Online Music, Sales Displacement, and Internet Search: Evidence from YouTube

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Abstract

Online content services have become an important means of enjoying music, and considerable debate exists over the performance rights of sound recordings. In this paper, we exploit the removal of Warner Music content from YouTube in January 2009, and its restoration in October 2009, as a plausible experimental design to investigate the impact of online content availability on album sales and Internet search. We find that the blackout had both statistically and economically significant positive effects on album sales, and we find no evidence that the blackout had any causal effects on the volume of Internet search for artists.

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1 Introduction

The Sound Recording Act of 1971 granted federal copyright protection to sound recordings in the U.S., but only a subset of those rights that are usually granted to writing authors are protected. That is, while music artists were granted exclusive rights to reproduce, distribute, and adapt their sound recordings, they were not granted the public performance right. It was not until 1995 when the Digital Performance Right in Sound Recordings Act (DPRA) partially provided, for the first time, such a right that artists were able to get compensated for the public performance of their sound recordings. The DPRA, however, set a limitation, which has become the center of debates in the market for digital content services.

Specifically, the DPRA created a public performance right for sound recordings that are performed only by means of a digital audio transmission, which targeted the growing number of satellite radio and, increasingly today, Internet companies. In particular, over-the-air broadcasters are free to use the sound recordings without any payment to the artists or the record companies, where the justification for this exemption was that the broadcasts supposedly have promotional effects.[1] The aim of this paper is to examine whether the digital content services have promotional effects or simply displaces album sales, which can inform the public policy debate on statutory and direct licensing for public performance of sound recordings.

A number of authors have tried to estimate the effects of music piracy on album sales. Our paper is different from these in two dimensions. First, the existing literature has focused on illegal, peer-to-peer file sharing while we investigate the sales displacement and the promotion effect from YouTube that pay licensing fees to record labels. File sharing has been in decline since its 2005 peak, and music sales increased year over year in 2009 with digital accounting for 40% of sales.[2] Now popular streaming services such as Spotify, iTunes Radio,

[1] Such a justification, however, has been increasingly criticized in light of recent technological developments and alternative sources of music, and in fact the U.S. executive branch has been supporting an equal treatment of terrestrial and online music services (Peters, 2007; Department of Commerce, 2013). Liebowitz (2007) also finds that radio play does not have a positive impact on record sales.

[2] See https://www.npd.com/wps/portal/npd/us/news/press-releases/the-npd-group-music-file-sharing-

and Google Play Music did not yet exist or were not operating in the U.S. while YouTube dominated the online multimedia market with an extensive music library. This provides a unique environment to study the policy implications for public performance rights by means of digital audio transmission.

Second, we provide a more strightforward identification of the effect of free access to music on album sales using a quasi-experimental approach, the removal of Warner content from YouTube for a nine-month period, which we call a blackout throughout this paper. Although there are a number of papers in the literature quantifying the effects of file sharing, the findings have oftentimes been inconclusive. For instance, Oberholzer-Gee and Strumpf (2007) use the number of German students on vacation in a particular week as an instrument for downloads; however, it has been criticized that the instrument is weak for various reasons (Liebowitz, 2010). As we will explain below, the removal of Warner music content from YouTube is a reasonably exogenous event, and we aim to provide definitive results using actual (rather than estimated) sales data.

Because YouTube has been by far the dominant online content service in the U.S. in terms of various website metrics, failing to find evidence in support of a promotional effect would mean that free digital content services are likely to be substitutes for consumer piracy in terms of industry profits. In addition to no promotional effect, we find a substantial sales displacement effect, especially in top albums. This effect diminishes quickly as we drop, say, the top 50 albums. To be specific, using a six-month window before and after the blackout of Warner content, the removal of content from YouTube is causally associated with an increase of 7375 units per week per album using the Billboard top 200 albums, 3155 units when excluding the top 10 ranks, 1968 units when dropping the top 25, and 555 units dropping the top 50.

We note here that the lack of a promotional effect through YouTube in our Billboard top 200 sample need not generalize to those outside of the top 200. That is, YouTube may

declined-significantly-in-2012/ and http://www.businesswire.com/news/home/20100106007077/en/2009-U.S.-Music-Purchases-2.1-2008-Music.

enable a vast array of user-generated contents and bring substantial benefits to emerging or independent bands. Waldfogel (2012) assembles comprehensive data on albums released between 1980 and 2010 including Billboard chart ranking, and finds that Internet radio play affects the number and kinds of products consumers have information on and an increasing number of albums find commercial success without substantial traditional airplay.[3] The difference is that Waldfogel focuses on the supply side (namely, number of new releases), where album sales are not actual but imputed from ranks.

We conduct another independent test of promotional effects that could confirm or offset the above findings, using keyword search volume for artists on Google as another outcome variable of interest. The hypothesis is that Internet search for Warner artists would decrease during the blackout period if YouTube exposure tends to increase the user's interest in and search for information about the artist. As we show below, the effect of the blackout on Internet search for Warner artists is negative but it is not statistically significant. Hence, we do not find strong evidence in support of the promotional effect in our Billboard top 200 sample. Finally, as a robustness check we repeated our analysis using only albums that extend across the start or end date of the blackout period, with the results largely unchanged.

Our results can shed some light on benchmark copyright licensing terms. Specifically, the DPRA permitted a statutory licensing for noninteractive subscription services and satellite radio stations. The statutory rates are set by three Copyright Royalty Judges in a way that "most clearly represent the fees that would have been negotiated in the marketplace between a willing buyer and a willing seller" (17 U.S.C. 112(e)(4)), which can potentially be used as a benchmark to negotiate a direct licensing agreement between digital service providers and music labels.[4] A direct deal with majors, which allows for more functionality (for example,

[3] Bourreau et al. (2013) find that the number of new releases can increase without having higher overall sales. Thus, a strong sales displacement effect at the top can be potentially consistent with online content services having some promotional effect for lesser-known artists, a 'long-tail' phenomenon.

[4] The reason for negotiating licenses directly with rights owners is that the statutory licensing places restrictions on the number of times a listener can skip a song, a particular artist or album can be played, and importantly the ability to choose a particular song by a particular artist. While smaller services like Pandora have opted for statutory licensing, larger companies such as Apple's iRadio and Google's YouTube have sought direct licensing agreements.

on-demand services), could require more revenue sharing, where our baseline estimates can be used as a reference to which to judge appropriateness of negotiated terms.

The remainder of this paper is organized as follows. Section 2 contains relevant literature. Section 3 explains the blackout episode. Section 4 describes the dataset. Section 5 presents empirical findings. Section 6 concludes.

2 Related Literature

As one of the first industries that were hit by the digital disruption, the music industry has received a great deal of attention in the past dozen or so years. Starting from the Napster case (*A&M Records, Inc. v. Napster, Inc.*), the sales displacement and the promotional effects of online file sharing services have been at the center of debates. While there have been some anecdotal evidence and surveys indicating that file sharing helps promote an artist's work, efforts to show the existence of promotional effect have been largely confined to theoretical works in the literature (for example, Gopal et al., 2006; Peitz and Waelbroeck, 2006).[5] Absent independent empirical measures for promotion, the literature evolved to focus heavily on sales displacement effects where promotional effects were regarded as a lack of the sales displacement effect.

While we do not intend to survey this literature here (see for example, Liebowitz (2006) and Waldman (2013) for surveys), most previous works on music piracy made use of either individual-level survey data (see Rob and Waldfogel, 2006; Zentner, 2006; Andersen and Frenz, 2010; Waldfogel, 2010; Hong, 2013) or country/city-level panel data (see Hui and Png, 2003; Peitz and Waelbroeck, 2004; Liebowitz, 2008; Zentner, 2010). A couple of papers (Blackburn, 2004; Oberholzer-Gee and Strumpf, 2007) have attempted to assess consumer piracy using an album-week as the unit of observation. Although Blackburn and Oberholzer-Gee and Strumpf had direct measures of file sharing activities for albums, the difficulty they

[5] In a survey conducted by Pew Research Center, 43 percent of the artists agreed that "file-sharing services aren't really bad for artists, since they help to promote and distribute an artist's work to a broad audience" (Pew Internet & American Life Project. Press Release 12/5/2004).

faced was to find a plausible instrumental variable, which was the key to their identification and led to opposite conclusions.[6]

Recently, researchers have begun to exploit quasi-experimental events to investigate the effects of digitization on sales. For instance, in a series of papers, Danaher et al. (2010), Danaher and Smith (2014), and Danaher et al. (2014) investigate the effects of copyright enforcement on digital content sales. Specifically, Danaher et al. (2014) find that France's graduated anti-piracy law caused iTunes sales from major music labels to increase by over 20% relative to other European countries. Similarly, using cross-country variation, Danaher and Smith (2014) show that the U.S. government's shutdown of a major piracy site caused digital revenues for major motion picture studios to increase by 7.5%. Our paper is similar in this regard, but the literature has not in fact exploited an experimental design in the context of online music, which we do in this paper.[7]

One notable exception is Kretschmer and Peukert (2013). In an independently developed paper, Kretschmer and Peukert exploit the fact that due to an ongoing royalty dispute between YouTube and rights holders' association in Germany, a large fraction of videos that contain music cannot be accessed in Germany while much of the same content is easily accessible in other European countries. They focus on the effects of this cross-country variation in online music on top 300 songs and albums sold on the iTunes Store, where unit sales are imputed from ranks assuming a Pareto distribution. They conclude that the promotional effect of online music is big enough to offset sales displacement.

We believe that there are a couple of reasons why the conclusion of our paper is different from Kretschmer and Peukert's. First, our sales figure includes both physical album sales as well as digital album sales, while Kretschmer and Peukert use sales rank information available

[6] While Oberholzer-Gee and Strumpf (2007) conclude that there is no evidence of sales displacement as we previously discussed, Blackburn (2004) using the timing of the RIAA lawsuits against consumers as an instrument concludes that filesharing has negative impacts on album sales.

[7] Similarly, Danaher et al. (2010) use the removal of NBC content from Apple's iTunes Store to show that the removal is not causally associated with changes in NBC's (imputed) DVD sales. In contrast to ours, their hypothesis is that users without access to a paid channel (for example, iTunes) would be more inclined to make more DVD purchases.

from iTunes Stores. This basically assumes a high correlation between digital sales and CD sales, which has not been substantiated. Second, most researchers, including Kretschmer and Peukert, do in fact use imputed unit sales from sales rank information because of the costs required to obtain the actual sales data. This is understandable, however, sales rank information can lead to qualitatively different conclusions. We were in fact surprised to find that our results changed when we obtained the actual sales data compared to when we looked at only ranks.

We also propose and construct a panel of keyword search index that captures the user's interests in bands, providing another test of promotional effects (rather than interpreting the lack of sales displacement as promotional effect). This provides a useful cross-check, and is based on the related evidence that Internet users are less likely to investigate additional content in depth after a removal of news articles compared to a control group (Chiou and Tucker, 2011). That is, our hypothesis is that users may be less likely to search for bands after their music is removed from YouTube. While Chiou and Tucker (2011) use web-browsing (namely clickstreams) data, we use the volume of search queries on a particular band, provided by Google Trends, which is growing in popularity as a measure of predicting contemporaneous activities (see Goel et al., 2010).

3 Background Information

YouTube was launched in November 2005 as a video sharing website. The site grew rapidly, and Reuters reported in 2006 that YouTube is the leader in Internet video content with 29% of the U.S. multimedia market share and 20 million unique users per month. According to data published by market research company comScore in 2010, YouTube's market share of online video content was 43.1% followed by Hulu (3.5%). Further, 84.8% of the total U.S. Internet audience viewed online video, where 144.1 million viewers watched 14.6 billion videos on YouTube (101.2 videos per viewer). At least since 2010, the web information company Alexa ranks YouTube as the third most visited website on the Internet, behind Google and Facebook. Hence, we believe that a media blackout on YouTube could have a substantial impact.

YouTube started as a platform to upload, view and share home-made, user-generated videos; however, soon the site contained many unauthorized clips of copyrighted content registered users could upload in an unlimited number and unregistered users could watch free. In this period, YouTube could be used as an on-demand radio, where almost every song a user wanted to hear could be found. Because YouTube did not review videos before they were posted, it was left to copyright owners to issue a takedown notice pursuant to the Digital Millennium Copyright Act.[8] However, in June 2007, Google having acquired YouTube in November 2006 began resolving copyright infringement claims that characterized YouTube's early days, both through licensing deals with major content providers and a content-management system, called Content ID.[9]

YouTube entered into a revenue-sharing partnership with the major content providers as early as 2006, and all the major labels had licensing agreements with YouTube in 2007. However, major labels were disappointed with those agreements having included a small fee for every video watched and a share of the advertising revenue, leading up to renegotiation of terms. In late December 2008, when it was time for licensing renewals, multiple press releases reported that Warner and YouTube failed to agree to terms on a new licensing deal. YouTube began to remove Warner music videos, both professionally made music videos and amateur material that may include Warner content. Apparently, most (if not all) of Warner music on YouTube was pulled or muted promptly, which the Electronic Frontier Foundation called the 'January Fair Use Massacre.'

The removal of Warner content came at a time when the other three majors (Universal

[8] In 2007, the media conglomerate Viacom filed a billion-dollar copyright-infringement suit against YouTube claiming that almost 160,000 unauthorized clips of Viacom's programming were made available on YouTube.

[9] When a video is uploaded, Content ID checks it against reference libraries of copyrighted audio and video material and alerts copyright owners whenever any part of their content went up on YouTube. Owners can then choose to remove the content or sell ads and share the revenue with YouTube.

Music, Sony BMG, and EMI) were also renegotiating their licensing deals with YouTube, which was set to expire soon. Interestingly, however, YouTube and the remaining three major labels reached a renewal agreement with no reported case of content removal. On 29 September 2009, YouTube and Warner announced that they finally reached an agreement and Warner's artists (both the full catalog and user-generated content containing Warner acts) were returning to YouTube. This created a nine-month blackout period during which it was extremely unlikely to find Warner content on YouTube, providing a natural experiment as YouTube had licensing deals with all the major labels (including Warner) for almost two years before and after the blackout.[10]

The licensing contracts between YouTube and record labels are not public information. Nonetheless, we believe that the breakdown of renegotiation between YouTube and Warner is not likely to be an endogenous outcome for the following reason. A person familiar with the situation told a reporter that YouTube and Warner were close to an agreement until the last moment, when Warner changed its terms. In response, Google made the move to remove the label's content, which may have been unexpected.[11] Further, all licensing deals with the majors were set to expire within a narrow time window, and Warner was obviously the first to see the negotiation outcome. One possible reason for why other labels did not see a similar breaking of negotiations is that Google was tough on Warner, preventing the other three majors from behaving opportunistically in renegotiation.

4 Data

The data in this paper comes from Nielsen Soundscan and is based on the Billboard 200, the U.S. industry standard for album sales. The Billboard 200 is a ranking of the 200 highest-

[10] This stands in contrast to the 2007 Viacom case, where there was no prior agreement; that is, users who posted Viacom's programming did so without authorization. In the Warner case, users legitimately posted Warner's songs for two years before suddenly their videos were taken down.

[11] This is consistent with the fact that Google did not say it is taking the music down at Warner's request. See http://allthingsd.com/20081220/warner-music-group-disappearing-from-youtube-both-sides-take-credit/.

selling music albums from any genre. The chart is based solely on sales (both physical and digital) of albums in the United States. Technically, this sample is a restricted sample of albums; therefore, there is no guarantee that the findings in this paper generalize to those not making it into this chart. However, we believe the main issue here is to get sufficient and accurate variation in sales. That is, the Billboard rankings omit any sales information making it impossible to determine, for instance, if the number one album this week sold as many as the number one from another week.

We thus obtained access to the weekly sales data for the Billboard 200 albums from Nielsen SoundScan, which includes both digital and physical album sales.[12] Looking at the data, there is considerable variation both within and across weeks. For example, on average the 200th ranked album has just 1.5% of the sales accrued to the top ranked album in a given week. Importantly, top sales also vary considerably across weeks. As previously mentioned, our results changed qualitatively using the actual sales data compared to rank alone. Although our sample is limited to albums in the Billboard 200, in this way we have a more precise measurement. On the other hand, it would be very costly to obtain complete sales data for those outside of top 200.

There are 2261 albums from 1663 artists in Table 1. This implies that there are a number of artists producing multiple albums over the sample period. We drop about 10-20 entries from every week because these are typically non-music albums and albums that are compilations (meaning no artist fixed effects are available) in the top 200. For each album, we construct the following variables: *twsales* is this week's sales, that is, the number of albums sold in a given week; *wkson* is the cumulative number of weeks on the chart, which increases by 1, and *wksonsq* is the square of *wkson*. Because demands for a new album often build up before the premiere week, we include a variable, *firstweek*, that indicates the first week of each album in our sample.

The data includes both new and catalog albums, so we create an indicator for new albums:

[12] SoundScan is the official basis for the Billboard charts ranking. We directly obtained access to the sales data from Nielsen under a licensing agreement.

firstalbum is 1 if it is the first album of an artist found in our SoundScan database. Because the database contains only the top 200 albums from year 2004, this variable indicates that an album is the first to make this chart since at least 2004.[13] Because the level of previous album sales has been found to be correlated with second album sales (Hendricks and Sorensen, 2009), we include two additional variables: *previousalbumduration* and *previousalbumsales* are the length on the Billboard chart and total sales for the last album an artist placed on the Top 200. These are equal to zero if they had no album on the charts in the database.

Our next step is to match album with genre and label information, which comes from the Discogs.com database. This database is exceptionally extensive (see Waldfogel, 2012). We manually coded all major/indie labels: majors being either directly one of the majors, or a subsidiary of one of the majors. This involved following the path for each sublabel. If the label was under a major, the indicator for that label was coded one. For some labels this involves finding a website or article about them. If we could find no connections anywhere, we labeled them as Indy. If Discogs.com labeled them as self-release or if the only releases under the label were of the artist, then we labeled it self-release. There are also fourteen standard base genres as listed by Discogs.com.

Finally, the data are merged with radio chart ranking. Our source of data on radio airplay is the weekly USA Airplay Top 200 ("The most played tracks on USA radio stations").[14] Billboard also has a radio chart listing 75 most aired songs of the week, but we preferred the USA Airplay Top 200 because of its broader coverage. Notice that the radio chart is for songs, while our sales data is for albums. Further, in many instances a song will be played extensively before the album is released. Therefore, when matching each song to the album-week in our data, we matched airplay for the weeks where an album was on the chart, while taking into account the fact that an album can appear on the radio chart several weeks before the album chart.

[13] We cross checked this indicator with the Billboard chart database (which goes back to 1998) and found very little change. That is, *firstalbum* indicates a first hit since 1998 for the vast majority of our sample.

[14] We scraped the radio chart from http://www.charly1300.com/usaairplay.htm.

We do not have data on airtime minutes, but we believe using the rank for airtime may be less of a concern as control variables because the total number of slots for songs in radio stations would be relatively fixed over time. Specifically, we construct the following variables: *lastweekradiorank* is the last week's chart ranking for matched songs for all album-weeks. Next, *wocradio* is the number of weeks on the chart (and zero if not on the chart); and *weeksinceradio* is the number of weeks since an album had a song on the radio chart for the last time (and zero if active on the chart). These two variables capture the chart duration, but allow for differential effects. Finally, *noradio* is an indicator for albums that have never had a song on the radio chart.

Given the wide viewership of YouTube content, we want to measure and compare the level of interests in each artist among Internet users. For this, we construct an index of search intensity (*twtrends*). Google Trends analyzes a percentage of Google search queries to determine how many searches have been done for the terms entered, compared to the total number of Google searches done for a certain region within a category for a specified time period. We decided to search queries for artists rather than albums because artist names are much more likely to give consistent results over time, and we also believe users are more likely to remember, and hence search on Google for artist names rather than album titles.

Specifically, we searched for each artist on Google Trends while restricting the search results to the U.S. region in the 'Music & Audio' category from January 2008 to September 2010 (one year before and after the blackout). Google has been the dominant search engine in the U.S. (with a consistent market share around 80%) in the past decade. Since our sample is from the U.S., this should give a good proxy for the search volume. We restricted our Trends search results to the Music & Audio category for more precise measurement because, for instance, some artists may have generic names. Google explains that filtering Trends data by category means that the data will be based on users searching for the term in a related context.[15]

[15] Google explains that "For example, if you're searching for the term java in the Food & Drink category, you won't see any information about searches done for java the computer programming language." See

For each search, Google Trends returns a weekly index for the search term, normalized by the highest search week, so that the peak in a series has a value of 100 (search terms with low volume would not appear). The major problem is that Google Trends allows only up to five terms to be searchable, hence comparable, at a time. To construct a globally consistent panel for our sample, we made pairwise comparisons between a benchmark artist and all other artists in a fixed reference week. This would give a consistent cross-section of all artists in that week. The next step is to search artists, one by one, and download the normalized weekly search index for each term. This gives the weekly time-series for each artist in the sample.

We then scaled each individual artist's normalized index in the reference week, so that the index is equal to the above-mentioned cross-sectional value. Finally, this artist-specific scale factor is multiplied to each week's normalized index for that artist. In this way we obtain a globally consistent panel of search indices for each artist in our sample. To be more precise, because the search intensity varied a great deal across the bands, we employed three benchmark artists (representing low, medium, and high search volume) to compare all the other artists in our sample against one of them. We then compared the three benchmark artists themselves in the reference week, which added another layer of scale adjustment.

5 Estimation and Results

Our results examine how the (lack of) availability of Warner music on YouTube affected album sales and Internet search. As Liebowitz (2005) elaborates, theoretically the so-called 'sampling' hypothesis can go in either direction with respect to sales. For instance, after listening to a song online consumers may like the music more or less than they did before. Thus, using sales as an outcome variable may not lead to a precise test of the promotional effect. To provide a less ambiguous test, we also use changes in search volume. That is, we hypothesize that for the sampling effect to be relevant, YouTube content must initially

https://support.google.com/trends/answer/4359597?hl=en&ref_topic=4365600.

stimulate the user's information search, and this would translate into increased search volume on the Internet in Music & Audio related context.

Following the literature, we start our analysis focusing on the sales displacement effect. Theoretical models often take into account the quality difference between a copy and the original; however, this concern becomes less relevant in the content of digital copies. Although the existing literature suggests a positive sales displacement effect, direct measures of illegal file sharing activities are often unavailable to researchers. On the other hand, proxies for file sharing such as Internet penetration have the potentially confounding effect of being correlated with both legal services like YouTube and illegal file sharing activities. By focusing on YouTube, we aim to provide direct evidence on whether a sales displacement effect extends to legal, free channels.

To test this hypothesis, we model the effects of the blackout as follows:

$$Y_{it} = \beta_0 + \beta_1 \, Warnereffect_{jt} + \boldsymbol{X}_{jt}\boldsymbol{\beta}_2 + Week_t + Artist_i + \epsilon_{ijt}$$

where Y_{it} is *twsales* in sales regressions, and *twtrends* in search regressions. We stacked the observations into an artist i, week t format because if we instead used fixed-effects at the album level, then all constant album-level characteristics would not be identified and hence useful variations within artists cannot be examined. Additionally, this allows a better comparison to the Google Trends terms than albums would. In fact, there are only a small number of cases where more than one album by the same artist show up in the same week (and hence had to be aggregated).

$\boldsymbol{X}_{jt}$ is the set of album j characteristics (including the radio chart-related characteristics matched to albums) as described in the previous section. These can vary over time t and also within artist i if the artist had more than one album in the SoundScan database over time. Label and genre indicators are album-specific and can also vary from album to album within artist i. To account for the unobserved heterogeneity of artists with respect to album sales and search volume, we include artist i fixed effects; to capture nonlinear trends over time, we also include a full set of week t dummies.

Warnereffect is one for the album-weeks with Warner (and its subsidiary) as labels during the blackout, which runs from the first week of January 2009 to the last week of September 2009. For instance, if Warner released an album five weeks prior to the blackout and it charted for 15 weeks, then *Warnereffect* would be zero for the first five weeks, and one for the following weeks. Some Warner albums will fall entirely within the blackout period, in which case we rely on variation across labels. In our baseline specifications, we prefer to use both variation within albums as well as across albums, but we also show that our results remain basically unchanged when we eliminate from our sample all albums that did not cross either of the blackout threshholds.

If all artists in our sample had only one album on the Billboard 200 chart, then of course no point estimates on the album-specific constant variables can be estimated. In our case, these point estimates are identified by 598 artists (out of 2261 in our sample) having multiple albums over the sample period. We believe that these album characteristics associated with multi-album artists are of interest in light of the existing literature. Below, we report our results using a full set of covariates. These point estimates are robust and significance is sometimes higher in simpler specifications. Similarly, we use unit sales because it yields a straightforward interpretation; however, using a log transformation of Y does not change the qualitative results.

Regression results from the fixed-effects model illustrating how content removal affected album sales are presented in Table 2 through Table 5. The four columns' specifications only differ by the time period before and after the nine-month blackout. That is, column (1) limits the period to one year before and after the blackout; column (2) to nine months before and after; column (3) to six months before and after; and column (4) to three months before and after. On the other hand, the tables differ by the size of the sample. That is, Table 2 uses the full sample; Table 3 drops the top 10 albums; Table 4 drops the top 25 albums; and

Table 5 drops the top 50 albums from the Billboard ranking. Standard errors are clustered by artist in all specifications.[16]

Table 2 shows that the Warner artists who had Billboard top 200 albums during the nine-month blackout on average sold larger quantities of albums, all else equal, when compared to the non-Warner artists during the same period. Looking across the four columns, the increase in sales (in units) ranges from 5,718 per week using one-year (pre and post) blackout to nearly 10,000 per week using a three-month window. The estimates represent causal effects and thus render support for the sales displacement hypothesis. The pattern in which these point estimates increase as we narrow the pre and post time periods shows that the results are stable and also consistent with the expectation that the policy effect would be stronger in the narrower time period.

The point estimates on the first week indicator show that sales are particularly high in the premiere week. The negative coefficients on the first album indicator suggest that new albums (either an artist's debut album or a new album in at least four year's time) tend to sell relatively small quantities compared to other albums on the Billboard chart. This finding is consistent with the Hendricks and Sorensen (2009)'s finding that consumers are less likely to be aware of new artists even with moderate levels of sales. On the other hand, we do not find any positive significant effects from either the sales or the chart duration of an artist's previous album. This result may be interpreted as that there is little forward spillover effect in our sample.

We also find that the number of weeks on the radio chart is positively associated with album sales while the number of weeks on the Billboard chart is negatively correlated with sales. The latter may capture the fact that sales tend to naturally decrease over time while the former may indicate some positive effects of radio play especially since we count radio weeks before an album may appear on the Billboard chart. However, we caution that these

[16] We also bootstrapped standard errors following Bertrand et al. (2004), but it did not change any of the qualitative results. Specifically, the point estimates on *Warnereffect* often became more significant at 1 percent in sales regressions with no change in search regressions.

are correlative effects, and we cannot provide a test of causal effects of radio play. Both the latest radio rank and the number of weeks since the last appearance on the radio chart are negatively associated with sales.[17] Albums with no radio rank tend to have lower sales all else equal.

Table 3 to Table 5 report the estimation results of the same specifications, dropping top albums from the sample. We do this because the distribution of album sales may be skewed toward superstars' sales but including interaction effects between the blackout and Billboard rank creates some endogeneity problems. The point estimates on *Warnereffect* exhibit a marked decrease in the estimated causal effect of the content removal on album sales, relative to the full sample estimates. Specifically, dropping the top 10, the point estimates range from 2500 to 3700 albums per week; dropping 25, they range from 1400 to 2000 albums per week; and dropping 50, they are just a few hundreds with statistical insignificance.

This is perhaps not an unexpected result, but assessing quantitative relevance of this attenuation pattern is of interest. That is, our results suggest not only that the sales displacement effect can be very large on average but it also diminishes relatively quickly as one goes down the chart ranking. The rest of the coefficient estimates follow similar patterns. For instance, the coefficient on the first week decreases and eventually becomes negative in Table 5. This is because as we drop the top 50, the remaining albums premiering in the bottom 150 debut lower than the superstars. New albums continue to sell less than those that are not, but the magnitude of the association keeps decreasing. Interestingly, in Tables 3 and 4, sales are negatively associated with the previous album's chart duration while in Table 5, they are positively correlated with previous album sales.[18]

As a robustness check, we run another set of regressions limiting the sample to albums

[17] This makes sense as the rank increases in absolute terms it is declining in terms of ranking. That is, this means that albums with higher ranked song(s) on the radio chart, conditional on having songs on the radio, are correlated with higher sales.

[18] Tables 3 and 4 contain the superstars, so they cannot increase too much in all likelihood. On the other hand, lower ranks were likely lower ranks before, so they are more likely to increase in Table 5.

that cross one of the two threshholds in the chart, either before and after January 1, 2009 or before and after the end of the blackout in September. To be precise, this eliminates the albums in the same period that did not cross either threshhold, so we lose some variations across albums; nonetheless, the results are consistent with the previous findings. Here, we do not restrict sample period, but each column in Table 6 represents the four rank drops we use in the previous tables.[19] Table 6 tells us that using only albums that experience both periods in our sample, the blackout is causally associated with an additional 7365 unit sales in the full sample, 2489 unit sales when the top 10 albums are dropped, 1343 unit sales dropping the top 25, and 486 unit sales dropping the top 50. Significance levels also taper off into insignificance.

Table 7 to Table 10 present the estimation results using the search intensity for artist names as the dependent variable, using the same specifications and independent variables as in Table 2 to Table 5. Tables 7 to 10 show that, while the point estimates on *Warner-effect* have negative signs, they are small relative to the sample mean and also statistically insigifnicant. Hence, the blackout seems to have little effect on the level of Internet user's search activity. We reiterate the caveat that our sample is limited to the top 200 most popular albums at any time, so lesser known artists may experience some greater promotional effect. However, the hypothesis that a greater exposure to free online content increases the user's level of interests finds no support in our data. Whether or not the 'sampling effect' leads to more or less album sales, our results suggest that more exposure does not lead to additional search.

There are other notable differences. For instance, first albums are positively associated with search volume (which is statistically significant in Tables 9 and 10) while this variable was negatively associated with sales. Previous album sales are negatively associated with search volume, where the point estimates are often statistically significant. This means that Internet users search more for the artist when a debut album (or a new album in a few years)

[19] If we additionally impose a specific time period restriction (for example three months before and after each threshold), then the qualitative results remain unchanged although point estimates become larger.

is ranked below say top 25, but they search less than they would if the artist previously had a hit album. Finally, Table 11 presents the regression results in parallel to Table 6 using only those albums that cross either of the two threshholds. The qualitative results remain unchanged.

6 Conclusion

Public performance rights are growing in importance as a source of remuneration for artists. Free online content services such as YouTube have become a prime music-listening portal with no software or subscription being required. While a great deal has been said about the potential role of these services in promoting and discovering new artists and music, our results cast some doubt on this widely discussed notion, at least with regards to top selling albums. Not only did we not find evidence for a promotional effect in terms of sales, we were unable to detect a statistically significant effect on the user's level of interests, as proxied by Internet search activities (Google Trends).

We showed that the removal of content from YouTube had a causal impact on album sales by upwards of on average 7,500 units per week for top albums. If we take, as an example, 4,000 to be the average effect of a top album, then a rough back-of-the-envelope calculation results in 4,000 $\times$ \$12 (the average CD price) $\times$ 20 (the average *wkson*) $\approx$ \$1 million of lost sales for a top album. If we assume additionally that Warner had say 40 albums on the Billboard 200 for a year, then the total lost sales become \$40 million per year. This sales displacement is not necessarily a problem per se, but whether the revenue generated from YouTube licensing is bigger or smaller than the lost sales is.

Specifically, in 2010, Warner had about 800 weekly observations in the top 200, where this week's album rank has a mean of 99 and a standard deviation of 58. Hence, this includes albums near the top as well as albums that never make it very far. Given our average chart weeks of 20.85, this means about 40 albums. The \$4,000 estimate used is close to the average treatment effect of \$3,678 in the last column of Table 4. That is, we assumed that in a typical year Warner may have one album hit the top 10 list and the rest are distributed in the top 10-200 range. \$40 million amounts to 1.6% of the Warner Music Group's Recorded Music revenues of \$2.455 billion for the fiscal year of 2010.

Ex ante there was little reason to expect that we would find sales displacement effect in this range. However, labels still choose to contract with YouTube, so the revenue from licensing must exceed that foregone from sales displacement. This is in contrast to the policy debates on the streaming services that do not make direct, private licensing agreements. For instance, as streaming has recently grown, some artists started to hold back new releases from subscription services during the peak selling period apparently recognizing this displacement effect while streaming services like Pandora have sought to lower the royalty rates through such bills as the Internet Radio Fairness Act, introduced in September 2012.[20]

Although we believe our study provides a strong support for creating digital performance rights such as the Digital Performance Right in Sound Recordings Act, we caution that any claims on social welfare cannot be made without knowing the overall effects of YouTube services on emerging and independent bands, as well as the compensation labels and artists receive from these services. Finally, we note that the promotional effect of noninteractive services (such as Pandora), where listeners have limited control over the content, could be different from that of interactive services (like Spotify), where users do have control. Our conjecture is that the latter is a stronger substitute for album sales than the former.

[20]This bill would move the "willing buyer, willing seller" standard for online music streaming to the one used for satellite and cable radio outlets like Sirius XM and Music Choice, which would result in a lower royalty rate, by requiring the Copyright Royalty Judges to consider factors including whether the licensing costs will have a "disruptive impact" on the industry.

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Table 1: Summary Statistics - Weekly Observations

Variable	Mean	Std. dev.
twsales	14016.101	30669.392
twtrends	948.447	2161.267
wkson	20.85	29.166
firstweek	0.12	0.325
firstalbum	0.244	0.429
previousalbumduration	17.39	28.424
previousalbumsales	564.427	1176.165
wocradio	5.183	8.436
lastweekradiorank	27.135	48.595
weeksinceradio	0.84	5.153
noradio	0.596	0.491
EMI	0.097	0.296
Sony	0.168	0.374
SonyBMG	0.081	0.273
Universal	0.247	0.431
Warner	0.145	0.352
Indy	0.245	0.43
Self-release	0.017	0.129
Blues	0.028	0.164
Children's	0.006	0.076
Christian	0.032	0.176
Classical	0.004	0.064
Electronic	0.09	0.287
Folk	0.165	0.371
Funk	0.093	0.29
Hip Hop	0.111	0.314
Holiday	0.015	0.121
Jazz	0.018	0.133
Latin	0.019	0.136
Pop	0.083	0.276
Reggae	0.002	0.04
Rock	0.334	0.472
N	20950	

The date range for this table is from January of 2008 to September 2010.

Table 2: Sales Regressions - Full Sample

	(1) One year	(2) Nine months	(3) Six months	(4) Three months
Warnereffect	5718.5*	7864.3**	7375.6*	9829.2**
	(2965.5)	(3385.6)	(3776.8)	(4159.7)
wkson	-534.2***	-554.2***	-554.9***	-641.3***
	(72.93)	(75.05)	(70.21)	(91.58)
wksonsq	1.928***	1.947***	1.914***	2.316***
	(0.458)	(0.448)	(0.381)	(0.446)
firstweek	26869.8***	28465.7***	27553.5***	26767.7***
	(1737.6)	(1990.0)	(2023.1)	(2215.9)
firstalbum	-4535.5	-9233.1**	-10723.5**	-10918.6*
	(2901.4)	(3924.7)	(4749.8)	(5679.7)
previousalbumduration	35.75	29.01	37.07	-101.2
	(65.27)	(79.01)	(98.01)	(71.41)
previousalbumsales	-1.367	-1.513	-1.961	-0.191
	(1.185)	(1.422)	(1.878)	(2.487)
wocradio	163.5***	115.1*	56.37	21.13
	(62.98)	(61.43)	(69.72)	(74.51)
lastweekradiorank	-22.89	-23.64	-28.62**	-22.45
	(16.30)	(15.27)	(12.43)	(15.01)
weeksinceradio	-412.5***	-604.6***	-866.5***	-1030.4***
	(63.44)	(98.63)	(140.0)	(165.4)
noradio	-3969.6*	-4981.2**	-5301.7**	-7597.1**
	(2033.5)	(2323.6)	(2485.9)	(3366.5)
N	20950	17314	13398	9533

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3: Sales Regressions - Dropping Top 10 Albums

	(1) One year	(2) Nine months	(3) Six months	(4) Three months
Warnereffect	2458.0**	2990.2**	3155.6**	3678.3**
	(1040.5)	(1197.0)	(1366.5)	(1738.0)
wkson	-166.7***	-179.0***	-198.4***	-247.4***
	(18.54)	(19.93)	(22.98)	(32.77)
wksonsq	0.540***	0.558***	0.632***	0.895***
	(0.114)	(0.111)	(0.116)	(0.169)
firstweek	3722.7***	4197.3***	4267.3***	4181.3***
	(412.5)	(473.9)	(562.2)	(709.7)
firstalbum	-958.9	-1514.9	-2427.8*	-4513.7**
	(901.6)	(1006.6)	(1310.2)	(1966.3)
previousalbumduration	-25.76*	-31.79**	-44.82***	-61.30**
	(15.03)	(14.66)	(16.90)	(25.43)
previousalbumsales	-0.0217	0.0735	0.374	0.749
	(0.285)	(0.321)	(0.432)	(0.780)
wocradio	120.0***	114.7***	122.3***	129.5***
	(23.77)	(26.09)	(29.73)	(35.50)
lastweekradiorank	-1.306	-3.280	-5.977	-7.441
	(3.536)	(3.602)	(3.990)	(5.021)
weeksinceradio	-154.4***	-235.3***	-333.7***	-496.6***
	(31.68)	(46.49)	(73.82)	(98.82)
noradio	-523.9	-1113.9	-1886.9**	-3108.6**
	(721.6)	(826.7)	(938.6)	(1415.3)
N	19809	16373	12671	9011

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4: Sales Regressions - Dropping Top 25 Albums

	(1) One year	(2) Nine months	(3) Six months	(4) Three months
Warnereffect	1418.8**	1871.0**	1968.6**	1869.6*
	(713.6)	(760.6)	(828.5)	(1057.1)
wkson	-78.72***	-87.31***	-99.75***	-134.8***
	(10.04)	(10.69)	(13.02)	(18.79)
wksonsq	0.220***	0.226***	0.270***	0.444***
	(0.0539)	(0.0503)	(0.0577)	(0.0930)
firstweek	810.4***	908.8***	645.4**	329.5
	(231.5)	(267.9)	(309.5)	(394.6)
firstalbum	-1044.6*	-1524.3**	-2046.0***	-3247.9***
	(538.2)	(637.2)	(788.6)	(1118.2)
previousalbumduration	-23.75***	-22.71***	-24.35***	-28.53***
	(7.441)	(6.661)	(8.282)	(10.95)
previousalbumsales	0.164	0.237*	0.326*	0.520
	(0.132)	(0.127)	(0.173)	(0.331)
wocradio	79.65***	79.73***	93.30***	98.30***
	(15.06)	(16.86)	(19.22)	(22.18)
lastweekradiorank	-1.400	-3.147	-5.120**	-6.928**
	(1.928)	(1.943)	(2.270)	(2.910)
weeksinceradio	-94.21***	-158.2***	-225.9***	-356.4***
	(28.89)	(37.29)	(48.81)	(79.34)
noradio	291.3	-42.86	-451.8	-826.5
	(408.8)	(458.9)	(537.0)	(784.0)
N	18165	15006	11603	8243

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 5: Sales Regressions - Dropping Top 50 Albums

	(1) One year	(2) Nine months	(3) Six months	(4) Three months
Warnereffect	218.2	491.9	555.1	347.4
	(448.5)	(481.7)	(491.7)	(615.6)
wkson	-29.52***	-38.30***	-48.40***	-71.75***
	(5.275)	(5.850)	(7.259)	(9.083)
wksonsq	0.0710***	0.0873***	0.125***	0.248***
	(0.0204)	(0.0224)	(0.0273)	(0.0362)
firstweek	-329.5**	-362.1**	-560.6***	-851.0***
	(129.2)	(148.8)	(172.2)	(215.7)
firstalbum	-503.7*	-633.4*	-637.2	-1142.9*
	(276.9)	(364.8)	(454.6)	(684.6)
previousalbumduration	-8.585*	-4.704	-3.973	-5.019
	(5.002)	(5.265)	(6.521)	(7.230)
previousalbumsales	0.204***	0.194**	0.259*	0.548***
	(0.0761)	(0.0937)	(0.143)	(0.158)
wocradio	30.44***	28.91***	33.44***	40.58***
	(8.587)	(9.909)	(11.78)	(13.39)
lastweekradiorank	-0.601	-1.452	-1.844	-3.477**
	(1.322)	(1.331)	(1.482)	(1.660)
weeksinceradio	-53.83***	-92.89***	-117.7***	-179.3***
	(15.58)	(20.26)	(23.13)	(37.60)
noradio	-169.0	-396.5	-656.7**	-955.7**
	(244.1)	(270.5)	(318.1)	(458.7)
N	15360	12693	9800	6967

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 6: Sales Regressions - Albums Crossing Thresholds

	(1)	(2)	(3)	(4)
	Full Sample	Drop Top 10	Drop Top 25	Drop Top 50
Warnereffect	7365.4**	2489.4**	1343.5*	486.5
	(3366.3)	(1140.4)	(724.0)	(515.8)
wkson	-362.1***	-132.9***	-64.38***	-30.21***
	(63.22)	(21.09)	(11.20)	(6.310)
wksonsq	1.259***	0.416***	0.163***	0.0628***
	(0.338)	(0.107)	(0.0512)	(0.0227)
firstweek	38137.5***	750.2	-1350.0***	-1097.7***
	(4583.1)	(785.6)	(368.1)	(213.2)
firstalbum	-16303.1**	-2806.6	-2877.3	-960.6
	(6757.3)	(3474.9)	(2309.8)	(761.4)
previousalbumduration	-9.196	-28.97	-16.11	6.289
	(76.40)	(26.17)	(17.15)	(9.232)
previousalbumsales	-3.576	-0.283	0.106	0.191
	(2.627)	(0.713)	(0.545)	(0.280)
wocradio	161.8**	138.2***	84.14***	33.62***
	(66.38)	(29.94)	(19.59)	(11.04)
lastweekradiorank	-25.53*	-10.50**	-6.699***	-3.303**
	(14.11)	(4.196)	(2.266)	(1.493)
weeksinceradio	-301.5***	-108.9***	-75.62**	-73.02***
	(102.4)	(38.08)	(29.51)	(22.10)
noradio	-2415.0	-1149.1	-568.8	-723.3*
	(3199.7)	(1060.0)	(633.2)	(387.5)
N	10489	9929	9102	6976

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 7: Trends Regressions - Full Sample

	(1) One year	(2) Nine months	(3) Six months	(4) Three months
Warnereffect	-81.43	-52.87	-70.26	-39.28
	(155.9)	(152.2)	(153.0)	(134.6)
wkson	-3.447	-2.686	-2.137	-0.899
	(3.317)	(3.732)	(4.179)	(4.716)
wksonsq	0.00412	-0.00195	-0.00234	-0.00239
	(0.0142)	(0.0153)	(0.0158)	(0.0182)
firstweek	108.6***	132.4***	104.6**	105.0*
	(38.53)	(41.46)	(51.92)	(56.92)
firstalbum	-86.69	-93.68	-61.65	83.66
	(175.6)	(200.3)	(189.3)	(176.9)
previousalbumduration	2.415	2.835	3.157	6.845**
	(2.090)	(2.462)	(2.560)	(3.475)
previousalbumsales	-0.111**	-0.122*	-0.151*	-0.252*
	(0.0558)	(0.0656)	(0.0774)	(0.138)
wocradio	7.858*	6.672*	7.304*	5.228
	(4.039)	(3.829)	(4.026)	(4.082)
lastweekradiorank	-0.788**	-0.974**	-1.121**	-1.209**
	(0.357)	(0.445)	(0.462)	(0.538)
weeksinceradio	-23.15**	-34.03***	-33.88***	-35.81***
	(10.44)	(13.01)	(11.08)	(9.514)
noradio	44.64	23.78	27.06	-54.35
	(141.6)	(160.8)	(168.6)	(190.3)
N	20950	17314	13398	9533

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 8: Trends Regressions - Dropping Top 10 Albums

	(1) One year	(2) Nine months	(3) Six months	(4) Three months
Warnereffect	-64.70	-45.34	-42.24	-54.71
	(84.15)	(87.84)	(90.80)	(84.32)
wkson	-0.506	-0.551	-1.605	-0.772
	(2.166)	(2.291)	(2.338)	(3.021)
wksonsq	-0.00293	-0.00567	-0.00259	-0.00417
	(0.00849)	(0.00920)	(0.00975)	(0.0130)
firstweek	16.96	20.95	-2.291	-8.568
	(25.47)	(28.12)	(33.88)	(40.00)
firstalbum	10.39	25.61	36.16	235.7
	(108.2)	(136.0)	(167.4)	(170.6)
previousalbumduration	-0.117	-0.165	0.0168	3.870**
	(1.102)	(1.395)	(1.436)	(1.775)
previousalbumsales	-0.0515*	-0.0540	-0.0759**	-0.178**
	(0.0293)	(0.0339)	(0.0323)	(0.0753)
wocradio	5.278**	5.076*	5.890*	6.102*
	(2.546)	(2.713)	(3.181)	(3.706)
lastweekradiorank	-0.376	-0.569	-0.586*	-0.772*
	(0.272)	(0.353)	(0.336)	(0.400)
weeksinceradio	-11.66***	-19.42***	-20.65***	-25.98***
	(3.916)	(5.271)	(5.639)	(5.377)
noradio	-20.23	-49.61	-54.12	-135.4
	(67.47)	(85.68)	(93.53)	(137.5)
N	18176	15006	11603	8243

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 9: Trends Regressions - Dropping Top 25 Albums

	(1)	(2)	(3)	(4)
	One year	Nine months	Six months	Three months
Warnereffect	-60.55	-40.65	-36.49	-57.75
	(78.97)	(83.11)	(83.80)	(80.09)
wkson	-0.150	-0.353	-1.717	-0.630
	(1.383)	(1.518)	(1.895)	(2.612)
wksonsq	-0.00371	-0.00572	-0.00239	-0.00475
	(0.00603)	(0.00652)	(0.00786)	(0.0114)
firstweek	25.84	27.81	-1.054	0.987
	(25.16)	(28.07)	(33.72)	(41.39)
firstalbum	164.2**	196.8*	248.0**	329.5*
	(72.32)	(104.4)	(124.7)	(187.9)
previousalbumduration	0.138	0.628	1.548	3.350**
	(0.867)	(1.171)	(1.057)	(1.416)
previousalbumsales	-0.0334	-0.0409	-0.0680**	-0.139**
	(0.0267)	(0.0362)	(0.0314)	(0.0616)
wocradio	1.957	2.516	3.361	3.880
	(1.717)	(1.777)	(2.294)	(3.198)
lastweekradiorank	-0.151	-0.299	-0.430*	-0.601**
	(0.219)	(0.241)	(0.244)	(0.303)
weeksinceradio	-9.013***	-16.70***	-20.17***	-28.03***
	(3.105)	(4.939)	(5.069)	(5.743)
noradio	-0.774	-21.57	-49.11	-126.4
	(69.80)	(84.53)	(98.14)	(136.5)
N	18176	15006	11603	8243

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 10: Trends Regressions - Dropping Top 50 Albums

	(1)	(2)	(3)	(4)
	One year	Nine months	Six months	Three months
Warnereffect	-51.12	-35.94	-37.88	-44.74
	(74.44)	(80.18)	(90.00)	(71.12)
wkson	-1.167	-1.879*	-3.249**	-1.979
	(1.027)	(1.044)	(1.506)	(1.680)
wksonsq	0.000331	-0.00103	0.00276	0.000833
	(0.00483)	(0.00470)	(0.00583)	(0.00875)
firstweek	21.59	20.76	-13.86	-19.22
	(26.05)	(26.70)	(30.18)	(29.51)
firstalbum	157.5**	196.3*	295.3**	282.5
	(70.50)	(100.1)	(146.2)	(185.5)
previousalbumduration	0.802	1.588	2.149*	2.788*
	(0.993)	(1.291)	(1.258)	(1.600)
previousalbumsales	-0.0486	-0.0681*	-0.0754**	-0.0960*
	(0.0299)	(0.0398)	(0.0325)	(0.0574)
wocradio	-0.385	-0.0690	0.313	-0.0262
	(1.479)	(1.281)	(1.199)	(1.657)
lastweekradiorank	-0.0888	-0.290	-0.344	-0.301
	(0.209)	(0.242)	(0.234)	(0.236)
weeksinceradio	-8.132**	-16.53***	-19.03***	-26.28***
	(3.606)	(6.304)	(6.700)	(7.899)
noradio	24.75	10.16	-7.729	-75.05
	(64.82)	(75.91)	(85.16)	(91.46)
N	15378	12692	9800	6967

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 11: Trends Regressions - Albums Crossing Thresholds

	(1) Full Sample	(2) Drop Top 10	(3) Drop Top 25	(4) Drop Top 50
Warnereffect	-51.63	-70.78	-68.60	-46.90
	(143.6)	(97.16)	(96.34)	(90.79)
wkson	-4.058	-1.463	-1.094	-2.032
	(3.969)	(2.475)	(1.733)	(1.454)
wksonsq	0.00373	-0.000606	-0.00152	0.00255
	(0.0141)	(0.00942)	(0.00686)	(0.00521)
firstweek	131.9*	-29.91	-18.18	-31.06
	(70.03)	(45.73)	(50.89)	(53.84)
firstalbum	284.3	202.9	148.7	95.66
	(242.4)	(143.1)	(138.6)	(140.0)
previousalbumduration	4.525	1.675	0.999	-0.0525
	(3.784)	(2.194)	(1.905)	(2.060)
previousalbumsales	-0.132	-0.0336	-0.0119	0.01000
	(0.101)	(0.0645)	(0.0645)	(0.0746)
wocradio	6.570*	5.367*	2.126	-0.611
	(3.741)	(3.145)	(2.206)	(1.842)
lastweekradiorank	-0.806*	-0.548	-0.299	-0.0816
	(0.428)	(0.339)	(0.286)	(0.289)
weeksinceradio	-28.82*	-14.72**	-12.77**	-11.40**
	(14.72)	(5.757)	(4.945)	(5.571)
noradio	45.13	-67.68	-34.18	31.41
	(219.9)	(105.9)	(111.9)	(95.49)
N	10489	9929	9102	6976

Standard errors in parentheses; genre, label, and week coefficients not shown for brevity.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$