

Automated Experiments on Ad Privacy Settings: A Tale of Opacity, Choice, and Discrimination

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Abstract—To partly address people’s concerns over web tracking, Google has created the Ad Settings webpage to provide information about and some choice over the profiles Google creates on users. We present AdFisher, an automated tool that explores how user behaviors, Google’s ads, and Ad Settings interact. Our tool uses a rigorous experimental design and analysis to ensure the statistical significance of our results. It uses machine learning to automate the selection of a statistical test. We use AdFisher to find that Ad Settings is opaque about some features of a user’s profile, that it does provide some choice on ads, and that these choices can lead to seemingly discriminatory ads. In particular, we found that visiting webpages associated with substance abuse will change the ads shown but not the settings page. We also found that setting the gender to female results in getting fewer instances of an ad related to high paying jobs than setting it to male.

I. INTRODUCTION

Problem and Overview. Online advertisers target ads to Internet users based on their browsing behaviors (e.g., [1]). Many websites providing content, such as news, outsource their advertising operations to large third-party ad networks, such as Google’s DoubleClick. These networks embed tracking code into webpages across many sites providing the network with a more global view of each user’s behaviors.

People are concerned about behavioral marketing on the web (e.g., [2]). They would like to know what information companies use to determine the ads they show to them. To increase transparency into these issues, Google provides the Ad Settings webpage, which is dynamically generated for each user (as approximated with tracking cookies). A user can see the page for himself at <https://www.google.com/settings/ads>. Figure 1 provides a screenshot. Yahoo¹ and Microsoft² also offer personalized ad settings.

However, they provide little information about how these pages operate. We would like to know how completely the settings describe their profiles of users. In particular, we want to know how a user’s behaviors, either directly with the advertiser or with content providers, alter the ads and settings shown to the user and whether these changes are in harmony. We also want to know how changing settings affects ads. To explain these matters, we ran experiments where automated agents simulating users interact with Google and content providers and we measure how these interactions alter the ads and settings that Google shows.

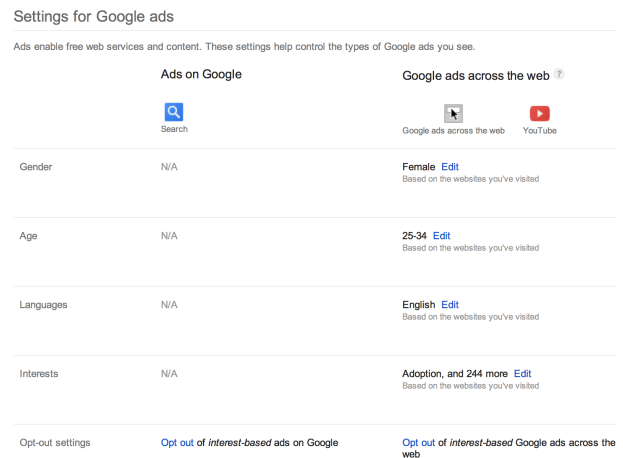


Fig. 1. Screenshot of Google’s Ad Settings webpage

To automate these experiments, we created AdFisher, a tool that automates randomized, controlled experiments for learning about online tracking. Our tool offers a combination of automation, statistical rigor, scalability, and explanation for determining the use of information by web advertising algorithms and by personalized ad settings, such as Google Ad Settings. The tool automates the simulation of having a particular interest or attribute by visiting webpages associated with that interest or by altering the ad settings provided by Google. It automates the collection of ads shown to the simulated users and the settings that Google provides. It automatically analyzes the data to determine whether statistically significant differences between groups of agents exist. To do so, AdFisher uses machine learning to automatically detect differences and then executes a test of significance specialized for the difference it might have found.

The only inputs the operator has to provide to our tool are the behaviors the two groups are to perform and the measurements to make over the agents after they perform those behaviors. Thus, we could easily run multiple experiments exploring the causal connections between the ads and settings that Google shows.

Motivating Experiments. For example, in one experiment, we explored whether visiting websites related to substance abuse has an impact on Google’s ads or settings. We created an experimental group and a control group of agents. The agents in the experimental group visited such websites while the agents in the control group simply waited idly. Then, both groups of agents visited the Times of India, a content providing

¹https://info.yahoo.com/privacy/us/yahoo/opt_out/targeting/details.html

²<http://choice.microsoft.com/en-us/opt-out>

webpage that uses Google for advertising. We collected the ads shown to the agents.

Having run the experiment and collected the data, we had to determine whether any difference existed in the outputs shown to the agents. One way to check would be to intuit what the difference could be and test for that difference. For example, we might have suspected that the experimental group would have received more ads containing the word “alcohol”. Thus, we could use the *test statistic* that counts the number of instances of “alcohol” in the ads of the experimental group and subtracts from it the number of times it appears in the control group. If no difference exists, we would expect this test statistic would have a value close to zero. If our suspicion is correct, we would expect a large positive value.

However, developing the intuition that “alcohol” is a difference between the groups can take considerable effort. Thus, we instead use machine learning to automatically find such patterns in a training subset of the data. AdFisher found a classifier that could, using just the ads shown to an agent, accurately determine whether the agent visited the webpages related to substance abuse. We used a separate test subset of the data to verify that the classifier found a statistically significant difference between the ads shown to each group of agents.

We also measured the settings that Google provided to each agent at its Ad Settings page after the experimental group of agents visited the webpages associated with substance abuse. We found no differences (significant or otherwise) between the pages for the agents. Thus, we have determined that Google is using information related to visiting webpages associated with substance abuse but not reporting this use. Rather than providing transparency, the ad settings are *opaque*.

In another experiment we examined whether the settings provided *choice* to the user. We found that removing interests from Google Ad Settings page changes the ads that a user sees. In particular, we had both groups of agents visit a site related to online dating. Then, only one of the groups removed the interest related to online dating. Thereafter, the top ads shown to the group that kept the interest were related to dating but not the top ads shown to the other group. Thus, the ad settings do offer the users a degree of choice over the ads they see.

In another experiment, we found evidence suggestive of *discrimination*. We set the agents’ gender to female or male on Google’s Ad Settings page. We then had both the female and male groups of agents visit webpages associated with employment. Again using the ads on the Times of India, we established that Google used this gender information to select ads, as one would expect. In this case, the interesting result was how the ads differed as learned by the classifier: Google showed the simulated males certain ads that promised large salaries more frequently than the simulated females, a finding suggestive of discrimination.

While neither of our findings of opacity or discrimination are clear violations of Google’s privacy policy,³ they are both concerning.

Contributions. In addition to the experimental findings highlighted above, we provide AdFisher, a tool for running

such experiments. AdFisher *automates* the running of the experiment and the analysis of the data.

AdFisher offers *rigor* by carefully selecting our experimental design and statistical analysis to not make questionable assumptions about the data we collect. In particular, we base our design and analysis on a prior proposal that makes no assumptions about the data being independent or identically distributed [3]. We find such assumptions, common in statistical analyses, problematic for studying online advertisers since advertisers update their behavior continuously in response to unobserved inputs (such as online ad auctions) and the experimenters’ own actions. Indeed, in practice, the distribution of ads changes over time and agents interfere with one another [3].

Our automation, experimental design, and statistical analyses allow us to *scale* to handling large numbers of agents for finding subtle differences. In particular, we modify the prior analysis of Tschantz et al. [3] to allow for experiments running over long periods of time. We do so by using *blocking* (e.g., [4]), a nested statistical analysis not previously applied to understanding web advertising. The blocking analysis ensures that agents are only compared to the agents that start out like it and then aggregates together the comparisons across blocks of agents. Thus, AdFisher may run agents in batches spread out over time while only comparing those agents running simultaneously to one another.

Our tool provides an *explanation* as to how Google alters its behaviors in response to different user actions. We use machine learning to determine ads that accurately distinguish the groups of agents. We allow the experimenter to find differences between how Google treats the agents with ads and how Google represents agents on the Ad Settings page.

To maintain statistical rigor, we carefully circumscribe our claims. We only claim statistical soundness in the sense that upon reporting that the difference in agent behavior causes an effect, such an effect really was caused by the difference in behavior with high likelihood (made quantitative by a *p*-value). We do not claim that we will always find a difference if one exists, nor that the differences we find are typical of those experienced by users. Furthermore, while we can characterize the differences, we cannot assign blame for them since either Google or the advertisers working with Google could be responsible.

Contents. After covering prior work next, we present, in Section III, privacy properties that our tool AdFisher can check: nondiscrimination, transparency, and choice. Section IV explains the methodology we use to ensure sound conclusions from using AdFisher. Section V presents the design of AdFisher. Section VI discusses our use of AdFisher to study Google’s ads and settings. We end with conclusions and future work. Appendix A provides details about our methodology.

Source code for AdFisher, raw data, and additional details about our experiments can be found at
<http://www.cs.cmu.edu/~mtschant/ife/>

II. PRIOR WORK

We are not the first to study how Google uses information. The work with the closest subject of study to ours is by Wills

³<https://www.google.com/intl/en/policies/privacy/>

and Tatar [5]. They studied both the ads shown by Google and the behavior of Google’s Ad Settings (then called the “Ad Preference Manager”). Like us, they find the presence of opacity: various interests impacted the ads and settings shown to the user and that ads could change without a corresponding change in Ad Settings. Unlike our study, theirs was mostly manual, small scale, lacked any statistical analysis, and did not follow a rigorous experimental design. Furthermore, we additionally study choice and discrimination.

The other related works differ from us in both goals and methods. They all focus on how visiting webpages change the ads seen. While we examine such changes in our work, we do so as part of a larger analysis of the interactions between ads and personalized ad settings, a topic they do not study.

Barford et al. come the closest in that their recent study looked at both ads and ad settings [6]. They do so in their study of the “adscape”, an attempt to understand each ad on the Internet. They study each ad individually and cast a wide net to analyze many ads from many websites while simulating many different interests. They only examine the ad settings to determine whether they successfully induced an interest. We rigorously study how the settings affects the ads shown (choice) and how behaviors can affect ads without affecting the settings (opacity). Furthermore, we use focused collections of data and an analysis that considers all ads collectively to find subtle causal effects within Google’s advertising ecosystem. We also use a different randomized experimental design and analysis to ensure that our results imply causation without making an assumption of independence between ads, which appears to not hold in our setting [3].

The usage study closest to ours in statistical methodology is that of Tschantz et al. [3]. They were only interested in finding a rigorous methodology for determining whether a system like Google uses information. Due to limitations of their methodology, they only ran small-scale studies. While they observed that browsing behaviors could affect Ad Settings, they did not study how this related to the ads received. Furthermore, while we build upon their methodology, we automate it by using machine learning to select an appropriate test statistic whereas they manually selected test statistics.

The usage study closest to ours in terms of implementation is that of Liu et al. in that they also use machine learning [7]. Their goal is to determine whether an ad was selected due to the content of a page, by using behavioral profiling, or from a previous webpage visit. Thus, rather than use machine learning to select a statistical test for finding causal relations, they do so to detect whether an ad on a webpage matches the content on the page to make a case for the first possibility. Thus, they have a separate classifier for each interest a webpage might cover. Rather than perform a statistical analysis to determine whether treatment groups have a statistically significant difference, they use their classifiers to judge the ratio of ads on a page unrelated to the page’s content, which they presume indicates that the ads were the result of behavioral targeting.

Lécuyer et al. present XRay, a tool that looks for correlations between the data that web services have about users and the ads shown to users [8]. Their tool looks through many changes to a type of input to determine whether any of them has a correlation with the frequency of a single ad. Our tool

instead looks for causal relations from a single change to an input by checking many different possible changes over many ads. To enable XRay examining many possible inputs using a small number of user accounts, which are expensive to create, they adopt assumptions including independence between ads. We do not assume independence since it appears to not hold in our setting [3], a setting that does not require creating accounts.

Guha et al. compares ads seen by three agents to see whether Google treats differently the one that behaves differently from the other two [9]. We adopt their suggestion of focusing on the title and URL displayed on ads when comparing ads to avoid noise from other less stable parts of the ad. Our work differs by studying the ad settings in addition to the ads and by using larger numbers of agents. Furthermore, we use rigorous statistical analyses whereas they used an ad hoc metric. Balebako et al. run experiments similar to Guha et al.’s to study the effectiveness of privacy tools [10].

Sweeney manually ran an experiment to determine that searching for names associated with African-Americans produce more search ads containing the word “arrest” than names associated with European-Americans [11]. Not only did hand collecting the data for her study take a considerable amount of time, but her study also required considerable insight to determine that the word “arrest” was a key difference. AdFisher can automate not just the collection of the ads, but also the identification of such key differences by using its machine learning capabilities. Indeed, it found on its own that simulated males were more often than simulated females shown ads encouraging the user to seek high paying jobs.

III. PRIVACY PROPERTIES

Motivating our methodology for finding causal relationships, we present some properties of ad networks that we can check with such a methodology in place. As a fundamental limitation of science, we can only prove the existence of a causal effect; we cannot prove that one does not exist (see Section IV-E). Thus, experiments can only demonstrate violations of nondiscrimination and transparency, which require effects. On the other hand, we can experimentally demonstrate that effectful choice and ad choice are complied with in the cases that we test since compliance follows from the existence of an effect. Table I summarizes these properties.

A. Discrimination

At its core, *discrimination* between two classes of individuals (e.g., one race vs. another) occurs when the attribute distinguishing those two classes causes a change in behavior toward those two classes. In our case, discrimination occurs when membership in a class causes a change in ads. Such discrimination is not always bad (e.g., many would be comfortable with men and women receiving different clothing ads) and we will make no attempt in this paper to determine the acceptability of discrimination.

Determining whether class membership causes a change in ads can be difficult since many factors not under the experimenter’s control or even observable to the experimenter may also cause changes. However, our experimental methodology can determine when membership in certain classes causes changes in ads by comparing many instances of each class.

Property Name	Requirement	Causal Test	Finding
Nondiscrimination	The same behavior toward two classes	Find that class membership causes an effect	Violation
Data use transparency	User allowed access to all data about him used for ad selection	Find individual attribute not shared that causes a change in ads	Violation
Effectful choice	Changing a setting has some effect	Find that a setting causes a change	Compliance
Ad choice	Removing an interest decreases the number ads related to that interest	Find setting causes a decrease in such ads	Compliance
		Find setting causes an increase in such ads	Violation

TABLE I. PRIVACY PROPERTIES

We are limited in the classes we can consider since we cannot create actual people that vary by the traditional subjects of discrimination, such as race or gender. Instead, we look at classes that function as surrogates for those classes of interest. For example, rather than directly looking at how gender affects people’s ads, we instead look at how altering a gender setting affects ads or at how visiting websites associated with each gender affects ads.

B. Transparency

Ad users should have some understanding of the information that ad networks collect and use about them. However, ad networks should not offer complete transparency in the sense of sharing all the information it has with users. To do so would create privacy violations since it would involve sharing information with one user about all the other users. Even sharing all the information that the network uses to decide the ads that it shows to a user can be problematic since networks often use complex models built using information from other users.

Thus, we study a more constrained form of transparency that focuses on information about the user in question. *Individual data use transparency* requires that the ad network shares the information that it uses about a user for selecting ads for him with him. Ad privacy settings satisfy this requirement if two people with the same settings who are accessing a webpage in the same manner receives the same distribution of ads. That is, the ad network does not discriminate between users with the same settings on the basis of any class memberships. Thus, we may test for transparency by looking for effects caused by factors not recorded in the ad settings.

C. Choice

Ad privacy settings offer the prospect of choice for users. In particular, some of them, such as Google’s Ad Settings, offer users the option of changing the settings inferred about them. However, the exact nature of how these changes impact the ad network is unclear. Thus, we examine two notions of choice.

A very coarse form we consider is *effectful choice*, which requires that altering the settings has some effect on the ads seen by the user. This shows that altering settings is not merely a “placebo button”: it has a real effect on the network’s ads. Ideally, the effect would be meaningful and related to the altered setting.

One way such an effect would be meaningful, in the case of removing an inferred interest, is for the change to decrease the number of ads related to the interest seen by the user. We call this requirement *ad choice*. (We focus on removing interests

since that’s the more privacy related form of choice. Much of what we write will also hold for adding interests but with the proper negations.) In the extreme case, the user no longer receives any ads related to that interest. This standard raises questions about defining when an ad is related to an interest.

One way to judge whether an ad is relevant is to check it for keywords associated with the removed interest. We will conclude that an ad network has ad choice when removing interests causes a statistically significant decrease in the number of ads with keywords related to that interest seen by the user.

We cannot find all violations of ad choice since when the difference in ads is small, we are unable to determine whether it represents a true difference between two distributions or just noise from sampling a finite number of ads from one underlying distribution. However, we can test for a statistically significant increase in the number of related ads to find egregious violations. Thus, by requiring the effect to have a fixed direction, we can find both compliance and violations of ad choice.

Note that many of the above conditions can be reformulated without causation. For example, rather than checking whether removing an interest results in no longer getting ads related to that interest, we could instead simply check whether those who have that interest removed do not get such ads. Only the second weaker definition will be satisfied in the case that the ad network would not show that person such ads in either case. We find the causal forms of these definitions to better capture *choice* since the non-causal forms are satisfied when no real choices are possible: the person does not see such ads regardless of their choices.

IV. METHODOLOGY

The goal of our methodology is to establish that a certain type of input to a system causes an effect on a certain type of output of the system. For example, in our experiments, we study the system of Google. The inputs we study are visits to content providing websites and users’ interactions with Google Ad Settings webpage. The outputs we study are the settings and ads shown to the users by Google. However, nothing in our methodology limits ourselves to these particular topics; it is appropriate for determining I/O properties of any web system.

Here we present just an overview of our methodology. Appendix A provides details of the statistical analysis.

A. Background: Hypothesis Testing

To establish causation, we start with the approach of Fisher (our tool’s namesake) to randomized hypothesis testing [12] as specialized by Tschantz et al. for the setting of online

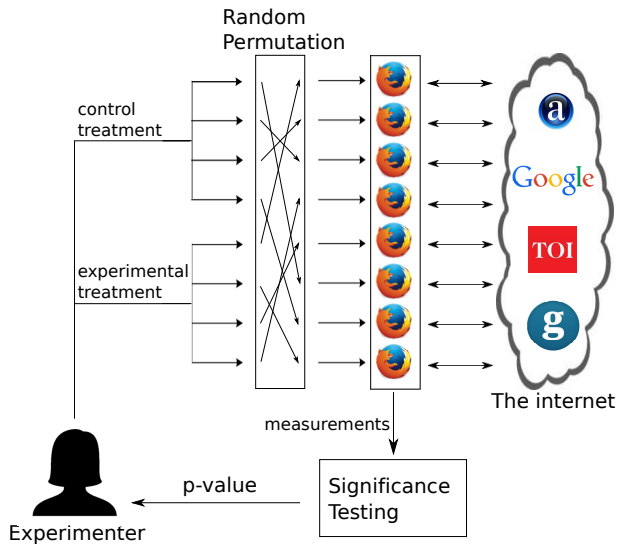


Fig. 2. Experimental setup to carry out significance testing on 8 browser agents comparing the effects of two treatments. Each agent produces performs actions on the web and produces measurements

systems [3]. The experimenter considers a *null hypothesis*, in our case, that the inputs do not affect the outputs. To test this hypothesis the experimenter selects two values that the type of inputs could take on, typically called the *control* and *experimental treatments*.

The experimenter applies the treatments to *experimental units*. In our setting, the units are the agents, that is, simulated users. The experimental units should be *exchangeable*, that is, initially identical as far as the inputs and outputs in question are concerned. For example, an agent created with the Firefox browser would not be exchangeable to one created with the Internet Explorer browser since Google can detect the browser used.

The experimenter randomly partitions the agents to a *control group* and an *experimental group*. (See Figure 2.) To each group, the experimenter applies the group’s respective treatment by having the agents perform actions producing inputs to Google. Next, the experimenter takes measurements of the outputs Google sends to the agents, such as ads. At this point, the experiment proper is complete and data analysis begins.

Data analysis starts by computing a *test statistic* over the measurements. The experimenter selects a test statistic that she suspects will take on a high value when the outputs to the two groups differ. She then uses the *permutation test* to determine whether the value the test statistic actually took on is higher than what one would expect by chance unless the groups actually differ. The permutation test compares the actual value of the test statistic to each hypothetical value it would have taken on if the random assignment of agents to groups had occurred differently and the null hypothesis is true. Since the null hypothesis is that the inputs have no effect and the units start exchangeable, the random assignment should have no effect on the value of the test statistic. Thus, under the null hypothesis, it is unlikely to see the actual value of the test statistic being larger than the vast majority of hypothetical

values.

The probability of seeing the test statistic take on a value as high as it did under the null hypothesis is called the *p-value*. If the value of the test statistic is so high that under the null hypothesis it would only take on as high of a value in 5% of the random assignments, then we conclude that the value is *statistically significant* (at the 5% level) and that causation is likely. Even lower p-values suggest that the results are even more statistically significant.

B. Blocking

In practice, the above methodology can be difficult to use since creating a large number of exchangeable agents might not be possible. In our case, we found that we could only run ten agents in parallel given our hardware and network connection. Agents running at different times are not exchangeable since Google can determine the time at which an agent interacts with it. Thus, under the above methodology, we were limited to just ten exchangeable units. Since some effects that the inputs have on Google’s outputs can be probabilistic and subtle, they might be missed looking at just ten agents.

To avoid this limitation, we extended the above methodology to handle nonexchangeable units using *blocking* [4]. To use blocking, we created *blocks* of exchangeable agents running in parallel. Each block’s agents were randomly partitioned into the control and experimental groups. Running these blocks in a staged fashion, the experiment proceeds on block after block. A modified permutation test now only compares the actual value of the test statistic to hypothetical values computed by reassignments of agents that respect the blocking structure.

Using blocking, we can scale to any number of agents by running as many blocks as needed. However, the computation of the permutation test increases exponentially with the number of blocks. Thus, rather than compute the exact p-value, we estimate it by randomly sampling the possible reassignments. We can use a confidence interval to characterize the quality of the estimation [4]. The p-values we report are actually the upper bounds of the 99% confidence intervals of the p-values.

C. Selecting Test Statistics

The above methodology leaves open the question of how to select the test statistic. In some cases, the experimenter might be interested in a particular test statistic. For example, an experimenter testing ad choice could use a test statistic that counts the number of ads related to the removed interest. In other cases, the experimenter might be looking for any effect. Thus, AdFisher offers the ability to automatically select a test statistic. To do so, it collects an additional data set used for training a classifier with machine learning. Figure 3 shows an overview of AdFisher’s workflow.

To select a classifier, AdFisher uses cross validation on the training data to select among several possibilities. It then converts the classifier that is most accurate on the training data into a test statistic by treating it as a function over the ads seen by the agents to the number of correctly classified agents. If there is a difference between the ads shown to the two groups of agents, we would expect that the number correctly classified to be high. If no difference exists, then we would expect the number to be near to the guessing rate of 50%.

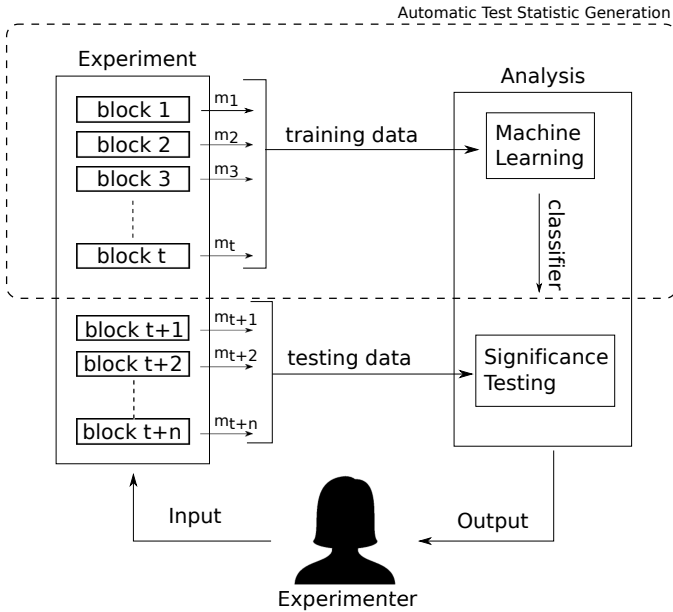


Fig. 3. Figure describing our methodology. The portion within the dotted lines comes into play if the experimenter desires to use machine learning to select test statistics.

To avoid the possibility of seeing a high accuracy due to overfitting, AdFisher evaluates the accuracy of the classifier on a testing data set that is disjoint from the training data set. That is, in the language of statistics, we form our hypothesis about the test statistic being able to distinguish the groups before seeing the data on which we test it to ensure that it has predictive power. AdFisher uses the permutation test to determine whether the degree to which the classifier’s accuracy on the test data surpasses the guessing rate is statistically significant. That is, it calculates the p-value that measures the probability of seeing the observed accuracy given that the classifier is just guessing. If the p-value is below 0.05, we conclude that it is unlikely that classifier is guessing and that it must be making use of some difference between the ads shown to the two groups.

D. Avoiding Pitfalls

The above methodology avoids some pitfalls. Most fundamentally, we use a statistical analysis whose assumptions matches those of our experimental design. Assumptions required by many statistical analyses appear unjustifiable in our setting. For example, many analyses assume that the agents do not interact or that the ads are independent and identically distributed (e.g., [6], [8]). However, given that all the agents receive ads from the same pool of possible ads governed by the same advertisers’ budgets, these assumptions appear unlikely to hold. Indeed, empirical evidence suggests that it does not [3]. Our use of the permutation test, which does not require this assumption, allows us to ensure the statistical soundness of our analysis without making these assumptions [13].

Our use of randomization implies that many factors that could be confounding factors in unrandomized design become noise in our design (e.g., [4]). While such noise may require us to use a large sample size to find an effect, it does not affect the soundness of our analysis.

Our use of two data sets, one for training the classifier to select the test statistic and one for hypothesis testing ensures that we do not engage in overfitting, data dredging, or multiple hypothesis testing (e.g., [14]). All these problems result from looking for so many possible patterns that one is found by chance. While we look for many patterns in the training data, we only check for one in the testing data.

Relatedly, by reporting a p-value, we provide a quantitative measure of the confidence we have that the observed effect is genuine and not just by chance [15]. Reporting simply the classifier accuracy or that some difference occurred fails to quantify the possibility that the result was a fluke.

E. Scope

We restrict the scope of our methodology to making claims that an effect exists with high likelihood as quantified by the p-value. That is, we expect our methodology to only rarely suggest that an effect exists when one does not.

We do not claim the converse of “completeness” or “power”: we might fail to detect some use of information. For example, Google might turn off some usage upon detecting that our tool does not behave like a normal user. Despite this limitation, we found interesting instances of usage.

Furthermore, we do not claim that our results generalize to all users. To do so, we would need to take a random sample of all users, their IP addresses, browsers, and behaviors, which is prohibitively expensive. For example, instead of turning off some usage upon detecting our tool, Google might turn it on. While the detected usage would be real (and our method sound), it might not be experienced by normal users. However, it would be odd if Google purposefully performs questionable behaviors only in front of those attempting to find it.

While we use webpages associated with various interests to simulate users with those interests, we cannot establish that having the interest itself caused the change to ads. It is possible that other features of the visited webpages causes change, a form of confounding called “profile contamination” [6], since the pages cover other topics as well. Nevertheless, we have determined that visiting webpages associated with the interest does result in seeing a change, which should give pause to those visiting webpages associated with sensitive interests.

Lastly, we do not attempt to determine how the information was used. It could have been used by Google directly for targeting or it could have been used by advertisers to place their bids. We cannot assign blame. We hope future work will shed light on these issues, but given that we cannot observe the interactions between Google and advertisers, we are unsure whether it can be done.

V. ADFISHER

We provide AdFisher, a tool implementing our methodology. AdFisher makes it easy to run experiments using the above methodology for the set of treatments, measurements, and classifiers (test statistics) we have implemented. AdFisher is also extensible allowing the experimenter to implement additional treatments, measurements, or test statistics. For example, an experimenter without any interest in methodological concerns could use AdFisher to run a study on an online system

we have not considered by writing just the code needed to interact with that system; she will not need to reimplement the code running and analyzing the experiment.

AdFisher is structured as a Python API providing functions for setting up, running, and analyzing experiments. We use the SciPy [16] library for implementing the statistical analyses of the core methodology. For browser automation, we used Selenium 2.39.0 and its bindings for Python 2.7 to drive Firefox browsers.

To simulate a new person on the network, AdFisher creates each agent from a fresh browser instance with no browsing history, cookies, or other personalization. AdFisher randomly assigns each agent to a group and applies the appropriate treatment, such as having the browser visit webpages. Next, AdFisher makes measurements of the agent, such as collecting the ads shown to the browser upon visiting another webpage. All of the agents within a block executes and finishes the treatments before moving on to the measurements to remove time as a factor. AdFisher runs all the agents on the same machine to prevent differences based on location or IP-address between agents.

We detail the particular treatments, measurements, and test statistics that we have implemented below. We also discuss how AdFisher aids the experimenter in understanding the results. Additional information about parameters that the experimenter may vary can be found in Appendix B.

Treatments. AdFisher automatically applies the treatments assigned to each group. Typically, these treatments involve invoking the Selenium WebDriver to make the agent interact with webpages.

AdFisher makes it easy to perform common treatments by providing ready-made implementations of these treatments. The simplest stock treatments we provide set interests in Google’s Ad Settings. We also provide ready-made treatments for setting the gender and age range. Another stock treatment is to visit a list of webpages.

To make it easy to see whether websites associated with a particular interest causes a change in behavior, we have provided the ability to create lists of webpages associated with each interest tracked by Alexa. For each category, Alexa tracks the top websites sorted according to their traffic rank measure (a combination of the number of users and page views).⁴ The function takes the URL of an interest page on Alexa and downloads the URLs of the top webpages Alexa associates with that interest. By default, it downloads 100. The list can then be converted into the treatment of visiting those webpages. While these treatments do not correspond directly to having such an interest, it allows us to study how Google responds to people visiting webpages associated with those interests.

Often in our experiments, we compared the effects of a certain treatment applied to the experimental group against the *null treatment* applied to the control group. Under the null treatment, agents do nothing while agents under a different treatment complete their respective treatment phase.

Measurements. Our tool currently measures the values set in Google’s Ad Settings page and the ads shown to the agents after the treatments. We provide stock measurements for collecting and analyzing text ads. Experimenters can add functionality for image, video, and flash ads as desired.

To find a good default webpage to visit for ad collection, we looked to news sites since they generally show many ads. We looked at the five of the top 20 news websites on alexa.com that also served text ads by Google: theguardian.com/us, timesofindia.indiatimes.com, bbc.com/news, reuters.com/news/us and bloomberg.com. Among these, we selected the Times of India since, at 5, it serves the most text ads per page reload.

We provide functionality that will visit any of the above listed news websites and reload the page a fixed number of times (default 10). For each page reload, it parses the page to find the ads shown by Google and stores the ads. The experimenter can add parsers for other webpages.

Classification. While the experimenter can provide AdFisher with a test statistic to use on all the collected data, AdFisher is also capable of splitting the data set into training and testing subsets. AdFisher automatically examines a training subset of the collected measurements to select a classifier that distinguishes between the measurements taken from each group. From the point of view of machine learning, each of the sets of ads collected by each agent corresponds to an *instance* of the concept the classifier is attempting to learn.

The machine learning algorithms operate over sets of *features*. AdFisher has functions for converting the text ads seen by an agent into three different feature sets. The *URL feature set* consists of the URLs displayed by the ads (or occasionally some other text if the ad displays it where URLs normally go). Under this feature set, the feature vector representing an agent’s data has a value of n in the i th entry iff the agent received n ads that display the i th URL where the order is fixed but arbitrary.

The *URL+Title feature set* looks at both the displayed URL and the title of the ad jointly. It represents an agent’s data as a vector where the i th entry is n iff the agent received n ads containing the i th pair of a URL and title.

The third feature set AdFisher has implemented is the *word feature set*. This set is based on word stems, the main part of the word with suffixes such as “ed” or “ing” removed in a manner similar to the work of Balebako et al. [10]. Each word stem that appeared in an ad is assigned a unique id. The i th entry in the feature vector is the number of times that words with the i th stem appeared in the agent’s ads.

We explored using a variety of classification algorithms provided by the scikit-learn library [17]. We found that logistic regression with an L2 penalty over the URL+title feature set consistently performed well compared to the others. Thus, we have disabled the others by default.

At its core, logistic regression predicts a class given a feature vector by multiplying each of the entries of the vector by its own weighting coefficient (e.g., [18]). It then takes the sum of all these products. If the sum is positive, it predicts one class; if negative, it predicts the other. Thus, since our feature vectors contain only 1s and 0s, coefficients of large positive

⁴<https://alexa.zendesk.com/hc/en-us/articles/200461970>

value imply that that feature is associated with being in the first group; negative values are associated with the second group; values near zero are uninformative.

While using logistic regression, the training stage consists of selecting the coefficients assigned to each feature to predict the training data well. Selecting coefficients requires balancing the training-accuracy of the model with avoiding overfitting the data with an overly complex model. To do so, we apply 10-fold cross-validation on the training data to select how complex of a model to use. By default, we split the data into training and test sets by using the last 10% of the data collected for testing.

Explanations. To explain how the learned classifier distinguished between the groups, we explored several methods. We found the most informative to be the model produced by the classifier itself. Recall that logistic regression weighted the various features of the instances with coefficients reflecting how predictive they are of each group. Thus, with the URL+title feature set, examining the features with the most extreme coefficients identifies the URL+title pair most used to predict the group to which agents receiving an ad with that URL+title belongs.

We also explored using the proportion of times an ad was served to agents in one group to the total number of times observed by all groups. However, this did not provide much insight since the proportion typically reached its maximum value of 1.0 from ads that only appeared once. Another choice we explored was to compute the difference in the number of times an ad appears between the groups. However, this metric is also highly influenced by how common the ad is across all groups.

VI. EXPERIMENTS

In this section, we discuss experiments that we carried out using AdFisher. We studied each of the properties of interest. We found violations of nondiscrimination and data transparency and cases of compliance with effectful and ad choice. Table II summarizes our findings. We start by providing additional background on Google’s Ad Settings page.

A. Background on Google Ad Settings

Google defines its Ad Settings page as “a Google tool that helps you control the ads you see on Google services and on websites that partner with Google”.⁵ This page contains demographics like gender, age, language, and interests. There are two classes for each of these demographics: (1) for ads on Google services like Search, Gmail, YouTube and Maps; and (2) for ads across the web. The demographics for ads on Google services show up only when a user is logged in, and follow the user irrespective of which browser the user logs in from. In this work, we do not analyze ads on Google services, or the corresponding demographics on Ad Settings. The demographics for ads across the web follow the browser irrespective of who is logged in (or not). They show up as “N/A” if the user has either opted out of interest-based Google ads across the web, or if he has cleared all cookies. In either case, the user has the option to enable interest-based ads. Once enabled, Google places a cookie on the browser to keep track

Property	Treatment	Version	Finding
Nondiscrimination	Gender	W/o jobs, May	Inconclusive
	Gender	Jobs, May	Violation
	Gender	Jobs, July	Inconclusive
	Gender	Jobs, Guardian, July	Inconclusive
	Gender	jobs & top 10, July	Inconclusive
Data use transparency	Substance abuse	TOI, May	Violation
	Substance abuse	TOI, July	Violation
	Substance abuse	Guardian, July	Violation
	Substance abuse	top 10, TOI, July	Violation
	Disability		Violation
	Mental disorder		Inconclusive
	Infertility		Inconclusive
Adult websites		Inconclusive	
Effectful choice	Opting out		Compliance
	Dating interest	May	Compliance
	Dating interest	July	Inconclusive
	Weight loss interest		Inconclusive
Ad choice	Dating interest		Compliance
	Weight loss interest	“fitness”	Inconclusive
	Weight loss interest	many keywords	Inconclusive

TABLE II. SUMMARY OF RESULTS TESTING PRIVACY PROPERTIES

of the browsing activities. A user can manually edit these demographics on their browser.

If the demographics on Google’s Ad Settings are left untouched, Google infers them over time. In the initial state, immediately after a DoubleClick cookie is placed on the browser, all the demographics are displayed as “unknown”. The page mentions that all demographics are inferred “based on websites you’ve visited”. As the user browses websites on the Internet, Google infers these demographics and displays them on this page. By giving users the option to edit these demographics, Google claims to have given choice to users about the ads Google serves them. So, the Ad Settings can function both as input to and output from Google. In our experiments, we explore these interactions.

B. Discrimination

AdFisher can demonstrate discrimination. If AdFisher finds a statistically significant difference in how Google treats two experimental groups, one consisting of members of the class in question and one whose members are not, then the experimenter has strong evidence that Google discriminates on that class. In particular, we use AdFisher’s ability to automatically select a test statistic to check for possible differences to test the null hypothesis that the two experimental groups have no differences in the ads they received.

For the treatments, we were interested in how providing information to Ad Settings would alter the ads seen. In particular, we explored whether Google’s reactions were appropriate. In a series of experiments, we set the gender of one group to female and the other to male. In one of the experiments, the agents went straight to collecting ads; in the others, they simulated an interest in jobs. In all but one experiment, they collected ads from the Times of India (TOI); in the exception, they collected ads from the Guardian. In one experiment, they also visited the top 10 websites for the U.S. according to Alexa.com to fill out

⁵<https://support.google.com/ads/answer/2662856?hl=en>

their interests.⁶

Table III summarizes the results. AdFisher found a statistically significant effect for the agents that first visited the job-related sites in May, 2014. We also found evidence of discrimination in the nature of the effect. In particular, in the gender and jobs experiment, we found that females received fewer instances of an ad encouraging the taking of high paying jobs than males. AdFisher did not find any statistically significant differences among the agents that did not visit the job-related pages or those operating in July, 2014. We detail the experiment finding a violation before discussing why we think the other results were insignificant.

Gender and Jobs. In this experiment, we examine how changing the gender demographic on Google Ad Settings affects the ads served and interests inferred for agents browsing employment related websites. We setup AdFisher to have the agents in one group visit the Google Ad Settings page and set the gender bit to female while agents in the other group set theirs to male. All the agents then visited the top 100 websites listed under the Employment category of Alexa.⁷ The agents then collected ads from the Times of India.

AdFisher ran 100 blocks of 10 agents each. (We used blocks of size 10 in all our experiments.) AdFisher used the ads of 900 agents (450 from each group) for training a classifier using the URL+title feature set. AdFisher used the remaining 100 agents' ads for testing. The learned classifier attained a test-accuracy of 93%, suggesting that Google did in fact treat the genders differently.

To test whether this response was statistically significant, AdFisher computed a p-value by running the permutation test on a million randomly selected block-respecting permutations of the data. The significance test yielded a p-value of $5.298 * 10^{-6}$.

We then examined the model learned by AdFisher to explain the nature of the difference. Table IV shows the five URL+title pairs that the model identifies as the strongest indication of being from the female or male group. How ads for identifying the two groups differ is concerning. The two URL+title pairs with the highest coefficients for indicating a male were for a career coaching service for "\$200k+" executive positions. Google showed the ads 1852 times to the male group but just 318 times to the female group. The top two URL+title pairs for the female group was for a generic job posting service and for an auto dealer.

We believe these results suggest the possibility of discrimination, intentional or otherwise. Encouraging only male candidates to seek high-paying jobs could further the current gender pay gap (e.g., [19]). Google or the advertiser might have purposely targeted just males. Alternatively, males might be more likely to click on an ad for a high-paying job than females, and Google may have picked up this signal from past interactions with other users. Google's policies allow it to serve different ads based on gender, but dissimilarities of this kind are concerning even if unintentional and allowed.

⁶<http://www.alexa.com/topsites/countries/US>, which lists google.com, facebook.com, youtube.com, yahoo.com, amazon.com, wikipedia.org, twitter.com, linkedin.com, ebay.com, and craigslist.org.

⁷<http://www.alexa.com/topsites/category/Top/Business/Employment>

The found discrimination in this experiment was predominately from a pair of job-related ads for the same service making the finding highly sensitive to changes in the serving of these ads. A closer examination of the ads from the same experimental setup ran in July showed that the frequency of these ads reduced from 2170 to just 48, with one of the ads completely disappearing. These 48 ads were only shown to males, continuing the pattern of discrimination. This pattern was recognized by the machine learning algorithm, which selected the ad as the second most useful for identifying males. (See Table V.) However, they were too infrequent to establish statistical significance. A longer running experiment with more blocks might have succeeded.

C. Transparency

AdFisher can demonstrate violations of individual data use transparency. AdFisher tests the null hypothesis that two groups of agents with the same ad settings receives ads from the same distribution despite being subjected to different experimental treatments. Rejecting the null hypothesis implies that some difference exists in the ads that is not documented by the ad settings.

In particular, we ran a series of experiments to examine how much transparency Google's Ad Settings provided. We checked whether visiting webpages associated with some interest could cause a change in the ads shown that is not reflected in the settings.

We ran such experiments for five interests: substance abuse, disabilities, infertility,⁸ mental disorders,⁹ and "adult" websites.¹⁰ Results from statistical analysis of whether the ads changed are shown in Table VI.

We examined the interests found in the settings for the two cases where we found a statistically significant difference in ads, substance abuse and disability. We found that settings did not change at all for substance abuse and changed in an unexpected manner for disabilities. Thus, we detail these two experiments below.

Substance Abuse. We were interested in whether Google's outputs would change in response to visiting webpages associated with substance abuse, a highly sensitive topic. Thus, we ran an experiment in which the experimental group visited such websites while the control group idled. Then, we collected the Ad Settings and the Google ads shown to the agents at the Times of India. For the webpages associated with substance abuse, we used the top 100 websites on the Alexa list for substance abuse.¹¹

AdFisher ran 100 blocks of 10 agents each. At the end of visiting the webpages associated with substance abuse, none of the 500 agents in the experimental group had interests listed on their Ad Settings pages. (None of the agents in the control group did either since the settings start out empty.) Thus,

⁸http://www.alexa.com/topsites/category/Top/Health/Reproductive_Health/Infertility

⁹http://www.alexa.com/topsites/category/Top/Health/Mental_Health/Disorders

¹⁰<http://www.alexa.com/topsites/category/Top/Adult>

¹¹http://www.alexa.com/topsites/category/Top/Health/Addictions/Substance_Abuse

Treatment	other visits	measurement	date	blocks	# ads (# unique ads)			accuracy	p-value	computation times	
					female	male	total			training	testing
Gender		TOI	May	100	20,137 (603)	20,263 (630)	40,400 (843)	48%	0.7714	25s	3m 22s
Gender	jobs	TOI	May	100	21,766 (545)	21,627 (533)	43,393 (736)	93%	0.000005298	23s	3m 26s
Gender	jobs	TOI	July	100	17,019 (673)	18,013 (690)	35,032 (954)	55%	0.2000	31s	3m 29s
Gender	jobs	Guardian	July	100	11,366 (410)	11,230 (408)	22,596 (581)	57%	0.1173	19s	3m 29s
Gender	jobs & top 10	TOI	July	100	14,507 (461)	14,231 (518)	28,738 (711)	56%	0.1448	23s	3m 24s

TABLE III. RESULTS FROM THE DISCRIMINATION EXPERIMENTS

Title	URL	Coefficient	appears in agents		total appearances	
			female	male	female	male
Top ads for identifying the simulated female group						
Jobs (Hiring Now)	www.jobsinyourarea.co	0.34	6	3	45	8
4Runner Parts Service	www.westernpatoyotaservice.com	0.281	6	2	36	5
Criminal Justice Program	www3.mc3.edu/Criminal+Justice	0.247	5	1	29	1
Goodwill - Hiring	goodwill.careerboutique.com	0.22	45	15	121	39
UMUC Cyber Training	www.umuc.edu/cybersecuritytraining	0.199	19	17	38	30
Top ads for identifying agents in the simulated male group						
\$200k+ Jobs - Execs Only	careerchange.com	-0.704	60	402	311	1816
Find Next \$200k+ Job	careerchange.com	-0.262	2	11	7	36
Become a Youth Counselor	www.youthcounseling.degreeleap.com	-0.253	0	45	0	310
CDL-A OTR Trucking Jobs	www.tadivers.com/OTRJobs	-0.149	0	1	0	8
Free Resume Templates	resume-templates.resume-now.com	-0.149	3	1	8	10

TABLE IV. TOP URL+TITLES FOR THE GENDER AND JOBS EXPERIMENT (MAY)

Title	URL	Coefficient	appears in agents		total appearances	
			male	female	male	female
Top ads for identifying the simulated male group						
Truck Driving Jobs	www.bestpayingdriverjobs.com	0.492	15	0	33	0
\$200k+ Jobs - Execs Only	careerchange.com	0.383	15	0	48	0
Aircraft Technician Program	pia.edu	0.292	6	0	14	0
Paid CDL Training	pamtransport.greatcdltraining.com	0.235	5	0	13	0
Unique Bridal Necklaces	margaretelizabeth.com/Bridal	0.234	5	0	19	0
Top ads for identifying agents in the simulated female group						
Business Loans for Women	topsbaloans.com	-0.334	1	13	1	70
Post Your Classified Ad	indeed.com/Post-Jobs	-0.267	16	20	24	56
American Laser Skincare	americanlaser.com	-0.243	5	8	8	14
Dedicated Drivers Needed	warrenttransport.com	-0.224	0	3	0	14
Earn Your Nursing Degree	nursing-colleges.courseadvisor.com	-0.219	3	11	10	31

TABLE V. TOP URL+TITLES FOR THE GENDER AND JOBS EXPERIMENT (JULY).

Treatment	Other visits	measurement	date	blocks	# ads (# unique ads)			accuracy	p-value	computation times	
					experimental	control	total			training	testing
Substance abuse		TOI	May	100	20,420 (427)	22,204 (530)	42,624 (673)	81%	0.000005298	18s	3m19s
Substance abuse		TOI	July	100	16,206 (653)	18,202 (814)	34,408 (1045)	98%	0.000005298	21s	3m29s
Substance abuse		Guardian	July	100	8,359 (242)	11,489 (319)	19,848 (387)	62%	0.007551	11s	3m26s
Substance abuse	top 10	TOI	July	100	15,713 (603)	16,828 (679)	32,541 (905)	65%	0.000005298	19s	3m19s
Disability		TOI	May	100	19,787 (546)	23,349 (684)	43,136 (954)	75%	0.000005298	21s	3m18s
Mental disorder		TOI	May	100	22,303 (407)	22,257 (465)	44,560 (600)	59%	0.0529655	31s	3m19s
Infertility		TOI	May	100	22,438 (605)	22,544 (625)	44,982 (856)	57%	0.113977	39s	3m23s
Adult websites		TOI	May	100	17,670 (602)	17,760 (580)	35,430 (850)	52%	0.416935	33s	3m28s

TABLE VI. RESULTS FROM TRANSPARENCY EXPERIMENTS

Fig. 4. Screenshot of an ad with the top URL+title for identifying agents that visited webpages associated with substance abuse

one might expect that Google did not factor the visits to the webpages into its ad selection algorithms.

However, the ads collected from the Times of India told a different story. The learned classifier attained a test-accuracy of 81%, suggesting that Google did in fact respond to the page visits. Indeed, using the permutation test, AdFisher found a p-value of 5.298×10^{-6} . Thus, we conclude that the differences are statistically significant: Google’s ads changed in response to visiting the webpages associated with substance abuse. Despite this change being significant, the Ad Settings pages provided no hint of its existence: the transparency tool is opaque!

We looked at the URL+title pairs with the highest coefficients for identifying the experimental group that visited the websites related to substance abuse. Table VII provides information on coefficients and URL+titles learned. The three highest were for “Watershed Rehab”. The top two had URLs for this drug and alcohol rehab center. The third lacked a URL and had other text in its place. Figure 4 shows one of Watershed’s ads. The experimental group saw these ads a total of 3309 times (16% of the ads); the control group never saw any of them nor contained any ads with the word “rehab” or “rehabilitation”. None of the top five URL+title pairs for identifying the control group had any discernible relationship with rehab or substance abuse.

These results remain robust across variations on this design with statistical significance in three variations. For example, two of these ads remain the top two ads for identifying the agents that visited the substance abuse websites in July using ads collected from the Guardian (Table VIII).

The fact that no interests are displayed on Google Ad Settings in this experiment despite Google’s ads differing for those possibly looking for rehab shows that the tool can be opaque. Users cannot see that this interest has been inferred or remove it. We cannot, however, determine whether this was an instance of deliberate targeting or by accident through some other feature of the visited webpages.

Disabilities. This experiment was nearly identical in setup but used websites related to disabilities instead of substance abuse. We used the top 100 websites on Alexa on the topic.¹²

For this experiment, AdFisher found a classifier with a test-accuracy of 75%. It found a statistically significant difference with a p-value of 5.298×10^{-6} .

Looking at the top ads for identifying agents that visited the webpages associated with disabilities, we see that the top two ads have the URL www.abilitiesexpo.com and the titles “Mobility Lifter” and “Standing Wheelchairs”. They were shown a total of 1076 times to the experimental group but never to the control group. (See Table IX.)

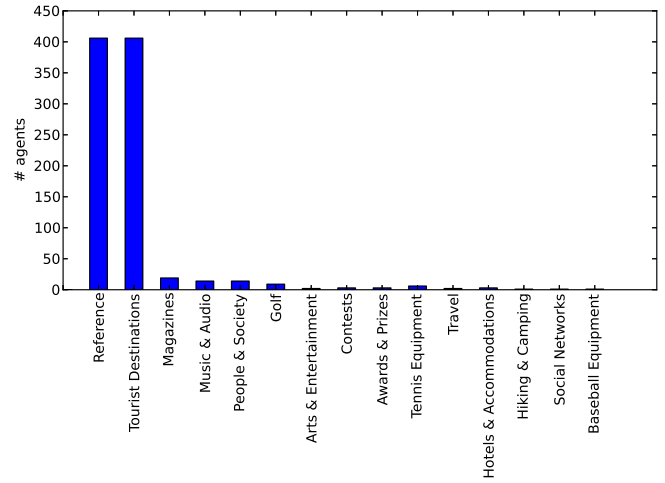


Fig. 5. For each interest selected for the agents that visited webpages associated with disabilities, the number of agents with that interest selected

This time, Google did change the settings in response to the agents visiting the websites. Figure 5 shows the interests selected for the experimental group. (The control group, which did nothing, had no interests selected.) None of them are directly related to disabilities suggesting that Google might have focused on other aspects of the visited pages. However, it remains difficult to explain the presence of the disability-related ads in just the experimental group.

D. *Effectful Choice*

We tested whether making changes to Ad Settings has an effect on the ads seen, thereby giving the users a degree of choice over the ads. In particular, AdFisher tests the null hypothesis that changing some ad setting has no effect on the ads.

First, we tested whether opting out of tracking actually had an effect by comparing the ads shown to agents that opted out after visiting car-related websites to ads from those that did not opt out. We found a statistically significant difference.

We also tested whether removing interests from the settings page actually had an effect. To do so, we set AdFisher to have both groups of agents simulate some interest. AdFisher then had the agents in one of the groups remove interests from Google’s Ad Settings related to the induced interest. We found statistically significant differences between the ads both groups collected from the Times of India for two induced interests: online dating and weight loss. Table X summarizes the results. We describe one in detail below.

Online Dating. We simulated an interest in online dating by visiting the website www.midsummerseve.com/, a website we choose since it sets Google’s ad setting for “Dating & Personals”. AdFisher then had just the agents in the experimental group remove the interest “Dating & Personals” (the only one containing the keyword “dating”). All the agents then collected ads from the Times of India.

AdFisher found statistically significant differences between the groups with a classifier accuracy of 74% and a p-value of 5.298×10^{-6} .

¹²<http://www.alex.com/topsites/category/Top/Society/Disabled>

Title	URL	Coefficient	appears in agents		total appearances	
			control	experi.	control	experi.
Top ads for identifying agents in the experimental group (visited websites associated with substance abuse)						
The Watershed Rehab	www.thewatershed.com/Help	-0.888	0	280	0	2276
Watershed Rehab	www.thewatershed.com/Rehab	-0.670	0	51	0	362
The Watershed Rehab	Ads by Google	-0.463	0	258	0	771
Veteran Home Loans	www.vamortgagecenter.com	-0.414	13	15	22	33
CAD Paper Rolls	paper-roll.net/Cad-Paper	-0.405	0	4	0	21
Top ads for identifying agents in control group						
Alluria Alert	www.bestbeautybrand.com	0.489	2	0	9	0
Best Dividend Stocks	dividends.wyattresearch.com	0.431	20	10	54	24
10 Stocks to Hold Forever	www.streetauthority.com	0.428	51	44	118	76
Delivery Drivers Wanted	get.lyft.com/drive	0.362	22	6	54	14
VA Home Loans Start Here	www.vamortgagecenter.com	0.354	23	6	41	9

TABLE VII. TOP URL+TITLES FOR SUBSTANCE ABUSE EXPERIMENT IN MAY ON THE TIMES OF INDIA

Title	URL	Coefficient	appears in agents		total appearances	
			control	experi.	control	experi.
Top ads for identifying agents in the experimental group (visited websites associated with substance abuse)						
The Watershed Rehab	www.thewatershed.com/Help	-0.626	0	231	0	1847
Watershed Rehab	www.thewatershed.com/Rehab	-0.207	0	12	0	52
Generator Sets Parts	www.mtspowerproducts.com	-0.152	0	1	0	10
The AntiChrist: Free Book	voiceofelijah.org/Rapture	-0.148	1	11	6	17
Israel at War	www.joelrosenberg.com	-0.148	1	4	1	19
Top ads for identifying agents in control group						
The Sound of Dear Voices	www.telephonebangladesh.com	0.243	21	3	59	5
5-15 Day Ireland Vacation	www.exploringvacations.com/Ireland	0.233	21	7	109	18
#1 Best Selling Blocker	plugnblock.com/Sentry-Call-Blocker	0.207	37	22	87	33
Dow Average Over 17,000	economyandmarkets.com	0.204	14	1	34	2
Block Annoying Phone Call	plugnblock.com/Sentry-Call-Blocker	0.176	27	18	62	25

TABLE VIII. TOP URL+TITLES FOR SUBSTANCE ABUSE EXPERIMENT IN JULY ON THE GUARDIAN

Title	URL	Coefficient	appears in agents		total appearances	
			control	experi.	control	experi.
Top ads for identifying agents in the experimental group (visited websites associated with disability)						
Mobility Lifter	www.abilitiesexpo.com	-1.543	0	84	0	568
Standing Wheelchairs	www.abilitiesexpo.com	-1.425	0	88	0	508
Smoking MN Healthcare	www.stillaproblem.com	-1.415	0	24	0	60
Bike Prices	www.bikesdirect.com	-1.299	0	24	0	79
\$19 Car Insurance - New	auto-insurance.quotelab.com/MN	-1.276	0	6	0	9
Top ads for identifying agents in control group						
Beautiful Women in Kiev	anastasiadate.com	1.304	190	46	533	116
Melucci DDS	AdsbyGoogle	1.255	4	2	10	6
17.2% 2013 Annuity Return	advisorworld.com/CompareAnnuities	1.189	30	5	46	6
3 Exercises To Never Do	homeworkoutrevolution.net	1.16	1	1	3	1
Find CNA Schools Near You	cna-degrees.courseadvisor.com	1.05	22	0	49	0

TABLE IX. TOP URL+TITLES FOR DISABILITY EXPERIMENT FROM THE TIMES OF INDIA IN MAY

Experiment	blocks	# ads (# unique ads)			accuracy	p-value	computation times	
		removed/opt-out	keep/opt-in	total			training	testing
Opting out	54	9,029 (139)	9,056 (293)	18,085 (366)	83%	0.000005298	4s	2m10s
Dating (May)	100	17,975 (518)	17,762 (457)	35,737 (669)	74%	0.000005298	13s	3m27s
Dating (July)	90	11,657 (727)	11,256 (706)	22,913 (1,014)	59%	0.07042	24s	3m20s
Weight Loss	83	15,826 (367)	15,449 (427)	31,275 (548)	60%	0.040948	16s	3m04s

TABLE X. RESULTS FROM EFFECTFUL CHOICE EXPERIMENTS

Furthermore, the effect appears related to the interests removed. The top ad for identifying agents that kept the romantic interests has the title “Are You Single?” and the second ad’s title is “Why can’t I find a date?”. None of the top five for the control group that removed the interests were related to dating (Table XI). Thus, the ad settings appear to actually give users the ability to avoid ads they might dislike or find embarrassing. In the next set of experiments, we explicitly test for this ability.

We repeated this experiment in July using the websites relationshipsurgery.com and datemypet.com, which also have an effect on Ad Settings, but did not find statistically significant differences.

E. Ad Choice

Whereas the other experiments tested merely for the presence of an effect, testing for ad choice requires determining whether the effect is an increase or decrease in the number of ads seen. Fortunately, since AdFisher uses a one-sided permutation test, it tests for either an increase or a decrease, but not for both simultaneously, making it usable for this purpose. In particular, after removing an interest, we check for a decrease to test for compliance using the null hypothesis that either no change or an increase occurred, since rejecting this hypothesis would imply that a decrease in the number of related ads occurred. To check for a violation, we test the null hypothesis that either no change or a decrease occurred.

Due to testing two hypotheses, we use an adjustment to the p-value cutoff considered significant to avoid finding significant results simply from testing multiple hypotheses. In particular, we use the standard Bonferroni correction, which calls for a p-value of $0.5/2 = 0.025$ or less (e.g., [20]).

We ran three experiments checking for ad choice. The experiments followed the same setup as the effectful choice ones, but this time we used all the blocks for testing a given test statistic. The test statistic counted the number of ads containing keywords. In the first, we again test online dating using relationshipsurgery.com and datemypet.com. In the next two we tested weight loss websites again but using two different sets of keywords. Table XII summarizes the results.

In particular, we found that removing online dating did in fact decrease the number of ads containing related keywords. We detail the inconclusive results for weight loss below.

Weight Loss. We induced an interest in weight loss by visiting dietingsucks.blogspot.com. Afterwards, the agents in the experimental group removed the interests “Fitness” and “Fitness Equipment and Accessories”, the only ones related to weight loss. We then used a test statistic that counted the number of ads containing the keyword “fitness”. Interestingly, the test statistic was higher on the group with the interests removed, although not to a statistically significant degree. We repeated the process with a longer keyword list and found that removing interests decreased test statistic this time, but also not to a statistically significant degree.

VII. CONCLUSIONS

Using AdFisher, we conducted 20 experiments using 16,570 agents and that collected 570,000 ads. Our experiments

found the presence of discrimination, opacity, and choice in targeted ads of Google. Discrimination, is at some level, inherent to profiling: the point of profiling is to treat some people differently. While customization can be helpful, we highlight a case where the customization appears inappropriate taking on the negative connotations of discrimination. In particular, we found that males were shown ads encouraging the seeking of high paying jobs more than females (Section VI-B). We know of no justification for such customization.

Opacity occurs when a tool designed to provide transparency into how ads are selected and the profile kept on a person actually fails to provide such transparency. Our experiment on substance abuse showed an extreme case in which the tool failed to show any profiling but the ad distributions were significantly different in response to behavior (Section VI-C). In particular, our experiment achieved a p-value of 5.298×10^{-6} — 9000 times more significant than the standard 0.05 cutoff for statistical significance. This experiment remained robust to variations showing a pattern of such opacity.

Ideally, tools, such as Ad Settings, would provide a complete representation of the profile kept on a person, or at least the portion of the profile that is used to select ads shown to the person. Two people with identical profiles might continue to receive different ads due to other factors affecting the choice of ads such as A/B testing or the time of day. However, systematic differences between ads shown at the same time and in the same context, such as those we found, would not exist for such pairs of people.

Lastly, we found that Google Ad Settings does provide the user with a degree of choice about the ads shown. In this aspect, the transparency tool operated as we expected. In at least some cases, removing interests from the settings behaved in a manner consistent with expectations and removed ads related to the removed interests.

Our work has made it possible to automate these experiments. Our tool, AdFisher, makes it easy to run additional experiments exploring the relations between Google’s ads and settings. It can be extended to study other systems. It’s design ensures that it can run and analyze large scale experiments to find subtle differences. It automatically finds differences between large data sets produced by different groups of agents and explains the nature of those differences. By completely automating the data analysis, we ensure that an appropriate statistical analysis determines whether these differences are statistically significant, ensuring sound conclusions.

VIII. FUTURE WORK

Some future work would be extensions of AdFisher. For example, enabling the analysis of image ads or the study of other Google services such as Gmail.

Other future work is more conceptual. For example, ideally we would like to know whether Google profiles people with substance abuse issues in violation of their policy promising not to market based on health information. However, our experiments check whether going to websites associated with substance abuse affects the ads Google shows. These two concerns diverge in two ways.

Title	URL	Coefficient	appears in agents		total appearances	
			kept	removed	kept	removed
Top ads for identifying the group that kept dating interests						
Are You Single?	www.zoosk.com/Dating	1.583	367	33	2433	78
Top 5 Online Dating Sites	www.consumer-rankings.com/Dating	1.109	116	10	408	13
Why can't I find a date?	www.gk2gk.com	0.935	18	3	51	5
Latest Breaking News	www.onlineinsider.com	0.624	2	1	6	1
Gorgeous Russian Ladies	anastasiadate.com	0.620	11	0	21	0
Top ads for identifying agents in the group that removed dating interests						
Car Loans w/ Bad Credit	www.car.com/Bad-Credit-Car-Loan	-1.113	5	13	8	37
Individual Health Plans	www.individualhealthquotes.com	-0.831	7	9	21	46
Crazy New Obama Tax	www.endofamerica.com	-0.722	19	31	22	51
Atrial Fibrillation Guide	www.johnshopkinshealthalerts.com	-0.641	0	6	0	25
Free \$5 - \$25 Gift Cards	swagbucks.com	-0.614	4	11	5	32

TABLE XI. TOP URL+TITLES FOR ONLINE DATING EXPERIMENT ON EFFECTFUL CHOICE

Experiment	Blocks	Keywords	# ads (# unique ads)			p-value	flip p-value	appearances	
			removed	kept	total			removed	kept
Dating	10	dating, romance, relationship	952 (117)	994 (123)	1,946 (181)	0.007590	0.9970	34	109
Weight Loss	10	fitness	1,461 (259)	1,401 (240)	2,862 (384)	0.7161	0.3818	21	16
Weight Loss	10	fitness, health, fat, diet, exercise	1,803 (199)	1,478 (192)	3,281 (293)	0.1792	0.9371	2	15

TABLE XII. RESULTS FROM AD CHOICE EXPERIMENTS. THE SAME KEYWORDS ARE USED TO REMOVE AD INTERESTS, AS WELL AS CREATE THE TEST STATISTIC FOR PERMUTATION TEST.

First, it is possible that Google profiling did not cause the changes; profiling by the advertisers working with Google could be the cause. Such advertisers may have found combinations of interests to bid on that allows them to target those with substance abuse problems without Google attempting to enable such targeting. Before we can assign blame, we must have a better understanding of the interactions between Google and the advertisers. Nevertheless, these results are useful in that web advertising companies can use AdFisher for internal auditing of their systems, whose complexity, scale, and dynamic models put them beyond the current capabilities of code analysis. Our results offer regulators the evidence needed to investigate companies. They also increase public awareness. Knowing of the existence of these concerning behaviors lets regulators and the public hold companies accountable.

Second, we cannot send a pure signal to Google that a person has substance abuse issues. We must simulate the behavior of such a person by visiting webpages we believe someone with a substance abuse issue might visit. However, not only might these pages do a poor job simulating such a person, but they also contain confounding factors, such as content unrelated to substance abuse. Thus, while we can soundly say visiting those webpages had an effect, we cannot assign that cause to substance abuse or explain what content on those pages caused the effect. We would like to find ways of controlling for these confounding factors.

For these reasons, we cannot claim that Google has violated its policies. In fact, we consider it more likely that Google has lost control over its massive, automated profiling system. Large scale machine learning can behave in unexpected ways. With this in mind, we hope future research will examine how to produce machine learning algorithms that automatically avoid discriminating against users in unacceptable ways and

automatically provide transparency to users. We suspect our tool could be used to test or even guide such algorithms.

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APPENDIX

A. Details of Methodology

Let the units be arranged in a vector \vec{u} of length n . Let \vec{t} be a *treatment vector*, a vector of length n whose entries are the treatments that the experimenter wants to apply to the units. In the case of just two treatments, \vec{t} can be half full of the first treatment and half full of the second. Let a be an *assignment* of units to treatments, a bijection that maps each entry of \vec{u} to an entry in \vec{t} . That is, an assignment is a permutation on the set of indices of \vec{u} and \vec{t} .

The result of the experiment is a vector of observations \vec{y} where the i th entry of \vec{y} is the response measured for the unit assigned to the i th treatment in \vec{t} by the assignment used. In a randomized experiment, such as those AdFisher runs, the actual assignment used is selected at random uniformly over some set of possible assignments \mathcal{A} .

Let s be a test statistic of the observations of the units. That is $s : \mathcal{Y}^n \rightarrow \mathcal{R}$ where \mathcal{Y} is the set of possible observations made over units, n is the number of units, and \mathcal{R} is the range of s . We require \mathcal{R} to be ordered numbers such as the natural or real numbers. We allow s to treat its arguments differently,

that is, the order in which the observations are passed to s matters.

If the null hypothesis is true, then we would expect the value of s to be the same under every permutation of the arguments that swaps only exchangeable units since the assignment of units to treatments should not matter under the null hypothesis. This reasoning motivates the permutation test. The value produced by a (one-tailed signed) permutation test given observed responses \vec{y} and a test statistic s is

$$\frac{|\{a \in \mathcal{A} \mid s(\vec{y}) \leq s(a(\vec{y}))\}|}{|\mathcal{A}|} = \frac{1}{|\mathcal{A}|} \sum_{a \in \mathcal{A}} I[s(\vec{y}) \leq s(a(\vec{y}))] \quad (1)$$

where the assignments in \mathcal{A} only swaps exchangeable units and $I[\cdot]$ returns 1 if its argument is true and 0 otherwise.

Blocking. For the blocking design, the set of units \mathcal{U} is partitioned into k blocks \mathcal{B}_1 to \mathcal{B}_k . In our case, all the blocks have the same size. Let $|\mathcal{B}_i| = m$ for all i . The set of assignments \mathcal{A} is equal to the set of functions from \mathcal{U} to \mathcal{U} that are permutations not mixing up blocks. That is, a such that for all i and all u in \mathcal{B}_i , $a(u) \in \mathcal{B}_i$. Thus, we may treat \mathcal{A} as k permutations, one for each \mathcal{B}_i . Thus, \mathcal{A} is isomorphic to $\times_{i=1}^k \Pi(\mathcal{B}_i)$ where $\Pi(\mathcal{B}_i)$ is the set of all permutations over \mathcal{B}_i . Thus, $|\times_{i=1}^k \Pi(\mathcal{B}_i)| = (m!)^k$. Thus, (1) can be computed as

$$\frac{1}{(m!)^k} \sum_{a \in \times_{i=1}^k \Pi(\mathcal{B}_i)} I[s(\vec{y}) \leq s(a(\vec{y}))] \quad (2)$$

Sampling. Computing (2) can be difficult when the set of considered arrangements is large. One solution is to randomly sample from the assignments \mathcal{A} . Let \mathcal{A}' be a random subset of \mathcal{A} . We then use the approximation

$$\frac{1}{|\mathcal{A}'|} \sum_{a \in \mathcal{A}'} I[s(\vec{y}) \leq s(a(\vec{y}))] \quad (3)$$

Confidence Intervals. Let \hat{P} be this approximation and p be the true value of (2). p can be understood as the frequency of arrangements that yield large values of the test statistic where *largeness* is determined to be at least as large as the observed value $s(\vec{y})$. That is, the probability that a randomly selected arrangement will yield a large value is p . \hat{P} is the frequency of seeing large values in the $|\mathcal{A}'|$ sampled arrangements. Since the arrangements in the sample were drawn uniformly at random from \mathcal{A} and each draw has probability p of being large, the number of large values will obey the binomial distribution. Let us denote this value as L , and $|\mathcal{A}'|$ as n . Since $\hat{P} = L/n$, $\hat{p} * n$ also obeys the binomial distribution. Thus,

$$\Pr[\hat{P} = \hat{p} \mid n, p] = \binom{n}{\hat{p}n} p^{\hat{p}n} (1-p)^{(1-\hat{p})n} \quad (4)$$

Thus, we may use a binomial proportion confidence interval. We use the Clopper-Pearson interval [21].

Test Statistic. The statistic we use is based on a classifier c . Let $c(y_i) = 1$ mean that c classifies the i th observation as having come from the experimental group and $c(y_i) = 0$ as

from the control group. Let $\neg(0) = 1$ and $\neg(1) = 0$. Let \vec{y} be ordered so that all of the experimental group comes first. The statistic we use is

$$s(\vec{y}) = \sum_{i=1}^{n/2} c(y_i) + \sum_{i=n/2+1}^n \neg c(y_i) \quad (5)$$

This is the number correctly classified.

B. Parameters

To run variants of experiments on AdFisher, the experimenter can set several parameters:

Blocks: This specifies the number of blocks in the experiment. The default value is 20. For most of our experiments, we used 100 blocks.

Agents: The experimenter can change the number of browser agents launched in each block by modifying this parameter. The default value is 2. We used 10 browser agents for our experiments.

Browser: AdFisher can run experiments on Firefox or Google Chrome. We carried out our experiments using Firefox.

Timeout: Although rare, at times, browser agents get stuck while performing some actions, which can stall the remainder of an experiment. An experimenter can specify a timeout period for a block. If timed out, all existing agents in a block are killed, and the next block is started.