

THE EFFECT OF PIRACY WEBSITE BLOCKING ON CONSUMER BEHAVIOR

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ABSTRACT

Understanding the relationship between copyright policy and consumer behavior is an increasingly important topic for participants in digital media markets. In this paper we seek to study how consumer behavior changes when Internet Service Providers are required to block access to major piracy websites. We do this in the context of two court-ordered events affecting consumers in the UK: The blocking order directed at The Pirate Bay in May 2012, and blocking orders directed at 19 major piracy sites in October and November 2013.

Our results show that blocking The Pirate Bay only caused a small reduction in total piracy — instead, consumers seemed to turn to other piracy sites or Virtual Private Networks that allowed them to circumvent the block. We thus observed no increase in usage of legal sites. In contrast, blocking 19 different major piracy sites caused a meaningful reduction in total piracy and subsequently led former users of the blocked sites to increase their usage of paid legal streaming sites such as Netflix by 12% on average. The lightest users of the blocked sites (and thus the users least affected by the blocks, other than the control group) increased their clicks on paid streaming sites by 3.5% while the heaviest users of the blocked sites increased their paid streaming clicks by 23.6%, strengthening the causal interpretation of the results. Our results suggest that website blocking requires persistent blocking of a number of piracy sites in order to effectively migrate pirates to legal channels, but also that the increased availability of legal digital services can make antipiracy efforts more effective.

Keywords: *Piracy, regulation, digital distribution, motion picture industry, natural experiment.*

1. Introduction

One of the most important challenges facing the media industries today is whether and how copyright policy should be adapted to the realities of the digital age. The invention and subsequent adoption of filesharing technologies¹ have eroded the strength of copyright law across many countries. In the ten years following the introduction of Napster in 1999, worldwide revenues from recorded music fell by 50% (IFPI 2010), and in the four years after the introduction of BitTorrent, home video sales declined in the film industry by 27% (Zentner 2010). The vast majority of the academic literature has found that digital piracy causes a significant reduction in sales of music and motion picture content (see Danaher et al. 2014a for a review of this literature). The recent literature suggests that in the film industry, diminished revenues from piracy have the potential to lead to a decrease in the quantity and quality of films that are produced (Telang and Waldfogel 2014). Thus it is important, not only from a business perspective but also from a social welfare perspective, to understand how to design and enforce copyright policy in an age of filesharing technologies.

Accordingly, there is tremendous interest in evaluating the impact of antipiracy legislations. Several papers exist that examine the impact of antipiracy interventions on legal consumption (Adermon and Liang 2014, Danaher et al. 2014b, Danaher and Smith 2014), but each of these use aggregate market data and thus cannot capture insights into how consumers choose whether to circumvent such legislation or why they increase their legal consumption if they do so. Our study is the first of which we are aware to use a consumer-level dataset to understand the

¹ As is customary in the economics and information systems literature, we use the terms filesharing and piracy interchangeably. As well, when we say filesharing, we are referring collectively to all of the major forms of Internet media piracy including BitTorrent and other peer-to-peer protocols, direct cyberlocker downloads, and illegal streaming sites.

various ways in which Internet users react to instances of anti-piracy legislation and provide insights into why..

Specifically, we analyze the effect of piracy website blocking, a type of legislation that has not yet been addressed in the literature.² Unlike shutting down entire sites (such as the shutdown of Megaupload.com), website blocking is a strategy whereby governments or courts order Internet Service Providers within a country to simply block users' access to a website that has been shown to facilitate illegal copyright infringement. This could include piracy cyberlockers, BitTorrent tracker sites (which do not host actual content but rather index the "tracker" files that filesharers require in order to download a media file through the BitTorrent protocol), or unauthorized media streaming sites.³ However, the effectiveness of website blocking may be different from a complete site shutdown (e.g., Megaupload) because the content is still available on the servers of the blocked sites and there are a number of ways in which consumers and suppliers of pirated content may circumvent the block to obtain access to the infringing content. Thus, website blocks are empirically interesting to study as consumers have a choice between finding ways to circumvent the blocks, finding other sites to access pirated content, increasing their use of legal channels, or simply decreasing their consumption of the media in question. Our data allow us to gain insight into how consumers make this choice, and thus what types of antipiracy interventions are more likely to increase legal consumption.

In this paper we study two specific periods of website blocking orders granted by the UK High Court: the first directed to The Pirate Bay in May 2012, and second directed to nineteen

² Although one study exists on the blocking of The Pirate Bay (Poort et al. 2014), this study looks only at the effect on total piracy levels and does not explore how consumer behavior changed following the block nor whether legal channels benefitted from the block.

³ To be specific, it is more common that blocks are ordered against sites that link to or stream from cyberlocker content, rather than blocking the cyberlockers themselves.

different major filesharing websites during October and November 2013.⁴ Our analysis uses a novel datasets that allow us to study these events with arguably greater precision than prior work. Unlike prior studies that used market data to examine the impact of anti-piracy interventions on sales alone, we obtained panel data on the actual behavior of a large group of Internet users in the UK. As such, we are able to determine the effect of these website blocks on not only legal purchasing activity, but also dispersion to other unblocked piracy websites and on the use of technologies that can circumvent the blocks.

These data show that the blocking of The Pirate Bay, one of the largest BitTorrent sites in the UK caused was associated with only a small decrease in total piracy and caused no increase in the adoption of legal distribution services for digital movies and television. The data suggest that former Pirate Bay users merely switched to unblocked “proxy” sites that mirrored the contents of The Pirate Bay or dispersed to other filesharing websites to consume media illegally.⁵ As we will note later, paid legal streaming services were relatively nascent during this block.

However, our data suggest that when nineteen major piracy websites were simultaneously blocked in October-November 2013, the results were different. Here we observed a strong decrease in total piracy levels as a result of these blocks and we also find that these blocks caused users of the blocked sites to increase their usage of paid legal streaming sites by 12%. The lightest users of the blocked sites (and thus the users least affected by the blocks, other than the control group) increased their clicks on paid streaming sites by 3.5% while the heaviest users of the blocked sites

⁴ Actually, 28 sites were ordered blocked during this period of time. However, 9 of them were music-only piracy sites, and this paper focuses on video content, which was accessible through only 19 of these sites. Thus, from this point on we will refer to the 19 site blocks in October-November 2013.

⁵ In March 2015, Aguiar, Peukert, and Claussen reported a similar result in the context of shutting down a major piracy linking site in Germany, kino.to. Their methodology was similar to our own and their results are largely consistent with our Pirate Bay result, as they found that while prior users of kino.to decreased their total piracy to a degree, many of them substituted to other piracy sites and the event only caused a modest increase in usage of paid licensed services.

increased their paid streaming clicks by 23.6%. Thus, our results show website blocking may have a significant impact on legal consumption when multiple sites are blocked at once and when legal digital services are well-developed and convenient. We discuss the explanations for and implications of these results in the conclusion of this paper.

2. Background on the Film Industry and Website Blocking

The film industry is a significant force in the world economy, with \$35.9 billion in total theatrical revenue in 2013.⁶ However, the advent of the BitTorrent filesharing protocol in 2003 led to a rapid spread of Internet movie piracy, and several studies (cited and discussed in section III) have causally linked this widespread piracy with significant lost revenues in the box office, home theater, and digital film markets.

The industry has reacted to this threat by changing their distribution strategies in a variety of ways. For example, Danaher and Waldfogel (2012) show that since the advent of BitTorrent, movie studios have steadily decreased the windows between the US box office premiere of a movie and the international premieres. Similarly, Danaher et al. (2010) and Danaher et al. (2015) demonstrate that making content available on legal digital channels, such as iTunes and Hulu, can reduce the incidence of piracy for that content as some consumers switch from piracy to legal consumption. In addition to changing their business strategies in an attempt to make legal consumption more attractive than piracy, the film and television industries have also attempted to make pirated content less attractive than legal consumption by supporting various government anti-piracy interventions such as the shutdown of Megaupload.com and Megavideo.com.

⁶ http://www.mpa.org/wp-content/uploads/2014/03/MPAA-Theatrical-Market-Statistics-2013_032514-v2.pdf

Recently many governments and courts worldwide have adopted “website blocking” as an additional anti-piracy approach. For example, the UK has used website blocking to fight piracy since October 2011 when British Telecom and five other UK ISPs⁷ were ordered by the High Court to block their customers from accessing Newzbin2, an indexing site for binary files posted to the Usenet. Following the Newzbin2 precedent, as of April 2015, over 125 copyright infringing sites were subject to court-ordered blocks in the UK.

Website blocking of this sort may be an attractive alternative strategy to graduated response laws and site seizures because, unlike graduated response laws it does not involve the legal and regulatory overhead necessary to adjudicate copyright claims against individuals, and unlike site seizures it does not involve cross-country cooperation for non-domestic websites. Instead, website blocking involves implementing requirements for domestic ISPs to block access to domain names or IP addresses that have been shown to facilitate access to copyright infringing content.

Our present analysis concerns UK blocks that occurred in 2012 and 2013. Specifically, in April 2012 five major UK ISPs were ordered by the court to block access to The Pirate Bay, a major website for indexing the tracker files necessary to gain access to pirated media files through BitTorrent.⁸ The Pirate Bay reportedly had 3.7 million users in the UK, and the record labels claimed that this site made about \$3 million in October 2011 alone from advertising revenues.⁹ Later, in October and November 2013, these five ISPs were ordered to block access to 19 piracy websites that provided access to copyrighted video content.

These orders, as well as other instances of mandated piracy website blocking around the world, were initially met with some degree of controversy, as detractors claimed that this was

⁷ Specifically the ISPs Everything Everywhere, Sky, TalkTalk, Telefónica and Virgin Media.

⁸ BT was subsequently ordered to block The Pirate Bay in June 2012.

⁹ <http://www.theguardian.com/technology/2012/apr/30/british-isps-block-pirate-bay>

opening the door to censorship of content on the Internet. This paper does not attempt to evaluate such claims – rather, our purpose is to understand the impact of piracy website blocking on user behavior, and to extend the prior literature by using granular consumer level dataset for our analysis.

It is important to note that, from a theoretical perspective, website blocking may have a different impact than site seizures because, given that the site is still operational and “connected” to the Internet and that the hosted content is still available, technically sophisticated users may be able to find ways around the ISP-level block through Virtual Private Network services or through proxy server sites. For example, if a court orders an ISP to block access to a particular domain, say ThePirateBay.com, operators of the blocked website may set up a “proxy server” at a different domain that links users to the same content on the blocked site — for example, ThePirateBay.se. Even if the ISPs are ordered to block all future incarnations of the site in question (as is the case in the UK), there may still be some time between the introduction of a new domain and the ISPs recognition of it as a proxy to a blocked site. Thus, website blocking has been compared to the game “whac-a-mole,”¹⁰ implying that it will be ineffective at increasing legal consumption as authorities or ISPs will be unable to keep up with agile piracy websites that are able to move domains and set up proxy servers more quickly than authorities can order those domains blocked or more quickly than ISPs can detect and then carry out orders that require them to block access to any future proxies.¹¹ In addition, because website blocking only blocks users from the country

¹⁰ See, for example, Nick Bilton’s New York Times August 2012 editorial titled “Internet Pirates Will Always Win.” (<http://www.nytimes.com/2012/08/05/sunday-review/internet-pirates-will-always-win.html>)

¹¹ See for example <http://www.theguardian.com/world/2014/sep/10/blocking-copyright-infringing-websites-derided-whacking-moles>

in which the block was ordered, pirates can use Virtual Private Network (VPN) services to bypass the block by appearing to be connecting from a different country.

Nonetheless, investing the time and money involved in finding new domains (and knowing whether to trust them) or purchasing and learning how to use VPN services may come at some cost to the user. In this regard, prior research has demonstrated that actions that make legal content more attractive to users or make illegal content less attractive to users can convert pirates into paid consumers (see Danaher et al. 2014 for a summary of such studies). Thus, website blocking may still be effective in changing consumer behavior if the potential workarounds have a sufficient level of inconvenience or sufficiently high learning costs. As well, the effectiveness of website blocking may depend on the attractiveness of legal alternatives. We explore these hypotheses below in the context of the two UK blocking events described above: the blocking of The Pirate Bay in May 2012, and the near-simultaneous blocking of nineteen major piracy sites in October-November 2013.

3. Literature Review

Our study fits into several streams of the academic literature. First, there is a significant body of work on the relationship between piracy and sales of video content, including Rob and Waldfogel (2006), Smith and Telang (2010), Danaher et al. (2010), Zentner (2012), Danaher and Waldfogel (2012), and Ma et al. (2014).¹² The vast majority of this literature finds evidence of sales displacement caused by piracy across a variety of media types, including the consumption of television content, DVDs, and box office attendance.

¹² We refer the interested reader to Danaher et al. (2014) for a thorough review of this literature.

Second, scholars in the information system and economics disciplines have begun to ask how government anti-piracy interventions can impact consumer behavior and revenues from legal media markets. Bhattacharjee et al. (2008) found that the RIAA's highly publicized lawsuits against music pirates had a significant negative impact on the availability of pirated content, though a substantial amount of infringing content remained available even after the lawsuits. Danaher et al. (2014) found that the French graduated response anti-piracy law "HADOPI" increased digital music sales for the major labels by around 25%. Danaher and Smith (2014) found that the shutdown of the popular piracy cyberlocker Megaupload.com increased digital movie revenues by 6-8%. Adermon and Liang (2014) demonstrated that the Swedish IPRED directive increased total music sales by 36% after being passed, but that sales reverted back to original levels within 6 months, possibly due to a lack of enforcement. In a yet unpublished study, Peukert, Claussen, and Kretschmer (2014) suggest that the shutdown of Megaupload led to a decrease in sales of smaller, independent films. Finally, in perhaps the closest study to our own, Poort et al. (2014) used survey data to study the impact of the Dutch courts' order to Internet Service Providers (ISPs) to block Dutch access to The Pirate Bay and related sites. They find little impact on total piracy activity.

Our study contributes to the literature in several ways. First, we look at the consumer behavior using consumer level data, an approach which has been missing in the prior literature. One notable advantage of this approach in our context is that it allows us to observe consumers' use of a number of alternative channels after they are blocked from specific piracy sites. Second, in the literature only the Poort et al. paper studies website blocking activities. Unlike their paper, we study not only piracy the effect on piracy activity but also changes in legal consumption and how pirates choose to continue pirating after the website is blocked. Third, we study both the

blocking of a single site (The Pirate Bay) and the subsequent nearly simultaneous blocking of nineteen different popular piracy sites. Our findings corroborate those of Poort et al. for the blocking of The Pirate Bay, but contrast them during the nineteen website block, allowing us to draw inferences as to when and how website blocking may be effective. Finally, due to our novel use of Internet consumer panel tracking data, we are able to utilize an identification strategy that might be employed to study a number of other natural experiments regarding behaviors on the Internet.

4. Data

Unlike previous studies analyzing the impact of antipiracy interventions, which relied on aggregate market data to analyze only the impact on legal activity, our analysis utilizes a novel Internet user dataset to better understand how consumers react to such interventions. We obtained data from an anonymous Internet consumer panel tracking company, which we refer to as PanelTrack in this paper.¹³ While PanelTrack could not provide us data at the consumer level, they were able to provide aggregate data for groups of consumers defined based on observed behavior. We requested that PanelTrack define the groups by sorting consumers based on their pre-block usage of the blocked sites. For studying the blocking of The Pirate Bay, consumers were sorted into ten different groups based on their total number of visits to The Pirate Bay during March, two months before the block. Similarly, for studying the blocking of nineteen sites in October/November 2013, consumers were sorted into ten different groups based on their total visits to any of the nineteen different blocked sites during August, two months before the block. Thus, for each event that we study, we have ten different consumer segments, each of which we

¹³ Despite their requirement to remain anonymous in our study, this tracking company is one of several leaders in the field and their data has been used in other peer reviewed papers to study the behavior of consumers on the Internet.

observe for seven months surrounding their respective blocks. For The Pirate Bay, we observe each segment from February through August 2012, and for the nineteen-site block, we observe each segment from August 2013 until February 2014.

For each month-segment, we observe the following outcome variables: visits to the blocked sites, visits to mirrors of the blocked sites, visits to other unblocked torrent sites, visits to cyberlockers and streaming piracy sites, visits to VPN sites, and visits to paid legal streaming sites (such as Netflix and Viewster). Thus we can observe how each consumer segment changes their behaviors over time, both before and after the blocks.

For The PirateBay block, Table 1 provides mean visits to each site during February, March, and April (the months before the block) for each of the consumer segments.

TABLE 1 – DESCRIPTIVE STATISTICS (PIRATEBAY BLOCK)

Consumer Segment	% of Sample in Group	Pre-block Pirate Bay Visits Per User	Unblocked Torrent Sites (1000's)	Piracy Cyberlockers (1000's)	VPN Sites (1000's)	Paid Legal Streaming (1000's)
0	N/A	0	59,735	51,362	1,507	7,094
1	9%	1	1,167	949	27	79
2	12%	2.5	1,737	1,113	26	97
3	13%	5	1,570	718	19	52
4	11%	8.2	1,421	603	13	179
5	10%	13.4	2,122	687	14	189
6	11%	20.8	1,568	675	23	155
7	10%	36	1,367	554	19	85
8	10%	67.9	1,721	493	36	65
9	14%	226.3	2,907	559	21	96

Note that we report the percent of the treated sample in each treated segment and that these are relatively equal. However, our data show that the control group makes up over 90% of the total sample, as most users were not accessing the Pirate Bay in the months before the block. Although piracy sites are substitutes for one another, the heaviest users of The Pirate Bay were also

disproportionately heavy users of other torrent sites, even before the block. On the other hand, heavier users of The Pirate Bay (a torrent site) were lighter users of cyberlocker sites, which may imply that pirates tend to stick to a particular protocol/method for filesharing. Interestingly, the heaviest users of legal streaming sites are actually the mid-tier users of The Pirate Bay. We note that legal streaming sites were still nascent at this time. For example, Netflix, now the largest legal streaming site in the UK, had just been introduced in January 2012.

Table 2 reports the same statistics but for the consumer segments that we used to study the blocking of nineteen sites in October/November 2013.

TABLE 2 – DESCRIPTIVE STATISTICS (19-SITE BLOCK)

Consumer Segment	% of Sample in Group	Pre-block Visits/User to Blocked Sites	Unblocked Torrent Sites (1000's)	Piracy Cyberlockers (1000's)	VPN Sites (1000's)	Paid Legal Streaming (1000's)
0	Unknown	0	26,452	25,744	1,555	53,863
1	24%	1.0	589	490	9	1,488
2	13%	2.0	394	343	8	695
3	9%	3.0	454	368	3	771
4	7%	4.0	208	217	45	323
5	9%	5.4	272	542	29	479
6	10%	8.2	486	494	10	614
7	9%	13.2	651	673	11	607
8	10%	23.8	624	753	23	422
9	9%	66.4	719	1,927	23	956

Again we report the percent of treated users in each treated segment. In this case, the control segment makes up about 95% of the sample, indicating that most internet users in the sample were not users of these 19 piracy sites in the months before the blocks. The heavier users of the 19 blocked sites were also heavier users of other torrent sites. However, in this case, they were also heavier users of cyberlocker piracy sites, which may be because the 19 blocked sites included 8 non-torrent sites. Finally, visits to paid legal streaming sites are much higher for each of the

segments than during the Piratebay blocks. This is likely due to the increased diffusion of the largest streaming sites (such as Netflix).

It is worth noting that the data from PanelTrack do show that both blocking injunctions – The Pirate Bay and the nineteen-site block – were effective in drastically reducing traffic to the blocked sites. Total visits to The Pirate Bay across all treated groups dropped by nearly 90% in the 3 months after the block as compared to the three months before.¹⁴ Total visits by the treated groups to the nineteen sites after the November blocks dropped by 83%.

One might ask why the drop was not 100% if the sites were blocked. One possible reason is that although we dropped the month the blocks were ordered from the analysis, we do not know exactly when each ISP implemented the blocks. Thus, it is possible that some Internet users had access to some of the blocked sites even in the early part of the post period. Additionally, to the degree that users circumvented the blocks by using a VPN or similar measure, this may continue to show up in our data as visits to the blocked sites even after the block was enforced. Finally, some smaller ISP's in the UK may not have participated in the blocks. Nonetheless, it is quite clear that the blocking injunctions caused major decreases in total visits to the blocked sites from all consumer segments, and thus these events constitute meaningful experiments with which to determine the impact of website blocking on consumer behavior.

In the next section, we present our empirical model to analyze these experiments and determine their causal effect on consumer behavior.

¹⁴ We ignore May itself, since the blocks occurred mid-month.

5. Empirical Model and Results

The Pirate Block in May 2012: We first turn our attention to the blocking of The Pirate Bay in 2012. Recognizing that changes in outcome variables, such as use of paid streaming channels or use of other piracy sites, might change over time for reasons other than the block, we identify the causal impact of the block by comparing treated users (those who used The Pirate Bay before the block) with “control” users (those who did not).¹⁵ We also divide treated users into nine different groups based on their number of visits to The Pirate Bay two months before the block — thus, we will call this variable for each user group the ‘treatment intensity’ variable as it serves as a proxy for the intensity of treatment that the block had on that group. Our identification relies on asking whether treated users change their visitation to paid legal viewing sites (or other potential outcomes) more than control users do, as well as examining how the pattern of visitation changes across different levels of treatment intensity.

Before turning to regressions to make causal inference, it makes sense to visualize the data in a way that best represents our identification strategy. Figure 3 shows a scatterplot where the x-axis represents the intensity of treatment (pre-treatment Pirate Bay visits) for each consumer group and the y-axis represents the percent change in visits to paid legal streaming sites. Figure 4 then shows a similar scatterplot except that the y-axis represents the percent change in visits to other, unblocked torrent sites.

¹⁵ More precisely, we consider a user a control user if they did not use The Pirate Bay in the two months prior to the block, i.e. the users in group 0. Some of these users may have been rare users who then would have made some use of The Pirate Bay after the block (and thus were partly treated by the block). If this is the case, our results will be conservative since the control group may have been lightly impacted by the block.

FIGURE 3: TREATMENT INTENSITY VS. CHANGE IN VISITS TO PAID LEGAL STREAMING SITES

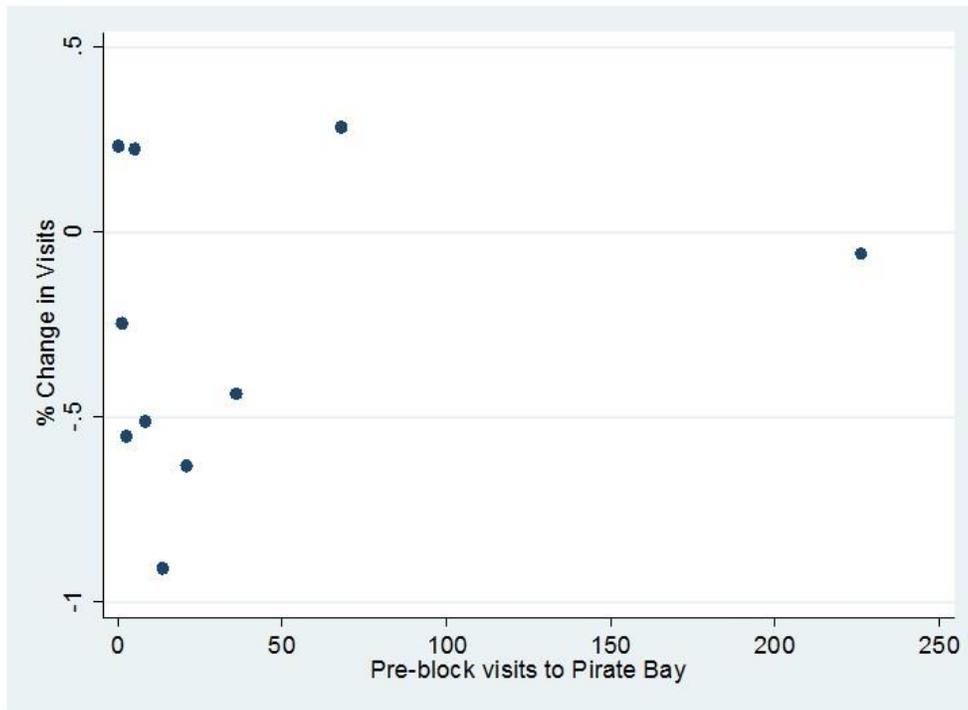
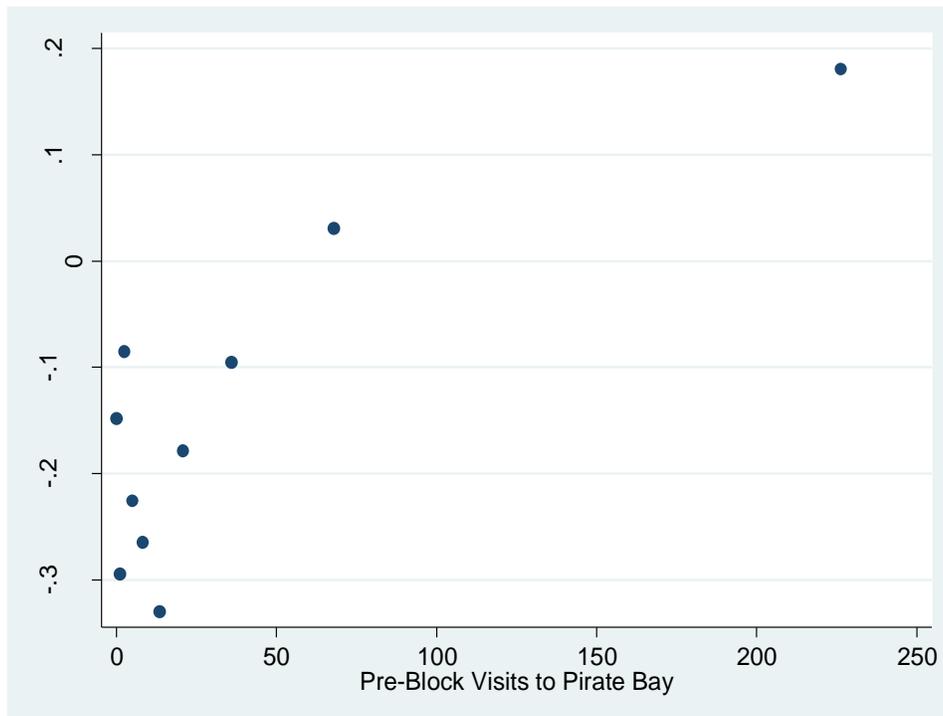


FIGURE 4: TREATMENT INTENSITY VS. CHANGE IN VISITS TO UNBLOCKED TORRENT SITES



Note the difference between these graphs. In Figure 3, while there is significant variance in the change in visits to paid legal streaming sites across consumer groups, there is no clear relationship between the change in visits and the measure of treatment intensity (we formally test this statement later in the regression analysis). Some of the groups with the lowest intensity of treatment have some of the largest increases in visits to paid streaming sites. However, in Figure 4, a more clear pattern emerges: Generally, the greater the intensity of treatment, the larger the increase in visits to other, unblocked torrent site after The Pirate Bay was blocked. These two scatterplots demonstrate the idea behind our methodology, applying a difference-in-difference estimate to our outcome variables where one of the differences is pre-treatment intensity of usage of the blocked site in question.

Having visually demonstrated the identification strategy for some of the data, we can now formally test the hypotheses of whether the block of The Pirate Bay causally impacted visits to paid streaming sites, visits to other torrent sites, visits to cyberlocker piracy sites, and visits to VPN sites.

To do this we run the following model:

$$LnVisits_{jt} = \beta_0 + \beta_1 After_t + \beta_2 TreatIntensity_j * After_t + \mu_j + \varepsilon_{jt} \quad (1)$$

where $LnVisits_{jt}$ indicates the natural log of visits (to whatever category of sites we are examining) made by consumer group j during period t . $After_t$ is a dummy variable equal to one if the month is June, July, or August. By including this variable, we control for differences between the pre-block period and the post-block period that would, on average, affect all segments evenly, such as any outside factors which increase or decrease the appeal of streaming services, VPN's, or piracy (for example, an increase in the quantity or quality of content offered on legal services). $Treatintensity_j$

indicates the number of visits that the average consumer in group j made to The Pirate Bay during March of 2012. Finally, μ_j is a vector of group fixed effects and ε_{jt} is an idiosyncratic error term. In this model, β_2 is the variable of interest and, under the assumption that each group's trend after the block would have been uncorrelated with that group's treatment intensity, it indicates the causal impact of the block on visits to sites in the outcome group in question (e.g. paid legal streaming sites).

This difference-in-difference approach should be a well-identified estimation strategy for the impact of the block on visits to other piracy channels or VPN's. However, we note that even though the highest users of The Pirate Bay were the most heavily treated by the block, they were also the heaviest users of other unblocked sites (see Table 1), which means that they had the least cost of finding alternate piracy sources after the block. As a result, they may have a lower probability of shifting their consumption to legal sites. The fact that they are the most affected by the block must dominate this secondary factor (because if you aren't impacted by the block, your probability of shifting activity to legal channels is irrelevant). But this factor implies that although the impact of the block (if it exists) should increase with treatment intensity, it may do so nonlinearly, as the highest treatment groups may only receive a small marginal impact from the block. Thus, for regressions involving the impact of the block on visits to legal sites, we also include a squared term for the variable of interest.

$$\ln Visits_{jt} = \beta_0 + \beta_1 After_{\tau} + \beta_2 TreatIntensity_j * After_{\tau} + \beta_3 TreatIntensity_j^2 * After_{\tau} + \mu_j + \varepsilon_{jt} \quad (2)$$

Table 3 below shows the results from models (1) and (2) for each of four outcome variables.

TABLE 3 – EFFECT OF PIRATE BAY BLOCK ON SITE VISITS

	Paid Streaming	Paid Streaming	Other Torrent	Cyberlockers	VPNs
After Block	-0.623+	-0.639	-0.242*	-0.370+	-1.009**
	(0.302)	(0.398)	(0.042)	(0.168)	(0.362)
TreatIntensity * After Block	0.001	0.001	0.002*	0.000	0.010+
	(0.003)	(0.019)	-0.001	(0.002)	(0.005)
TreatIntensity ² * After Block		0.000			
		(0.000)			
Constant	11.949*	11.949*	14.688*	13.866*	10.377*
	(0.185)	(0.198)	(0.026)	(0.103)	(0.221)
Observations	20	20	20	20	20
Consumer groups	10	10	10	10	10
R-squared	0.853	0.854	0.994	0.951	0.822

Robust standard errors in parentheses

p-values calculated based on a t distribution with 8 degrees freedom (# groups - 2)

+ significant at 10%; ** significant at 5%; * significant at 1%

The first two columns of Table 3 examine the impact of the block on visits to paid legal streaming channels, with the first column forcing a linear relationship and the second allowing for a quadratic curve. We note that β_2 and β_3 are close to zero and statistically indistinguishable from zero. Thus we are unable to clearly detect any increase in usage of paid legal streaming sites. In contrast, β_2 for other torrent sites (the third column) is positive and statistically significant at a 99% confidence level, indicating an increase in the use of other unblocked torrent sites caused by the block. The coefficient in the fourth column for cyberlockers is effectively zero, indicating that users of The Pirate Bay did not turn to cyberlockers as a piracy alternative after the block. Finally, in the fifth column, β_2 is measured as 0.01 (at 90% confidence), indicating a strong turn to VPN's to circumvent the block. The constant in this column is low and so the change in levels for VPN usage may not be large, but the percent change in VPN use caused by the block is the largest of any of the outcome variables. Further inspection of the data reveals that this coefficient is heavily

driven by the highest treatment intensity treatment group – the people who used The Pirate Bay over 200 times in the two months before the block. One might speculate that since these users had by far the strongest preferences for The Pirate Bay, they had the strongest incentive to find a way around the block rather than turning to legal sources for content or even other torrent sites.

In short, our regression results suggest that blocking The Pirate Bay in May 2012 caused users to gravitate toward other piracy sites or to use VPN’s to circumvent the block, but we see no indication of any increase in paid legal sources of video content from blocking just this site. We provide further support for this finding through an alternative specification in Appendix B.

TABLE 4 – EFFECT OF OCT/NOV BLOCKS ON SITE VISITS

	Paid Streaming	Paid Streaming	Other Torrent	Cyberlockers	VPNs
After Block	-0.027 (0.061)	-0.126+ (0.062)	-0.342* (0.093)	-0.377* (0.102)	0.201 (0.263)
TreatIntensity * After Block	0.006** (0.003)	0.022** (0.007)	0.006 (0.004)	-0.006 (0.004)	0.027** (0.011)
TreatIntensity ² * After Block		-0.0003** (0.0001)			
Constant	13.815* (0.036)	13.815* (0.028)	13.436* (0.055)	13.575* (0.060)	9.997* (0.155)
Observations	20	20	20	20	20
Consumer groups	10	10	10	10	10
R-squared	0.993	0.996	0.983	0.981	0.915

Robust standard errors in parentheses

p-values calculated based on a t distribution with 8 degrees freedom (# groups - 2)

+ significant at 10%; ** significant at 5%; * significant at 1%

19-Site Block in October/November 2013: We now turn our attention to the second event in our study: the blocking of 19 different video piracy websites within a thirty day period between

October and November of 2013. We again estimate the empirical model using OLS regression and report the results in Table 4.

In this case, the results are noticeably different than those for the single block above. First, there is a statistically significant increase in use of paid legal streaming sites, which if we assume that the segments should have trended similarly if not for the block, can be attributed to a shift toward legal channels caused by the block. An individual who made 10 visits to the blocked sites in the month before the block subsequently increased his visits to legal streaming sites by 21% more than an individual who didn't use the blocked sites, based on the second column.¹⁶ But the impact is non-linear – an individual who made 40 visits to the blocked sites in the month before the sites were blocked increased his visits to legal streaming sites by 49% more than a non-user of the blocked sites.¹⁷ This diminishing marginal increase in treatment intensity may imply that the heaviest pirates are somewhat more resistant toward turning to legal channels in reaction to the blocks. However, note that within the range of treatment intensity (pre-block visits to blocked sites) in the data, the relationship between treatment intensity and post-block visits to legal sites never turns negative, indicating that all levels of treatment groups increased their legal consumption by more than the control group. Appendix B contains an alternate specification that shows that higher treatment intensity was correlated with larger drops in total piracy and corroborates our result that this leads to an increase in paid legal streaming.

In the third and fourth columns we measure no statistically significant changes in visits to other unblocked torrent sites or to cyberlockers sites, although the point estimates indicate some

¹⁶ $(\exp(0.022*10 - 0.0003 * 10^2) - 1) * 100 = 20.9$

¹⁷ Note that even in the first column, which forces a linear relationship between treatment intensity and change in visits to paid streaming sites, the coefficient of interest is still positive and statistically significant. However, it is clear from the higher t-values and higher R^2 that a quadratic model is a better fit, and so we use this model for our calculations.

shifting of blocked consumers to unblocked torrent sites. Finally, we observe that the blocks caused a statistically significant increase in use of VPN services, much like The Pirate Bay block did.

TABLE 5 – PLACEBO TEST AUGUST TO SEPTEMBER 2013

	Paid Streaming	Paid Streaming	Other Torrent	Cyberlockers	VPNs
After Block	-0.256*	-0.2160+	-0.226*	-0.314*	-0.405**
	(0.078)	(0.106)	(0.070)	(0.069)	(0.142)
TreatIntensity * After Block	0.0003	-0.006	-0.003	-0.003	-0.002
	(0.003)	(0.015)	(0.002)	(0.003)	(0.006)
TreatIntensity ² * After Block		0.0001			
		(-0.0002)			
Constant	13.921*	13.921*	13.626*	13.800*	10.176*
	(0.046)	(0.048)	(0.040)	(0.041)	(0.084)
Observations	20	20	20	20	20
Consumer groups	10	10	10	10	10
R-squared	0.989	0.988	0.991	0.991	0.973

Robust standard errors in parentheses

p-values calculated based on a t distribution with 8 degrees freedom (# groups - 2)

+ significant at 10%; ** significant at 5%; * significant at 1%

Measuring an increase in visits to paid legal streaming sites in this case is important, and deserves further exploration. Our coefficients should only be interpreted as causal impacts if each segment’s trends would have been uncorrelated with treatment intensity if not for the block. Thus, before the block, we should observe no correlation in the month-to-month trend in visits and the treatment intensity variable. In Table 5 we run the same model, except that we consider only August in the pre period and only September in the post, thus “pretending” as if the block happened at the end of August. This is essentially a placebo test: a test to show that pre-existing trends are uncorrelated with treatment intensity.

The placebo test yields the expected results: not a single one of the B_2 's are statistically significant at even the 90% confidence level. Further, the coefficients are generally much smaller than those measured in Table 4 (studying the true period of the 19 blocks). Thus, at least in the two months before the blocks occurred, there was no relationship between each group's time trend in visits to each category of sites and that group's treatment intensity. The correlation appears only after the blocks happened, lending considerable strength to the causal interpretation of the results.

Table 6 – Estimated Causal Increase in Visits to Paid Legal Streaming Sites

Consumer Segment	Pre-block Visits/User to Blocked Sites	Causal Increase in Visits to Legal Sites
0	0	0.0%
1	1.0	2.2%
2	2.0	4.4%
3	3.0	6.5%
4	4.0	8.7%
5	5.4	11.7%
6	8.2	17.5%
7	13.2	26.8%
8	23.8	42.4%
9	66.4	14.8%

If we consider the estimates from the second column in Table 4 to measure the causal influence of the blocks on each segment's usage of paid legal streaming sites, we can then say that the blocks increased the natural log of visits to legal sites for a segment by $0.022x - 0.0003x^2$ where x is the treatment intensity (or pre-block visits to blocked sites per user) of the segment. Thus, the causal percent increase in visits to legal sites for a segment is $[\text{EXP}(0.022x - 0.0003x^2) - 1] * 100$. Table 6 shows these estimated causal increases in legal streaming site visits for each segment.

These figures are consistent with what we observed in the raw data – the greater the treatment intensity, the larger the increase in visits to paid legal streaming sites after the block. The exception is the heaviest group of pirates (segment 9), who still increase paid streaming more than the control group, but who show some resistance toward migrating to legal channels.

6. Discussion

While the use of website blocking has increased in recent years as a tool in the fight against intellectual property theft, ours is the first study we are aware of that analyzes their effectiveness in changing consumer behavior. We use data provided by a panel tracking company to analyze the impact of two website blocking events in the UK: The blocking of The Pirate Bay in May 2012 and the blocking of 19 additional sites in October and November of 2013.

Our results suggest that blocking The Pirate Bay in May 2012 led to an increase in the usage of other unblocked torrent sites and of VPN sites, causing only a small decrease in total piracy and having no statistical impact on legal entertainment sites. However, the blocking of 19 different sites in October and November of 2013 led to significant decreases in total piracy and caused a statistically significant increase in the usage of legal streaming sites: The more a consumer visited the blocked sites in the months leading up to the block, the more they increase their visits to legal sites after the block. This pattern was not observed in the months before the block, suggesting that this impact is causally related to the block.

A natural question to ask is: what was the total impact of the blocks on the use of legal streaming sites. Table 6 showed the percent increase in legal streaming for each consumer segment. For each segment, we can also start with the total post-block visits to paid legal streaming sites for that group and determine what the counterfactual visits would have been if the blocks had not happened (which is equivalent to estimating what would have happened if the treatment intensity

variable were 0). The difference between observed visits and counterfactual visits in the post-period is the causal impact of the block on visits for that segment.

If we combine groups 1, 2, and 3 (the lightest users of the blocked sites), our model suggests that the blocks caused users in these groups to increase their clicks on paid legal streaming sites by 3.5% on average. Combining groups 7, 8, and 9 (the heaviest users of the blocked sites), we find that the blocks caused them to increase their use of paid legal streaming sites by 23.6%. If we consider all users of the blocked sites together, we estimate that the blocks caused them to increase clicks to paid streaming sites by 12%.

We also note that these estimates are conservative in a variety of ways. Recall that after the blocks occurred we observed less than a 100% decrease in visits to blocked sites, which might be attributed to small non-participating ISP's, delays between the ordering of the blocks and their implementation, or visits to the blocked sites through VPN's. To be conservative, we do not attempt to correct our estimates for non-participating ISPs or VPN-based circumvention.

Notably, the blocks did cause some users to increase visits to VPN's, and may have caused dispersion of piracy to other torrent sites, though this latter finding was not measured with statistical significance. But while some people may circumvent the blocks, it is clear from the data that in addition to reducing visits to blocked sites, site blocking caused some former pirates to migrate their consumption toward legal channels. Importantly, because we are examining groups of individuals, we cannot determine the degree to which this increase comes from new consumers turning to these legal channels or increased usage of these legal channels by existing users. This remains an interesting question for future research.

One might ask why the 19 site blocks in November 2013 caused an increase in paid legal streaming but the Pirate Bay block in May 2012 did not. There are two explanations, each of which

appears to explain part of this finding. First, when only one site is blocked, it remains somewhat easy to find a reliable substitute piracy site, but when 19 sites are blocked finding an alternative requires substantially more effort. This explanation is supported by the fact that the Pirate Bay block caused a statistically significant increase in the use of unblocked piracy sites, while the November blocks showed only a statistically insignificant increase. Also, in Appendix B we show that the November blocks were more effective in decreasing total piracy levels than the Pirate Bay block was. Second, paid streaming channels were nascent and less well developed in May 2012 than they were in November 2013, and thus they may not have presented as viable/attractive an alternative. This explanation is supported by the fact that pre-block usage of paid legal services was much lower in May 2012 than it was in November 2013, and also by the fact that the Pirate Bay block caused no increase in paid legal streaming despite causing some reduction in piracy (see Appendix B). In other words, it appears that antipiracy enforcement is more effective when consumers have access to (and are more aware of) attractive legal alternatives.

In addition to the finding that blocking only one website was ineffective at increasing legal consumption while 19 site blocks did increase legal consumption, our results demonstrate that piracy does indeed displace usage of legal paid streaming sites, despite the relative convenience and low cost of such sites. This implies the flip side of our previous statement - making legal content available through more convenient channels is more effective when accompanied by policies that make illegal content less attractive.

There are several limitations to this study. First, we only study a one site blocking injunction and injunctions for blocking nineteen sites. Further study of additional blocking actions (including different numbers of sites) would be valuable to verify the conclusion that the number of sites blocked is a meaningful moderator of the impact and potentially isolate this from the effect

of more highly developed legal alternatives. Second, while we have shown that the heaviest pirates are more reticent to turn to legal channels (as observed in the non-linearity of the impact across segments), we can only suggest potential explanations for this and future work should delve further into the reason for this diminishing marginal impact. Third, the increase we observe in the use of paid streaming sites cannot be broken down into new users or increased usage from existing users, and separating these two effects may have important managerial implications, as converting an individual from piracy toward a new legal streaming service may have different implications than causing a pirate to increase usage of a service to which he was already subscribed. Fourth, we have used paid legal streaming sites in the UK as a proxy for increasing legal consumption behavior. Other legal channels exist in the UK such as paid digital downloads (on iTunes, for example) or DVD/Blu-Ray purchases, and understanding the impact of site blocks on these channels would be of interest to managers and policymakers as well. Data on such behavior were not available to us for this particular study. Finally, we are not able to fully estimate the social welfare implications of these blocks, because our data do not allow us to estimate the value of the impacts (just their relative sizes) or the costs of implementing the blocks, and because we have no data on the impact of increased profitability on industry output. Future work should focus on these issues to obtain a better understanding of the broader impacts of site blocking and other anti-piracy measures.

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**Appendix A – List of Piracy Sites (Allowing Access to Pirated Video Content)
Blocked in October-November 2013**

1. YIFY-torrents
2. FTVO
3. vodly.to
4. primewire.ag
5. watchfreemovies.ch
6. 1337X
7. Bitsnoop
8. Extratorrent
9. Monova
10. Torrentcrazy
11. Torrentdownloads
12. Torrenthound
13. Torrenreactor
14. Torrentz
15. Filecrop
16. Filestube
17. Rapidlibrary
18. solarmovie.so
19. tubeplus.me

Appendix B - Alternate Treatment Intensity Measure

Although the identification for the effect of the blocks on visits to alternate piracy sites and VPN sites appears valid, one worry may be that because more intense users of the blocked sites were more likely to turn to VPN's and alternate piracy sites, we should not expect them to have a larger increase in usage of legal sites in spite of their heavier treatment condition. In the main body, we attempted to account for this effect by adding the squared interaction term ($\text{after}_t * \text{treatintensity}^{2j}$). We justified this by stating that the somewhat lower likelihood of converting to legal only matters when an individual is actually treated, and so the effect of the higher treatment intensity should dominate the mitigating effect of a lower likelihood of legal conversion for high intensity pirates. It is also possible that even when blocked, the heaviest pirates are simply less likely to be willing to convert consumption toward legal sources, even if they don't find an illegal substitute. In short, heaviest users of the blocked sites were conceptually treated more intensely than lighter users, but one might ask whether they were truly treated with a larger decrease in total piracy (including blocked sites, unblocked sites, and VPN workarounds) and whether this treatment resulted in a larger increase in legal visits or not.

To ask if the intended treatment actually resulted in a true decrease in total piracy, we can plot the per capita change in total piracy visits¹⁸ against the treatment intensity (pre-block visits to blocked sites in the month before the block). This comparison is shown graphically in Figures 1B and 2B below.

¹⁸ We divide total change in piracy visits by the number of people in each segment to account for the fact that each segment has a different number of people.

Figure 1B: Per Capita Total Piracy Change Vs. Treatment Intensity – Pirate Bay Block

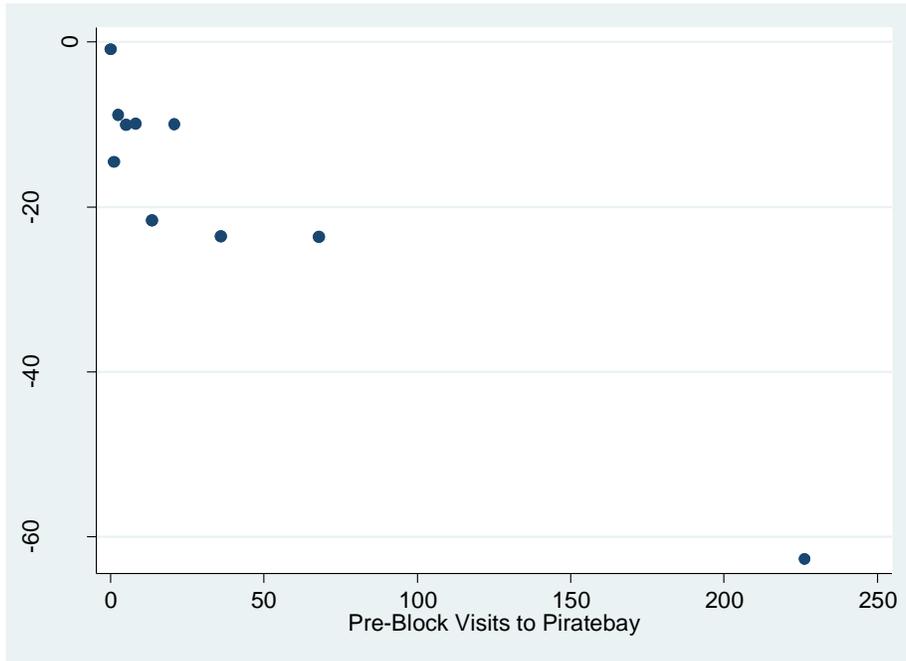
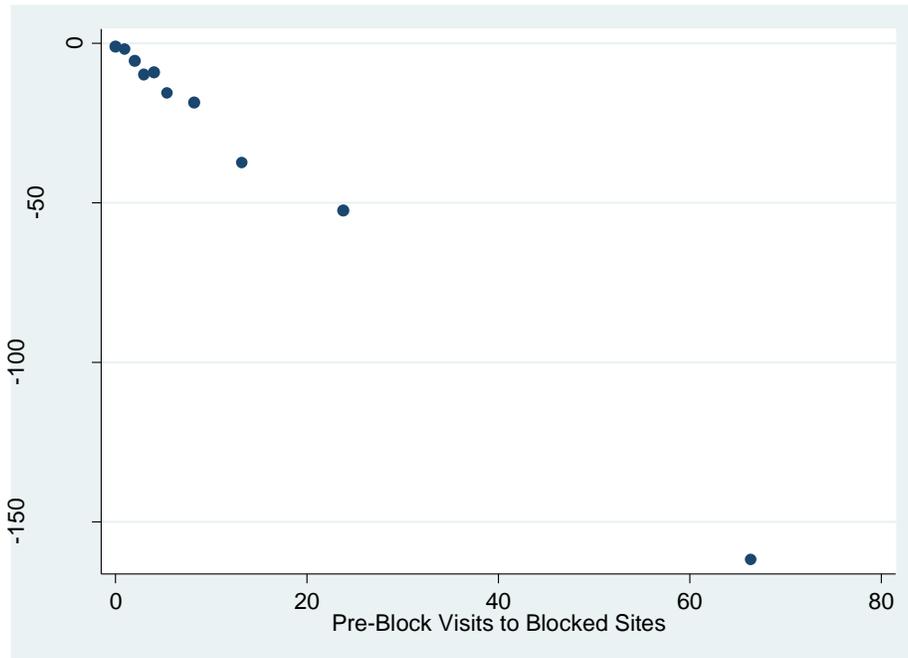


Figure 2B: Per Capita Total Piracy Change Vs. Treatment Intensity – Oct/Nov Blocks



Clearly there is a very strong relationship here – the more heavily a group was using the blocked sites in the month before the blocks, the larger their decrease in total visits to all piracy sites in the three months after the blocks. However, the magnitudes are different. A regression of the per capita total piracy change on treatment intensity for the Piratebay block yields a coefficient of -0.24 ($p=0.01$). Treatment intensity is the pre-block visits to Piratebay for one month, but the per capita total piracy change compares the three months after to the three months before. This means that 3 blocked visits to Piratebay (assuming one per month) was correlated with only 0.24 fewer piracy site visits in the post period, implying that many people substituted to other piracy channels. This is in line with our results from Table 3, which showed that the Piratebay block caused an increase in usage of other piracy sites.

In contrast, a regression of per capita total piracy change on treatment intensity for the October/November blocks returns a coefficient of -2.41 ($p=.01$). Thus, 3 blocked visits was associated with almost 2.5 fewer visits to piracy sites in the post period, suggesting only limited displacement to other piracy sites (which is in line with our statistically insignificant result for unblocked piracy sites in Table 4). In other words, in both cases the attempted treatment – blocking access to certain piracy sites – was correlated with an effective treatment of reduced piracy. The treatment was effective in decreasing piracy. But in the case of blocking just The Pirate Bay, the reduction in total piracy was significantly smaller than the October/November blocks.

Given this, we can then ask whether the per capita change in paid legal streaming site visits correlates with the per capita change in total piracy visits.

Figure 3B: Per Capita Legal Change vs. Per Capita Piracy Change – Pirate Bay Block

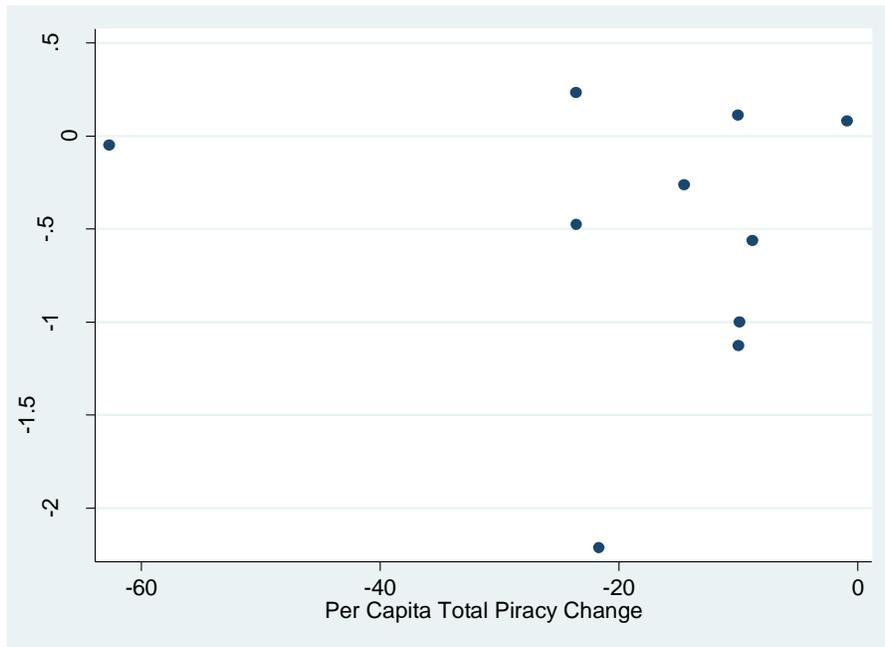
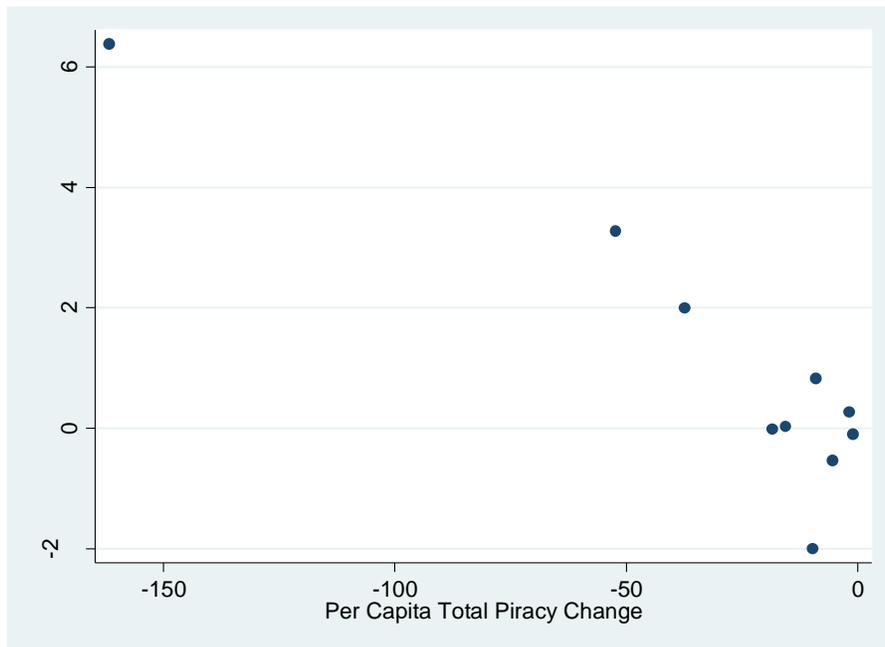


Figure 4B: Per Capita Legal Change vs. Per Capita Piracy Change – Oct/Nov Bay Blocks



In figure 3B, we see no meaningful relationship between the per capita total piracy change and the change in visits to paid streaming sites, just as in the results section we saw no significant

causal increase in paid legal streaming resulting from the Pirate Bay block. However, in 4B we observe a strong negative relationship between the per capita change in visits to legal streaming sites and the per capita total piracy change. The more that a group decreased their piracy, the more that they increased their visits to legal sites.

We also note that the relationship does not appear perfectly linear in that the group with the largest piracy change appears to have increased their legal usage by less than would be predicted by the linear relationship between the other nine segments. Ignoring this non-linearity, a regression of the change in per capita legal visits on the change in per capita total piracy yields a coefficient of $-.044$, statistically significant at a 99% confidence level. This means that, on average, an individual who decreased his visits to piracy sites by 100 visits increased his visits to the paid legal streaming sites in our study by 4.5. Importantly, this does not represent the percent of illegal downloads that displace sales for two reasons. First, we are tracking visits, not downloads or views. It may be that each additional visit to a legal streaming site results in more than one view of a movie or television show. Second, although our measure of total piracy tracks visits to all of the major piracy sites of which we are aware, our measure of legal consumption includes only visits to a few major paid streaming sites. It does not include legal but unpaid (ad-supported) streaming sites, nor does it include direct download sales or rentals, such as at the iTunes or Amazon stores. These may have increased as well, such that the effect of an individual's visits to piracy sites by 100 would be to increase visits to legal sites by more than 4.5.

Nonetheless, the results in this appendix are consistent with the results in the body of our paper. The blocking of The Pirate Bay was associated with a decrease in total piracy, but many of the blocked visits dispersed to other piracy sites. There was no corresponding increase in paid legal streaming. The October/November blocks also decreased piracy and much more effectively, and

this led to a meaningful increase in paid legal streaming. The fact that the October/November blocks more effectively decreased piracy suggests that blocking more sites has a greater impact than blocking fewer. However, the fact that the Pirate Bay block led to no increase in paid legal streaming despite having a negative impact on piracy suggests that the relatively newness (and low awareness) of legal streaming channels in the UK at the time of the block was a factor. A year and a half later, paid legal services were more widely known, leading blocked users of the 19 piracy sites to migrate some of their consumption to these channels.

**WEBSITE BLOCKING REVISITED:
THE EFFECT OF THE UK NOVEMBER 2014 BLOCKS ON CONSUMER BEHAVIOR**

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**WEBSITE BLOCKING REVISITED:
THE EFFECT OF THE UK NOVEMBER 2014 BLOCKS ON CONSUMER BEHAVIOR**

ABSTRACT

Whether and how copyrights should be enforced in the digital age has become an important policy question and an important question for empirical research. In a prior study, we found that the court ordered blocking of the Pirate Bay website in the UK in April 2012 had only a small impact on total piracy and no impact on paid legal streaming, but that the blocking of 19 major piracy websites in November 2013 caused a significant decrease in total piracy and a significant increase in usage of paid legal streaming sites.

In this update, we ask whether the blocking of 53 piracy websites in the UK in November 2014 — which more than doubled the total number of sites being blocked in the country — had an impact on consumer behavior and how that impact compared to the previous blocks. We found that these blocks caused a 90% drop in visits to the blocked sites while causing no increase in usage of unblocked sites. This led to a 22% decrease in total piracy for all users affected by the blocks (or a 16% decrease across all users overall). We also found that these blocks caused a 6% increase in visits to paid legal streaming sites like Netflix and a 10% increase in videos viewed on legal ad-supported streaming sites like BBC and Channel 5.

The evidence suggests that blocking large numbers of sites can still “move the dial” in terms of consumer behavior, but that there may be diminishing returns as remaining pirates may be more dispersed or else have lower willingness to pay for legal content. Nonetheless, such blocks can serve to mitigate the possibility of a long-term return to the prior status quo.

Keywords: *Piracy, regulation, digital distribution, motion picture industry, natural experiment.*

1. Introduction

In recent years, website blocking has become a primary tool in the attempt to mitigate the impact of online piracy on media sales. This study analyzes the effect of the UK court ordered blocking of 53 different piracy sites in November 2014 on consumer behavior across legal and illegal media channels. It is intended as an update to earlier work on website blocking in the UK.

Blocking access to sites that enable copyright infringement has the potential to make it harder for consumers to find pirated copies of media goods and thus it may diminish the appeal of illegal sources relative to legal ones. However, due to the vast supply of illegal sources for copyrighted works online, it is not always clear if blocking a particular website or set of websites will actually diminish the appeal of piracy. For example, if a consumer wants to pirate a copy of *The Avengers* movie and she knows three illegal sites from which she can reliably obtain a copy, blocking access to one of those sites may have no impact on her behavior. However, if all three sites are blocked, she has the choice of paying a cost (i.e. facing a search cost or a learning cost) to discover new reliable illegal sources or paying the legal price and easily obtaining a legal copy from a trusted source.¹

Prior theoretical work has established that indeed, when supply side piracy interventions (such as website blocking) are enacted, there are several possible equilibrium outcomes. From the perspective of theory, when the intervention is not strong enough, there will be no effect on total piracy or on legal consumption. But when the strength of the intervention clears a certain threshold, the action reduces total piracy and increases legal sales (Dey et al. 2015). Empirical findings have generally been consistent with this theoretical result. Danaher et al. (2015) found that court ordered blocking of The Pirate Bay in the UK caused only a small decrease in total piracy levels because

¹ This legal “price” may be a financial price or the inconvenience (disutility) of watching advertisements.

most former users of The Pirate Bay switched to other unblocked piracy sites. Thus, the event did not cause any increase in paid legal streaming of movies or television. However, when 19 major piracy sites were simultaneously blocked in November 2013, Danaher et al. found that this action caused a meaningful reduction in total piracy levels in the UK and also caused a statistically and economically significant increase in usage of paid legal streaming sites (like Netflix).

It is theoretically ambiguous what effect the UK blocking of 53 piracy websites in November 2014 would have on consumers. On one hand, the blocking of only 19 sites had a significant impact, and so one might hypothesize that blocking 53 sites should have at least as powerful an impact. It is also true that legal distribution channels such as Netflix were more established in 2014 than they were previously, and thus they may have presented a more appealing alternative to piracy than was the case in 2012 and 2013. On the other hand, usage of the sites that were blocked in 2014 was much less concentrated than usage among those that were blocked in 2012 and 2013, which may weaken the impact of the 2014 blocks on consumer behavior. It is also possible that the remaining illegal consumers in 2014 were more technically savvy consumers (with less technically savvy consumers having been dissuaded by the 2013 blocks) and, as a result, these users will find it easier to locate alternative piracy sites than the consumers who were blocked in 2013. Thus, the impact of the 2014 blocks is an open empirical question.

We analyze this question using a similar set of data to Danaher et al. (2015) and the same methodologies. We find that the impact of the 53 site blocks in November 2014 on total piracy and on legal consumption was greater than the impact of the blocking of The Pirate Bay but smaller than the impact of the nineteen site blocks in November 2013. Due to the availability of more granular data for the 2014 blocks, we are also able to show that the increase in legal consumption was distributed across both ad-supported and paid subscription streaming sites. In the final section

we discuss our interpretation of these results and what they may mean for the future of website blocking in the UK.

2. Background

Danaher et al. (2015) describe the importance of the film industry in the world economy and summarize the research on the effect piracy has on film and television. They also describe the various strategies that governments and firms have implemented to mitigate this effect.

One of these strategies involves legal requirements for Internet Service Providers to block access to sites providing copyright infringing content. For example, in April 2012 the UK courts ordered five major ISPs to block access to The Pirate Bay, one of the most popular piracy sites in the world (with reportedly 3.7 million UK users).² In October and November of 2013, the courts ordered the same ISPs to block access to 19 different website that enabled access to copyright infringing content. These two blocking actions were the subject of the empirical research in Danaher et al. (2015).

More recently, in February 2014 the courts ordered four more piracy sites to be blocked, including the popular website watch32.com. Then, in November 2014, the courts ordered six major ISPs in the UK to block access to 53 different sites/services,³ bringing the total number of blocked sites to 93.⁴ Thus, the set of blocks in November more than doubled the number of blocked piracy sites, and the goal of our research is to evaluate whether these blocks had an impact on consumer behavior and if so, how.

² <http://www.theguardian.com/technology/2012/apr/30/british-isps-block-pirate-bay>

³ A list of the 53 blocked sites can be found in Appendix I.

⁴ <http://www.bbc.com/news/technology-30234790>

It is clear why one might expect these blocks to reduce piracy and increase legal consumption, given the number of sites blocked. However, there are three primary reasons why the blocks may have less of an impact than earlier blocks:

1. A large proportion of piracy was concentrated among the 19 sites blocked in November 2013. Whatever piracy remained after those blocks may have been significantly more dispersed across other sites, making it harder to have an impact on behavior by blocking a limited number of sites. Whether the 53 sites blocked in November 2014 constituted a meaningful amount of total piracy to “move the dial” is part of the question we answer in this research.
2. Consumers who were dissuaded from piracy by the November 2013 blocks may have been less technically savvy internet users, such that blocking 19 sites made piracy inconvenient enough for them to want to increase their consumption through legal channels. If the remaining users of illegal sites were more technically savvy, they may have found it easier to learn where the newest or most reliable remaining piracy sites were located. If this is true, then it may be difficult to block enough sites to impact their behavior.
3. Consumers remaining on piracy sites by November 2014 may have had lower willingness-to-pay (WTP) for legal products than pirates from before the 2013 blocks. For example, if there were 100 pirates in 2013 and 40 of them had a Netflix subscription (or were very close to wanting to buy one), it may be these 40 who chose to consume more through Netflix after the November 2013 blocks. In this case, the remaining 60 pirates, even if they were inconvenienced by the November 2014 blocks, may simply be the exact consumers who are less interested in paying for Netflix and similar legal

services. Thus the 2014 blocks could have a smaller impact on legal consumption than prior blocks.

For these reasons, it is not clear what the marginal impact of additional website blocking will be, and thus it is unclear whether there are proportional returns to scale from additional blocks, or if returns are diminishing.

3. Literature Review

Our research in general fits into several streams of the academic literature.

First, research in economics and in information systems has asked whether piracy harms sales. The vast majority of studies find evidence that piracy of media content does displace sales of that content, including Rob and Waldfogel (2006), Danaher et al. (2010), Zentner (2012), Danaher and Waldfogel (2012), and Ma et al. (2014). Nonetheless, several studies indicate no impact from filesharing, including Oberholzer-Gee and Strumpf (2007) and Smith and Telang (2010).

Second, researchers have asked whether reduced revenue caused by piracy has impacted the supply of creative works. Waldfogel (2012) finds that the reduced costs of production, promotion, and distribution of music has offset the revenue losses due to piracy, and that there has subsequently been no decline in the supply of music that surpasses a certain quality threshold. Waldfogel (2015) also finds that in movies, due to reduced costs of bringing products to market and to the ex-ante unpredictability of a movie's ex-post success, the overall effect of digitization has been to increase the number of films being released and also the value that consumers get from these movies. Both of these studies examine situations where piracy was accompanied by meaningful reductions in the cost of production. In contrast, Telang and Waldfogel (2015) studied

the explosion of VCR piracy in India starting in 1985, an event that was not accompanied by cost-saving digital technologies. In this context, they found that increased piracy and lax intellectual property protection led to a decrease in the number of films produced and also a decrease in the quality of those films that were produced, according to ratings on imdb.com.

Finally, researchers have asked what policies or strategies can mitigate the impact of piracy on sales. Dey et al. (2015) use an analytical modeling approach to demonstrate that supply side policies (which target sources of piracy) will have better social outcomes than demand side policies (which target consumers of pirated goods). Several empirical studies (Danaher et al. 2014, Danaher and Smith 2014, Adermon and Liang 2014) demonstrate that government interventions targeting copyright infringement can reduce piracy, and that legal sales increase under such policies. But, Adermon and Liang also find that when enforcement of antipiracy policies loses credibility, piracy and sales revert to their original levels. Peukert et al. (2015) also suggest that the benefits of antipiracy interventions may be limited to more popular products rather than “long tail” products, at least in the context of the shutdown of Megaupload.com and its effects on film box office performance.

Our present analysis is an extension of the research conducted in Danaher et al. (2015), where we examined the impact of The Pirate Bay block in April 2012 and the 19 site blocks in the UK in November 2013. In the next section, we will describe our data and several ways in which it differs from the dataset in the prior study.

4. Data

We obtained data on the monthly behaviors of a panel of 58,809 UK Internet users from August 2014 to February 2015, which includes the three months before the blocks (August-October), the month of the blocks (November), and the three months after the block (December-

February). The data were obtained from PanelTrack, the same source as that in our previous work on UK website blocking. We do not observe the behavior of each user — rather, users are segregated into 10 different segments based on the number of times they visited the blocked sites during August and September of 2014. The control group includes users who did not visit the blocked sites at all during these months, while the tenth segment includes users who visited the blocked sites over 35 times during August and September. We observe the aggregated number of visits (and time spent) to different categories of sites for each segment during each month. In particular, we observe visits to blocked sites, visits to alternate unblocked piracy sites, visits to VPN sites, visits to legal ad-supported video sites (for example, the BBC iPlayer or Channel 5’s “Demand 5” streaming website),⁵ and visits to legal subscription based sites (e.g. Netflix or Amazon Prime). For the legal advertising supported streaming sites, we also observe the actual number of videos viewed on those sites, although we unfortunately do not observe this for subscription based sites. We also do not observe information related to behavior at download or rental sites. Purchases at such sites are very difficult to track, and visits to such sites have been suggested to have only a weak correlation with purchases.⁶

For our analysis, we drop observations from the month of November. The first court orders came at the end of October and the rest came in mid-November. Because it might take ISPs a few weeks to implement the blocks, November was a transition month in the blocking of these sites. We consider the pre-block period to be August-October 2014, and the post-block period to be

⁵ Notably, we did not include YouTube in ad-supported streaming. The vast majority of content on YouTube is comprised of short-form or user generated videos that are unaffected by piracy and thus would be unaffected by the blocks. But YouTube visits dwarf visits to all of the other ad-supported streaming sites, and so including these visits would make it extremely difficult to detect any effect of the blocks on legal consumption of content that is actually being pirated.

⁶ For example, the largest such site is iTunes, which is usually used as an app as opposed to as a storefront or website. This app is “opened” every time someone boots up their computer or plugs in their iOS device, though these events rarely lead to a purchase.

December 2014 — February 2014. Of course, there are many factors that could affect trends in media consumption (legal and illegal) from the pre to the post period. For example, very popular shows like Downton Abbey are typically released in the early Fall, and consumption of these shows may taper off in January and February of the following year. Following Danaher et al., we account for these time trends by examining the changes across different segments, and asking if the segments more heavily affected by the blocks (those using the blocked sites more) make larger changes in behavior than those less affected.

Table 1 describes the segments in our data and their usage of various media-related websites during the pre period.

TABLE 1 — DESCRIPTIVE STATISTICS

Consumer Segment	Users in Segment	Avg. Visits to Blocked Sites Per User	Total Piracy Visits	Legal Ad-Supported Visits	Legal Subscription Visits	VPN Visits
0	53,273	0	138,257	61,967	57,475	4,854
1	1,737	1	31,553	6,610	7,692	390
2	801	2	18,027	2,346	3,322	147
3	451	3	15,073	2,286	1,871	166
4	319	4	11,665	1,119	1,301	18
5	426	5.4	15,802	1,590	1,968	229
6	478	8.3	23,118	2,389	2,666	71
7	396	13.2	28,988	1,999	3,446	524
8	502	23.8	56,917	3,448	3,018	115
9	426	78.6	140,423	3,178	2,496	28

Note that sample sizes across segments are not equal. Clearly though, each segment was constructed to have differing levels usage of the blocked sites before the blocks occurred, meaning that each segment received a different level of treatment intensity from the blocks. Segment nine, the heaviest users of the blocked sites, are also the heaviest pirates in general as their total piracy visits are larger than every other segment including even segment 0, in spite of the fact that segment

0 has 125 times more users than segment 9. Simply put, segment 0 users are only very infrequent pirates, while segment 9 are the most intense pirates. Segment 9 was most impacted by the blocks since the average user in that segment visited the blocked sites almost 80 times before the blocks, while segment 0 were non-users of the blocked sites.

We also observe that, consistent with prior literature, the heaviest pirates are also heavier users of legal sites than the lighter pirates, reflecting the fact that individuals with strong preferences for consumption of television and movies tend to consume more television and movie content, through both pirate and legal channels. The question is whether these heavy pirates would have higher legal consumption in the absence of piracy, or in this case whether removing access to some of the piracy sites can induce these heavy pirates to make more use of legal channels.

Finally, it is important to note that the data indicate that the blocks were effective at reducing traffic to blocked sites. In the data, we observe a total of 86,735 visits to blocked sites in the pre-period. However, in the post period, we observe only 10,474 visits to blocked sites, indicating nearly a 90% drop in visits to blocked sites. There are several potential reasons for why the drop was not 100%. First, some ISPs may not have blocked all of the sites until some time in December, depending on how long it took them to comply. Second, some smaller ISPs were not targeted by the court orders and thus did not participate in the blocks. Finally, some consumers may have used VPNs to access the blocked sites, and such visits might show up in our data. Nonetheless, with a 90% drop in visits to blocked sites, it is clear that the blocks constitute a meaningful experiment as a negative shock to piracy at the blocked sites.

5. Empirical Model and Results

To determine the impact of the November 2014 blocks on various consumer behaviors, we run the following difference-in-different model:

$$LnVisits_{jt} = \beta_0 + \beta_1 After_t + \beta_2 TreatIntensity_j * After_t + \mu_j + \varepsilon_{jt} \quad (1)$$

where $LnVisits_{jt}$ indicates the natural log of visits (to whatever category of sites we are examining) made by consumer group j during period t . $After_t$ is a dummy variable equal to one if the month is December 2014, January 2015, or February 2015. By including this variable, we control for differences between the pre-block period and the post-block period that would, on average, affect all segments evenly, such as any outside factors which increase or decrease the appeal media consumption, piracy, or VPNs (for example, the quality of content released or being offered at the time of observation). $TreatIntensity_j$ indicates the number of visits that the average consumer in group j made to the 53 blocked sites during August and September of 2014. Finally, μ_j is a vector of group fixed effects and ε_{jt} is an idiosyncratic error term. In this model, β_2 is the variable of interest and, under the assumption that each group's trend after the block would have been uncorrelated with that group's treatment intensity, it indicates the causal impact of the block on visits to sites in the outcome group in question.

Table 3 below shows the results from model (1) for various outcome variables.

TABLE 2 — EFFECT OF NOVEMBER 2014 SITE BLOCKS

	Unblocked Piracy	VPNs	Legal Ad-supported	Legal Subscription
After Block	-1.053* (0.001)	-1.500* (0.004)	-0.586* (0.000)	-0.619* (0.000)
TreatIntensity * After Block	-0.002 (0.380)	0.030+ (0.066)	0.005** (0.050)	0.004 (0.251)
Constant	10.178* (0.000)	5.148* (0.000)	8.131* (0.060)	8.217* (0.000)
Observations	20	20	20	20
Consumer groups	10	10	10	10
R-squared	0.979	0.851	0.99	0.97

p-values in parentheses calculated based on a t distribution with 8 degrees freedom (# groups - 2)

+ significant at 10%; ** significant at 5%; * significant at 1%

First, note that the “after block” term is negative for all variables. The most likely explanation for this is a strong negative trend in interest in media consumption over this time. Some very popular shows are released in the UK in the fall, and it may be that interest drops off greatly during the winter. Our identification strategy controls for this by looking at how each group’s behavior differs from the general trend.

The coefficient of interest (treatintensity * after) is small, negative, and statistically insignificant for unblocked piracy sites. This indicates that the blocks did not cause former users of blocked sites to increase their consumption at other illegal sites. This is consistent with prior results. Danaher et al. showed that when one site was blocked (Pirate Bay), many users of that site moved to other unblocked piracy sites. But when 19 sites were blocked, there was only a small and statistically insignificant increase in usage of unblocked sites, presumably because consumers

had more difficult finding the remaining piracy sites. In this analysis, we find that when 53 sites are blocked, there is no causal increase in usage of unblocked piracy sites. The most likely explanation is that users of these sites found it difficult to learn about other illegal sites that they could trust after 53 of the most popular sites were blocked.

Some users of the blocked sites, however, did seem to employ technical workarounds to continue usage of the blocked sites. The coefficient of interest for visits to VPN sites is positive and significant at the 90% confidence level, indicating that for every 10 additional visits to blocked sites before the blocks, a consumer increased their visits to VPN sites after the blocks by an additional 30%. Table 1 shows that VPN usage in the data was still small relative to usage of other sites, but one visit to a VPN site can facilitate multiple private (and unblocked) visits to other sites, and so this causal increase in usage of VPN sites may indicate a number of Internet users employing VPNs to circumvent the blocks. This is consistent with the findings from the Pirate Bay and November 2013 blocks. In spite of this increase in VPN usage however, our data show the blocks caused a decrease in total piracy.

Turning to legal sites, we find a positive and statistically significant increase in usage of ad-supported sites. A 10 visit increase in pre-block visits to blocked sites is correlated with a 5% larger increase (or smaller decrease) in visits to ad-supported legal streaming sites after the blocks.⁷ This indicates that the blocks caused an increase in visits to legal ad-supported video sites. Notably, for this particular category of sites, we also have data on the number of videos actually viewed (as PanelTrack collects these data). When “visits” is replaced with “videos viewed”, the results are nearly identical in that the interaction coefficient remains 0.005 and it is measured with 90%

⁷ For both ad support legal sites and subscription legal sites, we tested a quadratic interaction term as in Danaher et al. (2015). In both cases, the linear model was a better fit.

confidence. This not only indicates that the blocks led to more legal video viewing (and thus advertising revenues) at ad-supported sites, it also provides some validation for our use of “site visits” in other categories as a proxy for viewing videos at those sites.

Finally, the interaction term for paid subscription-based legal streaming sites is positive and similar to that for ad-supported sites, although it is only measured with 75% confidence. The point estimate indicates that a 10 unit increase in visits to blocked sites before the blocks is correlated with a 4% increase in usage of paid subscription sites after the blocks. We note that this is smaller than the coefficient found for the November 2013 blocks. We will discuss the importance of this and the total causal increase in the next section. The fact that the coefficient for ad-supported legal sites is similar to that for subscription-based legal sites is intuitively appealing. Both categories of sites allow for unlimited “free” viewing of videos, at least if one has already paid the subscription price for the second category. Thus, it may not be surprising that both categories of sites receive similar uplifts from the blocks.

In Table 3 we report the estimated causal change in total piracy and each type of legal viewing for each segment based on our model estimates. Importantly, one should recall that the estimate for ad-supported viewing was measured with 95% confidence, while the estimate for subscription was measured with 75% confidence. These two estimates are derived directly from the coefficients from the regression model. However, the causal change in total piracy is computed differently. First, we assume that the immediate drop in piracy at blocked sites after the blocks (which was a 90% drop) was a result of the blocks. Next, we note that our regression showed no causal increase in usage of unblocked piracy sites. Thus, for each segment, we calculated the total piracy before the blocks (unblocked plus blocked piracy) and assumed that in the post period, if

nothing had changed except for the blocks, it would have been the same number less 90% of the blocked site visits. From this we calculate the causal change in total piracy for each segment.

Table 3 — Estimated Causal Change in Piracy and Legal Viewing

Pre-block Visits/User to Blocked Sites	Causal Decrease in Total Piracy	Causal Increase in Ad-Supported Viewing	Causal Increase in Subscription Viewing
0	0.0%	0.0%	0.0%
1	7.6%	0.5%	0.4%
2	11.4%	1.0%	0.8%
3	11.1%	1.5%	1.2%
4	13.5%	2.0%	1.6%
5.4	17.0%	2.7%	2.2%
8.3	20.2%	4.2%	3.4%
13.2	22.8%	6.8%	5.4%
23.8	25.3%	12.6%	10.0%
78.6	28.0%	48.1%	36.9%
		(p=0.05)	(p=0.25)

We thus show that our model predicts a larger decrease in piracy and a larger increase in legal viewing when users were heavier users of the blocked sites before the blocks were implemented. In the next section, we estimate the true total increase in legal viewing behavior caused by the blocks and discuss our results in the context of other site blocking actions.

6. Discussion

This study updates our prior study on UK website blocking by examining the largest wave of blocks to date in the UK — 53 piracy websites that were blocked in November 2014. The results from this update yield new insights into the effectiveness of website blocking as well as confirming some explanations we suggested for previous results.

Our results suggest that blocking these 53 sites led to an overall decrease in the total amount of piracy and an increase in consumption of video content through legal sites. To estimate the total

impact of the blocks for total piracy, ad supported video viewing, and subscription-based video viewing, we estimate the counterfactual visits to each of these groups of sites for all treated groups and determine the difference between the counterfactual and observed visits in the after period. We then sum these measurements across all treatment groups to determine the total impact on these consumers.

Based on these calculations, we conclude that the blocks caused a 22% reduction in total visits to piracy sites across all treated segments⁸ relative to how much they would have pirated if not for the blocks. This caused a 10% increase in legal viewing at ad-supported sites among the treated segments. It also caused a 6% increase in legal visits to subscription-based sites. Recall that in our analysis of the November 2013 blocks, we only had data on subscription based streaming, and we found that the 2013 blocks caused a 12% increase in subscription based streaming among the treated groups. In November 2014, the increase in subscription based streaming was still positive but only half as large as the increase caused by the 2013 blocks. Against, this lower result could be the result of lower popularity of the 53 sites in 2014 than the 19 sites in 2013, or it could be the result of the increased price sensitivity of the remaining pirates in 2014. In contrast, the 2014 increase in visits to ad-supported streaming was nearly as high as the subscription streaming increase in 2013. This could suggest a more price-sensitive market (since ad-supported sites are free), or it may be that the increase in ad-supported streaming in 2013 was much larger than this since we did not measure ad-supported streaming in the prior study.⁹

⁸ The reason it is not near 100% is because the 53 blocked sites only constituted a portion of the total available piracy sites that we tracked. Moreover, while highly treated segments were heavily reliant on these sites, some of more lightly treated segments relied more on other piracy sites (and all segments made at least some use of the unblocked sites).

⁹ In studying the November 2013 blocks, we did not have data on ad-supported streaming and so we did not measure this increase. It remains possible that the 2014 blocks actually caused similar conversions to legal but that those conversions were concentrated more on ad-supported sites. However, this is unlikely, as legal streaming sites were actually growing in recognition and popularity between 2013 and 2014, and so the most likely situation is that the

Another calculation one might consider would be a conversion ratio: What percentage of the reduced piracy visits resulted in an extra visit to a legal streaming site? We estimate that the blocks caused a reduction of about 25,800 pirate site visits in our sample, while causing 1,350 more visits to ad-supported sites and 925 more visits to subscription based sites than would have otherwise been observed. This implies a conversion ratio of about 8.8%, that is, about 8.8% of thwarted piracy visits resulted in visits to legal ad-supported or subscription based video sites. However, this should not be interpreted as the percent of pirated downloads that lead to lost sales, for two reasons. First, although we capture visits to nearly all of the major piracy sites, for legal sites we only capture the major ad-supported sites dedicated to long-form (movies and television) content. We do not capture data on some sites that may offer television or movie streaming (such as Youtube, since most of the streaming here is for short-form content), we do not capture legal download purchases or rentals, and we do not capture online and offline physical sales. Thus, the 8.8% would be a lower bound on the percent of pirated downloads that are otherwise lost legal consumption.

Perhaps more importantly, we have noted that the 53 blocks in November 2014 occurred after a number of other major blocking actions in the UK. Thus, the Internet users who remained as pirates at the beginning of these blocks likely are not representative of typical pirates. Rather, they are likely to be individuals who were both capable of and showed a preference for pirating long after many of the most popular and reliable piracy sites had been blocked, which could imply that they are either much more educated about piracy websites or that they have lower willingness-to-pay for legal content. In fact, a commonly referenced ratio for the percentage of pirated

November 2013 blocks also caused a larger increase in ad-supported streaming than the 2014 blocks, though we cannot be certain of this.

downloads that are otherwise would be purchases is 20% from Waldfogel (2006). It is intuitively appealing that our conversion ratio is lower than this, given that we do not track all potential sources of legal consumption and that we are likely examining a community of individuals who are more committed to pirated consumption than other individuals.

From a policy perspective, however, our research suggests that website blocking in the UK can still cause a decrease in total piracy and an increase in legal consumption, although it appears as if additional blocks have diminishing returns, at least in terms of their ability to increase visits to subscription based streaming services. Thus, this research complements the existing knowledge on website blocking, although it remains unclear whether there is some number of blocked sites such that additional blocks will not have an impact, or if there will always be positive returns to additional blocks on legal consumption.

Our research comes with many of the same limitations noted in Danaher et al. (2015), in spite of the fact that the present analysis contains somewhat more nuanced data on legal consumption. One of the main limitations is that we only observe a few types of legal behavior (ad supported streaming and subscription streaming), despite the fact that thwarted pirates may also turn to a la carte purchases, DVD/Blu-Ray purchases, or rentals).

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**Appendix A — List of Piracy Sites (Allowing Access to Pirated Video Content)
Blocked in November 2014**

bittorrent.am

btdigg.org

btloft.com

bts.to

limetorrents.com

nowtorrents.com

picktorrent.com

seedpeer.me

torlock.com

torrentbit.net

torrentdb.li

torrentdownload.ws

torrentexpress.net

torrentfunk.com

torrentproject.com

torrentroom.com

torrents.net

torrentus.eu

torrentz.cd

torrentzap.com

vi torrent.org

watchseries.lt

stream-tv.me

watchserieshd.eu
cucirca.eu
movie25.cm
watchseries.to
iwannawatch.to
warez-bb.org
icefilms.info
Tehparadox.com
heroturko.me
scnsrc.me
rapidmoviez.com
iwatchonline.to
losmovies.com
isohunt.to
torrentz.pro
torrentbutler.eu
iptorrents.com
sumotorrent.sx
torrentday.com
torrenting.com
bitsoup.me
torrentbytes.net
seventorrents.org
torrents.fm
yourbittorrent.com

tormovies.org

demonoid.ph

torrent.cd

vertor.eu

rarbg.com