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Out of Sight, Out of Mind? Evidence of Perceptual Factors in the Multiple-Category Discount

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Extant work shows that market actors who span multiple social categories tend to be devalued relative to their more specialized peers. Scholars typically explain this pattern of results with one of two arguments. Some contend that perceptual factors—namely, the difficulties that buyers have in making sense of category spanners—contribute to the observed pattern of devaluation. Others argue that the penalty for category-spanning stems from the fact that those who do not focus their efforts narrowly tend to offer products that are of lower quality. Because these two mechanisms often co-occur, it has been difficult to provide definitive evidence of the perceptually driven component of the multiple-category penalty. We employ a natural experiment on a peer-to-peer crowd-funding website to address this gap. Difference-in-difference analyses on matched samples show that category spanning is perceived negatively and can result in devaluation, even in the absence of underlying quality differences. This result supports the argument that perceptual issues contribute to the penalty for category spanning.

Key words: categorization; collective production market; peer-to-peer lending; natural experiment; difference-in-difference
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Introduction

Recent research in economic and organizational sociology has demonstrated that categories and categorization processes are highly influential in market settings, shaping how individuals and firms are perceived and thereby impacting a wide range of economic outcomes (Zuckerman 1999, 2000; Zuckerman et al. 2003; Hannan et al. 2007; Ruef and Patterson 2009). One key argument in this vein is that organizations, individuals, or products that span multiple categories tend to be devalued or ignored, either because organizational audiences, such as consumers, critics, and investors, find them confusing or because such audiences tend to penalize those who violate social codes against boundary crossing. Empirical work is consistent with this argument. The penalty for occupying a category-spanning market position has been found in a variety of contexts, including the stock market (Zuckerman 1999), feature films (Hsu 2006), careers (Zuckerman et al. 2003), online marketplaces such as eBay (Hsu et al. 2009), restaurants (Kovács and Hannan 2010), wines (Negro and Leung 2013), and the market for venture capital (Pontikes 2012); for a review, see Hannan (2010).

To establish that the observed penalty for actors spanning social categories is due to perceptual or sensemaking factors, however, researchers must first net out any underlying differences in capabilities that might render the offerings of category spanners lower quality or otherwise less appealing. This is a formidable empirical challenge. An extensive body of work in the organizational ecology tradition, as well as the strategic management and finance literatures, suggests a variety of reasons for why the offerings of those who span categories could truly be of lower quality. Niche-width theory in organizational ecology (Freeman and Hannan 1983) highlights the operational challenges that result from organizations attempting to do many different things at once. According to this theory, organizations face similar constraints in terms of the total effort they can expend. As a result, firms must trade off broader engagement across many areas for deeper engagement with one. Support for this theory has been found in a variety of contexts, including restaurants (Freeman and Hannan 1983) and car manufacturers (Dobrev et al. 2001), for example. Similarly, the strategic management and finance literatures report evidence of a corporate diversification discount (Wernerfelt and Montgomery 1988, Lang and Stulz 1994, Berger and Ofek 1995, Burch and Nanda 2003, Laeven and Levine 2007) but attribute the results entirely to operational mechanisms, such as inefficiency stemming from rent-seeking behaviors, agency problems, or information asymmetries that are more problematic in diversified firms than in focused ones (Bolton and Scharfstein 1990, Rotemberg and Saloner 1994, Berger and Ofek 1995, Jensen 1996, Denis et al. 1997, Scharfstein and Stein 2000, Ozbas 2005). Finally, differential selection into category spanning is a possible explanation as well. Perhaps the devaluation of category spanners such as diversified firms is due not to the difficulties of category spanning per se but rather to differential selection, whereby poor performers tend to enter new categories in an effort to improve performance (Campa and Kedia 2002, Villalonga 2004). Conversely, those who are the best at something might tend to focus, precisely because of their superior capabilities.

Noting that audience- and producer-side accounts of the multiple-category penalty are not necessarily mutually exclusive, Hsu et al. (2009) developed a theory integrating both perspectives on the devaluation of category spanners. The empirical aspect of their paper nicely illustrates how such processes may occur in tandem. Yet, precisely because the two causes do tend to occur together, it remains to be demonstrated that audiences perceive multiple-category members to be less appealing even in the absence of any underlying quality differences. To date, researchers have attempted to demonstrate the existence of a perceptual penalty for category spanning net of any underlying differences either by employing statistical controls for quality or by devising empirical tests that generate different predictions, depending on whether the penalty is driven by perceptual and underlying quality issues.

Perhaps the strongest evidence of a perceptually driven penalty for category spanning comes from Negro and Leung's (2013) study of Italian wineries. In that case, the authors found that critics tended to rate wines as less appealing if they were created by a winemaker who produced both traditional and modern wines, rather than being made by winemakers who focused on one style alone. The authors accounted for the potential heterogeneity in wine quality by exploiting the fact that the same wines received ratings from two different guides—the Gambero Rosso and Veronelli guides—that approached the evaluation process differently. Gambero Rosso critics rated the wines blindly, meaning that they did not know the identity of the producer, whereas the Veronelli critics were aware of the producers' identity. The authors reasoned that any underlying difference in quality between wines from category-spanning and more focused wineries would be captured in the ratings from the Gambero Rosso (blind ratings). They therefore interpreted the fact that wines from category-spanning wineries received lower ratings from the nonblind evaluators, even after controlling for the blind ratings, as evidence of a perceptually driven penalty for category spanning. Although the authors' efforts to control for underlying quality in this case were impressive, they are still open to a number of critiques. As they noted (p. 694), "[W]e are unable to definitely identify whether our measure for quality is completely subsumed by the Veronelli guide or whether the Gambero Rosso critics take into account additional characteristics.... ." The fact remains that the wines were evaluated by two different critics who might differ in terms of taste.

In this paper, we advance the literature on the role of categories in economic and organizational life by using a natural experiment to provide stronger empirical evidence that an audience-side or perceptually driven penalty for multiple-category membership exists in market settings. Our goal is not to compare the magnitude of effects stemming from audience perceptual factors to those from operational ones that result in quality differences, but rather to discern whether audience perceptions drive at least part of the multiple-category discount found in economic settings, even after accounting for functional differences. We employ unique evidence in the form of a natural experiment on a peer-to-peer crowd-funding website. The site enables people to borrow directly from one another without formal financial intermediaries such as banks. It incorporates social aspects, such as the ability for users to join site-specific "groups," which were classified in up to five categories. Labels indicating a borrower's group category affiliations were visible on the website at one time but were removed for reasons exogenous to our theoretical interests. We exploit this exogenous variation in the visibility of category spanning to test for the negative effects of multiple-category membership that might stem from perceptions rather than from quality- or capabilities-based differences.

By analyzing a matched sample of loans that were listed before and after the removal of category labels, we demonstrate that prospective lenders viewed a loan request as less attractive if it belonged to a group classified in a greater number of categories during the time frame when labels were visible. However, this disadvantage subsided after category labels were removed. Because any underlying quality or capabilities-based differences between multiple-category members and others are likely to have remained constant across the time period, and therefore were unlikely to have produced the observed change in the penalty, we conclude that audience perceptions were the main driver of the penalty when multiple-category members were clearly identifiable. More generally, these analyses provide evidence that the devaluation of multiple-category members in economic contexts can occur even in the absence of any operational differences.

The Multiple-Category Penalty: Theoretical Background and Existing Evidence

Recent research in economic and organizational sociology explores the role of classification structures, or categories, in organizational contexts (Zuckerman 1999, 2000; Zuckerman et al. 2003; Hsu and Hannan 2005;

Rao et al. 2005; Hsu 2006; Hannan et al. 2007). Hannan et al. (2007) defined a clustering of similar organizations as constituting a category when members of an audience, such as employees, critics, or consumers, attach a label to the cluster, reach a high degree of consensus about what the label means, and come to agreement about the set of organizations to which the label applies. Following this, if an actor claims membership in a category and if the audience accepts that claim, the audience concurrently makes inferences as to how the actor will behave and what features the actor will possess (Bruner 1957). These category-level expectations might actually fit any particular organization claiming category membership to a greater or lesser degree depending on how closely the actor resembles the typical category member (Rosch and Mervis 1975, Malt and Smith 1984, Porac and Thomas 1990). Thus, category membership generally corresponds in some measure to "real" characteristics of an actor, but, more precisely, it denotes the audience's perception of the actor vis-à-vis some ideal type in the prevailing classification structure (Zerubavel 1997). In this sense, we view category membership in economic settings as a positional attribute denoting an actor's place in an audience's cognitive representation of a market (Porac et al. 1995, Rosa et al.1999).

Categories are integral because they help audiences identify relevant product offerings and choose among them. Standard models of consumer decision making posit a two-stage selection process whereby consumers first identify the choice set of all reasonably relevant offers and then optimize among this smaller set of alternatives (e.g., Howard and Sheth 1969, Payne 1976, Lussier and Olshavsky 1979, Payne et al. 1988). Category membership looms large in the first stage because it represents a highly salient marker by which boundaries can be drawn between true contenders and irrelevant ones, thus circumscribing the set of individuals or products that are viewed as commensurable (Espeland and Stevens 1998, Lamont and Molnar 2002), and thereby receive a more thorough evaluation.

This process of commensuration and evaluation becomes problematic, however, when an offering is difficult to classify, either because it does not seem to fit into any category at all or because it fits into too many. In either case, audiences tend to exclude such ambiguous options from further consideration and instead direct their efforts toward evaluating more clearly relevant offerings. This may occur because evaluators are unsure of how to make sense of such offerings (Zuckerman 1999, Hsu 2006) or because they view the lack of focus on a single category as signaling something negative, such as a lack of engagement, commitment, or ability (Zuckerman et al. 2003, Hsu et al. 2009). Recent research suggests that this audience-driven or perceptual account of the penalty for multiple-category membership holds in market settings such as the stock market (Zuckerman 1999), French cuisine (Rao et al. 2005), feature films (Hsu 2006), eBay (Hsu et al. 2009), and wines (Negro and Leung 2013).

The convergent results across various empirical contexts support the notion that actors who defy institutionalized classification schemas are punished or ignored because evaluators have difficulty making sense of them or infer a lack of ability. However, the nature of this phenomenon makes it difficult to draw this conclusion definitively. As noted earlier, the strategic management and finance literatures, as well as niche-width theory in organizational ecology, offer other explanations as to why multiple-category members, such as diversified firms, might be devalued. These accounts argue that potential operational difficulties arise when actors attempt to do many different things at once (Freeman and Hannan 1983, Péli 1997, Dobrev et al. 2001, Hannan et al. 2007, Hsu et al. 2009).[1] Taken together, then, existing research suggests two possible sources of the penalty for multiple-category membership: (1) devaluation as a result of perceptual issues stemming from audience members having difficulty understanding or viewing illegitimate actors that do not fit clearly into culturally shared categories and (2) devaluation driven by operational challenges that result in poorer quality and performance for actors who try to do many different things at once. These two can and often do operate concurrently (Hsu et al. 2009, Negro and Leung 2013), which hampers efforts to isolate the negative effect of multiple-category membership that stems purely from the fact that audience members perceive category spanners to be confusing or illegitimate. Archival studies typically attempt to mitigate this issue through the use of statistical controls within a regression framework. These controls are often impressive and reflect a concerted effort to address the issue of underlying differences in quality that might be correlated with category spanning. However, to the extent that these controls are imperfect, an assumption that is inherently untestable, these results will always be subject to the critique we made earlier.

In this paper, our primary aim is to advance the line of research on the role of categories and classification processes in economic settings by establishing that audience perceptions alone can drive the devaluation of category spanners. Our approach stems from an insight about the role of labels in the devaluation of multiple-category members. Because consumers and evaluators use category labels to identify and make sense of individuals and products, category labels play a key role in the audience perceptions that have been posited to drive the ignoring and devaluing of multiple-category members. However, labels are not integral to operational difficulties or functional differences that might drive the lowered appeal of multiple-category members; any such ability differences would presumably be apparent even in the absence of category labels.

We apply this insight to a setting in which we were able to observe exogenous variation in the visibility of category labels that identify multiple-category members as such. We compare outcomes for a matched set of actors who vary in terms of both multiple-category membership and the presence of category labels highlighting their identities. Because labels are key to audience-side perceptual sources of devaluation but not to producer-side or quality-based causes, such a comparison allows us to determine whether audience perceptions contribute to devaluation. Our expectation is that members of more categories will be evaluated more negatively than others when labels are present, thereby highlighting their status as multiple-category members. However, if audience-driven perceptual processes account for part of this penalty, then we expect that any penalty will attenuate once labels are removed. Overall, by disentangling the audience-side perceptual component of the penalty for category spanners from any portion of the penalty that might be due to underlying differences in quality or appeal, we aim to advance understanding of the role of cognitive or perceptual factors in markets.

Data and Methods

Empirical Setting and Analytical Approach

The empirical context for our study is a peer-to-peer crowd-funding website, Prosper (http://www.prosper.com, accessed July 2009). Prosper defines itself as "an online community for lending and borrowing money. Lenders and borrowers come together to bid on personal loans: loans without a bank through peer-to-peer lending." Users of the website who wish to borrow money for any purpose can post an unsecured loan request (a listing) for up to $25,000 to be paid back over three years. Other website users who wish to loan money bid on loan listings by promising to fund a portion of the loan at a particular interest rate. They aim to profit from the interest they will charge.

Figure 1 shows an example of a loan listing posted online by a prospective borrower. Listings include the dollar amount and desired interest rate of the loan. Prospective borrowers also provide a short description of the purpose of the loan and submit financial information (e.g., credit rating, income, debts) verified by a third party. In addition, a listing shows other facets of a prospective borrower's profile. After reviewing such information, lenders then may choose to bid on a loan by offering to fund some portion of the borrower's total request at an interest rate that they can specify.

Listings remain active for some time period (usually one week) as specified by the prospective borrower. At the end of the listing period, bids on a loan are summed. If the total amount offered reaches the amount requested, the loan is considered to be "fully funded." In the case of a funded loan, bids are aggregated to form a single loan issued in the name of the website to the borrower.

The website bases its overall format on microlending co-ops in which individuals band together to lend and borrow outside of formal financial institutions. Historically, one premise behind microlending co-ops was that default rates would be lower for loans that occurred among group members, because borrowers would feel a stronger sense of obligation and commitment to repay those with whom they shared social bonds. Another advantage of group membership is the built-in population of possible lenders to borrow from. Finally, group members also may help one another by providing advice on how to make their listings more attractive. In the hopes of fostering this sense of community, the website allows participants to establish and/or join self-organized groups. Borrowers on the site may choose to join a group. If they do, they are limited to joining only one. These virtual groups are established by a self-appointed "group leader" who is responsible

Figure 1 **Screenshot of a Prosper Loan Request**

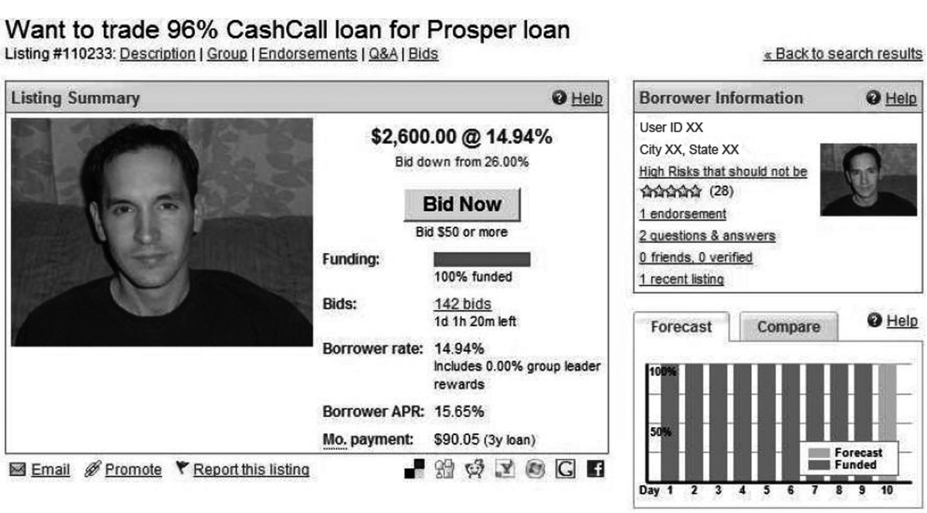
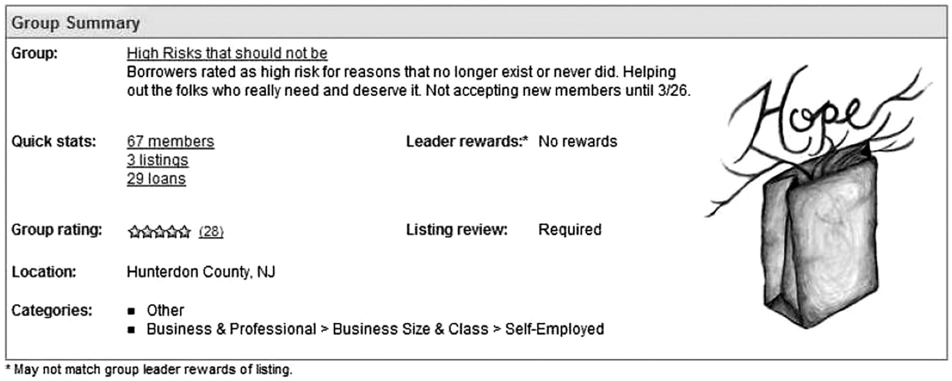


Figure 2 Screenshot of a Group Page with Category Labels Visible



for classifying the group into various categories. These categories serve to signify the group's identity and are used to help new members seek an appropriate group to join as well as assist lenders to identify potential borrowers with which they may have an affinity. Each group is required to take on at least one category label selected from a drop-down menu. For example, some nurses may choose to band together and categorize their group as "Nursing." There are 1,552 category labels from which a group leader can choose.

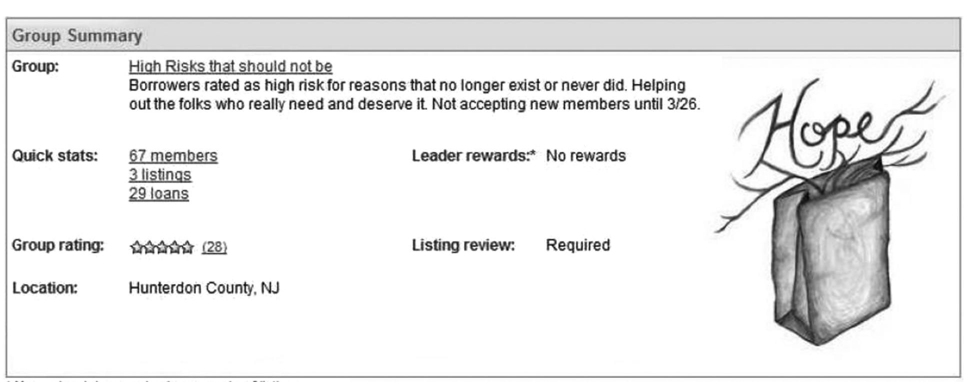
Thus, we conceive of an individual being a member of multiple categories if he belongs to a group that is classified in multiple categories. Category membership is closely connected to an individual's identity because a person's loan listing contains a link to the page of any group to which he or she belongs. Clicking on the group link takes users to a group page with a more detailed description of the group's mission, as well as information about the number of members, listings, and outstanding loans. Most relevant for the topic here, the page also prominently includes labels for the categories with which the group is affiliated. Using these category labels, lenders can search for groups with which they may have an affinity (such as an alumni association) to find borrowers to fund. See Figure 2 for an example of a group that has visible category labels.

On September 7, 2007, site features were changed such that the category labels once listed on the website were removed, leading to category membership no longer being a visible factor that potential lenders could use to evaluate loan listings. See Figure 3 for an image of a group page with category labels eliminated. Notice the only change is the removal of the group's categorical affiliations. Label removal provides us with exogenous variation in the extent to which multiple-category membership was apparent to prospective lenders. Our approach, then, is to use this exogenous variation to test how the visibility of the number of categories a group is affiliated with impacts its members' attractiveness to lenders. Presuming that any functional differences between those in greater or fewer categories would be the same before and after label removal, any difference in the rates at which loan listings from groups in varying members of categories receive funding across the two time periods is likely due to perceptual or sense-making issues that manifest themselves in the presence of labels that highlight category spanning.

Data

We analyzed data available on the Prosper.com website. We focused on listings that occurred in the 100-day periods before and after label removal (i.e., 200 days in

Figure 3 Screenshot of a Group Page After Category Labels Were Removed



total), excluding any listings that spanned the date of label removal. We did not analyze listings that spanned the change because we were uncertain what would happen to listings that had visible labels one day and none the next. This resulted in 77,888 listings. To identify the perceptually driven penalty to multiple-category membership, we employed difference-in-difference analyses (Card and Krueger 1994, Meyer 1995) that examine how any disparities in funding rates for those belonging to multiple-category groups relative to others differed in the periods before and after label removal. To show the robustness of our results, we contrasted members of multiple-category groups with two other sets of individuals: those who were in groups that were labeled with fewer categories and those who were not in groups at all.

We took a number of steps to ensure that the assumptions behind our difference-in-difference identification strategy held. First, we conducted interviews with Prosper employees to ensure that label removal was exogenous and was not driven by concerns particular to multiple-category members. Company employees confirmed that both of these conditions held. Specifically, they said that changes to the website were due to their shifting the focus of their business model more toward "family and friends" rather than affinity groups. They also pointed out that category label removal was done without advertising to the user community but merely as a functional change. We also verified that label removal did not occur concurrently with other changes on the site.

We tested this central empirical assumption by examining whether members of groups that were disadvantaged before label removal seemed to act in ways that would allow them to take advantage of this change. If this were the case, disadvantaged borrowers would delay their loan listings in anticipation of a more favorable outcome, and we would observe an increased likelihood of members of multiple-category groups listing a loan soon after label removal relative to just before. Although this would seem to require an implausibly high level of knowledge and foresight about site dynamics, we nonetheless tested for this possibility. We estimated the likelihood that a listing occurred before rather than after the date of label removal as a function of the number of categories that an individual's group claimed. We should be more comfortable with our assumption that there was no previous knowledge of this change occurring if we observe no evidence of a rise in disadvantaged listings *after* label removal relative to *before*.

For this analysis, we focused on listings that occurred within a two-week window before and after label removal. Because listings are, on average, only active for about seven days, we conjectured that those borrowers whose listings were within this two-week window would be the ones most likely to strategically alter the timing of when they list. This resulted in 8,506 listings.

Table 1 Logit Estimates of Likelihood a Listing Is Posted Before vs. After Label Removal

	Coefficient	SE
Number of categories	−0.039	(0.052)
Total daily listings	−0.008***	(0.000)
Not in a group (= 1)	−0.005	(0.225)
Constant	2.684***	0.249
Observations	8,506	
Log likelihood	−5,514.17	
Pseudo-R^2	0.06	

***$p < 0.001$.

Results reported in Table 1 support the assumption that this was an unannounced and exogenous change. We included controls for whether a borrower was in a group and the total number of listings on a given day. More relevant for testing the assumption of exogeneity, we include the number of categories a borrower was associated with and find that there is no significant correlation between this and whether or not he or she decided to list before or after the removal of the labels. Note also that group members overall were not any more or less likely to post a listing before the removal of the labels. We offer this as evidence that no strategic sorting occurred among disadvantaged borrowers.

Finally, we examined the assumption that the composition of the borrowing population remained constant across the two time periods of our 200-day observation window. Although we could control for observable changes in borrower characteristics, any shift in the composition of borrowers would make it more difficult to disentangle the effects of label removal from potentially unobservable differences in the types of people who posted loan listings before and after label removal. We suspected that this might be a valid concern given the timing of label removal: September 2007, in the midst of the subprime mortgage lending crisis. To investigate this possibility, we tested whether the mean observable characteristics of prospective borrowers differed before versus after label removal. The pattern of results reported in Table 2 indicates that characteristics did indeed vary from before to after label removal. For example, prospective borrowers had significantly higher income levels, were more likely to be homeowners, and had lower debt-to-income ratios in the period after label removal. The lack of balance in the composition of prospective borrowers across the two time periods may be problematic for our difference-in-difference analyses in that any observed disparate lending outcomes related to category membership might actually be due to compositional changes (or whatever underlying factor that drove the compositional changes) rather than category label removal itself.

We addressed this issue of imbalance by using a coarsened exact matching (CEM) procedure (Blackwell et al. 2009, Iacus et al. 2011) to identify a set of equivalent

Table 2 *t*-Tests of Difference in Means of Key Variables from Borrowers Before vs. After Label Removal on Sample Prior to Matching ($N = 37{,}151$ **Before and** 40,737 **After**)

Variable		Mean	SD	Significance[a]
Amount requested (dollars)	Before	6,715	34.33	$p < 0.000$
	After	7,290	33.17	
Interest rate requested	Before	0.178	3.6e−04	$p < 0.075$
	After	0.179	3.6e−04	
Income (coded 0–7, indicating strata of yearly income)	Before	3.15	0.006	$p < 0.000$
	After	3.36	0.006	
Credit grade (coded 1–8)	Before	5.79	0.008	$p < 0.000$
	After	5.62	0.008	
Current delinquencies (number)	Before	3.934	0.026	$p < 0.000$
	After	3.559	0.024	
Homeowner (=1)	Before	0.325	0.002	$p < 0.000$
	After	0.352	0.002	
Debt-to-income ratio	Before	0.512	0.015	$p < 0.014$
	After	0.457	0.015	

[a]Two-tailed *t*-test; after: < 0, before: > 0.

Table 3 *t*-Tests of Difference in Means of Key Variables from Borrowers Before vs. After Label Removal on Matched Sample ($N = 18{,}883$ **Before and** 18,883 **After**)

Variable		Mean	SD	Significance[a]
Amount requested (dollars)	Before	6,279	43.02	$p < 0.863$
	After	6,279	43.13	
Interest rate requested	Before	0.179	4.6e−04	$p < 0.973$
	After	0.179	4.6e−04	
Income (coded 0–7, indicating strata of yearly income)	Before	3.159	0.007	$p < 1.000$
	After	3.159	0.007	
Credit grade (coded 1–8)	Before	5.851	0.010	$p < 1.000$
	After	5.851	0.010	
Current delinquencies (number)	Before	3.159	0.027	$p < 0.567$
	After	3.137	0.027	
Homeowner (=1)	Before	0.294	0.003	$p < 1.000$
	After	0.294	0.003	
Debt-to-income ratio	Before	0.318	0.012	$p < 0.972$
	After	0.317	0.012	

[a]Two-tailed *t*-test; after: < 0, before: > 0.

listings that occurred before and after the removal of category labels on Prosper.com. The sample selection process proceeded as follows. First, we identified a set of observables, which we believed were required to ensure balance across the treatment and control groups. We then created and populated strata to ensure there is coverage across the entire support of the joint distribution of the covariates selected. For continuous covariates, we used the CEM's automatic binning algorithm (the "cem" command in STATA 11.2) to partition the observations into coarsened groups. We then randomly selected an equal number of treatment and matching control listings from these strata.

Because actual lower quality is the alternative explanation for why multiple-category members are discounted, we wanted to ensure the samples were balanced on observable metrics of quality. More specifically, for our main analyses, we matched on the following covariates: amount and interest rate requested, the debt-to-income ratio, a dummy variable indicating homeownership, and whether the individual has any current delinquencies. In addition, we matched on credit grade, income, and the number of categories associated with the group to which a person belonged. (For details on the coding of credit score and income, see Tables A.1 and A.2, respectively, in the appendix.) From our original population of 77,888 listings, we matched a total of 37,766 listings (18,883 before and after label removal). As Table 3 indicates, matching allowed us to achieve balance on these characteristics between individuals who listed before label removal and those who listed after.

Measures

Dependent Variable. The dependent variable in our analysis is the amount of funding a loan request (i.e., a listing) received as a percentage of the amount requested. Listings only became loans if they receive 100% of the amount requested, making the 100% mark a substantively important breakpoint. However, we use the percent funded in our analyses because we view it as a finer-grained measure of how much interest or appeal that a listing holds for potential lenders. This variable ranged from 0 to 1, (0%–100%). Of the 37,766 matched listings in our observation window, only 1,935 (5.1%) became loans. (We note, however, that all results reported later are similar if we instead run logistic regression models predicting whether a loan request gets funded.)

Independent Variables. The key independent variable in this analysis is how many categories a group was affiliated with. Prior to the removal of category labels, founders of groups were required to choose at least one out of a set of 1,552 possible category labels to describe their group. Groups in this data set belonged to anywhere between one to five categories. We measured the number of category labels as a count of these labels listed on the group's page. For example, the group "Atlanta Borrowers" was labeled as "Atlanta." We counted this as having one label. By contrast, the group "BORROWERS—Free instant Listings" was labeled as belonging to the following categories: "Entrepreneurs," "Latter Day Saints," "Families/Other," "Military/Other," and "Other." This group was operationalized as having five labels. We chose this approach because it reflected the way labels appeared visually on the group pages that prospective lenders could see when evaluating loan listings. Each label appeared as a separate line on the group page. Nongroup members were coded as having zero category labels.

Control Variables. Matching, combined with the exogenous shock of label removal, obviates the need for many controls. However, we included dummy variables for each high-level category with which a group was affiliated, thereby controlling for the possibility that some categories were disliked and that multiple-category groups might have been more likely to include a disliked category. Because our analyses included nongroup members as a "control" group that experienced all concurrent changes except for label removal, we also included an indicator for whether a prospective borrower belonged to a group. Table 4 summarizes the variables used in the CEM and analyses.

Models

Because our dependent variable is continuous, we estimated linear regression models using ordinary least squares (OLS). Specifically, we estimated

$$\begin{aligned} PercentFunded_i = {} & B_1 \cdot NumCategories_i + B_2 \cdot Before_i \\ & + B_3 \cdot (Before_i \cdot NumCategories_i) \\ & + B_n \cdot C_i + \varepsilon_i, \end{aligned}$$

where $PercentFunded_i$ is the percent funding that listing i received, $NumCategories_i$ is a count of the number of categories listing i had associated with it, $Before_i$ is an indicator variable set to 1 if listing i appeared before the removal of the labels and 0 otherwise (in essence, our treatment indicator), C_i is a vector of control variable covariates for listing i, and ε_i is the error term. B_3 is the coefficient of particular interest here because it estimates the change in the effect of the number of categories on the percent funding a listing received from *before* versus *after* the removal of the labels. Previous research suggests $B_1 < 0$, but in addition, here we expect $B_3 < 0$, indicating that the effect of belonging to more categories was more detrimental when labels were present.

Table 4 Summary Statistics ($N = 77{,}888$)

Variable	Mean	SD	Min	Max
Amount requested	7,016.22	6,665.19	1	25,000
Interest rate	0.179	0.072	0	0.36
Debt-to-income ratio	0.617	1.544	0	10.01
Homeowner (=1)	0.339	0.473	0	1
Current delinquencies	3.738	4.999	0	74
Credit rating (coded 1–8)	5.707	1.574	1	8
Income (coded 0–7)	3.263	1.227	0	7
Before label removal (=1)	0.477	0.499	0	1
Number of categories	0.771	1.711	0	5

Table 5 OLS Estimate of Percent Funding a Listing Received

Dummy	Model 1	Model 2
Religion	0.021*	0.022*
	(0.011)	(0.011)
Civic	−0.004	−0.005
	(0.011)	(0.011)
Sports	−0.011	−0.010
	(0.015)	(0.015)
Ethnicity	−0.007	−0.008
	(0.017)	(0.017)
Regional	0.030***	0.029**
	(0.009)	(0.009)
Military	−0.018	−0.018
	(0.010)	(0.010)
Education	−0.016	−0.016*
	(0.008)	(0.008)
Business	−0.058***	−0.059***
	(0.010)	(0.010)
Nonprofit	0.006	0.005
	(0.017)	(0.017)
Hobbies	−0.012	−0.012
	(0.009)	(0.009)
Art	0.073***	0.074***
	(0.016)	(0.016)
People	−0.014	−0.015*
	(0.007)	(0.007)
Science	0.034**	0.037**
	(0.014)	(0.014)
Other	0.005	0.005
	(0.009)	(0.009)
Not in a group	−0.121***	−0.121***
	(0.018)	(0.018)
Number of categories	−0.005*	−0.002
	(0.002)	(0.003)
Before label removal	−0.029***	−0.025***
	(0.003)	(0.003)
Before label removal × *Number of categories*		−0.004*
		(0.002)
Constant	0.213***	0.212***
	(0.015)	(0.015)
Observations	37,766	37,766
Adjusted R^2	0.010	0.010

Note. Robust standard errors are in parentheses.
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Results

Table 5 shows the results of regression models estimated on the listings from both group and nongroup members that occurred in the 200-day window around the removal of category labels that were matched as described earlier ($N = 37{,}766$). As a result of the matching process, these models are relatively parsimonious. However, we do include a set of dummy variables representing each of the major categories with which a group may be affiliated (i.e., geographic region, religion, ethnicity). We control for these categories to guard against the possibility that some categories may be viewed as objectionable by some people. Multiple-category listings, having more categories, would be more likely to include an undesirable one. We also include a dummy variable indicating whether the prospective borrower belongs to a group.

Model 1 in Table 5 indicates that listings from nongroup members tend to receive less funding than do listings from individuals in groups. More importantly for our theoretical interests, the significant negative effect of the variable indicating the number of categories demonstrates that multiple-category members tend to be devalued. As noted earlier, this is consistent with much prior work. However, this model alone does not allow us to distinguish whether the penalty is due to underlying differences between those listings for which the prospective borrower belongs to more categories or whether the negative effect is driven by confusion or other perceptual difficulties that multiple-category members tend to encounter. Model 2 incorporates the interaction between the indicator for being listed before label removal and the number of categories. In this model, the interaction term represents the penalty experienced by multiple-category members who listed before label removal. Note that the main effect of the number of categories now represents the devaluation of multiple-category members who listed after label removal. As we suggested, this variable is no longer statistically different from 0. In contrast, the interaction term is significant and negative ($\beta = -0.004$, $\chi^2 = 5.99$ (1, 37,747), $p < 0.014$). Borrowers incurred lower funding rates (0.4% less) for each additional category label that their group was associated with before label removal, but the penalty to multiple-category membership subsided once category spanning was less evident via the removal of category labels.

We attempted an even more rigorous examination of our hypothesis by limiting our sample to only group members. A concern with the models above could be that elimination of group labels differentially affects nongroup and group members, thereby leading to our findings. To guard against this, we resampled on listings, which have been matched on all the previously mentioned characteristics, and additionally matched on the specific group to which the prospective borrower belongs. We then ran models that include group-level fixed effects on this sample. Neither the dummies for major category labels nor the count of the number of categories are included because they are constant within a group. The drawback to this approach is the decrease in sample size (from 37,766 to 3,546 observations and from 757 groups to 164) as a result of the focus on only group members and more stringent matching. Yet these analyses represent the most conservative test of our arguments.

The results, reported in Table 6, show that listings were, on average, more likely to be funded if they occurred in the period prior to label removal. This is in contrast to the results in Table 5 but may represent some effect of unobserved heterogeneity across groups that was not captured in the prior analysis. More importantly, in Model 2, we include the interaction between the number of categories and the indicator of whether the listing occurred before label removal. This, as expected, is negative and significant ($\beta = -0.032$, $\chi^2 = 6.13$ $(1, 3{,}380)$, $p < 0.013$). Consistent with results reported earlier, those who span categorical boundaries incur more of a penalty when category spanning is made apparent via prominent labels.

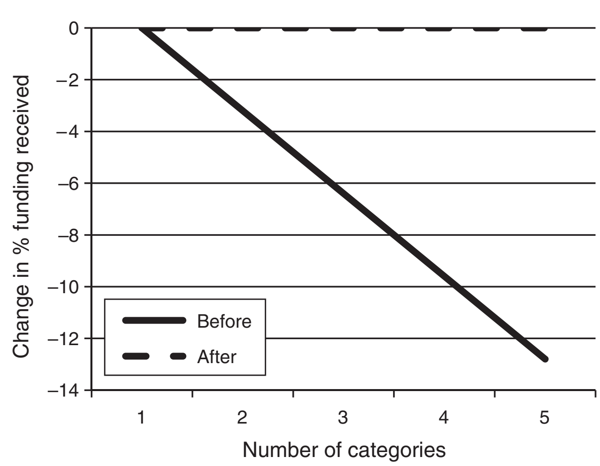
Table 6 **OLS Estimates of Percent Funding a Listing Received (Fixed Effects by Group on Matched Sample)**

	Model 1	Model 2
Before label removal	0.020*	0.164**
	(0.010)	(0.061)
Before label removal × *Number of categories*		−0.032*
		(0.013)
Constant	0.178***	0.179***
	(0.007)	(0.007)
Observations	3,546	3,546
Number of groups	164	164
Min	1	1
Max	488	488
Mean	20.800	20.800
Adjusted R^2	0.006	0.002

Note. Standard errors are in parentheses.
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

In Figure 4 we demonstrate graphically the effect of the number of categories on percent funding from before to after with results from Table 6. For comparison purposes, we have set the effect of being in a one-category group after the removal of the labels equal to 0. Each line represents the effect the number of categories has on the percent funding a listing receives, net of the factors of group membership. The linear probability model is straightforward to interpret, and we see that the change in the percent funded varies by the number of categories

Figure 4 **Change in Effect of Multiple-Category Membership to Percent Funding Received (Before vs. After Label Removal)**



a group member listing is affiliated with for the before period. Specifically, for each additional category, there is a corresponding 3.2% decrease in the amount of funding a listing receives. Note that the effect is amplified when we are comparing only group members versus the analyses in Table 5, which included nongroup members. Because nongroup members are likely not the appropriate comparison group, we feel that this estimate more accurately demonstrates the multiple-category penalty.

Given that any underlying quality differences between multiple-category members and others are the same across the time frame of our study, the fact that the penalty to category spanning is exacerbated when labels are present provides persuasive evidence that part of the penalty is perceptually driven. Taken together, the two analyses above provide support for the idea that perceptual difficulties underscore the penalty for category spanning and that such a penalty can occur even in the absence of underlying quality or capabilities-based differences.

Robustness Checks and Additional Considerations

A simple explanation for the observed results could be that the multiple-category group members are, in actuality, worse borrowers—that they defaulted at a higher rate. Under this scenario, the visibility of the labels signaled their riskiness, and rational lenders avoided them. However, with the removal of the labels, a valuable and correct signal as to a multiple-category member's inadequacies was no longer available to lenders, resulting in the observed pattern. Assuming that lenders would have learned of the poorer payback rate of the multiple-category members through previous observations of borrowers on the website, we addressed this concern by estimating the relationship between the likelihood a borrower defaulted on a loan and the number of categories he or she was affiliated with.

We tested this possibility by analyzing delinquency among all loans that were funded in the one-year period before label removal. We chose this period before label removal because prospective borrowers would need time to form their beliefs regarding category spanning and delinquency, thereby driving their behavior in our observation window. A loan was coded as 1 if at any time during its payback period the borrower was delinquent on his payments (basically, if payments were late by 30 days or more). We then modeled a logistic regression estimating the effect of the number of categories a borrower was affiliated with on his likelihood of being tardy in payments. Results are presented in Table 7.

Models 1 and 2 include both group and nongroup members, whereas Models 3 and 4 include only group members. All models include extensive controls for factors such as income, homeownership, and other lender characteristics that might influence delinquency and could be correlated with multiple-category membership. Note that merely being a member of a group increases the likelihood of delinquency ($\beta = 0.31$, $p < 0.05$) in Model 2, but this may be a selection issue as lower-quality borrowers may feel that they need to join groups. However, the variable of interest in these models is the number of categories with which a prospective borrower's group is affiliated. This variable is not statistically significant in any of the models, which can be interpreted as evidence that delinquency rates do not vary according to multiple-category membership. These results indicate that multiple-category members were not, in actuality, worse borrowers. Of course, these analyses do not rule out the possibility that people *think* categories spanners are worse loan risks. Rather, these analyses suggest that such a belief has no basis in fact.

Discussion

One of the principle sociological contributions to our understanding of markets is the idea that rewards are, in part, determined by one's social position rather than being solely a function of individual capabilities, skills, and effort (see White 1970, Podolny 1993, Sørensen 1996, Gould 2002). Recent work in economic sociology demonstrates that market actors tend to fare poorly when they attempt to spread their efforts broadly across categories. The devaluation of category spanners can be interpreted as positional to the extent that it stems from one's place in sociocognitive space (i.e., a consumers' or investors' mental map of a market) rather than from any underlying deficiency in performance. Although researchers in this vein have gone to great lengths to demonstrate that category spanning is penalized for reasons other than inferior performance, teasing apart these two sources of disadvantage has been challenging because they commonly co-occur and because it is difficult to know whether statistical controls for observable quality measures truly capture any underlying differences.

A natural experiment that resulted in the exogenous removal of category labels served as the basis for our test of whether the penalty for category spanning derives, in part, from perceptual issues. Using a natural experiment with a difference-in-difference strategy on a matched group of borrowers, we the devaluation of multiple-category members relative to their peers who belonged to fewer categories. Moreover, we found that the penalty for category spanning not only diminished but, in fact, subsided entirely when labels were not present to highlight the identity of category spanners. We investigated whether category spanners were any less attractive than others in terms of their default rates, the relevant performance dimension in this setting, and found that this was not the case. This leads us to conclude that, in this setting, category labels highlighted what turned out to be nonexistent performance differences. Although multiple-category members may, in some cases, be penalized for underlying quality differences, our analyses indicate that they will be devalued even in the absence of such issues.

Table 7 Logged Odds Estimates of the Likelihood of a Loan Becoming Delinquent

	Group and nongroup members				Group members only			
	Model 1		Model 2		Model 3		Model 4	
Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Amount requested	−4.1e−05***	(6.2e−06)	−4.1e−05***	(6.2e−06)	−4.7e−05***	(8.5e−06)	−4.6e−05***	(8.5e−06)
Borrower rate	−11.355***	(0.694)	−11.389***	(0.695)	−10.107***	(0.896)	−10.156***	(0.898)
Debt-to-income ratio	−0.024	(0.021)	−0.024	(0.021)	−0.023	(0.025)	−0.024	(0.025)
Homeowner	−0.104	(0.065)	−0.105	(0.065)	−0.047	(0.084)	−0.048	(0.084)
No. of delinquencies	−0.064***	(0.007)	−0.064***	(0.007)	−0.071***	(0.009)	−0.071***	(0.009)
A credit[a]	−0.286	(0.202)	−0.289	(0.202)	−0.311	(0.289)	−0.317	(0.289)
B credit[a]	−0.346	(0.184)	−0.347	(0.184)	−0.305	(0.270)	−0.308	(0.270)
C credit[a]	−0.710***	(0.172)	−0.712***	(0.172)	−0.668**	(0.251)	−0.671**	(0.251)
D credit[a]	−0.503**	(0.179)	−0.501**	(0.179)	−0.551*	(0.256)	−0.550*	(0.256)
E credit[a]	−0.550**	(0.193)	−0.546**	(0.194)	−0.648*	(0.273)	−0.641*	(0.274)
HR credit[a]	−1.175***	(0.195)	−1.169***	(0.195)	−1.311***	(0.274)	−1.303***	(0.274)
Income 2[b]	0.258*	(0.129)	0.251	(0.129)	0.215	(0.159)	0.206	(0.159)
Income 3[b]	0.313***	(0.081)	0.306***	(0.081)	0.288**	(0.100)	0.278**	(0.100)
Income 4[b]	0.398***	(0.100)	0.392***	(0.100)	0.391**	(0.126)	0.382**	(0.127)
Income 5[b]	0.389*	(0.152)	0.382*	(0.153)	0.230	(0.195)	0.218	(0.196)
Income 6[b]	0.406*	(0.166)	0.399*	(0.167)	0.419	(0.244)	0.406	(0.244)
Religion	−0.083	(0.134)	−0.101	(0.136)	−0.092	(0.134)	−0.108	(0.135)
Civic	−0.499***	(0.140)	−0.526***	(0.142)	−0.487***	(0.140)	−0.512***	(0.142)
Sports	0.016	(0.217)	−0.008	(0.219)	0.029	(0.218)	0.006	(0.219)
Ethnicity	0.551*	(0.248)	0.538*	(0.250)	0.514*	(0.249)	0.501*	(0.250)
Regional	−0.201	(0.118)	−0.232	(0.122)	−0.203	(0.118)	−0.232	(0.122)
Military	0.055	(0.141)	0.026	(0.144)	0.058	(0.141)	0.032	(0.144)
Education	0.220*	(0.108)	0.198	(0.110)	0.215*	(0.108)	0.194	(0.110)
Business	−0.249*	(0.109)	−0.306*	(0.122)	−0.257*	(0.109)	−0.309*	(0.122)
Nonprofit	0.337	(0.208)	0.303	(0.210)	0.336	(0.208)	0.305	(0.210)
Hobbies	−0.091	(0.113)	−0.100	(0.113)	−0.105	(0.113)	−0.114	(0.114)
Art	−0.162	(0.208)	−0.179	(0.209)	−0.156	(0.209)	−0.170	(0.209)
People	−0.175	(0.094)	−0.205*	(0.098)	−0.163	(0.094)	−0.190	(0.098)
Science	−0.140	(0.180)	−0.148	(0.181)	−0.136	(0.181)	−0.142	(0.182)
Other	−0.190	(0.117)	−0.193	(0.117)	−0.179	(0.117)	−0.182	(0.117)
In a group	0.391***	(0.117)	0.314*	(0.136)				
Number of categories			0.042	(0.038)			0.038	(0.039)
Constant	4.424***	(0.184)	4.431***	(0.184)	4.671***	(0.283)	4.610***	(0.289)
Observations	8,191		8,191		4,786		4,786	
Pseudo-R^2	0.1688		0.1689		0.1615		0.1617	
Log likelihood	−3,740.76		−3,740.16		−2,276.32		−2,275.83	
χ^2	1,518.87		1,520.06		876.74		877.72	

[a] AA credit is excluded.
[b] Income 1 is excluded.
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

We view this as strong evidence of the perceptual underpinnings of the multiple-category discount.

We note that the perceptually driven aspect of the penalty for category spanning can result either from cognitive confusion that audiences experience when they try to make sense of category spanners or from social sanctions that render boundary crossing taboo. Given the particular set of social categories involved in this setting, we suspect that the former mechanism drives our results. The categories in our setting were relatively banal descriptors that group leaders choose from drop-down boxes and applied to their groups. A priori, they do not appear to be the powerful, institutionalized forces that many researchers (e.g., Meyer and Rowan 1977, DiMaggio and Powell 1983) suggest social categories can represent. Yet even in a setting where transgression of the category structure might not be expected to entail severe social sanctions, we found negative effects stemming from seemingly cognitive processes.

In addition to providing evidence of the cognitive component of the multiple-category penalty, this finding underscores the role of category labels as identity markers that not only accentuate and amplify underlying differences but also may constitute them even when they are absent. Our belief is that the institutionalization of category systems provides a basis by which differences are delineated among market participants. Of course, this is not to claim that performance or quality differences do not exist between generalists and specialists in other settings. Rather, our results suggest that category labels are likely to exacerbate any underlying differences in quality, motivations, or other attributes. Future work should endeavor to demonstrate that categories matter in a more sociological sense by extending their reach beyond mere functional differences.

A point of departure that this paper takes from the study by Negro and Leung (2013) is in the micromechanisms driving the results. We both aimed to uncover perceptual reasons for the discount, but in their case, the nonblind wine critic (the Veronelli guide) professed to be biased against wineries that spanned the oppositional wine style categories. Specifically, Negro and Leung (2013) quoted a Veronelli critic as saying, "A producer's line of products should reflect a specific zone as well as that producer's philosophy. A winery is not a supermarket" (p. 689), thereby suggesting punishment and not confusion as the mechanism. As we noted earlier, the findings reported here are consistent with the extant literature in cognitive psychology, which suggests that ambiguous objects often arouse confusion. However, individuals make negative inferences about multiple-category members being less able or less committed (Durkheim 1893/1997), even when these inferences are incorrect. Building on the theoretical foundation developed elsewhere and strengthened empirically in this paper, future work should also attempt to more directly explore, at the micro level, how evaluators view and experience multiple-category members. It would be insightful to have more direct evidence on the extent to which confusion, compared with negative perceptions, accounts for the multiple-category penalty.

In addition, although our work has focused on documenting the challenges broadly faced by those who do not fit into the prevailing categorization system, it would be useful to explore different ways in which an actor might be perceived not to fit. Is it worse for a social actor to not fit into any category or to fit into too many? For example, is a film classified as a horror and comedy seen as being outside the category system altogether, or is it seen as a partial member of two potentially incompatible genres? How compatible are certain genres? More broadly speaking, it would be fruitful to examine whether there are certain relationships among categories that allow for some combinations to be perceived as more reasonable than others. Future research, for example, could explore this by measuring the impressions of multiple-category members in a more nuanced manner or conceptualizing varying distances among categories.

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Appendix

Table A.1 Credit Score Codes

Code	Credit score equivalent[a]
AA credit	760+
A credit	720–759
B credit	680–719
C credit	640–679
D credit	600–639
E credit	560–599
HR credit	520–559
NC credit	No credit score

[a]Credit scores are based on Experian Scorex PLUS; Experian is an independent credit scoring agency.

Table A.2 Income Codes

Code	Definition
Income 0	Not displayed
Income 1	\$0 or unverifiable
Income 2	\$1–\$24,999
Income 3	\$25,000–\$49,999
Income 4	\$50,000–\$74,999
Income 5	\$75,000–\$99,999
Income 6	\$100,000+
Income 7	Not employed

Endnotes

[1]Because niche-width theory assumes that all organizations have the same total capacity for performance, it is not applicable to situations where generalists enjoy economies of scale or scope (Hannan et al. 2007). The setting under consideration here meets that assumption.

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