Video Killed the Radio Star?
Online Music Videos and Recorded Music Sales

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Abstract

In experience goods markets, free samples help consumers make informed purchase decisions. However, sampling may also let consumers substitute purchases with free consumption. We study this trade-off in the market for recorded music where consumers can sample songs by watching free music videos online. Identification comes from two quasi-experiments in Germany. In 2009, virtually all official music videos were blocked from *YouTube* due to a legal dispute. The situation remained largely unchanged until the dedicated platform *VEVO* entered the market in 2013, making videos of a large number of artists available over night. We find that both restricting and enabling access to online videos has consistent complementary effects on digital music sales, but there is not much evidence for an effect on physical sales. Moreover, online videos are much more effective in triggering downloads of songs by new artists compared to established artists. This yields interesting implications for managers and policy.

Keywords: Sampling, Displacement, Promotion, Sales

1 Introduction

In markets with experience goods, product quality cannot be completely assessed prior to consumption. Hence, consumers collect external information from popularity rankings (Tucker and Zhang, 2011; Hendricks et al., 2012), recommendations (Oestreicher-Singer and Sundararajan, 2012; Dewan and Ramaprasad, 2012) and from related products whose quality attributes are already known (Hendricks and Sorensen, 2009). Firms also advertise to inform consumers about product quality. A specific form of advertising is to disclose product quality by letting consumers try (parts or versions of) the product for free.[1] Examples are coupons and tastings at retail stores, shareware, radio airplay, and music videos. This process of sampling helps consumers find out whether a product's characteristics match their preferences.

Disclosing quality with free samples is costly. Physical experience goods such as wine or food clearly have non-zero marginal cost. In other cases, such as digital music, marginal costs are negligible, but consumers may perceive the free consumption of an online music video as a close substitute to actually buying a song, especially if the song is not consumed repeatedly and via on- and offline channels. Trading-off these costs and benefits, firms will strive to set the optimal level of sampling, i.e. how much information to disclose and to whom (Jain et al., 1995; Bawa and Shoemaker, 2004; Chellappa and Shivendu, 2005; Halbheer et al., 2014).

However, finding the optimal level of sampling is often not a choice in digital markets. For example, music and movies files are regularly uploaded to Internet platforms (which may or may not have licenses for the distribution of such content), leaving the firm with little control about whether and how much product information to disclose (Peitz and Waelbroeck, 2006; Gopal et al., 2006; Bhattacharjee et al., 2006, 2007). The interesting question then is if sampling can still be an effective trigger of demand even if the firm cannot keep some consumers from consuming the sample instead of buying the product

[1] When quality is costly, advertising may not be credible. Theory suggests two mechanisms to solve this problem. The firm can either build a reputation for quality in a repeated interaction with the consumer (Klein and Leffler, 1981; Shapiro, 1983; Allen, 1984), or directly disclose its true level of quality. The latter is credible either because it is costly to reveal quality or because firms have an incentive to be associated with their true quality in a sequential process of quality unraveling in the market (Grossman, 1981; Milgrom, 1981; Dranove and Jin, 2010).

(Wang and Zhang, 2009). Further, the effect of free samples may depend on how accurately consumers can form expectations about a product's characteristics. Information about related products may sometimes help. In the music industry for example, Hendricks and Sorensen (2009) show that sales of older (lesser known) albums increase when an artist releases a new album. Therefore the promotional effect of sampling may be stronger for products about which there is less information available to consumers.

We study these questions in the empirical context of recorded music. Like radio airplay, music videos have long been a tool to promote sales of songs and albums by letting consumers sample to assess whether the music matched their preferences.

Online video has become an important channel for music listening and – either through direct search or (automated) recommendation – for music discovery.[2] This is true for almost all countries, but much less so for Germany. Because of a royalty dispute between *YouTube* and the monopoly royalty collection society which represents artists and publishers (not record labels), a large fraction of videos that contain music cannot be accessed in Germany since April 2009.[3] Much of the same content is easily accessible in a vast majority of other countries.[4] As a (delayed) response, a consortium driven by two major record labels negotiated their own deal with the royalty collection society and launched the dedicated platform *VEVO* in October 2013, which in most other countries is simply a channel on *YouTube*.

We make use of this unique setting to study the causal link between sampling through online music video and sales of recorded music. We identify effects in a standard difference-in-differences setting, looking at sales dynamics in response to (positive and negative) shocks in the supply of online videos directly before and after the events in 2009 and 2013.

[2] According to an online survey of 3,000 consumers in the US (Nielsen ePanel 2012, `http://www.nielsen.com/us/en/press-room/2012/music-discovery-still-dominated-by-radio--says-nielsen-music-360.html`), the top three channels by which consumers discover music is through radio (48%), friends and relatives (10%), and *YouTube* (7%). Consumers under the age of 20 listen to music more often on *YouTube* (64%) than on the radio (56%), through iTunes (53%) or on CD (50%). Digital stores such as *Amazon*, *iTunes* and *Beatport* also offer 30–90 seconds samples for free. However search results for songs usually list music video pages much higher than digital stores.

[3] See New York Times, 'Royalty Dispute Stops Music Videos in Germany', April 2, 2009, `http://www.nytimes.com/2009/04/03/technology/internet/03youtube.html`.

[4] More than 60% of the 1000 worldwide most viewed videos (which do not all contain music) are blocked in Germany, while only 0.9% are not accessible in the US, see `http://apps.opendatacity.de/gema-vs-youtube/en`.

Using these two quasi-experiments affecting the German market, we make a distinction between the effect of removing access to online music videos and the effect of making online music videos available. Our data lets us distinguish between paid downloads and physical sales. Further, to test the idea that sampling is more effective if consumers have less information about product quality from related products, we also look at differences between new and established artists.

We find that the promotional effect of online music videos is big enough to offset sales displacement of songs even when firms cannot control how intensely consumers sample. This is true in the digital download market and the effect is consistent in both settings: Removing access to music videos decreases sales, enabling access to music videos increases sales. Interestingly, we do not find much of an effect on physical sales, suggesting that substitutability across physical and digital channels is low. The impact is stronger for artists that are new to market, suggesting that sampling plays a more important role for artists that are less known to consumers.

Related to our work, Hiller (2015) looks at the effect of *YouTube* on US album sales in response to a dispute between Warner Music and *YouTube* in 2009. The paper provides evidence that the promotional effect varies with popularity, however the main result is that online videos displace sales, especially for best-selling albums. Our approach differs in that our data and set-up lets us cover almost the entire market (top 1000 bestselling songs in a given week), to distinguish between physical and digital sales, to observe song-level information on video availability, and to differentiate between removing and adding online videos by exploiting two quasi-experiments at different points in time.

We discuss implications for firms trying to successfully introduce new products to the market as well as implications for copyright policy. If promotional effects can offset losses even without considering indirect compensation via royalties or shared advertising revenues, this may mean that restricting unpaid consumption can not only have negative effects on overall welfare, but may help to provide the dynamic incentives needed for artists and firms to invest in cultural goods.

2 Mechanisms and background

2.1 Substitution and promotion through sampling

Availability of samples can have two intuitive effects: First, samples can inform potential consumers about the existence and characteristics of a given horizontally differentiated product. This lets consumers match products to their preferences, affecting pricing and ultimately profits (Peitz and Waelbroeck, 2006). However, samples may replace consumption of the actual product, especially if the sample is very similar to the product, fo example (near-perfect) copies of digital goods (Danaher et al., 2010; Danaher and Smith, 2014). For both effects to matter, the sample has to be informative about the core product – otherwise sampling would convey no information about the product itself and there would be no promotion effect, and it has to be a sufficiently close substitute – otherwise potential customers would not weigh up continued free consumption versus purchase of the product.

In what follows, we use variations in one (i.e. the *substitutability* of sample and product) conditional on the other (i.e. the *informativeness* about the product) in an attempt to pin down the promotional effect. In our study period, downloads from retailers such as iTunes and Amazon were the dominant source of revenues in the market. This makes the digital download market of prime interest to us. We then vary *substitutability* with respect to online music videos by looking at physical purchases, where the effect of online music videos may be weaker. We further distinguish between artists that are new to market and established artists to vary *informativeness*. The idea is that online music videos may be more effective in promoting songs of relatively unknown artists, because there is less other information about the artist available to consumers (e.g. via earlier songs and albums, Hendricks and Sorensen, 2009).

Finally, it is not obvious that removing and enabling access to music videos has symmetric effects. We therefore look at two distinct quasi-experiments described below.

2.2 Variation from *YouTube* and the *GEMA* shock

The video platform *YouTube* provides a unique setting to study the effect of online music videos on recorded music sales because a large portion of the most popular videos on *YouTube* are music video clips. While *YouTube* has contracts with rights-holders in most countries, the question of corresponding compensation is subject to an ongoing legal dispute between *YouTube* and *GEMA* in Germany.

GEMA (Gesellschaft für musikalische Aufführungs- und mechanische Vervielfältigungsrechte, society for musical performing and mechanical reproduction rights) is the state-authorized (de-facto monopolist) collecting society and performance rights organization in Germany.[5] Collecting societies exist to ensure that royalties from any kind of reproduction (e.g. physical and digital reproduction, public performance, radio airplay, etc.) arrive at artists and publishers, making them important institutions for artists because royalties are a major part of income, independent of any private contracts with record labels (Kretschmer, 2005). A large international network of sister collection societies represents the rights of German artists/publishers in international markets, and *GEMA* fulfills the same role for international artists/publishers in the German market. That is, virtually every professional musician is either directly or indirectly a member of *GEMA*, which is also reflected in the so-called '*GEMA* presumption', a case law presumption that rights of all musical works are managed by *GEMA*.[6]

After an initial agreement between *YouTube* and *GEMA* had expired in 2009, negotiations about the appropriate level of compensation were repeated. In fear of high subsequent payments, *YouTube* began blocking music videos on April 1st 2009.[7] Figure 1, depicting *Google* Trends search volume for the term "gema" from April 2008 to April 2010, shows a spike in the week when the blocking began, but not much systematic movement before and after. This suggests that the shock came unexpected to consumers and most artists, publishers and record labels.[8]

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[5] Examples for international counterparts are BMI, ASCAP and SESAC in the United States of America, PRS in the United Kingdom, SACEM in France and SGAE in Spain.

[6] See `http://kluwercopyrightblog.com/2012/10/01/the-gema-presumption-and-the-burden-of-non-liquet-germany/`. According to the annual report, *GEMA* had 67,266 members and distributed 692,3 million Euro in royalties in 2012.

[7] See New York Times, 'Royalty Dispute Stops Music Videos in Germany', April 2, 2009, `http://www.nytimes.com/2009/04/03/technology/internet/03youtube.html?_r=1`.

[8] There is no evidence that *YouTube* systematically warned content owners in Germany before blocking

Still in 2015, 60% of the 1000 most viewed videos worldwide are blocked in Germany, while only 0.9% are not accessible in the US.[9] However, note that this does not necessarily imply that German *YouTube* users do not have access to any music (videos). Publishers and artists can negotiate independent contracts with any online and offline licensee, so publishers and artists may decide to drop out of *GEMA* or their national collecting society to reach individual agreements with *YouTube* in Germany.[10] However, this may not be optimal for at least two reasons. First, royalty income from digital distribution may represent too small an amount to forgo all other royalty income (e.g. income from public performance). Second, by joining a collecting society, individuals benefit from reduced contracting cost and increased bargaining power. This is even more beneficial for members of international collecting societies where it can be especially costly to negotiate with various potential licensees abroad, e.g. because of substantial differences across legal systems. To ensure we do not pick up this potential endogeneity of selecting into (or out of) *YouTube* in our estimates, we focus on a very short time window of four weeks before and after the blocking began on April 1st 2009.

Specific legal issues seem to make it complicated to reach an agreement between *GEMA* and *YouTube*. According to a statement by Rolf Budde, member of the *GEMA* advisory board, *YouTube* insists on a non-disclosure agreement.[11] Because *GEMA* is required by law to publish the exact royalty paying schemes in Bundesanzeiger, an official publication of the Federal Republic of Germany (similar to the Federal Register in the United States), this is not feasible.[12] Reportedly, because of this deadlocked situation, negotiations have been suspended, and the involved parties started to consult the arbitration board of the German Patent and Trademark Office for mediation in January 2013.[13]

videos.

[9] See `http://apps.opendatacity.de/gema-vs-youtube/en`.

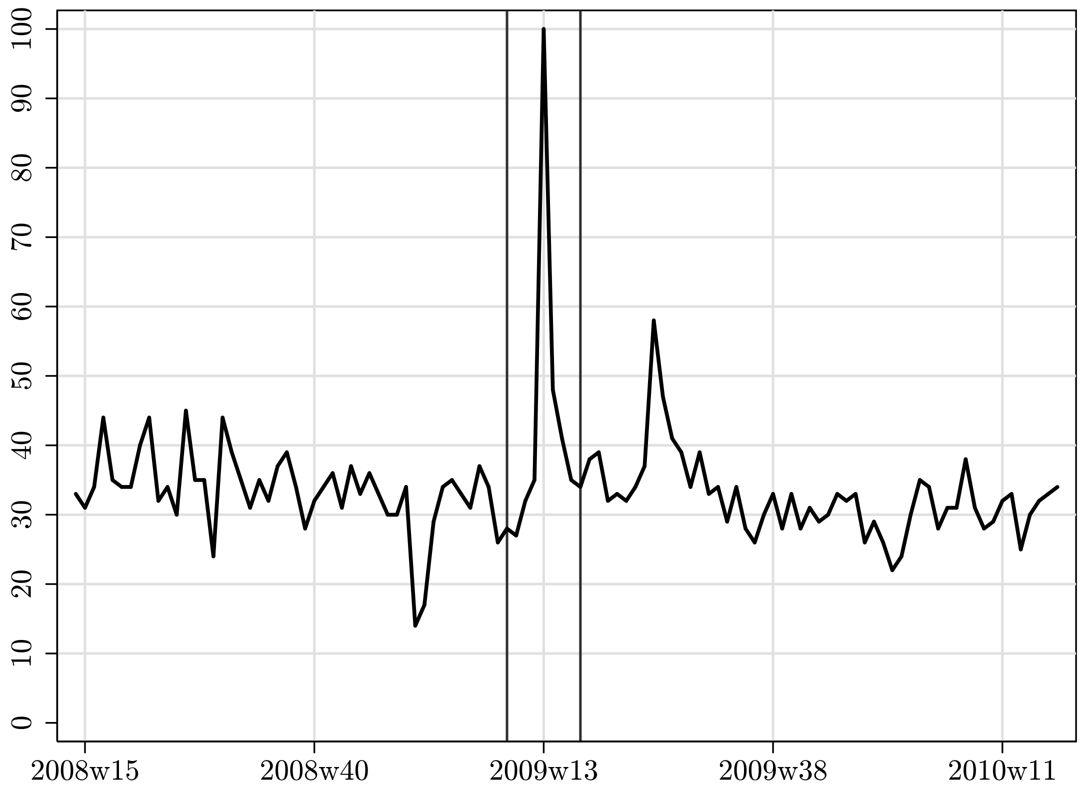
[10] After careful research, we could only find anecdotal evidence of one case where a band seemingly has opted out of *GEMA*. Videos in the official *YouTube* channel of the successful German punk-rock band 'Die Ärzte' are accessible in Germany. See `http://www.spiegel.de/netzwelt/web/netzwelt-ticker-warum-das-neue-aerzte-album-komplett-auf-youtube-laeuft-a-828244.html`. It is not clear whether the band opted out of *GEMA*. When we asked the management of the band for a statement, they did not want to comment on the issue.

[11] Budde made that statement being a panelist at an industry conference in January 2013. Ironically, the corresponding video can be found on *YouTube*: `https://www.youtube.com/watch?v=Hh3Ks4Kxvtk`.

[12] §13(2), Gesetz über die Wahrnehmung von Urheberrechten und verwandten Schutzrechten (UrhWahrnG; Law on the Administration of Copyright initiated in 1965).

[13] See `https://www.gema.de/uploads/media/Press_Release_GEMA_YouTube_Arbitration_Board_eng.pdf`. This is an official procedure provided in §14 UrhWahrnG.

Figure 1: Google Trends Search Volume for *GEMA*



Relative Google *search volume for "gema" in Germany, April 2008 – April 2010. Vertical lines indicate the sample period for the econometric analysis below.*

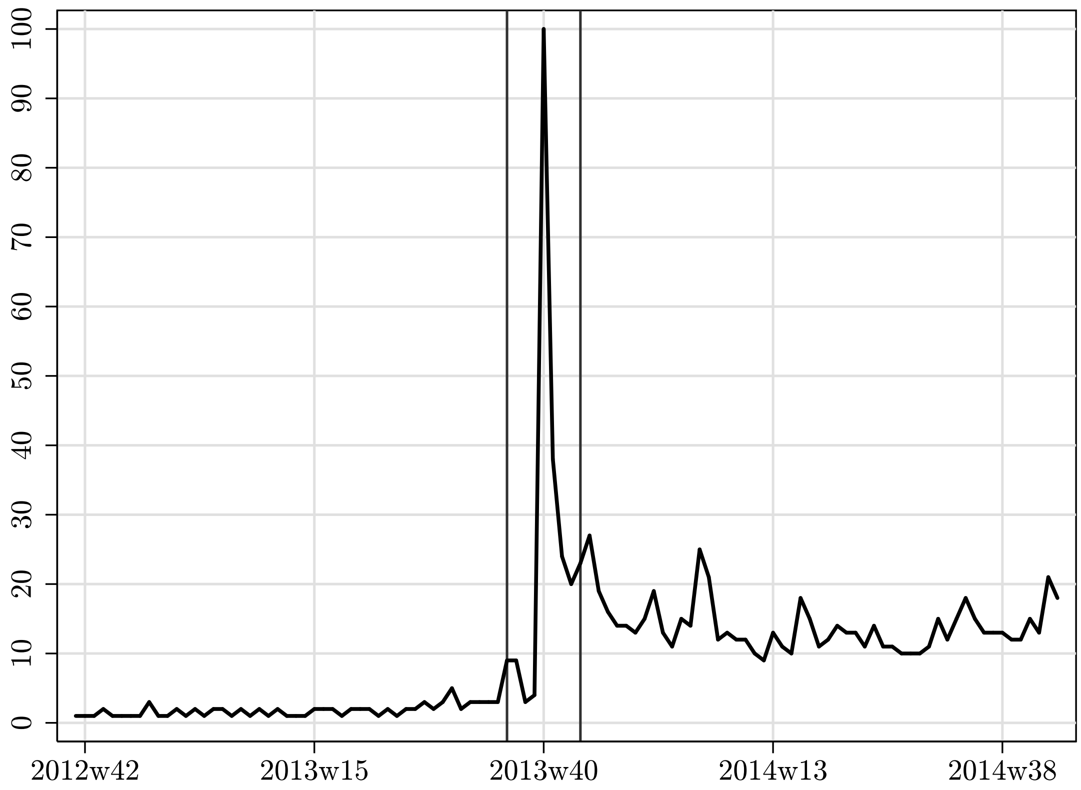
2.3 Supply-side reactions and the launch of *VEVO*

Anecdotal evidence suggests that the *GEMA*-*YouTube* dispute is controversial among German artists, which may explain why the negotiation strategy of *GEMA* (democratically representing its members) appears to be unchanged since 2009.

Some artists seem to believe in the promotional effect of online music videos. For example, the popular electro/hip-hop band *Deichkind* posted a raging comment on their *Facebook* page after finding out that their newly uploaded music video was being blocked.[14] Much in contrast, rap musician *Jan Delay* and the rockband *Element of Crime* said in interviews that they do not think that a potential promotional effect of *YouTube* can outweigh losses

[14]The posting from March 9th, 2012 reads "Whether it's the record label, *YouTube* or *GEMA*, whoever's responsible. We want our videos to be seen. Finally get your s*** sorted out and do your homework! You are a barrier to evolution and you are irritating the crap out of us.", `http://www.spiegel.de/netzwelt/web/deichkind-zum-gema-streit-ihr-seid-evolutionsbremsen-a-820703.html`.

Figure 2: Google Trends Search Volume for *VEVO*



Relative Google *search volume for "vevo" in Germany, October 2012 – October 2014.*
Vertical lines indicate the sample period for the econometric analysis below.

due to substitution.[15] Accordingly, both argue for an adequate compensation from video streaming services to counteract sales displacement.

Record labels are per definition not members of *GEMA* and therefore do not receive any royalty income. However, on top of a potential positive effect on record sales, they can directly benefit from advertising revenues generated by *YouTube*. Not surprisingly therefore, representatives of *Sony Music* and *Universal Music* have publicly criticized *GEMA* for not working harder towards an agreement.[16] These two labels with a joint market share of more than 46% in 2012 hold majority stakes in the music video service *VEVO*.[17] Since its

[15] The interview with *Jan Delay* was published in Der Spiegel 16/2012, `http://www.spiegel.de/spiegel/print/d-85065968.html`. In an interview with Radio Bayern 2, Sven Regener, singer of *Element of Crime*, says (referring to *YouTube*): "A business model based on people who produce the content not getting any money is not a business model, it's crap. Otherwise people are welcome to have Kim Schmitz *(founder of the filesharing website Megaupload)* sing the songs to them", see `http://www.br.de/radio/bayern2/sendungen/zuendfunk/regener_interview100.html`.

[16] See billboard.com, 2011, `http://www.billboard.com/biz/articles/news/publishing/1177342/gema-under-fire-for-royalties-dispute-with-youtube`.

[17] Market share data according to Nielsen Soundscan for the US, see `http://www.statista.com/`

launch in 2009, *VEVO* is partnering with *YouTube* in most countries. Accordingly, 97% of its 51.6 million unique viewers accessed *VEVO*-content through *YouTube* in December 2012, making *VEVO* the most viewed channel on *YouTube*, accounting for a third of all unique viewers on *YouTube*.[18] As a result of the dispute between *YouTube* and *GEMA* in Germany, *VEVO* negotiated its own deal with *GEMA* and launched the dedicated platform `vevo.com` – with content hosted outside of *YouTube* – in the German market on October 1st 2013.[19] Overnight, 75,000 music videos, including highly popular ones such as Justin Bieber's hit "Baby" with over 900 million *YouTube* views (as of August 2013, excluding Germany), became available on the German Internet. The launch was accompanied by press reports in national outlets, triggering exceptional interest among German consumers in the first week, with a lasting effect throughout the following year (see *Google* Trends search volume in figure 2). As a result, *Google* in Germany, compared to *Google* in the US, lists results that link to `vevo.com` more prominently than results that link to `youtube.com` when consumers search for songs (see table A.1).

3 Methods and data

3.1 Identification strategy

We identify the effect of music videos on sales of recorded music using exogenous variation from removing access to videos on *YouTube* (in April 2009) and from making videos available through the entry of *VEVO* (in October 2013).

The econometric model is based on a standard difference-in-differences before and after comparison, i.e.

$$log(Sales^k_{it} + 1) = \alpha + \beta \log(Age_{it} + 1) + \delta\,(After_t \times Video_i) + \nu_t + \mu_i + \varepsilon_{it}, \qquad (1)$$

where $Sales^k_{i,t}$ are unit sales of song i via channel k (physical, digital) in week t. $Age_{i,t}$ is a measure of the song's stage in its lifecycle. As explained in more detail below, $Video_i$

`statistics/317632/market-share-record-companies-label-ownership-usa/`. For *VEVO* see `https://en.wikipedia.org/wiki/Vevo`.

[18] See `http://www.comscore.com/Insights/Press-Releases/2013/1/comScore-Releases-December-2012-U.S.-Online-Video-Rankings`.

[19] See `http://thenextweb.com/media/2013/10/01/music-video-site-vevo-launches-in-germany/`.

indicates whether there is at least one video on *YouTube* and/or *VEVO* corresponding to song i. $After_t$ indicates the time period after the *GEMA* shock or *VEVO*'s entry, respectively. The preferred specification further includes week fixed effects ν_t and song fixed effects μ_i. Therefore we cannot separately identify coefficients for $Video_i$ and $After_t$ as they are absorbed by song and week fixed effects, respectively The error term ε_{it} has the standard assumptions, we report White-robust estimates and estimates clustered on the song-level.

3.2 Data and specification

3.2.1 Sales data

We obtain data from *GfK Entertainment*, which collects sales figures from 50 (online and offline) retail outlet chains and 27 digital retailers to virtually cover the entire German and Austrian markets.[20] We observe the 1,000 highest grossing songs (across all distribution channels) for nine weeks each in 2009 and 2013. In 2009, we observe units sold physically and digitally from week 10 to week 18, in 2013 we observe weeks 36 to 44.

Songs enter and exit the top 1000 list at different points in time. We balance the panel by setting sales values to zero in weeks where we do not observe a given song in the data.[21] We control for the lifestage of a song by constructing the variable Age_{it}, which gives the number of weeks since the song appeared in the top 1000 list for the first time in our sample. Table A.4 shows that the average song enters our sample after two weeks. To ease interpretation in a log-log model, we take the natural logarithm, but add 1 to $Age_{it}$to avoid losing observations.

Table A.2 shows the average sales volume of the best and worst selling (top 1 and top 1000) song, as well as average sales volumes of top 10 and top 100 songs. Shifts in consumption towards digital purchases become evident. While it took 14,400 weekly physical units to be on top of the chart list in 2009, it is only 210 weekly physical units in 2013. It is important to keep in mind that the chart ranking is based on a combination of all distribution channels. An average of 370 physical sales of a top 10 song in 2013 therefore

[20] Note that *YouTube* plays are not covered in those data.

[21] Using other extrapolations, e.g. lowest observed unit sales, does not qualitatively change the results.

suggests that (at least in our sample) the best selling song is disproportionately sold via digital channels.

3.2.2 Newcomers

To look deeper into the role of the sampling mechanism, we distinguish between established artists and newcomers. We therefore collect the release history of each artist in our dataset from various Internet sources including *iTunes*, *Wikipedia*, *Discogs* and *Musicbrainz*. We then classify an artist as a newcomer if her oldest song or album was released no earlier than two months prior to the *GEMA* shock or the entry of *VEVO*, respectively. We choose two months prior because the median lifetime of songs in the top 1000 list is 8 weeks in our data, but the robustness of our results does not hinge on this. We observe 31 songs (1.09% of the total number of songs) of newcomer artists in 2009, while the number is 43 in 2013 (2.25% of the total number of songs). Examples of newcomers in the 2009 data are *David May*, *Oceana*, *Steve Appleton*, and *Klingande*, *Milky Chance*, *SSIO* in the 2013 data.

3.2.3 Online video data

GEMA shock, 2009 To build a song-level measure of video availability on *YouTube*, we would ideally be able to observe which songs had corresponding videos on German *YouTube* just before the ban on April, 1st 2009. In the absence of historical data we construct a proxy by querying artist name and song title on the US version of *YouTube* and recording the first 20 search results.[22] We use data from US *YouTube*, because there is evidence that German *YouTube* is very different from US *YouTube* after the *GEMA* shock (George and Peukert, 2014), but we can quite realistically assume that it would be very similar to US *YouTube* had the *GEMA* shock not happened. A simple plausibility check for the latter is to compare US search results to that from Austria, Germany's neighbor which shares the same language and similar culture, but was not affected by the *GEMA* shock. George and Peukert (2014) conduct such an exercise with a random selection of almost 1,000 songs released between 2006 and 2011, collecting search results on German, Austrian and US

[22]This query was performed on April 15, 2015.

YouTube for each song. They show that the Austrian version of *YouTube* looks very much like the US version of *YouTube*, while top search results on German *YouTube* are clearly different, including less relevant videos (see definition below), less popular videos, and a lower number of official music videos (see table A.3).

In many cases, not all videos that *YouTube* returns in response to a query for a song are directly related to that song. Sometimes search results include videos to other songs of the same artist, songs from similar artists, etc. We follow George and Peukert (2014) and treat videos as relevant if the title includes the artist name and at least three words of the song title. Using the upload date of each thus defined video, we construct our measure of availability. We set the dummy variable $Video_i$ to 1 if at least one video corresponding to song i was uploaded before April 1st, 2009. Identification thus comes from differences between songs that had corresponding videos on *YouTube* and those that did not.

In essence therefore, we have a measure of video availability just before the ban, based on US *YouTube*, not German *YouTube*, which, however, is likely to be essentially the same before April 2009. Table A.4 shows that there is at least one corresponding *YouTube* video that predates the *GEMA* shock for 54% of the songs in our sample.

We also report results using a cross-country comparison between Germany and Austria. Under the assumption that all music videos were blocked in Germany, but remained online in Austria, we do not need song-specific information on *YouTube* videos. This should provide a lower bound to our estimates.

VEVO entry, 2013 Similarly, we would like to observe which songs had corresponding videos on the German *VEVO* website when it was launched on October 1st, 2013. *VEVO* doesn't provide such a list, but we can make use of the fact that *VEVO* is part of *YouTube* in many other countries, including the US. There is no reason to expect that `vevo.com` in Germany would have systematically different content than *VEVO* on US *YouTube*. Again, we query artist name and song title on the US version of *YouTube* and record the first 20 search results. We then take advantage of the fact that *VEVO* uses artist-specific usernames to upload videos to *YouTube*.[23] For example, the corresponding username for

[23] All 247 artists listed under *VEVO*'s main *YouTube* account have "vevo" in their *YouTube* username, see `https://www.youtube.com/user/VEVO/channels?view=56`. We also manually checked all artists in our sample to make sure we don't miss a *VEVO* account that does not follow this convention.

official videos by the artist *2 Chainz* is `2ChainzVEVO` (see table A.5). Accordingly, we set the dummy $Video_i$ to 1 if the uploading username of at least one video corresponding to song i includes "VEVO". Table A.4 shows that 37% of the songs in our sample have at least one corresponding video uploaded to *YouTube* by a *VEVO* account.

4 Results

We now describe and discuss estimation results from taking equation (1) to the data. We first look at the 2009 data to estimate the effect of removing access to music videos and then proceed to study the effect of making music videos available using the 2013 data. Wherever data availability permits, we run robustness checks using cross-country variation. We then look into the causal mechanism more closely by distinguishing between new and established artists, the idea being that this gives useful variation in the amount of sampling consumers have to carry out before making an informed purchase decision.

4.1 Estimating the effect of removing access to online video

4.1.1 Song-level identification

Table 1 shows estimates of a model as specified in equation (1) with week fixed effects ν_t and song fixed effects μ_i. The dependent variable in the first two columns is the logarithm of the number of weekly physical sales, in columns (4) and (5) it is the logarithm of the number of weekly digital sales. To deal with serial correlation that could result in incorrect inference, we take two approaches. We report standard errors clustered at the song-level in columns (2) and (5), while columns (3) and (6) contain estimates from a model that neglects most of the time dimension by using averages at the month-level (Bertrand et al., 2004).

Across all specifications, we do not find strong evidence for an effect of removing music videos on physical record sales. Point estimates of $After \times Video$ are negative, but relatively close to zero, and only significantly different from zero when we use White-robust standard error estimates (the least conservative method). Results for digital record sales are similarly stable across specifications. However, the coefficients are significantly nega-

Table 1: Songs sales before and after *GEMA* blockage in 2009

	Physical sales			Digital sales		
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Age)	-0.133***	-0.133***	-0.117***	-1.229***	-1.229***	-1.399***
	(0.013)	(0.015)	(0.029)	(0.036)	(0.035)	(0.086)
After			0.071**			1.333***
			(0.032)			(0.091)
After × Video	-0.031**	-0.031	-0.026	-0.230***	-0.230***	-0.406***
	(0.015)	(0.029)	(0.033)	(0.032)	(0.052)	(0.067)
Constant	0.506***	0.506***	0.648***	1.758***	1.758***	3.200***
	(0.013)	(0.013)	(0.021)	(0.026)	(0.026)	(0.054)
Fixed Effects	Song	Song	Song	Song	Song	Song
	Week	Week		Week	Week	
Standard Errors	White	Song	Song	White	Song	Song
Observations	25686	25686	8174	25686	25686	8174
$\overline{R}^2$	0.819	0.819	0.750	0.733	0.733	0.442

Dependent variables: (Log+1) weekly sales in units in columns (1), (2), (4), (5).
(Log+1) monthly sales in units in (3) and (6).
After indicates weeks after week 14 of 2009.
Video indicates (at least one) song-specific video on U.S. *YouTube*, uploaded by prior to April 1st, 2009.
Video (and *After* except in columns 3 and 6) not separately identified because of fixed effects.
Standard errors in parentheses, either White-robust or clustered on the song-level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

tive and sizeable in magnitude throughout. According to our results, removing access to videos reduces digital sales by 21% per week or 33% per month, respectively.[24]

4.1.2 Cross-country identification

A source of identification that does not need song-level video information comes from cross-country variation. Austria is a prime candidate to serve as a control group, because it is culturally similar and shares a common language with Germany. We proceed with two identification strategies, with results reported in table A.6.

The first strategy is to treat all songs in Germany as being affected by the *GEMA* shock, i.e. not using song-specific data on the availability on music videos on *YouTube*. Comparing the sales performance of songs across both countries before and after April 1, 2009 should then at least give a lower bound of the effect. Data on digital sales in Austria are not

[24] Coefficient values are transformed to percentage values as follows: (exp(Coefficient)-1) * 100.

available for the relevant time window, so we can only estimate the effect on physical sales. Corresponding results reported in columns (1)–(3) confirm the findings described above. We do not find much evidence for an effect, and the point estimates are smaller in magnitude compared to the within-Germany results in columns (1) and (2). When looking at monthly averages as the dependent variable, the point estimate becomes larger in absolute terms (which translates into an effect of 8%) and significantly different from zero.

A related identification strategy is to look at differences in sales across countries and across songs that are treated with a least one video on *YouTube* prior to April 1st 2009. We implement this by estimating a difference-in-difference-in-differences model (a model similar to equation 2 below). A plausibility check is to show that there is no significant correlation between $Video$ and sales in the unaffected Austrian market. Estimates reported in columns (4)–(6) of table A.6 largely support this. However, the estimates of $After \times Video \times Germany$ are significant at the 10% level and negative. This suggests some evidence for an effect within Germany, albeit its magnitude is rather small with about 5%. Our estimates are likely to be more precise in the cross-country comparison because sample size is doubled, which adds useful variation. Looking at the monthly averages regression reported in column (6), we arrive at a similar conclusion. The estimated effect size is larger, however taking into account that it represents an increase in monthly compared to weekly sales, it is similar to the results we get when looking at weekly data.

Overall, we find a strong negative effect of the *GEMA* quasi-experiment on digital sales of recorded music. Evidence on the effect on physical sales is less conclusive. We do not find much evidence in our preferred, most conservative specification, but a consistent small negative effect in the less clean cross-country approach.

4.2 Estimating the effect of making online video available

4.2.1 Song-level identification

Table 2 shows results corresponding to equation (1) using data from 2013, estimating the effect making music videos available via *VEVO* as a dedicated platform. We report specifications with week and song fixed effects in columns (1), (2) and (4), (5). The

Table 2: Songs sales before and after *VEVO* entry in 2013

	Physical sales			Digital sales		
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Age)	-0.074***	-0.074***	-0.086***	-2.474***	-2.474***	-2.281***
	(0.013)	(0.016)	(0.025)	(0.058)	(0.044)	(0.106)
After			0.055**			1.704***
			(0.025)			(0.109)
After × Video	0.001	0.001	0.001	0.154***	0.154*	0.201**
	(0.014)	(0.028)	(0.029)	(0.058)	(0.091)	(0.101)
Constant	0.340***	0.340***	0.367***	2.832***	2.832***	4.757***
	(0.014)	(0.014)	(0.017)	(0.044)	(0.045)	(0.066)
Fixed Effects	Song Week	Song Week	Song	Song Week	Song Week	Song
Standard Errors	White	Song	Song	White	Song	Song
Observations	17163	17163	6190	17163	17163	6190
$\overline{R}^2$	0.825	0.825	0.779	0.633	0.633	0.260

Dependent variables: (Log+1) weekly sales in units in columns (1), (2), (4), (5).
(Log+1) monthly sales in units in (3) and (6).
After indicates weeks after week 40 of 2013.
Video indicates (at least one) song-specific video on U.S. *YouTube*, uploaded by *VEVO*.
Video (and *After* except in columns 3 and 6) not separately identified because of fixed effects.
Standard errors in parentheses, either White-robust or clustered on the song-level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

dependent variables are physical sales in columns (1)–(3) and digital sales in columns (4)–(6), on the weekly level in the first two columns and averaged at the monthly level in the third column, respectively. We report White-robust and song-level clustered standard errors.

We do not find evidence that the entry of *VEVO* had an effect on physical record sales; point estimates of $After \times Video$ are very close to zero and standard errors are relatively large. Much in contrast, the results show an increase in digital record sales of 17% per week, or 22% per month. Note however that the coefficient estimate in our preferred specification (column 5) is significant only at the 10% level.

4.2.2 Cross-country identification

We again use cross-country variation as an alternative identification strategy. The distinctive difference between the *GEMA* ban and the entry of *VEVO* is that the former affected all songs and their respective videos on *YouTube*, while *VEVO*, by design, only makes videos by specific record labels available. Hence, it does not make much sense in this case to define the treatment group as all songs in Germany, so we use data from Austria to increase the size of the control group. Table A.7 reports corresponding difference-in-difference-in-differences results.

Estimates of $After \times Video$ in columns (1)–(3) are small and not significantly different from zero, which provides some reassuring evidence that the entry in the German market did not affect sales dynamics in Austria. The same is true for digital sales (columns 4 and 5). Turning to the coefficients of main interest, we do not find evidence for an effect on physical sales in Germany, as indicated by small and non-significant estimates of $After \times Video \times Germany$. Conversely, the effect on digital sales is sizeably positive (24%) and significant (columns 4 and 5). Looking at monthly averages, although point estimates suggest that digital sales of "VEVO songs" are higher after *VEVO* entered the German market, the difference between Austria and Germany (17%), it is not significantly different from zero.

Summarizing our findings both on removing and adding online music videos, we have not found a robust causal relationship between online music videos and physical record sales. However, we find evidence for a sizeable effect on digital sales, which is remarkably similar in magnitude when comparing estimates from our two quasi-experiments. In the following we investigate the idea that the estimated effect across all songs is the result of countervailing effects across different groups of observations. We proceed by differentiating between artists that appear on the market for the first time shortly before the respective *GEMA* and *VEVO* quasi-experiments happen (*Newcomers*) and those that have a longer history of top 1000 songs. We expect this to provide meaningful variation in the *informativeness* of online videos as samples for recorded songs.

4.3 Exploring heterogeneity

To look into heterogeneity across different groups of observations, we estimate a difference-in-difference-in-differences model specified as

$$\begin{aligned} log(Sales^k_{ijt}+1) = {} & \alpha + \beta\ log(Age_{it}+1) + \gamma Newcomer_j + \delta\,(After_t \times Video_i) \\ & + \theta\,(After_t \times Newcomer_j) + \lambda\,(After_t \times Video_i \times Newcomer_j) \\ & + \nu_t + \mu_i + \varepsilon_{ijt}, \end{aligned} \tag{2}$$

where $Newcomer_j$ indicates whether an artist j has released her first song or album no longer than two months prior to the respective quasi-experiments. The most conservative and therefore preferred specification has week fixed effects ν_t, song fixed effects μ_i and standard errors clustered at the song-level. The parameters of most interest are δ and λ. We first report estimates exploiting the *GEMA* quasi-experiment in 2009 and then turn to results from the *VEVO* quasi-experiment.

4.3.1 Newcomers and the effect of removing access to online video

Table 3 reports results of the preferred specification and additional estimates with White-robust standard errors and monthly averages, respectively. Regarding both physical and digital sales, estimates of the coefficient on $After \times Video$ are very similar to those reported in the baseline specification in table 1, suggesting that the average effect is mainly driven by established artists. Columns (1)–(3) show no significant differential effect for newcomer artists regarding physical sales. Note however that the point estimates are quite large in absolute terms (16% per week and 34% per month).

Turning to digital sales, we do find a pronounced difference in the *GEMA* effect across different types of artists. $Newcomer \times Video \times After$ is significantly negative, suggesting that especially sales of new artists suffer from removing access to online music videos. The effect magnitude is large (88% per week and 84% per month), which on average translates into a decrease from 550 to 66 weekly sales.

Table 3: Songs sales before and after *GEMA* blockage in 2009, New artists

	Physical sales			Digital sales		
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Age)	-0.129***	-0.129***	-0.115***	-1.221***	-1.221***	-1.395***
	(0.013)	(0.015)	(0.029)	(0.036)	(0.035)	(0.086)
After			0.057*			1.303***
			(0.031)			(0.091)
After × Video	-0.023	-0.023	-0.017	-0.202***	-0.202***	-0.379***
	(0.014)	(0.028)	(0.032)	(0.032)	(0.051)	(0.066)
After × Newcomer	1.017***	1.017*	0.805	1.767***	1.767***	1.691**
	(0.211)	(0.527)	(0.509)	(0.304)	(0.683)	(0.694)
After × Video × Newcomer	-0.176	-0.176	-0.419	-2.128***	-2.128**	-1.832*
	(0.328)	(0.677)	(0.666)	(0.490)	(1.055)	(1.018)
Constant	0.506***	0.506***	0.647***	1.758***	1.758***	3.197***
	(0.013)	(0.013)	(0.021)	(0.026)	(0.026)	(0.055)
Fixed Effects	Song Week	Song Week	Song	Song Week	Song Week	Song
Standard Errors	White	Song	Song	White	Song	Song
Observations	25686	25686	8174	25686	25686	8174
$\overline{R}^2$	0.821	0.821	0.751	0.734	0.734	0.442

Dependent variable: (Log+1) weekly sales in units.
Week fixed effects. *After* indicates weeks after week 14 of 2009.
Video indicates (at least one) song-specific video on U.S. *YouTube*, uploaded by prior to April 1st, 2009.
Newcomer indicates artists that have released their first song no earlier than February 1, 2009.
Standard errors clustered on the song-level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3.2 Newcomers and the effect of making online video available

Estimates exploiting exogenous variation from the entry of *VEVO* are reported in table 4. For both physical and digital sales, coefficient estimates of $After \times Video$ are similar to those reported in the baseline specification in table 2. Again, we do not find much evidence for a differential effect for newcomer artists regarding physical sales. Although the coefficient of $After \times Video \times Newcomer$ is negative and sizeable (19%), it is statistically different from zero only in the specification with White-robust standard errors.[25]

In stark contrast, $After \times Video \times Newcomer$ is positive and significant when looking at digital sales. Effect magnitudes are larger compared to those estimated from 2009 data.

[25] Note that this result is qualitatively consistent with the findings reported in Hiller (2015).

Table 4: Songs sales before and after *VEVO* entry in 2013, New artists

	Physical sales			Digital sales		
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Age)	-0.077***	-0.077***	-0.087***	-2.478***	-2.478***	-2.278***
	(0.013)	(0.016)	(0.025)	(0.058)	(0.044)	(0.106)
After			0.051**			1.712***
			(0.025)			(0.109)
After × Video	0.008	0.008	0.007	0.153***	0.153*	0.185*
	(0.014)	(0.027)	(0.028)	(0.058)	(0.092)	(0.101)
After × Newcomer	0.205***	0.205	0.158	0.174	0.174	-0.295
	(0.069)	(0.172)	(0.205)	(0.209)	(0.333)	(0.368)
After × Video × Newcomer	-0.178**	-0.178	-0.127	2.317***	2.317***	2.430***
	(0.071)	(0.174)	(0.206)	(0.819)	(0.424)	(0.463)
Constant	0.340***	0.340***	0.368***	2.832***	2.832***	4.756***
	(0.014)	(0.014)	(0.017)	(0.044)	(0.044)	(0.066)
Fixed Effects	Song Week	Song Week	Song	Song Week	Song Week	Song
Standard Errors	White	Song	Song	White	Song	Song
Observations	17163	17163	6190	17163	17163	6190
$\overline{R}^2$	0.825	0.825	0.779	0.633	0.633	0.260

Dependent variable: (Log+1) weekly sales in units.
Week fixed effects. *After* indicates weeks after week 40 of 2013.
Video indicates (at least one) song-specific video on U.S. *YouTube*, uploaded by *VEVO*.
Newcomer indicates artists that have released their first song no earlier than August 1, 2013.
Standard errors clustered on the song-level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We find an effect of about 915% per week (1035% per month), which translates into an average increase from 736 to 6237 weekly digital sales for newcomer artists.

5 Discussion and conclusions

In this paper, we exploit two quasi-experiments in the German market for online music videos to identify the effect of free sampling on physical and digital sales of recorded music. The first quasi-experiment lets us identify the effect of removing access to online music videos (in April 2009), while in the second we identify the effect of making music videos available (in October 2013). Our analysis is based on a rich dataset that combines sales data that cover a large fraction of all music sales (top 1000 songs) with song-level information on music video availability.

We find strong evidence that online videos are complementary to digital record sales. It may seem surprising at first glance that giving consumers access to a product for free is not hurting sales of a very similar version of that product, but can actually increase sales. However, sampling was ever since considered an important mechanism to increase sales in the recorded music industry. In essentially every record store (including digital record stores such as *iTunes*, *Google Play* or *Amazon*) consumers can listen to (parts of) songs before buying. Radio stations have been promoting songs for ages, and *MTV* and other music television channels are based on the idea that music videos create attention. The difference to streaming websites such as *YouTube* or *Soundcloud* is that on such sites firms cannot control how intensely consumers sample, i.e. consumers may use such (on-demand) services as a substitute to actual purchases.[26] Our contribution therefore is to show that sampling can still be an effective trigger of demand even if the firm cannot keep some consumers from sticking to the sample instead of purchasing the product.

Sales of physical records do not respond as strongly to changes in the supply of online videos, which could imply lower substitutability between online video and physical sales (compared to digital). We find some limited evidence for an effect on physical sales in the 2009 quasi-experiment, but not in the 2013 quasi-experiment. This could be because we are looking at two different sets of quasi-experiments. Removing access to online videos may hurt physical sales more than adding online video helps boost sales. However, it seems more likely that this reflects overall industry dynamics. Our sales data shows that consumers bought more physical records in 2009 than they did in 2013. Hence, a technical explanation could be that we do not observe enough variation in the 2013 data to be able to statistically pin down an effect.[27] It could also be the case that the type of consumer that buys physical records has changed over time. Consumer that buy physical records in 2013 may not be using online video to discover music as much as consumers that bought physical records in 2009.

Looking at differences between new and established artists, we find that digital sales

[26] See also Aguiar and Waldfogel (2015), who argue that the streaming service *Spotify* is revenue-neutral for the record industry.

[27] Physical sales in 2013 are very low. The 75th percentile of physical sales is 0, the 90th percentile is 1.03.

dynamics of newcomer artists are affected much stronger by music videos. This provides further insights into the mechanism that we propose: When characteristics that define vertical and horizontal product quality are comparably unknown to consumers, online music videos act as a sampling device that helps make purchase decisions which trigger higher sales.

There are important implications for managers when online videos constitute an effective tool to trigger record sales, especially in the digital market which is characterized by unlimited shelf-space and huge choice sets for consumers. At the same time the cost of producing and distributing video has fallen sharply in the digital age, making the investment in music videos a lucrative strategy to make artists known and boost sales. Indeed, casual observation suggests that firms and artists place increasing emphasis on music videos in recent years. For example, pop singer Beyonce released a music video for each track on her 2013 album.[28]

In digital markets, consumers infer product quality from hit lists when deciding which products to purchase (Tucker and Zhang, 2011; Hendricks et al., 2012). Here, a cycle that leads from music videos to higher sales, which again lead to higher sales can be self-enforcing. In that sense, our finding that video clips are especially effective in triggering sales of new artists suggests that investing into complementary videos is promising when trying to launch new products or artists: Record labels typically spend 10% to 15% of their total investment volume in a new artist on video production.[29] Our results suggest that a music video increases digital sales on average by roughly 15%, but for new artists this effect is almost two orders of magnitude stronger. However, it is important to keep in mind that our empirical setting only looks at the short-run effects of blocking or making online video available. Whether the sales effect we identified is sustainable in the long-run calls for further research.

Our study can inform managers in many other industries producing or selling experience goods such as movies, books and software. In a broader sense, potential complementary effects of free and paid consumption give rise to interesting implications for how firms can

[28]See http://www.billboard.com/articles/columns/the-juice/5827398/beyonce-unexpectedly-releases-new-self-titled-visual-album-on

[29]See http://www.ifpi.org/how-record-labels-invest.php.

strategically use intellectual property protection.

In reference to a song by *The Buggles* – which happens to be the first music video shown on MTV in 1981 – it is fair to conclude that our study doesn't provide much evidence that "video killed the radio star". If anything, we find the opposite. In that sense, causal evidence of complementary effects between free and paid consumption of digital media on the Internet also provides important policy insights. While it is straightforward to conclude that free consumption increases consumer surplus, conclusions about overall welfare are more ambiguous. When positive externalities of unpaid consumption can offset forgone royalties income, sampling may indeed to increase producer rents at the same time. This would in turn speak to the issue of dynamic incentives that an intellectual property protection system is designed to solve, but may come at a lower social loss. If confirmed in other studies and empirical settings, our findings may therefore inform the ongoing public debate about copyright policy.

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Appendix

Table A.1: Google search ranks

	Google Search Rank	
	Youtube (1)	VEVO higher (2)
Germany	1.188***	0.210***
	(0.349)	(0.079)
VEVO-Video × Germany	2.017***	
	(0.678)	
Constant	2.551***	0.147***
	(0.159)	(0.037)
Observations	564	143
$\overline{R}^2$	0.613	0.548

Based on a random sample of 300 songs from the 2013 sales data.
Top 20 *Google* Search results in Germany and the US, querying "Artist Song" as of July 8th, 2015.
Germany indicates observations from *Google* Germany.
Dependent variables:
(1) Lowest rank of search result including link to *YouTube*.com.
(2) Indicates if lowest rank of search result including link to *VEVO*.com is lower than lowest rank of search result including link to *YouTube*.com.
Song-level fixed-effects and clustered standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Sales, descriptive statistics

	Top 1	Top 10	Top 100	Top 1000
2009				
Physical				
Mean	14435.66	1861.20	99.26	1.93
S.D.	(2844.65)	(515.54)	(60.68)	(3.62)
Digital				
Mean	22531.00	4549.11	630.11	66.56
S.D.	(6216.89)	(1913.05)	(316.78)	(25.28)
2013				
Physical				
Mean	210.22	370.89	109.74	7.25
S.D.	(264.58)	(470.21)	(203.47)	(14.5)
Digital				
Mean	16121.56	10276.89	2499.86	0.00
S.D.	(9887.45)	(7175.80)	(3072.05)	(0.00)

Note: Sales figures for the German market for week 10 to week 18 in 2009, and week 36 to week 44 in 2013.

Table A.3: YouTube in the United States, Germany and Austria

	Share of directly relevant videos	Share of total views	Official video share
United States			
Mean	0.7755	0.8250	0.0868
Standard Error	0.0014	0.0014	0.0025
Austria			
Mean	0.7726	0.8213	0.0893
Standard Error	0.0014	0.0014	0.0026
Germany			
Mean	0.7485	0.7483	0.0502
Standard Error	0.0016	0.0021	0.0020

Source: George and Peukert (2014).
Top 20 *YouTube* search results for 950 randomly selected songs released between 2006 and 2011. The search was carried out on August 21st, 2014. Relevancy is defined as a *YouTube* video title containing the artist name and at least three words of the song title. Total views are calculated as the cumulative number of views of all 500 videos shown on the first 20 results pages. Official videos are identified by the word "official" in the title or uploader name.

Table A.4: Descriptive statistics

	2009		2013	
	Mean	S.D.	Mean	S.D.
Log(Physical)	0.477	1.360	0.310	1.082
Log(Download)	1.745	2.502	2.820	2.987
Log(Age)	0.987	0.815	0.966	0.821
After	0.444	0.497	0.444	0.497
Video	0.542	0.498	0.370	0.483
Newcomer	0.011	0.104	0.023	0.148
# Songs	2854		1907	
# Artists	1679		1088	
Observations	25686		17163	

Note: Sales figures for the German market for week 10 to week 18 in 2009, and week 36 to week 44 in 2013.

Table A.5: YouTube search results

Result	Youtube Title	Uploader
1	2 Chainz - We Own It ft. Wiz Khalifa (Fast & Furious)	mchssz
2	We Own It (Fast & Furious) 2 Chainz & Wiz Khalifa [With Lyrics]	brkunal4
3	2 Chainz feat. Wiz Khalifa - We Own It (Lyrics) (Fast & Furious)	VoxixMixtape1
4	2 Chainz feat. Wiz Khalifa - We Own It (Lyrics)	TheVictorMHR
5	01. We Own It (Fast & Furious) - 2 Chainz (Feat. Wiz Khalifa) Fast & Furious 6 Soundtrack [OST]	McFlyMyWorlds
6	Fast and Furious 6 OST We own it - 2 Chainz ft Wiz Khalifa (Music Video)	DjTreacks
7	We Own It (Fast & Furious) (Lyric Video)	2ChainzVEVO
8	We Own It (Fast & Furious 6) 2 Chainz ft. Wiz Khalifa	DeusJ35
9	Wiz Khalifa & 2 Chainz - We Own It (Fast & Furious 6) (Subtitulado espanol)	themedizine
10	2 Chainz & Wiz Khalifa - We Own It (Fast & Furious) Video Clip	OfficialGangstaVideo
11	We Own It (fast and furious) 2 chainz feat - Wiz Khalifa Instrumental (original version) HD///HQ	andreseuphoria
12	FAST & FURIOUS 6 - We own it, 2 Chainz & Wiz Khalifa	UniversalSpain
13	Wiz Khalifa - We Own It ft. 2 Chainz (Fast & Furious 6) Subtitulado Espanol Ingles	diamante183
14	Fast & Furious 6: We Own It Video Montage (2 Chainz, Wiz Khalifa)	universalpicturesuk
15	2 chainz, Wiz Khalifa - We own it (Fast & Furious) Lyrics HQ	FuckSwagZ
16	Wiz Khalifa Feat. 2 Chainz - We Own It (Fast & Furious) (Bass Boosted) (HQ)	TheUSFDave
17	2 Chainz Ft. Wiz Khalifa - We Own It (Rmx) ft. T Mills, Sammy Adams, & Niykee Heaton - Mike Posner	bGq00L6A1h81l6kK8JBR4w
18	Spitz - We Own it (2 Chainz ft. Wiz Khalifa Fast & Furious 6 Official UK Remix)	SpitzOnline
19	Wiz Khalifa ft 2 Chainz - We Own It (Fast and Furious 6 Soundtrack)	TcISounds
20	Fast & Furious 6 - We Own It (Fast & Furious)	fastandfuriousmovie

Note: Top 20 search results for “2 Chainz feat. Wiz Khalifa - We Own It (Fast & Furious)” on US *YouTube*.

Table A.6: Songs sales before and after *GEMA* blockage in 2009, Cross-country

	Physical sales					
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Age)	-0.161***	-0.161***	-0.133***	-0.161***	-0.161***	-0.132***
	(0.010)	(0.008)	(0.020)	(0.010)	(0.008)	(0.020)
After			0.064***			0.018
			(0.023)			(0.026)
Germany	0.298***	0.298***	0.332***	0.072***	0.072**	0.062
	(0.009)	(0.025)	(0.031)	(0.013)	(0.035)	(0.043)
After × Germany	-0.005	-0.005	-0.085***	0.022	0.022	-0.018
	(0.014)	(0.014)	(0.018)	(0.020)	(0.021)	(0.027)
Video × After				0.021	0.021	0.095***
				(0.018)	(0.014)	(0.020)
Video × Germany				0.418***	0.418***	0.492***
				(0.019)	(0.050)	(0.060)
Video × After × Germany				-0.051*	-0.051*	-0.119***
				(0.028)	(0.029)	(0.035)
Constant	0.203***	0.203***	0.384***	0.203***	0.203***	0.372***
	(0.012)	(0.014)	(0.021)	(0.012)	(0.014)	(0.021)
Fixed Effects	Song	Song	Song	Song	Song	Song
	Week	Week		Week	Week	
Standard Errors	White	Song	Song	White	Song	Song
Observations	51372	51372	15453	51372	51372	15453
$\overline{R}^2$	0.441	0.441	0.399	0.450	0.450	0.409

Dependent variables: (Log+1) weekly sales in units in columns (1), (2), (4), (5).
(Log+1) monthly sales in units in (3) and (6).
After indicates weeks after week 14 of 2009.
Video indicates (at least one) song-specific video on U.S. *YouTube*, uploaded by prior to April 1st, 2009.
Video (and *After* except in columns 3 and 6) not separately identified because of fixed effects.
Digital sales data are not available for Austria.
Standard errors in parentheses, either White-robust or clustered on the song-level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Songs sales before and after *VEVO* entry in 2013, Cross-country

	Physical sales			Digital sales		
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Age)	0.032	0.032	-0.002	-2.545***	-2.545***	-0.382***
	(0.025)	(0.032)	(0.014)	(0.093)	(0.133)	(0.057)
After			0.026*			-0.022
			(0.014)			(0.066)
After × Video	0.007	0.007	-0.003	0.077	0.077	0.180**
	(0.023)	(0.017)	(0.017)	(0.058)	(0.068)	(0.073)
Germany	0.463***	0.463***	0.401***	2.096***	2.096***	1.857***
	(0.018)	(0.046)	(0.044)	(0.047)	(0.080)	(0.085)
Video × Germany	-0.031	-0.031	-0.007	-0.028	-0.028	0.076
	(0.027)	(0.071)	(0.070)	(0.067)	(0.115)	(0.125)
After × Germany	-0.131***	-0.131***	-0.097***	-0.462***	-0.462***	-0.359***
	(0.023)	(0.028)	(0.025)	(0.062)	(0.064)	(0.064)
After × Video × Germany	0.038	0.038	0.024	0.213**	0.213**	0.153
	(0.035)	(0.036)	(0.033)	(0.092)	(0.090)	(0.094)
Constant	0.121***	0.121***	0.079***	2.337***	2.337***	2.469***
	(0.020)	(0.024)	(0.022)	(0.052)	(0.072)	(0.069)
Fixed Effects	Song	Song	Song	Song	Song	Song
	Week	Week		Week	Week	
Standard Errors	White	Song	Song	White	Song	Song
Observations	20956	20956	7450	20956	20956	7450
$\overline{R}^2$	0.607	0.607	0.580	0.605	0.605	0.657

Dependent variables: (Log+1) weekly sales in units in columns (1), (2), (4), (5).
(Log+1) monthly sales in units in (3) and (6).
After indicates weeks after week 40 of 2013.
Video indicates (at least one) song-specific video on U.S. *YouTube*, uploaded by *VEVO*.
Germany indicates an observation from the German market, with Austria being the omitted category.
Video (and *After* except in columns 3 and 6) not separately identified because of fixed effects.
Standard errors in parentheses, either White-robust or clustered on the song-level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$