Being in the Right Place: A Natural Field Experiment on the Causes of Position Effects in Individual Choice

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Abstract

This paper uses a natural field experiment to better understand the reasons why individuals show a disproportionate tendency to select items placed in the top position of a list. After randomizing the order in which new economics research papers are presented in email alerts and tracking economists' subsequent download activity, we provide evidence of position effects and reject three common explanations regarding item order, choice fatigue and quality signals. Instead, after developing some novel tests based on the user-level nature of our data, we show that three more subtle explanations are consistent with the behavior of different groups of individuals.

Keywords: Position Effects; Order Effects; Choice Fatigue; Prominence; Lists

JEL Codes: D01, D03, C93, L00

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1 Introduction

Whether comparing options from search results, choosing products from websites, considering employment opportunities from job listings, or searching for suppliers from a directory, individuals frequently make choices from lists. It is well known that when faced with such lists, individuals often show a disproportionate tendency to select items that are placed at the top. This is evident from the findings from a broad range of academic studies, as well as from the large expenditures that firms pay for sponsored links and the recent antitrust cases regarding the alleged bias within Google's search results.[1] For example, as later reviewed, the literature has shown that demand increases markedly for firms at the top of search results, investors trade more frequently with stocks at the top of investment listings, consumers are more likely to select items at the top of a menu, and voters are more inclined to choose candidates at the top of ballots. However, the explanations for such choice-based 'top position effects' or 'primacy effects' remain far less clear. Are top-placed options more likely to be selected simply because the higher quality options have been placed in top positions and if not, why might individuals show a systematic tendency to select options in top position?

Insights into these questions would help understand a variety of important issues across many active areas of economics. For instance, such insights would help understand the potential for policy to assist individuals in selecting beneficial options, such as more suitable savings and insurance plans or healthier foods (e.g. Dayan and Bar-Hillel 2011). Alternatively, as recently reviewed by Armstrong (2017), such insights could help analyze the extent to which firms can manipulate consumers' choices through the presentation of their product ranges (e.g. Petrikaitė 2017), the incentives for suppliers to compete for the top positions within search engines and directories, and a variety of broader issues regarding the design and effects of such platforms (e.g. Athey and Ellison 2010, McDevitt 2014, de Cornière and Taylor 2014).

To help address these issues, this paper analyzes the causes of top position effects by using a natural field experiment with a group of subjects that should be the least likely to depart from standard theory - economists. Economists often make their research papers available on a well-known online database, Research Papers in Economics (RePEc). Many economists also choose to be kept informed of recent additions to the database by subscribing to a free email alert service conducted by New Economic Papers (NEP) which regularly compiles lists of new papers. After randomizing the order in which items are presented within such lists and measuring users' subsequent download activity, this paper offers an excellent environment to cleanly measure and assess the causes of top position effects. The paper largely rejects three common explanations regarding item order, choice fatigue and position as a quality signal. Instead, after exploiting the user-level nature of our data, we highlight the heterogeneity of explanations by showing how three more subtle explanations are consistent with the behavior of different groups of users.

The first part of the paper estimates how list position influences users' download decisions and shows strong evidence of top position effects. As well as controlling for observable features of the lists and papers, the estimations also allow for two levels of random effects to control for unobservable user characteristics and paper characteristics.

[1] See `https://www.nytimes.com/2017/06/27/technology/eu-google-fine.html`, accessed 01/04/18.

By analyzing these results, we first assess a common explanation based simply on the order in which the items are presented. Under H1 (Specific Item Order), top position effects arise only because an item with a relatively high value happens to be in top position. However, inconsistent with a pure explanation of H1, highly significant top position effects remain, albeit at a smaller magnitude, even when the order of items is deliberately randomized as part of the experiment.

We then use our estimations to evaluate two other common explanations. Under H2 (Value Signals), users cannot fully assess the quality of items but are more likely to select the item in top position because they expect (perhaps incorrectly) that the items have been arranged in descending order of value. Alternatively, under H3 (Choice Fatigue), top position effects occur because the costs of evaluating or selecting an item are increasing from top position downwards, as consistent with users who consider the items from the top downwards and have total costs of effort that are convex. Both H2 and H3 imply that download activity should be weakly decreasing from the top to the bottom of the list. Yet, we find that items in bottom position are significantly more likely to be downloaded than average and significantly more likely to be downloaded than the items in the position immediately above them, such that the data is characterized by both top position effects and some relatively smaller, 'bottom position' or 'recency' effects. This contradicts H2 and H3, and rules out the possibility that top position effects exist in the data simply because NEP usually sorts its listed items in descending order of estimated value.

Consequently, the paper rejects the possibility that the common hypotheses H1-H3 can offer a major explanation for top position effects. To help provide a better understanding, the paper then tests three other, more subtle, explanations.

Unlike Choice Fatigue (H3), Choice Fatigue with Heterogeneous Direction (H4) explains the simultaneous existence of top and bottom position effects by suggesting that the two effects derive from two different groups of users - one group who always make their download decisions in a descending order from top downwards, and another group who always make their download decisions in an ascending order. Experimental evidence for such ascending decisions is provided by Caplin et al (2011) and is consistent with users reading the items in descending order before making their selection decisions from the bottom up. Under this hypothesis, some users' position effects should always be decreasing from top position downwards while other users' position effects should always be increasing from bottom position upwards. However, by conducting a set of random parameter estimations that allow the estimated position effects to vary across users, we show that only 1-2% of users exhibit position effects that monotonically increase or decrease with position. Instead, in contrast to H4, 72% of users display *both* top and bottom position effects, with position effects that decrease with position until bottom position.

Hence, unlike H4, our next two explanations do not attribute top and bottom position effects to different groups of users. Under Choice Fatigue with Mixed Direction (H5), each user varies between inspecting lists in a descending and ascending order such that they exhibit top position effects on some occasions, but bottom position effects on others. Alternatively, under Non-Monotonic Download Costs (H6), users exhibit top and bottom position effects because such items are relatively prominent or salient as consistent with the following two examples. Under H6a, as theoretically analyzed in a related setting by Fishman and Lubensky (2017), users make their initial download decisions in a descending direction (such that higher-placed items are initially more prominent), but then return up the list to reconsider some items (such that bottom placed items are then relatively prominent). Under H6b, users replace fully rational decision rules with heuristics to economize on cognitive resources, and such heuristics make items in top and bottom position appear more salient (Salant 2011).

In line with the predictions of both H5 and H6, we show that top position effects become relatively larger in longer lists. However, to further test these hypotheses, we exploit a useful feature of our data which records the exact time that each download was made (to the nearest second). Specifically, we use this information to recover the order in which users made their downloads in instances where they selected multiple items from a list. As particularly consistent with the predictions of Choice Fatigue with Heterogeneous Direction (H4) and Choice Fatigue with Mixed Direction (H5), we show that such items are downloaded in a monotonic descending order in 67% of such instances, and in a monotonic ascending order in 3-6% of such instances. However, as more in line with Non-Monotonic Download Costs (H6), we also show that 27-30% of instances exhibit a non-monotonic download behavior where, for example, a user makes their multiple downloads in a descending then ascending direction.

As a more accurate test of the explanations, we then consider cases where we observe the same user downloading multiple items from different lists. Here, we find that 40-43% of users 'always' download their items in a strict descending order, 0-2% of users 'always' download their items in a strict ascending order, and 2-3% of users 'always' download their items in a monotonic order with varied directions. This gives only limited support for H4 and H5 respectively, due to the relatively low percentages of users that i) download their items in ascending, rather than descending, direction, or ii) employ varied directions. Instead, this suggests that a substantial explanation rests with the 52-58% of users that do not always download their items in a monotonic order. As more consistent with Non-Monotonic Download Costs (H6), we show that such users typically download their selected items in a non-monotonic order 49-54% of the time.

Hence, in summary, our results reject the common explanations H1-H3 as major explanations for top position effects, and point to the relevance of three additional explanations, H4-H6, for different user groups, with an especially large role for Non-Monotonic Download Costs (H6).

The paper continues as follows. After reviewing the existing literature, Section 2 discusses the NEP email alert service, the experimental procedures and the data. Section 3 introduces the initial analysis by detailing H1-H3, outlining two empirical tests, and presenting some initial results. Section 4 then outlines the further analysis by outlining H4-H6 and the results of two additional tests. Section 5 concludes. All tables and figures are included in Appendix B unless otherwise stated.

Previous Literature: The existence of top (and bottom) position effects has been previously well documented in a variety of contexts, but our paper focuses on position effects in individual choice from visually presented lists.[2] Moreover, in addition to carefully documenting the existence of such position effects, we differ from much of the previous literature by testing between different explanations.

[2]Other contexts include how individuals i) use lists of evidence to form impressions or judgments (e.g. Asch 1946), ii) evaluate between alternatives in contests or product sampling tests (e.g. Haan et al 2005; Biswas et al 2010), iii) choose responses in surveys (e.g. Schwarz et al 1992) and iv) recall items in memory tasks (e.g. Tan and Ward 2000).

As an indication of the broad importance of position effects, many previous studies come from outside economics. Hence, we now provide a relatively detailed review, and classify studies into two settings: i) limited selection settings, and ii) unlimited selection settings.

i) Limited Selection Settings: In this setting, individuals may only select one item (or some other fixed number of items) from a list. This is the most common setting for studying top position effects within individual choice, but differs from our 'unlimited selection' setting where individuals are not inherently constrained in the number of items they are willing or able to select.

Many 'market' studies use data from online search results to provide evidence of top position effects. As such search results often place the most relevant items first, researchers must employ some method to rule out a simple explanation of Specific Item Order (H1). To do this, studies often use a variety of econometric techniques.[3] However, an exception is Ursu (2017) who randomizes the order of search results at an online travel agent to show that position effects are significant but lower than typically estimated. Murphy et al (2006) and Dayan and Bar-Hillel (2011) also use randomization in a different setting within restaurant websites and menus. Unlike the papers on search results, they also test for, and provide evidence of, bottom position effects. However, contrary to our paper, none of these market studies focus on testing different explanations of position effects.

Other studies show how voters tend to select the candidate placed at the top of a ballot.[4] As legislation often requires ballot orders to be (quasi-) random, these results cannot be explained by specific item order (H1). Instead, most papers jump to an explanation of satisficing (Simon 1955) where individuals consider items sequentially from the top downwards, face marginal inspection costs for each item, and optimally stop to select an item that is sufficiently attractive. However, by exploiting some features of multi-winner elections, this explanation is rejected by Meredith and Salant (2013). Augenblick and Nicholson (2012) consider a different setting where voters have to vote on multiple different contests within the same ballot. As consistent with voters depleting their cognitive resources as they work down the ballot paper, they show that voters become more likely i) to abstain, ii) vote for the default option, or iii) display a bias towards candidates listed first. Augenblick and Nicholson refer to this as 'choice fatigue'. In contrast, we use a variety of novel tests to analyze some different forms of choice fatigue as explanations for top position effects within our alternative context.

ii) Unlimited Selection Settings: Within this setting i) there is no inherent constraint on the number of items an individual can select, and ii) the items are sufficiently non-substitutable that individuals often wish to select multiple items. In addition to our download environment, other examples include choosing stock options from investment listings, browsing amongst different items on a website, or selecting items from a bestsellers list.

Within this setting, some work has found top position effects in financial contexts. For instance, as lists of stocks are often presented in alphabetical order, Itzkowitz et al (2016) and Jacobs and Hillert (2016) show that firms with earlier names have higher trading activity even after extensive controls.

[3] E.g. Ansari and Mela (2003), Narayanam and Kalyanam (2015), Baye et al (2016a, 2016b), de los Santos and Koulayev (2016).

[4] E.g. Miller and Krosnick (1998), Koppell and Steen (2004), Ho and Imai (2008), Meredith and Salant (2013), Kim et al (2015).

Other work focuses on academic settings. Pinkowitz (2002) and Coupe et al (2010) use clever strategies to show how some top position effects arise from Specific Item Order (H1). Pinkowitz (2002) uses data from the Journal of Finance website where individuals can download fully published papers and accepted papers that have yet to be allocated to an issue. As consistent with H1, papers that are later allocated a top position receive significantly more downloads before being assigned their position. However, as consistent with other explanations, such papers also receive an additional download effect after being listed first. Alternatively, Coupe et al (2010) show top position effects exist within issues of the European Economic Review even when the order of papers is determined alphabetically rather than by the editor. Closest to our research is the excellent paper by Feenberg et al (2017) who use the random ordering of NBER paper alerts to show top and bottom position effects in individuals' download and citation activity. Among other results, they find that such effects increase in longer lists, but weaken in the summer when individuals are less busy. They suggest that the most consistent explanation is 'skimming' where, similar to our H6, time-constrained individuals focus on salient positions such as top and bottom. In contrast, while our NEP alerts have a lower readership than NBER alerts and are therefore less influential on citation activity, our data contains dis-aggregate information on download decisions at the *user-level*. This allows us to take a different approach by analyzing the timing of downloads and by employing random parameter techniques in order to i) test between competing hypotheses more precisely, and ii) explicitly allow for heterogeneity in explanations across users.

Finally, while not focusing specifically on position effects, two papers provide related results on search and choice behavior.[5] First, by using data on consumers that click on more than one online search result, Jeziorski and Segal (2015) demonstrate that less than half of such consumers make their clicks in a monotonic descending order. Second, within a search-theoretic laboratory experiment, Caplin et al (2011) provide support for satisficing - stopping search after having discovered a sufficiently attractive listed item - but show that i) some subjects inspect items in ascending rather than descending order, and ii) some subjects who usually search from top to bottom behave differently when faced with more complex items. Within our different setting, we also find some related patterns of behavior, and use such evidence to analyze the causes of top position effects.

2 Setting, Experiment, and Data

2.1 RePEc and NEP

Research Papers in Economics (RePEc) is a popular online database of economics research papers. As part of RePEc, New Economics Papers (NEP) offers a free email alert service to notify individuals about new papers that have been recently added to the RePEc database. Such alerts are often provided on a weekly basis and are generated for separate research subfields, such as health economics or monetary economics. Subscribers can select which subfields they wish to subscribe to and NEP has well over 75000 total subscriptions.[6]

[5] Tests of standard search theory, such as de los Santos et al (2012), are not so relevant here as they do not consider settings with pre-defined lists. However, they do find that individuals often go back to select a previously searched option.

[6] For more, see `http://nep.repec.org/`, accessed 01/04/18.

Each email alert has two sections of text. An extract from an example alert is provided in Appendix A. The top section states how many papers are included in the alert and presents a brief list of the papers with their titles and authors. If a reader clicks on the title of any paper within the list, or scrolls down, she is taken to the bottom section of the alert. The bottom section repeats the same list of papers but with additional summary information including each paper's abstract, keywords, JEL classification codes, date (if these are available) and most importantly, a link to a full text version of each paper. By clicking on a paper's link, a new window is opened and the paper is downloaded.[7]

The alerts for each subfield are managed by an editor, who is a volunteer from academia or the public sector. Although never made explicit to subscribers, the list of papers within each alert is compiled as follows. First, NEP gathers a master list of all new papers that have been recently added to the RePEc database. An algorithm then uses past data together with information about each paper's title and abstract to arrange the papers into descending order of estimated popularity. This master list is then passed to the subfield editors for them to extract the papers that are relevant for their next subfield alert. After selecting their relevant papers, each editor is free to amend the order in which the papers are presented within their alert or leave them in the order suggested by the algorithm. Most editors amend the order of their lists with the intention of further improving upon the algorithm's attempts to put the more interesting and relevant papers towards the top.

As later discussed in more detail, papers can be selected to be in the alert of more than one subfield. Therefore, to avoid confusion, we will now make a distinction between 'papers' and 'items'. An item will refer to an entry on a specific alert, whereas a paper will refer to the underlying piece of research that can appear as an item in multiple subfield alerts. For ease of exposition, we will also refer to 'alerts' and 'lists' interchangeably.

RePEc measures the download activity for each item in an extremely precise manner. First, it measures downloads that occur specifically via the links contained within NEP alerts, not just those that occur through RePEc more generally. Second, in cases where a paper appears in multiple subfield alerts, RePEc records the downloads within each separate alert. Hence, the measurement of downloads is item-specific, not paper-specific, such that the relationship between list position and subsequent download activity can be analyzed in a meaningful manner. Finally, for each download, RePEC records the individual device (anonymized ip address) to which the download was made, and the time at which the download was initiated (to the nearest second).

2.2 Experimental Procedure

After requesting permission from NEP, we were granted access to the download data for the alerts released over a 5-month period across 29 subfields.[8] Moreover, to explore position effects in more

[7]Given the importance of bottom position effects within our later analysis, one may ask whether they users are artificially drawn to the bottom item via the two-section design of the alerts. However, this is not the case. When inspecting the summary information of the top item in the lower section of the alert, the bottom item within the upper section of the alert is off-screen. Hence, such a user would have to deliberately scroll upwards in order to see the bottom item.

[8]The 29 subfields appear representative and cover a wide range of different areas of economics: Africa, Ageing, Agricultural, Cognitive and Behavioural, Collective Decision Making, Computational Economics, Dynamic General Equilibrium, Education, Efficiency and Productivity, Time Series, Experimental, Forecasting, Happiness, Health, History and Philoso-

detail, we were given permission to manipulate the order in which the items were presented for a small proportion of alerts. To do this, we asked NEP and the relevant editors to continue collecting and ordering their alerts as they would do under normal circumstances. However, before the release of any given alert, we intervened and randomly allocated the alert into one of two groups. Within each subfield, around two-thirds of the alerts were allocated to a control group and the remaining alerts were allocated to a treatment group. Any alert within the control group was sent to subscribers with no alterations - the list of items was left completely unchanged. In contrast, any alert within the treatment group had its list of papers rearranged into a new random order before the alert was sent to subscribers. Beyond this, no changes were ever made to the content or presentation of the alerts, and the subscribers were left unaware of the experiment.

2.3 Data

Our analysis considers how download activity is related to four list positions within the email alerts: top, second, second from bottom, and bottom. As these positions are ill-defined in lists with less than four items, we drop the 43 such alerts from our initial sample to leave a final sample of 530 alerts.[9]

Some summary statistics are provided in Table 1 (within Appendix B). Across the 530 alerts, the sample covers a total of 6624 items with an average of 12.5 items per alert. The 6624 listed items stem from 4942 different papers such that an average paper appears on 1.33 subfield alerts within our sample (or 3.90 subfield alerts across all of NEP). We later address this feature of the data within our estimation procedures.

Table 1 also uses NEP's item-specific download measures to record the aggregate number of downloads made from the release of each item's subfield alert until a single cut-off date, almost two years later. This measurement period is easily sufficient to cover all relevant downloads as most downloads are made within a few weeks after the alert is released. However, the use of a single cut-off date does imply that alerts with different release dates are monitored for slightly different lengths of time, and we later control for this fact within our analysis. Within the sample, downloads were made from 9364 ip addresses (individual computers). To ease exposition, we broadly refer to an ip address as a 'user'. After deleting a handful of duplicative cases whereby the same user had downloaded the same item more than once, we end up with a total of 35004 downloads.

In subsequent sections, we often combine the download data with a range of alert-specific and item-specific control variables. These as summarized in Table 2. The alert-specific control variables include the total number of items within the alert and a measure of each alert's 'availability' - the number of days between the alert's release date and the final download cut-off date. The item-specific (or paper-specific) control variables are constructed from each item's summary information. They include variables related to an item's title language, length of title, number of authors, length of abstract, length of keywords, number of JEL classification codes, and a measure of the total number of lists

phy, Human Capital, International Trade, Intellectual Property, Knowledge Management, Microfinance, Microeconomics, Migration, Marketing, Monetary, Post Keynesian, Project and Portfolio Management, Risk Management, Sports, and Transition.

[9]Re-estimating our results with the inclusion of these lists to study top and bottom position only does not change our main conclusions.

(within the entire population of NEP) in which the item's underlying paper appeared.

3 Initial Analysis

To begin the analysis, Section 3.1 first outlines some common explanatory hypotheses for top position effects, before Section 3.2 specifies two empirical tests. The descriptive and econometric results are then provided in Sections 3.3 and 3.4 respectively.

3.1 Initial Explanatory Hypotheses

This subsection outlines three initial explanatory hypotheses for top position effects, H1-H3. As the existing literature contains no theoretical model of top position effects within our unlimited selection setting, we use the following simplified framework to help clarify our discussion.

Consider some alert or 'list' l with $n_l \geq 2$ items. Define the position of item j as $p_j \in \{1, ..., n_l\}$, where $p_j = 1$ if item j is in top position, and $p_j = n_l$ if item j is in bottom position. Let user i's true value of downloading item j, V_{ij}, derive from two additive components, such that $V_{ij} = v_i(s_j) + u_i(\omega_j)$. The first component, $v_i(s_j)$, refers to the 'observable value', which can be assessed by inspection of item j's listed summary information s_j (title, authors, abstract, keywords, JEL codes and date). In contrast, the second component, $u_i(\omega_j)$, refers to the 'unobservable value'. This cannot be assessed until after user i has downloaded the item and relates to the underlying quality of the item, ω_j, where we assume $u_i'(\omega_j) > 0$ for all i. Before downloading item j, user i can only estimate this second component as $\hat{\omega}_j$.

To explain why individuals might download some items and not others, one must assume some form of costs. For instance, our explanatory hypotheses can be presented in terms of search costs that users must incur to inspect each item's summary information. However, this only adds unnecessary complexity. Instead, and without loss for our illustrative purposes, we present our hypotheses in terms of download costs. In particular, suppose that user i can freely inspect each item j's summary information, but faces a cost of effort to actually download any given item, c_i. As there is no inherent constraint on the number of items that users are able to download, the following simple decision rule is then optimal for user i - download any item j if its expected value is greater than or equal to its associated download cost, $\hat{V}_{ij} = (s_j) + u_i(\hat{\omega}_j) \geq c_i$.

Top position effects can then be defined to exist when items in top position are significantly more likely to be downloaded than items in other positions. We now consider three common explanatory hypotheses.

H1: Specific Item Order. Top position effects exist because items in top position have a relatively large observable value.

This explanation is rather trivial - top-positioned items are more likely to be selected because they are observably better than other items. As an extreme example, suppose that user i believes that each item's position reveals no information about its unobservable value, $\hat{\omega}_j = \omega \; \forall j$. H1 then suggests that top position effects exist only because items in top-position happen to have relatively large observable value, e.g. $\hat{V}_{ij} = v(s_j) + u_i(\omega) \geq c_i$ for $p_j = 1$, but $\hat{V}_{ik} = v_i(s_k) + u_i(\omega) < c_i$ for some $p_k > 1$.

H2: Value Signals. Top position effects exist because users believe (perhaps incorrectly) that the items have been arranged in descending order of value.

In contrast, H2 suggests that top-positioned items are more likely to be downloaded because users expect top-positioned items to have larger unobservable value. For example, suppose that user i believes that a better informed agent m has arranged the items in descending order according to m's values, such that $p_j < p_k$ if $V_{mj} > V_{mk}$. Then, for ease of exposition, consider an extreme case where each item's summary information reveals nothing about its relative value, $v_i(s_j) = v(s)\ \forall i, j$. User i then believes that $p_j < p_k$ implies that item j has a higher unobservable value than item k, $\omega_j > \omega_k$, such that $V_{ij} = v(s) + u_i(\omega_j) > V_{ik} = v(s) + u_i(\omega_k)$ for any $p_j < p_k$.

H3: Choice Fatigue. Top position effects exist because users have download costs that are increasing from top position downwards.

Finally, H3 suggests that top placed-items are more likely to be downloaded because lower-placed items are increasingly costly to download, $c_i(p_j) > c_i(p_k)$ for any $p_j > p_k$. This is consistent with the possibility where users i) exhibit total effort costs that are convex in the number of downloads they complete, and ii) make their download decisions sequentially in a strict descending order from top position downwards.

3.2 Initial Empirical Tests

To assess the validity of these hypotheses, we specify the following two empirical tests. While a version of Empirical Test I has already been employed within the existing literature to rule out artificial position effects, Empirical Test II is entirely original.

Empirical Test I: Comparison of the Control and Treatment Groups. Under H1, top position effects exist only because an item with a relatively large observable value has been placed in top position. Consequently, under H1, any such effects should only arise within the control group where the items are likely to have been deliberately ordered. Hence, H1 can be rejected as a full explanation of top position effects if significant top position effects remain within the treatment group where the item order has been randomized. In contrast, any evidence of top positions within the treatment group cannot be used to rule out the explanations of value signals (H2) and choice fatigue (H3) as users might still persist in holding (now incorrect) beliefs that top-placed items have high value or continue to find lower positioned items too costly to download.

Empirical Test II: Analysis of Other Position Effects. The explanations of value signals (H2) and choice fatigue (H3) can be tested by analyzing a broader set of position effects beyond top position. Under H2 and H3, the likelihood of download activity is predicted to be decreasing in position because i) users expect item values to be decreasing from top position downwards, or ii) users have increasing download costs. Hence, H2 and H3 can be rejected as pure explanations of top position effects if items in some position p, are significantly more likely to be downloaded than items in some preceding position, $p' < p$.

3.3 Descriptive Results

For an initial descriptive analysis, we first study position effects by considering the aggregate number of downloads received by each item. In particular, Table 3 and Figure 1 show how the aggregate number of downloads per item varies with list position within the control and treatment groups. First, as expected, strong top position effects are observed in the control group: top-positioned items receive 57% more aggregate downloads than an average item. Second, while the randomization of item order slightly reduces the size of this effect, top-positioned items still receive substantially more downloads than average within the treatment group. Indeed, despite the order of items having been randomized, top-positioned items still receive 42% more downloads than an average item. Hence, with the use of Empirical Test I, Specific Item Order (H1) is unlikely to be a full explanation of top position effects. Third, if we consider a broader set of positions beyond top position but ignore bottom position (as often done in some parts of the existing literature), then downloads appear to be strictly decreasing in item position in line with Empirical Test II. However, bottom-positioned items i) receive 9% more downloads than average in the control group, ii) 23% more downloads than average in the randomized treatment group, and iii) attract approximately 22-26% more downloads than items in the preceding, second from bottom, position (across both the control and treatment groups). This contradicts Value Signals (H2) and Choice Fatigue (H3), and rules out the possibility that top position effects exist in the data simply because NEP typically sorts the items in descending value.

3.4 Econometric Results

To consider Empirical Tests I and II more deeply, we now provide a more rigorous analysis of how list position affects download activity. Such an analysis could be done in several ways. For instance, one could continue to use the aggregate download data from the previous subsection to estimate how list position affects the total number of downloads received by each item. Alternatively, one could investigate the data at a dis-aggregated 'user' level to estimate how an active user's decision to download an item is affected by its list position. To demonstrate the robustness of our results, we take both approaches. In the main text, we now focus on a dis-aggregate analysis in order to i) control for unobservable user-effects, and ii) provide a useful foundation for Section 4. However, in the Supplementary Appendix, we also show that our main conclusions remain robust under the alternative aggregate analysis.

To conduct the dis-aggregate analysis, we construct a dataset of active user download decisions. In particular, for any list l where user i has downloaded one or more items, we construct n_l user-item level observations where d_{ipl} equals one if user i downloaded the item in position p of list l, and zero if not. For example, if a user had downloaded the first two papers from a list of four items, four observations would be created with values of d_{ipl} equal to $\{1, 1, 0, 0\}$ respectively. After applying this procedure over all users and all lists, we finish with a dataset of 288,788 user-item level observations.

3.4.1 Random Effects Estimations

To understand how users' decisions to download a given item are influenced by the item's position, we first estimate a double-level random effects (RE) probit model. In particular, to consider user i's decision of whether to download the item in position p of list l we construct the following latent variable:

$$d^*_{ipl} = \beta_0 + \Pi'\beta_\Pi + z'_l\beta_z + q'_{pl}\beta_q + \mu_{pl} + \psi_i + \varepsilon_{ipl} \tag{1}$$

The vector Π includes a set of position dummies for items in top, second, second from bottom, and bottom positions. Any position effects will then be captured by the estimated values within $\beta_\Pi = \{\beta_{top}, \beta_{sec}, \beta_{secbot}, \beta_{bot}\}$. The vectors z_l and q_{pl} include the list-specific and item-specific control variables that were presented in Section 2.3. To control for user heterogeneity and the fact that some papers are included on more than one alert, we then include random effects at two levels. First, we include a 'user random effect', ψ_i, to capture the unobservable effects of an individual IP address. Second, we include a 'paper random effect', μ_{pl}, to control for the unobservable effects of the underlying paper in position p of list l.

After estimating equation (1) on the control and treatment groups separately, we then assess Empirical Test I by formally examining how the estimated position effects differ between the control and treatment groups. To do this, we estimate equation (1) on the full sample with the following additional variables: $treat_l$ - a dummy variable that equals one only if list l is in the treatment group, and $\Pi' * treat_l$ - a vector of interacted position terms.

Table 4 presents the results. Within each estimation, we report the marginal effects together with the random effects coefficients, where all (robust) standard errors are given in parentheses. For comparison, we present three specifications involving i) no random effects, ii) only the user random effects, and iii) both the user and paper random effects. The estimated user random effects are heavily significant across all cases implying substantial heterogeneity across users - an issue we later return to in Section 4. However, the reported heterogeneity across papers is less pronounced, with the associated random effects only being significant within the control group. For ease, we also provide Figure 2 which plots the estimated position effects for the control and treatment groups for an example specification (iii).

The results offer some robust evidence for the existence of top position effects, and reject all three of the common explanatory hypotheses H1-H3 as pure explanations.

First, as expected within the control group, items in top position are significantly more likely to be downloaded. However, highly significant top position effects also remain within the treatment group despite the order of items having been randomized. This rules out an explanation based purely on the specific order of items (H1). Nevertheless, H1 does appear to play a minor explanatory role, as the randomization of item order significantly weakens the size of the estimated top position effects by approximately 30%.

Second, there are smaller, yet significant, position effects for items placed in second position and bottom position in both the control and treatment groups. Indeed, randomization has *no* significant effect in reducing the size of the bottom position effect.

Third, contrary to the common hypotheses of value signals (H2) and choice fatigue (H3), the estimated position effects are not strictly decreasing in size from top to bottom. To evaluate this formally, the bottom of the estimation table reports a series of LR tests to assess i) the overall equality of the estimated position effects, $\beta_{top} = \beta_{sec} = \beta_{secbot} = \beta_{bot}$, and ii) the equality of 'adjacent' position effects; $\beta_{top} = \beta_{sec}$, $\beta_{sec} = \beta_{secbot}$, and $\beta_{secbot} = \beta_{bot}$. For both groups, these tests confirm that the position effects are strictly decreasing from top to second, and from second to second from bottom, but show that the bottom position effects are significantly larger than those in the preceding, second from bottom, position. With the use of Empirical Test II, this rules out Value Signals (H2) and Choice Fatigue (H3) as pure explanations for top position effects - it cannot be that top position effects exist just because users expect items to be arranged in a strictly descending order of value (H2), or that users find it increasingly costly to make downloads as they progress down the list (H3).

Finally, we note some secondary results from Table 4. i) Interestingly, randomization appears to actually increase users' download activity as indicated by the positive effect of the variable, $treat_l$. This may suggest that users were able to infer that the randomized alerts were not ordered as usual, and consequently chose to inspect the items more thoroughly. ii) It is also worth mentioning the estimated effects of the control variables, which are best considered within the treatment group. Perhaps surprisingly, items with a higher number of authors are less likely to be downloaded. Items with an English title are more likely to be downloaded. The probability of download is U-shaped in an item's length of title. The length of abstract provides no effect, but items with no abstract have a higher download probability. Items with more keywords have a slightly higher download probability, and items without any JEL codes are less likely to be downloaded. Lastly, the probability of download is mildly increasing in the number of lists in which the item's underlying paper appears, perhaps reflecting the paper's general appeal.

3.4.2 The Role of List Length

The results of the previous subsection have ruled out H1-H3 as major explanations of top position effects. Before moving to the next section, we briefly offer a further clue to the cause of top position effects by studying how our estimates vary with the number of items contained within an alert or 'list'. To proceed, we re-estimate the random effects estimations from (1) with an additional set of interaction terms, $\Pi' * n_l$, to measure how each position effect varies with list length, n_l.

The results are presented in Table 5. Within the control group, the four position effects are all significantly decreasing in list length. However, within the treatment group, while we continue to observe a similar pattern for most positions, the estimated top position effects are not significantly decreasing in list length. Hence, as list length increases, users' download activity does not dilute away from top position in the way observed for other positions. Instead, with a weak level of significance, the top position effects actually increase and become relatively more pronounced. This pattern is even stronger in our analysis of the aggregate data within the Supplementary Appendix, and related findings have also been documented by Ho and Imai (2008) and Feenberg et al (2017).

While this is an interesting result with important implications, we are careful to not place too much emphasis on using it to distinguish between explanations of position effects for two reasons. First, variations in list length are unlikely to be fully exogenous. For instance, in our setting, list length varies due to differences in the supply of academic papers over time and across subfields, and may be correlated with variations in the quality of papers. Second, as we later discuss, a pattern of increasing top position effects is predicted by most remaining explanations.

4 Further Analysis

Contrary to specific item order (H1), value signals (H2), and choice fatigue (H3), the previous section demonstrated that i) significant top position effects remain even when the order of items has been randomized, and ii) top position effects co-exist with smaller, but highly significant bottom position effects. To help better explain these initial findings, we now introduce and test three more explanatory hypotheses (H4-H6).

4.1 Further Explanatory Hypotheses

H4: Choice Fatigue with Heterogeneous Direction. Top and bottom position effects co-exist because some users have download costs that are increasing from top position downwards, while some other users have download costs that are increasing from bottom position upwards.

Unlike the simple version of choice fatigue (H3), this hypothesis explains the simultaneous existence of top and bottom position effects by recognizing the potential heterogeneity in users' behavior. In particular, it is consistent with users exhibiting total effort costs that are convex in the number of downloads they complete, but where some users always make their download decisions from top position downwards, such that $c_i(p_j) > c_i(p_k)$ for any $p_j > p_k$, while some other users always make their download decisions from bottom positions upwards such that $c_i(p_j) < c_i(p_k)$ for any $p_j > p_k$. In other words, top position effects derive from a group of users who consider the items in a descending direction, while bottom position effects arise from a *different* group of other users who consider the items in an ascending direction.

The possibility of users selecting items from bottom position upwards may seem odd. However, such behavior is later evidenced directly in Section 4.3.1, and is consistent with users first reading the items in descending order before then making their selection decisions from bottom item upwards. In addition, Caplin et al (2011) also provide strong experimental evidence that some subjects inspect items in ascending order.

Contrary to our empirical findings, H4 predicts that an increase in list length should increase the relative size of both top and bottom position effects. As the two groups of users are more active towards the top and bottom of the list respectively, additions in list length should increase the download activity of both top and bottom items relative to the average item.

Unlike Choice Fatigue with Heterogeneous Direction (H4), our last two hypotheses, H5 and H6, do not attribute the simultaneous existence of top and bottom position effects to different groups of users. Instead, they suggest that any individual user can display both top and bottom position effects.

H5: Choice Fatigue with Mixed Direction. Top and bottom position effects co-exist because each individual user has download costs that sometimes increase from top position downwards and sometimes increase from bottom position upwards.

This hypothesis is consistent with each user varying the order in which they considers items depending on the context - a user may make their download decisions in a descending direction for some lists, while making their download decisions in an ascending direction for other lists.

Under H5, an increase in list length can have similar effects to those under H4. However, if users are relatively more likely to make their download decisions in a descending, rather than an ascending, direction when faced with longer lists, then H5 can also better explain why top position effects increase while bottom position effects do not.

H6: Non-Monotonic Download Costs. Top and bottom position effects co-exist because users have non-monotonic download costs that are relatively lower for items in top and bottom position and relatively higher for items in other positions.

This hypothesis suggests that items in top and bottom position are relatively salient or prominent in the sense that they have lower download costs than items in other positions. To illustrate such a non-monotonicity in download costs, we now provide two more detailed examples of H6:

Under Choice Fatigue with Return Direction (H6a) users i) exhibit total effort costs that are convex in the number of downloads they complete, ii) make their initial download decisions from top position downwards, but iii) potentially return up the list to reconsider some items that they did not download previously. Hence, items towards the top are initially more prominent and easier to download, but once the user reaches the bottom, lower-placed items become relatively more prominent. Fishman and Lubensky (2017) provide some related theoretical results. By building on Janssen and Parakhonyak (2014), they consider a related limited selection setting when individuals face i) positive inspection costs, and ii) positive return costs to reconsider previously inspected options. As consistent with our findings, they show that options at both the start and the end of a sequence are more likely to be selected, and that top position effects become relatively more important in longer sequences.

Under Bounded Rationality (H6b) users replace fully rational decision rules with heuristics to economize on cognitive resources, and such heuristics exhibit top and bottom position effects by making the items in top and bottom position appear more salient. For instance, rather than making all the necessary complex comparisons to make the fully optimal selection, a user may employ the following realistic decision-rule: i) decide to download a maximum of z items, ii) sequentially inspect the items in an descending direction from the top down and immediately download any items with an expected quality above some aspiration level, $\bar{V}$, then if necessary, iii) sequentially inspect any remaining items again in an ascending direction from the bottom up and immediately download any items with an expected quality above $\bar{\bar{V}} < \bar{V}$ until the user completes z downloads or reaches the top of the list. Salant (2011) provides some related theoretical results within a limited selection setting. As consistent with our findings, he shows that any choice rule that is procedurally simpler than rational choice displays top and bottom position effects, and that such heuristics are optimally employed when the number of options is large.

Both H6a and H6b appear consistent with Feenberg et al's (2017) explanation of 'skimming' where individuals, perhaps under time pressure, focus on items in prominent positions. Moreover, unlike H4 and H5, H6 does not require users to make their download selections in a strictly monotonic direction from top down or bottom up. Instead, it permits users to make their downloads in a non-monotonic order by, say, selecting the third item, the fifth item, and then the second item.

To test between H4-H6, we now propose and conduct two further empirical tests.

4.2 Empirical Test III

Empirical Test III: Other Position Effects and User Heterogeneity. Under Choice Fatigue with Heterogeneous Direction (H4), the likelihood of download activity is predicted to be decreasing in position from top position downwards for those users who always inspect lists in descending order, but increasing in position from bottom position upwards for those users who always inspect lists in ascending order. In contrast, no such fixed monotonic patterns are required under Choice Fatigue with Mixed Direction (H5) where users vary their download direction, or under Non-Monotonic Download Costs (H6) where users may download in a non-monotonic direction.

4.2.1 Random Parameter Estimations

Empirical Test III hinges on how the estimated position effects vary across different users. Hence, rather using our previous random effects model, (1), we now switch to a random parameters model to allow the set of estimated position effects, $\beta_{i,\Pi}$, to vary across each user i, as illustrated in (2):[10]

$$d^*_{ipl} = \beta_0 + \Pi' \beta_{i,\Pi} + z'_l \beta_z + q'_{pl} \beta_q + \varepsilon_{ipl} \tag{2}$$

For each estimation, we report the marginal effects of the main variables, together with the coefficients of the random parameters. All (robust) standard errors are presented in parentheses. While the overall results are consistent with the previous random effects estimations, the random parameter results document a substantial heterogeneity in position effects across users. This is illustrated in Figure 3 where the estimated random parameters are recovered following the method by Train (2009) and presented graphically.

For the context of Empirical Test III, Table 7 now summarizes some user-level features of the estimated random parameters, $\beta_{i,\Pi} = \{\beta_{i,top}, \beta_{i,sec}, \beta_{i,secbot}, \beta_{i,bot}\}$. Within the treatment group, as seemingly consistent with Choice Fatigue with Heterogeneous Direction (H4), 64% of users are estimated to have their largest position effect in top position, $max\{\beta_{i,\Pi}\} = \beta_{i,top}$, while 16% of users are estimated to have their largest position effect in bottom position, $max\{\beta_{i,\Pi}\} = \beta_{i,bot}$. However, contrary to H4, only one percent of users are estimated to have position effects that monotonically decrease with position, $\beta_{i,top} > \beta_{i,sec} > \beta_{i,secbot} > \beta_{i,bot}$, and even fewer users are estimated to have position effect that monotonically increase with position, $\beta_{i,top} < \beta_{i,sec} < \beta_{i,secbot} < \beta_{i,bot}$. Instead, as more consistent with Choice Fatigue with Mixed Direction (H5) or Non-Monotonic Download Costs

[10] Adding additional paper-level heterogeneity to the random parameters makes little difference to the results, and only complicates interpretation. Therefore we focus only on user heterogeneity for these estimations.

(H6), most users' position effects show no monotonic pattern. Indeed, 72% of users are estimated to exhibit both top and bottom position effects, with position effects that decrease with position until bottom position, $\beta_{i,top} > \beta_{i,sec} > \beta_{i,secbot} < \beta_{i,bot}$.

4.3 Empirical Test IV

Empirical Test IV: Ordering of Multiple Downloads. This test restricts attention to instances where a user downloads more than one item from a list. Under Choice Fatigue with Heterogeneous Direction (H4) some such users are predicted to always make their downloads in a monotonic descending order from top position downwards, while other such users are predicted to always make their downloads in a monotonic ascending order. Under Choice Fatigue with Mixed Direction (H5), each such user will vary in making their downloads in a monotonic descending or monotonic ascending order. In contrast, such user's downloads are not required to be made in a monotonic order under Non-Monotonic Download Costs (H6).

4.3.1 Download Timing

Empirical Test IV is based upon the order in which individual users make their selections in instances where they download more than one item from a list. For each such instance, we recover the order in which the user downloaded their multiple items by utilizing the data on download timing which records the exact time at which each download was made (to the nearest second).

Table 8 summarizes some results for all instances where a user downloads k items from an individual list. First, let $k \geq 2$, such that we focus on the 6370 instances where a user downloads at least two items from a list. Users download their top-most selected item first in 76% of the instances, and download their bottom-most selected item first in 18% of instances. This gives clear evidence that not all users select their items from the top down, and that some users start their download activity from the bottom.

However, to study whether users download their items in a monotonic order, it is better to focus on instances where users download more than two items from a list. While this reduces the sample size, it avoids artificially including instances of monotonic behavior when a user downloads exactly two items. The right-hand side of Table 8 presents some results for instances where users download at least three items ($k \geq 3$) or four items ($k \geq 4$) per list. They show that items are downloaded in a monotonic order 70-73% of the time: 67% of instances exhibit a monotonic descending order, while 3-6% exhibit a monotonic ascending order. While this is particularly in line with Choice Fatigue with Heterogeneous Direction (H4) and Choice Fatigue with Mixed Direction (H5), it also implies that 27-30% of instances exhibit non-monotonic download behavior as more consistent with Non-Monotonic Download Costs (H6).

To consider whether users show systematic behavior across different lists, one can analyze the 992 users within our sample who download at least $k = 2$ items in more than one instance. On average, we observe such users' multiple download behavior across 4 different lists. Table 9 shows that 52% of such users 'always' download their top-most selected item first, while 3% of such users 'always' download their bottom-most selected item first.

Moreover, to consider Empirical Test IV more directly, we now examine the extent to which these 'multiple download users' show systematic monotonic ordering behavior across different lists. To do so, we restrict attention to users who are observed to download at least three or four items per list (with $k \geq 3$ or $k \geq 4$) in more than one instance. Table 9 shows that 42-48% of such users 'always' download their items in a monotonic order. In more detail, 40-45% of users 'always' download their items in a monotonic order with the *same* direction , and only 2-3% of users 'always' download their items in a monotonic order with varied directions. The fact that the proportion of users employing mixed directions is so low gives only limited evidence for Heterogeneous Choice Fatigue with Mixed Direction (H5). Further, if we consider the direction with which users monotonically download their items, Table 9 indicates that 40-43% of users always download their items in a strict descending order, only 0-2% of users always download their items in a strict ascending order. Hence, while seemingly consistent with Choice Fatigue with Heterogeneous Direction (H4), the evidence for H4 also remains limited as the percentage of users that systematically download their items in an ascending, rather than descending, order appears small relative to the size of the documented bottom position effects.[11]

Therefore, with only limited evidence for H4 and H5, a substantial explanation must rest with the remaining 52-58% of users that do not always download their items in a monotonic order. Indeed, as more consistent with Non-Monotonic Download Costs (H6), such users typically download their selected items in a non-monotonic order 49-54% of the time.

Finally, in further positive support of H6, we briefly consider how these results vary with list length. For H5 to offer a strong explanation for why top- but not bottom position effects increase in longer lists, users would have to be relatively more inclined to make their downloads in descending order when selecting from longer lists. Contrary to this, Table 10 shows that when faced with a list of above-median length, users show very little change in their download ordering. However, as potentially more in line with Non-Monotonic Download Costs (H6), we see that users are slightly more inclined to download their items in a non-monotonic order when faced with more items.

Hence, overall, while this section finds some limited support for Choice Fatigue with Heterogeneous Direction (H4) and demonstrates that a small fraction of users are consistent with Choice Fatigue with Mixed Direction (H5), it suggests that top position effects are best described by Non-Monotonic Download Costs (H6).

5 Conclusion

This paper has used a natural field experiment to better understand the causes of top position effects in individuals' choices from lists. Contrary to three common explanations, our results have shown that i) significant top position effects remain even when the order of items is randomized, and ii) top position effects co-exist with smaller, but highly significant, bottom position effects. Instead, after developing original tests based on the user-level aspect of our data, we have provided evidence for some more subtle explanations involving different forms of choice fatigue and bounded rationality.

[11]This finding also explains why we observe significant position effects for second position, but not second-from-bottom position.

As listed in the introduction, such insights can have implications for many areas of economics. For instance, the evidence of heterogeneity in the explanations across users should impact on i) how policymakers can best assist individuals in selecting beneficial options, ii) how firms can present their product ranges most profitably, and iii) how regulators can best enhance the development and use of search engines. Future research would be very useful in further testing the explanations of top position effects, and further understanding *why* individuals employ the documented behaviors. The use of eye-tracking software offers much hope in this regard. For instance, Reutskaja et al (2011) use such software to analyze subjects' choices from a grid of options. Among many other results, they find that subjects look more frequently at, and are more likely to choose, items located in the top left-hand corner, or the middle, of the grid. The full application of such techniques to study position effects in lists is likely to be very fruitful.

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Appendix A: An Example Email Alert

nep-cbe
New Economics Papers on Cognitive and Behavioural Economics
Issue of 2017-06-18
six papers chosen by
Marco Novarese
Università degli Studi del Piemonte Orientale
http://econpapers.repec.org/pno2

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1. Nudging in education: A survey
Mette Trier Damgaard; Helena Skyt Nielsen

2. Digestible information: The impact of Multiple Traffic Light nutritional labeling in a developing country
Defago, Daniel; Geng, José F.; Molina, Oswaldo; Santa María, Diego

3. Facing Yourself: A Note on Self-image
Armin Falk

4. Essays on behavioral finance
Terzi, Ayse

5. Revealing the Economic Consequences of Group Cohesion
Simon Gaechter; Chris Starmer; Fabio Tufano

6. The Merit Primacy Effect
Alexander Cappelen; Karl Ove Moene; Siv-Elisabeth Skjelbred; Bertil Tungodden

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1. Nudging in education: A survey

Date: 2017-06-08

By: Mette Trier Damgaard (Department of Economics and Business Economics, Aarhus University, Denmark) ; Helena Skyt Nielsen (Department of Economics and Business Economics, Aarhus University, Denmark)

Can we nudge children, youths and their parents to make better educational decisions? Educational decisions involve immediate costs and potential future benefits. Research suggests that in such settings behavioral barriers (such as lack of self-control, limited attention and social norms) are likely to influence choices. This raises the question whether low cost ”nudges” can improve people’s educational choices. While interventions targeting cognitive or attentional limitations seem to be effective, it is too soon to provide a roadmap for introducing nudges in the education sector.

Keywords: Behavioural bias, boost policies, education choice, human capital investment
JEL: D03 D04 I20
URL: http://d.repec.org/n?u=RePEc:aah:aarhec:2017-05&r=cbe

The remaining items 2-6 are then presented in a similar format.

Appendix B: Tables and Figures

Table 1: Descriptive Statistics

	All	Control	Treatment
Number of alerts	530	350	180
Total number of items	6624	4269	2355
Average number of items per alert	12.50	12.64	12.20
Total number of downloads across items	35002	22856	12146
Average number of downloads per item	5.28	5.35	5.16
Total number of users that downloaded at least one item	9367	7024	4065
Average number of items downloaded per active user per alert	1.73	1.72	1.75
Average number of days between download and alert release	14.69	14.16	15.67
Total number of papers	4942	-	-
Average number of alert appearances per paper (within sample)	1.34	-	-
Average number of alert appearances per paper (within NEP)	3.90	-	-

Table 2: Alert- and Item-Specific Control Variables

Name	Description	Mean	St. Dev.	Min	Max
n	Number of items in alert (divided by 10)	1.25	0.83	0.40	1.18
ln(av)	Number of days alert was available (log)	6.64	0.06	6.54	6.73
authors	Number of item authors	2.16	1.11	1	15
engtitle	=1 if item has English title	0.99	0.10	0	1
title	Number of characters in item title (divided by 100)	0.75	0.28	0.10	2.43
title2	Title variable squared (divided by 10)	0.06	0.05	0.00	0.59
zeroab	=1 if item has no abstract	0.02	0.15	0	1
abstract	Number of characters in abstract (divided by 1000)	0.97	0.55	0	14.82
zerokey	=1 if item has no keywords	0.20	0.40	0	1
keywords	Number of item keywords (divided by 10)	0.37	0.28	0	3.20
keywords2	Keywords variable squared	0.21	0.38	0	10.24
zerojel	=1 if item has no JEL codes	0.42	0.49	0	1
jel	Number of item JEL codes	1.84	1.90	0	13
repstotal	Number of lists within NEP in which paper appears	3.90	1.39	2	12

Note: The descriptive statistics are calculated at the relevant alert- or paper-level.

Table 3: Aggregate Downloads by Position

	All	Control	Treatment
Number of Alerts	530	350	180
Average downloads per item across all positions	5.28	5.35	5.16
Average downloads per item in top position	8.05	8.42	7.33
Average downloads per item in second position	6.49	6.88	5.85
Average downloads per item in second from bottom position	4.86	4.78	5.02
Average downloads per item in bottom position	5.99	5.81	6.35

Figure 1: Aggregate Downloads by Position

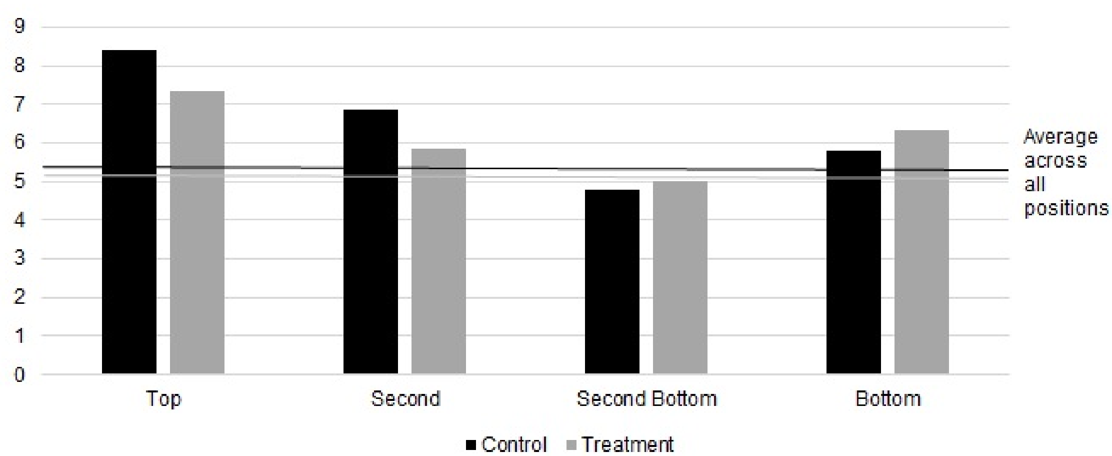


Table 4: Estimated Position Effects from Random Effects Estimations

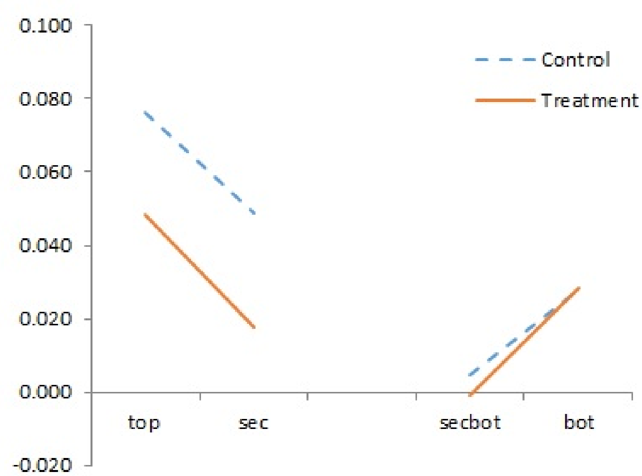
	Control			Treatment			All		
	i)	ii)	iii)	i)	ii)	iii)	i)	ii)	iii)
top	0.080	0.076	0.076	0.051	0.049	0.049	0.079	0.075	0.075
	(0.003)***	(0.003)***	(0.003)***	(0.004)***	(0.004)***	(0.004)***	(0.003)***	(0.003)***	(0.003)***
top*treat	-	-	-	-	-	-	-0.023	-0.023	-0.023
	-	-	-	-	-	-	(0.004)***	(0.004)***	(0.004)***
sec	0.053	0.049	0.049	0.021	0.018	0.018	0.052	0.048	0.048
	(0.003)***	(0.003)***	(0.003)***	(0.004)***	(0.004)***	(0.004)***	(0.003)***	(0.003)***	(0.003)***
sec*treat	-	-	-	-	-	-	-0.030	-0.030	-0.030
	-	-	-	-	-	-	(0.005)***	(0.005)***	(0.005)***
secbot	0.008	0.005	0.005	0.002	-0.001	-0.001	0.007	0.003	0.003
	(0.003)**	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)*	(0.003)	(0.003)
secbot*treat	-	-	-	-	-	-	0.001	0.000	0.000
	-	-	-	-	-	-	(0.005)	(0.004)	(0.005)
bot	0.031	0.028	0.028	0.032	0.029	0.029	0.030	0.026	0.026
	(0.003)***	(0.003)***	(0.003)***	(0.004)***	(0.004)***	(0.004)***	(0.003)***	(0.003)***	(0.003)***
bot*treat	-	-	-	-	-	-	0.007	0.006	0.006
	-	-	-	-	-	-	(0.005)	(0.004)	(0.004)
treat	-	-	-	-	-	-	0.004	0.004	0.004
	-	-	-	-	-	-	(0.002)*	(0.002)*	(0.002)*
n	-0.021	-0.019	-0.019	-0.036	-0.034	-0.034	-0.024	-0.022	-0.022
	(0.001)***	(0.002)***	(0.002)***	(0.001)***	(0.003)***	(0.003)***	(0.001)***	(0.002)***	(0.002)***
ln(av)	-0.015	0.005	0.005	-0.143	-0.123	-0.117	0.003	-0.006	-0.006
	(0.011)	(0.015)	(0.018)	(0.040)***	(0.051)*	(0.052)*	(0.009)	(0.016)	(0.015)
authors	-0.004	-0.004	-0.004	-0.005	-0.005	-0.005	-0.004	-0.004	-0.004
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
engtitle	0.077	0.075	0.075	0.102	0.096	0.096	0.083	0.082	0.082
	(0.009)***	(0.011)***	(0.011)***	(0.016)***	(0.017)***	(0.017)***	(0.008)***	(0.009)***	(0.009)***
title	-0.052	-0.065	-0.066	-0.093	-0.094	-0.094	-0.068	-0.077	-0.077
	(0.009)***	(0.010)**	(0.010)***	(0.014)***	(0.014)***	(0.014)***	(0.008)***	(0.008)***	(0.008)***
title2	0.098	0.163	0.170	0.327	0.318	0.318	0.192	0.231	0.231
	(0.057)	(0.060)**	(0.059)**	(0.081)***	(0.080)***	(0.080)***	(0.045)***	(0.046)***	(0.046)***
zeroab	0.037	0.039	0.039	0.036	0.034	0.034	0.036	0.037	0.037
	(0.005)***	(0.005)***	(0.005)***	(0.007)***	(0.007)***	(0.007)***	(0.004)***	(0.004)***	(0.004)***
abstract	-0.001	-0.002	-0.002	0.000	0.000	0.000	-0.001	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
zerokey	0.017	0.018	0.018	0.009	0.012	0.012	0.014	0.016	0.016
	(0.003)***	(0.003)***	(0.003)***	(0.004)*	(0.005)*	(0.005)*	(0.003)***	(0.003)***	(0.003)***
keywords	0.023	0.163	0.020	-0.015	-0.013	-0.013	0.011	0.009	0.009
	(0.008)**	(0.060)*	(0.008)*	(0.010)	(0.011)	(0.011)	(0.006)	(0.006)	(0.007)
keywords2	-0.006	-0.005	-0.005	0.016	0.014	0.014	0.000	0.000	0.000
	(0.004)	(0.004)	(0.004)	(0.006)*	(0.006)*	(0.006)*	(0.003)	(0.004)	(0.004)
zerojel	-0.001	-0.005	-0.005	-0.006	-0.010	-0.010	-0.002	-0.007	-0.007
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)*	(0.004)*	0.002	(0.002)**	(0.002)**
jel	-0.002	-0.002	-0.002	-0.001	-0.002	-0.002	-0.001	-0.002	-0.002
	(0.001)*	(0.001)**	(0.001)**	(0.001)	(0.001)	(0.001)	(0.001)*	(0.001)**	(0.001)**
repstotal	-0.001	-0.005	-0.001	0.001	0.002	0.002	0.000	0.001	0.001
	(0.001)*	(0.003)	(0.001)	(0.001)	(0.001)**	(0.001)**	(0.000)	(0.001)	(0.001)

Estimated Position Effects from RE Estimations cont.

User RE	-	0.353	0.340	-	0.324	0.324	-	0.358	0.358
	-	(0.023)***	(0.023)***	-	(0.027)***	(0.028)***	-	(0.024)***	(0.024)***
Paper RE	-	-	0.093	-	-	0.000	-	-	0.000
	-	-	(0.021)***	-	-	(0.000)	-	-	(0.000)
Observations	189313	189313	189313	99475	99475	99475	288788	288788	288788
Lists	350	350	350	180	180	180	530	530	530
LogLik	-67533	-65355	-65343	-36004	-35012	-35012	-103615	-100081	-100081
BIC	135297	135309	135321	72227	72239	72250	207531	207544	207556
LR Tests:									
All 4 pos equal	462.7***	498.8***	498.9***	99.9***	111.0***	111.0***	-	-	-
Top=Sec	66.9***	71.8***	71.8***	37.4***	42.7***	42.7***	-	-	-
Sec=Secbot	153.3***	165.8***	165.9***	13.1***	13.9***	13.9***	-	-	-
Secbot=Bot	(-) 39.8***	(-) 43.4***	(-) 43.4***	(-) 32.9***	(-) 35.8***	(-) 35.8***	-	-	-

Note: Marginal effects are reported for the main variables, with the coefficients of the random effects. (Robust) standard deviations are presented in parentheses. Test significance is denoted by * at 5%, ** at 1%, and *** at 0.1%.

Figure 2: Illustration of Estimated Position Effects from RE Estimations



Note: These position effects are derived from the estimated marginal effects in Table 4 for specification (iii).

Table 5: Estimated Effect of List Length on Position Effects

	Control			Treatment		
	i)	ii)	iii)	i)	ii)	iii)
top	0.137	0.122	0.122	0.056	0.044	0.044
	(0.007)***	(0.008)***	(0.008)***	(0.009)***	(0.008)***	(0.008)***
top*n	-0.035	-0.026	-0.026	0.001	0.007	0.007
	(0.005)***	(0.005)***	(0.005)***	(0.006)	(0.005)	(0.005)
sec	0.094	0.080	0.080	0.063	0.049	0.049
	(0.006)***	(0.007)***	(0.007)***	(0.010)***	(0.009)***	(0.009)***
sec*n	-0.027	-0.019	-0.019	-0.028	-0.021	-0.021
	(0.004)***	(0.005)***	(0.005)***	(0.007)***	(0.007)**	(0.007)**
secbot	0.075	0.063	0.063	0.064	0.048	0.048
	(0.010)***	(0.010)***	(0.010)***	(0.010)***	(0.010)***	(0.010)***
secbot*n	-0.046	-0.039	-0.039	-0.043	-0.033	-0.033
	(0.008)***	(0.008)***	(0.008)***	(0.008)***	(0.007)***	(0.007)***
bot	0.095	0.082	0.082	0.084	0.069	0.069
	(0.007)***	(0.006)***	(0.006)***	(0.010)***	(0.009)***	(0.009)***
bot*n	-0.042	-0.035	-0.035	-0.036	-0.027	-0.027
	(0.005)***	(0.004)***	(0.004)***	(0.007)***	(0.006)***	(0.006)***
n	-0.018	-0.016	-0.016	-0.032	-0.033	-0.032
	(0.001)***	(0.002)***	(0.002)***	(0.002)***	(0.003)***	(0.003)***
Controls	Yes	Yes	Yes	Yes	Yes	Yes
User-level RE	No	Yes	Yes	No	Yes	Yes
Paper-level RE	No	No	Yes	No	No	Yes
Observations	189313	189313	189313	99475	99475	99475
Lists	350	350	350	180	180	180
LogLik	-68515	-66424	-66424	-36216	-35201	-35198
BIC	137309	137321	137334	72698	72709	72721

Table 6: Estimated Position Effects from Random Parameter (RP) Estimations

	Control	Treatment
top	0.068	0.035
	(0.003)***	(0.006)***
sec	0.034	-0.001
	(0.004)***	(0.008)
secbot	-0.011	-0.019
	(0.005)*	(0.007)*
bot	0.017	0.017
	(0.004)***	(0.006)**
Full Controls	Y	Y
User-level RPs:		
top	0.377	0.410
	(0.038)***	(0.054)***
sec	0.420	0.451
	(0.038)***	(0.072)***
secbot	0.406	0.433
	(0.047)***	(0.062)***
bot	0.388	0.379
	(0.043)***	(0.057)***
Observations	189313	99475
Lists	350	180
LogLik	-67343	-35917
BIC	134954	72087

Figure 3: Estimated Random Parameter Distributions [Control (left) and Treatment (right)]

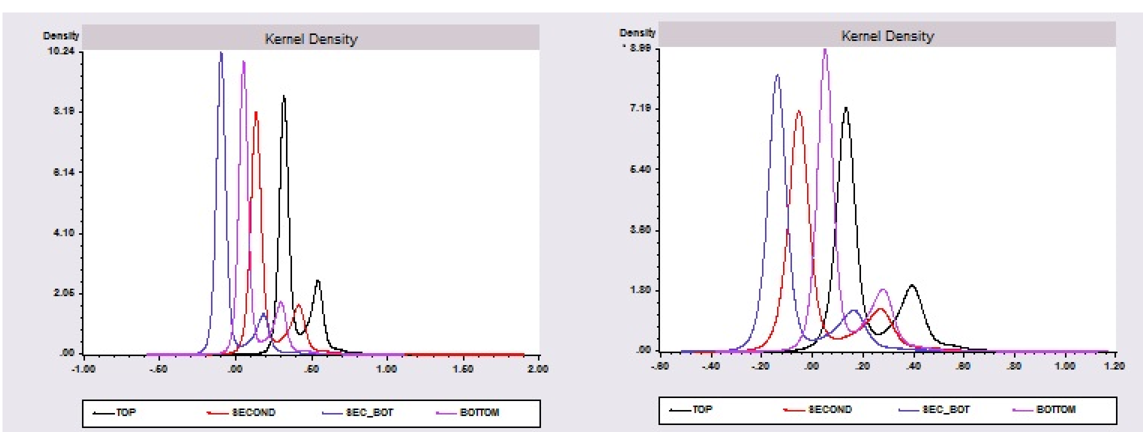


Table 7: Summary of Users' Random Parameter (RP) Patterns

	Control	Treatment
Proportion of Users with Highest RP = top	0.81	0.64
Proportion of Users with Strictly Decreasing RPs	0.01	0.01
Proportion of Users with Highest RP = bot	0.03	0.16
Proportion of Users with Strictly Increasing RPs	0.00	0.00
Proportion of Users with Strictly Decreasing RPs (excluding bot)	0.70	0.72

Table 8: Summary of Download Ordering in Instances of Multiple Downloads

	$k \geq 2$			$k = 2$	$k \geq 3$	$k \geq 4$
Proportion of Instances	All	Control	Treatment	All	All	All
Top-Most Item Downloaded First	0.760	0.759	0.761	0.756	0.764	0.785
Bottom-Most Item Downloaded First	0.181	0.183	0.179	0.244	0.105	0.064
Items Downloaded in Monotonic Order	0.880	0.881	0.879	1.000	0.734	0.698
Items Downloaded in Descending Order	0.718	0.717	0.720	0.756	0.672	0.670
Items Downloaded in Ascending Order	0.162	0.163	0.158	0.244	0.062	0.028
Number of Instances	6370	4096	2274	3494	2876	1562

Note: Specifically, these refer to instances where a user downloads k items from an individual list.

Table 9: Summary of Download Ordering for Multiple Download Users

Proportion of Such Users	$k \geq 2$	$k \geq 3$	$k \geq 4$
Always Download Top-Most Item First	0.52	0.53	0.56
Always Download Bottom-Most Item First	0.03	0.04	0.00
Always Download Items in a Monotonic Order	0.67	0.48	0.42
Always Download Items in a Monotonic Order with Same Direction	0.49	0.45	0.40
Always Download Items in a Monotonic Order with Descending Direction	0.46	0.43	0.40
Always Download Items in a Monotonic Order with Ascending Direction	0.03	0.02	0.00
Number of Such Users	992	441	233

Table 10: Effect of List Length on Download Ordering in Instances of Multiple Downloads

	$k \geq 2$		$k \geq 3$	
Proportion of Instances	n=4-13	n=14+	n=4-14	n=15+
Top-Most Item Downloaded First	0.750	0.768	0.758	0.769
Bottom-Most Item Downloaded First	0.198	0.166	0.117	0.095
Items Downloaded in Monotonic Order	0.900	0.861	0.749	0.721
Items Downloaded in Descending Order	0.717	0.719	0.676	0.669
Items Downloaded in Ascending Order	0.183	0.142	0.072	0.052
Number of Instances	3023	3347	1384	1492

Supplementary Appendix: Robustness Analysis with Aggregate Data

This appendix confirms that the dis-aggregated user-level results from Section 3.4 are robust under an alternative aggregate approach. In particular, we consider how an item's list position affects the total number of downloads it receives.

Mirroring the user-level estimation in (1), the total number of downloads received by the item in position p of list l, d_{pl}, is modeled as a function of the position dummies, Π, the list-specific control variables, z_l, and the item-specific control variables, q_{pl}:

$$\beta_0 + \Pi'\beta_\Pi + z_l'\beta_z + q_{pl}'\beta_q \tag{3}$$

Any such estimation procedure needs to take account of two features of the aggregate data. First, item downloads can only take the form of a non-negative integer, $d_{pl} \in \{0, 1, 2...\}$. Rather than using a negative binomial model, which is argued to be less robust, we address this issue by using a quasi-maximum likelihood estimator based on the Poisson distribution (Poisson QMLE).[12] Second, to account for the fact that some papers are included on the lists of more than one subfield, we cluster the standard errors by paper. This allows the error terms of observations with the same underlying paper to have a correlated error structure, while maintaining the assumption of independent errors for observations with different underlying papers. Similar to before, after estimating (3) on the control and treatment groups separately, we also estimate (3) on the full sample with the addition of $treat_l$ and the interacted position effects, $\Pi' * treat_l$.

Table 11 (below) presents the results. Within each estimation, we report the marginal effects for the position variables with their (robust) standard errors in parentheses. For comparison, we present two specifications with and without the list-specific and item-specific control variables, but do not report the marginal effects of the control variables for brevity (available on request).

The results provide similar conclusions to the main user-level analysis. First, contrary to H1, items in top position within the treatment group still receive significantly more downloads than average despite the randomization of item order. In particular, items in top position receive 36-52% more downloads than average. Second, smaller, yet significant, effects still exist for items in second position and bottom position, even after randomization. Third, contrary to H2 and H3, bottom position effects are still estimated to be significantly larger than the effects from the preceding, second from bottom, position, as detailed in the tests at the bottom of the table.

[12] See Wooldridge (1999) for more details on the QMLE Poisson and its relative advantages. The Poisson QMLE fully recognizes that the Poisson distribution may be inappropriate, but persists in using it in the knowledge that i) the coefficient estimates are still consistent, ii) one can correct for the biased standard errors by using a robust estimator for the variance-covariance matrix, and iii) the model is robust to any further forms of mis-specification. Unless otherwise stated, all our main results can be replicated using the negative binomial model.

Table 11: Estimated Position Effects with Aggregate Data

	Control i)	Control ii)	Treatment i)	Treatment ii)	All i)	All ii)
top	3.768	2.976	2.724	1.915	3.731	2.872
	(0.378)***	(0.337)***	(0.507)***	(0.444)***	(0.374)***	(0.332)***
top*treat	-	-	-	-	-0.589	-0.490
	-	-	-	-	(0.378)	(0.364)
sec	2.143	1.658	1.195	0.537	2.120	1.588
	(0.350)***	(0.319)***	(0.461)**	(0.382)	(0.346)***	(0.314)***
sec*treat	-	-	-	-	-0.666	-0.690
	-	-	-	-	(0.413)	(0.378)
secbot	-0.035	-0.418	0.319	-0.240	-0.035	-0.451
	(0.287)	(0.256)	(0.392)	(0.331)	(0.284)	(0.253)
secbot*treat	-	-	-	-	0.364	0.372
	-	-	-	-	(0.508)	(0.478)
bot	1.076	0.683	1.716	0.989	1.064	0.627
	(0.398)**	(0.351)	(0.427)***	(0.358)**	(0.393)**	(0.344)
bot*treat	-	-	-	-	0.586	0.505
	-	-	-	-	(0.526)	(0.479)
treat	-	-	-	-	-0.098	0.092
	-	-	-	-	(0.157)	(0.152)
Controls	No	Yes	No	Yes	No	Yes
Observations	4268	4268	2355	2355	6623	6623
Lists	350	350	180	180	530	530
Clusters	3317	3317	1895	1895	4942	4942
LogLik	-14800	-14100	-7910	-7525	-22700	-21700
BIC	29663	28430	15859	15197	45529	43679
$\hat{\sigma}^2$	4.69	4.07	4.29	3.68	4.55	3.96
Wald Tests:						
All 4 pos equal	79.9***	79.3***	16.5***	18.8***	-	-
Top=Sec	11.4***	9.31**	5.45*	6.30*	-	-
Sec=Secbot	27.1***	30.5	2.37	2.73	-	-
Secbot=Bot	(-) 5.71*	(-) 7.25**	(-) 6.31*	(-) 7.05*	-	-

Note: Marginal effects are reported with (robust) standard deviations in parentheses. Test significance is denoted by * at 5%, ** at 1%, and *** at 0.1%. In support for our chosen methodology and in rejection of a basic Poisson model, the estimates suggest a mean-variance ratio, $\hat{\sigma}^2$, that is always substantially greater than one. The reported Wald tests assess i) the overall equality of the estimated position effects, $\beta_{top} = \beta_{sec} = \beta_{secbot} = \beta_{bot}$, and ii) the equality of 'adjacent' position effects; $\beta_{top} = \beta_{sec}$, $\beta_{sec} = \beta_{secbot}$, and $\beta_{secbot} = \beta_{bot}$.

Finally, we now show the robustness of the dis-aggregated results about list length. Mirroring Section 3.4.2, we add the interaction terms, $\Pi' * n_l$, to (3) in order to measure how each position effect varies with list length, n_l. The results are presented below in Table 12. As consistent with our previous results, individuals focus their download activity more towards items in top position as list length increases. In particular, items in top position receive a significantly larger number of total downloads as list length increases, especially in the treatment group.

Table 12: Estimated Effect of List Length on Position Effects with Aggregate Data

	Control		Treatment	
	i)	ii)	i)	ii)
top	2.029	1.892	-0.028	0.133
	(0.482)***	(0.463)***	(0.647)	(0.616)
top*n	0.060	0.061	0.135	0.116
	(0.020)**	(0.020)**	(0.041)**	(0.036)**
sec	0.652	0.743	0.562	0.679
	(0.469)	(0.461)	(1.086)	(1.037)
sec*n	0.064	0.060	0.002	-0.013
	(0.029)*	(0.028)*	(0.063)	(0.059)
secbot	-0.470	-0.329	0.475	0.375
	(0.475)	(0.478)	(0.938)	(0.861)
secbot*n	0.001	-0.012	-0.055	-0.053
	(0.032)	(0.031)	(0.059)	(0.056)
bot	0.119	0.280	1.234	1.054
	(0.460)	(0.441)	(0.813)	(0.782)
bot*n	0.036	0.028	-0.012	-0.007
	(0.029)	(0.028)	(0.044)	(0.045)
n	-0.055	-0.050	-0.098	-0.072
	(0.004)***	(0.004)***	(0.014)***	(0.013)***
Controls	No	Yes	No	Yes
Obs	4268	4268	2355	2355
Lists	350	350	180	180
Clusters	3317	3317	1895	1895
LogLik	-14600	-14100	-7758	-7505
BIC	29256	28412	15594	15189
$\hat{\sigma}^2$	4.52	4.06	4.04	3.67

References

[1] Wooldridge J.M. (1999) "Quasi-Likelihood Methods for Count Data" Ch.8 in 'Handbook of Applied Econometrics Volume II: Microeconomics', Pesaran M.H and Schmidt P. (Eds), Blackwell