the three groups for this measure (Table 2), and the VMP was 63.3% (95% CI, 46.1–80.6%; McNemar's test, $P < 0.001$).

For the more sensitive measure (the 11-point scale), we found no significant differences among the three groups with respect to the likelihood of voting for one candidate or the other before Web research [K-W test: $\chi^2(2) = 0.888$, $P = 0.642$]. Following Web research, the likelihood of voting for either candidate in the bias groups diverged from their initial scale values by 4.44 points in the predicted directions (M-W test: $u = 237.5$, $P < 0.001$). In addition, the proportion of people who showed no awareness of the manipulation increased from 75% in experiment 1 to 85% in experiment 2, although the difference between these percentages was not significant ($\chi^2 = 2.264$, $P = 0.07$).

In experiment 3, we sought to further increase the proportion of subjects who were unaware of the manipulation by using a more aggressive mask. Specifically, the search result that had appeared in the third position on the first page of the search results favoring Abbott in experiment 1 was swapped with the corresponding search result favoring Gillard (Fig. 1*E*). This mask is a more aggressive one because higher ranked results are viewed more and taken more seriously by people conducting searches (1–12).

Before Web research, we found no significant differences among the three groups with respect to subjects' ratings of candidates (Table S2). Following the Web research, all candidate ratings in the bias groups shifted in the predicted directions compared with candidate ratings in the control group (Table 1).

Before Web research, we found no significant differences among the three groups with respect to voting proportions (Table 2). Following Web research, significant differences did not emerge among

Table 2. Comparison of voting proportions before and after Web research by group for studies 1 and 2

			Simulated vote before Web research			Simulated vote after Web research			
Study	Experiment	Group	Gillard	Abbott	χ^2	Gillard	Abbott	χ^2	VMP
1	1	1	8	26	5.409	22	12	8.870*	48.4%**
		2	11	23		10	24		
		3	17	17		14	20		
	2	1	16	18	2.197	27	7	14.274***	63.3%***
		2	20	14		12	22		
		3	14	20		22	12		
	3	1	17	17	2.199	22	12	3.845	36.7%*
		2	21	13		15	19		
		3	15	19		15	19		
2	4	1	317	383	1.047	489	211	196.280***	37.1%***
		2	316	384		228	472		
		3	333	367		377	323		

McNemar's test was conducted to assess VMP significance. VMP, percent increase in subjects in the bias groups combined who said that they would vote for the favored candidate.

*$P < 0.05$; **$P < 0.01$; and ***$P < 0.001$: Pearson χ^2 tests were conducted among all three groups.

the three groups for this measure (Table 2); the VMP, however, was 36.7% (95% CI, 19.4–53.9%; McNemar's test, $P < 0.05$).

For the more sensitive measure (the 11-point scale), we found no significant differences among the three groups with respect to the likelihood of voting for one candidate or the other before Web research [K-W test: $\chi^2(2) = 0.624$, $P = 0.732$]. Following Web research, the likelihood of voting for either candidate in the bias groups diverged from their initial scale values by 2.62 points in the predicted directions (M-W test: $u = 297.0$, $P < 0.001$). Notably, in experiment 3, no subjects showed awareness of the rankings bias, and the difference between the proportions of subjects who appeared to be unaware of the manipulations in experiments 1 and 3 was significant ($\chi^2 = 19.429$, $P < 0.001$).

Although the findings from these first three experiments were robust, the use of small samples from one US city limited their generalizability and might even have exaggerated the effect size (48).

Study 2: Large-Scale National Online Replication of Experiment 3

To better assess the generalizability of SEME to the US population at large, we used a diverse national sample of 2,100 individuals[†] from all 50 US states (Table S1), recruited using Amazon's Mechanical Turk (mturk.com), an online subject pool that is now commonly used by behavioral researchers (49, 50). Subjects (mean age, 33.9 y; SD = 11.9 y; range, 18–81 y) were exposed to the same aggressive masking procedure we used in experiment 3 (Fig. 1*E*). Each subject was paid USD$1 for his or her participation.

Regarding ethical concerns, as in study 1, our manipulation could have no impact on a past election, and we were not concerned that it could affect the outcome of future elections. Moreover, our study was designed so that it did not favor any one candidate, so there was no overall bias. The study presented no more than minimal risk to subjects and was approved by AIBRT's IRB. Informed consent was obtained from all subjects.

Subjects' political inclinations were less balanced than those in study 1, with 19.5% of subjects identifying themselves as conservative, 24.2% as moderate, 50.2% as liberal, and 6.3% as indifferent; 16.1% of subjects identified themselves as Republican, 29.9% as Independent, 43.2% as Democrat, 8.0% as Libertarian, and 2.9% as other. Subjects reported having little or no familiarity with the candidates (mean, 1.9; SD = 1.7). As one might expect in a study using only Internet-based subjects, self-reported search engine use was higher in study 2 than in study 1 [mean searches per day, 15.3; SD = 26.3; $t(529.5)^{‡} = 6.9$, $P < 0.001$], and more subjects reported having previously used a search engine to learn about political candidates (86.0%, $\chi^2 = 204.1$, $P < 0.001$). Subjects in study 2 also spent less time using our mock search engine [mean total time, 309.2 s; SD = 278.7; $t(381.9)^{‡} = -17.6$, $P < 0.001$], but patterns of search result clicks and time spent on Web pages were similar to those we found in study 1 [clicks: $r(28) = 0.98$, $P < 0.001$; Web page time: $r(28) = 0.98$, $P < 0.001$] and to those routinely found in other studies (1–12).

Before Web research, we found no significant differences among the three groups with respect to subjects' ratings of the candidates (Table S3). Following the Web research, all candidate ratings in the bias groups shifted in the predicted directions compared with candidate ratings in the control group (Table 3).

Before Web research, we found no significant differences among the three groups with respect to voting proportions (Table 2). Following Web research, significant differences emerged among the three groups for this measure (Table 2), and the VMP was 37.1% (95% CI, 33.5–40.7%; McNemar's test, $P < 0.001$). Using post-stratification and weights obtained from the 2010 US Census (46) and a 2011 study from Gallup (51), which were scaled to size for age, sex, race, and education, the VMP was 36.7% (95% CI, 33.2–40.3%; McNemar's test, $P < 0.001$). When weighted using the same demographics via classical regression poststratification (52) (Table S4), the VMP was 33.5% (95% CI, 30.1–37.0%, McNemar's test, $P < 0.001$).

For the more sensitive measure (the 11-point scale), we found no significant differences among the three groups with respect to the likelihood of voting for one candidate or the other before Web research [K-W test: $\chi^2(2) = 2.790$, $P = 0.248$]. Following Web research, the likelihood of voting for either candidate in the bias groups diverged from their initial scale values by 3.03 points in the predicted directions (M-W test: $u = 1.29 \times 10^5$, $P < 0.001$). As one might expect of a more Internet-fluent sample, the proportion of subjects showing no awareness of the manipulation dropped to 91.4%.

The number of subjects in study 1 was too small to look at demographic differences. In study 2, we found substantial differences in how vulnerable different demographic groups were to SEME. Consistent with previous findings on the moderators of order effects (30–32), for example, we found that subjects reporting a low familiarity with the candidates (familiarity less than 5 on a scale from 1 to 10) were more vulnerable to SEME (VMP = 38.7%; 95% CI, 34.9–42.4%; McNemar's test, $P < 0.001$) than were subjects who reported high familiarity with the candidates (VMP = 19.3%; 95% CI, 9.1–29.5%; McNemar's test, $P < 0.05$), and this difference was significant ($\chi^2 = 8.417$, $P < 0.01$).

We found substantial differences in vulnerability to SEME among a number of different demographic groups (*SI Text*). Although the groups we examined were overlapping and somewhat arbitrary, if one were manipulating an election, information about such differences would have enormous practical value. For example, we found that self-labeled Republicans were more vulnerable to SEME (VMP = 54.4%; 95% CI, 45.2–63.5%; McNemar's test, $P < 0.001$) than were self-labeled Democrats (VMP = 37.7%; 95% CI, 32.3–43.1%; McNemar's test, $P < 0.001$) and that self-labeled divorcees were more vulnerable (VMP = 46.7%; 95% CI, 32.1–61.2%; McNemar's test, $P < 0.001$) than were self-labeled married subjects (VMP = 32.4%; 95% CI, 26.8–38.1%; McNemar's test, $P < 0.001$). Among the most vulnerable groups we identified were Moderate Republicans (VMP = 80.0%; 95% CI, 62.5–97.5%; McNemar's test, $P < 0.001$), whereas among the least vulnerable groups were people who reported a household income of $40,000 to $49,999 (VMP = 22.5%; 95% CI, 13.8–31.1%; McNemar's test, $P < 0.001$).

Notably, awareness of the manipulation not only did not nullify the effect, it seemed to enhance it, perhaps because people trust search order so much that awareness of the bias serves to confirm the superiority of the favored candidate. The VMP for people who showed no awareness of the biased search rankings ($n = 1{,}280$) was 36.3% (95% CI, 32.6–40.1%; McNemar's test, $P < 0.001$), whereas the VMP for people who showed awareness of the bias ($n = 120$) was 45.0% (95% CI, 32.4–57.6%; McNemar's test, $P < 0.001$).

Having now replicated the effect with a large and diverse sample of US subjects, we were concerned about the weaknesses associated with testing subjects on a somewhat abstract election (the election in Australia) that had taken place years before and in which subjects were unfamiliar with the candidates. In real elections, people are familiar with the candidates and are influenced, sometimes on a daily basis, by aggressive campaigning. Presumably, either of these two factors—familiarity and outside influence—could potentially minimize or negate the influence of biased search rankings on voter preferences. We therefore asked if SEME could be replicated with a large and diverse sample of real voters in the midst of a real election campaign.

Study 3: SEME Evaluated During the 2014 Lok Sabha Elections in India

In our fifth experiment, we sought to manipulate the voting preferences of undecided eligible voters in India during the 2014 national Lok Sabha elections there. This election was the largest democratic election in history, with more than 800 million eligible voters and more than 430 million votes ultimately cast. We accomplished this by randomly assigning undecided English-speaking

[†]As in study 1, although all participants claimed to be eligible voters in the prescreening, we later discovered that 4.7% of subjects marked "I don't know" and 2.6% of subjects marked "No" in response to a question asking "If you are not currently registered, are you eligible to register for elections?"

[‡]Degrees of freedom adjusted for significant inequality of variances (Welch's *t* test).

Table 3. Postsearch shifts in voting preferences for study 2

		Mean deviation from control (SE)			
Candidate	Rating	Gillard bias	u	Abbott bias	u
Gillard	Impression	0.65 (0.10)***	288,299.5	−1.25 (0.12)***	168,203.5
	Trust	0.61 (0.10)***	283,491.0	−1.21 (0.11)***	167,658.5
	Like	0.50 (0.10)***	279,967.0	−1.25 (0.11)***	166,544.0
Abbott	Impression	−0.96 (0.13)***	189,290.5	1.35 (0.12)***	326,067.0
	Trust	−1.09 (0.14)***	183,993.0	1.31 (0.12)***	318,740.5
	Like	−0.85 (0.13)***	195,088.5	0.94 (0.11)***	302,318.0

***P < 0.001: Mann–Whitney u tests were conducted between the control group and each of the bias groups.

voters throughout India who had not yet voted (recruited through print advertisements, online advertisements, and online subject pools) to one of three groups in which search rankings favored either Rahul Gandhi, Arvind Kejriwal, or Narendra Modi, the three major candidates in the election.[§]

Subjects were incentivized to participate in the study either with payments between USD$1 and USD$4 or with the promise that a donation of approximately USD$1.50 would be made to a prominent Indian charity that provides free lunches for Indian children. (At the close of the study, a donation of USD$1,457 was made to the Akshaya Patra Foundation.)

Regarding ethical concerns, because we recruited only a small number of subjects relative to the size of the Indian voting population, we were not concerned that our manipulation could affect the election's outcome. Moreover, our study was designed so that it did not favor any one candidate, so there was no overall bias. The study presented no more than minimal risk to subjects and was approved by AIBRT's IRB. Informed consent was obtained from all subjects.

The subjects (n = 2,150) were demographically diverse (Table S5), residing in 27 of 35 Indian states and union territories, and political leanings varied as follows: 13.3% identified themselves as politically right (conservative), 43.8% as center (moderate), 26.0% as left (liberal), and 16.9% as indifferent. In contrast to studies 1 and 2, subjects reported high familiarity with the political candidates (mean familiarity Gandhi, 7.9; SD = 2.5; mean familiarity Kejriwal, 7.7; SD = 2.5; mean familiarity Modi, 8.5; SD = 2.1). The full dataset for all five experiments is accessible at Dataset S1.

Subjects reported more frequent search engine use compared with subjects in studies 1 or 2 (mean searches per day, 15.7; SD = 30.1), and 71.7% of subjects reported that they had previously used a search engine to learn about political candidates. Subjects also spent less time using our mock search engine (mean total time, 277.4 s; SD = 368.3) than did subjects in studies 1 or 2. The patterns of search result clicks and time spent on Web pages in our mock search engine was similar to the patterns we found in study 1 [clicks, $r(28) = 0.96$; $P < 0.001$; Web page time, $r(28) = 0.91$; $P < 0.001$] and study 2 [clicks, $r(28) = 0.96$; $P < 0.001$; Web page time, $r(28) = 0.92$; $P < 0.001$].

Before Web research, we found one significant difference among the three groups for a rating pertaining to Kejriwal, but none for Gandhi or Modi (Table S6). Following the Web research, most of the subjects' ratings of the candidates shifted in the predicted directions (Table 4).

Before Web research, we found no significant differences among the three groups with respect to voting proportions (Table 5). Following Web research, significant differences emerged among the three groups for this measure (Table 5), and the VMP was 10.6% (95% CI, 8.3–12.8%; McNemar's test, $P < 0.001$). Using poststratification and weights obtained from the 2011 India Census data on literate Indians (53)—scaled to size for age, sex, and location (grouped into state or union territory)—the VMP was 9.4% (95% CI, 8.2–10.6%; McNemar's test, $P < 0.001$). When weighted using the same demographics via classical regression poststratification (Table S7), the VMP was 9.5% (95% CI, 8.3–10.7%; McNemar's test, $P < 0.001$).

To obtain a more sensitive measure of voting preference in study 3, we asked subjects to estimate the likelihood, on three separate 11-point scales from −5 to +5, that they would vote for each of the candidates if the election were held today. Before Web research, we found no significant differences among the three groups with respect to the likelihood of voting for any of the candidates (Table S6). Following Web research, significant differences emerged among the three groups with respect to the likelihood of voting for Rahul Gandhi and Arvind Kejriwal but not Narendra Modi (Table S6), and all likelihoods shifted in the predicted directions (Table 4). The proportion of subjects showing no awareness of the manipulation in experiment 5 was 99.5%.

In study 3, as in study 2, we found substantial differences in how vulnerable different demographic groups were to SEME (*SI Text*). Consistent with the findings of study 2 and previous findings on the moderators of order effects (30–32), for example, we found that subjects reporting a low familiarity with the candidates (familiarity less than 5 on a scale from 1 to 10) were more vulnerable to SEME (VMP = 13.7%; 95% CI, 4.3–23.2%; McNemar's test, $P = 0.17$) than were subjects who reported high familiarity with the candidates (VMP = 10.3%; 95% CI, 8.0–12.6%; McNemar's test, $P < 0.001$), although this difference was not significant ($\chi^2 = 0.575$, $P = 0.45$).

As in study 2, although the demographic groups we examined were overlapping and somewhat arbitrary, if one was manipulating an election, information about such differences would have enormous practical value. For example, we found that subjects between ages 18 and 24 were less vulnerable to SEME (VMP = 8.9%; 95% CI, 5.0–12.8%; McNemar's test, $P < 0.05$) than were subjects between ages 45 and 64 (VMP = 18.9%; 95% CI, 6.3–31.5%; McNemar's test, $P = 0.10$) and that self-labeled Christians were more vulnerable (VMP = 30.7%; 95% CI, 20.2–41.1%; McNemar's test, $P < 0.001$) than self-labeled Hindus (VMP = 8.7%; 95% CI, 6.3–11.1%; McNemar's test, $P < 0.001$). Among the most vulnerable groups we identified were unemployed males from Kerala (VMP = 72.7%; 95% CI, 46.4–99.0%; McNemar's test, $P < 0.05$), whereas among the least vulnerable groups were female conservatives (VMP = −11.8%; 95% CI, −29.0%–5.5%; McNemar's test, $P = 0.62$).

A negative VMP might suggest oppositional attitudes or an underdog effect for that group (54). No negative VMPs were found in the demographic groups examined in study 2, but it is understandable that they would be found in an election in which people are highly familiar with the candidates (study 3). As a practical matter, where a search engine company has the ability to send people customized rankings and where biased search rankings are likely to produce an oppositional response with certain voters, such rankings would probably not be sent to them. Eliminating the 2.6% of our sample (n = 56) with oppositional responses, the overall VMP in this experiment increases from 10.6% to 19.8% (95% CI, 16.8–22.8%; n = 2,094; McNemar's test: $P < 0.001$).

As we found in study 2, awareness of the manipulation appeared to enhance the effect rather than nullify it. The VMP for people

[§]English is one of India's two official languages, the other being Hindi.

Table 4. Postsearch shifts in voting preferences for study 3

Candidate	Rating	χ^2	Mean (SE) Gandhi bias	Kejriwal bias	Modi bias
Gandhi	Impression	3.61	−0.16 (0.06)	−0.21 (0.06)	−0.30 (0.06)
	Trust	21.19***	0.14 (0.06)	−0.04 (0.07)	−0.20 (0.06)
	Like	12.99**	−0.09 (0.07)	−0.17 (0.06)	−0.34 (0.06)
	Voting likelihood	10.79**	0.16 (0.07)	−0.04 (0.07)	−0.18 (0.07)
Kejriwal	Impression	17.75***	−0.30 (0.06)	−0.11 (0.06)	−0.39 (0.05)
	Trust	26.69***	−0.17 (0.07)	0.15 (0.06)	−0.16 (0.06)
	Like	24.74***	−0.31 (0.06)	0.05 (0.06)	−0.23 (0.06)
	Voting likelihood	13.22**	−0.03 (0.06)	0.17 (0.07)	−0.12 (0.06)
Modi	Impression	24.98***	−0.22 (0.06)	−0.21 (0.06)	0.12 (0.05)
	Trust	18.78***	−0.04 (0.06)	−0.10 (0.06)	0.23 (0.06)
	Like	16.89***	−0.16 (0.05)	−0.09 (0.06)	0.19 (0.06)
	Voting likelihood	31.07***	−0.07 (0.07)	−0.10 (0.06)	0.33 (0.06)

$^{**}P < 0.01$ and $^{***}P < 0.001$: for each rating, a Kruskal–Wallis χ^2 test was used to assess significance of group differences.

who showed no awareness of the biased search rankings (n = 2,140) was 10.5% (95% CI, 8.3–12.7%; McNemar's test, $P <$ 0.001), whereas the VMP for people who showed awareness of the bias (n = 10) was 33.3%.

The rankings and Web pages we used in study 3 were selected by the investigators based on our limited understanding of Indian politics and perspectives. To optimize the rankings, midway through the election process we hired a native consultant who was familiar with the issues and perspectives pertinent to undecided voters in the 2014 Lok Sabha Election. Based on the recommendations of the consultant, we made slight changes to our rankings on 30 April, 2014. In the preoptimized rankings group (n = 1,259), the VMP was 9.5% (95% CI, 6.8–12.2%; McNemar's test, $P <$ 0.001); in the postoptimized rankings group (n = 891), the VMP increased to 12.3% (95% CI, 8.5–16.1%; McNemar's test, $P <$ 0.001). Eliminating the 3.1% of the subjects in the postoptimization sample with oppositional responses (n = 28), the VMP increased to 24.5% (95% CI, 19.3–29.8%; n = 863).

Discussion

Elections are often won by small vote margins. Fifty percent of US presidential elections were won by vote margins under 7.6%, and 25% of US senatorial elections in 2012 were won by vote margins under 6.0% (55, 56). In close elections, undecided voters can make all of the difference, which is why enormous resources are often focused on those voters in the days before the election (57, 58). Because search rankings biased toward one candidate can apparently sway the voting preferences of undecided voters without their awareness and, at least under some circumstances, without any possible competition from opposing candidates, SEME appears to be an especially powerful tool for manipulating elections. The Australian election used in studies 1 and 2 was won by a margin of only 0.24% and perhaps could easily have been turned by such a manipulation. The Fox News Effect, which is small compared with SEME, is believed to have shifted between 0.4% and 0.7% of votes to conservative candidates: more than enough, according to the researchers, to have had a "decisive" effect on a number of close elections in 2000 (40).

Political scientists have identified two of the most common methods political candidates use to try to win elections. The core voter model describes a strategy in which resources are devoted to mobilizing supporters to vote (59). As noted earlier, Zittrain recently pointed out that a company such as Facebook could mobilize core voters to vote on election day by sending "get-out-and-vote" messages en masse to supporters of only one candidate. Such a manipulation could be used undetectably to flip an election in what might be considered a sort of digital gerrymandering (44, 45). In contrast, the swing voter model describes a strategy in which candidates target their resources toward persuasion—attempting to change the voting preferences of undecided voters (60). SEME is an ideal method for influencing such voters.

Although relatively few voters have actively sought political information about candidates in the past (61), the ease of obtaining information over the Internet appears to be changing that: 73% of online adults used the Internet for campaign-related purposes during the 2010 US midterm elections (61), and 55% of all registered voters went online to watch videos related to the 2012 US election campaign (62). Moreover, 84% of registered voters in the United States were Internet users in 2012 (62). In our nationwide study in the United States (study 2), 86.0% of our subjects reported having used search engines to get information about candidates. Meanwhile, the number of people worldwide with Internet access is increasing rapidly, predicted to increase to nearly 4 billion by 2018 (63). By 2018, Internet access in India is expected to rise from the 213 million users who had access in 2013 to 526 million (63). Worldwide, it is reasonable to conjecture that both proportions will increase substantially in future years; that is, more people will have Internet access, and more people will obtain information about candidates from the Internet. In the context of the experiments we have presented, this suggests that whatever the effect sizes we have observed now, they will likely be larger in the future.

Table 5. Comparison of voting proportions before and after Web research for study 3

Group	Simulated vote before Web research: Gandhi	Kejriwal	Modi	χ^2	Simulated vote after Web research: Gandhi	Kejriwal	Modi	χ^2	VMP
1	115	164	430	3.070	144	152	413	16.935**	10.6%***
2	112	183	393		113	199	376		
3	127	196	430		117	174	462		

McNemar's test was conducted to assess VMP significance. VMP, percent increase in subjects in the bias groups combined who said that they would vote for the favored candidate.

$^{**}P < 0.01$; and $^{***}P < 0.001$: Pearson χ^2 tests were conducted among all three groups.

PSYCHOLOGICAL AND COGNITIVE SCIENCES

The power of SEME to affect elections in a two-person race can be roughly estimated by making a small number of fairly conservative assumptions. Where i is the proportion of voters with Internet access, u is the proportion of those voters who are undecided, and VMP, as noted above, is the proportion of those undecided voters who can be swayed by SEME, W—the maximum win margin controllable by SEME—can be estimated by the following formula: $W = i{*}u{*}VMP$.

In a three-person race, W will vary between 75% and 100% of its value in a two-person race, depending on how the votes are distributed between the two losing candidates. (Derivations of formulas in the two-candidate and three-candidate cases are available in *SI Text*.) In both cases, the size of the population is irrelevant.

Knowing the values for i and u for a given election, along with the projected win margin, the minimum VMP needed to put one candidate ahead can be calculated (Table S8). In theory, continuous online polling would allow search rankings to be optimized continuously to increase the value of VMP until, in some instances, it could conceivably guarantee an election's outcome, much as "conversion" and "click-through" rates are now optimized continuously in Internet marketing (64).

For example, if (i) 80% of eligible voters had Internet access, (ii) 10% of those individuals were undecided at some point, and (iii) SEME could be used to increase the number of people in the undecided group who were inclined to vote for the target candidate by 25%, that would be enough to control the outcome of an election in which the expected win margin was as high as 2%. If SEME were applied strategically and repeatedly over a period of weeks or months to increase the VMP, and if, in some locales and situations, i and u were larger than in the example given, the controllable win margin would be larger. That possibility notwithstanding, because nearly 25% of national elections worldwide are typically won by margins under 3%,[¶] SEME could conceivably impact a substantial number of elections today even with fairly low values of i, u, and VMP.

Given our procedures, however, we cannot rule out the possibility that SEME produces only a transient effect, which would limit its value in election manipulation. Laboratory manipulations of preferences and attitudes often impact subjects for only a short time, sometimes just hours (65). That said, if search rankings were being manipulated with the intent of altering the outcome of a real election, people would presumably be exposed to biased rankings repeatedly over a period of weeks or months. We produced substantial changes not only in voting preferences but in multiple ratings of attitudes toward candidates given just one exposure to search rankings linking to Web pages favoring one candidate, with average search times in the 277- to 635-s range. Given hundreds or thousands of exposures of this sort, we speculate not only that the resulting attitudes and preferences would be stable, but that they would become stronger over time, much as brand preferences become stronger when advertisements are presented repeatedly (66).

Our results also suggest that it is a relatively simple matter to mask the bias in search rankings so that it is undetectable to virtually every user. In experiment 3, using only a simple mask, none of our subjects appeared to be aware that they were seeing biased rankings, and in our India study, only 0.5% of our subjects appeared to notice the bias. When people are subjected to forms of influence they can identify—in campaigns, that means speeches, billboards, television commercials, and so on—they can defend themselves fairly easily if they have opposing views. Invisible sources of influence can be harder to defend against (67–69), and for people who are impressionable, invisible sources of influence not only persuade, they also leave people feeling that they made up their own minds—that no external force was applied (70, 71). Influence is sometimes undetectable because key stimuli act subliminally (72–74), but search results and Web pages are easy to perceive; it is the pattern of rankings that people cannot see. This invisibility makes SEME especially dangerous as a means of control, not just of voting behavior but perhaps of a wide variety of attitudes, beliefs, and behavior. Ironically, and consistent with the findings of other researchers, we found that even those subjects who showed awareness of the biased rankings were still impacted by them in the predicted directions (75).

One weakness in our studies was the manner in which we chose to determine whether subjects were aware of bias in the search rankings. As noted, to not generate false-positive responses, we avoided asking leading questions that referred specifically to bias; rather, we asked a rather vague question about whether anything had bothered subjects about the search rankings, and we then gave subjects an opportunity to type out the details of their concerns. In so doing, we probably underestimated the number of detections (47), and this is a matter that should be studied further. That said, because people who showed awareness of the bias were still vulnerable to our manipulation, people who use SEME to manipulate real elections might not be concerned about detection, except, perhaps, by regulators.

Could regulators in fact detect SEME? Theoretically, by rating pages and monitoring search rankings on an ongoing basis, search ranking bias related to elections might be possible to identify and track; as a practical matter, however, we believe that biased rankings would be impossible or nearly impossible for regulators to detect. The results of studies 2 and 3 suggest that vulnerability to SEME can vary dramatically from one demographic group to another. It follows that if one were using biased search rankings to manipulate a real election, one would focus on the most vulnerable demographic groups. Indeed, if one had access to detailed online profiles of millions of individuals, which search engine companies do (76–78), one would presumably be able to identify those voters who appeared to be undecided and impressionable and focus one's efforts on those individuals only—a strategy that has long been standard in political campaigns (79–84) and continues to remain important today (85). With search engine companies becoming increasingly adept at sending users customized search rankings (76–78, 86–88), it seems likely that only customized rankings would be used to influence elections, thus making it difficult or impossible for regulators to detect a manipulation. Rankings that appear to be unbiased on the regulators' screens might be highly biased on the screens of select individuals.

Even if a statistical analysis did show that rankings consistently favored one candidate over another, those rankings could always be attributed to algorithm-guided dynamics driven by market forces—so-called "organic" forces (89)—rather than by deliberate manipulation by search engine company employees. This possibility suggests yet another potential danger of SEME. What if election-related search results are indeed being left to the vagaries of market forces? Do such forces end up pushing some candidates to the top of search rankings? If so, it seems likely that those high rankings are cultivating additional supporters for those candidates in a kind of digital bandwagon effect. In other words, for several years now and with greater impact each year (as more people get election-related information through the Internet), SEME has perhaps already been affecting the outcomes of close elections. To put this another way, without human intention or direction, algorithms have perhaps been having a say in selecting our leaders.

Because search rankings are based, at least in part, on the popularity of Web sites (90), it is likely that voter preferences impact those rankings to some extent. Given our findings that search rankings can in turn affect voter preferences, these phenomena might interact synergistically, causing a substantial increase in support for one candidate at some point even when the effects of the individual phenomena are small.[||]

Our studies produced a wide range of VMPs. In a real election, what proportion of undecided voters could actually be

¶Some of the data applied in the analysis in this publication are based on material from the "European Election Database." The data are collected from original sources and prepared and made available by the Norwegian Social Science Data Services (NSD). NSD is not responsible for the analyses/interpretation of the data presented here.

||A mathematical model we developed—highly conjectural, we admit, and at this point unverifiable—shows the possible dynamics of such synergy (Fig. S1).

shifted using SEME? Our first two studies, which relied on a campaign and candidates that were unfamiliar to our subjects, produced overall VMPs in the range 36.7–63.3%, with demographic shifts occurring with VMPs as high as 80.0%. Our third study, with real voters in the midst of a real election, produced, overall, a lower VMP: just 10.6%, with optimizing our rankings raising the VMP to 12.3% and with the elimination of a small number of oppositional subjects raising the VMP to 24.5%, which is the value we would presumably have found if our search rankings had been optimized from the start and if we had advance knowledge about oppositional groups. In the third study, VMPs in some demographic groups were as high as 72.7%. If a search engine company optimized rankings continuously and sent customized rankings only to vulnerable undecided voters, there is no telling how high the VMP could be pushed, but it would almost certainly be higher than our modest efforts could achieve. Our investigation suggests that with optimized, targeted rankings, a VMP of at least 20% should be relatively easy to achieve in real elections. Even if only 60% of a population had Internet access and only 10% of voters were undecided, that would still allow control of elections with win margins up to 1.2%—five times greater than the win margin in the 2010 race between Gillard and Abbott in Australia.

Conclusions

Given that search engine companies are currently unregulated, our results could be viewed as a cause for concern, suggesting that such companies could affect—and perhaps are already affecting—the outcomes of close elections worldwide. Restricting search ranking manipulations to voters who have been identified as undecided while also donating money to favored candidates would be an especially subtle, effective, and efficient way of wielding influence.

Although voters are subjected to a wide variety of influences during political campaigns, we believe that the manipulation of search rankings might exert a disproportionately large influence over voters for four reasons:

First, as we noted, the process by which search rankings affect voter preferences might interact synergistically with the process by which voter preferences affect search rankings, thus creating a sort of digital bandwagon effect that magnifies the potential impact of even minor search ranking manipulations.

Second, campaign influence is usually explicit, but search ranking manipulations are not. Such manipulations are difficult to detect, and most people are relatively powerless when trying to resist sources of influence they cannot see (66–68). Of greater concern in the present context, when people are unaware they are being manipulated, they tend to believe they have adopted their new thinking voluntarily (69, 70).

Third, candidates normally have equal access to voters, but this need not be the case with search engine manipulations. Because the majority of people in most democracies use a search engine provided by just one company, if that company chose to manipulate rankings to favor particular candidates or parties, opponents would have no way to counteract those manipulations. Perhaps worse still, if that company left election-related search rankings to market forces, the search algorithm itself might determine the outcomes of many close elections.

Finally, with the attention of voters shifting rapidly toward the Internet and away from traditional sources of information (12, 61, 62), the potential impact of search engine rankings on voter preferences will inevitably grow over time, as will the influence of people who have the power to control such rankings.

We conjecture, therefore, that unregulated election-related search rankings could pose a significant threat to the democratic system of government.

Materials and Methods

We used 102 subjects in each of experiments 1–3 to give us an equal number of subjects in all three groups and both counterbalancing conditions of the experiments.

Nonparametric statistical tests such as the Mann–Whitney *u* and the Kruskal–Wallis *H* are used throughout the present report because Likert scale scores, which were used in each of the studies, are ordinal.

In study 3, the procedure was identical to that of studies 1 and 2; only the Web pages and search results were different: that is, Web pages and search results were pertinent to the three leading candidates in the 2014 Lok Sabha general elections. The questions we asked subjects were also adjusted for a three-person race.

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Supporting Information

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SI Text

Demographic Differences in VMP. In study 2, we found substantial differences in how vulnerable different demographic groups were to SEME. Although the groups we examined are somewhat arbitrary, overlapping, and by no means definitive, they do establish a range of vulnerability to SEME. Ten groups ($n \geq 50$) that appeared to be highly vulnerable in study 2, as indicated by their VMP scores, were, in order from highest to lowest, as follows:

i) Moderate Republicans (80.0%; 95% CI, 62.5–97.5%)
ii) People from North Carolina (66.7%; 95% CI, 42.8–90.5%)
iii) Moderate Libertarians (73.3%; 95% CI, 51–95.7%)
iv) Male Republicans (66.1%; 95% CI, 54–78.2%)
v) Female conservatives age 30 and over (67.7%; 95% CI, 52.5–82.7%)
vi) People from Virginia (60.0%; 95% CI, 38.5–81.5%)
vii) People earning between \$15,000 and \$19,999 (60.0%; 95% CI, 42.5–77.5%)
viii) Hispanics (59.4%; 95% CI, 42.4–76.4%)
ix) Independents with no political leaning (58.3%; 95% CI, 38.6–78.1%)
x) Female conservatives (54.7%; 95% CI, 41.3–68.1%)

Ten groups that appeared to show little vulnerability to SEME, as indicated by their VMP scores, were, in order from highest to lowest, as follows:

i) People from California (24.1%; 95% CI, 15.1–33.1%)
ii) Moderate independents (24.0%; 95% CI, 15.4–32.5%)
iii) Liberal independents (23.4%; 95% CI, 13.1–33.8%)
iv) People from Texas (22.9%; 95% CI, 11–34.8%)
v) Liberal Libertarians (22.7%; 95% CI, 5.2–40.2%)
vi) People earning between \$40,000 and \$49,999 (22.5%; 95% CI, 13.8–31.1%)
vii) Female independents (22.0%; 95% CI, 13.5–30.5%)
viii) Male moderates age 30 and over (19.3%; 95% CI, 9.1–29.5%)
ix) Female independent moderates (17.9%; 95% CI, 13.5–30.5%)
x) People with an uncommon political party (15.0%; 95% CI, −0.6% to 30.6%)

In study 3, as in study 2, we found substantial differences in how vulnerable different demographic groups were to SEME. Although the groups we examined are somewhat arbitrary, overlapping, and by no means definitive, they do establish a range of vulnerability to SEME. Ten groups ($n \geq 50$) that appeared to be highly vulnerable in study 3, as indicated by their VMP scores, were, in order from highest to lowest, as follows:

i) Unemployed males from Kerala (72.7%; 95% CI, 46.4–99.1%)
ii) Unemployed Christians (68.8%; 95% CI, 46.0–91.5%)
iii) Unemployed moderate males (50.0%; 95% CI, 33.2–66.8%)
iv) Moderate Christian males (47.6%; 95% CI, 26.3–69.0%)
v) Christian moderates (42.9%; 95% CI, 26.5–59.3%)
vi) Males from Kerala (40.4%; 95% CI, 26.4–54.5%)
vii) Unemployed moderates (33.3%; 95% CI, 22.0–44.7%)
viii) Male Christians (32.7%; 95% CI, 19.9–45.4%)
ix) People from Kerala (32.4%; 95% CI, 21.8–43.1%)
x) Unemployed females with no political ideology (31.6%; 95% CI, 10.7–52.5%)

Ten groups that appeared to show little vulnerability to SEME, as indicated by their VMP scores, were, in order from highest to lowest, as follows:

i) People from Tamil Nadu with no political ideology (0.0%; 95% CI, −0.01%–0.04%)
ii) Employed females with no political ideology (0.0%; 95% CI, −0.01%–0.06%)
iii) People earning between Rs 10,000 and Rs 29,999 (−3.2%; 95% CI, −7.6%–1.3%)
iv) Married people who are separated (−3.3%; 95% CI, −10.0%–3.3%)
v) People with a pre-university education (−4.3%; 95% CI, −10.5%–1.81%)
vi) Unemployed liberals (−4.3%; 95% CI, −10.5%–1.81%)
vii) Unemployed conservatives (−5.0%; 95% CI, −15.0%–5.0%)
viii) People from Gujarat (−5.9%; 95% CI, −17.8%–6.0%)
ix) Unemployed male liberals (−8.0%; 95% CI, −19.5%–3.5%)
x) Female conservatives (−11.8%; 95% CI, −29.0%–5.5%)

Bias Awareness. Subjects were counted as showing awareness of the manipulation if (*i*) they had clicked on a box indicating that something "bothered" them about the rankings and (*ii*) we found specific terms or phrases in their open-ended comments suggesting that they were aware of bias in the rankings, such as "biased," "bias," "leaning towards," "leaning toward," "leaning against," "slanted," "skewed," "favorable towards," "favorable toward," "favorable for," "favorable against," "favorable results," "favored towards," "favored toward," "favored for," "favored against," "favored results," "favor toward," "results favor," "favor Modi," "favor Kejriwal," "favor Gandhi," "negative toward," "negative for," "negative against," "all negative," "all positive," "mainly negative," "mainly positive," "nothing positive," "nothing negative," "more results for," "less results for," "most of the articles were negative," "most of the articles were positive," "pro Modi," "pro Kejriwal," "pro Gandhi," "Modi leaning," "Kejriwal leaning," "Gandhi leaning," "pro Gillard," "pro Abbott," "favor Gillard," "favor Abbott," "Gillard leaning," and "Abbott leaning."

Derivation of the Formulas for Computing *W*, the Maximum Win Margin Controllable Through SEME, in Two- and Three- Person Races.

Two-person race. Where $T =$ total number of eligible voters in a population, $i =$ proportion of T who are internet users, $u =$ proportion of i who are undecided, $p =$ proportion of u who are prone to vote for the target candidate, and VMP = proportion of p who can be shifted by SEME.

The number of votes that can be shifted by SEME is given by

$$n = T * i * u * p * \mathrm{VMP}.$$

In a two-person race, the number of votes for the candidate favored by SEME when the vote is initially evenly split is

$$\frac{T}{2} + n,$$

and the number of votes for the losing candidate is

$$\frac{T}{2} - n.$$

The vote margin in favor of the winning candidate is therefore the larger vote minus the smaller vote, or, simply: $2n$.

Therefore, the margin of voters, expressed as a proportion, that can be shifted by SEME is

$$\frac{2n}{T}=\frac{2*T*i*u*p*\mathrm{VMP}}{T}=2*i*u*p*\mathrm{VMP}.$$

Because the undecided voters in a two-person race have only two voting options, the value of p before outside influence is exercised can reasonably be assumed to be 0.5.

Therefore, W can be calculated as follows:

$$W=2*i*u*0.5*\mathrm{VMP},$$

and the calculation can be simplified as follows:

$$W=i*u*\mathrm{VMP}.$$

In other words, the maximum win margin controllable by SEME in a two-person race is equal to the proportion of people who can be influenced by SEME (the VMP) times the proportion of undecided Internet voters in the population. ($i*u$).

Three-person race. Where $T=$ total number of voters in a population, $i=$ proportion of T who are internet users, $u=$ proportion of i who are undecided, $p=$ proportion of u who are prone to vote for the target candidate, and VMP = proportion of p who can be shifted by SEME.

The number of votes that can be shifted by SEME is given by

$$n=T*i*u*p*\mathrm{VMP}.$$

In a three-person race, because the winning candidate can draw votes from either of the two losing candidates, W can vary between two extremes:

i) At one extreme, one of the two losing candidates draws zero votes, in which case the formula for the two-person case (above) is applicable.

ii) At the other extreme, voting preferences are initially split three ways evenly, and the winning candidate draws votes equally from the other two. This distribution will give us the lowest possible value of W in the three-person race, as follows.

The number of votes for the candidate favored by SEME will still be

$$\frac{T}{2}+n.$$

However, because of the split, the number of votes for each of the losing candidates will now be

$$\frac{T}{2}-\frac{n}{2}.$$

The vote margin in favor of the winning candidate will therefore be the larger vote minus either of the smaller votes or, simply, $1.5n$.

Therefore, the margin of voters, expressed as a proportion, that can be shifted by SEME is

$$\frac{2n}{T}=\frac{1.5*T*i*u*p*\mathrm{VMP}}{T}=1.5*i*u*p*\mathrm{VMP}.$$

Therefore, W can be calculated as follows:

$$W=1.5*i*u*0.5*\mathrm{VMP},$$

and the calculation can be simplified as follows:

$$W=0.75*i*u*\mathrm{VMP}.$$

Therefore, in a three-person race, W will vary between 75% and 100% of the W found in the two-person case, depending on how votes are distributed between the two losing candidates; the more even the split, the smaller the controllable win margin.

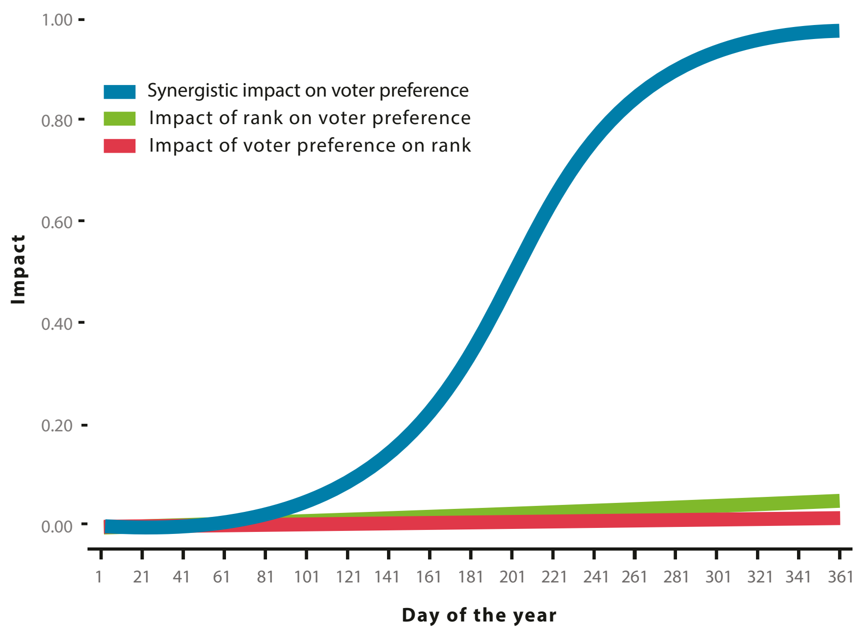

Fig. S1. A possible synergistic relationship between the impact that search rankings have on voter preferences and the impact that voter preferences have on search rankings. The lower curves (red and green) show slow increases that might occur if each of the processes acted alone over the course of a year (365 iterations of the model). The upper curve (blue) shows the result of a possible synergy between these two processes using the same parameters that generated the two lower curves. The curves are generated by an iterative model using equations of the general form $V_{n+1} = V_n + r[R_n \times (1 - V_n)] + r[O_n \times (1 - V_n)]$, where V is voter preference for one candidate, R is the impact of voter preferences on search rankings, O is the impact (randomized with each iteration) of other influences on voter preferences, and r is a rate-of-change factor. Because a change in voter preference alters the proportion of votes available, its value in the model cannot exceed 1.0.

Table S1. Demographics for studies 1 and 2

Category	Value	Census 2010†		Study 1		Census and study 1	Study 2	
		n	%	*n*	%	*Z*	*n*	%
Age	18–24	26,718	12.7%	51	16.7%	2.097*	446	21.2%
	25–44	70,472	33.4%	122	39.9%	2.385*	1,274	60.7%
	45–64	75,865	36.0%	95	31.0%	1.800	342	16.3%
	65–74	20,605	9.8%	20	6.5%	1.906	33	1.6%
	75+	17,140	8.1%	18	5.9%	1.438	5	0.2%
Race	White	152,929	72.5%	179	58.5%	5.502***	1,645	78.3%
	Black	25,632	11.8%	38	12.4%	0.349	126	6.0%
	Hispanic	21,285	9.8%	52	17.0%	4.169***	121	5.8%
	Asian	7,638	3.9%	7	2.3%	1.528	123	5.9%
	Other	3,316	2.0%	30	9.8%	10.977***	85	4.0%
Sex	Male	101,279	48.0%	162	52.9%	1.715	1,148	54.7%
	Female	109,521	52.0%	144	47.1%	1.715	947	45.1%
	Other	n/a	n/a	0	0.0%	n/a	5	0.2%
Education	Less than ninth grade	6,655	3.2%	2	0.7%	2.504*	0	0.0%
	Ninth to 12th grade	15,931	7.6%	45	14.7%	4.724***	22	1.0%
	High school graduate	65,951	31.3%	68	22.2%	3.417***	231	11.0%
	Some college or associate degree	62,655	29.7%	145	47.4%	6.753***	820	39.0%
	Bachelors	39,272	18.6%	30	9.8%	3.963***	752	35.8%
	Advanced	20,336	9.6%	16	5.2%	2.616**	275	13.1%
Used‡	Yes	126,477	60.0%	119	38.9%	7.531***	1,509	71.9%
	No	84,323	40.0%	187	61.1%	7.531***	591	28.1%
Income	Under $10,000	5,496	3.6%	67	21.9%	20.009***	137	6.5%
	$10,000 to $14,999	5,069	3.3%	33	10.8%	8.538***	131	6.2%
	$15,000 to $19,999	4,549	2.9%	28	9.2%	7.446***	124	5.9%
	$20,000 to $29,999	12,632	8.2%	45	14.7%	4.800***	282	13.4%
	$30,000 to $39,999	13,182	8.5%	34	11.1%	1.857	288	13.7%
	$40,000 to $49,999	10,807	7.0%	17	5.6%	1.143	239	11.4%
	$50,000 to $74,999	25,516	16.5%	30	9.8%	3.602***	405	19.3%
	$75,000 to $99,999	17,597	11.4%	11	3.6%	4.932***	235	11.2%
	$100,000 to $149,999	16,586	10.7%	5	1.6%	5.916***	148	7.0%
	$150,000 and over	12,102	7.8%	0	0.0%	5.893***	46	2.2%
	Prefer not to say	30,875	20.0%	36	11.8%	4.069***	65	3.1%
Marital status	Married	113,421	53.8%	48	15.7%	13.364***	751	35.8%
	Widowed	13,612	6.5%	27	8.8%	1.682	15	0.7%
	Divorced	23,035	10.9%	68	22.2%	6.324***	141	6.7%
	Separated	4,528	2.1%	15	4.9%	3.317***	33	1.6%
	Never married	56,203	26.7%	148	48.4%	8.576***	1,160	55.2%

$*P < 0.05$; $**P < 0.01$; and $***P < 0.001$.
†Census numbers are in hundred thousands.
‡For census data, "No" includes "unemployed" and "not in labor force."

Table S2. Voting preferences by group for study 1

Experiment	Voting preferences	Mean (SE) Group 1 (Gillard bias)	Group 2 (Abbott bias)	Group 3 (control)	Kruskal–Wallis (χ^2)	Mann–Whitney *u*
1	PreImpressionAbbott	8.09 (0.34)	7.74 (0.40)	7.41 (0.26)	3.979	525.0
	PreImpressionGillard	7.06 (0.42)	7.47 (0.35)	6.88 (0.32)	1.395	529.5
	PreTrustAbbott	7.82 (0.31)	7.85 (0.39)	7.35 (0.28)	3.275	538.5
	PreTrustGillard	6.38 (0.40)	7.56 (0.30)	6.88 (0.32)	5.213	407.0
	PreLikeAbbott	6.06 (0.52)	5.68 (0.47)	5.79 (0.38)	0.296	538.5
	PreLikeGillard	5.29 (0.48)	5.76 (0.41)	5.29 (0.37)	1.335	500.0
	PostImpressionAbbott	4.24 (0.49)	7.29 (0.51)	5.85 (0.38)	19.029***	252.0***
	PostImpressionGillard	7.26 (0.45)	4.71 (0.47)	5.65 (0.46)	14.667**	286.0**
	PostTrustAbbott	4.59 (0.43)	7.32 (0.51)	6.15 (0.38)	18.385***	260.5***
	PostTrustGillard	6.91 (0.42)	4.97 (0.43)	6.15 (0.40)	10.809**	326.5**
	PostLikeAbbott	3.88 (0.43)	6.24 (0.58)	5.18 (0.42)	11.026**	341.5**
	PostLikeGillard	5.68 (0.49)	4.15 (0.45)	5.41 (0.42)	5.836	403.0*
2	PreImpressionAbbott	6.76 (0.43)	7.50 (0.34)	6.76 (0.44)	1.761	477.0
	PreImpressionGillard	6.50 (0.36)	7.29 (0.43)	6.12 (0.45)	4.369	449.5
	PreTrustAbbott	6.41 (0.44)	7.12 (0.30)	7.32 (0.44)	2.700	499.0
	PreTrustGillard	6.56 (0.41)	7.32 (0.36)	6.35 (0.43)	3.094	465.0
	PreLikeAbbott	5.56 (0.46)	5.65 (0.43)	5.76 (0.49)	0.170	575.0
	PreLikeGillard	5.79 (0.44)	5.79 (0.48)	5.47 (0.45)	0.306	568.0
	PostImpressionAbbott	3.79 (0.41)	7.15 (0.49)	5.24 (0.48)	20.878***	226.5***
	PostImpressionGillard	7.35 (0.39)	4.79 (0.47)	6.00 (0.38)	15.270***	279.5***
	PostTrustAbbott	3.82 (0.40)	7.18 (0.47)	5.53 (0.51)	21.917***	207.5***
	PostTrustGillard	7.32 (0.41)	4.97 (0.46)	6.18 (0.36)	13.410**	302.0**
	PostLikeAbbott	3.91 (0.42)	6.09 (0.53)	5.56 (0.48)	9.822**	353.0**
	PostLikeGillard	6.68 (0.45)	4.29 (0.48)	5.79 (0.40)	12.905**	311.5**
3	PreImpressionAbbott	7.24 (0.39)	7.18 (0.39)	7.88 (0.27)	1.346	568.5
	PreImpressionGillard	6.12 (0.43)	7.09 (0.39)	7.26 (0.34)	4.134	452.0
	PreTrustAbbott	7.18 (0.35)	6.41 (0.41)	7.53 (0.32)	3.837	478.0
	PreTrustGillard	6.65 (0.38)	6.68 (0.40)	6.97 (0.33)	0.259	568.5
	PreLikeAbbott	6.59 (0.42)	5.94 (0.39)	6.59 (0.43)	2.301	491.0
	PreLikeGillard	5.85 (0.46)	5.85 (0.43)	6.26 (0.41)	1.065	576.5
	PostImpressionAbbott	5.29 (0.48)	6.82 (0.41)	6.26 (0.48)	5.512	384.0*
	PostImpressionGillard	6.50 (0.45)	5.47 (0.43)	6.21 (0.48)	3.027	445.5
	PostTrustAbbott	5.38 (0.49)	6.85 (0.45)	6.47 (0.47)	5.091	399.0*
	PostTrustGillard	6.44 (0.45)	5.76 (0.47)	6.29 (0.44)	1.365	493.0
	PostLikeAbbott	5.29 (0.48)	6.03 (0.48)	5.79 (0.53)	1.129	487.0
	PostLikeGillard	6.12 (0.47)	5.26 (0.54)	6.09 (0.51)	1.475	491.5

*$P < 0.05$; **$P < 0.01$; and ***$P < 0.001$: Kruskal–Wallis tests were conducted between all three groups, and Mann–Whitney *u* tests were conducted between groups 1 and 2. Preferences were measured for each candidate separately on 10-point Likert scales.

Table S3. Voting preferences by group for study 2

Voting preferences	Mean (SE) Group 1 (Gillard bias)	Group 2 (Abbott bias)	Group 3 (control)	Kruskal–Wallis (χ^2)	Mann–Whitney *u*
PreImpressionAbbott	7.40 (0.07)	7.36 (0.08)	7.37 (0.07)	0.458	241,861.5
PreImpressionGillard	7.13 (0.07)	7.12 (0.08)	7.13 (0.07)	0.081	243,115.0
PreTrustAbbott	7.26 (0.07)	7.22 (0.08)	7.18 (0.07)	0.954	241,924.5
PreTrustGillard	6.95 (0.07)	6.89 (0.08)	6.92 (0.07)	0.222	241,779.0
PreLikeAbbott	6.42 (0.08)	6.39 (0.08)	6.23 (0.08)	2.987	243,677.5
PreLikeGillard	6.24 (0.08)	6.30 (0.08)	6.11 (0.08)	3.178	239,556.0
PostImpressionAbbott	4.61 (0.09)	6.88 (0.09)	5.53 (0.09)	289.065***	120,660.0***
PostImpressionGillard	6.87 (0.08)	4.95 (0.09)	6.21 (0.09)	237.034***	133,106.5***
PostTrustAbbott	4.56(0.10)	6.94 (0.09)	5.57 (0.10)	281.560***	121,786.5***
PostTrustGillard	6.84 (0.09)	4.95 (0.09)	6.19 (0.09)	221.709***	136,689.0***
PostLikeAbbott	4.55 (0.09)	6.31 (0.09)	5.21 (0.09)	177.225***	146,957.0***
PostLikeGillard	6.34(0.09)	4.64 (0.09)	5.71 (0.09)	176.066***	147,372.5***

***$P < 0.001$: Kruskal–Wallis tests were conducted between all three groups, and Mann–Whitney *u* tests were conducted between groups 1 and 2. Preferences were measured for each candidate separately on 10-point Likert scales.

Table S4. Treatment effect estimates for study 2 voting preferences

	Presearch vote		Postsearch vote	
Predictor variable	Coefficient	SE	Coefficient	SE
Intercept	−0.073	0.540	0.062	0.543
Sex				
Female	0	Referent	0	Referent
Male	0.039	0.110	−0.135	0.119
Other	−0.430	0.922	−0.568	0.924
Race/ethnicity				
White	0	Referent	0	Referent
Black	0.115	0.224	0.090	0.245
Hispanic	−0.435	0.235	−0.280	0.237
Asian	0.366	0.238	0.668	0.291*
Other	0.133	0.274	−0.072	0.291
Age group				
18–24	0	Referent	0	Referent
25–44	−0.024	0.144	−0.083	0.157
45–64	0.241	0.184	0.029	0.200
65+	0.258	0.411	0.685	0.519
Education level				
Less than ninth grade	0	Referent	0	Referent
Ninth to 12th grade	0.024	0.548	0.732	0.550
High school graduate	0.074	0.528	0.927	0.528
Bachelors	0.094	0.529	0.842	0.530
Advanced	−0.050	0.543	0.549	0.544

The presearch and postsearch columns report the estimate and variance for both treatment groups using classical regression poststratification. Data for sex, race/ethnicity, age group, and education level came from the 2010 US Census. Data on the number of people who identify their sex as "other" came from a 2011 Gallup study.

*$P < 0.05$.

Table S5. Demographics for study 3

Category	Value	Study 3		Indian Census 2011 (literates)	
		n	%	*n*	%
Age	18–24	602	28.0%	160,241,457	21.0%
	25–44	1410	65.6%	347,587,712	45.6%
	45–64	124	5.8%	188,197,343	24.7%
	65+	14	0.7%	66,185,333	8.7%
Religion	Buddhism	14	0.7%	—	—
	Christianity	262	12.2%	—	—
	Hinduism	1512	70.3%	—	—
	Islam	314	14.6%	—	—
	Jainism	21	1.0%	—	—
	Other	15	0.7%	—	—
	Sikhism	12	0.6%	—	—
Sex	Male	1518	70.6%	388,428,872	51.0%
	Female	632	29.4%	373,782,973	49.0%
Education	None	0	0.0%	—	—
	Primary school	4	0.2%	—	—
	Higher secondary	71	3.3%	—	—
	Pre-university	136	6.3%	—	—
	Bachelors	1225	57.0%	—	—
	Masters	699	32.5%	—	—
	Doctorate	15	0.7%	—	—
Used	Yes	1635	76.0%	—	—
	No	515	24.0%	—	—
Income	Under Rs 10,000	121	5.6%	—	—
	Rs 10,000 to Rs 29,999	206	9.6%	—	—
	Rs 30,000 to Rs 49,999	131	6.1%	—	—
	Rs 50,000 to Rs 69,999	106	4.9%	—	—
	Rs 70,000 to Rs 89,999	146	6.8%	—	—
	Rs 90,000 to Rs 109,999	181	8.4%	—	—
	Rs 110,000 to Rs 129,999	172	8.0%	—	—
	Rs 130,000 to Rs 149,999	132	6.1%	—	—
	Rs 150,000 to Rs 169,999	124	5.8%	—	—
	Rs 170,000 to Rs 189,999	118	5.5%	—	—
	Rs 190,000 and over	486	22.6%	—	—
	I prefer not to say	227	10.6%	—	—
Marital status	Married	1,144	53.2%	—	—
	Widowed	5	0.2%	—	—
	Divorced	4	0.2%	—	—
	Separated	78	3.6%	—	—
	Never married	919	42.7%	—	—
Location	State	1,144	53.2%	749,758,470	98.4%
	Union Territory	5	0.2%	12,453,375	1.6%

Table S6. Voting Preferences by Group for Study 3

	Mean (SE)			
Voting preferences	Group 1 (Gandhi bias)	Group 2 (Kejriwal bias)	Group 3 (Modi bias)	Kruskal–Wallis (χ^2)
PreImpressionGandhi	5.94 (0.10)	5.73 (0.10)	5.65 (0.10)	4.782
PreImpressionKejriwal	6.80 (0.09)	7.07 (0.09)	7.09 (0.08)	6.230*
PreImpressionModi	7.49 (0.10)	7.46 (0.10)	7.48 (0.09)	0.188
PreLikableGandhi	5.71 (0.10)	5.64 (0.10)	5.61 (0.10)	0.722
PreLikableKejriwal	6.68 (0.09)	6.78 (0.09)	6.87 (0.09)	2.030
PreLikableModi	7.40 (0.10)	7.29 (0.10)	7.29 (0.10)	1.483
PreTrustGandhi	5.57 (0.11)	5.52 (0.11)	5.42 (0.10)	0.955
PreTrustKejriwal	6.54 (0.10)	6.74 (0.10)	6.85 (0.09)	4.546
PreTrustModi	7.22 (0.11)	7.31 (0.11)	7.27 (0.10)	0.159
PreLikelyToVoteGandhi	0.10 (0.12)	0.08 (0.12)	0.08 (0.12)	1.587
PreLikelyToVoteKejriwal	1.19 (0.11)	1.38 (0.11)	1.55 (0.10)	5.178
PreLikelyToVoteModi	2.15 (0.12)	2.12 (0.12)	2.06 (0.12)	0.202
PostImpressionGandhi	5.78 (0.10)	5.52 (0.10)	5.35 (0.10)	9.552**
PostImpressionKejriwal	6.50 (0.09)	6.96 (0.09)	6.70 (0.08)	14.288**
PostImpressionModi	7.27 (0.10)	7.26 (0.10)	7.60 (0.09)	7.860*
PostLikableGandhi	5.62 (0.10)	5.46 (0.10)	5.26 (0.10)	6.322*
PostLikableKejriwal	6.37 (0.09)	6.84 (0.09)	6.64 (0.08)	13.456**
PostLikableModi	7.24 (0.11)	7.20 (0.11)	7.47 (0.10)	3.874
PostTrustGandhi	5.71 (0.11)	5.48 (0.10)	5.22 (0.10)	11.386*
PostTrustKejriwal	6.38 (0.10)	6.89 (0.10)	6.68 (0.08)	15.840***
PostTrustModi	7.18 (0.11)	7.20 (0.11)	7.49 (0.10)	4.758

*$P < 0.05$; **$P < 0.01$; and ***$P < 0.001$: Kruskal–Wallis tests were conducted between all three groups. Preferences were measured for each candidate separately on 10-point Likert scales.

Table S7. Treatment effect estimates for study 3 voting preferences

	Presearch vote		Postsearch vote	
Predictor variable	Coefficient	SE	Coefficient	SE
Intercept	−0.716	0.090***	−0.552	0.088***
Sex				
Male	0	Referent	0	Referent
Female	0.168	0.100	0.030	0.099
Age group, y				
18–24	0	Referent	0	Referent
25–44	0.031	0.103	0.067	0.101
45–64	−0.222	0.217	−0.057	0.208
65+	−0.213	0.598	−0.366	0.598
Location				
State	0	Referent	0	Referent
Union Territory	−0.401	0.294	−0.321	0.279

The presearch and postsearch columns report the estimate and variance for both of the treatment groups using classical regression poststratification. Data for sex, age group, and location came from the 2011 India Census.

***$P < 0.001$.

Table S8. Minimum VMP levels needed to impact two-person races with various projected win margins and proportions of undecided Internet voters

Proportion of undecided Internet voters in the population (*i*u*)	Projected win margin									
	0.01	0.02	0.03	0.04	0.05	0.06	0.07	0.08	0.09	0.10
0.01	1.000	—	—	—	—	—	—	—	—	—
0.02	0.500	1.000	—	—	—	—	—	—	—	—
0.03	0.333	0.667	1.000	—	—	—	—	—	—	—
0.04	0.250	0.500	0.750	1.000	—	—	—	—	—	—
0.05	0.200	0.400	0.600	0.800	1.000	—	—	—	—	—
0.06	0.167	0.333	0.500	0.667	0.833	1.000	—	—	—	—
0.07	0.143	0.286	0.429	0.571	0.714	0.857	1.000	—	—	—
0.08	0.125	0.250	0.375	0.500	0.625	0.750	0.875	1.000	—	—
0.09	0.111	0.222	0.333	0.444	0.556	0.667	0.778	0.889	1.000	—
0.10	0.100	0.200	0.300	0.400	0.500	0.600	0.700	0.800	0.900	1.000
0.11	0.091	0.182	0.273	0.364	0.455	0.545	0.636	0.727	0.818	0.909
0.12	0.083	0.167	0.250	0.333	0.417	0.500	0.583	0.667	0.750	0.833
0.13	0.077	0.154	0.231	0.308	0.385	0.462	0.538	0.615	0.692	0.769
0.14	0.071	0.143	0.214	0.286	0.357	0.429	0.500	0.571	0.643	0.714
0.15	0.067	0.133	0.200	0.267	0.333	0.400	0.467	0.533	0.600	0.667
0.16	0.063	0.125	0.188	0.250	0.313	0.375	0.438	0.500	0.563	0.625
0.17	0.059	0.118	0.176	0.235	0.294	0.353	0.412	0.471	0.529	0.588
0.18	0.056	0.111	0.167	0.222	0.278	0.333	0.389	0.444	0.500	0.556
0.19	0.053	0.105	0.158	0.211	0.263	0.316	0.368	0.421	0.474	0.526
0.20	0.050	0.100	0.150	0.200	0.250	0.300	0.350	0.400	0.450	0.500

Other Supporting Information Files

Dataset S1 (XLS)