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The Competitive Effects of "Consideration Payments": Lessons from Radio Payola

Adam D. Rennhoff*
Middle Tennessee State University, Murfreesboro, TN

Abstract

It is not uncommon for upstream manufacturers to make payments to downstream firms in order to obtain preferential treatment. These payments may generally be called "consideration payments." Examples of this include the slotting allowance payments often discussed in the grocery, pharmaceutical, and consumer electronics industries.

Payola in the radio industry shares many of the same characteristics as slotting allowances. The prohibition of radio payola in 1960 gives us an opportunity to empirically examine the effect that these payments had on the record labels using them and on overall product variety. We construct a unique variety measure based on the musical styles of Billboard chart artists and supplement this with information on radio airplay from *Billboard* charts to evaluate the effects of payola. We find that the prohibition of payola reduced musical variety and overall record sales, but may have helped increase access for smaller record labels. These findings support the theory that payola payments, which may impose a non-trivial financial burden on the record label, serve to reduce the radio station's risk.

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Key words: Radio; Payola; Regulation; Slotting Allowances

JEL category: L42

*Adam D. Rennhoff, Assistant Professor Department of Economics and Finance, Middle Tennessee State University, Murfreesboro, TN 37132, phone: 615-898-2931, fax: 615-898-5596, email: rennhoff@mtsu.edu

1. Introduction & Background

In many consumer-product industries, such as groceries, pharmaceuticals, and electronics, the use of "consideration payments" is commonplace. The term "consideration payment" refers to payments that upstream firms (manufacturers) make to downstream firms (retailers) in order to obtain favorable treatment for the upstream firm's product(s). This favorable treatment encompasses things like improved shelf or display space or prominent in-store promotion.[1] Academics and government agencies, such as the Federal Trade Commission (FTC), have been concerned with the welfare implications of such payments. Specifically, they are concerned with the impact these payments have on downstream retail prices and the variety of product offerings. In general, the exact implication of these payments is difficult to decipher. Given the widespread use of these payments and the secrecy of the practice, it is difficult to conceive of an empirical test that would conclusively determine the welfare effect.[2] This has contributed to a lack of consensus on the welfare implications (see, for example, Bloom et al. 2000).

Fortunately, there is a historical event that may help us shed light on the subject: the radio payola scandal of the late 1950s. As noted in a recent Department of Justice (DoJ) / FTC hearing on firm conduct, radio payola, which is the practice of paying disc jockeys for radio play, has much in common with the current system of consideration payments (Sullivan 2006). In the case of radio payola, record labels (upstream firms) make payments to radio stations or the disc jockeys responsible for selecting the music played at these radio stations (downstream firms) in return for favorable treatment, namely more "spins" or airplay for their records. Given that radio stations have a fixed or limited amount of time in which to play music, payola is designed to

[1] The term "consideration" is courtesy of the Financial Accounting Standards Board (FASB). The FASB uses the term in their rules on accounting for payments like slotting allowances, pay-to-stay fees, and merchandising allowances.

[2] Thanks to access to confidential data, Bronsteen et al. (2005) and Wright (2007) are able to look at the effect slotting allowances have on price. Both studies conclude that slotting allowances do not result in higher retail prices.

influence the disc jockeys' music selections. From a welfare perspective, one might be concerned with the way in which payola alters the variety of music played.

In this research, we examine the impact that payola and its eventual prohibition had on the variety of music played by radio stations using a unique index of musical variety. We construct a unique historical data set of music airplay and sales for 25 record labels from *Billboard*'s record charts. This data set is then used to examine the impact that payola had on product variety and the record labels' ability to place singles on the charts. Our findings indicate that payola increased the variety of musical styles on the record charts, but may also have restricted access to radio stations for smaller record labels. The post-payola period, therefore, is characterized by decreased musical variety but an increased presence by smaller record labels. A simple descriptive regression suggests that the prohibition of payola lead to lower record sales.[3]

References to forms of radio payola date back to the late 1930s, but it was not until radio stations shifted overwhelmingly to playing recorded music, as opposed to the airing of live "big band" programs, in the 1950s that the practice of paying disc jockeys for airplay became commonplace.[4]

Though becoming increasingly widespread over time, the use of payola did not attract significant attention or scrutiny for most of the 1950s. Indeed, it was not until late 1959 that government officials and agencies began publicly commenting and engaging in payola inquiries. There are numerous reasons cited for the eventual regulatory interest in payola. Perhaps the most prominent reason involved the renewed focus on truth, disclosure, and fraud brought about by the recent revelations that the outcomes of several popular television quiz shows were rigged (Coase 1979). Whatever the reasons may have been, in the final months of 1959, government

[3] We discuss the implications of our findings in the broader context of slotting allowances in Section 5.
[4] A detailed history of radio payola is beyond the scope of this paper. We refer the interested reader to more thorough descriptions of radio payola, including Coase (1979), Segrave (1994), and Sanjek (1996).

agencies like the Federal Communications Commission (FCC) and the FTC, along with the U.S. Congress, began actively trying to curtail payola. These interests culminated with the September 1960 Amendments to the Communications Act, which effectively outlawed payola.[5,6]

2. Data

The data used in this study come from several sources. The primary data source is the *Billboard* Hot 100 Singles Chart. The *Billboard* Hot 100 Singles Chart, which is published weekly, tracks the top songs (or "singles") in popular music. Rankings in the chart are based on both airplay and sales. Given the interest in examining how payola impacted radio station disc jockeys, it would be preferable to utilize information solely on airplay. Unfortunately, *Billboard* discontinued the "Most Played by Jockeys" rankings prior to the introduction of payola legislation or enforcement. Other potential airplay rankings, such as *Billboard*'s Hot 100 Airplay rankings were not introduced until after the desired sample period. The end result is that the only ranking that covers both pre- and post-payola regulation/enforcement and incorporates information on radio airplay is the Hot 100 chart.[7] The *Billboard* Hot 100 chart has historically been considered the definitive list of popular music.

For the three year (156 week) period from January 1, 1959 to December 31, 1961, a number of measures were collected from each of the weekly *Billboard* Hot 100 charts.[8] These measures allow us to characterize the impact that payola had on different chart "activity level"

[5] Technically speaking, the Communications Act amendments prohibited the use of payola *unless* disc jockeys explicitly (on-air) disclosed any compensation they received for playing a particular song. This caveat aside, the Communications Act amendments are typically described as a "law prohibiting payola," (Coase 1979) and we, therefore, use this terminology throughout the current paper.

[6] Although outlawed in 1960, headlines on payola re-emerged in 2004 when New York Attorney General Eliot Spitzer successfully investigated four record labels and two radio companies accused of engaging in payola.

[7] While most practitioners generally believe that there is a positive correlation between airplay and sales, Liebowitz (2004) has shown that while sales of an individual song may benefit from radio play, aggregate per-capita record sales are not positively impacted by increased radio airplay.

[8] The 1960 Amendments to the Communications Act became effective as of September 13, 1960 (Coase, 1979). This means that the data include 89 weeks prior to the formal prohibition of payola and 67 weeks post.

variables. These activity measures are meant to capture the "survivability" of songs and/or the amount of "churn" on the charts (Bhattacharjee et al. 2007). Among the measures collected were:

- Whether there is a new number one single in a given week
- The average "age" of the top five and top 20 singles on the chart during a given week. This "age" measure captures the average number of weeks each of the songs has been ranked in the Hot 100.
- The number of new songs appearing in the Hot 100 during a given week ("turnover").

Recall that the primary variable of interest in this study is variety. To allow us to address potential changes in variety, we construct a weekly variety index that represents the degree to which artists in each week's top 20 are similar musically. This variety index is a continuous variable between 0 and 1. Values of the variety index closer to zero indicate that variety is low (i.e. that the artists in a week's top 20 have similar musical styles), while values closer to one indicate higher variety. We construct this index using information on the identity of each artist in the *Billboard* top 20 along with information from the All Music Guide database, which labels musical artists as being associated with specific musical styles. The specific details of the construction of this index appear in the Appendix.

The detailed nature of the *Billboard* Hot 100 also makes it possible to construct a disaggregated data set with which the outcomes for individual record labels can be examined. To do so, the following information is collected for the top 25 record labels during the 156 week sample period:[9]

- The identity of the record label distributing each week's number one hit on the Hot 100 chart.

[9] The top 25 record labels is determined by counting the number of times each record label is represented on the Hot 100 during the 156 week sample period. In other words, these 25 record labels had the most appearances on the Hot 100 chart from January 1, 1950 to December 31, 1961. All 25 labels were in operation for the entire sample period, creating a balanced panel (Hoffman 2005).

- The number of top twenty hits each record label distributes on each week's Hot 100 chart.
- The number of top 100 hits each record label distributes on each week's Hot 100 chart.

These measures help determine the impact payola had on each record label's probability of landing top singles. As discussed in greater detail in Section 3.2, observed and unobserved record label heterogeneity can be controlled for thanks to the panel nature of the data.

While the stated focus of this research is determining the impact payola had on product variety, the 1960 Amendments to the Communications Act is not the only relevant limitation on payola during the sample period. Prior to passage of the Communications Act amendments, the FTC began citing numerous record labels and disc jockeys for the use of payola, which the FTC claimed violated consumer protection laws on deceptive practices in interstate commerce (Blair 1959). These citations began in December 1959 and continued through the end of July 1960.[10] Articles in the *New York Times* and *Broadcasting*, published between December 1959 and July 1960, contain lists of which record labels were cited by the FTC for payola and when these citations were issued. In total, our sample includes 13 record labels that were cited by the FTC. This observed "enforcement," which affected different record labels at different times, adds an important element to the panel regressions discussed in Section 3.2.

Summary statistics for the collected measures are presented in Table 1 below.

[10] July 1960 marks the end of FTC payola citations *prior* to the Communications Act amendments. Over the years, additional citations have been issued, although none during the remainder of the sample period.

Table 1.
Summary Statistics

	Total Sample	Pre-Amendment	Post-Amendment	T-stat on Difference
Number of Weeks	156	89	67	
Mean Variety Index	0.8045	0.8210	0.7820	3.1141***
	(0.05)	(0.04)	(0.04)	
New #1?	35.25%	29.21%	43.28%	-1.8286*
	(0.48)	(0.46)	(0.50)	
Mean Age (top 5)	9.069	9.254	8.823	1.7681*
	(1.52)	(1.47)	(1.55)	
Mean Age (top 20)	8.740	8.905	8.521	3.0033***
	(0.81)	(0.73)	(0.86)	
Turnover	11.910	11.090	13.000	-4.2354***
	(2.94)	(10.55)	(3.04)	
	Total Sample	Pre-Amendment	Post-Amendment	T-stat on Difference
Percentage of #1 Hits				
Major Labels	39.74%	48.31%	28.36%	2.5577**
Top 10 Labels	57.05%	64.04%	47.76%	2.0481**
Top 25 Labels	71.94%	74.16%	68.66%	0.7523
Bottom 10 Labels	5.77%	0.00%	13.43%	-3.6923***
Percentage of Top 20 Hits				
Major Labels	24.23%	26.80%	20.82%	4.4040***
Top 10 Labels	42.02%	44.21%	39.10%	3.5285***
Top 25 Labels	63.43%	61.24%	66.34%	-1.4769
Bottom 10 Labels	9.55%	6.34%	13.80%	-7.9023***
Percentage of Top 100 Hits				
Major Labels	22.47%	25.01%	19.09%	9.8525***
Top 10 Labels	37.84%	41.18%	33.40%	9.4326***
Top 25 Labels	57.47%	58.49%	56.11%	3.3853***
Bottom 10 Labels	9.51%	8.16%	11.33%	-9.2449***

Notes: Standard deviation values in parentheses. T-test based on 154 degrees of freedom. *** - significant at the 1% level. ** - significant at the 5% level. * - significant at the 10% level.

The first column in Table 1 shows the overall summary statistics for the entire 156 sample period and columns 2 and 3 present statistics for the pre- and post-amendment periods, respectively. The final column in Table 1 shows the t-statistics (and significance) on a test of whether the pre-amendment mean for each variable is different than the post-amendment mean. The majority of the sample differences are statistically significant. These summary statistics

illustrate some interesting trends. Firstly, variety is higher in the pre-amendment time period and the decrease in the post-amendment period is statistically significant. In looking at the other aggregate measures, turnover increase, there is a new number one song more frequently, and songs disappear from the chart faster in the post-amendment period. The survivability of songs appears to go down in the post-amendment time period.

The bottom half of Table 1 breaks down the composition of songs on the chart by record label size. The noticeable trend is that the larger record labels are losing share on the charts to the smaller record labels.

3. Empirical Models

We divide the empirical models into two categories: aggregate regressions (Section 3.1) and panel data regressions (Section 3.2). We discuss each type separately.

3.1. *Aggregate Regressions*

The aggregate regressions examine how aggregate measures of variety and survivability are related to payola. If Y_t is used to represent the outcome measure of interest, then the aggregate regression model can be expressed as:

$$Y_t = \beta_1 * 1[Payola\ Law\ in\ Effect] + \beta_2 * Time_t + \beta_3 * Time_t^2 + \sum_m Month\ fixed\ effects + \sum_y Year\ fixed\ effects + \varepsilon_t \quad (1)$$

The indicator variable $1[Payola\ Law\ in\ Effect]$, which accounts for the period of payola prohibition, takes the value 1 after September 13, 1960 and 0 prior. Time and time-squared terms are included to capture general trends, which cannot be captured by other observed measures. Month fixed effects are included to capture monthly differences (for example, Christmas-themed

songs were prominent on the November and December charts) and year fixed effects capture annual differences.[11]

The five outcome measures used, which were noted in Table 1, are (1) the weekly variety index, (2) weekly turnover, (3) the average age of the top 5 singles in a given week, (4) the average age of the top 20 singles in a given week, and (5) a binary variable indicating whether there was a new number one single during a given week. OLS is used to estimate equation (1) for measures 1 – 4. Given the binary nature of the fifth outcome measure, equation (1) is estimated as a binary probit in this case.

3.2. *Panel Data Regressions*

Panel regression techniques enable us to examine the impact payola had on record labels, while controlling for observed and unobserved heterogeneity. The impact of payola is examined for three observable measures: (1) the number of top 100 singles each label had on the Hot 100 in a given week, (2) the number of top 20 singles each label had on the Hot 100 in a given week, and (3) whether a label had the number one single during a given week. These three measures serve as the dependent variables in panel regressions. Using $Y_{l,t}$ to denote the dependent variable for record label *l* during time *t*, then the panel regression model can be expressed as:

$$\begin{aligned} Y_{l,t} = {} & \beta_1 * 1[\textit{Payola Law in Effect}] + \beta_2 * [\text{Time Since FTC Complaint}_{l,t}] \\ & + \beta_3 * [\text{Time Since FTC Complaint}_{l,t}]^2 + \beta_4 * [\text{Major label fixed effect}] \\ & + \sum_l \text{Record label fixed effects}_l + \sum_l \text{Record label fixed effects}_l * \textit{Time}_t \\ & + \sum_m \text{Month fixed effects} + \sum_y \text{Year fixed effects} + \varepsilon_{l,t} \end{aligned} \qquad (2)$$

In equation (2), both the payola law and the FTC payola citations are allowed to affect the dependent variable. Given the somewhat dubious nature of payola methods, it is not safe to

[11] With the time trend variables, the year fixed effects add little to the analysis, but we keep them for completeness. Excluding these year fixed effects does not impact the primary coefficient of interest (that of the payola law).

say with certainty that record labels not cited by the FTC did not use payola. However, it does seem reasonable to assume that those firms cited by the FTC were among the most likely to be using payola. For this reason, we include the FTC citation information to determine the effect of prohibiting record labels from using payola. We adopt an approach similar to Wolfer's (2006) study of divorce laws and allow the impact of FTC complaints to change over time. The time-varying impact of being cited by the FTC is captured through coefficients β_2 and β_3.

In terms of the controls used, fixed effects for each record label are included to help account for unobserved heterogeneity, as well as record label-specific time trends. As in the aggregate regressions, month and year fixed effects are also included.

4. Results

4.1. *Aggregate Regression Results*

The aggregate regression results are presented in Table 2.

Table 2.
Payola Law Impact Regressions

	Variety Index	Turnover	Av. Age of Top 5	Av. Age of Top 20	New #1[a,c]
Payola Law in Effect (=1)	-0.051 **	1.534	-0.843 *	-0.388 *	0.279 *
	(0.020)	(1.037)	(0.508)	(0.218)	(0.168)
Time	-0.013	-0.012	-0.031	0.036	0.017
	(0.014)	(0.184)	(0.090)	(0.044)	(0.032)
Time Squared	2.25E-05	-6.57E-05	8.19E-05	2.55E-05	-1.98E-05
	(4E-05)	(1E-04)	(9E-04)	(4E-03)	(3E-04)
Controls					
Month Fixed Effects	F = 2.31	F = 0.65	F = 3.04	F = 5.26	F = 4.36
Year Fixed Effects	F = 0.39	F = 0.28	F = 0.19	F = 0.47	F = 0.26
R-squared	0.5266	0.1558	0.2394	0.3693	0.1492
Chow Test[b]	F = 3.31	F = 1.12	F = 4.12	F = 3.52	F = 1.95

Number of Observations = 156

Notes: a - Probit Marginal Effects are shown. Pseudo R-squared value presented. b - Chow test of whether coefficients are equal in the pre- and post-payola law change samples ($F_{14, 128}$). c - Chow test is calculated using linear probability model, not the probit estimates. * - significant at the 10% level. ** - significant at the 5% level.

We begin by examining the variety index results, as this is our primary outcome variable of interest. The coefficient on "Payola Law" is negative and statistically significant at the 5%

level, indicating that variety falls in the post-payola period. This finding reinforces the t-test result in Table 1, but is more robust because we now control for a variety of factors. The final row in Table 2 shows the F-value on a Chow test. The Chow test, as used in this study, is a test of whether the model coefficients are (statistically) different if we estimate the model using the pre- and post-amendment sample separately.[12] The result of the Chow test, which is statistically significant at the 1% level, indicates that there is a difference between the pre- and post-amendment coefficients.

The remainder of the results are not particularly strong, perhaps due to the lowered variation among the aggregated measures. In the average age regressions (columns 3 and 4) and the binary probit (column 5) the only significant estimate, aside from the joint significance of the month fixed effects, is for the payola prohibition. The Chow tests are significant for these three sets of results, as well. The payola prohibition coefficient is negative and significant in both of the age regressions, indicating that songs stayed at the top of the charts for shorter periods of time when payola was outlawed. Similarly, the likelihood of a brand new number one single in a given week goes up by approximately 28 percent once payola is prohibited. While these coefficients are significant only at the 10% level, collectively they provide support for the theory that prohibiting payola decreased the survivability of songs and lead to increased churn.

4.2. *Panel Regression Results*

The panel regression results are presented in Tables 3 – 5.

[12] The Chow tests exclude the "Payola Law" variable as it cannot be identified when the model is estimated using the divided sample.

Table 3.
FTC Complaint and Payola Law Panel Regressions:
Number of Top 100 Hits

	Full Sample	Top 10 Labels	Bottom 10 Labels
Time Since Complaint	-0.005	-0.003	-0.008
	(0.006)	(0.011)	(0.007)
Time Since Complaint Squared	-0.001 ***	-2.0E-04 **	2.66E-05
	(4E-04)	(8E-05)	(5E-05)
Payola Law in Effect (=1)	-0.088	-0.843 ***	0.291 ***
	(0.099)	(0.182)	(0.106)
Major Label Fixed Effect	2.572 ***	0.785 **	---
	(0.272)	(0.353)	
Controls			
Record Label Fixed Effects	F = 92.91	F = 92.99	F = 52.88
Record Label-Specific Time Trend	F = 51.08	F = 52.62	F = 51.56
Month Fixed Effects	F = 2.24	F = 5.82	F = 2.71
Year Fixed Effects	F = 0.49	F = 0.71	F = 1.59
Chow Test[a]		F = 4.79	
Adjusted R-squared	0.66	0.56	0.35

Number of Observations = 3900

Notes: a - Chow test of whether coefficients are equal between the Top 10 and Bottom 10 samples ($F_{35, 3048}$). *** - significant at the 1% level. ** - significant at the 5% level. * - significant at the 10% level.

The dependent variable in Table 3 is the number of top 100 hits record label *l* has during week *t*. Column 1 shows estimates of equation (2) using the full sample of 25 record labels. The coefficient on our main variable of interest ("Payola Law") is negative, but not statistically significant. Because of heterogeneity among labels, it may be the case that the effect of the payola law is different for large labels than it is for smaller labels. To allow for this possibility, we also separately estimate equation (2) for the top 10 and bottom 10 labels.[13] These estimates are presented in columns 2 and 3. Using the partitioned samples we see that the payola law had a negative and statistically significant impact on the number of top 100 hits for larger labels, while there is a positive and statistically significant impact for smaller record labels. It is important to

[13] We would like to thank an anonymous referee for this suggestion.

note that these results control for time trends and other unobserved factors. A Chow test of the hypothesis that the coefficient estimates are the same for the top 10 and bottom 10 samples is rejected at the 1% significance level.[14] It is interesting to note that the effect of the FTC complaints is far overshadowed by the effect of the payola law itself.

Table 4 shows the results of replicating Table 3 using the number of top 20 hits as the dependent variable.

Table 4.
FTC Complaint and Payola Law Panel Regressions:
Number of Top 20 Hits

	Full Sample	Top 10 Labels	Bottom 10 Labels
Time Since Complaint	-0.004	-0.012 **	0.011 ***
	(0.003)	(0.006)	(0.004)
Time Since Complaint Squared	-2.00E-05	-4.53E-05	-5E-05 **
	(2E-05)	(4E-05)	(2E-05)
Payola Law in Effect (=1)	0.006	-0.186 *	-8.19E-05
	(0.050)	(0.096)	(0.052)
Major Label Fixed Effect	0.223	-0.466 **	---
	(0.135)	(0.187)	
Controls			
Record Label Fixed Effects	F = 28.42	F = 27.29	F = 11.74
Record Label-Specific Time Trend	F = 17.48	F = 19.28	F = 20.41
Month Fixed Effects	F = 0.93	F = 1.10	F = 0.71
Year Fixed Effects	F = 1.45	F = 2.88	F = 0.16
Chow Test[a]		F = 1.77	
Adjusted R-squared	0.34	0.28	0.19
Number of Observations = 3900			

Notes: a - Chow test of whether coefficients are equal between the Top 10 and Bottom 10 samples ($F_{35, 3048}$). *** - significant at the 1% level. ** - significant at the 5% level. * - significant at the 10% level.

We again present the estimates for the full sample and for the two subsamples. The results are generally similar to those in Table 3. The coefficient on "Payola Law" is negative and statistically significant for the top 10 label subsample. The estimate is insignificant for the

[14] The Chow test excludes the major label fixed effect as the variable is not identified using the bottom 10 sample.

bottom 10 labels, however. A Chow test, once again, rejects the hypothesis that the coefficients are equal for the two subsamples, although at a lower level of significance. These results may, at least partially, be due to the fact that it is more difficult to predict the number of top 20 singles (note the R-squared in Tables 3 and 4).

The final set of results is for the binary dependent variable: whether record label *l* has the number one hit in week *t*. We exploit the panel nature of the data and estimate a panel probit that includes a record label random effect. Also included are controls for the month and calendar year. Estimated marginal effects from the panel probit appear in Table 5.

Table 5.
FTC Complaint and Payola:
Number 1 Hit (Panel Probit Marginal Effects)

	Full Sample	Top 10 Labels	Bottom 10 Labels
Time Since Complaint	0.015 **	0.014 *	3.94E-04
	(0.007)	(0.008)	(3E-04)
Time Since Complaint Squared	-1.97E-03 **	-1.71E-03 *	-4.99E-05
	(8.00E-04)	(1E-03)	(4E-05)
Payola Law in Effect (=1)	-0.209	-0.286	0.016
	(0.216)	(0.265)	(0.009)
Major Label Fixed Effect	0.817 ***	0.162	---
	(0.156)	(0.215)	
Controls			
Month Fixed Effects	F = 8.92	F = 12.23	F = 1.35
Year Fixed Effects	F = 0.56	F = 1.57	F = 0.54
Chow Test[a,b]		F = 2.52	
Log-likelihood	-433.38	-306.76	-401.22
Number of Observations = 3900			

Notes: a - Chow test of whether coefficients are equal between the Top 10 and Bottom 10 samples ($F_{17, 3086}$). b - Chow test is calculated using linear probability model, not the probit estimates. *** - significant at the 1% level. ** - significant at the 5% level. * - significant at the 10% level.

While the marginal effect is negative for the full sample and the top 10 label subsample, the payola law does not appear to have had a significant effect on the likelihood of achieving a number one single. The FTC citations, however, did have a significant impact. The estimates

indicate that the citations had a negative impact, although this was not felt immediately. In the full sample, it takes approximately two months (eight weeks) before the probability of attaining a number one hit decreases for record labels cited for payola. For the top 10 label subsample the lag is slightly longer (nine weeks). The fact that number one songs tend to rise up the chart over time may help explain this finding. Of the number one hits during the 156 weeks in the sample, not one song reached number one on its first week on the charts. In other words, singles move up the chart over time. The two month lag we observe may simply capture the fact that songs already moving up the charts, possibly because of prior payola payments, were unaffected by the payola citations, but subsequent singles released by the label were affected and, therefore, had a lower probability of attaining the number one spot on the Hot 100 chart.

5. Discussion & Conclusion

In this paper we have investigated the impact that payola had on radio airplay variety during the three years surrounding the 1960 Amendments to the Communications Act. Empirical estimates account for monthly effects, general time trends, and, where applicable, record label specific time trends. The main finding is that payola did result in greater musical variety. This implies that, perhaps, payola was used to incentivize radio stations to "take a chance" and play a wide variety of musical styles.[15] The greater variety was accompanied by less churn on the charts, with songs lingering for longer time periods. A series of panel regressions illustrate that larger record labels were the greatest beneficiaries of payola, perhaps due to their ability (at least relative to smaller record labels) to make the necessary financial payments. The implication being that payola was an effective instrument in gaining radio airplay for those labels willing to pay.

[15] Rossman et al. (2008) develop a model of diffusion that explains how radio stations adopt new songs for their playlists. Their findings indicate that this pattern of adoption is notably different when payola is used.

These findings help give us a greater understanding of the role that payola played in the vertical channel. We now discuss how this role, as characterized by our empirical findings, relates to economic theories of consideration payments (namely slotting allowances). A number of views on both the role and impact of slotting allowances have emerged in the growing literature on slotting.[16] Two things make direct comparison difficult. First, the slotting literature has focused primarily, although not exclusively, on understanding the impact that slotting allowances have had on retail prices. In broadcast radio, listeners are able to "purchase" the product at zero monetary cost. Studies of radio listenership, for example Mooney (2009), assume that listeners receive disutility from hearing radio advertisements and that this is the "price" of listening. Examining the amount of on-air advertising might allow us to address this price issue. Unfortunately, detailed information regarding the amount of on-air advertisement does not exist for the time frame of this study. The second detail that makes direct comparison difficult is that many of the slotting models assume either homogenous products (Shaffer 1991, for example) or that the downstream firm offers only one product to consumers (Chu 1992, for example). Given our focus on variety, these models do not seem particularly applicable.

The findings do, however, confirm some of the roles of slotting allowances outlined in previous papers. For example, Sullivan (1997) argues that slotting allowances are likely to emerge in areas where the growth in new products outpaces growth in available retail space. In this context, slotting allowances serve to equate supply and demand. While we do not have precise evidence of this for the 1959-1961 time period, it is generally acknowledged that radio plays only a small fraction of the music available, making the supply/demand explanation seem applicable. In addition to this role, others (Chu 1992) have argued that slotting allowances may be used when there is asymmetry regarding the quality of the products. In such models, slotting

[16] For an up-to-date summary of the literature on slotting, please see Deltas (2006) and Klein and Wright (2007).

allowances (or payola in our case) are used by the upstream firm to signal their strong belief in the quality or popularity of their product. Record labels using payola did not provide uniform support for all songs they released, indicating they may have been selectively promoting "higher quality" songs.

While we find that variety increased because of payola (or, equivalently, decreased because of its prohibition), we have thus far avoided linking this conclusions regarding consumer welfare. The simple reason for this is that variety changes, on its own, do not allow us to draw conclusions regarding welfare. This fact can be illustrated simply using a standard Hotelling model of horizontal differentiation. In the duopoly case with fixed prices, the efficient locations are ¼ and ¾. Starting from maximum differentiation (both firms located at the endpoints), a move to [¼,¾] would decrease variety by reducing differentiation but would increase welfare. On the other hand, starting from minimum differentiation (both firms located at the midpoint), a move to the efficient locations would increase variety and increase welfare. Wright (2007) argues that to get an accurate picture of the welfare impact, one needs a measure of aggregate product category performance. For example, Wright examines the welfare consequences of slotting allowances by determining whether category sales, as opposed to sales of specific brands, changed with the introduction of slotting allowances. The equivalent measure on our model might be total radio listeners or total record sales. We were unable to find detailed radio listenership data, however we were able to find annual record sales information through the Recording Industry Association of America (RIAA).[17] While aggregate annual record sales is less than ideal for such a short time period, it does allow us to make at least a small step towards

[17] The RIAA figures are reprinted in Harvey Rachlin's *Encyclopedia of the Music Business*, published in 1981 by Harpercollins.

identifying the welfare effect. Using annual sales from 1950 through 1966, we estimate a simple regression.[18] The results are presented in Table 6 below.

Table 6.
Payola Law and Annual Record Sales

	Record Sales (in millions)
Payola Law in Effect (=1)	-103.814 *
	(48.55)
Time	-29.026
	(26.59)
Time-squared	9.637 **
	(3.59)
Time-cubed	-0.302 **
	(-0.13)
Adjusted R-squared	0.976

Number of Observations = 17

Notes: * - significant at the 10% level. ** - significant at the 5% level.

The results indicate that overall record sales were lower in the post-payola periods, after controlling for time trends. The payola law dummy and the time trends explain record sales well. The coefficient estimates indicate that payola reduced not only variety, but also overall consumer welfare. To make strong predictions regarding the welfare effect, however, we believe future research is necessary beyond this simple descriptive regression. At a minimum, however, this research has shown that banning payola may have increased access to radio stations for smaller record labels but the ban lead to reduced variety and an apparent reduction in total record sales, hinting at a reduction in consumer welfare.

[18] After 1966 the RIAA's sales figures include both records and cassette tapes. To make comparison easier, we restrict our sample to the pre-1967 period.

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Appendix A: Constructing Variety Index

The variety index is the primary variable of interest in the aggregate regressions. We constructed this index based on the average degree of similarity between artists on the charts in a given week. Each artist appearing in the *Billboard* Top 20 during the sample period was researched using the All Music Guide database (http://www.allmusic.com). In total, there were 291 artists. For each artist, the All Music Guide lists a genre and a set of musical styles. While typical genre labels, such as "Pop/Rock" or "Jazz", provide very little information regarding the style of music, the All Music Guide's musical style listings are intended to capture the true characteristics of each artist's music. Examples of these musical styles include "Rockabilly", "Nashville Sound/Countrypolitan", "Doo Wop", "Traditional Folk", and "Dixieland Jazz". For each of the 291 artists in the sample, we construct of a 31 x 1 vector of dummies that indicate which of the 31 music styles are attributed to the artist by the All Music Guide. Artists have, on average, around three musical styles listed in their database profile.

Using these vectors, we calculate the percentage of overlap or similarity between each pair of artists in a given week's Top 20. To illustrate this process, consider the following simple example: suppose there are two artists (A & B) and three possible musical styles (X,Y, & Z). Artist A is associated with musical style X, but neither of the other two styles. Artist B, on the other hand, is associated with musical styles X and Y, but not Z. The degree of similarity between A and B would be 0.50 because Artist A only shares one of Artist B's two possible musical styles. This process is repeated for each pair of artists in the week's Top 20. We then average these similarity percentages across all artist pairs to get a weekly average. This weekly average is then subtracted from 1 to yield our variety index. In this context, "variety" measures the average dissimilarity in the musical styles of the artists in each week's Billboard Top 20.