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Jennifer Arlen
jennifer.arlen@nyu.edu

Matthew L. Spitzer
Columbia Law School, mspitzer@law.usc.edu

Eric L. Talley
Columbia Law School, etalley@law.columbia.edu

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ENDOWMENT EFFECTS WITHIN CORPORATE AGENCY RELATIONSHIPS

by
Jennifer Arlen, Matthew Spitzer and Eric Talley†

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Abstract

Behavioral Law and Economics has become an increasingly prominent field within legal scholarship, and most recently within the corporate area. A behavioral bias of particular relevance in corporate contexts is the differential between individuals’ willingness to pay to obtain a legal entitlement and her willingness to accept to part with one, known as the “endowment effect.” Should endowment effects pervade relationships within business organizations, it would significantly complicate much of the common wisdom within corporate law, such as the presumed optimality of ex ante voluntary agreements. Existing experimental research, however, does not adequately address whether and to what extent the endowment effect operates within corporate environments.

This Article presents an experimental test for endowment effects within a principal-agent relationship that typifies many firms. We find that subjects situated in an agency relationship do not exhibit a significant endowment effect. Using an additional experimental test, we argue that this dampening phenomenon is likely due to the fact that the agency context induces subjects to view property rights principally for their exchange value, thereby causing them to “disendow” their initial legal entitlements.

† Jennifer Arlen is Visiting Professor of Law, Yale Law School and Ivadelle and Theodore Johnson Professor of Law & Business, USC Law School; Matthew Spitzer is Dean and Carl Mason Franklin Professor of Law, USC Law School; and Eric Talley is Professor of Law, USC Law School and Visiting Professor of Law, California Institute of Technology. For helpful comments, we thank Scott Altman, Ian Ayres, Linda Babcock, Omri Ben-Shahar, Colin Camerer, Bill Carney, Rachel Croson, Sam Fraidin, Michal Gal, Rafael Gely, Henry Hansmann, Elizabeth Hoffman, D. Bruce Johnson, Ed Kitch, James Konow, Russell Korobkin, Lewis Kornhauser, Don Langevoort, Tom Lyon, Ed McCaffery, Jeffrey Rachlinski, Uriel Procaccia, Mark Ramseyer, Dan Simon, Lynn Stout, and participants at workshops at American University, Boalt Hall, Cincinnati, Cornell, Fried, Frank, Harris, Shriver & Jacobson, NYU, Michigan, Miami, USC, Virginia, Texas (GSB), the 2001 USC/Caltech Conference on Behavioral Law, Economics and Organizations, the 2000 annual meeting of the American Law & Economics Association, the 2000 UCLA Conference on Cognition, Emotion and Rational Choice, and the 1999 UCLA Law School Conference on Corporate Governance. This research was funded in part by a grant from the National Science Foundation (Grant No. SBR 973081). The views expressed herein are solely those of the authors and do not represent the views of the NSF.
INTRODUCTION

In recent years, behavioral law and economics (BLE) has risen to prominence within legal scholarship. Its ascension is due -- at least in part -- to its aim of unifying two historically distinct analytic paradigms: economics and psychology. While embracing the conventional economic premise that legal rules embody behavior-altering incentives, behavioral law and economics nevertheless disengages from neoclassical economic theory, adopting an account of human behavior in which people display numerous systematic, cognitive biases at variance with the predictions of rational choice theory.

BLE has fostered renewed academic skepticism about central tenets of law and economics that depend on strong rationality assumptions.\(^1\) This skepticism poses an important challenge to conventional corporate law scholarship, much of which is premised on the Coasean prediction that -- in the absence of significant externalities, information asymmetries, or garden-variety transaction costs -- the law can (and should) rely on private parties to allocate rights and obligations optimally.\(^2\) This prediction leads naturally to the prescription that corporate law should generally avoid imposing immutable (or “mandatory”) rules, except when necessary to address conventional market failures, and thus should be primarily confined to two tasks: (i) providing default rules that most rational corporate stakeholders would tend to favor ex ante, and (ii) providing doctrines for interpreting parties’ endeavors to contract around the default rules.\(^3\) By casting doubt on the accuracy of strong rationality assumptions, BLE raises the serious possibility that notwithstanding market failures, privately negotiated organizational relationships systematically diverge from efficiency. And, if such deviations are persistent, then the normative case for mandatory regulation is significantly larger than the conventional economic approach suggests -- resting not only on previously articulated market-failure considerations, but also on some form of measured paternalism.\(^4\)

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Although BLE spotlights a number of non-rational biases, one which has particular relevance for corporate scholarship is the frequently observed divergence between willingness to pay (WTP) and willingness to accept (WTA). While rational actor models generally assume that mere ownership or possession of an asset does not influence an individual’s valuation of it (once one controls for wealth and income effects), an increasing body of empirical and experimental evidence demonstrates that possession can affect value. Specifically, the maximum amount a non-owner would be willing to pay for an entitlement is often significantly less than the minimum amount she would demand to part with it if she initially owned it. This divergence between willingness to pay and willingness to accept has become known as the “endowment effect.”

The presence of endowment effects within firms would have important implications for legal rules governing business organizations. As noted above, it would undermine the premise of the Coase theorem that free, fully informed, unimpeded private bargaining necessarily results in the efficient distribution of property rights. Rather, an entitlement may remain “stuck” with its initial owner, even though another would value the entitlement more if she were initially endowed with it. As applied to specifically corporate scholarship, endowment effects undercut the widespread presumption that the “nexus of contracts” voluntarily entered into by informed parties is presumptively welfare maximizing, both immediately before and after execution. Indeed, this presumption leans heavily on preference stability – and in particular that initial possession does not affect valuation. When, in contrast, endowment does affect valuation, the agreements people enter into before they become endowed need not equate to those that maximize their welfare immediately once becoming endowed. Such a finding would have obvious implications for corporate law, since most corporate governance agreements are often executed prior to endowment,

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6 See Elizabeth Hoffman & Matthew Spitzer, Willingness to Pay vs. Willingness to Accept: Legal and Economic Implications, 71 WASHINGTON UNIVERSITY LAW QUARTERLY. 59, 99 (1993); Kahneman, Knetsch & Thaler, Experimental Tests, supra note 17. Indeed, the endowment effect undermines -- or at least complexifies -- the concept of social welfare itself, making it difficult to define the cost and benefit of reallocating an entitlement. See Hoffman & Spitzer, supra, at 103-112; see also Edward McCaffery, Daniel Kahneman, and Matthew Spitzer, Framing the Jury: Cognitive Perspectives on Pain and Suffering Awards, 81 VIRGINIA LAW REVIEW 3141 (1995).

7 The endowment effect is particularly likely to have an adverse effect on bargaining if, as evidence suggests, buyers and sellers do not fully anticipate the effect on either themselves. See George Loewenstein and Daniel Adler, A Bias in the Prediction of Tastes, 105 THE ECONOMIC JOURNAL 929 (1995)(suggesting people do not anticipate their own endowment effects); Leaf Van Boven, David Dunning, George Loewenstein, Trading Places: Egocentric Empathy Gaps Between Others and Buyers (unpublished manuscript 2000)(suggesting owners and buyers overestimate the similarity between their own valuation of an objects and that of others).
and yet govern the parties’ relationship subsequent to one (or more) parties becoming endowed. Thus, unanticipated endowment effects might undercut the existing rationale behind judicial deference toward corporate charters over later governance agreements (such as bylaws, collective bargaining agreements, and shareholder proposals). In addition, unanticipated endowment effects in managerial positions might provide insight into why managers of public corporations agree in good faith to charters that do not contain takeover defenses, only to seek the subsequent implementation of such defenses.

While widespread endowment effects would clearly have important implications for corporate law, any serious effort to incorporate endowment effects into normative policy prescriptions must contend with a problem of domain uncertainty. Existing evidence on the endowment effect suggests that it is pronounced in certain circumstances, but fails to manifest in others. Much like other deviations from rational choice, the existence and magnitude of the endowment effect is context dependent. Moreover, neither theory nor existing experimental evidence yields robust predictions as to whether people will manifest the endowment effect within business organizations. Thus, before commencing with normative or prescriptive theories based on the endowment effect, it is important first to ascertain whether the phenomenon transcends into the business organizational context.

This Article provides an experimental test of whether the endowment effect affects individual choices within business agency relationships: i.e., the “principal-agent” relationships that populate many business organizations. Specifically, we modify the classic endowment effect experiments to determine whether subjects situated in a business agency context display endowment effects in contracting with their employer. This is the first test of whether agents

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8 Of course, a normative utilitarian analysis allowing for endogenous preferences thus must specify a time frame at which welfare-relevant preferences are deemed to “matter” for efficiency computations (pre- or post-endowment).

9 Similarly, in the employment area, scholars have argued that endowment effects undermine the presumed validity of employment-at-will, on the grounds that, ex ante, employees voluntarily agree to an at-will employment relationship only because they fail to anticipate that, once employed, they will endow their positions, and thus value them more. Samuel Issacharoff, Contracting for Employment: The Limited Return of Common Law, 74 Texas Law Review 1783, 1800-1803 (1996); see Cass R. Sunstein, Human Behavior and the Law of Work, 87 U. Va. L. Rev. 205 (2001) (arguing in the context of at-will doctrines that “[s]o long as the right is initially allocated to one or another side, an endowment effect cannot be avoided”).


12 To be sure, legal rules regulate may types of agency problem, but we concentrate solely on the canonical “employer-employee” relationship within firms.
manifest an endowment effect in exchanges with their employers, to our knowledge.\textsuperscript{13}

Our central finding is that situating subjects in an agency context significantly dampens the magnitude (and indeed even the existence) of the endowment effect. Specifically, although our control subjects manifested an endowment effect similar to that detected in prior literature, our agency subjects manifested virtually no endowment effect whatsoever. This result holds even when we control for demographic differences among subjects.

We then conducted an experiment to assess why agency relationships mute the endowment effect. The existing evidence yields two central hypotheses as to why agency relationships might mute the endowment effect: (1) the agency relationship may cause subjects to focus on the exchange value of the asset, as opposed to its use value (which should mute endowment effects given evidence that subjects typically fail to manifest the effect for entitlements they hold primarily for exchange purposes), and (2) the agency relationship may mute endowment effects if agents exhibit other-regarding preferences towards the firm which cause them to disendow their own assets when needed by the firm. As our initial agency experiment design implicates both of these reasons, we ran a second agency experiment which was designed to implicate exchange-value concerns but not to given rise to significant firm-regarding concerns. Again, we found no statistically significant evidence of an endowment effect. These results are not only consistent with the findings of our initial agency experiment, but also suggest that the business context itself caused subjects to focus on the exchange value of the asset, thereby eliminating any statistically significant endowment effect.

Our results have both specific and general implications. Most directly, they counsel a degree of caution for those involved in developing normative policy prescriptions for corporate or employment law based on endowment effects. More generally, our results underscore an important tension between context-specific experimental research and broad-based normative inquiries, highlighting the need for experimental inquiries targeted to each specific policy domain. Nevertheless, while we did not find evidence of an endowment effect in a corporate agency context, this does not imply that corporate scholars can ignore behavioral economics altogether. On the contrary, there remains considerable evidence that certain cognitive biases profoundly affect some agency relationships. Thus, increased attention to cognitive biases by corporate scholars seems prudent, if accompanied by both a prudent dose of caution and, ideally, more primary research into the area.\textsuperscript{14} Along these same lines, it is important to note that our analysis is largely limited to agency relationships that resemble the employer-employee relationship. It may certainly be the case that in other types of agency relationship, the endowment effect resurfaces with significant force.

Our discussion consists of three parts. Section I reviews the empirical evidence and theories

\textsuperscript{13} Previous analysis has shown that agents do not manifest endowment effects when acting on behalf of their principal in exchanges involving the principal’s assets. See James D. Marshall, Jack L. Knetsch, and J.A. Sinden, Agents’ Evaluations and the Disparity in Measures of Economic Loss, 7 JOURNAL OF ECONOMIC BEHAVIOR AND ORGANIZATION 115 (1986). This could arise because agents fail to anticipate their principal’s endowment effects. By contrast, we examine agents’ behavior when contracting on their own behalf.

\textsuperscript{14} See, e.g., sources in infra note 79, and the literature discussed in Donald Langevoort, Organized Illusions, supra note 4.
underlying the endowment effect, focusing on current literature. Section II describes our control and initial agency experiments and presents our results. Section III presents the results of our “exchange value” agency experiment, tentatively positing some implications that may flow therefrom.

I. ENDOWMENT EFFECTS: EXISTING THEORY AND EVIDENCE

This section examines the existing evidence on the endowment effect to assess its implications for corporate law scholarship. We find that there is considerable evidence that people manifest this bias in a number of different circumstances, including in certain market settings. Nevertheless, both the theory and evidence present reasons to expect that corporate participants (employees, managers, and shareholders) might not generally manifest this effect in their intra-firm exchanges.

A. Evidence of the Endowment Effect

A vast experimental and theoretical literature exists on the endowment effect. The classic experiment demonstrating the endowment effect involved Cornell coffee mugs, which sold at the nearby university bookstore for approximately $6 (as was indicated by price tags on the bottom of the mug). The experimenters randomly distributed the mugs to half of their subjects. Each participant was then told she could sell/buy a mug by stating a sale/purchase price which was then given to the experimenters. The experimenters then determined the market clearing price, and all those who made offers equal to or above that price participated in an exchange.

Economic theory predicts that, since the mugs were distributed randomly, half the mugs would trade, moving from those who valued mugs less to those who valued them more. Yet in fact, few mugs traded because on average those endowed with the mugs valued them significantly more than those who were not endowed. Indeed, while the median buyer was not willing to pay more than $2.25-$2.75 to purchase the mug, the median seller required $5.25 to part with her mug. Thus, in apparent contrast with rational choice theory, evidence suggested that each mug owner came to value the mug more simply because she now owned it.

Efforts to discredit the basic endowment effect result have largely failed. Indeed, experimental evidence of the effect has now transcended numerous experimental settings, using a variety of items -- such as coffee mugs, chocolate bars, and pens -- as well as subjects from

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15 This result could not be fully explained by wealth effects, as the mugs did not significantly affect the participants’ total wealth. Nor was it likely the product of transactions costs, as equivalent mugs were easily available a short distance away at the campus bookstore. See generally Id.

16 Indeed, a recent ambitious analysis of the existing evidence based on combined data from 45 existing studies found that both experimental tests and empirical studies of real world choices consistently reveal evidence of an endowment effect. John K. Horowitz and Kenneth E. McConnell, A Review of WTA/WTP Studies, Department of Agricultural and Resource Economics, University of Maryland, College Park MD 20742-5535 (October 2000).
different countries.  The endowment effect also persists when subjects are repeat players within the experiment.  Similarly, empirical studies of real world exchanges also reveal evidence of an endowment effect. Thus, the endowment effect appears to be a genuine phenomenon, and not simply the product of faulty experimental design.

B. Scope of the Endowment Effect

Existing evidence that individuals frequently exhibit endowment effects does not necessarily imply that corporate law must take this effect into account, however. Evidence that people can exhibit endowment effect does not mean that they invariably do so. Indeed, quite the contrary: the existing evidence suggests that people display endowment effects in some circumstances but do not do so in others. This raises the issue, do endowment effects exist within the corporate context?

Existing theoretical explanations for the endowment effect, unfortunately, do not enable us to predict confidently when the effect will arise. Economists, psychologists, legal scholars, and others have offered alternative (and often overlapping) explanations for the endowment effect, each of which may have a claim to some veracity. These different theories produce different domains for the endowment effect, as well as different implications for legal policy.

Indeed, some theorists argue that one can account for the WTA/WTP differential within the conventional rational-actor paradigm. Perhaps the best known of these rational actor explanations posits that the WTA/WTP difference occurs when individuals perceive the good to partially unique (e.g., not to have a perfect consumption substitute) and are unable to easily trade the good through market transactions. This explanation predicts that the effect should not occur if a good has perfect substitutes, or if thick markets in the good exist such that individuals can treat the good first and

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18 See Jason F. Shogren, Seung Y. Shin, Dermont J. Hayes, and James B. Kleibenstein, Resolving Differences in Willingness to Pay and Willingness to Accept, 84 AMERICAN ECONOMIC REVIEW 255 (1994) (repeated markets utilizing incentive compatible bidding systems does not eliminate the endowment effect for nonmarket goods).

19 In fact, the evidence suggests that the endowment effect manifested by the public generally exceeds that manifested by the college students. See Horowitz & McConnell, supra note 15.


foremost as a simple store of value, rather than as a consumption good with some uniqueness attributes.22

If the uniqueness hypothesis were accurate, it would be much easier to predict the endowment effect, because then the phenomenon would co-vary with other objectively verifiable factors, such as the presence of thick markets in a good. Yet existing experimental evidence lends only partial support for the uniqueness conjecture.23 In particular, although individuals tend to exhibit no WTA/WTP differential for goods that have only an exchange value (such as money),24 and exhibit only a modest one for “ordinary market goods,”25 most evidence indicates that the presence of close substitutes merely reduces, but generally does not eliminate, the WTA/WTP differential.26 Thus, the endowment effect apparently is not simply a product of non-substitutability.27

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22 Hanemann, supra note 20.

23 See, e.g., Horowitz and McConnell, supra note 15 (broad literature survey suggests that, on average, the less the good is like an “ordinary market good,” the higher is the WTA/WTP ratio).

24 The best individual evidence for this generalization comes from experiments on “securities” – paper instruments created by experimenters that have no value other than to be redeemed for cash, frequently after some random event (such as drawing a ball from an urn) determines the amount. Broadly construed, such experiments suggest that when comparing securities and cash, WTA and WTP tend strongly to converge. Kahneman, Knetsch & Thaler, supra note 16; Peter Knez, et. al., Individual Rationality, Market Rationality, and Value Estimation, 75 AM. ECON. REV. 397 (1985); Harinder Singh, The Disparity Between Willingness To Pay and Compensation Demanded: Another Look at Laboratory Evidence, 35 ECON. LETTERS 263 (1991); Gary H. McClelland & William D. Schulze, The Disparity Between Willingness to Pay Versus Willingness to Accept as a Framing Effect, in FRONTIERS OF MATHEMATICAL PSYCHOLOGY: ESSAYS IN HONOR OF CLYDE COOMBS (Donald R. Brown & J.E. Keith Smith eds., 1991).


26 See, e.g., Wiktor L. Adamowicz, Vinay Bhardwaj, and Bruce Macnab, Experiments on the Difference Between Willingness to Pay and Willingness to Accept, 69 LAND ECONOMICS 416, 425 (1993)(finding that WTA/WTP is larger for nonmarket goods than for market goods, but nevertheless finding a WTA/WTP differential for all types of goods).

In addition, some scholars cite experimental evidence of a difference between WTA and WTP for lotteries as evidence that the endowment effect operates for market goods. See, e.g., Jack Knetsch and J.A. Sinden, Willingness to Pay and Compensation Demand: Experimental Evidence of an Unexpected Disparity in Measures of Value, 99 QUARTERLY JOURNAL OF ECONOMICS 507 (1994) (finding a WTA/WTP differential for lottery/raffle tickets with small stakes). Question arise, however, whether the lottery valuation results are driven by endowment effects or by biases in the evaluation of uncertain choices, however. See Don Coursey, John Hovis, William Schulze, The Disparity Between Willingness to Accept and Willingness to Pay Measures of Value, QUARTERLY JOURNAL OF ECONOMICS 679, 680 (1987).

27 Additional evidence exists which is inconsistent with rational choice explanations for the endowment effect. For example, subjects who are only given a voucher that can later be exchanged for a mug – and not the actual mug – exhibit a weak endowment effect. See Kahneman, Knetsch & Thaler, supra note 16, at 1342 n. 7. (1990). In addition, the evidence suggests that duration of ownership appears to affect valuation -- even when duration does not convey new
Cognitive psychologists tend to attribute the endowment effect not to traditional rational choice, but rather to “loss aversion” — the tendency for people to attach greater importance to losses than to gains. 28 While descriptively powerful, this psychological explanation does little to predict when the effect will occur in practice: simply concluding that a person “endowed” with a good exhibits loss aversion adds little to our understanding of which circumstances induce a person to “endow” a good in the first instance.29

Some have attempted to give additional content to the concept of loss aversion by conjecturing that subjects exhibit loss aversion -- even with respect to a university coffee mug they have held for only a short time -- because they consider the future use value of this particular mug. This might ground loss aversion largely in the attributes of the object in the subject’s possession -- its future use value.

Yet the evidence suggests, however, that whether a subject endows an object depends as much on the psychological context of the choice than on any objective aspects of the asset. For example, researchers have found that subjects’ psychological sense of “endowment” depends not only on the legal fact of ownership, but also on whether subjects feel their ownership of the good has an independent moral justification. Subjects who were told they had “earned” -- or deserved -- an object exhibited more of an endowment effect than those who were told they had won the object in a game of chance.30 Nevertheless, the evidence does suggest that whether an object is viewed primarily for its consumption value or its exchange value does affect the endowment effect, although this can depend as much on the context of the exchange as on the attributes of the asset.

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29 See, e.g., Arlen, *supra* note 11, at 1777-78; Rachlinski & Jourden, *supra* note 19, at 1557; Cass Sunstein, *Behavioral Analysis of Law*, 64 UNIVERSITY OF CHICAGO LAW REVIEW 1175, 1180 (1997). Given the uncertain domain of the effect, and the resulting need for experimental analysis, we need not take any position in this paper as to which theory best explains the endowment effect. Each leave open the question of “under what circumstances do subjects feel endowed?” This experiment is designed to shed additional light on this issue.

30 See George Loewenstein and Samuel Issacharoff, *Source Dependence in the Valuation of Objects*, 7 JOURNAL OF BEHAVIORAL DECISION MAKING 157 (1994) (reporting this result in an experiment with coffee mugs); see also Elizabeth Hoffman and Matthew Spitzer, *Entitlements, Rights and Fairness: An Experimental Examination of Subjects’ Concepts of Distributive Justice*, 14 JOURNAL OF LEGAL STUDIES 259 (1985) (telling subjects they were entitled to an object affected their willingness to behave selfishly in exercising their property rights in different circumstances); and Elizabeth Hoffman, Kevin McCabe, Keith Shachat, and Vernon Smith, *Preferences, Property Rights, and Anonymity in Bargaining Games*, 7 GAMES AND ECONOMIC BEHAVIOR 346 (1994) (same).
For example, a field experiment testing subjects’ willingness to trade sports memorabilia found that while nondealers generally manifested a significant endowment effect in sports memorabilia, dealers in sports memorabilia did not. Nevertheless, nondealers with substantial trading experience behaved more like dealers than like other nondealers. Thus, subjects who may endow a particular good in a context where its consumption value is salient, may not do so in other contexts, particularly if they have come to focus on its exchange value.

C. Implications for Agency Relationships

The existing literature thus leaves unresolved the issue of whether business agents likely manifest endowment effects in intra-firm exchanges. On the one hand, evidence that people manifest endowment effects with respect to University coffee mugs even when told they are buyers and sellers suggests the endowment effect may exist in a business context. On the other hand, the experimental evidence provides two reasons to expect that perhaps agents do not manifest endowment effects in intra-firm exchanges.

First, the evidence that subjects typically fail to manifest the effect for entitlements they hold primarily for exchange purposes raises the possibility that endowment effects will not arise in intra-firm exchanges if those entering agency relationships view their contractual rights primarily for their exchange value (“exchange value hypothesis”).

Second, evidence that subjects are less likely to endow an asset to which they do not feel “entitled” suggests that agents might not manifest an endowment effect if a sense of loyalty or obligation to the firm mutes their sense of entitlement (“shared entitlement” hypothesis). In particular, agents might not endow if entering into an agency relationship causes them to feel the firm in entitled to the assets implicated in the relationship, resulting in a sense of shared or incomplete entitlement. This would be particularly likely to mute the endowment effect where an agent’s decision whether to keep the asset affects the firm’s welfare.

To assess these possibilities, we conducted an experimental test with subjects situated in an agency context. It is to this analysis we now turn.


32 See Kahneman, Knetsch & Thaler, supra note 16, at 1337-1338.

33 The Exchange value hypothesis -- that situating subjects in an agency context might mute the endowment effect by causing them to focus on its exchange value -- is potentially consistent with evidence that situating subjects in a hypothetical “business” context mutes other-regarding behavior. See Hoffman, McCabe, Shachat, & Smith, supra note 29, at Table II, page 357; K. Binmore et. al, Testing Noncooperative Bargaining Theory: A Preliminary Study, 75 AMERICAN ECONOMIC REVIEW 1178 (1985).

34 See Loewenstein & Issacharoff, supra note 29.

35 This would be particularly likely to the extent that agents manifest other regarding preferences towards their principal, as fiduciary duty law asserts they should (but economic principal-agent models generally assume they do not).
II. THE EXPERIMENT

To examine whether agency relationships alter the endowment effect, we conducted a series of experiments to test the robustness of the endowment effect result to situating subjects in an agency relationship involving intra-firm exchanges. In each set of experiments, subjects were differentiated by whether they were endowed with an asset at the beginning of the experiment or not. The initial “control” experiment replicated prior experiments on the endowment effect. Subjects participating in this experiment exhibited significant endowment effects consistent with prior research. We then conducted a pair of “agency” experiments in which subjects were told they were to be employed by a for-profit firm, and that they would face a choice between keeping/obtaining the tangible good and receiving higher monetary wages. They also were told that the good was a potential factor of production for the firm and that the firm’s profits would be higher if it used the good. After choosing whether to join the firm, those that did then determined whether to keep/retain the asset or obtain a pre-specified higher monetary wage (with endowed subjects deciding whether to give up the asset and unendowed subjects deciding whether to take the firm’s asset). We determined the subjects’ WTA or WTP by eliciting their willingness to exchange the mug for money (the higher wage offered varied), conditioned on whether they were initially endowed. Our central finding is that, in contrast with our control subjects, our agency subjects did not manifest a statistically significant endowment effect. This is evidence of a significant interaction between endowment effects and the principal-agent context which operates to mute -- perhaps eliminate -- the endowment effect. This section describes these experiments and presents our results.

A. Experimental Procedures

The experiment involved a total of 180 subjects: 35 participated in a control experiment establishing the endowment effect in a traditional setting, and the remaining 145 subjects participated in a series of experiments designed to explicitly test for endowment effects in agency relationships. All subjects were first-year students at USC Law School who participated either in the three weeks before entering law school or in the first few weeks of their first semester of law school. The subjects were roughly equally divided between men and women, with 94 women in our total pool of 180. All subjects were assigned a subject I.D., and were truthfully informed that all data were to be recorded only by subject I.D. number, thereby preserving anonymity.

As in the classic endowment effect experiments, our subjects were randomly divided between those who were endowed with a university (USC) coffee mug at the beginning of the

36 The final experiments were run in Fall, 2000. We also collected data on a different, earlier, version of the experiment run between in March and May, 1999, with over 100 second- and third-year students comprising the subject pool. The results of that prior experiment appear largely consistent with those we report here.

37 Throughout the main part of the experiment (when subjects were deciding on whether to exchange the mug) the experimenters stayed in the front of the room where we could monitor the progress of the experiment but could not see the subjects’ answers to specific questions.
experiment, and those who were not. Consistent with the literature, the identical mugs were purchased from the nearby campus bookstore, sported a familiar school logo, and were clearly labeled as being available at the bookstore for $5.95. 38 We selected university mugs over other possible tangible assets because prior work had already established a relatively reliable baseline for such mugs. Thus mugs were particularly suited to testing the robustness of prior results to situating subjects in a business agency relationship. 39

To ensure that all subjects understood the experiment, each received a written description of the experiment at the outset. We also read this description aloud. In addition, in the agency experiments, each subject participated in two practice rounds of the experiment following the description and then answered a series of questions which tested whether the subject understood the experiment. (At the beginning of the experiment we told subjects that they would not be permitted to complete the experiment if they could not answer the test questions adequately). 40

In all experiments, each endowed subject was given a mug at the very beginning of the experiment, prior to the consent form being read. The mug rested in front of the subject for the approximately thirty minutes of explanation and testing prior to the subjects participating in the main part of the experiment, and remained with the subjects during the experiment. Previous experiments have shown this is more than long enough to produce an endowment effect. 41 The unendowed subjects were shown a mug at the beginning of the experiment but were not endowed

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38 Easy availability is important to ensure that we are testing endowment effects, as opposed to the transactions costs to an endowed subject of obtaining another mug should she sell one in this experiment.

39 We employed a mug because previous literature has shown the endowment effect is sensitive to the type of asset used. Thus, to help ensure that any results we obtain were the product of a change in context -- as opposed to a change in the type of asset -- we employed the same asset as previous experiments. Needless to say, had we found an endowment effect, we would then have needed run additional experiments with assets that more closely approximate those involved in business agency relationships before we could conclude that the endowment effect affects agents exchanges with their employers. While such a research agenda is worthwhile, we concluded that at this point the more pressing policy issue is to determine the degree to which experimental results are robust to being situated in the agency context.

40 Prior to beginning the formal experiment, each subject also answered a series of demographic questions -- including age, gender, race, marital status (single, married, co-habiting), number of siblings, whether English was the subject’s native language, undergraduate major, and a question designed to measure their attitude toward risk. This information was on a sheet of paper identified only with the subject’s I.D. We coded for gender because it has been shown to influence endowment effects in other settings. See, e.g., Michael Strahilevitz and George Lowenstein, Gender Differences in the Effect of Duration of Ownership on Object Valuation (unpublished manuscript, 2000). We coded other demographic variables because research has shown that variables such as undergraduate major affect various biases. See, e.g., Robert Frank, Dennis Regan & Tom Gilovich, Does Studying Economics Inhibit Cooperation? 7 JOURNAL OF ECONOMIC PERSPECTIVES 159, 160-12 (Spring 1993); Robert Frank, Dennis Regan & Tom Gilovich, Do Economists Make Bad Citizens? 10 JOURNAL OF ECONOMIC PERSPECTIVES 187 (Winter 1996).

41 Evidence suggests that subjects endow objects quite quickly -- after possessing them for a very short period of time (e.g., five minutes). For women, the endowment effect appears to increase with duration of ownership. Strahilevitz & Lowenstein, supra note 39.
with it. Each subject received $5 simply to participate in the experiment. The “control group” subjects were told that if they completed the experiment they would receive additional remuneration, ranging from $1-$9 in cash, or, potentially, a mug, depending on the choices they made during the experiment. The agency subjects were told they faced potential additional remuneration of $2-$15 and/or, potentially, a mug, depending on the choices they made during the experiment. Prior experimental literature indicates that these payoff levels are sufficient to induce subjects to care about the choices they make.  

### B. The Control Group

The 35 control group subjects participated in an experiment designed to replicate the basic endowment effect experiments, in order to ensure that our subjects would indeed manifest an endowment effect with respect to a USC coffee mug outside of the agency context. These control subjects were divided into two, essentially equal, groups: “endowed” (18) and “unendowed” (17). The endowed subjects were given a mug at the beginning of the experiment; the unendowed were not. Each group was presented with a piece of paper which asked them in a series of questions to choose between the mug and a stipulated amount of money. Each endowed subject was asked whether she would “sell” the mug at each specified amount. Each unendowed was asked whether she would “choose” the mug or the money at each specified amount. The stipulated prices ranged from $1 to $9, in $.50 increments.

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42 Each subject participated in only one version of the subject (we did not allow any subject to participate in the endowed version and then later in the unendowed version). This is important because evidence suggests that past ownership of an object -- for example, in a prior endowed experiment -- affects unendowed subjects’ behavior. See Strahilevitz & Lowenstein, supra note 26.


44 The subjects were not told the name of the groups that they were placed in.

45 Specifically, the endowed subjects in the control group were given the following instructions:

- If the price is $1
  - I will keep the mug
  - I will sell

- If the price is $1.50
  - I will keep the mug
  - I will sell

- If the price is $9
  - I will keep the mug
  - I will sell

The unendowed subjects received similar instructions, though these asked them to “choose” between a money and mug
The subjects were told that at the end of the experiment a dollar amount would be selected randomly, and each subject’s choice at that amount would be effectuated. Thus, if at the end of the experiment the amount $5.50 was selected randomly, then each subject would receive either $5.50 or a mug depending on whether she had indicated a preference for the mug or $5.50 on the choice sheet.

As in previous experiments, we tested for the endowment effect by comparing the price at which our endowed subjects were willing to exchange the mug for money with the price at which our unendowed subjects opted for money instead of the mug: in other words, we compared the WTA of our endowed subjects with the WTP of our unendowed subjects.

Consistent with prior evidence, our control group manifested a pronounced endowment effect. As shown in Figure 1, the mug contribution/rejection rate is consistently greater for unendowed subjects than for endowed subjects at virtually every price. For example, in order to induce 50% of the subjects to select money instead of the mug, it would be necessary to offer endowed subjects approximately $5, whereas a payment of only just over $2 was sufficient to induce 50% of the unendowed subjects to pass up the mug -- thus, the endowed subjects required a premium of approximately $3 to induce a 50% contribution rate. Moreover, at the modest $2 price at which approximately half the unendowed subjects chose cash, more than 90% of the endowed subjects opted to keep the mug over the money. Overall, the median WTA of endowed subjects ($4.50) exceeded the median WTP of unendowed subjects ($2.50) by fully $2.00.

C. Initial Agency Experiment

Having established that our subject population displays endowment effects in the standard experimental context, we next examined whether these effects persist when subjects are placed in

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which they did not yet own (as opposed to assessing sale prices). These instructions are based on Kahneman, Knetsch and Thaler, supra note. We used this specification rather than the instructions from the original endowment effect experiment because of the possibility of strategic behavior inherent in the original formulation.

46 Specifically, subjects were told that slips of paper representing each listed monetary amount would be put into a hat and a subject would draw an amount from the hat.
a corporate agency context. Our initial hypothesis was that corporate agency relationships would mute the endowment effect for one of two principal reasons. First, agents of corporations might not display such effects if transacting with the firm causes them to focus on their entitlements as units of exchange, rather than objects with consumption value (the *exchange value* hypothesis).\(^{47}\) Second, agents’ sense of loyalty to their corporate employers might cause them to feel that the firm is somehow also “entitled” to assets they own that might benefit the firm, thus muting the endowment effect by causing endowed subjects not to feel fully entitled to their own assets (the *shared entitlement* hypothesis). To test whether either or both of these effects in business agency relationships dampened the endowment effect, we altered the experiment introduce an employment relationship. We describe the specifics of this experiment below.

1. Detailed Description of the Experiment

The agency cost experiment involved 69 subjects: 29 “endowed” subjects who were given a mug at the beginning of the experiment and 40 “unendowed” subjects. Unlike the control, in this experiment subjects were informed that they were being offered a managerial job by a for-profit corporation, Amalgamated Products, and that the mug was a potential factor of production for the firm.\(^{48}\) In each round of the experiment, subjects received a contractual offer to work as a manager for the firm. The contract was contingent on the firm’s performance, however, and would pay a different amount of money depending on whether the firm earned a low profit or a high profit. The amount offered if the firm earned a low profit was always $2, but the amount offered if the firm earned a high profit (the “high wage”) varied from $3 to $15.

Subjects were told that whether the firm earned low or high profits depended, in turn, on whether the firm used a mug as a factor of production. Each endowed subject was told that the firm could only use a mug if the subject parted with her own. Each unendowed subject was told she would be given control of the firm’s mug, and could either leave the mug with the firm or take for it for herself. Each subject was told that if the firm did not use the mug, profits would be low, and the subject would get a wage of only $2. By contrast, should the subject let the firm use the mug, profits would be high and the subject would earn the higher wage, specified in the contract during that round.

Thus in each round the subjects faced two choices. The first decision was whether to agree to work for the firm.\(^{49}\) Any subject who refused the contract, would earn nothing for that round. (The endowed subjects could keep the mug; the unendowed obviously could not). All subjects who

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\(^{47}\) This might be particularly true to the extent that agents expect to use their assets as a factor of production.

\(^{48}\) To gain insight into the robustness of prior experimental results to the agency context we purposefully situated our subjects in a very “thin” agency relationship -- a fictitious firm which made unilateral offers on pre-printed pages (with no human representative. Evidence that such a weak relationship altered experimental results would suggest that these results are not robust to the agency context, and would provide strong evidence that endowment effects would be altered by the stronger agency relationships prevalent in the “real world.”

\(^{49}\) We selected a two-stage decision process because we were interested in whether the act of first committing to work for a firm -- knowing the firm needed the mug as a factor of production -- might depress the endowed subjects’ sense of entitlement, thereby reducing or eliminating the endowment effect.
agreed to work for the firm then faced a second choice: whether to allow the firm to use the mug as a factor of production in return for a higher salary or whether to obtain the mug for themselves. For the endowed subjects, this effectively involved a decision of whether to sell the mug to the firm for a specified higher wage; for the unendowed subjects, this involved a decision of whether to claim the mug from the firm in return for a specified lower wage.

Each subject participated in eight rounds of the experiment. Subjects were told that their actual compensation from the experiment would include their earnings from one of the eight rounds, selected at random at the end of the experiment. We emphasized to the students that their choices in one round would not affect their options in other rounds. To make this promise credible, each subject was handed a package representing all the questions they would be asked in advance, so that they would feel confident that the actions taken would not affect the other bids they would be offered. Moreover, in order to avoid one subject’s choices influencing others’, the subjects were seated apart from each other. The high wage offers in each packet were also presented in a different order for each subject, so that in any given round no two subjects seated near each other had the same offer. Upon the completion of the final round, we used an eight-sided die to choose the round that would determine the subjects’ actual payoffs. The subjects then accepted payment in cash, and, if they had chosen to contribute their mug to the firm in the selected round, the subject also handed her mug back to the experimenter.

This design allows us to impute each subject’s approximate valuation of the mug at the end of the eight rounds. First, note that for either group, a strategy of accepting the contract and keeping the mug strictly dominates rejecting the contract. By rejecting the contract the subject gets either the mug (if endowed) or nothing (if unendowed), but by accepting the contract and then keeping the mug the subject can guarantee himself the mug plus $2. Thus, we predicted -- correctly -- that virtually all subjects who understood the experiment would accept the contract in every round. The principal choice of interest for those accepting the contract, then, is whether to choose the package outcome of {low wage, mug} or the alternative allocation of {high wage, no mug}. By varying the high wage offers by round, we were determine the critical wage $w_h$ that would just induce subjects in each condition to choose the non-mug allocation, thereby allowing the firm to use the mug in production. Subtracting the $2 baseline wage from this amount yielded an estimate of the subject’s implicit valuation.

Evidence that the endowed subjects required a greater payoff to part with the mug than

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50 Each subject also received $5 for simply participating in the experiment.

51 In addition, in order to ensure that students’ answers were not affected by future questions, we told students they must all proceed at the same pace, could not turn a page unless told, and then carefully monitored whether they did so (given the set up in the rooms, this was easily done). Moreover, we accurately informed students that although each student was given the same set of contracts, the order in which the contracts were presented to the students varied across students, to ensure that students would not adjust their answers based on those given by their neighbors. Also, the “high wage” contracts were not done sequentially: a subject might well receive a $3 “high wage” offer, followed by a $15 one, and then a $4 one.

52 Interestingly, there were a very few subjects who rejected the contract in one or more rounds. Though these subjects are excluded from the current paper (on the basis of assumed inconsistency), one might be able to tell a convincing story for leaving them in. We shall address this surprising phenomenon in future work.
unendowed subjects would require to abstain from claiming it would be consistent with subjects displaying endowment effects in this context.\textsuperscript{53} A lack of significant difference between the respective wage offers required to induce the two groups to part with (not take) the mug would thereby constitute evidence that the agency context interacts importantly with the endowment effect, former significantly dampening its magnitude.

2. The Results

In contrast with our control group, our agency subjects did not display a significant endowment effect. Thus our data suggests a significant interaction between the agency context and endowment effects, with the business agency context dampening or eliminating the impact of initial ownership on implicit valuations. We detail these conclusions below.

To evaluate our results, it is perhaps most helpful to examine the analog to Figure 1 above, and thus to examine the relative frequency with which the endowed and unendowed subjects passed up possession of the mug, conditional on the high wage offer. This is shown in Figure 2: The horizontal axis of Figure 2 depicts the difference between the wage offered to the subjects in the “high payoff” and the “low payoff” states of the world, respectively. The vertical axis depicts the empirical frequency with which subjects chose the allocation that left them without a mug.\textsuperscript{54} As before, the Figure also illustrates the trends in each cell through use of a 2-period moving average

Examination of Figure 2 suggests that situating subjects in the agency context had a significant effect in muting the endowment effect. In contrast with the control experiment – in which the endowed subjects valued the mug on average $1.57 more than the unendowed subjects -- the difference in relative valuations evidenced by our endowed and unendowed agency subjects are nowhere near that level, and indeed do not appear to be of particular economic significance. On first blush, then, this figure provides preliminary evidence that our subjects did not manifest an economically significant endowment effect -- and certainly nowhere near the effect found in previous experiments (if at all). Indeed, the two samples display an identical median valuation of $3.00.

Examination of the summary statistics are consistent with this conclusion. The slight numerical difference between WTA and WTP in our agency condition subjects is not statistically

\textsuperscript{53} Such a finding would only have been consistent with such a result, because there would have been other possible explanations – for example, that subjects felt guilty about “taking” the firm’s mug, which would result in unendowed subjects placing a particularly low value on the mug. Since guilt would only exacerbate a valuation differences between our endowed and unendowed subjects, we need not control for it given our result of no significant differences in the behavior of the two groups.

\textsuperscript{54} Thus, for the endowed condition, the vertical axis depicts the frequency with which subjects “contributed” their mug; for the unendowed condition, it depicts the frequency with which subjects “abstained from taking” the firm’s mug; and for the Exchange value condition, it depicts the frequency with which subjects chose the no-mug/high-payoff contract.
different from zero.\textsuperscript{55} Specifically, examining the subjects’ implied valuations, we found that the endowed subjects had a mean WTA of $3.08, where as the unendowed subjects had a mean WTP of $2.95. These means are not statistically significant at any conventional significance level (two-tailed t-test $p=0.806, df=67$). Likewise, a non-parametric two-sample Wilcoxon rank sum test fails to reject the null hypothesis of distributional symmetry ($z=0.025, p=0.9803$).

Although these summary statistics are relatively powerful themselves, we employed regression analysis to confirm that the dampening effect observed in our agency analysis was not the result of some other observed demographic trait. Our primary question is whether the endowed and unendowed subjects exhibit significant cross-group difference, controlling for wage (and possibly other factors). In order to make full use of our data structure, we first estimated the following basic relation:

$$Pr \{\text{NoMug}_{ij} = 1\} = Pr\{ \gamma'X_{ij} + \varepsilon_{ij} > 0\},$$  

(1)

where the left-hand side denotes the probability that a randomly-selected subject (subject $i$) in a randomly-selected round (round $j$) will choose to part with the mug ($\text{NoMug}_{ij}=1$); and the right-hand denotes the probability that a linear combination of independent variables for that subject/round ($X_{ij}$) representing subject characteristics (e.g., wage offers, subject treatment dummy variables, and demographic characteristics) with a set of coefficients ($\gamma$); summed with an error term ($\varepsilon_{ij}$) exceeds zero, and in which $\varepsilon_{ij}$ has a specified distribution. The panel structure of our data set necessitates that we control for individual subject effects through the rounds. Therefore, we report the results from a random-effects logit approach\textsuperscript{56} in Table 1, with the endowed condition as the baseline. For purposes of controlling for error variance, we also included other demographic variables\textsuperscript{57} in specification [1] (though we obviously are not attempting to test a positive theory about the relationship between demographic variables on manifest endowment effects).

\textsuperscript{55} On inspection of Figure 2, however, one can make some conclusions about whether our results are numerically (as opposed to statistically) significant. Indeed, one stark aspect about the Figure is how close each of the depicted schedules are to one another. This certainly does not suggest the many-fold WTA/WTP ratios found in previous endowment effect experiments. See \textit{supra} Section I A.

\textsuperscript{56} In performing these estimations, it is important to keep two factors in mind. First, the dependent variable of interest ($\text{NoMug}_{ij}$) is qualitative in nature, and thus ordinary least squares regression is inappropriate. Second, our data set is a panel data set, involving both cross-sectional and time-series dimensions (i.e., numerous subjects, each playing multiple rounds). As such, error terms in the above estimation ($\varepsilon_{ij}$) are likely to be highly correlated across subjects. See, e.g., M.R. Conway, \textit{A Random Effects Model for Binary Data}, 46 Biometrics 317 (1990). As a robustness check, we have done the same set of estimations using fixed effect approaches and a linear probability (GLS) approach. Those results are not reproduced here, but are qualitatively similar.

\textsuperscript{57} In particular, we included demographic characteristics such as a sex dummy, a dummy for whether the subject had two or more siblings in his/her household growing up, the marital/domestic partner status of the subject, whether the subject was non-white, and whether the subject was a social science major as an undergraduate. We included such demographics as a robustness check, given the existence of some research indicating that certain demographic traits (such as sex) are predictive of manifest biases. See sources cited in note 39, supra.
Table 1: Random Effects Logit on Agency-Condition Subjects

Baseline Case: Endowed condition

<table>
<thead>
<tr>
<th></th>
<th>[1]</th>
<th>[2]</th>
<th>[3]</th>
</tr>
</thead>
<tbody>
<tr>
<td>$w_{it}$</td>
<td>1.058897</td>
<td>1.033449</td>
<td>1.034438</td>
</tr>
<tr>
<td></td>
<td>0.1286107**</td>
<td>0.122762**</td>
<td>0.1230016**</td>
</tr>
<tr>
<td>Unendowed</td>
<td>0.3510971</td>
<td>0.3579227</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.7365878</td>
<td>0.8592371</td>
<td></td>
</tr>
<tr>
<td>ManySibs (&gt;=2)</td>
<td>-1.888621</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.7710764**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.5573271</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.7432729</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Married/Cohab</td>
<td>1.392177</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.8834465*</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Non-White</td>
<td>0.4721307</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.8822647</td>
<td></td>
<td></td>
</tr>
<tr>
<td>SocSci</td>
<td>-1.757632</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.8089902**</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Log Likelihood -163.31138 -170.56895 -170.65723
Constant Term Not Reported

(*= Significant at 90% level; **= Significant at 95% level)

Most importantly, note from specification [2] in Table 1 that membership in the unendowed condition has no numerically or statistically significant effect on the likelihood of parting with the mug. This statistical insignificance appears holds for any number of alternative specifications of the estimated model, and does not seem to turn on the addition of other demographic regressors (reported in specification [1]), or various interaction terms (not reported).

The observation that the agency context causes imputed values to converge, while important in its own right, begs the question of the precise manner in which this interaction effect takes hold. In particular, it leaves open questions of whether introducing the agency context (1) dampens the initial ownership valuation of an endowed subject; (2) strengthens the initial valuation of an unendowed subject; or (3) has both effects simultaneously. It is to this question we now turn.
Table 2 reports the results of a series of ordinary least squares regressions in which the dependent variable is the subject’s valuation of the mug. As noted above, for the subjects in the control conditions, this value was elicited directly; for the subjects in the agency conditions, we computed this valuation (as noted above) by taking the difference between the high and low wages offered at the lowest wage at which the subject opted for the money rather than the mug. Perhaps the most telling specification is [2], which regresses subjects’ imputed valuations on three dummy variables corresponding to our differing treatments, with the unendowed control group serving as a baseline. Note from the table that membership in either the endowed or unendowed agent group does not have a significant effect on predicted valuation. In other words, subjects in these groups appear to be behaving similarly to those in the baseline group of unendowed control subjects. Indeed, testing the hypothesis that these coefficients are jointly zero suggests confirmation of this conjecture ($F(2,100)=0.05; p=0.95$). Indeed, the only subgroup that appears to act in a significantly different fashion is the control group subjects who were endowed with a mug: it is these subjects who valued the mug more, thus generating a significant endowment effect. By contrast, putting an endowed individual into an agency relationship apparently induces her to behave as if she was never endowed to begin with. This pattern is consistent in the other two
Because the agency group subjects had their valuations elicited in dollar increments only, Figure 3 superimposes only the integer dollar observations of the control group (from Figure 1) atop the agency group (from Figure 2).

3. Quality of the Results

Although the interaction effect between agency contexts and endowment effects identified above appears to be strong, we nonetheless considered whether certain limitations of our experimental design hampered the reliability of our statistical analysis. Below, we discuss four of the most pertinent limitations.

a. Experiment Complexity and Insignificant Stakes

We first considered the possibility that the experiment was too complex for subjects to understand and/or that monetary stakes involved in the experiment were too low to induce the subjects to attend seriously to the decision before them. We rejected these alternative explanations for several reasons.

First, previous experimental literature suggests that our payoff levels are sufficient to induce subjects to care about the choices they make. Second, payoff levels in the agency condition were within the range of the payoffs employed in the control experiment in which subjects did display an endowment effect. Third, the subjects’ behavior suggests that the stakes were sufficient to generate meaningful results. Both endowed and unendowed subjects were responsive to financial incentives: as “price” for the mug (i.e., the wage difference, \( w_h - w_l \)) increases, subjects increasingly selected the all-cash allocation instead of keeping the mug. Indeed, at the low end of the scale (\( w_l = $3 \)) no group of subjects passed up the mug at a rate of more than 35%, whereas at the high rate of the scale (\( w_h = $11 \)), subjects in all groups parted with the mug at a greater than 95% rate. Moreover, Figure 2 reveals that the frequency with which subjects part with the mug is monotonically increasing in the high wage, \( w_h \). This is a strong indication that

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58 Because the agency group subjects had their valuations elicited in dollar increments only, Figure 3 superimposes only the integer dollar observations of the control group (from Figure 1) atop the agency group (from Figure 2).

59 See Camerer & Hogarth, supra note 55.

60 The one weak exception occurs with unendowed subjects, who part with the mug at a constant 75% rate for both \( w_h = $6 \) and \( w_h = $7 \).
the monetary amounts at stake are both sufficiently large and variable to obtain meaningful results. Indeed, if the monetary stakes were so small as to be considered trivial by subjects, we would expect to observe seemingly random behavior in all conditions. Finally, consistent with well-behaved results we note that the unendowed subjects are everywhere more willing to give up the mug than the endowed subjects (in other words, the two lines do not cross) -- although the difference is neither economically nor statistically significant.

The evidence that the frequency with which subjects parted with the mug is monotonically increasing in the high wage offers -- as theory would predict -- also suggests that subjects were not confused by the experiment. Confused subjects would be expected to behave more erratically.\textsuperscript{61}

\textit{b. Differences in the Subject Populations}

We also considered the possibility that differences in the subject pools in the two groups -- endowed and unendowed -- might be driving our results. We believe this is unlikely. The subjects were randomly distributed between the two groups, and we therefore have no systematic reason to expect that the two subject pools are different.

We nevertheless did consider the possibility that demographic differences in the population might be masking the endowment effect. In particular, there is evidence to suggest that women may value objects differently than men.\textsuperscript{62} Thus we considered whether this possibility might be affecting our findings as well. Gender differences in the subject populations could mask an endowment effect if, for example, women valued the mug \textit{less} than men, and if the endowed subjects were disproportionately female while the endowed subjects were either evenly distributed or disproportionately male.

Our data confirm the importance of considering the role of gender differences, revealing that women and men subjects did tend to value the mugs differently. For instance, in Table 1, women were more likely, all else held constant, to part with the mug at every wage level. Similarly, in Table 2, the implicit value placed on the mug by women subjects was approximately 90 cents lower than that of men.

Nevertheless, we did not find any evidence that these gender differences account for our interaction result, that the agency context mutes the endowment effect. Gender differences would plausibly mute evidence of an endowment effect only if a disproportionate number of the endowed subjects were women in the agency experiment. In our agency experiment, 57\% of the total group were women, while the endowed subgroup consisted of 52\% women (very close to our representative total). Moreover, in both specifications [1] and [3] above (and in Table 1), our principal results attempt to control for gender and our interaction result is robust to that finding. Finally, if gender biases were muting significant endowment effects in the agent condition, we

\textsuperscript{61} Our conclusion that they understood the experiment is supported by our interactions with the subjects, which indicated they were not confused. After reading the consent form, we gave the subjects an opportunity to ask questions. Subjects then played two practice rounds, to help ensure that they understood the experiment. Following the practice rounds, the subjects answered a detailed questionnaire designed to ensure that they understood the experiment. Finally, the choice the subjects had to make was quite straightforward: have a mug or get more money.

\textsuperscript{62} See Strahilevitz & Lowenstein, \textit{supra} note 52.
would expect to find evidence of an endowment effect in a single-gender subgroup. Testing for endowment effects in the male-only and female-only sub-populations of the agent condition reveals no significant evidence of an endowment effect.\footnote{Similarly, we did not find an endowment effect when we separated the sample based on race, domestic/marital status, or number of siblings.}

c. Condition Mismatch

We also considered the possibility that our results from the control and agency conditions are simply not comparable. While the predominant distinction between these two groups is one of framing and context (i.e., the exact effect we wish to test), it is important to acknowledge that the two sets of data were gathered using slightly different protocols. In particular, the control group valuations were elicited simultaneously in a single round, while the agency group valuations were collected ad seriatim in randomized rounds of bargaining. Moreover, the minimal increments for valuation tested were smaller (at $0.50) for the control group than they were for the agency group (at $1.00). Finally, subjects in the agency group were always guaranteed a payment of $2 more than those in the control group would receive, possibly giving rise to income effects. Given our data structure, then, one might conceivably argue that any cross-condition differences we find are an artifact of some of these other asymmetries.

We believe, however, that such concerns are assuaged by a few important considerations. First, our agency condition generates implicit valuations that square almost precisely with those in the unendowed control group. Indeed, as noted above, there is no statistical difference between the responses in these three constituent groups. As such, we feel relatively confident that even despite the slight differences in experimental protocol, the two groups are statistically comparable. Second, to our knowledge, the existing literature provides little indication that sequential elicitation of valuations works to eliminate the endowment effect entirely. While some studies have found that learning reduces the phenomenon over time,\footnote{See, e.g., Shogren et al, supra note 17 (finding that the divergence of WTP and WTA value measures is persistent, even with repeated market participation and full information). Accord Jack L. Knetsch, The Endowment Effect and Evidence of Non} the subjects within our agency conditions exhibit virtually no endowment effect at all on average.

Nevertheless, it is possible to gain at least some purchase on whether such learning considerations carry significant weight by concentrating our analysis on the initial rounds of the agency experiment. If a learning effect is substantially at play, we would expect a larger difference between the behavior of endowed and unendowed subjects early on in that experiment (both numerically and statistically), with convergence occurring only later.\footnote{Note that because the first round of the agency experiment only nominated a single wage, we cannot directly compare the first round of the agency experiments with the control (where a series of prices was nominated).} To explore this possibility, we reran the logit analysis reported in Table 1 (explicitly, specification [2]), but on a truncated data set consisting of only the first round, first two rounds, and first three rounds (respectively) of the
We did the same for truncated versions of the data set consisting of the first two, the first three, and the first four rounds.

Of course, without gathering more data it would be impossible to state with certainty whether the slight difference in protocols has a dispositive effect. There are other slight differences (such as the knowledge that only one of the eight rounds would count for final payoffs) that we cannot directly test.

Table 3 reports on these analyses.

**Table 3: Random Effects Logit on Early Rounds of Agency-Condition**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>$w_i$</td>
<td>0.492</td>
<td>0.759</td>
<td>0.870</td>
</tr>
<tr>
<td>Unendowed</td>
<td>0.161**</td>
<td>0.190**</td>
<td>0.180**</td>
</tr>
<tr>
<td></td>
<td>0.799</td>
<td>0.575</td>
<td>0.274</td>
</tr>
<tr>
<td></td>
<td>0.626</td>
<td>0.639</td>
<td>0.711</td>
</tr>
<tr>
<td>Constant Term Not Reported</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Standard Error in Italics

(*= Significant at 90% level; **= Significant at 95% level)

Recall from Table 1 that the coefficient on the “Unendowed” dummy in the 8-round case was equal to 0.3510971 with an insignificant standard error of 0.7366. The results of Table 3 are largely consistent with these earlier results. Indeed, even when the analysis is limited to Round 1 alone, unendowed subjects are not significantly more likely to pass up the mug for the money at any conventional significance level ($p$-value = 0.21). Limiting the analysis to the first two and the first three rounds, respectively, yielded results that were progressively similar to the full experiment. At the same time, even as the first-round analysis continues to give insignificant results, the results do appear to be slightly stronger than those of the full, eight-round analysis. This is, of course, consistent with the argument noted above that endowment effects can weaken through learning. However, Table 3 suggests that at least within our agency condition, they appear to be starting from a relatively weak, not statistically significant, condition to begin with.

**d. Framing Effects**

Finally, we considered a possible objection to the agency conditions in our study on the grounds that they introduce a type of framing effect: subjects in the endowed agency condition were told that they had to choose between contributing their own mug and refusing to do so, while those in the unendowed agency condition were told that they must choose between taking the firm’s mug and abstaining. This added contextual detail, one might argue, may have inadvertently introduced a moral distinction between the actions of the subjects in the two conditions. Subjects who possessed other-regarding preferences toward the firm might feel greater anxiety over the
prospect of taking the firm’s mug than failing to contribute their own to the enterprise.

While we recognize this potential criticism, we remain relatively confident of our results for a number of reasons. Most immediately, the framing effect concern tends to support our results instead of undercutting them. Indeed, the principal consequence of the framing effect described above would be to place the patina of moral blameworthiness on subjects who “took” the firm’s mug, making them more reluctant to do so than subjects who already felt a sense of rightful ownership. This would be expected to increase the differential between the endowed and unendowed subjects’ valuations. Thus, had we detected an endowment effect among our agency subjects, this critique would might render our results somewhat ambiguous, for we would not be able to identify whether the detected “wedge” separating the subjects was an artifact of endowment effects, framing effects, or some combination thereof. Given that we were unable to find such a wedge in the first place, however, the framing effect critique seems inapposite.

Nevertheless, as a failsafe, we ran one additional experimental treatment (not reported upon in detail here) in which unendowed subjects were simply given the choice between accepting a contract that contained a low wage with a mug (and in which the firm would receive a low payoff) verses a contract that contained a high wage with no mug (in which the firm would receive a high payoff). Such an experimental design helps to remove the possible moral blameworthiness of taking the firm’s mug. Comparing these unendowed agency “choosers” with our endowed agency subjects, we did not find evidence of a significant difference in valuations.

IV. UNDERSTANDING THE RESULTS WITH AN ADDITIONAL TEST

The data from our agency conditions suggests that the business agency context has a significant effect on subjects’ valuations, arguably bringing private valuation behavior in line with the predictions of rational choice theory. The obvious question is, why? This section posits two possible distinctions and attempts to test between them using an additional experimental condition.

The “Exchange Value” hypothesis: The first possible explanation for our results, and the simplest, is that the agency context makes salient the exchange value of the mug rather than its consumption value, thereby causing subjects to treat it as a fungible good. This conjecture is consistent with evidence that individuals do not endow goods that they view predominantly as trading goods or stores of value.\(^{69}\) It also is consistent with evidence that agents who repeatedly trade goods with uniqueness attributes (like sports memorabilia) do not display significant endowment effects.\(^{70}\) Confirmation of this hypothesis would comprise additional evidence that the

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\(^{68}\) In addition, we deliberately designed the experimental materials to involve a rather “thin” and unembellished description of the firm. “Amalgamated Products” had no individual associated with it, and the consequences (if any) of a low payoff for the firm were never made salient to the experimental subjects. Given that our primary enterprise was to test for endowment effects (and not other-regarding preferences) within organizational environment, such an approach is entirely appropriate.

\(^{69}\) See supra text accompanying note 43 and notes 33, 35 - 35.

\(^{70}\) See List, supra note 43.
endowment effect does not depend simply on objective factors – such as the presence of thick markets – but also on psychological factors affecting whether the subject focuses on the exchange value or the consumption value of the good.

The “Shared Entitlement” hypothesis: A second possible explanation is that the organizational context blurs the distinction between the agent’s own endowment and that of the firm (effectively substituting “our” in place of “my”). Viewed from this perspective, subjects who agreed to work for the firm knowing that the firm’s profits depended on its ability to use the asset might come to view the firm as jointly entitled to the asset. Their ensuing sense of obligation to the firm might cause them to be less protective of their entitlement than they would be otherwise. Moreover, this sense of asset commingling could also make unendowed agents feel more entitled to claim a share of the firm’s assets, and thereby more likely to value the mug highly. Combined, these joint or “shared entitlement” effects would cause a convergence of WTP and WTA in the organizational context, and could conceivably yield our agency results.

In order to assess which of these two hypotheses is most consistent with our results, we formulated an additional experiment designed to distinguish between them. In particular, in this experiment, we tested for the existence of endowment effects within an agency context which implicated the “exchange value” hypothesis, but in which the preconditions for the “shared entitlement” hypothesis were removed. We again did not find any evidence of a significant endowment effect, both confirming the results of our prior agency experiment and providing evidence consistent with the exchange value hypothesis.71

A. Detailed Description of the “Exchange Value” Experiment

The exchange value experiment was fundamentally similar to the basic agency endowment experiment in its basic procedures. As before, we divided the subjects into two groups: endowed and unendowed. As before, the subjects were told that they were being recruited to be a manager for Amalgamated Products,72 and were offered a choice between receiving a low wage coupled with a mug and receiving a higher wage and no mug. In this experiment, however, the subjects made this decision at the same time they decided whether to work for the firm. Moreover, in this experiment the subjects were not told that their choice would affect the firm’s profits: the subjects simply could increase their monetary wage if they chose, respectively, not to accept or to retain the mug.

Specifically, both endowed and unendowed exchange value subjects were told that, in each round, Amalgamated Products would make them an offer to come work for it consisting of a choice

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71 By implication, then, our results also suggest that our findings in Section II are not driven by the fact that the mug was a potential factor of production.

72 Although they were informed that their job at the firm would be one of a “manager,” these subjects were told nothing about any duties that such a job might entail (See Appendix, infra). This was done in order to minimize any potential effects of the shared entitlement hypothesis. (It is, of course, possible that the very use of the term manager might itself trigger a sense of shared entitlement, though we conjecture that such an effect would require greater contextual richness than the use of a single word).
These subjects also were first year law students who participated either in the summer before starting law school or in the first few weeks of the first semester. The payoffs and procedures were the same as the agency experiment.

of one of two employment contracts. The unendowed subjects were told that: “the two contracts will differ in the monetary amount offered and in whether or not the firm agrees to pay you a mug, in addition to a monetary wage.” The endowed subjects were told that the “two contracts will differ in the monetary amount offered, depending on whether or not you agree to contribute your mug to the firm when you go to work for it.” Each subject in each groups was then told that she would receive a $2 wage if she took/kept the mug, and a higher wage (ranging from $3 to $15 depending on the round) if she did not keep/take the mug. Each subject thus had three choices: reject both contracts and refuse to work for the firm for that round (in which case she would not get any monetary payoff), or agree to work for the firm and accept one of the two contracts. As before, each subject was told that one of the rounds would be selected at random at the end of the experiment and that she would receive the payoff they selected in that round.

The experimental instructions for this condition did not mention any impact of the mug on the firm’s profits. Nor were subjects told that the mug could be used as a factor of production. Thus, the primary difference between this experiment and the control experiment is that subjects were placed in a business context where the amount they were being offered for the mug took the form of wages for their services.

B. The Results

There were 76 subjects participating in this experiment: 73 endowed with a mug and 43 unendowed. Figure 4 presents an analysis of the relative frequency with which the endowed and unendowed subjects passed up possession of the mug, conditional on the high wage offer. As in previous experiments, both the endowed and unendowed subjects’ willingness not to end up with possession of the mug is monotonically increasing in the high wage offer. This is consistent with the payoffs being sufficiently large to motivate subjects to make meaningful choices; it also is consistent with subjects understanding the experiment.

[INSERT FIGURE 4 ABOUT HERE]

Visual examination of Figure 4 reveals evidence of a small WTP/WTA differential, but not one that is particularly economically significant. Endowed exchange value subjects are slightly less likely to choose the mug over cash at each implied valuation level, but the effect appears minimal at any valuation level. And indeed, the mean valuation of endowed subjects ($3.81) exceeded that of the unendowed subjects ($3.38) by $0.43, approximately 27 percent of the observed difference in the control group of $1.57. Moreover, as with the agency condition, the two samples display an identical median valuation of $3.00.

Statistically comparing the mean valuations indicates that this modest numerical difference is not only economically modest but also is statistically insignificant (one-sided $t$-test, $p=0.252$,

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73 These subjects also were first year law students who participated either in the summer before starting law school or in the first few weeks of the first semester. The payoffs and procedures were the same as the agency experiment.
A Dictator game is one in which one subject is given a specific amount of money (such as $1), and told she can share as much (or as little) of it as she likes with the other player.

See, e.g., Robert Forsythe, Joel L. Horowitz, N. E. Savin, and Martin Sefton, *Fairness in Simple Bargaining Experiments*, 6 GAMES AND ECONOMIC BEHAVIOR 347 (1994). This is in apparent contrast with the presumptions of many law and economics scholars. Although nothing in economic theory pre-ordains it, conventional law and economics generally defines “self interest” rather myopically, giving rather scant attention to other regarding preferences (or “ORPs”) such as guilt, altruism, or other independent motivations for inter-personal fairness. Some theorists have suggested that the evidence of other-regarding behavior is simply an artifact of experimental observation -- of people’s desire to appear “fair” to the experimenters in a low stakes situation -- and people do not exhibit other-regarding behavior in the real world. The existing evidence, however, does not support this hypothesis: although experimenter observation does affect other-regarding behavior, subjects manifest ORPs even in a “double blind” Dictator games in which neither the other experimental subject, nor the experimenter, would know of the Dictator’s decision. See, e.g., Hoffman, McCabe, Shachat, & Smith, supra note 38; see also Gary Bolton & Rami Zwick, *Anonymity vs Punishment in Ultimatum Bargaining*, 10 GAMES & ECONOMIC BEHAVIOR 95 (1995).

Likewise, a non-parametric two-sample Wilcoxon rank sum test fails to reject the null hypothesis of distributional symmetry (z=0.68, p=0.49), though less emphatically than in the agency condition.

C. Discussion

These results are consistent with the hypothesis that situating subjects in an agency context mutes the endowment effect because the subjects focus on the exchange value of the entitlements for trade.

Our conclusion that our agency experiment results were primarily a product of exchange value effects, not “shared entitlement” explanations, is supported by some of the literature on other-regarding preferences, which suggests that subjects would have been unlikely to manifest significant other-regarding preferences in the context of our experiment. The possibility that agents might manifest other-regarding behavior towards their principals motivated the “shared entitlement” hypothesis. Yet, the evidence suggesting that people exhibit other-regarding behavior also suggests they would not be particularly likely to do so in the context of our experiment. For example, although there is evidence involving “Dictator” games showing that Dictators do share a portion of the available surplus, when Dictators are told that the two players are in a “buyer” and “seller” relationship they are significantly more selfish. In addition, the evidence suggests that subjects are particularly disposed to be other-regarding with regard to a known recipient with whom they have had contact, and are much less disposed to be other-regarding with regard to an

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* A Dictator game is one in which one subject is given a specific amount of money (such as $1), and told she can share as much (or as little) of it as she likes with the other player.

* See, e.g., Robert Forsythe, Joel L. Horowitz, N. E. Savin, and Martin Sefton, *Fairness in Simple Bargaining Experiments*, 6 GAMES AND ECONOMIC BEHAVIOR 347 (1994). This is in apparent contrast with the presumptions of many law and economics scholars. Although nothing in economic theory pre-ordains it, conventional law and economics generally defines “self interest” rather myopically, giving rather scant attention to other regarding preferences (or “ORPs”) such as guilt, altruism, or other independent motivations for inter-personal fairness. Some theorists have suggested that the evidence of other-regarding behavior is simply an artifact of experimental observation -- of people’s desire to appear “fair” to the experimenters in a low stakes situation -- and people do not exhibit other-regarding behavior in the real world. The existing evidence, however, does not support this hypothesis: although experimenter observation does affect other-regarding behavior, subjects manifest ORPs even in a “double blind” Dictator games in which neither the other experimental subject, nor the experimenter, would know of the Dictator’s decision. See, e.g., Hoffman, McCabe, Shachat, & Smith, supra note 38; see also Gary Bolton & Rami Zwick, *Anonymity vs Punishment in Ultimatum Bargaining*, 10 GAMES & ECONOMIC BEHAVIOR 95 (1995).

* Hoffman, McCabe, Shachat, & Smith, supra note ?, at Table II, page 357. Similarly, players in an Ultimatum Games -- in which the first player is given money and told she can share as much as she likes with the other, but where the other can reject the offer, in which case neither player gets anything -- display significant evidence of other-regarding behavior in many circumstances, but when subjects are told at the outset to maximize their winning, the first players generally make minimal offers to share with the other player. K. Binmore et. al, *Testing Noncooperative Bargaining Theory: A Preliminary Study*, 75 AMERICAN ECONOMIC REVIEW 1178 (1985). For a discussion of this experiment see THALER, WINNER’S CURSE, supra note 5, at 26-27.
unknown person.77 Thus, a fictitious profit-maximizing company -- Amalgamated Products -- with no human representative would appear a particularly unlikely subject of other-regarding sentiment, given the existing evidence.78

Nevertheless, although our results suggest that the exchange value condition is sufficient to mute endowment effects – even absent other-regarding “shared entitlement” concerns – our results do suggest some role for such other-regarding, albeit only a weak one. Specifically, comparing the mug valuations of the exchange value subjects with those of the agency subjects reveals that the exchange value subjects valued the mug on average more than the agency subjects. Specifically, the mean valuation of the exchange value subjects was $3.57, which is $0.54 larger than the mean valuation of the agency subjects of $3.03. This suggests that subjects concerns for the firm may cause both endowed and unendowed subjects to value the mug less – while not necessarily affecting the differential value between them. Nevertheless, in the context of our experiment, this effect is relatively weak: the $0.54 difference is not only modest numerically but statistically different from zero only at relatively weak significance levels (one-sided t-test, \( p=0.098, \; df=142; \) two-tailed t-test, \( p=0.193, \; df=142 \)). This basic finding emerges from more complicated regression analyses as well (which we therefore omit in the current analysis). Given the purely fictitious nature of our firm, our finding of some evidence consistent with other-regarding behavior even in this context suggests that further exploration of other-regarding preferences in the business context may be warranted.79

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77 For example, Bohnet and Frey ran a set of experiments involving three versions of the Dictator game: (1) the Dictator and recipient did not know each other’s identity; (2) the Dictator and recipient look at each other (so they can identify each other) but do not speak; (3) the Dictator and recipient may speak with each other, face-to-face, but may not make a binding contract. They found that while the Dictator allocated, on average, one quarter of the initial endowment to the recipient in the first version of the game, the second and third treatments produced virtually even splits, and were statistically indistinguishable from one another. Iris Bohnet and Bruno S. Frey, *The Sound of Silence in Prisoner’s Dilemma and Dictator Games*, 38 JOURNAL OF ECONOMIC BEHAVIOR & ORGANIZATION 43 (1999); Iris Bohnet and Bruno S. Frey, *Social Distance and Other-Regarding Behavior in Dictator Games: Comment*, 89 AMERICAN ECONOMIC REVIEW 335 (1999). Bohnet and Frey seem to find more sharing in the anonymous condition than do Hoffman, McCabe, Shachat, & Smith, supra note 38. However, the experimental protocols are not given with enough specificity to allow the reader to conclude whether or not the results are inconsistent.

78 This is not to say that people do not manifest other-regarding preferences towards institutions. For example, Eckel and Grossman compared willingness to donate to ordinary recipients versus the American Red Cross and found that Dictators allocated on average only 10% of their payoffs to the ordinary subject recipients, but allocated 31% of their payoffs to the American Red Cross. Catherine C. Eckel and Philip J. Grossman, *Altruism in Anonymous Dictator Games*, 16 GAMES AND ECONOMIC BEHAVIOR 181, 187 (1996). Nevertheless, this evidence is consistent with subjects’ preferring known and worthy recipients to those they are not familiar with. Amalgamated Products was neither a charitable organization nor one that was known to the subjects (since it is a fictitious company).

79 For example, there is evidence of other-regarding preferences in the agency context when the principal is an individual rather than a company. See, e.g., Vital Anderhub, Simon Gächter, and Manfred Königstein, *Efficient Contracting and Fair Play in a Simple Principal-Agent Experiment*, INSTITUTE FOR EMPIRICAL RESEARCH IN ECONOMICS, UNIVERSITY OF ZURICH, WORKING PAPER NO 18 (August 1999); W. Guth, Manfred Königstein, J. Kávacs, and E. Zala, *Fairness within Firms: The Case of One Principal and Multiple Agents*, Working Paper, Humboldt University Berlin (1999); W. Guth, W. Klose, Manfred Königstein, and J. Schwalbach, *An Experimental Study of a Dynamic Principal-Agent Relationship*, 19 MANAGERIAL AND DECISION ECONOMICS 327 (1998); see also E. Fehr,
CONCLUSION

Behavioral law and economics has considerable potential implications for the study of corporate law. Yet scholars’ efforts to analyze corporate law from this perspective have been hampered by considerable uncertainty about whether phenomena identified in the laboratory persist in the corporate context. This article has attempted to take a first step towards a more systematic application of behavioral law and economics to company law by inquiring into the robustness of a well-known cognitive bias—the endowment effect—within an agency relationship. We find that introducing a business agency relationship tends to dampen (and virtually eliminate) this effect.

There are several possible implications of our results, both for the study of business organizations and for behavioral law and economics generally. Specifically, our results suggest that the business agency context may operate (at least in part) as a debiasing mechanism, thereby representing an underappreciated consequence of organizing trade within the firm.\(^{80}\) Of course, just as the business environment mitigates some behavioral biases (such as the endowment effect), it could certainly exacerbate others. Indeed, this is an area where increased attention to cognitive biases by corporate scholars clearly seems to be warranted.\(^{81}\)

In addition, our results highlight a broader message concerning the relationship between normative policy analysis and experimental research. Reform proposals, by their very nature, tend to be sweeping and general, implicating large populations in highly heterogeneous environments. By contrast, experimental research is necessarily constrained within a specific, controlled environment, purposely isolated from other aspects of the real world. This isolation is both its great strength and its profound weakness. Observing a predictable behavioral pattern within a controlled experimental setting enables researchers to make causal claims with minimal fear that unobserved phenomena or reverse causality are driving the results. Yet this very controlled setting makes it difficult to generalize to real-world settings that are the focus of policy reform proposals. To be sure, we as a society must sometimes make difficult policy choices even in a world of

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\(^{81}\) See Langevoort, *supra* note 4. Indeed, given the context-dependency of endowment effects, we cannot say that we have shown that subjects would invariably fail to manifest endowment effects in the aggregate. Under different conditions (for example, a different asset), subjects might manifest endowment effects. The central message of our results is that scholars cannot simply assume that behavioral results found in outside the agency context are present within the agency context.
imperfect empirical knowledge. Nonetheless, that informational constraint should also not lead us to ascribe undue explanatory power to any methodological paradigm, be it economics, psychology, behavioral law and economics, or something else.
APPENDIX

Subject Instructions

1. Agency Experiment: Endowed Subjects

If you proceed with the experiment, you will be given a USC mug, which can be purchased at the campus bookstore.

The central purpose of the experiment is to examine decision making by managers of corporations. You are to assume that you are being recruited by a large consumer marketing firm, Amalgamated Products Corporation, which is seeking a manager for a new marketing project. Amalgamated will offer to pay you for your managerial services. Should you accept its offer, the project you are to manage will produce a monetary payoff for the firm, which could consist of either a low payoff or a high payoff, an outcome that depends on actions you take as manager. Specifically, the success of the project depends on whether you contribute your mug to the project: if you contribute your mug the project will produce a high payoff for the firm, if you do not it will produce a low payoff for the firm.

In each round of the experiment a representative from Amalgamated Products will offer you a contract to work for the firm in exchange for money. The amount the representative of the firm offers to pay you may depend on whether the project yields a low payoff or a high payoff. Alternatively, the representative may offer to pay you a fixed amount of money regardless of whether the project eventually yields a low payoff or a high payoff. You may accept or reject the firm's offer as you see fit. All offers are made on a take-it-or-leave-it basis, and are good only for the round in which they are made. There is no bargaining. Thus, if you reject the representative's offer the round will end immediately, and your personal payoff for that round will consist solely of your mug. You will indicate your acceptance or rejection on the sheet of paper which states the offer.

Should you accept the firm's offer, you will become the manager of the project. You will then be asked to make a choice between contributing your mug to the project or keeping it for yourself. This decision is strictly yours. Indeed, regardless of your choice, the firm must pay you whatever wage it offered you. However, as previously stated, your decision about whether to contribute your mug determines whether the project yields a high payoff or a low payoff.

You will indicate your choice of whether to contribute your mug on a sheet of paper which you will return to the experimenter. Your personal payoff for the round can then be determined. Your personal payoff for the round will consist of whatever the firm owes you (depending on whether it was a high payoff or low payoff project), plus your mug (if you had decided to keep it instead of contributing).

You will play eight rounds of this experiment (in addition to two practice rounds), for a total of approximately one hour. The terms of the contract you are offered will vary from round to round, and do not depend on anything that happened in any previous round. Your additional compensation (if any) for participating in the experiment will be based on the outcome of one out of the eight rounds of the experiment. In particular, at the end of the experiment the experimenter will use a random process to determine which of the eight rounds counts for everyone. You will then be paid whatever you earned in that round. For example, if the random process were to select round 1, and if in round one you had rejected the firm’s contract, your payoff would consist solely of $5 plus a mug. Alternatively, if the random process were to select round 4, and if in round 4 you had agreed to a contract paying you a flat wage of $2, and if you decided to keep your mug, your total compensation would be $7 (=5+$2) plus the mug. Or, if the random process were to select round 2, and if in round 2 you had agreed to a contract paying you a wage
of $2 for a low payoff and $8 for a high payoff, and if you decided to contribute your mug, your total compensation would be $13 (=5+8) and no mug. You will begin each round with your original mug, even if you had chosen to contribute it in the previous round.

2. Agency Experiment: Unendowed Subjects

The central purpose of the experiment is to examine decision making by managers of corporations. You are to assume that you are being recruited by a large consumer marketing firm, Amalgamated Products Corporation, which is seeking a manager for a new marketing project. Amalgamated will offer to pay you for your managerial services. Should you accept its offer, the project you are to manage will produce a monetary payoff for the firm, which could consist of either a low payoff or a high payoff, an outcome that depends on the actions you take as manager.

Specifically, the success of the project depends on whether a mug the firm owns is used in the project: the project will yield a high payoff for the firm if the mug is used, and a low payoff if the mug is not used for the project.

In each round of the experiment a representative from Amalgamated Products will offer you a contract to work for the firm during that round in exchange for money. The representative may offer to pay you one amount if the project generates a low payoff for the firm and a different amount if the project generates a high payoff. Alternatively, the representative may offer you a fixed amount of money regardless of whether the project eventually yields a low payoff or a high payoff for the firm. You may accept or reject the representative's offer as you see fit. All offers are made on a take-it-or-leave-it basis, and are good only for the round in which they are made. There is no bargaining. If you reject the representative's offer, the round will end immediately for you, and your payoff for that round will consist of nothing. (Be aware, however, that a rejected offer still "counts" as one of the rounds you played.) You will indicate your acceptance or rejection of the proposal on the sheet of paper which states the offer.

Should you accept the representative's offer, you will become the manager of the project. As manager you will have exclusive access to some of the firm's property (and in particular, the mug). This access affords you the opportunity to take the firm's mug for yourself, rather than leaving it to be used as an input for the project. Indeed, regardless of your choice, the firm must pay you whatever wage it offered you. However, your decision about whether to take the firm's mug determines whether the project yields a high payoff or a low payoff.

You will indicate your decision as to whether to take the mug on a sheet of paper and return it to the experimenter. This will determine whether the project yields a low payoff or a high payoff, and thus what the firm owes you. Your personal payoff for the round, therefore, will consist of whatever the firm owes you, plus the mug (if you had decided to take it).

You will play eight rounds of this experiment (in addition to two practice rounds), for a total of approximately one hour. [Similar instructions to those above follow]

3. “Exchange value” Experiment: Unendowed Subjects

The central purpose of the experiment is to examine decision making by managers of corporations. You are to assume that you are being recruited by a large consumer marketing firm, Amalgamated Products Corporation, which is seeking a manager for a new marketing project. Amalgamated will offer to pay you for your managerial services. In each round of the experiment, a representative from Amalgamated Products will give you a proposal to work for the firm during that round. The proposal will give you a
choice of one of two employment contracts. The two contracts will differ in the monetary amount offered and in whether or not the firm agrees to pay you a mug, in addition to a monetary wage.

All offers are made on a take-it-or-leave-it basis. There is no bargaining. You have three choices. You may reject both contracts and refuse to work for the firm for that round. If you do this the round will end immediately for you, and your payoff for that round will consist of nothing. (Be aware, however, that a rejected offer still “counts” as one of the rounds you played). Alternatively, you may agree to work for the firm and accept one of the two contracts. You will indicate your choice on the sheet of paper which states the offer and return it to the experimenter.

Your choice will determine what the firm owes you. Your personal payoff for the round, therefore, will consist of whatever money the firm owes you, plus the mug (if you had decided to accept the contract that gave you a mug.)

You will play eight rounds of this experiment (in addition to two practice rounds), for a total of approximately one hour. The terms of the contracts you are offered will vary from round to round, and do not depend on anything that happened in any previous round. Your additional compensation for participating in the experiment (if any) will be based on the outcome of one out of the eight rounds of the experiment. In particular, at the end of the experiment the experimenter will use a random process to determine which of the eight rounds counts for everyone. You will then be paid whatever you earned in that round. For example, if the random process were to select round 1 and if in round 1 you had rejected the firm’s contracts, your payoff would consist of nothing, so your total compensation would be your $5 initial payment. Alternatively, if the random process were to select round 4, and if in round 4 you had agreed to a contract paying you a flat wage of $2 and a mug, your total compensation would be $7 (= $5 + $2) plus the mug. Alternatively, if the random process were to select round 2, and if in round 2 you had agreed to a contract paying you a wage of $8 and no mug, your total compensation would be $13 (= $5 + $8) and no mug.

4. Exchange value Experiment: Endowed Subjects

At the beginning of the experiment, you will be given a USC mug, which can be purchased at the campus bookstore.

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The central purpose of the experiment is to examine decision making by managers of corporations. You are to assume that you are being recruited by a large consumer marketing firm, Amalgamated Products Corporation, which is seeking a manager for a new marketing project. Amalgamated will offer to pay you for your managerial services.

In each round of the experiment, a representative from Amalgamated Products will give you a proposal to work for the firm as a manager during that round. The proposal will give you a choice of one of two employment contracts. The two contracts will differ in the monetary amount offered, depending on whether or not you agree to contribute your mug to the firm when you go work for it. Specifically, the firm will offer you $2 to work for the firm if you retain your mug; the firm will offer you a higher wage if you agree to work for them and contribute your mug to the firm, with the amount ranging from $3 to $15. All offers are made on a take-it-or-leave-it basis. There is no bargaining.

Thus, you have three choices. You may reject both contracts and refuse to work for the firm for that round. If you do this the round will end immediately for you, and your payoff for that round will consist of nothing. (Be aware, however, that a rejected offer still “counts” as one of the rounds you played).
Alternatively, you may agree to work for the firm and accept one of the two contracts. You will indicate your choice on the sheet of paper which states the offer and return it to the experimenter. You will indicate your choice on a sheet of paper which you will return to the experimenter. Your choice will determine what the firm owes you for that round. Your personal payoff for the round, therefore, will consist of whatever money the firm owes you, plus the mug (if you had decided to accept the $2 wage contract and retain your mug.)

You will play eight rounds of this experiment (in addition to two practice rounds), for a total of approximately one hour. [Similar instructions to those above follow].
FIGURES FOR ARLEN, SPITZER & TALLEY
FIGURE 1: RATE AT WHICH CONTROL GROUP OPTS FOR MONEY OVER MUG
Figure 2: Rate at which Agency Condition Subjects Opt for Money over Mug

(*Horizontal axis denotes $w_h - w_l$*)
Figure 3: Comparing Control Subjects to Agency Subjects
Figure 4: Rate at which Exchange value Subjects Opt for Money over Mug

(*Horizontal axis denotes [w_h - w_i]*)