Effect of neighborhood stigma on economic transactions

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The hypothesis of neighborhood stigma predicts that individuals who reside in areas known for high crime, poverty, disorder, and/or racial isolation embody the negative characteristics attributed to their communities and experience suspicion and mistrust in their interactions with strangers. This article provides an experimental test of whether neighborhood stigma affects individuals in one domain of social life: economic transactions. To evaluate the neighborhood stigma hypothesis, this study adopts an audit design in a locally organized, online classified market, using advertisements for used iPhones and randomly manipulating the neighborhood of the seller. The primary outcome under study is the number of responses generated by sellers from disadvantaged relative to advantaged neighborhoods. Advertisements from disadvantaged neighborhoods received significantly fewer responses than advertisements from advantaged neighborhoods. Results provide robust evidence that individuals from disadvantaged neighborhoods bear a stigma that influences their prospects in economic exchanges. The stigma is greater for advertisements originating from disadvantaged neighborhoods where the majority of residents are black. This evidence reveals that residence in a disadvantaged neighborhood not only affects individuals through mechanisms involving economic resources, institutional quality, and social networks but also affects residents through the perceptions of others.

neighborhoods | stigma | discrimination | transactions

Cities in the United States are characterized by high levels of racial segregation and by concentrated pockets of poverty and of affluence (1, 2). The stratification of American neighborhoods means that individuals living in disadvantaged communities are exposed to fewer economic opportunities, lower quality institutions, greater levels of crime and environmental pollution, and less advantaged social networks (3–6). However, extreme neighborhood inequality may also affect individuals through processes of association, perception, and stigma.

The hypothesis of neighborhood stigma predicts that individuals who reside in areas known for high crime, poverty, disorder, and/or racial isolation embody the negative characteristics attributed to their communities, and experience suspicion and mistrust in their interactions with strangers when their neighborhood of residence is revealed (7–11). Similar to other forms of stereotype, the consequences of neighborhood stigma arise when negative perceptions of a place are attached to individuals, leading to systematic disapproval, discrimination, and/or exclusion (12, 13). Assumptions about residents from disadvantaged neighborhoods could have consequences in the form of lost job opportunities, suspicion by law enforcement, or mistrust in market transactions. Through all of these pathways, the stigma of place may be an important mechanism through which neighborhood segregation reinforces social inequality (5, 14–19). Despite the strong theoretical support for this concept, no previous studies have estimated the effects of neighborhood stigma. This article provides an experimental test of how neighborhood stigma affects individuals in one domain of social life: economic transactions.

To evaluate the neighborhood stigma hypothesis, this study adopts an audit design in a locally organized, online classified market, using advertisements for used iPhones and randomly manipulating the stated neighborhood of origin of the seller. The primary outcome under study is the number of responses generated by sellers from disadvantaged relative to advantaged communities. This approach assesses the effect of neighborhood stigma in a real-life setting instead of relying on stated perceptions of different communities under survey conditions (20, 21). By focusing on aggregated rates of responses to items posted for sale, the study design avoids making inferences about individual discriminatory attitudes or intentions, and captures instead the full penalty of neighborhood stigma as experienced by individuals within disadvantaged neighborhoods. Importantly, the effect of neighborhood stigma encompasses both assumptions about the individual and the community from which he or she originates. These assumptions may pertain to the race or ethnicity of the individual, the economic status of the individual, the potential criminality of the individual, or some other dimension of the community that is attached to the individual seller. Although these potential assumptions are not parsed in this study, the design allows for a causal estimate of the total impact of neighborhood stigma, arising from any and all aspects of the community, on economic interactions. In this sense, the design captures, in its purest form, the full effect of neighborhood stigma as reflected in community names.

Randomized Audit Design

A randomized audit study, or experimental field study, allows the researcher to observe actual market behavior across a predetermined range of variables under controlled conditions. This approach provides a more realistic test of neighborhood stigma

Significance

Although previously theorized, virtually no rigorous empirical evidence has demonstrated an impact of neighborhood stigma on individual outcomes. To test for the effects of neighborhood stigma on economic transactions, an experimental audit of an online classified market was conducted in 2013–2014. In this market, advertisements were placed for used iPhones in which the neighborhood of the seller was randomly manipulated. Advertisements identifying the seller as a resident of a disadvantaged neighborhood received significantly fewer responses than advertisements identifying the seller as a resident of an advantaged neighborhood. The results provide strong evidence for an effect of neighborhood stigma on economic transactions, suggesting that individuals carry the stigma of their neighborhood with them as they take part in economic exchanges.

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than survey methods because it observes what people do as opposed to what they say (22). Audits have previously been used in research on discrimination in service provision, housing, and job applications (23–26). Recent analyses have studied racial discrimination in online markets (27–31), but the method has not yet been applied to test for the effects of neighborhood stigma in economic transactions.

The experiment in this study entailed posting advertisements on an active online market using titles and texts that reflect common advertisements for used iPhone 5's (Apple, Inc.), listed at competitive prices in 12 different large urban markets throughout the United States. The iPhone 5 was selected because it is a well-known product with an active online secondary market. The cities were selected to represent a geographically diverse set of large cities across the country featuring communities with high levels of disadvantage and unique racial and ethnic profiles.

Each posted advertisement revealed a seller's neighborhood of residence, which was experimentally manipulated to represent communities that provide stark variation in the level of disadvantage. Disadvantaged and advantaged communities were identified by aggregating tract-level census data on racial composition and poverty to Zillow neighborhood boundaries (www.zillow.com), which define neighborhoods in US cities by name. Zillow neighborhood names were cross-referenced with frequency of search results in news articles on LexisNexis to confirm that they are commonly used neighborhood names and that the selected neighborhoods are generally portrayed as either advantaged or disadvantaged.

The particular online market was selected because sellers commonly indicate their neighborhood of residence in their classified advertisements (additional information on sellers revealing their geographic location is provided in *SI Text*, section 3.2). The colloquial use and validity of the chosen neighborhood names in each city were verified with searches for advertisements posted by other sellers in the same online marketplace that also used the same neighborhood names.

To account for any potential effect of proximity and local market conditions on response variation, the experiment included two proposed meeting locations: one in the buyer's neighborhood of choice and a second in a central meeting location roughly equidistant to the advantaged and disadvantaged communities. By directly proposing a meeting location in the advertisement that is independent of the seller's neighborhood of residence, the experiment ensured that any effects of neighborhood disadvantage were not attributable to the extra distance that would have to be traveled by potential buyers or by variable levels of consumer demand in the advertising seller's neighborhood. Each advertisement included a randomized combination of text indicating the seller's neighborhood, the suggested meeting location, the locally adjusted price, and one of several equivalent versions of posting language used to convey identical information about the product.

Results

From October 2013 to April 2014, 664 advertisements were posted for iPhone 5's on the online market, 49 (7.38%) of which were flagged and removed by the administrators of the market or other users. The analysis was restricted to the posts that were not flagged, because flagging typically occurred within 1 h of the post submission. (Advertisements might be flagged by other sellers seeking to thin the local market, by the online market's site administrators, or by other users for a variety of reasons, including suspicion of a scam or repetitious posting of the same advertisement. Results were substantively the same when flagged posts were included. Only five flagged posts received any responses.) The analysis sample of nonflagged posts generated an average of 3.72 responses within 60 h of posting. (No differences in results were found in models assessing the effect of neighborhood disadvantage on the number of posts within 12, 24, or 60 h or when there was no time limit.) Fig. 1 shows that the large majority (75%) of posts generated five or fewer responses, with a small minority generating 15 or more responses. One hundred three posts (15.51%) did not generate any responses.

Fig. 2 shows the average number of responses within 60 h of posting, grouped by city and by neighborhood disadvantage. In all but three cities (Los Angeles, NY Manhattan, and Seattle), posts from disadvantaged neighborhoods received fewer responses on average. However, the samples within cities were small, and the difference in average responses between advantaged and disadvantaged communities was statistically significant only in Atlanta, where posts from sellers in disadvantaged neighborhoods received fewer than half as many responses as posts from sellers in advantaged neighborhoods ($P < 0.01$).

Table 1 presents results from negative binomial models estimating the number of responses received within 60 h as a function of neighborhood disadvantage. All models included city fixed effects and controls for variation in post details and market characteristics (full results are shown in *SI Text*). Results are displayed as incident rate ratios (i.e., exponentiated coefficients). Model 1 revealed that, controlling for all other factors, posts from sellers in disadvantaged neighborhoods received ∼83.9% as many responses as posts from sellers in advantaged neighborhoods ($P < 0.001$). Model 2 assessed whether the effect of neighborhood disadvantage varies depending on the proposed meeting location, and found no statistically significant interaction between neighborhood disadvantage and the proposed meeting location.

Models 3 and 4 estimated the effect of neighborhood disadvantage in neighborhoods that are predominantly African American, limiting the sample to include only the cities where the selected disadvantaged neighborhood was majority-black (Atlanta; Baltimore; Boston; Chicago; Los Angeles; NY Brooklyn; NY Manhattan; Philadelphia; Seattle; and Washington, DC). Results in model 3 showed that neighborhood disadvantage reduced the number of responses by ∼21% when the disadvantaged community was majority-black, and model 4 indicated that this result did not vary depending on the proposed meeting location. Models 5 and 6 showed the same results for cities where the disadvantaged community was majority-Latino (Philadelphia, Phoenix, and San Antonio), and found no effect of neighborhood disadvantage. Notably, estimates from models 3 through 6 were substantively similar when estimated for the full sample ($n = 615$). Additional estimates (see *SI Text*) show that the main findings hold even after excluding the city with the largest disparity

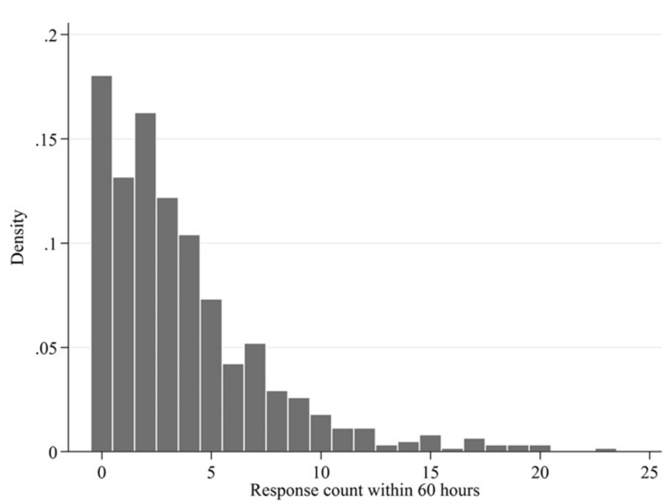

Fig. 1. Distribution of the number of responses to posts within 60 h of posting.

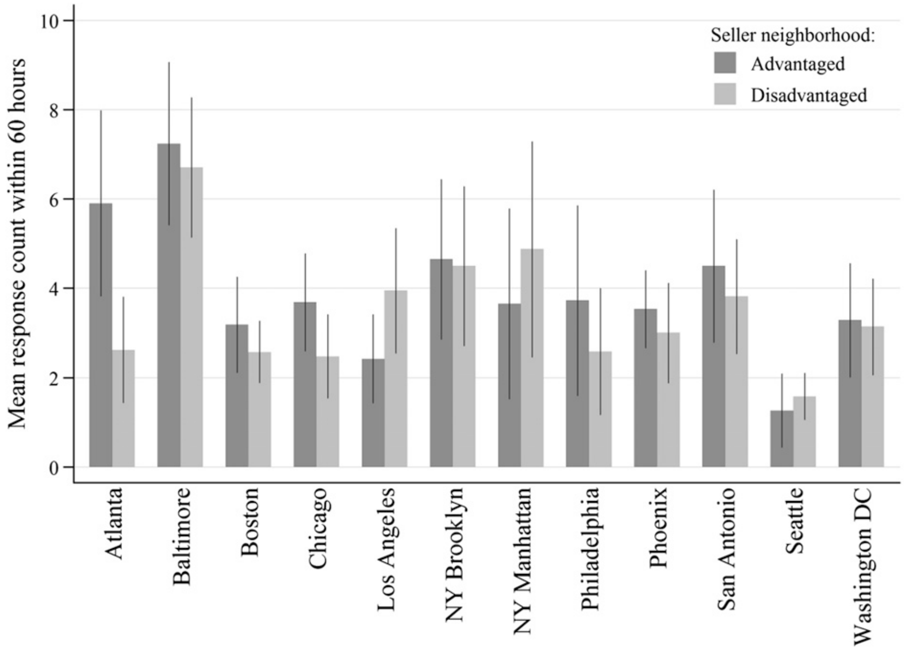

Fig. 2. Average number of responses within 60 h of posting by city and neighborhood disadvantage.

between advantaged and disadvantaged neighborhoods (Atlanta) from the sample.

To assess whether neighborhood stigma affected other aspects of the economic interaction, the same models were estimated using alternative outcome measures. No statistically significant effects were found in models predicting whether a post was flagged; whether a post received any responses at all; the mean, minimum, or maximum price counteroffers; or the proportion of responses with direct price counteroffers.

Discussion

In spatially differentiated cities marked by racial and socioeconomic segregation, neighborhoods come to be known and recognized in terms of their institutions, the level of crime and disorder, their appearance, and their population characteristics (4). All of these characteristics of neighborhoods may influence the life chances of residents directly, but the stigma attached to the name of the community itself also may affect the daily experiences of residents. Processes of selection and sorting make it difficult to identify the effect of neighborhood conditions or neighborhood stigma through traditional observational methods, creating the need for new methods to understand the full set of consequences of neighborhood stratification. The randomized audit design used in this study is one approach that provides leverage to overcome the problem of selection bias and to assess the role of neighborhood stigma arising from a community's name on economic transactions in an open, online marketplace.

This audit study provided experimental evidence that individuals from disadvantaged neighborhoods bear a stigma that influences their prospects in potential economic exchanges. The effect of neighborhood stigma varied across the 12 different geographical markets, but the pooled estimate strongly supports the neighborhood stigma hypothesis. Specifically, advertisements from disadvantaged neighborhoods received ~16% fewer responses than those advertisements claiming to be from advantaged neighborhoods. This disparity was greater (~21% fewer responses) for black, disadvantaged neighborhoods, and it was not present in disadvantaged neighborhoods that were majority-Latino. These findings are descriptive. One interpretation is that the stigma of neighborhood disadvantage is dependent on the racial composition of the neighborhood. Another interpretation, however, is that the effects of neighborhood disadvantage may be amplified by other features of the neighborhood that could be correlated with racial composition,

Table 1. Negative binomial model estimates of the effect of neighborhood disadvantage on number of responses to posted advertisements within 60 h

	Full sample (n = 615)		Sample of cities with black neighborhoods (n = 462)		Sample of cities with Latino neighborhoods (n = 204)	
Treatment definition	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Disadvantaged neighborhood	0.839*** (0.044)	0.892 (0.066)				
Disadvantaged neighborhood × central location		0.885 (0.092)				
Black disadvantaged neighborhood			0.794*** (0.050)	0.831* (0.072)		
Black neighborhood × central location				0.912 (0.113)		
Latino disadvantaged neighborhood					0.981 (0.097)	1.088 (0.153)
Latino neighborhood × central location						0.809 (0.168)
Central meeting location	0.937 (0.048)	0.991 (0.069)	0.977 (0.060)	1.016 (0.082)	0.894 (0.091)	0.979 (0.132)

This table displays exponentiated coefficients with SEs in parentheses. *$P < 0.05$; ***$P < 0.001$ (two-tailed tests).

Table 2. Neighborhood advantage and disadvantage, by city

City	Neighborhood	Classification	Poverty rate,* %	Selected racial composition,* %	Observations†
Atlanta	Midtown	Advantaged	9.1	70.2 white	32
	Oakland City	Disadvantaged black	35.4	87.5 black	25
Baltimore	Canton	Advantaged	11.8	75.4 white	27
	West Baltimore	Disadvantaged black	37.9	83.7 black	27
Boston	Back Bay	Advantaged	9.7	86.0 white	26
	Dorchester	Disadvantaged black	18.8	45.8 black	28
Chicago	Lincoln Park	Advantaged	11.6	82.5 white	34
	North Lawndale	Disadvantaged black	41.8	91.7 black	23
Los Angeles	Century City	Advantaged	9.7	76.8 white	28
	Crenshaw	Disadvantaged black	25.3	68.9 black	24
NY Brooklyn	Cobble Hill	Advantaged	4.3	71.2 white	28
	Bedford-Stuyvesant	Disadvantaged black	29.6	77.3 black	28
NY Manhattan	Upper East Side	Advantaged	6.0	81.2 white	28
	East Harlem	Disadvantaged Latino	35.5	56.6 Latino	24
Philadelphia	Fox Chase	Advantaged	8.9	78.9 white	25
	Nicetown	Disadvantaged black	32.2	93.8 black	14
	Juniata	Disadvantaged Latino	39.3	52.1 Latino	18
Phoenix	Ahwatukee Foothills	Advantaged	6.1	73.3 white	32
	Central City	Disadvantaged Latino	44.2	64.4 Latino	23
San Antonio	North Central	Advantaged	3.8	74.0 white	29
	Southwest San Antonio	Disadvantaged Latino	38.8	92.2 Latino	28
Seattle	Madrona	Advantaged	4.4	74.8 white	17
	Leschi	Disadvantaged black	18.1	36.2 black	27
	International District	Disadvantaged Asian	43.1	49.0 Asian	12
Washington, DC	Dupont Circle	Advantaged	11.1	73.6 white	28
	Anacostia	Disadvantaged black	31.6	97.1 black	29

*Source: Authors' compilation, derived from Zillow neighborhood boundaries and aggregated 2007–2011 American Community Survey tract-level data.
†Forty-nine posts were flagged for removal, and were not included in the main regression analysis in Table 1.

such as the degree of concentrated poverty, the prevalence of public housing, or the crime rate. The interaction between neighborhood disadvantage and racial/ethnic composition warrants additional research designed specifically to disentangle the effects of racial composition and concentrated disadvantage.

The online classified marketplace provided an ideal case for testing neighborhood stigma because buyers with limited information were forced to discriminate between advertisements based not only on the price, convenience, and quality of the product but also on their willingness to transact with each potential seller in the market. Sellers from disadvantaged neighborhoods may have attracted fewer responses because buyers used residence to infer the seller's race or ethnicity, economic status, trustworthiness, or dependability. The total effect measured here invites future research to evaluate these potential underlying dimensions of discrimination and to understand whether neighborhood stigma operates in addition to these attributes or if it serves as a proxy in lieu of more information about seller characteristics. Further research might also investigate whether market actors are aware of neighborhood stigma and, if so, whether they use management strategies to minimize its negative effects. Finally, the results presented here justify additional empirical inquiry into the salience of neighborhood stigma in other social arenas where initial perceptions matter and where individuals must signal their neighborhood of origin, such as in employment application screening, mate selection, credit applications, and judicial processing.

Evidence for the effect of neighborhood stigma reveals that residence in a disadvantaged neighborhood not only affects individuals through mechanisms involving economic resources, institutional quality, and social networks but also affects residents through the perceptions of others. Individuals embody the characteristics of their communities, with tangible consequences when they enter the marketplace. In this way, the stigma of place represents an important, and frequently overlooked, byproduct of residential segregation.

Materials and Methods

The experiment for this study was conducted on one of the largest online classified markets in the country. The market provided multiple advantages for deploying an experimental audit. First, listings were posted at the citywide level, allowing them to be searched by anyone looking for a particular product in a given city. Second, it is common for sellers to mention their location or preferred site of transaction in their advertisements; this fact ensures that the tests of neighborhood stigma do not introduce an artificial signal. Third, the website permitted advertisers to control the content, including the advertisement's title, text, and price listing of any item. Control over advertisement titles and content allowed for the posting of similar advertisements revealing only the iPhone 5 specifications, the price, and the location without revealing any other characteristics of the seller.

Cities were selected using multiple criteria. First, cities were chosen for geographical spread across the United States. Second, cities with large secondhand iPhone 5 markets were selected (note that the boroughs of NY Manhattan and NY Brooklyn were considered independent markets). Because the price of an iPhone 5 varies across cities and over time, the median price within each city's iPhone 5 market was calculated on the first of every month in all 12 cities and advertisements were adjusted accordingly. Third, cities with geographically proximal advantaged and disadvantaged neighborhoods were used. Neighborhoods were operationalized as geographic subareas within each city that had a recognizable name. Zillow neighborhood boundaries with names and aggregated census tract data were used to guide neighborhood selection. All neighborhoods (aggregated census tracts) had a population of at least 5,000 residents. Zillow neighborhood names were verified by searching local newspaper articles. Any neighborhood names that did not come up repeatedly when searching in local newspaper archives were ruled out. Each of these neighborhood names was additionally cross-referenced by searching the local markets where posting would occur (further details are provided in *SI Text*, section 3.2). The neighborhoods for each city in the study are identified in Table 2.

Advantaged and disadvantaged neighborhoods were defined based on a combination of concentrated white vs. minority populations and low vs. high poverty rates. Calculations were made from the 2007–2011 American

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Community Survey (32). Because levels of racial segregation and poverty differ across cities, the thresholds that were used were based on the distribution of neighborhoods within each city. Specifically, advantaged neighborhoods had a poverty rate at the low end of the poverty distribution within each city. The poverty rate for advantaged neighborhoods ranged from 4.3–11.8%. Alternately, disadvantaged neighborhoods had high poverty rates within their respective cities, ranging from 25.3–43.1%. Because economic disadvantage is consistently conflated with race in US neighborhoods (33, 34), neighborhoods with a high concentration of black, Latino, or Asian residents were selected. In fact, the neighborhood with the highest poverty in each city was black or Latino, with the exception of NY Brooklyn. Although all selected disadvantaged neighborhoods are majority nonwhite, it was difficult to identify specific black-majority neighborhoods in Boston and Seattle that also met the poverty and distance criteria necessary for the study; in those cities, neighborhoods with a high black resident composition relative to other neighborhoods in the same cities were chosen. In Phoenix, San Antonio, NY Manhattan, and Philadelphia, disadvantaged Latino neighborhoods ranging from 52.1–96.6% Latino were also selected, as was a disadvantaged Asian neighborhood (49.0% Asian) in Seattle.

Once appropriate neighborhoods were located and prices were calculated, two advertisements per week in each city were posted. All advertisements contained the same information in the title and text; however, these features were varied superficially to avoid detection by return buyers. The advertisements randomly varied both whether the seller originated from an advantaged or disadvantaged neighborhood and also the proposed meeting location (i.e., either in the buyer's neighborhood of choice or at a central location). The central meeting locations in each city were identified as heavily trafficked commercial centers where two individuals might reasonably meet to conduct an iPhone 5 sales transaction (most often "downtown") and that reflected local nomenclature (in Philadelphia, for example, this location is called "Center City" instead of downtown).

Advertisements were posted between October 2013 and April 2014 at noon local time, and all email responses from buyers were collected, coded, and linked to each original post.

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Supporting Information

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SI Text

1. Modeling Strategy

The primary outcome of interest is the total number of responses each posted advertisement received. In the rare cases where the same individual sent more than one inquiry about the same advertisement, only the first message was counted because the key outcome of interest is meant to capture the rate of unique potential buyers (transactional opportunities), not the gross rate of messages received. The response count to email replies received was limited to those email replies received within 60 h of posting an advertisement. The shortest time window of all posts began on Monday at 3:30 PM and ended on Thursday at 11:00 AM, which is 67.5 h of exposure. The vast majority (almost 95%) of responses were received within 60 h. This count variable was overdispersed (mean = 3.72, variance = 14.41), and there were a large number of zeros (16.75% of posts received no responses), so a negative binomial distribution was used, which includes a parameter reflecting unobserved heterogeneity across observations. Models using a Poisson distribution were qualitatively the same. The Stata-based evaluative tool "countfit" (1) also supported the decision to use negative binomial modeling. Both Akaike's Information Criterion and Bayesian Information Criterion values were lower (i.e., indicators of better model fit) for the negative binomial than for Poisson, zero-inflated Poisson, or zero-inflated negative binomial models (1).

The main explanatory variable was whether the neighborhood signaled in the advertisement text is disadvantaged or advantaged. This dummy variable was coded 0 for advantaged neighborhoods and 1 for disadvantaged neighborhoods. Because all aspects of the advertisements were randomized (instead of creating matched pairs of advertisements by neighborhood advantage and disadvantage), there is some imbalance in post content and timing across cities and neighborhoods within cities. Table S1 displays the mean values for post characteristics, local market characteristics, and post timing by city and neighborhood. The statistical models estimating the number of received responses contain controls for the advertisement's characteristics and the circumstances under which it was posted to the online market. Dummy variables were created for each of the 10 post title variations (below), the 10 advertisement text variations (below), the three possible ways neighborhood could be signaled, and the two potential meeting locations. Controls were created for the listed price and price squared. To capture variation across post time, models also included dummy variables for the day of the week and year. A dummy variable for posts submitted to the market in the week of a national holiday was created in the event that responses were sensitive to holidays (Columbus Day; Veteran's Day; Thanksgiving; Christmas; New Year's Day; Martin Luther King, Jr., Day; and President's Day).

City fixed effects were used to control for unique, unchanging aspects of each local market. Controls for the total number of advertisements for iPhone 5's within each market and the median price (below) posted in those advertisements, measured at the first of every month, were also created to account for how the market for used iPhone 5's changed within each city across time. Finally, adjustments for period changes in iPhone sales were made by including a variable that measures, for each post, the cumulative number of posts made within each city up to that point and a squared version of that variable.

A number of slightly modified specifications were used to investigate nuance in the relationship between neighborhood disadvantage and number of responses. Neighborhood disadvantage was interacted with the meeting location suggested in the post advertisement to identify whether potential buyers are more or less willing to meet with a seller from a disadvantaged neighborhood in their own neighborhood. Dummy variables for black disadvantaged neighborhoods and Latino disadvantaged neighborhoods were used instead of using the pooled neighborhood disadvantage dummy variable to estimate the difference in number of responses for each. The coefficients of interest are presented in Table 1, and the full results of these models are presented in Table S2.

In supplementary analyses, neighborhood disadvantage was tested for its effect on whether or not a post was flagged, whether or not a post received at least one response, the proportion of responses with counteroffers, the mean counteroffer amount, the minimum counteroffer amount, and the maximum counteroffer amount. Binary outcomes were analyzed with logistic regression models with city fixed effects, whereas linear outcomes were estimated with ordinary least squares with city fixed effects. The models showed no statistically significant effect of neighborhood disadvantage across any of these alternative outcomes. Results are presented in Table S3.

To test whether the main findings were driven by the city with the largest disparity in responses between advantaged and disadvantaged neighborhoods, the same models as above were estimated, excluding Atlanta. The effect of neighborhood disadvantage is smaller but remains marginally significant in this smaller sample that does not include Atlanta (Table S4). A set of additional analyses was conducted to assess how sensitive the results are to the exclusion of other outlier cities where the effect of neighborhood disadvantage was particularly large or small. The sensitivity of results to outlier cities and to model specification is explored further in Table S5. In this table, we conduct several analyses using four different samples: a sample with all cities pooled, a sample excluding Atlanta, a sample excluding the two cities with the most extreme difference between advantaged and disadvantaged neighborhoods (Atlanta and Los Angeles), and a sample excluding the four cities with the most extreme difference between advantaged and disadvantaged neighborhoods (Atlanta, Chicago, Los Angeles, and NY Manhattan).

Column 2 of Table S5 begins by comparing the mean number of responses in advantaged and disadvantaged neighborhoods. As demonstrated in the table, the raw difference in responses is not significant when excluding Atlanta. When all cities are pooled, and when four most extreme cities are excluded, the difference in mean number of responses is statistically significant. Similar results are obtained when we use the negative binomial specification to model the number of responses but without any control variables included (column 3 in Table S5).

The fourth and fifth columns in Table S5 estimate the effect of neighborhood disadvantage on all four samples while controlling for characteristics of posts that are included in the main specifications from the text as well as a dummy variable for each site, excluding Atlanta. The main difference between these models and the models that do not adjust for post characteristics is that the SEs are smaller, as expected, after adjusting for random variation in characteristics of the advertisements, such as the price offered, when the advertisement was posted, and so forth. Column 4 of Table S5 uses linear regression to estimate treatment effect and finds that once simple controls and city fixed effects are added to the model, effect estimates vary slightly but are robust when excluding Atlanta. In column 5, which uses the preferred negative binomial model with city fixed effects, the effect of neighborhood disadvantage is at least marginally significant in all four models. The magnitude of the estimated effects does not vary substantially across any of these models or subsamples, but the estimates are more precise once controls are included.

Why does including controls affect the precision of the estimates? As shown in Table S1, there is some imbalance in the average characteristics of posts from advantaged and disadvantaged neighborhoods. In Atlanta, for instance, 63% of posts from disadvantaged neighborhoods were randomly assigned the higher experimental price (higher price = 0.63), whereas only 40% of posts from advantaged neighborhoods were assigned the higher experimental price (higher price = 0.40). Additionally, 46% of posts from disadvantaged neighborhoods suggested meeting at a central location (central meeting location = 0.46) compared with 60% of posts from advantaged neighborhoods (central meeting location = 0.60).

This imbalance arises from the method that was used to post advertisements. A typical matched audit study would post identical advertisements at the same time while changing the key variable of interest. However, a 1:1 matched design was not used in this study because of the particular constraints of the live market. To minimize the risk of the market administrators noticing repetitious posting patterns, advertisements were posted twice per week in each city, and all characteristics of all posts were randomly assigned. In this study, the choice not to use 1:1 matching allowed tests of more than one disadvantaged neighborhood in some cities (e.g., Philadelphia, Seattle). Rather than using matched pairs, all posts were randomly assigned to either the treatment or control. The advantage of generalized randomization (rather than matched pairs) is that it allowed the repeated posts to avoid being detected by site administrators. The disadvantage is that there were slight imbalances in some characteristics of posts from advantaged and disadvantaged neighborhoods that were present across the different cities.

An additional model was also estimated to explore treatment effect heterogeneity across sites. A negative binomial estimation model similar to the main effects estimate in model 1 (Table 1) was fit with a two-way interaction term between neighborhood disadvantage and indicator variables for each city in the analysis. Table S6 reports the parameters from the treatment effect heterogeneity model. Rather than testing for differences against a particular city (omitted in the heterogeneity estimation model as a reference category), the coefficients of disadvantaged vs. advantaged posts in Table S6 are compared against the weighted grand mean; that is, the coefficient for neighborhood disadvantage in each city is compared with an adjusted grand mean (constrained at 1). Cities with a coefficient less than 1 are those cities where the predicted number of responses to posts from disadvantaged neighborhoods is lower than the grand mean, and cities with a coefficient greater than 1 are those cities where the predicted number of responses to posts from disadvantaged neighborhoods is greater than the grand mean. The P values reported in Table S6 demonstrate modest evidence of treatment effect heterogeneity. The difference between the predicted number of posts in disadvantaged neighborhoods relative to the grand mean is significant in Atlanta ($P < 0.05$) and NY Manhattan ($P < 0.10$). A joint F test of interaction effects between neighborhood disadvantage and city yields a χ^2 statistic of 15.61 ($P = 0.156$).

Despite evidence of heterogeneity between sites, the main results from Table 1 are not dependent on any one city, and the pooled estimates (as well as the estimates excluding Atlanta in Table S4) support the generalized claim of a significant negative effect of neighborhood stigma.

2. Potential Confounders and Alternatives

2.1. Variation in Local Market Price Across Cities. At the beginning of each month, variation in each city's market for used iPhone 5's was captured. Finding variation entailed measuring the total number of advertisements for iPhone 5's for sale by searching for "iPhone 5" within each specific market. Estimates for the median price for a used iPhone 5 were based on price data for every used iPhone on the first page (the first 50 advertisements) of a within-market search.

In February 2014, the procedure was changed slightly: To exclude a growing number of spam advertisements (i.e., prices well below or above the typical price), only advertisements priced above \$100 or below \$600 were included. Both supply (i.e., advertisements) and demand (i.e., median prices) declined steadily from October 2013 to April 2014.

2.2. Counteroffers. It is possible that signaling neighborhood disadvantage also affects the amount potential buyers are willing to pay for an item. To investigate this possibility, the mean difference between any counteroffer and the posted price among responses to an advertisement, the proportion of responses with a counteroffer, the lowest counteroffer, and the highest counteroffer were all modeled.

2.3. Flagging. On the online market, users may "flag" posts, at which point the advertisement posts are removed from the site and no longer able to attract new potential buyers. Advertisements might be flagged by other sellers seeking to thin the local market; by the online market's administrators; or by other users for a variety of reasons, including suspicion of a scam or repetitious posting of the same advertisement. Flagging was minimized by spacing out the advertisements on Mondays and Thursdays, and by using multiple text prompts (below) that avoid repetition. Despite these efforts, 49 (7.38%) of 664 posts were flagged. A sensitivity analysis confirmed that neighborhood disadvantage did not predict flagging (Table S3), so the sample in the main analysis was limited to only those posts that were not flagged ($n = 615$).

3. Motivations, Methodology, and Sampling Strategy

3.1. Experimental Methodology. Scholarship on cities has generally stressed the importance of residential context in the lives of individuals. The past three decades in particular have produced a large amount of research focused on neighborhood effects, or the particular set of constraints one's neighborhood of residence places on one's life. Increasingly, the neighborhood effects line of research has sought to specify mechanisms that lead to disparate outcomes for individuals across residential contexts. The broader scope of this current study is to measure how residential context stigmatizes market actors and how different residential contexts produce variation in economic transactions.

Online transactions were used because of the anonymous nature of the market, which allows interpersonal factors, which may influence transactional outcomes, to be minimized. The particular website was chosen because it is the largest online marketplace for second-hand goods, with over 60 million unique users per year. In the cities where the experiment was conducted, annual user rates span from around 700,000 (Baltimore) to 3.4 million (New York City).

Over 7 mo, 664 advertisements were posted, which received 2,436 responses. Advertisements put up in October 2013 received the highest number of responses (7.90), whereas the average declined over the remaining months, likely because two updated versions of the iPhone (the iPhone 5c and iPhone 5s) were released in September, leading to a decline in demand for the iPhone 5. Although it is possible this decline reflects that potential buyers in the online market identified repetitious advertisements from the audit study (i.e., they figured out the advertisements were false), the 10 text, 10 title, 2 or 3 neighborhood, and 2 price variations made identification unlikely. Furthermore, the local market data show a marked decline in the demand for the iPhone 5 over time, which is suggestive of a macrolevel trend.

No replies were sent to individuals who inquired about the advertisements, because this method is the least disruptive. Nonresponse is a very common, if not the most common, experience for the online market's users, likely because of anonymity and the large number of queries received by those users who post advertisements. The latter makes it very difficult for sellers in the market to address each individual inquiry adequately, whereas the former means there is little, if any, social incentive to do so. Furthermore, the more time spent communicating about a product, the more attached to that product a respondent could become, which could, in turn, increase the harm incurred if the transaction is not completed. By not responding, the project avoided this risk, and therefore minimized any negative impact of the experimental advertisement.

3.2. Sampling Specifications. Twelve cities were selected for the analysis (NY Brooklyn and NY Manhattan are analyzed separately). At least one city from each census region was included. An oversampling of cities from the Northeast occurred because these cities have the ideal combination of identifiable neighborhoods, density, and central meeting locations that facilitate the ideal conditions for the audit. The structure of a city's online market also played a role in the selection process. In some places (e.g., Miami, Houston), the website did not segment the city geographically, which resulted in too large a market to signal neighborhood effectively. In others (e.g., San Francisco/Oakland), the website is overly segmented, which meant markets were too small with not enough demand. From the 12 selected cities in the analysis, only New York City and Los Angeles have segmented within-city regions (boroughs and telephone area codes, respectively), but the segmented regions still had considerably large markets.

Neighborhoods within cities were identified and selected based on multiple criteria. Aside from their demographic and economic characteristics (described below), neighborhood names were deemed plausible and usable if they were (*i*) used in local newspaper articles and (*ii*) used in the local markets where posting would occur. LexisNexis searches were used to check local news sources and the prevalence of particular neighborhood names. A neighborhood name was deemed usable if it was found in at least four news sources, including newspapers and web-based publications, within the past month. If a neighborhood name met these criteria, additional searches were run that included the neighborhood name and key search terms ("crime," "homicide," "poverty," and "theft") to ensure that local media portrayed selected neighborhoods as advantaged or disadvantaged. Neighborhood names were then checked for usability in the live market. A neighborhood name was deemed usable if it had been included in at least five posts for second-hand cell phones in the past week. This process ensured that all neighborhood names, both advantaged and disadvantaged, met the same base standard of saturation within the online market and that live users did not completely avoid mentioning particular neighborhoods.

Advantaged and disadvantaged neighborhoods are characterized by a combination of concentrated white vs. minority populations and low vs. high poverty rates. Because levels of racial segregation and poverty differ across cities, relative rates within each city were used when identifying appropriate neighborhoods for the study.

Advantaged neighborhoods have a poverty rate at the low end of the poverty distribution within each city. Those neighborhoods selected ranged from a 4.3% poverty rate (Cobble Hill-NY Brooklyn) to an 11.8% poverty rate (Canton-Baltimore). All advantaged neighborhoods had a majority white population. The lowest percentage of a white population among advantaged neighborhoods was in Atlanta (70.2% in Midtown), and ranged up to 86.0% (Boston-Back Bay).

Disadvantaged neighborhoods had a poverty rate at the high end of the poverty distribution within each city. Those selected ranged from a 25.3% poverty rate (Los Angeles-Crenshaw) to a 44.2% poverty rate (Phoenix-Central City). Among high-poverty neighborhoods within each city, neighborhoods with a high concentration of black, Latino, or Asian residents were sought. It was difficult to identify specific black-majority neighborhoods in Boston and Seattle that also met the poverty and distance criteria; in those cities, neighborhoods with a high black resident composition relative to other neighborhoods in those specific cities were chosen. In San Antonio, Phoenix, NY Manhattan, and Philadelphia, we also identified disadvantaged Latino neighborhoods ranging from 52.1% to 96.6% Latino, and in Seattle, we identified a disadvantaged Asian neighborhood (49.0% Asian). Note that given the race constraints in Seattle, the Leschi neighborhood in Seattle is considered a disadvantaged neighborhood even though its poverty rate was only 18.1%; this neighborhood had the highest percentage of a black population in all of Seattle, but it is still very integrated compared with black neighborhoods in other cities.

Distances from chosen signal neighborhoods were approximated by dropping pins near the centroid of each neighborhood/central location on Google maps and measuring driving distances. Neighborhoods and central meeting locations that are approximately an equal distance from one another were selected. Finding appropriate advantaged and disadvantaged neighborhoods that were equal distance from central meeting locations was nearly impossible in some cities, given the additional poverty and race criteria. In addition, driving distance has different practical meanings in cities with higher rates of drivership vs. public transportation ridership and in cities with higher vs. lower population density. Therefore, distance was used as a general check to ensure that any neighborhoods that were unreasonably far from the central meeting location or the other neighborhoods in a particular city were not selected. The signal neighborhoods were generally between 2 and 8 miles from the meeting locations. One outlier was Philadelphia, where the advantaged neighborhood was 13 miles from the central location. Another outlier was San Antonio, where the disadvantaged Latino neighborhood was ~10 miles from the central location.

Evidence gathered during the pilot supported the legitimacy of mentioning seller neighborhood of residence in the advertisements. During the time period in which the experiment took place, it was quite common for sellers in this market to include this information. Neighborhood choices were validated based on the fact that they were mentioned in advertisements from other sellers in the used phone market. Today, sellers in the online market are given the option to include a zip code and/or specific address for their location (this option was not available when the data collection began), which allows buyers to search for products via a map of their city and strongly implies a demand for more locally organized, neighborhood-aware content. The majority (57%) of the newest advertisements on the site (as of September 4, 2014) in each of the 12 cities studied (the most recent 100 posts in all cities but Seattle and Washington, DC, which only had 90 and 54 posts, respectively) currently take advantage of this feature (i.e., they have a mapped location). Fig. S1 shows how common neighborhood identification is across all cities. Of the 43% of advertisements that do not include a specific location, the vast majority include some other geographic marker, typically a neighborhood name.

A power analysis was conducted to determine the number of posts per city required to measure a difference in response rates reliably between advantaged and disadvantaged neighborhoods significant at the $P < 0.05$ level. Using the Optimal Design Software for Multi-Level and Longitudinal Research program (2) and fixing the statistical power threshold at 0.80, a standard level, it was estimated that a neighborhood stigma effect of 0.20 SDs would require a minimum of 25 posts per city (or about 300 total posts) and that a neighborhood effect of 0.15 SD would require a minimum of 40 posts per city (or about 480 total posts). An average of 55 posts per city was made to ensure valid estimates.

4. Audit Specifications, Variations, and Coding

4.1. Title Variations. Titles were developed similar to those titles tested in a preexperiment pilot. Most were simple and forgettable, although a few had special characters and patterns typical in the online marketplace. Advertisements were randomly assigned a title:

i) Black 16GB iPhone 5 (AT&T)
ii) AT&T Black iPhone 5 (16G)
iii) :::///LIKE NEW BLACK IPHONE 5 FOR AT&T///:::
iv) iPhone 5 - 16GB @ Black AT&T//EXCELLENT
v) —Black 5 iPhone - 16GB ATT—
vi) 16GB IPHONE 5 - ATT - BLACK - LIKE NEW!
vii) **LIKE NEW IPHONE 5 FOR AT&T - BLACK**
viii) Black, iPhone 5 (16GB) AT&T–PERFECT
ix) ~@ 16GB Black 5 iPhone for ATT @~
x) iPhone Black 5 16G (AT&T)

4.2. Text Variations. The text within each advertisement was designed to explain quickly and benignly that the iPhone 5 was used, in good condition, and contained all of the components present in a retail box. As with title variations, texts were based on pilot testing and advertisements were each randomly assigned one of 10 text variations:

i) Like new iPhone 5 for sale - just a few months old. Comes with box and all items that were in the box. No scrapes or dents.
ii) iPhone 5 (just 4 months old) for sale with charger, headphones, and box. In perfect shape - no damage.
iii) If you want a good deal on a basically new (no scratches, dents, etc.) iPhone 5, this is it. You'll get all of the things that were in the original box.
iv) I'm selling my 5, which I got a couple months ago and is as good as new. All of the stuff in the original box included.
v) Barely used black iPhone 5 with charger and headset. If interested, email me.
vi) Selling my 4 month old iPhone 5 with original box and everything it came with. No damage.
vii) I have an iPhone 5 with no scratches. Comes with original charger, headphones, and box.
viii) iPhone 5 black good condition used no scratches no dents original box charger and headphones.
ix) I'm selling my black iPhone 5. It's in great condition and includes all of the original items - head phones, box, charger. It's never been dropped or scratched.
x) 4 month old black iPhone 5 for sale. Includes original box, headphones, and charger. Perfect condition, no scratches.

4.3. Location Text Variation. A short sentence after each text variation was added indicating where the advertiser was from and where the transaction would take place:

i) I live in X and can meet Y.
ii) I'm in X. Meet Y.
iii) Meet in X. I live in Y.

4.4. Price Variation. Advertisements included one of two randomly assigned variations of listed price: one $5 above 85% of the local market median price and one $5 below 90% of the local market median price. The median price was captured as described above.

4.5. Posting/Deleting Days/Times. Advertisements were put up twice a week in each local market at noon local time on Mondays and Thursdays. Before posting a new advertisement, the previous advertisement was taken down (if it had not been flagged). Each advertisement was randomly assigned a neighborhood, price, title, and text.

4.6. Coding Responses. Various details of market response were captured from secure email messages sent to the seller email accounts and then automatically forwarded to a single master account. Details from each email message were entered into a spreadsheet using an input form. Information captured included the link of the original advertisement to which the interested buyer is replying, the email address of the potential buyer (anonymized by the online market but consistent for multiple replies by one individual to the same posting), the date and time of the response, and the counteroffer amount (if any).

Throughout the study, a single coder processed all email responses. Once the study was completed, 25% of the responses were randomly selected and recoded to ensure confidence in the quality of coding. Among these responses, there was a 97.4% coding agreement rate for all coded data points, indicating very high interrater reliability.

1. Long JS, Freese J (2006) *Regression Models for Categorical Dependent Variables Using Stata* (Stata Press, College Station, TX).
2. Raudenbush SW, et al. (2011) Optimal Design Software for Multi-Level and Longitudinal Research (William T. Grant Foundation, New York), Version 3.01. Available at sitemaker.umich.edu/group-based/optimal_design_software. Accessed October 1, 2013.

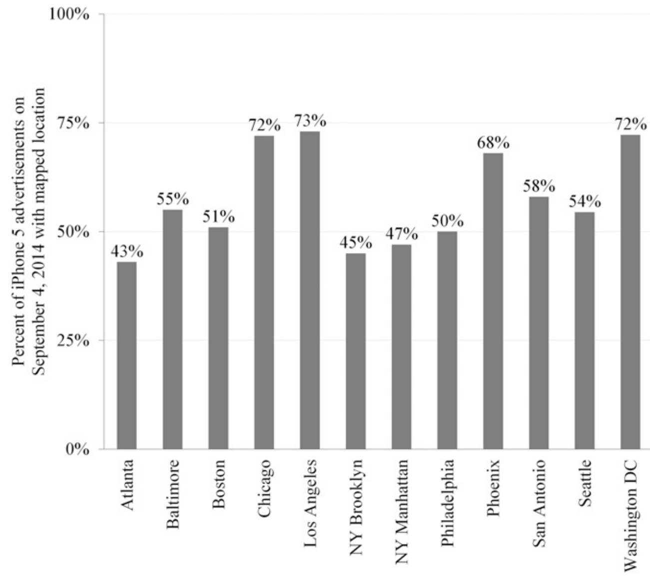

Fig. S1. Percentage of iPhone posts in each of the 12 cities advertised on September 4, 2014, that include specific information about the seller's location.

Table S1. Model covariate means for disadvantaged and advantaged neighborhoods within each city

	Atlanta		Baltimore		Boston		Chicago		Los Angeles		NY Brooklyn		NY Manhattan		Philadelphia		Phoenix		San Antonio		Seattle		Washington, DC	
Characteristics	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.	Adv.	Disadv.
Post variables*																								
Text	5.03	6.25	5.64	4.74	6.14	4.35	5.53	5.04	4.96	6.16	4.65	5.39	5.27	5.92	5.96	5.10	4.97	5.95	4.89	5.48	4.93	5.78	5.46	5.48
Title	4.83	5.13	5.68	4.74	6.05	5.39	4.81	5.22	5.65	4.95	6.00	5.73	5.23	6.13	5.32	6.28	5.67	5.75	5.42	5.89	6.13	6.75	5.89	5.72
Location text	1.73	2.08	2.04	2.00	2.09	2.12	1.97	1.74	1.96	1.63	1.78	2.08	2.00	2.25	1.91	1.93	2.20	2.00	1.96	2.00	1.67	2.06	1.71	1.90
Central meeting location	0.60	0.46	0.40	0.44	0.36	0.58	0.53	0.61	0.46	0.68	0.39	0.39	0.42	0.63	0.59	0.59	0.57	0.60	0.46	0.56	0.53	0.53	0.61	0.52
Higher price	0.40	0.63	0.52	0.56	0.50	0.50	0.34	0.65	0.58	0.53	0.57	0.35	0.42	0.46	0.50	0.48	0.57	0.45	0.54	0.41	0.47	0.42	0.61	0.52
Market																								
Posts in market	1,164.60	1,123.42	443.96	477.74	204.96	187.77	476.81	442.44	408.08	382.90	311.04	272.85	434.85	439.17	971.96	878.83	273.67	278.15	1,278.62	1,321.26	231.27	205.56	94.71	99.28
Mean market price	330.20	337.79	330.80	327.87	413.18	402.23	361.22	341.37	353.75	320.26	351.44	335.65	356.15	341.25	338.05	321.21	316.33	309.50	356.92	351.11	387.67	386.46	381.36	374.07
Market price change	0.17	0.42	0.36	0.22	0.14	0.35	0.22	0.44	0.35	0.37	0.26	0.27	0.42	0.21	0.18	0.35	0.27	0.35	0.27	0.37	0.27	0.33	0.32	0.28
Post timing																								
Posted on Thursday	0.53	0.38	0.52	0.52	0.41	0.50	0.38	0.61	0.50	0.37	0.48	0.50	0.42	0.50	0.55	0.55	0.47	0.50	0.46	0.56	0.47	0.56	0.54	0.45
Posted in 2014	0.60	0.58	0.72	0.52	0.55	0.62	0.53	0.74	0.62	0.84	0.48	0.69	0.65	0.63	0.46	0.69	0.60	0.70	0.62	0.63	0.53	0.69	0.61	0.59
*n*th post within city	26.47	31.04	32.88	26.19	24.32	31.50	25.50	35.87	32.65	34.74	24.78	30.81	32.89	28.42	22.82	32.31	28.67	31.90	29.69	30.44	28.00	31.78	28.54	29.45
Holiday	0.30	0.21	0.24	0.26	0.27	0.31	0.31	0.17	0.27	0.11	0.26	0.19	0.23	0.25	0.27	0.24	0.23	0.30	0.27	0.15	0.27	0.22	0.32	0.17

*Post text and title variables are coded 1 through 10, and post location text is coded 1, 2, or 3. Central meeting location and higher price variables are coded 0 or 1. Adv., advantaged neighborhood; Disadv., disadvantaged neighborhood.

Table S2. Negative binomial model estimates of the effect of neighborhood disadvantage on number of responses to posted advertisements within 60 h, all covariates reported

	Full sample (n = 615)		Sample of cities with black neighborhoods (n = 462)		Sample of cities with Latino neighborhoods (n = 204)	
Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Disadvantaged neighborhood	0.839***	0.892				
	(0.044)	(0.066)				
Disadvantaged neighborhood × Central location		0.885				
		(0.092)				
Black disadvantaged neighborhood			0.794***	0.831*		
			(0.050)	(0.072)		
Black disadvantaged neighborhood × Central location				0.912		
				(0.113)		
Latino disadvantaged neighborhood					0.981	1.088
					(0.097)	(0.153)
Latino disadvantaged neighborhood × Central location						0.809
						(0.168)
Central meeting location	0.937	0.991	0.977	1.016	0.894	0.979
	(0.048)	(0.069)	(0.060)	(0.082)	(0.091)	(0.132)
Post text 1	0.902	0.893	0.894	0.895	0.930	0.895
	(0.103)	(0.102)	(0.132)	(0.132)	(0.187)	(0.180)
Post text 2	1.117	1.097	1.208	1.199	1.087	1.060
	(0.130)	(0.128)	(0.172)	(0.171)	(0.255)	(0.248)
Post text 3	1.151	1.135	1.343*	1.333*	0.902	0.884
	(0.130)	(0.128)	(0.184)	(0.183)	(0.198)	(0.194)
Post text 4	1.013	0.999	1.199	1.199	0.726	0.687^{+}
	(0.111)	(0.110)	(0.167)	(0.167)	(0.150)	(0.146)
Post text 5	0.940	0.935	1.093	1.097	0.771	0.749
	(0.109)	(0.109)	(0.159)	(0.159)	(0.176)	(0.171)
Post text 6	1.108	1.103	1.256^{+}	1.258^{+}	0.653^{+}	0.646^{+}
	(0.118)	(0.118)	(0.166)	(0.165)	(0.150)	(0.148)
Post text 7	0.901	0.895	1.037	1.033	0.795	0.782
	(0.102)	(0.101)	(0.157)	(0.156)	(0.149)	(0.147)
Post text 8	1.110	1.101	1.307^{+}	1.308^{+}	0.957	0.927
	(0.127)	(0.126)	(0.180)	(0.180)	(0.201)	(0.195)
Post text 9	1.100	1.082	1.161	1.155	0.892	0.851
	(0.134)	(0.133)	(0.177)	(0.176)	(0.188)	(0.183)
Post title 1	1.247*	1.244*	1.296*	1.291*	1.305	1.269
	(0.135)	(0.134)	(0.165)	(0.164)	(0.277)	(0.270)
Post title 2	1.041	1.047	1.146	1.144	0.937	0.970
	(0.118)	(0.118)	(0.154)	(0.154)	(0.202)	(0.211)
Post title 3	0.524***	0.521***	0.540***	0.536***	0.490**	0.484**
	(0.068)	(0.067)	(0.083)	(0.082)	(0.120)	(0.118)
Post title 4	0.832	0.833	0.948	0.949	0.653^{+}	0.654^{+}
	(0.099)	(0.099)	(0.132)	(0.132)	(0.164)	(0.163)
Post title 5	1.183	1.181	1.293^{+}	1.294^{+}	1.179	1.160
	(0.137)	(0.137)	(0.182)	(0.181)	(0.238)	(0.233)
Post title 6	1.050	1.044	1.117	1.107	0.890	0.883
	(0.122)	(0.121)	(0.158)	(0.157)	(0.207)	(0.204)
Post title 7	0.790^{+}	0.790^{+}	0.856	0.851	0.868	0.886
	(0.097)	(0.097)	(0.124)	(0.124)	(0.209)	(0.213)
Post title 8	1.193	1.192	1.192	1.189	1.082	1.072
	(0.128)	(0.128)	(0.155)	(0.155)	(0.222)	(0.220)
Post title 9	0.755*	0.752*	0.785^{+}	0.786^{+}	0.664^{+}	0.644*
	(0.090)	(0.090)	(0.114)	(0.114)	(0.143)	(0.140)
Post location text 2	0.979	0.975	1.028	1.025	1.067	1.052
	(0.061)	(0.061)	(0.076)	(0.075)	(0.140)	(0.139)
Post location text 3	0.975	0.975	0.899	0.898	1.142	1.131
	(0.063)	(0.063)	(0.071)	(0.071)	(0.151)	(0.149)
Actual posted price	0.979*	0.979*	0.970*	0.969*	0.988	0.990
	(0.010)	(0.010)	(0.013)	(0.013)	(0.021)	(0.021)
Actual posted price squared	1.000	1.000	1.000	1.000	1.000	1.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

Table S2. Cont.

Variables	Full sample (n = 615)		Sample of cities with black neighborhoods (n = 462)		Sample of cities with Latino neighborhoods (n = 204)	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Post on Thursday	1.162**	1.158**	1.117^{+}	1.115^{+}	1.111	1.111
	(0.060)	(0.060)	(0.068)	(0.068)	(0.116)	(0.116)
Post in 2014	1.562***	1.555***	1.499**	1.491**	1.895**	1.881**
	(0.173)	(0.172)	(0.199)	(0.198)	(0.459)	(0.456)
*n*th post within city	0.920***	0.921***	0.914***	0.915***	0.933***	0.934***
	(0.008)	(0.008)	(0.011)	(0.011)	(0.017)	(0.017)
*n*th post squared	1.001***	1.001***	1.001***	1.001***	1.000	1.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Local iPhone 5 market size	1.000	1.000	0.999*	0.999^{+}	1.001^{+}	1.001^{+}
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Median iPhone market price	0.990*	0.990*	0.995	0.995	0.992	0.991
	(0.005)	(0.005)	(0.006)	(0.006)	(0.009)	(0.009)
Flag for change in market price calculation	1.164	1.157	1.198	1.196	1.231	1.225
	(0.176)	(0.174)	(0.224)	(0.223)	(0.378)	(0.373)
Holiday	1.086	1.091	1.037	1.040	1.212	1.206
	(0.065)	(0.066)	(0.073)	(0.073)	(0.147)	(0.145)
N	615	615	462	462	204	204

This table displays exponentiated coefficients with SEs in parentheses. $^{+}P < 0.10$; $*P < 0.05$; $**P < 0.01$; $***P < 0.001$ (two-tailed tests).

Table S3. Model estimates for alternative outcomes

Variables	Flagged post	At least one response	Proportion of responses with counteroffer	Mean counteroffer	Minimum counteroffer	Maximum counteroffer
Disadvantaged neighborhood	−0.266	0.093	−0.003	1.722	4.629	−0.540
	(0.355)	(0.311)	(0.024)	(3.439)	(3.939)	(3.757)
Central meeting location	−0.689^{+}	−0.623*	0.037	−2.408	−1.670	−3.304
	(0.357)	(0.311)	(0.024)	(3.399)	(3.893)	(3.713)
Post text 1	−0.651	0.888	−0.031	−5.931	−2.106	−9.817
	(0.717)	(0.696)	(0.051)	(6.987)	(8.004)	(7.634)
Post text 2	−0.393	0.919	−0.116*	−3.173	−1.177	−4.189
	(0.732)	(0.705)	(0.051)	(7.706)	(8.827)	(8.419)
Post text 3	−0.329	1.228	−0.059	0.050	7.294	−5.021
	(0.671)	(0.818)	(0.052)	(7.054)	(8.080)	(7.707)
Post text 4	−2.700*	−0.612	−0.047	1.443	3.342	0.380
	(1.152)	(0.627)	(0.053)	(7.293)	(8.354)	(7.968)
Post text 5	−1.436	−.082	−0.090^{+}	−8.566	−3.034	−13.954^{+}
	(0.882)	(0.632)	(0.053)	(7.503)	(8.595)	(8.198)
Post text 6	−1.436^{+}	0.259	−0.122*	4.439	6.558	3.823
	(0.800)	(0.638)	(0.051)	(6.913)	(7.919)	(7.553)
Post text 7	−0.698	−0.109	−0.026	−6.674	−4.074	−8.712
	(0.730)	(0.670)	(0.054)	(7.321)	(8.386)	(7.999)
Post text 8	0.084	−0.419	−0.061	−4.283	2.115	−8.348
	(0.647)	(0.649)	(0.054)	(7.294)	(8.355)	(7.969)
Post text 9	0.601	−0.433	−0.110^{+}	−1.063	4.710	−6.199
	(0.625)	(0.646)	(0.057)	(8.405)	(9.628)	(9.183)
Post title 1	0.665	−0.027	−0.025	2.498	−8.881	13.297^{+}
	(0.664)	(0.710)	(0.053)	(7.328)	(8.394)	(8.006)
Post title 2	0.404	0.326	0.001	−6.207	−10.427	−1.929
	(0.702)	(0.753)	(0.053)	(7.382)	(8.456)	(8.066)
Post title 3	−1.113	−0.973	0.014	−3.511	−0.200	−6.926
	(0.962)	(0.659)	(0.052)	(7.722)	(8.846)	(8.437)
Post title 4	−0.004	−0.684	−0.052	−4.987	−2.224	−7.662
	(0.701)	(0.676)	(0.053)	(7.589)	(8.693)	(8.291)
Post title 5	−0.395	0.071	0.004	0.509	−3.015	3.172
	(0.775)	(0.754)	(0.055)	(7.329)	(8.395)	(8.007)
Post title 6	0.461	−0.232	−0.042	−3.086	−3.078	−4.219
	(0.747)	(0.704)	(0.055)	(7.548)	(8.646)	(8.247)
Post title 7	−0.421	0.209	−0.161**	4.920	6.264	2.420
	(0.831)	(0.688)	(0.052)	(8.612)	(9.865)	(9.409)
Post title 8	−1.385	0.747	−0.111*	−2.721	−7.250	1.352
	(0.934)	(0.693)	(0.050)	(7.247)	(8.301)	(7.918)
Post title 9	−0.217	−0.830	−0.103^{+}	10.425	11.419	8.510
	(0.743)	(0.633)	(0.053)	(7.765)	(8.895)	(8.484)
Post location text 2	−0.177	−0.152	−0.065*	−0.840	−3.288	1.507
	(0.441)	(0.370)	(0.029)	(4.184)	(4.792)	(4.571)
Post location text 3	0.136	−0.021	−0.041	2.097	3.976	−0.879
	(0.416)	(0.378)	(0.029)	(4.123)	(4.723)	(4.505)
Actual posted price	−0.109^{+}	0.112	0.006	0.355	0.443	0.363
	(0.058)	(0.068)	(0.005)	(0.697)	(0.799)	(0.762)
Actual posted price squared	0.000*	−0.000*	−0.000	0.001	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Post on Thursday	−0.058	1.721***	−0.003	2.518	3.322	1.355
	(0.352)	(0.339)	(0.024)	(3.431)	(3.931)	(3.749)
Post in 2014	−0.187	1.136	0.097*	3.926	−1.714	9.842
	(0.958)	(0.811)	(0.049)	(7.019)	(8.041)	(7.669)
*n*th post within city	−0.219***	−0.356***	−0.005	−0.816	−0.012	−1.670**
	(0.058)	(0.089)	(0.004)	(0.567)	(0.649)	(0.619)
*n*th post squared	0.004***	0.003**	0.000	0.001	−0.007	0.010
	(0.001)	(0.001)	(0.000)	(0.011)	(0.012)	(0.011)
Local iPhone 5 market size	−0.000	−0.001	−0.000	−0.001	0.002	−0.004
	(0.001)	(0.001)	(0.000)	(0.005)	(0.006)	(0.005)
Median iPhone market price	0.005	−0.014	0.000	−0.049	−0.093	0.000
	(0.031)	(0.027)	(0.002)	(0.302)	(0.346)	(0.330)

Table S3. Cont.

Variables	Flagged post	At least one response	Proportion of responses with counteroffer	Mean counteroffer	Minimum counteroffer	Maximum counteroffer
Flag for change in market price calculation	0.097	0.741	−0.046	8.429	4.585	12.092
	(1.038)	(0.641)	(0.065)	(9.345)	(10.705)	(10.210)
Holiday	0.476	−0.034	0.014	−0.533	−3.739	3.037
	(0.462)	(0.452)	(0.029)	(4.044)	(4.633)	(4.419)
Constant	16.436*	−0.016	−0.817	127.345	98.232	143.966
	(7.431)	(10.113)	(0.667)	(92.906)	(106.422)	(101.507)
N	664	615	512	314	314	314
Outcome units	Logits	Logits	Proportion	Dollars	Dollars	Dollars
Regression model	Logistic	Logistic	Ordinary least squares	Ordinary least squares	Ordinary least squares	Ordinary least squares

This table displays nonexponentiated coefficients with SEs in parentheses. All regression models include city fixed effects. $^{+}P < 0.10$; $*P < 0.05$; $**P < 0.01$; $***P < 0.001$ (two-tailed tests).

Table S4. Negative binomial model estimates of the effect of neighborhood disadvantage on number of responses to posted advertisements within 60 h, excluding advertisements posted in Atlanta

	Full sample (n = 561)		Sample of cities with black neighborhoods (n = 408)		Sample of cities with Latino neighborhoods (n = 204)	
Treatment definition	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Disadvantaged neighborhood	0.881* (0.049)	0.919 (0.072)				
Disadvantaged neighborhood × central location		0.919 (0.103)				
Black disadvantaged neighborhood			0.865* (0.063)	0.875 (0.088)		
Black neighborhood × central location				0.974 (0.140)		
Latino disadvantaged neighborhood					0.981 (0.097)	1.088 (0.153)
Latino neighborhood × central location						0.809 (0.168)
Central meeting location	0.937 (0.051)	0.976 (0.075)	0.979 (0.072)	0.991 (0.099)	0.894 (0.091)	0.979 (0.132)

This table displays exponentiated coefficients with SEs in parentheses. $*P < 0.05$ (two-tailed tests).

Table S5. Differences in estimates of the effect of neighborhood disadvantage on number of responses to posted advertisements within 60 h across samples and model specifications

Specification	Full sample (n = 615)	Excluding Atlanta (n = 561)	Excluding Atlanta and Los Angeles (n = 516)	Excluding Atlanta, Chicago, NY Manhattan, and Los Angeles (n = 411)
Mean posts in advantaged neighborhoods	4.022	3.818	3.964	4.052
	(0.232)	(0.229)	(0.247)	(0.278)
Mean posts in disadvantaged neighborhoods	3.429	3.497	3.464	3.414
	(0.199)	(0.210)	(0.220)	(0.229)
Mean difference between advantaged and disadvantaged neighborhoods	−0.594^{+}	−0.322	−0.499	−0.639^{+}
	(0.305)	(0.311)	(0.330)	(0.357)
Estimate of disadvantaged neighborhood effect using negative binomial regression with no controls	−0.160^{+}	−0.088	−0.135	−0.172^{+}
	(0.082)	(0.085)	(0.089)	(0.097)
Estimate of disadvantaged neighborhood effect using linear regression with controls	−0.537*	−0.404^{+}	−0.351	−0.378
	(0.210)	(0.218)	(0.229)	(0.258)
Estimate of disadvantaged neighborhood effect using negative binomial regression with controls	−0.176***	−0.126*	−0.119*	−0.137*
	(0.052)	(0.055)	(0.057)	(0.063)

This table displays nonexponentiated coefficients with SEs in parentheses. $^{+}P < 0.10$; $*P < 0.05$; $***P < 0.001$ (two-tailed tests).

Table S6. Treatment effect heterogeneity estimates of neighborhood disadvantage on number of responses to posted advertisements within 60 h, by city, compared with the weighted grand mean

City	Interaction coefficient, disadvantaged neighborhood × city	
Atlanta	0.817*	(0.070)
Baltimore	0.913	(0.063)
Boston	1.099	(0.102)
Chicago	0.897	(0.077)
Los Angeles	1.057	(0.100)
NY Brooklyn	1.037	(0.082)
NY Manhattan	1.157^{+}	(0.098)
Philadelphia	0.919	(0.081)
Phoenix	0.937	(0.082)
San Antonio	1.099	(0.087)
Seattle	1.146	(0.150)
Washington, DC	1.019	(0.082)

This table displays exponentiated coefficients with SEs in parentheses. Model results are estimated from a negative binomial regression model with two-way interactions between neighborhood disadvantage and an indicator variable for each city. The reported coefficients capture the ratio of posts in the two-way interaction (city by neighborhood disadvantage) relative to the grand mean. A coefficient value of 1 indicates no deviation from the grand mean in the expected number of responses within 60 h. $^{+}P < 0.10$; $^{*}P < 0.05$ (two-tailed tests).