Discrimination and Minority Ownership in Radio Broadcasting

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Abstract

Minority preferences in communications regulation are common. In this paper, one particular justification for minority preferences in radio broadcasting is examined. Specifically, we evaluate whether or not minority-owned radio stations experience a revenue penalty simply because they are minority owned. Evidence of such discrimination may support minority preferences in radio broadcasting under the more stringent legal standard for minority preferences established by the Supreme Court decision of *Adarand v. Peña*. We find no evidence of discrimination in the radio industry, at least discrimination that would be detected by a revenue handicap for minority-owned stations.

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I. Introduction

The costs and benefits of regulation typically are evaluated in terms of their effect on market performance, where performance is measured by economists' concepts of static efficiency (production and allocative efficiency) and dynamic efficiency (innovation and full employment). While often ignored, market performance also includes the notion of equity (Scherer and Ross 1990). Equity, however, is terribly difficult to define and, as a consequence, economists have had little use for it. Nevertheless, in the regulatory arena, equity often has first chair (Owen and Breautigam 1978; Bolter and Duvall, *et al.* 1984; Zajac 1996).

Minority preferences in communications regulation are a case in point. In the early 1970's, the Federal Communications Commission (FCC) began developing minority preference programs to encourage minority ownership in the broadcasting industry. The primary minority preference programs were "qualitative enhancements" for minorities in comparative licensing hearings and tax certificates allowing the seller to defer capital gains taxation by selling to a minority-owned or controlled groups (Evans 1990). While these programs have been the subject of constitutional challenge, two recent events have substantially weakened the federal agencies ability to construct race or gender based preference programs. First, in the spring of 1995, Congress repealed Section 1071 of the Internal Revenue Code ending the FCC's ability to grant tax certificates to sellers of communications properties. Second, the legal hurdle for Federal minority preference programs was raised by the Supreme Court's decision in *Adarand Constructors, Inc. v. Peña* (1995). In *Adarand*, the Supreme Court held that federal minority preference programs must meet the standard of "strict scrutiny," which implies a) the program must serve a compelling governmental interest (such as remedying past discrimination); b) the program must be narrowly tailored to serve that particular interest; and c) the need for a federal program must have a "strong basis in evidence (Bush and Martin 1996)."

In an effort to support its own minority preference policies and implement Section 257 of the Telecommunications Act of 1996, the FCC commissioned a number of studies to determine whether or not racial discrimination is an entry barrier for small telecommunications businesses. The first of these studies was released in January 1999 entitled "When Being No. 1 Is Not Enough: The Impact of Advertising Practices on Minority-Owned & Minority-Formatted Broadcast Stations."[1],[2] Using anecdotal evidence, the FCC commissioned study reaches two general conclusions: a) "stations that target programming to minority listeners are unable to earn as much revenue per listener as stations that air general market programming; and b) minority-owned radio stations earn less revenues per listener than majority broadcasters that own a comparable number of stations (p. ii)."[3]

These revenue handicaps are attributed to race-based discrimination by the advertising industry. The impact of these revenue handicaps is to "hinder a broadcaster's ability to attract investment capital, and to produce high quality news, information and entertainment programming in response to the needs of listeners … and to … undermine competition and detract from the First Amendment goal of diversity of viewpoints (p. iv)." Since the Supreme Court has held that "diversity" is a compelling government interest, the minority preference policies in broadcasting might survive judicial scrutiny if these findings can be supported by statistical analysis.

In this paper, we propose and conduct an empirical test of racial discrimination in the radio industry. Specifically, this investigation evaluates whether or not minority-owned radio stations are systematically penalized in terms of advertising revenues for being, quite simply, minority owned (as the FCC study suggests). The balance of the paper is organized as follows. In Section II, the econometric model is specified and the results summarized in Section III. Concluding comments are provided in Section IV.

II. The Empirical Model

There exists an expansive economic literature, both theoretical and empirical, on racial discrimination, and we draw from this literature in our assessment of minority ownership in the radio broadcast industry. Specifically, using the Blinder decomposition model, a model that is frequently used to test for discrimination in labor markets, we estimate via least squares the effect of minority ownership on station revenues (Blinder 1973; Flanagan 1973; Jackson and Lindley 1989; Sharpe and Abdel-Ghany 1996). The decomposition model is essentially a fully interactive econometric model that specifies the dependent variable (e.g., income, revenue) as a function of relevant explanatory variables, a dummy variable indicating the status of a particular group (e.g., majority or minority), and

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[11] The study is available at the FCC's web site: (http://www.fcc.gov/Bureaus/Mass_Media/Informal/ad-study/).

[2] Additionally, an attempt to reinstate minority tax certificates recently has been initiated by Senator John McCain (R-AZ). "McCain Floats New Tax Certificate Program," Communications Daily, Tuesday, September 14, 1999.

[3] Other studies have made similar claims (Evans 1990; Ofori and Lloyd 1999).

dummy interaction terms that capture the difference in the coefficients between the groups.

More formally, this decomposition model partitions group-means differences into two parts: the *endowment effect* and a *residual difference*. The endowment effect measures revenue differences arising from variations in exogenous attributes (e.g., education, age, station service class). Since these factors are exogenous, the endowment effect does not measure discrimination. The residual difference captures the source of group means differences that cannot be attributed to exogenously determined endowments. Thus, the residual difference, appropriately interpreted, reveals the presence and nature of discrimination.

In an effort to separate the endowment effect and the residual difference, Blinder's decomposition technique utilizes the following behavioral equation:

$$Y_i = a + \sum_j b_j X_{ji} + e_i \qquad i = 1,\ldots,n;\ j = 2,\ldots,k \tag{1}$$

In the context of this paper, Y_i is the revenue earned by the i^{th} station, the X_{ij} are the j = 1,...,k station-specific characteristics (e.g. station class, ratings, etc.) of the i^{th} station, e_i is a stochastic disturbance, and a and b_j are the parameters to be estimated by least squares regression.

One approach to determining whether discrimination explains income-related differences across stations is to consider (1) in terms of distinct samples. That is, to consider the average revenue of one sample made up only of majority-owned stations, and the average revenue of another sample, made up only of minority-owned stations. These equations are given (respectively) as:

$$\bar{Y}^{\mathrm{A}} = a^{\mathrm{A}} + \sum_j b_j^{\mathrm{A}} \bar{X}_j^{\mathrm{A}} \tag{2}$$

$$\bar{Y}^{\mathrm{B}} = a^{\mathrm{B}} + \sum_j b_j^{\mathrm{B}} \bar{X}_j^{\mathrm{B}} \tag{3}$$

where a and b_j are estimates of a and b_j respectively, the A and B superscripts denote that the results refer to the majority sample and minority sample (respectively), and the bar superscripts denote sample means.

The total difference of (3) and (2) yields an expression containing the three effects mentioned above:

$$\overline{Y}^{B} - \overline{Y}^{A} = \left(a^{B} - a^{A}\right) + \left[\sum_{j} \overline{X}_{j}^{B}\left(b_{j}^{B} - b_{j}^{A}\right)\right] + \left[\sum_{j} b_{j}^{A}\left(\overline{X}_{j}^{B} - \overline{X}_{j}^{A}\right)\right]. \qquad (4)$$

The third RHS term of equation (4) is the endowment effect, measuring the influence of exogenously determined factors on the dependent variable. The first two terms on the right hand side measure the residual difference and include the constant effect (the first term) and the coefficient effect (the second term).

The coefficient effect measures differences in the response of the dependent variable (revenue) to changes in the explanatory variables. For example, does a rating point of a minority-owned station generate less revenue than that of a majority-owned station? Such a differential response, when the market values changes in the characteristics of minority-owned stations differently from changes in the characteristics of majority-owned stations, is consistent with what we should expect from the situation where discrimination is present. The constant effect determines whether revenue differences across stations are explainable for reasons other than different endowments or different responses to endowments. For example, the constant effect would be nonzero if, after controlling for endowment and coefficient effects, a minority-owned station earned lower revenues simply because it was not a majority-owned station. Although both the coefficient effect and constant effect are thought to indicate the presence of discrimination, it is the latter effect (the constant effect) that Jackson and Lindley (1989) say provides more conclusive evidence of discrimination. The reason for this stems from supply side choices that potentially impact the coefficient effect (Jackson and Lindley, p. 522). In the case of radio stations, differences in how minority and majority-owned stations operate with some given set of endowments may differently affect how the station revenues of minority vs. majority-owned stations change with changes in characteristics like market size.[4]

In the context of the decomposition model, discrimination is correctly inferred when residual difference is statistically significant. Following Jackson and Lindley (1989), pooling the data and specifying a fully interactive model can assess the statistical significance of the residual difference.[5] To do so, we create a dummy variable to indicate minority status ($\delta = 1$ if station is minority owned, 0 otherwise) and then multiply this dummy variable by each of the k explanatory variables.[6] Summing the k interactive terms and k non-interactive terms, we obtain the following estimation equation:

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[4] For example, minority-owned stations might better target minority audiences, making their advertising spots more valuable to advertisers seeking to target that particular audience.

[5] Pooling requires that the disturbance variances of the two group regressions are equal.

[6] Jackson and Lindley (1989, pp. 535-7) discuss in detail the consequences of the choice of index group (i.e., the group for which the dummy variable equals unity). While defining the index group differently does affect the estimated size of the residual difference, the statistical tests are not influenced by this choice.

$$Y = a + \sum_{j=2}^{k} b_j X_j + \left(d \cdot \left[a + \sum_{j=2}^{k} b_j X_j \right] \right). \qquad (5)$$

Premultiplying the subvector of interaction coefficients ($\mathbf{b}^B - \mathbf{b}^A$) by a row vector consisting of the sample means of the explanatory variables for the majority group ($\overline{X}^A$), we obtain an expression that is identical to the traditional residual difference: [7]

$$\overline{X}^A \left[\mathbf{b}^B - \mathbf{b}^A \right] = \left(a^B - a^A \right) + \sum_{j=2}^{k} \overline{X}_j^A \left(b_j^B - b_j^A \right). \qquad (6)$$

Jackson and Lindley (1989) point out that (6) suggests a simple and direct test of the significance of the residual difference.[8] That is, after estimating (5) with the pooled data set we can determine the statistical significance of the residual difference by jointly testing the k interaction terms with a standard F-test. The test statistic is:

$$F^* = \frac{SSE_c - SSE_u}{k} \bigg/ \frac{SSE_u}{N - 2k} \qquad (7)$$

where SSE_c and SSE_u are the sum of squared residuals obtained from estimating the pooled model excluding and including, respectively, the dummy and the other $k - 1$ dummy interaction variables. The test statistic, F^*, follows an F distribution with k and $N - 2k$ degrees of freedom. We can conclude that ($\mathbf{b}^B - \mathbf{b}^A$), the residual difference, is statistically different from zero at the α level if $F^* > F_\alpha(k, N - 2k)$. The statistical significance of the coefficient effect alone can be tested using Equation (7) with the exception that SSE_c relates to a model using a dummy without interaction terms (the dummy variable for minority ownership is not included in the constraints).

III. Model Specification

In order to more precisely assess the revenue impact of minority ownership (as opposed to minority audience), we must limit our attention to stations with similar audience profiles. Differences in station revenues between differentially formatted stations may arise simply because audience demographics (e.g., purchasing power) vary by format and not because of racial discrimination.[9] For example, members of minority

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[7] Note that, as above, the first term on the right hand side is the constant effect and the second the coefficient effect.

[8] Jackson and Lindley state that $\mathbf{b}^B = \mathbf{b}^A$ represents a sufficient condition for the residual difference to equal zero.

[9] In terms of revenue or revenue per rating share, minority-targeted formats rank higher than some format categories and lower than other categories. Using the sample of 2,911 stations (as described in section IV, part 1), Urban formatted stations rank second (of 17 major format categories) in average revenue per

audiences have, on average, incomes that are substantially below that of the members of majority audiences (about 40% less on average) and advertisers may value minority audiences less because of this lower average purchasing power.[10] Alternatively, finding that minority-ownership produces lower revenues *within the same format* (i.e., demographics constant) is far more compelling evidence of discrimination than revenue differences across formats. Of course, even within-format revenue differences could be explained by systematic disparities in the quality of station management or sales staff, but there is no a priori reason to expect such disparities.

For three reasons, we choose to limit our attention to those stations that target Black or Hispanic audiences (the Urban or Spanish format categories). First, we do not have data on the income of audiences that listen to particular stations or formats.[11] We do know, however, that the average income of Blacks and Hispanics are similar and well below that of Whites and Asians. Second, while we could just as easily evaluate the effects of minority ownership in majority-targeted stations, the small sample sizes of minority-owned, majority formatted stations preclude this approach.[12] Third, the Black-Hispanic distinction is that adopted by the FCC sponsored study and, as such, appears to be focus of the policymakers. In sum, our sample contains stations that are either minority or majority-owned, but target minority audiences only.

BIA MasterAccess, a database containing station and market data for a large number of radio stations, provides most of data for the empirical model.[13] From this database we able to construct a final sample of 129 minority-targeted radio stations, 57 of which are minority-owned.[14] Recognizing the potential for sample selection bias (i.e., the sample is

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station. Urban ranked eleventh for AM stations. Spanish format ranked fifth and fourth for FM and AM stations, respectively.

[10] For evidence of this point, see the working paper by Richard W. Ault, George S. Ford, and John D. Jackson, *Discrimination in Advertising? An Empirical Test* (Auburn University Working Paper).

[11] While we attempted to acquire data on station specific audience characteristics, the cost of the data was prohibitive. The effect of audience characteristics can be very important, as illustrated by some preliminary research on magazine advertising rates (see www.egroupassociates.com / download / raceinc.htm).

[12] There are not Asian targeted stations with sufficient data to include in the final sample.

[13] This database is the same one employed by the FCC funded study by Ofori (1999).

[14] We limited the full sample of 10,403 stations (287 minority owned) in a number of ways. First, *MasterAccess* does not provide 1996 revenue estimates for a large number of stations (6,595 stations, 123 minority owned). These, of course, are excluded. Second, *MasterAccess* reports only the combined revenues for stations that are in AM-FM combos, making it impossible to separate out the revenues for the individual stations and forcing us to eliminate all combos from the sample (897 stations; 87 minority owned). Again, these stations are excluded from the final sample. After these two restrictions, 2,911 stations remained of which 77 are minority owned. Of the minority-owned stations, 57 stations (74%) were formatted to specifically target minority audiences falling into either the Urban or Spanish major format category. Finally, the 57 minority-formatted stations in the sample were all located in 38 different markets. We restricted the sample to include only those markets where minority-owned stations were located (total sample is 129 stations, 57 minority owned).

non-random) arising from both necessary and conceptually desired sample restrictions, we employ a two-stage selection procedure where a probit selection equation is estimated and the results passed on to the least squares regressions as the Inverse Mill's ratio (Heckman 1976; Barnow, et al. 1980). Due to the nature of our sample restrictions, we estimate two selection equations. In the first, we specify a probit equation that determines whether or not a station has usable revenue data (dummy = 1) as a function of station and market characteristics. The Inverse Mill's ratio from this equation is then passed on to a second probit equation, where the dependent variable equals one if the station is in our final sample (129 observations), zero otherwise. The Inverse Mill's ratio from this regression (*IMR*) is passed on to the decomposition regression to remedy the potential sample selection problem.[15]

Equation (5) provides the general specification for the empirical model. The dependent variable of the fully interactive decomposition model is (the natural log of) annual station revenues (*lnSTAREV*). A number of explanatory variables are included in the model to account for endowment effects. First, the primary factor affecting a station's revenue will be its rating share. Radio stations are in the business of selling access to their audience and the bigger the audience, the more valuable the station's advertising time. In our model, we measure each station's rating share variable (*RATE*) as the station's average rating share over the four quarters of 1996.

Given the perceived higher quality of FM signals, FM stations are expected, *ceteris paribus*, to be more valuable to advertisers than AM stations.[16] Likewise, stations with larger coverage areas (or contours) will be, whether AM or FM, more valuable to advertisers. Station class dummy variables are employed to measure the effect on revenue of both station service type and coverage area. FM stations are divided into three (major) classes (A, B, and C) and AM stations into four classes (A, B, C, and D).[17] These station classes roughly correspond to the size of their coverage contours and, in some cases, to other quality factors such as restrictions on broadcasting hours. We include three dummy variables in the regression to account for FM station classes (*FMA*, *FMB*, and *FMC*).[18] These variables will measure the difference in the revenues of FM stations and AM stations and the effect of contour size for FM stations. Preliminary regressions did not

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[15] We are extremely grateful to John D. Jackson and G. S. Maddala for comments on our approach to the sample selection problem.

[16] See Robert B. Ekelund, Jr., "Minority-owned Radios Doing OK Financially," *USA Today*, June 17, 1999.

[17] AM classifications less directly indicate coverage contours. AMA, AMB, and AMD stations all have a maximum power of 50kW, while AMC has a maximum power of 1kW. AMA stations have a higher minimum broadcast power than do AMB stations. AMD stations may have time of day restrictions on operation or broadcast power. More detailed information on station classes can be obtained from the FCC's web page at: www.fcc.gov/mmb/asd/fmclasses.html (or amclasses.html).

[18] The broadcast power of the station was included in some preliminary regressions, but was statistically insignificant when the class dummy variables were included (and was also statistically insignificant in the final specification of the model).

reveal any differences in revenues between AM station classes, so all AM classes are subsumed into the constant term. Since AM class D stations may be required to decrease broadcast power in the evening, a variable is included to measure the ratio of night-to-day transmission power (*PWRAT*) for those stations. No AM Class A stations survived the sample restrictions.

The revenues of stations may also be affected by the overall size of the market where the station is located. Thus, we include 1996 total radio advertising dollars and its squared value as a measure of market size ($MREV$, $MREV^2$).[19] Owning multiple stations in a single market may allow an operator to offer a more desirable advertising product. Thus, we include a variable that measures the number of stations operated by the station owner (*NUMSTA*).

The list of minority-owned radio stations in NTIA's *Minority Ownership Report* (1997) was used to determine ownership status.[20] A minority ownership dummy variable (M) is equal to 1 if the station is owned by a minority, 0 otherwise. In the decomposition model, this dummy variable is interacted with all the explanatory variables in addition to being a regressor itself. Variable definitions and descriptive statistics are provided in Table 1.

One particular interaction term of interest is $M \cdot RATE$. The FCC study concludes that there are differences between the success of minority and majority-owned stations in converting rating share into revenues, where minority-owned stations earn less revenue per rating share than do majority-owned stations. If this is true, the sign of $M \cdot RATE$ should be negative and statistically significant.[21] Alternatively, if minority owners are better at targeting minority audiences and discrimination does not exist, then the coefficient on $M \cdot RATE$ should be positive and statistically significant.

1. Other Specification Issues

In the decomposition model described above, a finding of discrimination can arise from model mis-specification or discrimination. Thus, it is crucial that the model be specified correctly. Econometric specification errors such as omitted variables, endogenous explanatory variables, errors in measurement, and an incorrect functional form can each cause least-squares estimates of the between-group constant and coefficient

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[19] The RESET test could not be passed without the squared *MREV* term. We could not reject the hypothesis that the other squared terms were (jointly) zero.

[20] The *Minority Ownership Report* can be downloaded from the NTIA web site at: (http://www.ntia.doc.gov/reports/97minority/index.html).

[21] This approach is superior to using the power ratio as an explanatory variable. First, a station with low revenues and low ratings could still have a high power ratio. In fact, the power ratio can be undefined, since stations with zero ratings do generate revenues.

differences to be biased, inconsistent, and inefficient.[22] The RESET test is a rather general test of specification error, and detects the specification problems listed above (Ramsey 1969; Ramsey and Schmidt 1976; Thursby 1979).[23] Note that the null hypothesis for RESET is 'no specification error,' so the desire is to accept the null at higher probability levels.

The RESET *F*-statistics for the regressions are presented in Table 2. The *F*-statistic for the decomposition regression is quite low, and we are unable to reject the null hypothesis of 'no specification error' at anything near standard statistical levels. This result is encouraging since we can be reasonably certain that our model does not suffer from statistically significant specification errors of the types listed above. The low RESET *F*-statistic indicates that there is no serious specification error – at least not enough to result in statistically significant biases in our estimates. The semilog specification we have chosen performed best in terms of the RESET test.

Another general test for specification error is the White test (White 1980). While typically used as a test for heteroskedasticity, the null hypothesis of the White test assumes that the errors are both homoskedastic and independent of the regressors and that the linear specification of the model is correct. Failure of any of these conditions could lead to a rejection of the null hypothesis. We are encouraged by the inability to reject the null hypothesis of the White test for the decomposition regression, indicating (at a minimum) that heteroskedasticity is not an issue.

Another potential specification issue regards the validity of the statistical test of the residual effect. Our pooled data approach and the validity of the statistical tests on the residual effect are valid only if the two groups (minority, majority-owned) have equal disturbance variances. Using a Goldfeld-Quandt test, we cannot reject the hypothesis that the disturbance variances are equal across groups ($F^{*}_{.05}(0.05, 61, 46) = 1.41 < 1.60$).[24]

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[22] This class of error violates the least squares assumption of a non-null mean for the theoretical disturbance vector.

[23] RESET is conducted by first estimating the model, and then re-estimating the model including the powers of the predicted value of the first regression as explanatory variables. Thursby and Schmidt (1977) suggest that the best variant of the RESET test includes the square, cube, and fourth powers of the predicted value. These variables act as proxies for omitted variables and determine the likelihood of an omitted variables problem and other specification errors that lead to a non-null mean for the theoretical disturbance vector.

[24] The disturbance variances are acquired by estimating the econometric model (only the first *k* coefficient) for each of the two samples (minority-majority) independently. The ratio of the disturbance variances is distributed according to the *F* distribution with (n - k) degrees of freedom. This test is a variant of the Goldfeld-Quandt test in that no middle observations are dropped because there is no ranking of the residuals in this context.

IV. Results

Four models are specified and estimated. Model (1) is the decomposition model defined by equation (5). The coefficients for the majority and minority samples are estimated independently in Models (2) and (3). In Model (4), the data is pooled again but the interaction terms of the decomposition equation are not included. As the decomposition model implies, the difference between the estimated coefficients of Models (2) and (3) are simply the coefficients of the interaction terms in Model (1), and the coefficients of Model (2) are equal to the first *k* coefficients of Model (1). The results from the least squares estimation of the decomposition regression and the other specifications are summarized in Table 2.

The overall fit of the decomposition model (and the other regressions) is exceptionally good for cross sectional data, having an adjusted R^2 of 0.89. Thus, the model explains about 90 percent of the variation in station revenues for this sample. All of the (first *k*, non-interacted) explanatory variables are of expected sign, and most are statistically significant. The RESET and White F-statistics are 0.31 and 1.06, respectively. Both these statistics are far below their critical values, so that the respective null hypotheses of 'no specification error' and 'homoskedastic disturbances' cannot be rejected at standard levels.

The explanatory variables that are not interacted with *M* measure the endowment effect. As expected, a higher ratings share (*RATE*) is associated with higher station revenues. A one percentage-point increase in *RATE* produces a 26 percent increase in revenues.[25] Note that the interaction term $M \times RATE$ is not statistically significant, contradicting Ofori's (1999) claim that the ratings of minority owned stations are discounted relative to their majority owned counterparts. The station class dummy variables also are highly statistically significant. For AM stations that must reduce power at night, those that reduce power by less earn more revenue (*PWRAT*). For this sample of stations, we do not find that stations operated by a large group owner fare better than other stations (*NUMSTA*). Market size (*MREV*) has a positive effect on station revenues, but this effect increases at a decreasing rate ($MREV^2 < 0$).

As noted earlier, it is the residual effect, not the endowment effect, which is intended to measure discrimination in the radio industry. Note that none of the interaction terms in Model (1) are statistically significant, and neither is the minority dummy variable (*M*). To test for discrimination, however, we need to jointly test the significance of all the interaction terms. Conducting the statistical test described in equation (6), we calculate the *F*-statistic for the residual effect to be 0.27 (probability level 0.98), which is far below the critical value of 1.97, so that we cannot reject the null hypothesis that the residual effect is

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[25] At the sample mean of *RATE*, this one percentage-point change is roughly a 33 percent increase in rating share.

zero. In other words, the decomposition model estimated here does not support the claim of systematic discrimination in the radio advertising industry.[26]

We can also evaluate the two components of the residual difference separately. Again applying the F-test of equation (7), but leaving out the constant term from the set of coefficient restrictions, we cannot reject the null hypothesis that the coefficient effect is zero ($F^* = 0.30$). Similarly, we are unable to reject the hypothesis that the constant effect is zero ($F^* = 0.21$).

The results from the decomposition model and the fact that the disturbance variances across the samples are equal imply that Model (4) can be estimated without concern. There is no reason, at least for our sample, to treat minority ownership as a contributing factor to the revenue of a station. While the coefficients between Models (2) and (3) differ slightly, they are not statistically different. While we could test for coefficient equivalence across the models, the results from that approach would not differ from our findings.

V. Conclusion

The issue of discrimination has been the focus of empirical studies for many years. Borrowing an empirical model from this literature, the Blinder decomposition model, we test for discrimination against minority-owned radio stations in radio advertising markets. We find no evidence of discrimination against minority-owned stations. These findings contradict the assertions of a recent FCC study that (cautiously) concludes there is widespread and systematic discrimination against minorities in the radio advertising industry. While our findings are contrary to the findings of the FCC study, our failure to find supporting evidence does not prove that discrimination is absent. However, since Becker (1971) shows that discrimination diminishes the welfare of both the majority and minority, and because discrimination is inconsistent with profit maximization in competitive markets, the burden of proof rests on those who claim this non-maximizing activity exists.

Our findings also contribute to the legal analysis of minority preferences in radio broadcasting. Under the *Adarand* strict-scrutiny standard, where comprehensive statistical analysis is required to support minority policies, minority preferences in broadcasting based on revenue handicaps is not supported by our findings. We fail to find any support for the influence of discrimination on station revenues. Our approach, however, is limited to a narrow question – do minority-owned stations have less revenues simply because they are minority owned. If discrimination were present, but not operating in such a way as to affect revenues, then we would fail to find evidence of that discrimination. For example, the FCC study also claims that minority-formatted stations, irrespective of the ethnicity of ownership, make less revenue per listener than do stations formatted to a

[26] Of course, this finding does not prove discrimination is absent entirely.

more general audience. We do not address this issue in our analysis. Clearly, further empirical analysis of this topic is warranted if minority preferences in radio broadcasting are to continue.

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Table 1. Definitions and Descriptive Statistics				
Variable	*Definition*	*Mean*		
		(Standard Deviation)		
		Pooled	Majority	Minority
STAREV	Annual station revenues (000)	3,521 (5,218)	4,307 (5,894)	2,529 (4,049)
FMA	FM Class A Station	0.209 (0.408)	0.139 (0.348)	0.298 (0.462)
FMB	FM Class B Station	0.194 (0.397)	0.194 (0.398)	0.193 (0.398)
FMC	FM Class C Station	0.256 (0.438)	0.347 (0.479)	0.140 (0.350)
PWRAT	Ratio of night to day broadcast power	0.169 (0.349)	0.189 (0.377)	0.144 (0.313)
MREV	Total radio advertising revenue for the market	135,322 (161,977)	164,256 (168,436)	98,773 (146,845)
$MREV^2$	MREV squared	4.4+10 (8.6+10)	5.5+10 (9.3+10)	3.1+10 (7.6+10)
NUMSTA	Total stations operated by the owner of station	20.527 (26.197)	32.15 (30.18)	5.842 (4.511)
RATE	Rating share of the station.	0.032 (0.028)	0.032 (0.029)	0.032 (0.027)
IMR	Inverse Mills Ratio	0.713 (0.498)	0.932 (0.347)	0.136 (0.289)
M	Dummy variable indicating minority ownership	0.442 (0.499)	0.00	1.00
	Observations	129	72	57

Table 2. Results and Descriptive Statistics (t-statistics in parentheses)

Variable	*Pooled*	*Majority*	*Minority*	*Pooled*	*Mean*	*St. Dev.*
C	4.602 (18.71)[a]	4.602 (17.99)[a]	4.743 (27.96)[a]	4.686 (36.59)[a]	…	…
FMA	0.451 (1.86)[b]	0.451 (1.79)[b]	0.191 (1.05)	0.344 (2.47)[a]	0.209	0.408
FMB	0.726 (3.05)[a]	0.726 (2.94)[a]	0.590 (2.75)[a]	0.661 (4.26)[a]	0.194	0.397
FMC	0.482 (2.44)[a]	0.482 (2.35)[a]	0.067 (0.27)	0.338 (2.37)[a]	0.256	0.438
PWRAT	0.421 (1.84)[b]	0.421 (1.77)[b]	0.002 (0.01)	0.265 (1.66)[b]	0.169	0.349
MREV	1.5-05 (10.44)[a]	1.5-05 (10.04)[a]	1.5-05 (9.66)[a]	1.6-05 (15.11)[a]	135,322	161,977
$MREV^2$	-1.9-11 (7.28)[a]	-1.9-11 (7.00)[a]	-1.9-11 (6.52)[a]	-1.9-11 (10.38)[a]	4.43+10	8.63+10
NUMSTA	-1.9-04 (0.09)	-1.9-04 (0.09)	-0.019 (1.19)	-8.9-04 (0.52)	20.527	26.197
RATE	26.456 (11.35)[a]	26.456 (10.91)[a]	28.952 (10.52)[a]	26.942 (16.09)[a]	0.032	0.028
IMR	0.106 (0.49)	0.106 (0.47)	0.469 (1.77)[b]	0.146 (1.09)	0.713	0.498
M	0.140 (0.46)	…	…	…	0.442	0.499
$M \times FMA$	-0.260 (0.84)	…	…	…	…	…
$M \times FMB$	-0.136 (0.41)	…	…	…	…	…
$M \times FMC$	-0.415 (1.27)	…	…	…	…	…
$M \times PWRAT$	-0.419 (1.21)	…	…	…	…	…
$M \times MREV$	1.4-08 (0.01)	…	…	…	…	…
$M \times MREV^2$	-2.2-13 (0.06)	…	…	…	…	…
$M \times NUMSTA$	-0.019 (1.11)	…	…	…	…	…
$M \times RATE$	2.497 (0.67)	…	…	…	…	…
$M \times IMR$	0.363 (1.03)	…	…	…	…	…
Adjusted R-squared	0.89	0.90	0.90	0.90	…	…
White F-statistic	1.02	1.17	1.12	0.98	…	…
RESET F-statistic	0.31	1.02	0.79	0.62	…	…
$\mathbf{b}^B = \mathbf{b}^A$	0.44	…	…	…	…	…
Observations	129	72	57	129	129	129

[a] Statistically significant at the 5 percent level or better.
[b] Statistically significant at the 10 percent level or better.